Essays on the Political Economy of Subnational Public Finances

Thesis by
Welmar Eduardo Rosado Buenfil

In Partial Fulfillment of the Requirements for the
degree of
Doctor of Philosophy

CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

2017
Defended May 15th 2017

Welmar Eduardo Rosado Buenfil
ORCID: 0000-0001-5179-7812

ACKNOWLEDGEMENTS

First, I would like to express my sincere gratitude to my academic advisor and chair of my commitee, Rod Kiewiet, for his guidance and support. His knowledge and expertise of state and local public finances is unmatched.

I am also indebted to my committee members, Michael Alvarez, Michael Ewens, and Phil Hoffman, who provided me with invaluable comments, making better research.

I would like to extend my thanks to other two faculty members, Alex Hirsch and Jean-Laurent Rosenthal, who helped me greatly at various stages of my degree progress.

I would also thank my Caltech colleagues and friends, in particular, Tatiana Mayskaya, Myungkoo Song, Li Song, Lucas Núñez, Jay Viloria, and Alejandro Robinson for their comments, discussions, and suggestions.

I am also grateful to Reyes Ruiz, for his support on the public pension chapter. Emma Burris-Janssen at the Hixon Writing Center for helping me with my writing skills. Laurel Auchampaugh and Kapáuhi Stibbard for their invaluable support.

I could not have fulfilled anything without the help and support of my parents, Welmar and Magaly, and my grandparents, Eduardo, Gloria, Roger, and Addy, and my aunts, Gorethy and Yalile.

Most importantly, I am incredible grateful to my wife, Stephany. For your love and support, for agreeing to make this journey with me, and for sharing my belief that, it does not matter where we are or what we do, as long as we are together nothing else really matters.

ABSTRACT

This dissertation is comprised of three essays addressing the connections between democracy and public finance at the subnational level. Chapter 2 focuses on variation within public expenditure, revenue, and debt at the state level in the United States for the period 1977-2011 seeking to uncover whether there is evidence of efforts by incumbents to manipulate state public finances to influence election outcomes. I find that total public expenditure increased the year before elections, especially during the period 1977-1994, but it is mostly driven by intergovernmental expenditure. Meanwhile, for later years there is a sizable reduction in tax revenue during electoral years, which results a debt increase. In Chapter 3, I extend the literature of political budget cycles at the subnational level to include pension funding. I explore the relationship between state pension funding and gubernatorial elections in the United States for the years 2001-2014. I show that one is more likely to observe the government and other employers undercontributing to pension funds during the pre-electoral year. I also found fluctuations in the pension fund's portfolio composition depending on how close a gubernatorial election is. Finally, in Chapter 4, I investigate whether the credit rating agencies (Fitch, Moody's, and Standard and Poor's) adjust the timing of their ratings as a function of the electoral calendar. I collect a novel database using credit ratings of Mexican states, and estimate panel models for gubernatorial elections in Mexico for 2005-2015. My results indicate that credit rating agencies delay announcing rating downgrades until after elections, especially when elections are very competitive.

TABLE OF CONTENTS

LIST OF ILLUSTRATIONS

LIST OF TABLES

Chapter 1

INTRODUCTION

This dissertation focuses on the political economy of state public finance. It contains three chapters which detail how state public finance changes when there is a gubernatorial election on the horizon. The chapters follow a logical order. It goes from the traditional study of public finance and elections, to unexplored indicators of the political budget cycle, to new players in the budgetary game.

The chapter titled "Public Finances for Electoral Purposes? Evidence from US States" takes the traditional approach of studying political budget cycles by observing how indicators of public finance vary depending on how soon a gubernatorial election is. For this, I estimate variation within state expenditure, revenue, and public debt, for states in the US, between the years 1977 and 2011. This chapter extends the current literature by analyzing an extended time period, and by considering three indicators of public finance, instead of only focusing on expenditure. I find that total public expenditure increased during the year before the elections, especially for the first half of the sample, but this increase is mostly driven by intergovernmental expenditure. Meanwhile, for more recent years there is a sizable reduction in tax revenue during electoral years which results in debt increases.

The traditional approach to studying political budget cycles uses cash-based accounting. This underestimates the real magnitude of the distortions generated by manipulating public finances for electoral purposes. For example, if wages are persistent over time, an increase in wages today for political reasons will translate into a higher fiscal pressure during the next period. Therefore, it would be more convenient if the analysis of political budget cycles were to use accrual-based accounting in order to fully understand the economics of the cycle. In chapter 3, I

take a step in this direction. In this chapter, titled "Political Pension Contribution Cycles, Evidence from US States", I incorporate pension funding into the study of political cycles. Pension fund obligations are not considered public debt. However, state and local pension funding decisions made today will impact the balance sheet of state governments in the future. Undercontribution to, or changes in pension fund assets will determine how underfunded pension plans will be, and therefore how much extraordinary resources will be required to pay for pension fund obligations. In this chapter I explore how contributions calculated as a percentage of the Annual Required Contribution (ARC) change as a function of the gubernatorial electoral calendar. In this chapter I also investigate whether portfolio composition changes during this cycle; in particular, I study how the percentage invested in equity varies during electoral periods. I show there is undercontribution to pension funds during pre-electoral years. I also find that investments in more conservative assets are made in non-electoral years.

After observing the results of the first two chapters, I can conclude that there is evidence of changes in public finances as a function of the electoral calendar. In anticipation of this, there are institutional arrangements which are in place so that there are incentives for the government to fulfill their roles in an efficient and optimal way, preventing these manipulations. In the fourth and last chapter titled "Political Credit Rating Cycles: Evidence from Gubernatorial Elections in Mexico", I explore one of these institutions: The Credit Rating Agencies (CRAs). CRAs are in charge of analyzing different indicators of public finance (including budget and pension funding) and assigning a rating to each entity they rate. So, in principle, CRAs should adjust their ratings if there are changes in the status of public finances. However, they are not functioning appropriately. The problem with CRAs as monitors of public finances is that the structure of the market generates a conflict of interest which results in inaccurate ratings. In this chapter I analyze

whether CRAs are delaying announcements of credit rating downgrades during electoral periods. For this I collected a novel database based on press releases and credit rating reports for Mexican states and observe how these ratings fluctuate as a function of the gubernatorial electoral calendar. My results indicate that CRAs delay announcing rating downgrades until after elections, especially when elections are very competitive.

Chapter 2

PUBLIC FINANCES FOR ELECTORAL PURPOSES? EVIDENCE FROM US STATES

2.1 Abstract

In this chapter I analyze variation within public expenditure, revenue, and debt at the state level in the United States for the period 1977-2011 to uncover whether there is evidence of efforts by incumbent politicians to influence the outcome of elections. I find that total public expenditure increased during the year before elections especially for the period 1977-1994, but is mostly driven by intergovernmental expenditure. Meanwhile, after 1994, there is a sizable reduction in tax revenue during electoral years which results in a debt increase.

2.2 Introduction

> "When you think economics, think elections. When you think elections, think economics."
>
> -Edward Tufte (1978)

In light of the recent global financial crisis, the dynamics and efficiency of public finance have reemerged as central parts of public discourse. In the past few years in the U.S. we have seen a federal government shutdown, cities like Detroit and San Bernardino are going bankrupt, debates are raging about the public deficit, taxes, health care services, and many other important issues of public finance. States were particularly affected by the crisis: in the second quarter of 2009, personal income tax revenue fell 27% from the previous year, contributing to an overall decline of 17% in total state tax collections (Gordon, 2012; Kiewiet and McCubbins, 2014). The recovery has been slow. In real terms state tax revenues as well as state and

local government consumption and investment were 5% lower in 2013 than in 2008 (Kiewiet and McCubbins, 2014). Perhaps it is now more important than ever to understand the motivation for each budgetary decision and cut down the electorally motivated public expenditures and debt.

In this chapter I examine the dynamics of public finance at the state level in the United States. I want to determine whether budgetary variations are politically motivated rather than part of an efficient economic plan. Are adjustments to public finance a function of the electoral calendar? In principle, state budgetary decisions should be made in response to the situation of the economy and the necessities and requirements of its inhabitants, not because an election is on the horizon.

As Nordhaus first showed, in an incumbent's term in office there is a predictable pattern in policy, in which there is austerity in the term's early years and more flexibility before elections (Nordhaus, 1975). Indeed, Tufte (1980) demonstrated that economic movements in the months preceding an election can tip the political balance and decide the election's outcome. The electorate rewards incumbents for prosperity and punishes them for recession. So spurts in economic growth in the months immediately preceding an election can benefit the incumbent. These spurts can come in different forms and affect governments differently; for example, in his famous paper, Hibbs (1977) acknowledged that conservative governments benefit from inflation reduction in the months preceding elections, while liberal governments benefit more from reductions in unemployment. Unfortunately (or fortunately), governments do not have full control of the economy, but more so of the budget, as it was mentioned by Blais and Nadeau (1992): "... governments control their budgets whereas they can only hope to have some indirect impact on the economy; economic performance is the outcome of a myriad of decisions taken by consumers, workers, producers as well as governments, national and local, in the country but also internationally". Therefore, it is more useful to study the cycle

using the budget (political budget cycle) instead of the general state of the economy (political business cycle).

The literature on political budget cycles has traditionally focused on the national level. In particular, for the United States, Drazen (2001) compiled the literature of political budget cycles. He concludes that aggregate economic conditions before an election have a significant effect on voting patterns, but there is no significant increase in aggregate economic activity prior to elections in either the United States or other OECD countries. However, if we use fiscal policy as an alternative indicator of the existence of a political budget cycle, the conclusion is different: there is evidence of pre-electoral increases in transfers and other fiscal policy instruments in a number of countries. In the United States, this effect appears the strongest prior to 1980 (Drazen, 2001).

At the subnational level the literature is less developed, not only for the study of the political budget cycles themselves, but also for the determinants of public finance. As Garand, Ultrick, and Xu (2013) mention "It would be only a slight exaggeration to suggest that the study of state fiscal policy has become somewhat of a forgotten area of study". Paradoxically, states are a better setting to examine fiscal policy and political budget cycles, as Alt and Rose (2007) have argued: "Restricting the domain to U.S. states holds relatively constant a wide range of socio-economic, political, and cultural characteristics that might otherwise confound the analysis". States are more comparable between themselves as opposed to making comparisons between nations. Moreover, states are unable to expand the monetary base so they have to fund expenditure either by taxes or by debt making the cycle evident using these indicators.

Can we extrapolate Drazen's observations regarding the existence of a political budget cycle at the national level to the state level? John E. Chubb (1988) showed that

state economic conditions, and the assumption of gubernatorial responsibility for them, have a small but significant impact on gubernatorial election outcomes. Additional public monetary resources might be helpful to the governor to win elections. They can be used to generate employment or increase public works, or they can be directly spent in transfers to the voters. To pay for these extraordinary expenditures, politicians need to tap into additional financial resources. One alternative would be to increase taxes. However, taxes are unpopular in general so increasing them would diminish, or even reverse the effect of the additional expenditure in terms of votes. An alternative way to fund additional expenditure is by increasing public debt. It is true that debt has to be paid by the taxpayers eventually, but it is possible that voters are not aware of this, or are biased towards immediate consumption. Voters might see debt as windfall revenue as opposed to taxation. Voters are less interested in accountability when resources come from windfalls and not from taxation (Paler, 2013).

The electoral calendar is just one possible determinant of state fiscal policy. There are several other determinants which have been studied through the years. The vast majority of the literature focuses on a limited number of determinants in the states. One exception is Merrifield (2000), who estimates a very large model including ideological, institutional, demographic, and economic variables in order to find correlations between these variables and the indicators of fiscal policy. He finds that ideological and demographic variables are important determinants of tax and revenue, while institutional ones are less influential. Merrifield's model contains over two dozen independent variables, but he only uses 130 observations taken from pooled cross-samples spanning three non-consecutive years to determine which variables are most influential. This means that his results are not very precise and cannot provide us with much information on the role of any particular variable.

Even with the limited nature of his sample, Merrifield's finding that institutional

restrictions are relative unimportant determinants of public finance deserves attention. In principle, institutional constraints should limit the ability of public officials at the state level to manipulate the budget, in particular to generate a budget cycle. However, the effectiveness of tax and expenditure limits (TEL) has been highly questioned in the literature. Most studies about these limitations found that they were mostly ineffective. Cox and Lowery (1990) find that the behavior of states with limitations was similar to those with no limitations. Where differences are observed, they are as often as not the direction of what might be expected. Bails (1990) also shows that TELs had virtually no impact on the growth of statewide expenditures or revenues, and thus concludes that TELs as presently constructed are an ineffective means of limiting growth in state budgets. Kousser (2008) has also shown that TELs are ineffective in controlling expenditure at the state level.

With respect to public debt we reach a similar conclusion. Kiewiet and Szakaly (1996) find that most of institutional limitations are ineffective in controlling the increase in debt, the only exception being debt approval by referendum. The reason behind the ineffectiveness of institutional limitations in public finance might be that money is fungible and can easily move from one account to another while simultaneously complying with the letter of the law (McCubbins and McCubbins, 2013). These claims, however, are not undisputed. Rose (2006) finds that states that restrict the closing of budget gaps with burrowed funds make the political budget cycle negligible. New (2010) distinguishes TELs by origin and finds that TELs which were originated from ballot initiatives actually reduce government growth, but this is not the case for TELs with legislative origins. A more in-depth discussion of the literature on TELs can be found in Rose (2010).

Another branch of the literature has focused on the political determinants of state public finance. Some scholars have studied how the item veto power of the governor modifies the dynamics of public finance. For example, Nice (1988) finds no evidence

that the item veto restrains spending. On the other hand, Alm and Evers (1991) find that the effectiveness of the item veto depends largely upon the political parties of the governor and legislature, with the item veto more likely having a negative impact on expenditures in those states in which the parties differ. Partisanship has been studied on its own as well. Lowry, Alt, and Ferree (1998) find that Democrats are rewarded for unexpected, moderate increases in fiscal scale under unified Democratic government but punished for fiscal cuts. These results are symmetrically opposite for Republicans. They also find support for the conjecture that accountability for state-level policy is stronger when one party controls the executive and legislative branches. Rogers and Rogers (2000) find that greater political competition in the race for governor acts as a check against bigger government. Alt, Bueno de Mesquita, and Rose (2011) find that economic growth is higher and taxes, spending, and borrowing costs are lower under reelection-eligible incumbents than under term-limited incumbents, and under reelected incumbents than under first-term incumbents.

There is also a side of the literature which is more skeptical of political factors as determinants of public finance. Ellis and Schansberg (1999) find that economic, institutional, and demographic variables are important determinants of public debt, meanwhile political variables such as partisanship or control of the legislature are irrelevant for debt financing. Gilligan and Matsusaka (2001) use historical evidence from the first half of the 20th century, to show that it is the size of the state legislature and not its partisanship that is related to public expenditure.

As we can see, there are several pieces of literature regarding political variables as determinants of fiscal policy. However, the literature related to public finance and the timing of gubernatorial elections in the US is less developed. In other countries these have been explored in various occasions. For Canada, Blais and Nadeau (1992) found an expenditure increase of 1% in provinces in electoral years during 1951-

1984. Kneebone and McKenzie (2001) show that the predilection for provincial governments of all political stripes to increase taxes is temporarily halted during electoral periods, and also show expenditure increases in highly visible areas during 1966-1997. For Portugal, Veiga and Veiga (2006) found an increase in expenditure and deficit and a reduction in taxes in municipalities for the period 1979-2001. For West Germany, Galli and Rossi (2002) found increases in total expenditure, financed by public deficit at the Lander level for years 1974-1994.

In the United States, Melchior et al. (1993) analyzed the local level in urban areas (more than 100,000 people) for the period 1978-1985, and found that there was a disproportionate tendency for mayor-councils to have the lowest tax increase in the fiscal year of an election, but without an increase in public expenditure. At the state level two pieces of the literature explore this relationship. Using panel data from 1979-1999, Rose (2006) finds evidence of political business cycles in deficits and spending, but not taxes. In this paper, I revisit this conclusion by incorporating more recent data, which provides evidence of a more pronounced cycle measured with tax revenue. I also correct a methodological problem of this paper, by removing the yearly fixed effects in its estimations. Including yearly fixed effects is problematic since most gubernatorial electoral years are performed on even years, and most of them are two years after the presidential electoral year. This makes the vector of gubernatorial elections almost collinear with the yearly fixed effects and makes its interpretation complicated.

The second paper in this area of research is Alt and Rose (2007). They analyze factors that influence the political budget cycle in the level of public spending at the state level during 1974-1999. They search for determinants that made the political budget cycle larger. They accomplished this by using as dependent variable the difference between the expenditure per capita for the electoral year and for the middle of the electoral cycle (two years before the election). However, I find their

approach can be improved in several ways. First, their approach reduces by half the number of studied years by only considering the electoral year and the year in the middle of the cycle. Meanwhile, there are interesting dynamics in the year immediately before and after the election. Second, they only focus on expenditure, and this was perhaps the best approach for the time frame they studied, but the cycle could be reflected in other indicators of public finance as time goes on. To account for this, I use all expenditure, revenue, and debt to measure the cycle. Third, they estimate the cross effects of different variables with gubernatorial electoral years, but many of these variables are themselves problematic. For example, they use approval rating of the governor as proxy of electoral competitiveness; however this variable is highly endogenous since we expect that the unobserved spending mood of the governor would be correlated with her approval in first place. Instead, I use a proxy of competitiveness that is independent of the popularity of the governor and is explained in the methodology section below.

2.3 Hypotheses and Model

In order to acknowledge the existence of a fiscal political budget cycle at the state level I analyze the three main components of public finance: expenditure, revenue, and debt. In order to make comparisons among states of very different sizes, my variables are presented in per capita terms. My hypotheses are the following: 1. I expect increases in public state expenditure levels in electoral years, 2. I expect a reduction of revenue in electoral years, and 3. I expect public debt to increase in these years.

To test the hypotheses I use fixed and random effects models with panel data. I incorporate yearly data that includes expenditure by state, tax revenue, and level of state debt, and takes into account electoral years. I propose the following model:

$$\Delta Y_t = \alpha + \sum_{k=1}^{3} \beta_k \cdot electionyear_{k,t} + \gamma \cdot X_t + u_t,$$

where ΔY_t is the change in per capita terms of the variable of interests in year t with respect to the same variable in year $t-1$. The variable of interest can be expenditure, revenue, or public debt. Variables $electionyear_{k,t}$ are a series of dummies which indicate the year in the electoral cycle in year t. I include the year before the elections, the electoral year itself, and the year after the elections, the year in the middle of the cycle being the omitted category. X_t is the matrix of covariates and u_t is the residual term. The parameters of interests are β_k. My baseline estimations include state fixed effects. With this, I control for unobserved time-invariant patterns of expenditure inherent to each state, including the institutional framework. I also present my results for the random effects model, which is also useful to test hypothesis when making partitions of the data.

I make three interesting partitions of the data. The first is to divide my sample in half: the earlier years against the most recent ones. This allows finding how the political budget cycle has been changing over time, and is also useful to revisit the current literature regarding the topic. The second is between states which have Tax and Expenditure Limitations and states that do not. This has a double objective: to observe if these rules are serving their purpose, and to see how they change the political budget cycle. Finally, I divide the sample in states which have gubernatorial election during the same year as the presidential and other states. This is important since the presidential election is every four years (same as gubernatorial elections) and could be confounding the analysis.

In my estimations I include other variables which change over time and could be affecting indicators of public finance. These variables are change in unemployment, the Hoolbrook Van Dunk index of electoral competitiveness (HVD), the party of

the governor, and if the state has a unified government.

2.4 Data

The variables of interests are changes in total expenditure, tax revenue, and debt for states in the US for the period 1977-2011. All amounts are expressed in per capita terms in 2010 US Dollars. The source of this data is the United States Census Bureau State Government Finances (2016), and the United States Census Bureau Population and Housing Unit Estimates (2016).

For public expenditure, I use the canonical indicator of total expenditure, a broad measure that comprises nearly all state level spending. For state revenue, I focus on tax revenue, since the state has more control over it compared to other types of revenue such as intergovernmental revenue. For debt, I use *Debt at the end of the fiscal year* this being defined as all interest-bearing short-term credit obligations and all long-term credit obligations incurred in the name of the government and all its dependent agencies. This includes all debt, whether backed by the government's full faith and credit or non-guaranteed.

The variable "change in unemployment" accounts for economic downturns. This is relevant for public expenditure, revenue, and debt. If unemployment is rising the government might want to stimulate the economy by increasing expenditure or reducing tax rates. For debt, an increase in unemployment has a double effect: in tandem with increase in expenditure the economic downturn might reduce government revenue, generating additional debt. I use national unemployment instead of state unemployment to avoid problems of endogeneity of the stimuli that can be generated in the economy when there is an expansionary fiscal policy.

In some models I partition the data by tax and expenditure limitations (TEL). I use the document published by the National Conference of State Legislatures (2012) which

classifies each state as having a TEL or not. The document mentions that as of 2010, 30 states operate under a TEL. I coded this variable as dichotomous according to the list published in the aforementioned document. It is important to note that by using this variable I am making the strong assumption that all TELs have similar effects across all states, and this is not true in general. All states have different legislation that is implemented in different ways (McCubbins and McCubbins, 2013). A more detailed analysis is beyond the scope of this dissertation, but can be found in Kiewiet and Szakaly (1996) for debt, McCubbins and McCubbins (2013) for taxes, and, for expenditure, Kousser et al. (2008).

Partisanship could also be relevant to determine the dynamics of public finance, and if the governor has majority in the state legislature or not. In order to account for this, I include the interaction of the political party of the governor by the control of the legislature. Since Democrats are thought to be more liberal towards budget decisions I expect the variable to be positively correlated with public expenditure, taxes, and public debt. The status of having divided government or not was published by Carl Klarner (2014). The value of this variable is one if all three institutions of state government (i.e., the two chambers of the legislature and the governor's office) are not controlled by the same party.[1]

I also control for electoral competitiveness in the state. I expect governors in very competitive states to have more incentive to increase public expenditure and debt (and to reduce taxes) since any increase in electoral performance could have large returns (this is winning or losing the election). I use the Holbrook van Dunk (HVD) index. This index is available for almost all states and years in my sample, and it

[1] Klarner does not publish this variable for Nebraska, since its legislative power only resides in one chamber, and senators are elected in a non partisan election. However I coded this variable according to the biographies published in "The Blue Book" of Nebraska (http://nebraskaccess.ne.gov/bluebookbios.asp) which has the biographies of all senators. These biographies usually have the partisanship of each senator, allowing me to determine if there was a divided government or not.

is clean in the sense that it only uses electoral data (Holbrook 1993). The HVD index has different versions depending on the number of years it averages. I use the four-year HVD index since it is the shortest in the series, better reflecting the short run competitiveness of the state. My source for this index is the database published by Carl Klarner (2014).

In Figures 2.1-2.6 we can see that expenditure, revenue, and debt per capita have been increasing in real terms during the period of interest for all analyzed states. Expenditure increased from an average[2] of 3,200 to 3,800, 4,800, and 6,500 in 1980, 1990, 2000, and 2010, respectively. Tax revenue increased from an average of 1,600 to 1,950, and 2,300 in 1980, 1990, and 2000, respectively, but then did not increase in real terms from 2000 to 2010. Debt has increased from 1,600, to 2,300, 2,500, and 3,600 in 1980, 1990, and 2000, respectively.

(Figures 2.1-2.6 found at the end of this chapter)

Given the increasing trend in all our variables of interest, in order to perform a better analysis it is necessary to work with first differences. Once we observe the first differences of the variables of interest (Figures 2.2-2.6) we notice that the trend is eliminated.

For this chapter, I only considered the states which for the whole sample had a four-year gubernatorial electoral calendar. Therefore, I removed the states that had (or still have) a two year calendar. These states are Arkansas, New Hampshire, Rhode Island, and Vermont. I also removed Alaska from the sample since its public finances are so different from the other states it would require a different type of analysis. Given the 34 years of the sample, most states had eight to nine ordinary gubernatorial elections in the studied period.

[2] In 2010 USD

2.5 Results

Public Expenditure

In Table 2.1 column (3), I estimate variations in public expenditure as a function of the electoral calendar. I observe increases in public expenditure of 44 dollars per capita in the pre-electoral year compared to the year immediate before it. This pattern is consistent with the expected direction of the hypothesis. However, it is also important to notice that we also observe an increase of 35 dollars in the post-electoral year, which was not expected.

(Table 2.1 found at the end of this chapter)

This result is not new, the studies by Rose (2006) by Alt and Rose (2007) found increases in expenditure during the pre-electoral year, but their sample corresponds only to the first half of mine, and perhaps expenditure patterns have been changing for most recent years. In order to test this, I divide my sample in half, and with this estimate if there has been a transition between both periods. My estimates for both subsamples can be found in Table 2.1 columns (1), and (2). At simple sight, it appears that changes in expenditure as a function of the electoral calendar in both periods might be different, but since several things in the estimation are changing, such as coefficients, standard errors, and unobserved fixed effects, it is necessary to test if these differences are indeed statistically significant. In order to do this, I perform series of Chow tests.

For making the coefficients in both samples comparable, it is necessary to remove the constant term (including fixed effects). This procedure would not be adequate if the fixed effects in my model are substantially explaining the estimations. For this reason I compare the model with and without fixed effects (Table 2.1 columns (1,2,3) against columns (4,5,6) and perform a Hausman test. According to the Hausman test,

for both subsamples, fixed and random effects model are indistinguishable (p = 0.61 and p=0.96 respectively). For the aggregate sample, fixed effects have explanatory power (p= 0.00), but this difference is being driven by the change in the coefficient of the control of Unified Government with Republican Governor. Without this control both models using the full sample are indistinguishable (p=0.14).

Based on this, I pool both samples, remove the fixed affects (see Table 2.2 Column 1) and perform Chow test to see if the estimation of the political budget cycle has changed. The first question that arises is how can we compare the magnitude of the political budget cycle. It is easy to see that expenditure has been increasing faster in all stages of the cycle during the second half of the sample by comparing the coefficients for each period in Table 2.2. In all cases expenditure is increasing faster for more recent years, and this difference is statistically significant (p<.02) in all quarters. Therefore, the direct comparison of the political budget cycle in expenditure is not appropriate. A better alternative is to see how expenditure changes from one year to another within each of these groups. In order to perform this comparison, I choose a year in the cycle and see how much other years change with respect to it. For example, if we wanted to test if expenditure is larger in the pre-electoral year compared to the year in the middle of the cycle in the first half of the sample, the test to perform would be:

$$Election_{-1} \times SecondHalf - Election_{-2} \times SecondHalf = Election_{-1} \times FirstHalf - Election_{-2} \times FirstHalf$$

With this setup it is possible to test whether the budget cycle in expenditures changed between both periods. The answer to this question using my sample is that it depends on the reference year. I do not find statistically significant differences when the year of reference is the year of the middle of the cycle ($Election_{-2}$), or the post-electoral year ($Election_{+1}$). However, there is an important difference between both time

periods regarding the change in expenditure from the pre-electoral to the electoral year. I estimate the cycle to be larger and statistically significant in the earlier period by $45 dollars per capita (p<.1).

(Table 2.2 found at the end of this chapter)

These differences are relevant in the sense that the political budget cycle was larger, and easier to see in earlier years. For the most recent years, the political budget cycle measured with expenditure is less evident.

Expenditure and Tax and Expenditure Limitations

As I mentioned before, it is important to incorporate the role of institutional restrictions on the liberty of public officials to adjust the public finances to gain political favor. Several states have in place tax and expenditure limitations which, in principle, could prevent or at least limit the size of the political budget cycle. Therefore, the natural question whether tax and expenditure limitations are indeed changing the dynamics of expenditure arises.

(Table 2.3 found at the end of this chapter)

The approach I take to investigate whether this is the case or not is to follow a similar strategy as for comparing the earlier and later parts of the sample. I estimate changes of expenditure as a function of the electoral calendar independently for the subsample of states which had a TEL, and for the ones that did not (See Table 2.3), and then estimate a model using the pooled sample (See Table 2.4, column 1) and do a Chow test comparing the coefficients of both. By visually inspecting the estimations for states which have TELs and the ones that do not it is easy to see that the budget cycle is similar for both groups. This suspicion is confirmed

when I perform the Chow test to determine whether the differences are statistically significant. It does not matter which year of the electoral cycle is picked as the reference, all differences between both groups are not statistically significant. This result is consistent with the aforementioned results in the literature that found TELs are not accomplishing their objective of limiting expenditure.

Expenditure and Presidential Electoral Calendar

In this section I explore how the Presidential Electoral calendar influences the results estimated above. This is an important verification since the presidential electoral calendar is also every four years. Changes in federal budget and policy could be well having an impact on the public finances of the states. As I did before, I split the sample between states which their gubernatorial electoral calendar coincide with the presidential one, and the ones which do not.

(Table 2.5 found at the end of this chapter)

Different from before, the sample sizes are very different. Only a handful states with a four-year electoral calendar had gubernatorial elections at the same time of the presidential one. These states are: Delaware, Indiana, Missouri, Montana, North Carolina, North Dakota, Washington, and Utah. In Table 2.5, we can compare the estimates between these two groups of states. In column (1) we have states in which the gubernatorial elections coincide with the presidential election. In column (2) all other states. At first glance it appears that the estimations for the political budget cycle are smaller and do not reach statistical significance for the first group of states. However, this is mainly derived from the smaller sample. This is confirmed when I perform a Chow test to observe if the differences in both groups are statistically significant or not. For this, consider the estimations in Table 2.6. It does not matter

which year of the electoral cycle is chosen as the reference, all differences between both groups are not statistically significant. Again, it is very difficult to conclude that both groups of states are indeed equal in their expenditure cycle, since it is difficult to make inference based on only a handful of states. Therefore, additional data is necessary to confirm or reject this hypothesis.

(Table 2.6 found at the end of this chapter)

Expenditure and Other Controls

One natural question that arises when discussing different patterns of public finance is partisanship. Again, the use of fixed effects in the estimations complicates the identification of partisanship. If a state does not change the party of the governor often, the fixed effect of that state is going to be prevalent over partisanship. For this reason, it is more convenient to analyze the estimations in which fixed effects were omitted from the analysis. The conclusion is that partisanship was correlated to expenditure increases only in the first half of the sample (refer to Table 2.1 columns 1 and 2). For the second half of the sample, the difference in the average level of expenditure increase is indistinguishable between both parties.

As for changes in national unemployment, they have a positive correlation with expenditure. This estimation reaches statistical significance in all models and subsets of the sample. In all cases, increases in national unemployment is associated with increases in expenditure. In particular, this is similar for states that have a TEL and states that do not. Looking at this result from another perspective, in principle TELs could have the repercussion of limiting the incumbent office holding to adjust spending for legitimate reasons such as downturns in the economy, but we do not observe this is the case. Again, TELs are not reflected much on the expenditure side.

Expenditure Components

An interesting result regarding expenditure is when we analyze what components of it are prone to fluctuate more as a function of the electoral calendar. In Table 2.7, I show estimations for changes in different components of expenditure as a function of the electoral calendar (using the full sample). Interestingly enough, expenditure is not increasing because of the components we would expect when an election is on the horizon. Expenditures in education, public welfare, and hospitals do not fluctuate much when an election is coming. The change in total expenditure that we observed earlier is mostly being driven by intergovernmental expenditure, which is defined by the US Census Bureau (2016) as "Amounts paid to other governments as fiscal aid in the form of shared revenues and grants-in-aid, as reimbursements for performance of general government activities and for specific services for the paying government, or in lieu of taxes. Exclude amounts paid to other governments for purchase of commodities, property, or utility services, any tax imposed and paid as such, and employer contributions for social insurance".

(Table 2.7 found at the end of this chapter)

The reduction of the size of the expenditure cycle in recent years does not necessarily imply that political budget cycles are decreasing in the United States. Perhaps what is driving this is the increase party polarization, which results into ideological heterogeneity. For example, some governors might benefit more for the increase in expenditure, while others from reduction in taxes. These two types would both generate a political budget cycle in the public finances, but only the first would be evident using the traditional approach of looking at expenditures. For this reason it is relevant to study revenue and debt as a function of the electoral calendar as well.

Tax Revenue

In Table 2.8 I estimate how tax revenue fluctuates as a function of the electoral calendar. Analogous to the analysis done for public expenditure, In column (1) I perform this estimation for the first half of the sample, in column (2) for the second half, and in column (3) for the full sample, always including fixed effects.

(Table 2.8 found at the end of this chapter)

Public revenue has not been studied much in its role in the political budget cycle. In the first half of the sample (prior to 1994) we see that the decrease in public revenue coming from taxes was observed in the year after the election (estimated reduction of 31 dollars per capita), perhaps as a result of campaign promises. This does not immediately help the incumbent governor in their reelection prospects since the reduction in taxes was not prior to the election. However, for more recent years (after 1994), we see that the decrease in revenue from taxes is during the electoral year. I estimate this decrease in 38 dollars per capita. As it was the case with expenditure, several components of the estimation are changing, such as coefficients, standard errors, and unobserved fixed effects. So in order to determine if the estimated differences in the periods are statistically significant, I follow the same methodology done with expenditure and perform a series of Chow tests. In Table 2.8 columns (4), (5), and (6), remove the fixed effects to compare the estimates, and I perform Hausman tests to see if random and fixed effects models are different among each other. Coefficients indeed look very similar when working with and without fixed effects, and the Hausman test cannot reject the null of both models being identical for both subsamples.

Since both models are comparable, I remove the constant and fixed effects, and calculate the pooled model to perform the Chow test. These estimates used for the

Chow tests are show in Table 2.2 column (2). The Chow test indicates that there are important differences between both periods. The cycle measured with tax revenue is statistically significant larger in the second half of the sample when we measure them in the decrease in tax revenue in the electoral year with respect to all other years in the cycle.

Tax Revenue and Tax and Expenditure Limitations

In Table 2.9 I compare the estimations for the political budget cycle in tax revenue when dividing the sample in states which have TEL and states that do not. As I did before, I perform a Chow test to observe if changes in the political budget cycle measured with tax revenue and dividing the sample by TEL produces statistically significant differences.

(Table 2.9 found at the end of this chapter)

In this case, I estimate that tax revenue is decreasing during the electoral year only in states which have TELs. For all other years of the electoral calendar we cannot discard the hypothesis that tax revenue increases equally in states with TELS and states without. Given this, my estimations suggest that the political budget cycle is larger in states with TEL if we measure the change in Tax revenue from the electoral year to the post-electoral year. Interpreting if Tax and Expenditure limitations are fulfilling their objective of restricting tax growth is not straightforward. I estimate they indeed they do during the electoral year, but not so in other years. It is hard to image that the objective of a TEL would only focus on one year of the electoral calendar.

Tax Revenue and Presidential Elections

Analogous to the exercise performed for changes in expenditure, I make a partition of the sample regarding possible differences in the political budget cycle measured with tax revenue depending if the gubernatorial election calendar coincides with the presidential (See Table 2.10). As before, we notice that the difference in sample size is large since only eight states have concurrent elections with the presidential. Given this, estimations for this group have sizable standard errors. Therefore, the Chow tests to identify differences between these groups are almost always negative. Only in one case there is a difference between groups: I find that for gubernatorial post-electoral year, tax revenue is increasing faster in states which have concurrent elections with the presidential. This finding is interesting in its own right, but does not affect the finding of this paper regarding decreases in tax revenue during the gubernatorial electoral year, and in any case would only attenuate its size. If non-concurrent states would increase tax revenue at the same rate as concurrent states, the relative decrease in revenue during the electoral year would be even larger.

(Table 2.10 found at the end of this chapter)

Tax Revenue and Other Controls

As for the relationship between tax revenue and unemployment, we can observe that there is indeed a negative relationship: in all models and subsets of the sample, increases in the national unemployment rate is associated with decreases in tax revenue. In particular, for the full sample I estimate that an increase in 1% unemployment rate in the country decreases tax revenue in $60 per capita. Partisanship of the governor and having unified or divided government does not reach statistical significance when estimating changes in tax revenue.

Subsets of Tax Revenue

When considering only subsets of tax revenue (See Table 2.11), we can observe that the decrease in electoral years are being driven by General Sales Tax and Individual Income Tax. It is interesting to notice that these taxes are being paid directly by the voters themselves, as opposed to taxes that are paid only by a reduced percentage of the population such as license and corporate income taxes. This is consistent with the hypothesis of an electoral motivation behind decreases in tax revenue during electoral periods. I also estimate that all types of tax revenue decrease when national unemployment increases.

(Table 2.11 found at the end of this chapter)

Public Debt

In Table 2.12 I show my estimations regarding how public debt fluctuates as a function of the electoral calendar. I follow the same procedure as for public expenditure and tax revenue. In column (1) I show my estimates for the first half of the sample. In column (2) for the second half, and in column (3) for the full sample, always including state fixed effects.

(Table 2.12 found at the end of this chapter)

At first glance, comparison between both columns suggest that the dynamics of debt has completely changed from the first half of the sample (1977-1994) to the second half (1994-2011). During the first period there was no clear relationship between debt and elections, but in the second one, we see that indeed debt increases much more rapidly during the electoral year, compared to the year before and after the election. In order to formally test these results I proceed to perform a series of Chow

tests using both subsamples. For this, I remove the state fixed effects (the Hausman test confirms that fixed effects and random effects models are indistinguishable) and the constant and compare the estimations for both subsamples (See Table 2.2 column 3). The results of the Chow test confirm this. In the first half of the sample there is no clear political budget cycle measured with public debt, but there is for the second period. During the second half of the sample there is a larger increase of public debt during electoral years compared to the post-electoral years, in line with the hypothesis. I estimate this difference to be of 60 dollars per capita in the second part of the sample. Another finding was that public debt is also increasing more during the year of the middle of the cycle. This does not go in line with my hypothesis, but a likely explanation of it is shown below when I discuss how the introduction of the presidential electoral calendar into the estimation changes the results.

These results are enlightening: we are finding a recurring and strong relationship of having elections in the dynamics of public finance at the state level in the United States when measured with debt. The current literature on political budget cycles has focused on expenditure, but my results show that tax revenue and debt fluctuate more in recent years. Debt is a much more sensitive indicator than expenditure since it is robust to different causes of budgetary deficit, either increase in expenditure, reduction in taxes, or both. This is helpful if we consider heterogeneity in states. We expect that more liberal governments base their campaigns in generation of employment and in providing more services to the population. Conservatives, on the other hand, are in favor of a smaller government and reduction in taxes. When we use public debt as an indicator, then both phenomena are reflected when we estimate the cycle.

Public Debt and TEL

In Table 2.13, I compare estimations for the political budget cycle measured with public debt when dividing the sample in states which have TEL and states that do not, and I perform a Chow test to identify differences between both. I estimate that debt is increasing faster on states which have a TEL, and this is only the case during the year in the middle of the electoral cycle. Therefore, there is no evidence that TELs are restricting the increase in public debt, in any case it is the opposite.

Public Debt and Presidential Elections

In Table 2.14 we make different estimations for states which have gubernatorial elections at the same time as the presidential election, and for all other states. As mentioned before, since there are eight states in the sample that are in the first category, standard errors for these estimations are rather large. As before, I also perform Chow tests to be able to determine if the observed differences are statistically significant.

For both groups debt increases faster in the electoral year compared to the post-electoral year. However, there is also an important difference. Even when considering the large standard errors for the first group, I estimate that debt increases faster in the pre-electoral year; meanwhile for the non-concurrent group it increases faster in the post-electoral year. What this implies is that debt is increasing faster for both groups of states when there is a presidential election on the horizon. So the conclusion for this exercise is that the increase in debt during the gubernatorial electoral year is robust to having a presidential election, but other increases in debt are not independent from the presidential electoral calendar.

2.6 Conclusion

My analysis of expenditure, revenue, and debt at the state level in the US shows that the political budget cycle has transitioned in its form over time. Indeed, for earlier years (1977-2011), the cycle was observed mainly on expenditures. Meanwhile, during recent years (1994-2011), I do find a sizable political budget cycle measured with revenue and debt. When we use tax revenue and debt instead of expenditure, the estimation of the cycle is clearer, larger and more robust to different specifications of the model.

I also incorporate the role of Tax and Expenditure limitations and I found that in general there is no significant statistical difference between the states that have these limitations and those that do not in different indicators of public finance. This result is consistent with the majority of the literature in this topic. My results are also robust to the incorporation of the presidential electoral calendar, although it is an important determinant of state public finance.

With these findings I propose reexamining the current status of the literature regarding political budget cycles in different countries and levels of government. By only considering expenditure, the current literature could be well underestimating the real size of the cycle.

2.7 Tables

Table 2.1: Estimations - Changes in Total Expenditure by Period

	Dependent variable:					
	Δ Expenditure PC					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	44.957***	33.718*	43.902***	43.713***	33.053*	43.128***
	(16.692)	(19.796)	(13.015)	(15.948)	(19.819)	(12.967)
Election	4.901	37.398*	18.413	2.677	38.393*	17.386
	(15.786)	(19.667)	(12.629)	(15.127)	(19.690)	(12.555)
Election + 1	24.711	44.510**	35.689***	22.547	43.516**	35.140***
	(16.250)	(19.112)	(12.636)	(15.750)	(19.112)	(12.559)
Δ Unemployment	12.286**	43.781***	32.876***	13.266**	45.599***	33.259***
	(6.015)	(7.019)	(4.595)	(5.984)	(7.002)	(4.610)
Divided + Democrat	16.708	16.038	20.172	7.857	22.059	17.331
	(22.325)	(24.933)	(14.419)	(12.564)	(18.180)	(11.470)
Unified + Democrat	−11.983	64.641***	16.352	2.399	41.645**	9.570
	(17.310)	(23.685)	(12.961)	(11.819)	(19.679)	(11.321)
Unified + Republican	−13.091	−51.594*	3.068	−16.318	−20.194	−9.298
	(32.839)	(28.360)	(16.706)	(19.354)	(17.217)	(12.336)
Δ Competitiveness	1.814	−2.451	−0.371	0.114	−2.918	−1.848
	(3.339)	(3.500)	(2.415)	(3.134)	(3.432)	(2.387)
Constant				46.397***	83.284***	65.082***
				(11.766)	(17.235)	(10.515)
Years	1977-1994	1995-2011	1977-2011	1977-1994	1995-2011	1977-2011
State FE	Y	Y	Y	N	N	N
Observations	765	765	1,530	765	765	1,530
Adjusted R^2	0.022	0.086	0.046	0.021	0.080	0.047

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.2: Estimations for Chow Test for Differences Between Periods

	Dependent variable:		
	Δ Expenditure PC	$\Delta TaxesPC$	$\Delta DebtPC$
	(1)	(2)	(3)
Election − 1 × Late	129.306***	47.900***	30.012
	(14.316)	(10.354)	(20.465)
Election × Late	128.148***	9.081	111.499***
	(14.117)	(10.211)	(20.192)
Election + 1 × Late	130.648***	28.941***	53.460***
	(13.558)	(9.831)	(19.519)
Election + 2 × Late	90.000***	47.143***	105.037***
	(14.268)	(10.322)	(20.412)
Election − 1 × Early	81.652***	20.099*	43.924**
	(14.656)	(10.586)	(20.872)
Election × Early	36.574***	33.956***	52.595***
	(13.548)	(9.803)	(19.387)
Election + 1 × Early	59.559***	−2.209	40.944**
	(14.287)	(10.315)	(20.324)
Election + 2× Early	34.743**	29.823***	15.211
	(14.367)	(10.371)	(20.429)
Δ Unemployment	29.563***	−62.040***	−2.438
	(4.586)	(3.278)	(6.341)
Divided + Democrat	22.805**	−2.278	19.053
	(11.190)	(8.233)	(16.637)
Unified + Democrat	22.518**	4.034	4.617
	(11.021)	(8.038)	(16.002)
Unified + Republican	−21.466*	5.430	−16.929
	(12.503)	(9.283)	(19.045)
Δ Competitiveness	−2.100	2.346	−1.010
	(2.357)	(1.689)	(3.277)
Years	1977-2011	1977-2011	1977-2011
State FE	N	N	N
Observations	1,530	1,530	1,530
Adjusted R^2	0.076	0.196	0.011
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table 2.3: Estimations - Changes in Total Expenditure by TEL

	Dependent variable:					
	Δ Expenditure PC					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	50.124**	37.233**	43.902***	49.451**	40.251**	43.128***
	(22.624)	(15.867)	(13.015)	(22.582)	(15.731)	(12.967)
Election	22.775	19.415	18.413	22.068	16.276	17.386
	(22.463)	(15.226)	(12.629)	(22.361)	(15.017)	(12.555)
Election + 1	33.036	36.304**	35.689***	29.022	37.556**	35.140***
	(22.182)	(15.288)	(12.636)	(22.074)	(15.148)	(12.559)
Δ Unemployment	35.488***	29.587***	32.876***	35.235***	31.192***	33.259***
	(8.062)	(5.630)	(4.595)	(8.016)	(5.609)	(4.610)
Divided + Democrat	30.890	24.722	20.172	−5.609	21.211	17.331
	(25.763)	(18.483)	(14.419)	(17.405)	(14.575)	(11.470)
Unified + Democrat	74.550***	−8.449	16.352	35.191*	−9.579	9.570
	(23.913)	(16.424)	(12.961)	(18.312)	(13.922)	(11.321)
Unified + Republican	3.209	3.525	3.068	−4.712	−8.046	−9.298
	(28.805)	(21.887)	(16.706)	(17.303)	(16.595)	(12.336)
Δ Competitiveness	1.221	−2.783	−0.371	1.235	−3.325	−1.848
	(3.763)	(3.275)	(2.415)	(3.660)	(3.174)	(2.387)
Constant				57.395***	70.907***	65.082***
				(16.771)	(13.226)	(10.515)
TEL	Y	N	Both	Y	N	Both
State FE	Y	Y	Y	N	N	N
Observations	577	953	1,530	577	953	1,530
Adjusted R^2	0.063	0.042	0.046	0.059	0.054	0.047

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.4: Estimations for Chow Test for Differences Between TEL

	Dependent variable:		
	Δ Expenditure PC	$\Delta TaxesPC$	$\Delta DebtPC$
	(1)	(2)	(3)
Election − 1 ×TEL	104.611***	21.570*	23.642
	(15.358)	(11.039)	(22.418)
Election × TEL	76.696***	3.963	90.289***
	(15.228)	(10.947)	(22.235)
Election + 1 × TEL	87.129***	20.914*	49.702**
	(14.967)	(10.778)	(21.964)
Election + 2 × TEL	51.000***	35.727***	93.950***
	(15.535)	(11.157)	(22.619)
Election − 1 × NO TEL	107.026***	42.002***	42.306**
	(13.122)	(9.453)	(19.273)
Election × NO TEL	81.587***	31.118***	72.972***
	(12.368)	(8.923)	(18.234)
Election + 1 × NO TEL	103.913***	9.612	43.652**
	(12.472)	(9.003)	(18.426)
Election + 2× NO TEL	70.093***	39.417***	38.380**
	(12.898)	(9.289)	(18.930)
Δ Unemployment	31.857***	−60.674***	−1.828
	(4.648)	(3.267)	(6.303)
Divided + Democrat	23.530**	−0.128	21.626
	(10.721)	(7.932)	(16.654)
Unified + Democrat	14.130	1.786	0.981
	(10.745)	(7.844)	(16.025)
Unified + Republican	−5.500	9.619	−5.784
	(11.551)	(8.641)	(18.534)
Δ Competitiveness	−2.597	1.951	−1.180
	(2.396)	(1.692)	(3.284)
TEL	Both	Both	Both
FE	N	N	N
Observations	1,530	1,530	1,530
Adjusted R^2	0.045	0.190	0.004
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 2.5: Estimations - Changes in Total Expenditure by Concurrent with Presidential

	Dependent variable:					
	Δ Expenditure PC					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	27.154	47.722***	43.902***	27.505	47.229***	43.128***
	(28.631)	(14.667)	(13.015)	(28.309)	(14.720)	(12.967)
Election	−0.338	23.495*	18.413	−0.847	22.880	17.386
	(28.512)	(14.174)	(12.629)	(28.191)	(14.222)	(12.555)
Election + 1	26.783	40.666***	35.689***	26.529	40.507***	35.140***
	(28.773)	(14.197)	(12.636)	(28.467)	(14.240)	(12.559)
Δ Unemployment	21.127*	35.632***	32.876***	21.373**	35.964***	33.259***
	(10.752)	(5.130)	(4.595)	(10.643)	(5.150)	(4.610)
Divided + Democrat	4.986	26.071	20.172	−0.019	22.768*	17.331
	(30.872)	(16.568)	(14.419)	(29.769)	(12.929)	(11.470)
Unified + Democrat	−11.192	19.244	16.352	−16.270	14.823	9.570
	(37.261)	(13.890)	(12.961)	(35.015)	(12.189)	(11.321)
Unified + Republican	33.655	−5.658	3.068	20.004	−16.859	−9.298
	(37.322)	(18.771)	(16.706)	(33.113)	(14.312)	(12.336)
Δ Competitiveness	−2.712	0.088	−0.371	−2.869	−1.527	−1.848
	(6.046)	(2.646)	(2.415)	(5.975)	(2.613)	(2.387)
Constant				78.747***	59.224***	65.082***
				(28.360)	(11.909)	(10.515)
Coincides Presidential	Y	N	Both	Y	N	Both
State FE	Y	Y	Y	N	N	N
Observations	272	1,258	1,530	272	1,258	1,530
Adjusted R^2	0.028	0.053	0.046	0.028	0.055	0.047
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 2.6: Estimations for Chow Test for differences in Presidential Elections

	Dependent variable:		
	Δ Expenditure PC	$\Delta TaxesPC$	$\Delta DebtPC$
	(1)	(2)	(3)
Election − 1 × Non-Concurrent	105.762***	30.495***	22.797
	(11.722)	(8.425)	(16.848)
Election × Non-Concurrent	80.600***	19.301**	82.545***
	(11.201)	(8.065)	(16.172)
Election + 1 ×Non-Concurrent	96.563***	14.566*	56.708***
	(11.257)	(8.108)	(16.267)
Election + 2 × Non-Concurrent	56.192***	37.915***	59.461***
	(11.558)	(8.309)	(16.625)
Election − 1 × Concurrent	103.783***	60.583***	95.427***
	(24.170)	(17.339)	(34.537)
Election × Concurrent	71.785***	42.273**	60.299*
	(23.809)	(17.091)	(34.082)
Election + 1 × Concurrent	98.261***	20.819	−14.146
	(23.838)	(17.110)	(34.117)
Election + 2× Concurrent	92.685***	46.562***	61.332*
	(24.170)	(17.336)	(34.529)
Δ Unemployment	31.316***	−60.930***	0.184
	(4.650)	(3.279)	(6.341)
Divided + Democrat	22.989**	−4.057	21.072
	(11.536)	(8.372)	(16.831)
Unified + Democrat	15.919	3.027	0.045
	(11.157)	(8.038)	(15.981)
Unified + Republican	−4.848	5.944	−4.492
	(12.631)	(9.240)	(18.789)
Δ Competitiveness	−2.183	1.998	−1.193
	(2.392)	(1.691)	(3.279)
Concurrent Presidential	Both	Both	Both
State FE	N	N	N
Observations	1,530	1,530	1,530
Adjusted R^2	0.045	0.190	0.006
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 2.7: Estimations - Changes in Expenditure by Category

	Dependent variable:					
	Δ Expenditure PC					
	Education	Public Welfare	Hospitals	Healthcare	Highways	Intergovernmental Exp
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	6.900	5.059	0.729	−1.452	1.315	11.858**
	(5.436)	(5.291)	(1.427)	(1.547)	(3.691)	(5.749)
Election	−3.089	−4.302	−0.235	0.080	3.649	−3.982
	(5.275)	(5.134)	(1.384)	(1.501)	(3.582)	(5.711)
Election + 1	8.014	3.040	1.665	0.490	2.829	4.347
	(5.278)	(5.137)	(1.385)	(1.502)	(3.584)	(5.594)
Δ Unemployment	−0.440	9.047***	1.503***	−0.034	−2.090	−4.063**
	(1.919)	(1.868)	(0.504)	(0.546)	(1.303)	(2.054)
Divided + Democrat	5.777	5.989	2.395	−1.251	−0.032	−3.442
	(6.023)	(5.861)	(1.580)	(1.714)	(4.089)	(6.431)
Unified + Democrat	0.022	6.460	0.562	1.107	−2.433	3.659
	(5.414)	(5.269)	(1.421)	(1.541)	(3.676)	(5.824)
Unified + Republican	7.204	−5.580	1.061	1.050	0.637	−3.152
	(6.978)	(6.791)	(1.831)	(1.986)	(4.738)	(7.435)
Years	1977-2011	1977-2011	1977-2011	1977-2011	1977-2011	1977-2011
Observations	1,530	1,530	1,530	1,530	1,530	1,530
, Adjusted R^2	0.010	0.041	0.009	0.013	0.018	0.009

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.8: Estimations - Changes in Tax Revenue by Period

	Dependent variable:					
	$\Delta TaxesPC$					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−6.976	10.558	1.806	−7.290	10.681	0.108
	(11.385)	(14.154)	(9.238)	(10.624)	(14.184)	(9.196)
Election	3.336	−38.103***	−10.398	2.261	−37.931***	−12.311
	(10.767)	(14.061)	(8.963)	(10.085)	(14.092)	(8.907)
Election + 1	−31.076***	−20.780	−17.910**	−30.475***	−21.779	−19.510**
	(11.084)	(13.665)	(8.969)	(10.579)	(13.682)	(8.909)
Δ Unemployment	−34.750***	−89.149***	−60.317***	−33.755***	−88.300***	−60.099***
	(4.103)	(5.018)	(3.261)	(4.087)	(5.014)	(3.266)
Divided + Democrat	−22.505	11.283	−6.292	−2.768	8.858	−1.148
	(15.227)	(17.827)	(10.233)	(7.156)	(14.043)	(8.333)
Unified + Democrat	7.519	24.970	7.040	8.802	16.171	2.837
	(11.807)	(16.934)	(9.199)	(6.931)	(14.846)	(8.166)
Unified + Republican	4.351	−11.683	8.575	−1.784	5.434	9.616
	(22.399)	(20.276)	(11.857)	(11.025)	(13.744)	(9.025)
Δ Competitiveness	3.742	2.151	3.055*	1.069	1.429	2.207
	(2.277)	(2.502)	(1.714)	(2.067)	(2.472)	(1.695)
Constant				28.031***	44.735***	33.595***
				(7.266)	(13.105)	(7.578)
Years	1977-1994	1995-2011	1977-2011	1977-1994	1995-2011	1977-2011
State FE	Y	Y	Y	N	N	N
Observations	765	765	1,530	765	765	1,530
Adjusted R^2	0.103	0.292	0.186	0.102	0.294	0.185

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.9: Estimations - Changes in Tax Revenue by TEL

	Dependent variable:					
	$\Delta TaxesPC$					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−3.176	4.258	1.806	−3.107	1.034	0.108
	(13.175)	(12.538)	(9.238)	(13.080)	(12.441)	(9.196)
Election	−25.272*	−2.763	−10.398	−24.611*	−8.085	−12.311
	(13.081)	(12.032)	(8.963)	(12.941)	(11.867)	(8.907)
Election + 1	−10.272	−24.479**	−17.910**	−11.391	−28.762**	−19.510**
	(12.918)	(12.080)	(8.969)	(12.778)	(11.968)	(8.909)
Δ Unemployment	−82.855***	−46.974***	−60.317***	−81.769***	−46.501***	−60.099***
	(4.695)	(4.449)	(3.261)	(4.650)	(4.454)	(3.266)
Divided + Democrat	−7.576	−7.808	−6.292	−13.242	9.963	−1.148
	(15.003)	(14.605)	(10.233)	(9.337)	(10.107)	(8.333)
Unified + Democrat	6.390	7.205	7.040	3.532	1.222	2.837
	(13.926)	(12.978)	(9.199)	(9.887)	(9.752)	(8.166)
Unified + Republican	−18.145	24.663	8.575	−12.746	27.736**	9.616
	(16.774)	(17.295)	(11.857)	(9.386)	(10.873)	(9.025)
Δ Competitiveness	3.075	2.797	3.055*	2.035	1.375	2.207
	(2.191)	(2.588)	(1.714)	(2.109)	(2.469)	(1.695)
Constant				36.636***	32.587***	33.595***
				(9.430)	(9.571)	(7.578)
TEL	Y	N	Both	Y	N	Both
State FE	Y	Y	Y	N	N	N
Observations	577	953	1,530	577	953	1,530
Adjusted R^2	0.352	0.112	0.186	0.358	0.117	0.185

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.10: Estimations - Changes in Tax Revenue by Concurrent with Presidential

	Dependent variable:					
	$\Delta TaxesPC$					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	37.024	−7.251	1.806	36.544	−7.660	0.108
	(24.481)	(9.929)	(9.238)	(24.270)	(9.956)	(9.196)
Election	20.526	−18.583*	−10.398	19.626	−19.048**	−12.311
	(24.379)	(9.596)	(8.963)	(24.171)	(9.619)	(8.907)
Election + 1	−3.877	−22.732**	−17.910**	−4.659	−22.413**	−19.510**
	(24.602)	(9.611)	(8.969)	(24.401)	(9.629)	(8.909)
Δ Unemployment	−58.285***	−60.571***	−60.317***	−58.122***	−60.267***	−60.099***
	(9.193)	(3.473)	(3.261)	(9.121)	(3.484)	(3.266)
Divided + Democrat	−35.507	2.146	−6.292	−35.499	5.834	−1.148
	(26.397)	(11.217)	(10.233)	(25.805)	(7.926)	(8.333)
Unified + Democrat	−15.501	9.497	7.040	−18.039	4.863	2.837
	(31.860)	(9.403)	(9.199)	(30.709)	(7.696)	(8.166)
Unified + Republican	24.944	0.598	8.575	24.550	−4.600	9.616
	(31.912)	(12.708)	(11.857)	(29.742)	(8.694)	(9.025)
Δ Competitiveness	1.727	2.935	3.055*	1.526	1.359	2.207
	(5.170)	(1.791)	(1.714)	(5.124)	(1.749)	(1.695)
Constant				33.935	36.589***	33.595***
				(27.343)	(7.696)	(7.578)
Coincides Presidential	Y	N	N	Y	N	N
State FE	Y	Y	Y	N	N	N
Observations	272	1,258	1,530	272	1,258	1,530
Adjusted R^2	0.160	0.199	0.186	0.164	0.199	0.185

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.11: Changes in Tax Revenue by Subsets

	Dependent variable:				
	General Sales	Selective Sales	License Taxes	Individual Income	Corporate Income
	(1)	(2)	(3)	(4)	(5)
Election − 1	−0.618	−0.817	−2.339**	−0.630	2.330
	(3.587)	(1.984)	(1.191)	(4.429)	(1.883)
Election	−4.925	3.011	−0.881	−8.425*	−1.034
	(3.475)	(1.925)	(1.156)	(4.306)	(1.829)
Election + 1	−5.122	−1.669	−4.112***	−0.517	−0.548
	(3.478)	(1.926)	(1.156)	(4.309)	(1.830)
Δ Unemployment	−16.665***	−6.390***	−1.928***	−26.001***	−10.451***
	(1.264)	(0.701)	(0.420)	(1.565)	(0.665)
Divided + Democrat	−6.947*	1.091	−0.042	−0.296	3.542*
	(3.974)	(2.198)	(1.319)	(4.914)	(2.089)
Unified + Democrat	5.493	0.253	2.020*	0.805	0.097
	(3.562)	(1.976)	(1.186)	(4.411)	(1.875)
Unified + Republican	−1.551	−0.474	0.914	3.093	2.912
	(4.604)	(2.547)	(1.529)	(5.710)	(2.423)
Δ Competitiveness	−0.302	0.639*	0.444**	1.349	0.726**
	(0.666)	(0.368)	(0.221)	(0.821)	(0.349)
Years	1977-2011	1977-2011	1977-2011	1977-2011	1977-2011
Observations	1,524	1,530	1,530	1,518	1,522
Adjusted R^2	0.111	0.056	0.027	0.159	0.141

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.12: Estimations - Changes in Public Debt by Period

	Dependent variable:					
	$\Delta DebtPC$					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	8.294	−78.653***	−35.776**	−0.439	−79.472***	−36.797**
	(24.678)	(26.297)	(17.784)	(23.940)	(26.269)	(17.709)
Election	18.246	6.158	9.450	10.312	5.647	8.173
	(23.339)	(26.125)	(17.256)	(22.693)	(26.099)	(17.163)
Election + 1	2.596	−48.810*	−23.663	−3.536	−49.519*	−24.937
	(24.025)	(25.389)	(17.267)	(23.526)	(25.333)	(17.168)
Δ Unemployment	−12.641	9.096	−0.267	−12.681	9.459	−0.080
	(8.893)	(9.324)	(6.279)	(8.848)	(9.281)	(6.279)
Divided + Democrat	5.290	10.722	10.715	8.209	−8.789	3.751
	(33.007)	(33.121)	(19.702)	(21.493)	(24.189)	(16.806)
Unified + Democrat	−4.416	−8.323	−11.321	−22.829	−3.756	−16.884
	(25.592)	(31.463)	(17.710)	(19.588)	(26.154)	(16.215)
Unified + Republican	3.498	−48.679	−22.552	13.589	−42.252*	−20.008
	(48.551)	(37.673)	(22.827)	(33.017)	(22.941)	(18.463)
Δ Competitiveness	−2.895	3.948	0.285	−3.438	1.751	−0.550
	(4.936)	(4.649)	(3.300)	(4.735)	(4.551)	(3.271)
Constant				54.066***	120.348***	83.325***
				(19.012)	(22.904)	(15.157)
Years	1977-1994	1995-2011	1977-2011	1977-1994	1995-2011	1977-2011
State FE	Y	Y	Y	N	N	N
Observations	765	765	1,530	765	765	1,530
Adjusted R^2	0.004	0.025	0.007	0.008	0.026	0.007

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.13: Estimations - Changes in Public Debt by TEL

	Dependent variable:					
	$\Delta DebtPC$					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−82.422***	−8.573	−35.776**	−83.584***	−8.949	−36.797**
	(28.439)	(22.839)	(17.784)	(28.284)	(22.487)	(17.709)
Election	−10.370	20.682	9.450	−12.145	21.516	8.173
	(28.236)	(21.915)	(17.256)	(28.072)	(21.457)	(17.163)
Election + 1	−43.776	−12.235	−23.663	−45.952*	−9.740	−24.937
	(27.884)	(22.004)	(17.267)	(27.708)	(21.641)	(17.168)
Δ Unemployment	21.327**	−14.095*	−0.267	21.304**	−12.883	−0.080
	(10.135)	(8.104)	(6.279)	(10.034)	(8.039)	(6.279)
Divided + Democrat	32.876	12.814	10.715	25.777	−4.671	3.751
	(32.385)	(26.604)	(19.702)	(29.311)	(19.058)	(16.806)
Unified + Democrat	−8.990	−17.502	−11.321	−14.084	−16.733	−16.884
	(30.060)	(23.639)	(17.710)	(28.294)	(18.371)	(16.215)
Unified + Republican	−36.109	−4.427	−22.552	−36.006	−4.494	−20.008
	(36.209)	(31.502)	(22.827)	(31.277)	(20.877)	(18.463)
Δ Competitiveness	−0.576	0.576	0.285	−0.169	−2.034	−0.550
	(4.730)	(4.714)	(3.300)	(4.670)	(4.489)	(3.271)
Constant				111.993***	63.286***	83.325***
				(27.435)	(17.737)	(15.157)
TEL	Y	N	Both	Y	N	Both
State FE	Y	Y	Y	N	N	N
Observations	577	953	1,530	577	953	1,530
Adjusted R^2	0.030	0.007	0.007	0.033	0.006	0.007

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.14: Estimations - Changes in Public Debt by Concurrent with Presidential

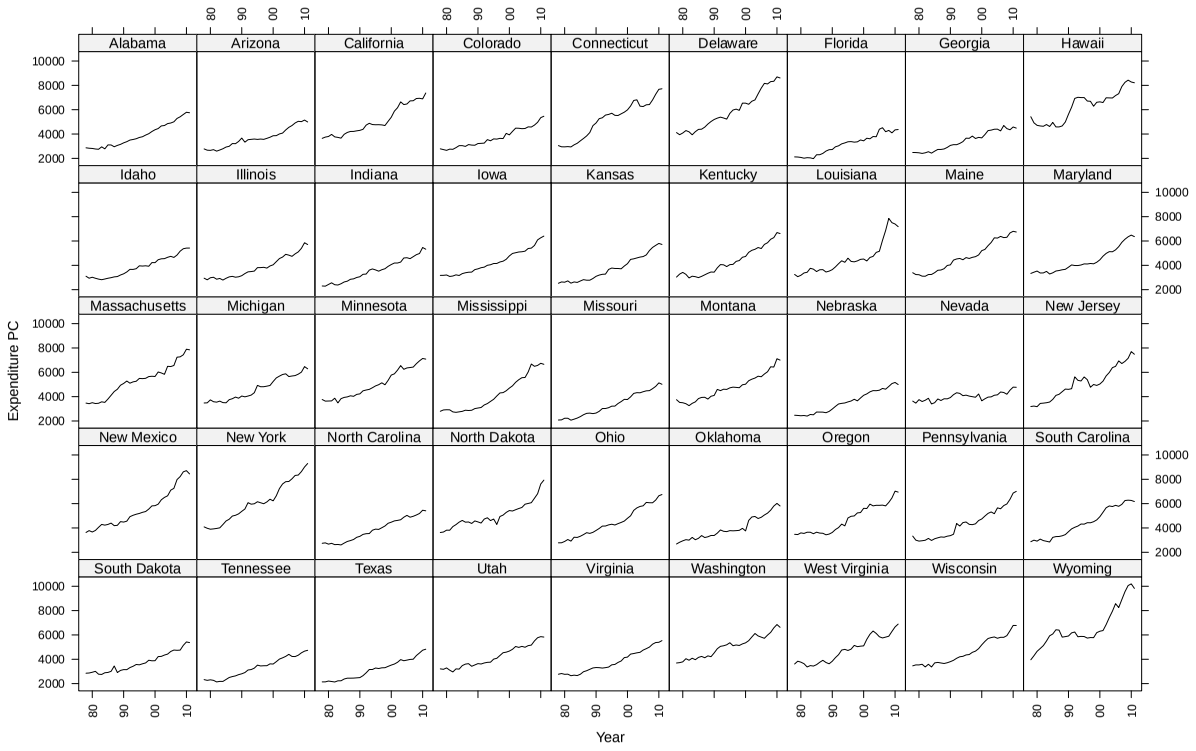
	Dependent variable:					
	$\Delta DebtPC$					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	5.071	−38.173**	−35.776**	3.016	−38.081**	−36.797**
	(51.679)	(18.453)	(17.784)	(51.009)	(18.456)	(17.709)
Election	−38.992	21.419	9.450	−40.305	21.247	8.173
	(51.464)	(17.833)	(17.256)	(50.793)	(17.834)	(17.163)
Election + 1	−119.263**	−5.582	−23.663	−120.327**	−5.506	−24.937
	(51.936)	(17.862)	(17.267)	(51.297)	(17.860)	(17.168)
Δ Unemployment	31.932	−3.584	−0.267	31.813*	−3.431	−0.080
	(19.407)	(6.455)	(6.279)	(19.180)	(6.456)	(6.279)
Divided + Democrat	−39.376	19.808	10.715	−29.655	7.660	3.751
	(55.725)	(20.846)	(19.702)	(53.385)	(18.123)	(16.806)
Unified + Democrat	−53.888	−7.043	−11.321	−57.297	−11.129	−16.884
	(67.257)	(17.476)	(17.710)	(62.472)	(16.344)	(16.215)
Unified + Republican	−85.722	−9.891	−22.552	−62.853	−15.014	−20.008
	(67.368)	(23.617)	(22.827)	(58.523)	(20.276)	(18.463)
Δ Competitiveness	−15.015	2.597	0.285	−14.955	1.884	−0.550
	(10.913)	(3.329)	(3.300)	(10.764)	(3.305)	(3.271)
Constant				141.109***	70.195***	83.325***
				(49.618)	(16.261)	(15.157)
Coincides Presidential	Y	N	N	Y	N	N
State FE	Y	Y	Y	N	N	N
Observations	272	1,258	1,530	272	1,258	1,530
Adjusted R^2	0.040	0.011	0.007	0.040	0.011	0.007

Note: *p<0.1; **p<0.05; ***p<0.01

2.8 Appendix

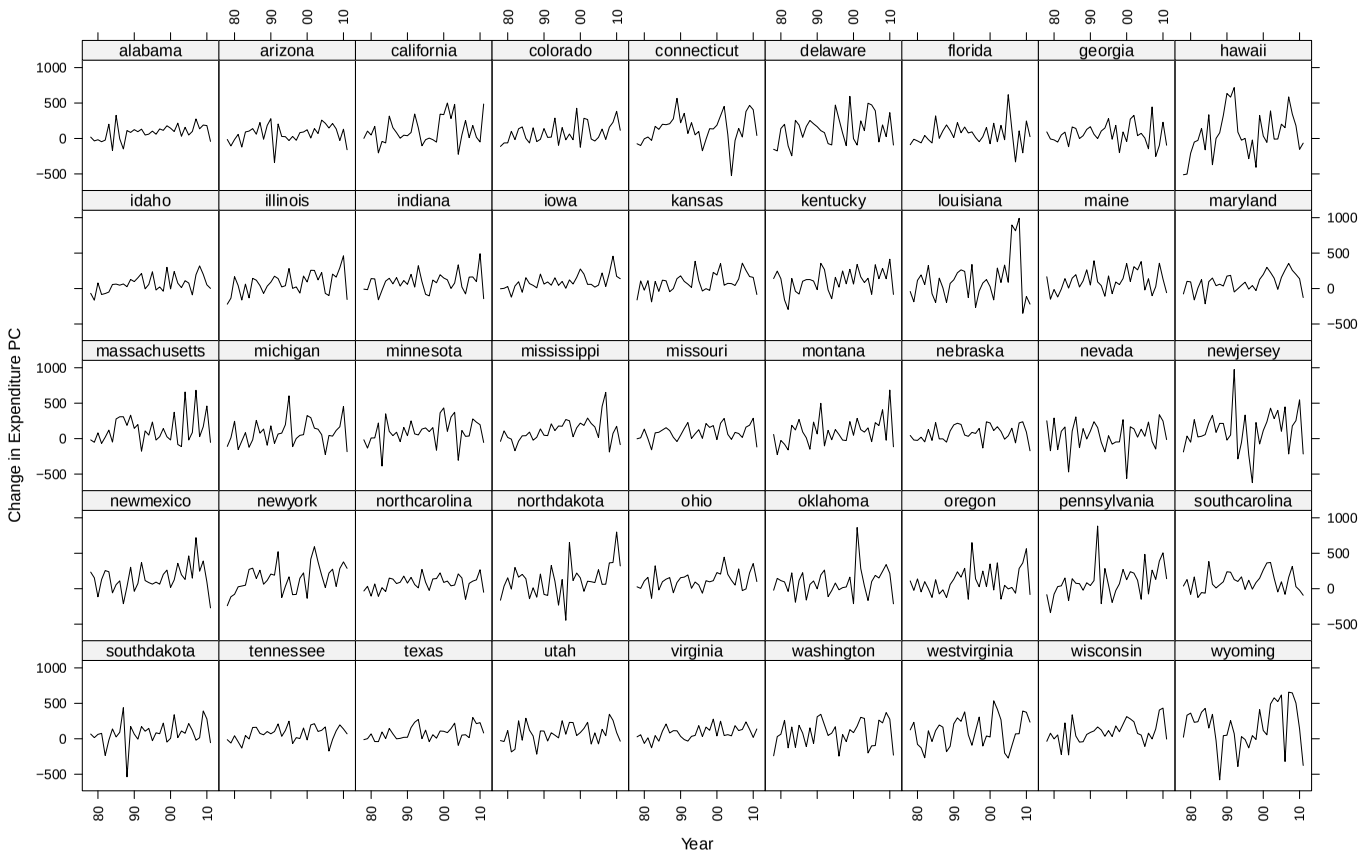
Evolution of Public Expenditure Per Capita 1977-2011

Figure 2.1: Evolution of Public Expenditure



Evolution of Changes in Public Expenditure Per Capita 1977-2011

Figure 2.2: Evolution of Changes in Public Expenditure



Evolution of Tax Revenue Per Capita 1977-2011

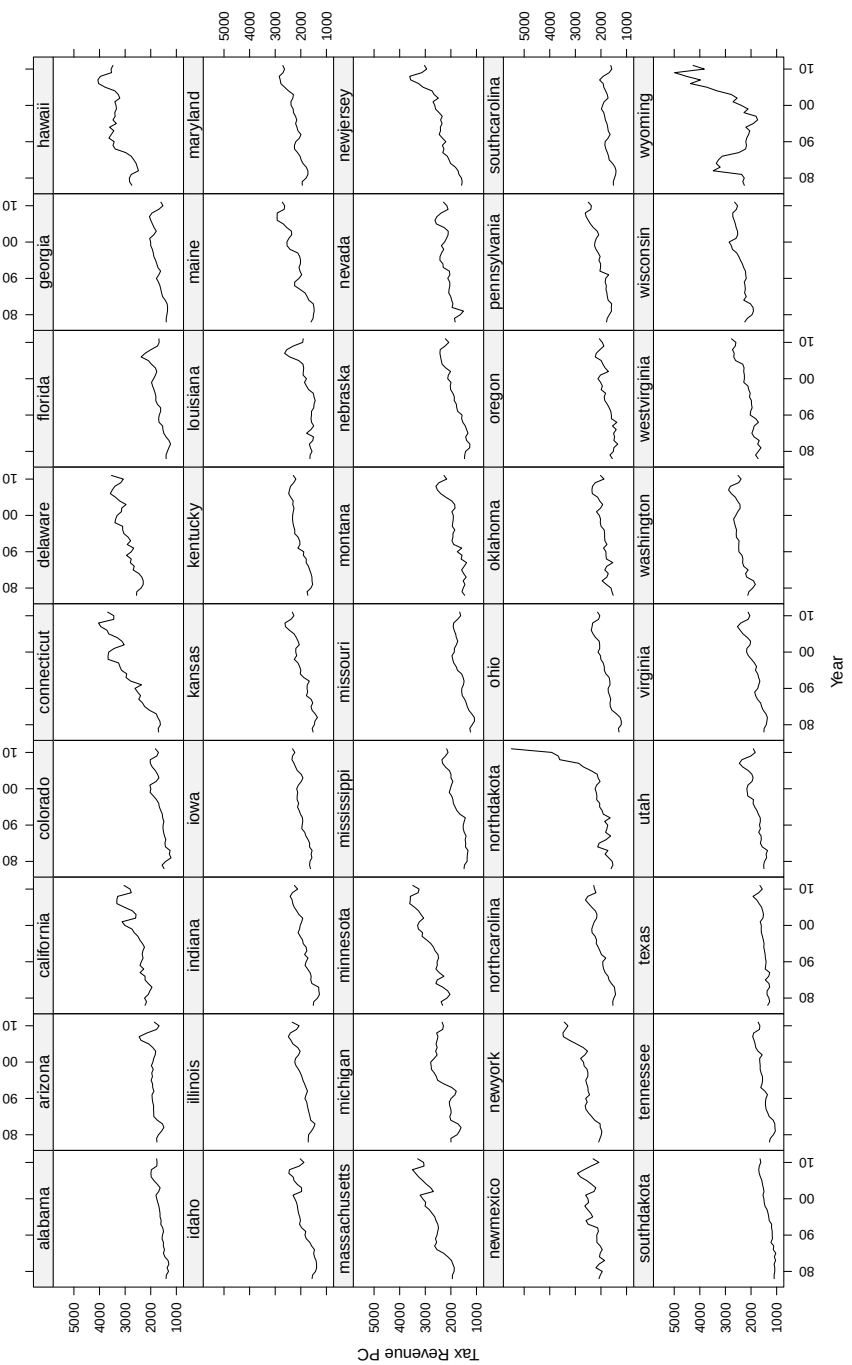


Figure 2.3: Evolution of Tax Revenue

Evolution of Changes in Tax Revenue Per Capita 1977-2011

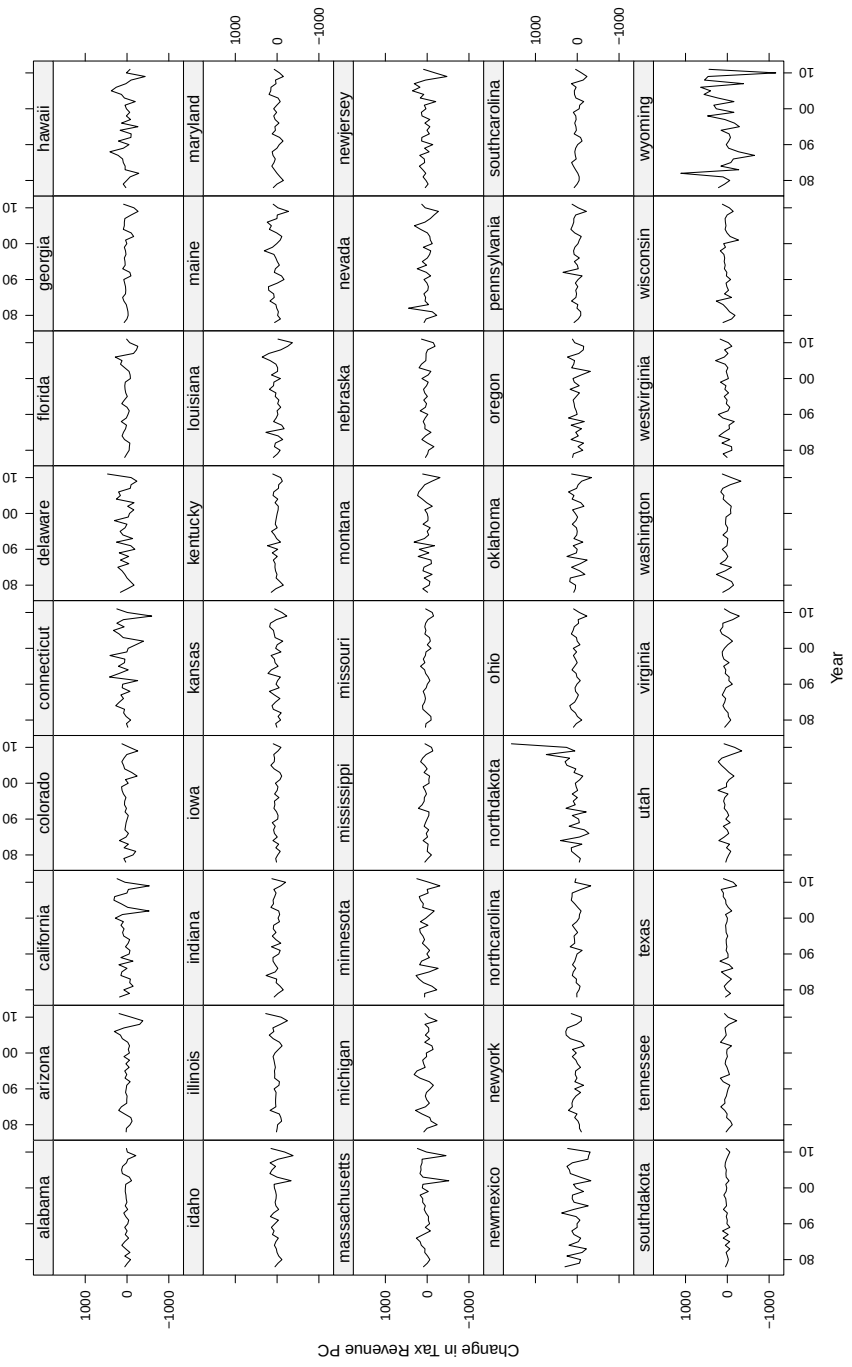
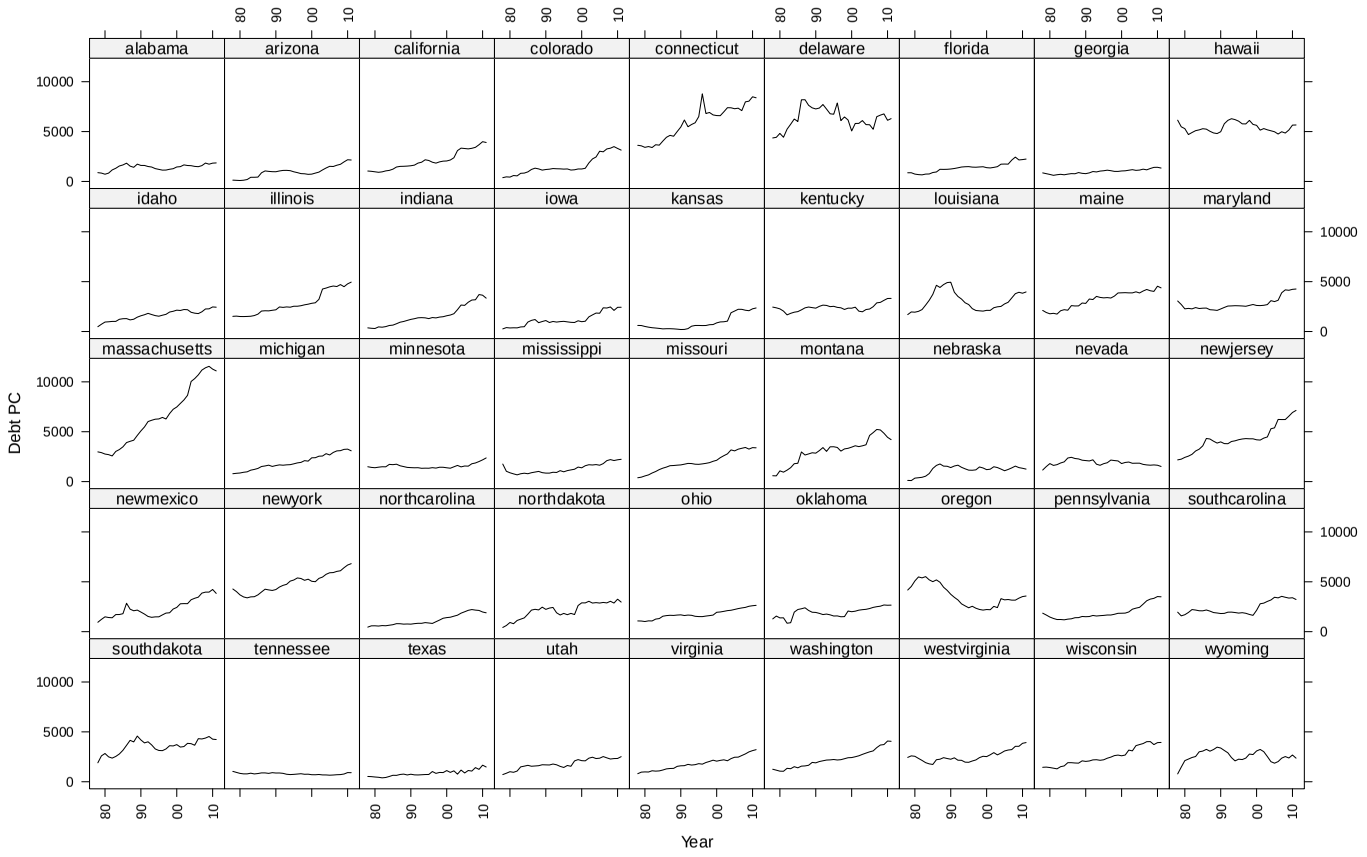


Figure 2.4: Evolution of Changes in Tax Revenue

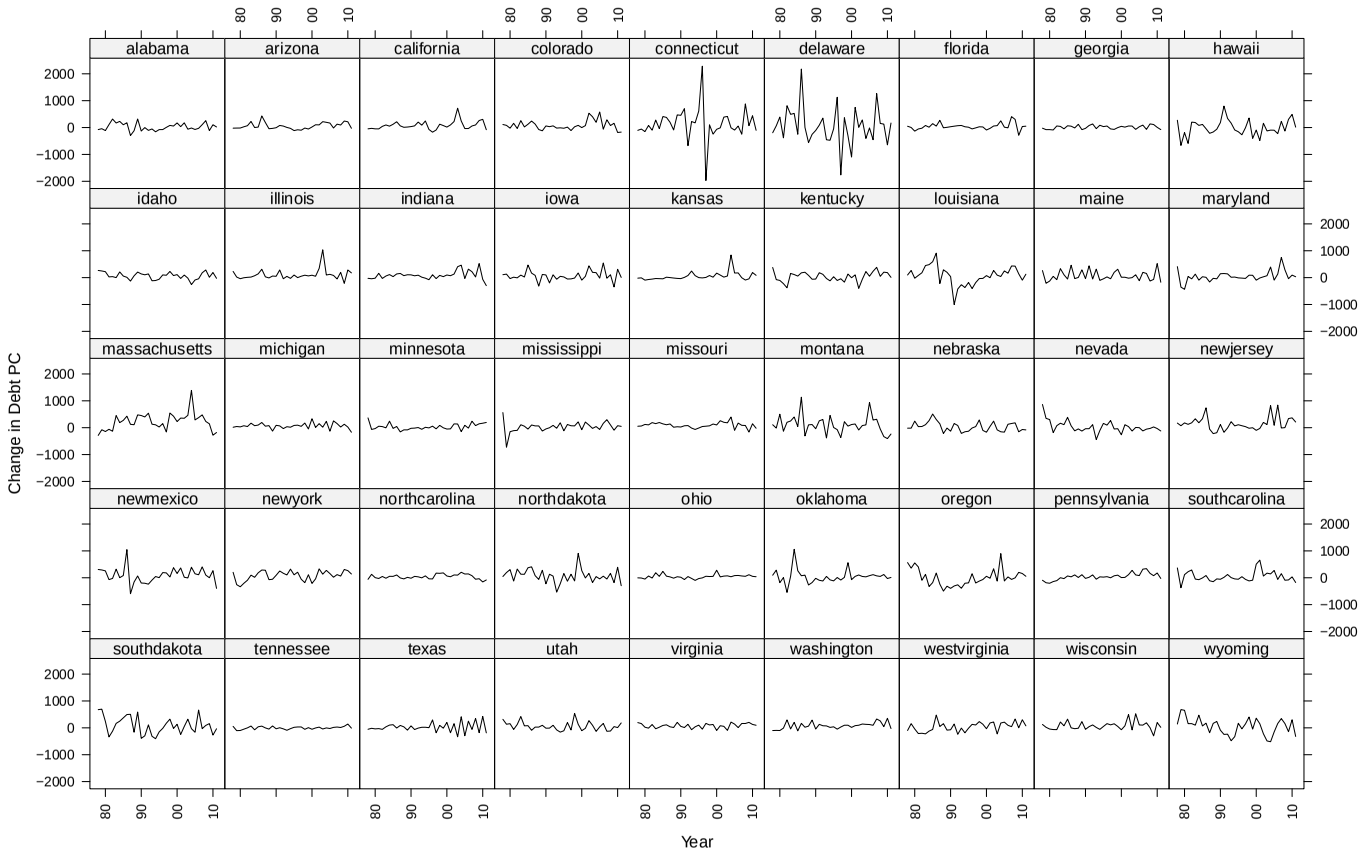
Evolution of Public Debt Per Capita 1977-2011

Figure 2.5: Evolution of Public Debt



Evolution of Changes in Public Debt Per Capita 1977-2011

Figure 2.6: Evolution of Changes in Public Debt



Chapter 3

POLITICAL PENSION CONTRIBUTION CYCLES: EVIDENCE FROM US STATES

3.1 Abstract

I extend the literature of political budget cycles at the subnational level to include pension funding. I explore the relationship between state pension funding and gubernatorial elections in the United States for the years 2001-2014. I show that it is more likely to observe undercontributions to pension funds during the pre-electoral year. I also found changes in the percentage of the pension funds invested in equity as a function of the electoral calendar, where decisions to invest in more conservative assets (i.e. bonds) are made in non-electoral years.

3.2 Introduction

> "At the bottom of it all is a political culture that rewarded politicians who made unsustainable promises (...) People enjoy the pleasure of getting stuff without the pain of paying for it and there is always a strong temptation in state and local government to imbalance spending and taxation and use the pension fund to hide debt. The bill doesn't come due until well after the legislators who wrote the check have left office..."
>
> -The Providence Journal (2011), as quoted by McGuinn (2014)

Subnational public pension sustainability in the US is a highly discussed and fairly controversial issue. While Munell (2012) argues that not all state and local systems are in a critical situation, the estimated aggregated unfunded liabilities in 2014 accrued to roughly $1.1 trillion, while total projected liabilities amount up to $4.3 trillion (Munell and Aubry, 2015). Kiewiet and McCubbins (2014) detail the problem further: there are now close to nine million people receiving retirement

benefits from state and local government employers, and this number will double by 2030.

Calculating pension underfunding is difficult. It requires incorporating many variables and making several important assumptions. As Kiewiet (2010) summarizes, in order to calculate underfunding we need to know (1) how many covered employees are going to be retiring over time; (2) the cost of the benefits they have been promised; (3) how long they and their covered dependents are going to live; (4) the amount of money that is going to be contributed into the fund by current and future employees, and (5) the fund's return on investment of assets. Despite these difficulties most estimates agree that this is a sizable problem. As Brown, Clark, and Rauh (2011) put it, "Although there is disagreement between economists and plan administrators on the size of the unfunded liabilities, everyone agrees that state and local pensions in the US have assets that are substantially below the present value of the promises that have been made to public sector workers and retirees". The problem is particularly egregious given the fact that these workers have already fulfilled their obligations and are now in a vulnerable position (Kiewiet, 2010).

Despite the magnitude of the problem, its full consequences will be felt most acutely in the future. The decision of repudiating pension-related liabilities due to financial constraints will most likely be faced by future generations. As stated by Thom and Randazzo (2015), "even pension funds with significant unfunded liabilities are rarely at risk of defaulting on benefit payments in the short term". Actions should be taken now to identify sources of further deterioration of state and local pensions' sustainability and prevent them, since we are still allegedly far away from defaulting on these benefit payments.

Several efforts to explain pension underfunding have already been done. One area of research involves studying how fiscal stress contributes to pension underfunding.

Mitchell and Smith (1994) find that fiscal pressure appears to cause some public employers to reduce their annual contributions below the required level. Eaton and Nosfinger (2004) find that when government sponsors of public pension plans experience tight fiscal constraints, they manipulate the plans' actuarial assumptions to lower their required contribution; additionally, they find these plans have a greater propensity for underfunding than plans from less fiscally constrained states. Thom (2013) also finds a negative relationship between pension funding and outstanding state debt. The complete opposite conclusion is reached by Triest and Zhao (2013), who find that plan sponsors do not reduce their contributions in response to negative fiscal or economic shocks.

Another area of research relates to issues with workers' demographic forecasting and accounting standards. Mitchell and Smith (1994) indicate that in the late 1980s there were no instances of egregious misuse of actuarial and economic assumptions for the purpose of reducing employers' pension fund contributions. Meanwhile, using CalPERS[1] as an example, Sabin (2015) found that employees retired earlier than expected, wages increased faster, and people lived longer, which led the State of California to greatly underestimate the accurate contributions.

Another relevant subject is the government level in charge of pension fund administration, as discussed by Munell and Aubry (2015), who found that state administered plans are not only bigger with respect to local systems (they receive 58% of sponsor pension contributions), but also have a better administration, since despite having smaller payments with respect to their funding needs, their asset levels have higher returns.

The relationship between labor unions and pension funding has also been studied. Mitchell and Smith (1994) show that growth in employee unionization reduces the rate at which public sector employers actually contribute what they are required each

[1] California Public Employees' Retirement System

year. Kiewiet (2010) finds that the strength of labor unions is associated with higher wages, but not so with public pension underfunding. Recently, Frandsen (2016) and Frandsen and Webb (2017) analyzed the effect of organized union power, finding that collective bargaining rights resulted in higher benefits for public employees, which vary according to the type of job performed, and in turn generated bigger contributions towards pension plans.

Partisanship and its role in the funding of state pensions has also been explored, but with mixed conclusions. Anzia and Moe (2013) find that, during favorable economic times, state legislatures tended to increase the generosity of public pensions, and voting patterns were heavily bipartisan, but, after the recession, union financial support and legislator conservatism (both correlated with partisanship) became stronger predictors of pension votes. Elder and Wagner (2015) argue that partisan polarization and electoral uncertainty lead to greater underfunding. Thom (2013) finds a significant relationship between pension funding and legislative partisanship and citizen ideology. Partisanship has also been related to pension reform. Thom (2013) finds that Republican legislative, but not executive, partisanship is a strong predictor of pension reform.

Until my contribution, the electoral cycle has been conspicuously absent from these conversations even though it has been proven to be a determinant for local governments' spending patterns (Rose, 2006). My analysis contributes to several areas of research in the current literature. The main contribution is to relate pensions to the subnational budget shocks caused by electoral cycles. To the best of my knowledge, this is the first time an empirical study shows pension-related adjustments being motivated by the political cycle.

Regarding political budget cycles at the state and local level in the US, there is evidence that during election years states' public finances change as a function of

the electoral calendar. Melchior et al. (1993) analyzed the local level in urban areas for the period 1978-1985 and found that there was a disproportionate tendency for mayor-councils to have the lowest tax increase in the fiscal year of an election. Alt and Rose (2007) investigated factors which are associated with an increase in expenditure in electoral periods. State public finance forecasts are also affected by the electoral calendar, as argued by Boylan (2008), who measured higher deficit in those periods. There were no relevant effects on forecasted deficits, which means that they undergo midyear tax increase or expenditure cuts. It should be noted that the measure of debt of the political budget cycles is usually based on cash flow instead of accrual accounting (Irwin, 2015). This could contribute to the apparent lack of effect on deficits forecast. However, observing the underpayment of pension funds goes one step towards the accrual accounting of political budget cycles.

One argument against the short-term use of budget dynamics for electoral purposes is the negative impact it could have on citizens, who in turn would penalize this course of action. While local fiscal multipliers are low and not statistically significant, as shown by Clemens and Miran (2012), using the upper bound on the range of estimates, behaviors such as pro-ciclicality in expenditures could account for roughly 4% of state-level income variation, which is not negligible. Therefore, a state administration affected by this kind of electoral-induced distortion in public spending would like to reduce the immediate negative impact on voters' welfare.

State contributions to local public pensions are an "ideal" expense to be cut in such an environment. Splinter (2017) concludes that states cut their pension contributions eight times more than other spending in response to fiscal stress. This relevant reduction is derived from the fact that voters usually underestimate the long-term impact of a poor pension system and place more value on the benefits derived from increased public expenditures with immediate impact.

I connect these branches of the literature by showing that electoral fiscal stress is translated in reductions to pension contributions. Voter shortsightedness regarding pension liabilities could not only be derived from impatience, but also from the fact that the impacts of such contingencies are hard to measure. As I mentioned before, actuarial studies depend on a series of variables, both current and forecasted, which include demographic information about employees, wages, and pension financial asset returns, among other factors.

In addition to regular issues with pension liability valuation, qualitative studies show that it is not uncommon for state governments to take actions that change the main parameters in ways to contribute less (or exaggerate financial sustainability). Minahan (2014) talks about "social opacity" in social security liability valuation, stating that "in the public pension world, red herrings play a central role in preventing open communication", to the extent that when the author "raise[d] the topic of economic value of liabilities to a plan sponsor or actuary, and even some investment professionals, he would encounter these responses as ways of dismissing the relevance of economics to pension liability valuation". Such conduct was also documented through a panel analysis by Eaton and Nofsinger (2004), who expose that when states experience tight fiscal constraints, they manipulate the plans' actuarial assumptions to lower their required contribution. My investigation also contributes to the literature by providing empirical support of this practical use of valuation parameters to reduce political budget cycle stress.

While investment decisions about pension funds are usually taken by a board of trustees, this body is not free of political pressures or conflicts of interest, as documented by Dobra and Lubich (2013). They provide evidence showing that pension board composition, in addition to influencing asset allocation, may also have an effect on portfolio risk; investment decisions vary depending on whether the board consists of active, retired, or ex-officio members. In principle, these types of port-

folio allocation decisions could also be related to the political calendar. Eaton and Nosfinger (2004) find further evidence that plans subject to political pressure are also more likely to have optimistic accounting assumptions and to be more underfunded than those plans not facing political pressure.

Most retirement systems have guidelines that regulate the allocation of investment in diverse types of assets such as equity (international and domestic), bonds and real estate, to name just a few. I reviewed the investment policies of the seven biggest retirement systems (California Public Employees' Retirement System [CalPERS], California State Teachers' Retirement System [CalSTRS], Florida Retirement System [FRS], Teacher Retirement System of Texas [TRS], New York State Teacher's Retirement System [NYSTRS], School Employees' Retirement System of Ohio [OHSERS], and Ohio Public Employees Retirement System [OPERS]), and found that the potential share of the overall investment allotted in equity ranges between 45% to 65%, which gives a great deal of freedom to allocate resources, should it be needed. The investment portfolio allocations of the aforementioned pension system can be found starting on Table 3.9.

Furthermore, even with the assistance of investment advisors and experts, operating within different system-specific investment guidelines, there is room for biased selection of asset allocation: all reviewed policies coincide in the fact that the board of trustees have the ultimate decision on where to invest the assets. Members of a retirement system's board usually represent workers, retirees, the governor, and some offices of the three branches of local government. Clearly, an administration that wants to manipulate the expected required contribution by changing investment return expectations can do so through their influence with the board. This is easily attainable in a case such as Texas TRS, where all the board members are ultimately appointed by the governor. While there are cases where the local executive is less represented as in OPERS, where only 1 member is appointed by the governor, there

is certainly room for influence nonetheless. For a more detailed discussion on the subject of the political dynamics within boards of trustees, view Dobra and Lubich (2013).

3.3 Hypotheses and Model

In this chapter, I focus on two factors regarding pension funding. The first is the percentage of the Annual Required Contribution (ARC),[2] paid by employers and the state government. My first hypothesis is the following: 1) I expect an undercontribution to pension funds during pre-electoral years. This hypothesis follows the same logic of increasing public debt during the pre-electoral year: it is costly to increase revenue to fund pensions during electoral years as well as making budgetary adjustments. The second hypothesis is related to the distortion in contingent liabilities valuation through increases in asset returns (thus it also deals with how the ARC is calculated in first place): the percentage of the fund assets that are invested in equity. In general, returns from equity are assumed to be higher than returns from safer assets such as bonds. From this, and the studies detailed in the past section, I deduce my second hypothesis. 2) It is more likely to observe increases in the percentage of the fund invested in equity during pre-electoral years. Analogously, if there is a decision to reduce the percentage of the fund invested in equity, it would be done so during non-electoral years.

To test the hypotheses I use models estimated from panel data that include changes in the percentage of ARC contributed every year by employer and the state, changes in percentage of the fund invested in equity, changes in state unemployment, changes in state revenue, changes in union membership, the annual returns on investments in the S&P index (current and with a lag), the annual return on the 10-year treasury

[2]The Annual Required Contribution is the one which would cover the cost of benefits accruing in the current year and payment to amortize the plan's unfunded actuarial liability (Munell, 2012).

bond (current and with a lag), and partisanship of the governor of the state. I use the following reduced form specifications:

$$\Delta Y_t = \alpha + \sum_{k=1}^{t} \beta_k \cdot electoralyear_{k,t} + \gamma X_t + u_t$$

$$\Delta Y_t = \alpha + \sum_{k=1}^{t} \beta_k \cdot electoralyear_{k,t} + \sum_{k=1}^{t} \delta_k \cdot electoralyear_{k,t} \cdot partisanship + \gamma X_t + u_t$$

where ΔY_t is the change in percentage points of the variable of interest in year t with respect to the same variable in year $t-1$. The variable of interest is either percentual change in *ARC* contributed or the share of fund assets invested in equity. Variables $electoralyear_{k,t}$ are a series of dummies which indicate the year in the electoral cycle. I include the year before the elections, the electoral year itself, and the year after the elections, being the year in the middle of the cycle the omitted category. X_t is the matrix of covariates and u_t the residual. The parameters of interest are β_k and δ_k. I include state fixed effects when I do not include partisanship as a covariate. Unfortunately, to avoid collinearity, I have to remove the state fixed effects in order to incorporate the variable partisanship, since the majority of states did not experience a party change during the analyzed period.

3.4 Data

Variables related to pension plans were incorporated from the Public Plans Database, built, maintained, and published by the Center for Retirement Research at Boston College, Center for State and Local Government Excellence, and National Association of State Retirement Administrators (2017). I use 14 years in my sample, from

2001 to 2014;[3] I include 154 state and local pension systems in the US and use both local and state administered plans for 48 states. I removed from the analysis the plans of New Hampshire and Vermont, since their gubernatorial elections are done every two years as opposed to four years. The electoral variables *timing of the election* and *party of the governor* were coded using information from David Leip's US Atlas of elections (2017). The public finance variables incorporated in the estimations as controls were downloaded from the U.S. Census Bureau (2016). Data for unemployment at the state level was downloaded from the U.S. Bureau of Labor Statistics (2017). Annual return on investments on the S&P and 10-year treasury bond were calculated by Damodaran (2017). Data from labor unions consists on the percentage of public sector workers unionized by state, this data comes from Unionstats.com by Hirsch and Macpherson (2017). The distribution of the variables used in the analysis can be found in Table 3.8.

Regarding ARC paid, states have evolved differently in recent years (see Table 3.11). Some states such as Alabama, Arizona, Rhode Island, South Carolina, and Utah, exhibit a very stable pattern of paying the full ARC during the analyzed period. Other states such as Alaska, Massachusetts, Nevada, and New York have been increasing in recent years the percentage of ARC they have been contributing. Unfortunately, there are also states with a negative trend of ARC contributions, such as Illinois, Indiana, Minnesota, Montana, New Jersey, Pennsylvania, Virginia, and West Virginia. The distribution of changes in ARC at the state level can be found in Figure 3.1. As we can see, there are many observations in which the contribution matched exactly the ARC. There is also an important number of cases in which the ARC was not met. Contributions larger than the ARC are scarce.

(Figure 3.1 found at the end of this chapter)

[3] At the time this document was written there was only partial information for the year 2015.

Regarding changes in ARC contributed (first differences of percentage ARC contributed), we can see in Figure 3.11 that some states experience larger changes in ARC contributed than others, but there is no clear trend for most states. The distribution of changes in contributions as percentage of ARC can be found in Figure 3.2, where it can be seen that (save from the mass of observations exactly at zero) the data distribution is very well-behaved and symmetric around zero.

(Figure 3.2 found at the end of this chapter)

It is important to mention that even when contributing the full ARC, the pension fund could be underfunded. The ARC is calculated using many assumptions which could not be accurate. However, there is evidence that higher ARC contributions are associated with better pension funding. Munell (2012) finds that making the full ARC contribution increases the funded ratio by 6.8%. Using my sample, I confirm the positive correlation between both. I calculate the correlation between the average ARC contribution at the state level for 2001-2014 and the estimated funding ratio for 2014 (See Tables 3.6 and 3.7). This correlation is 0.47, which is sizable considering I am only incorporating 14 years of ARC contributions.

My second variable of interest is the percentage of the fund invested in equity. States have evolved similarly with respect to the trend of equity as percentage of pension funds. As we can observe in the Figure 3.11, the vast majority of states have decreased the share of their funds invested in equity. This share declined during the financial crisis, but even in recent years the trend continues to be negative. The distribution of these percentages can be found in Figure 3.3. The mean and median of the distribution of the fraction of funds invested in equity is around 55%. The tail is heavier towards higher levels of equity.

(Figure 3.3 found at the end of this chapter)

When we observe annual changes in the share of equity the trend disappears (see Figure 3.11). The only important pattern I am able to identify is the sudden drop during the financial crisis (2007-2008). However, there is not an abnormal mass in the distribution of changes in ARC, as this can be seen in Figure 3.4. The distribution of changes is centered around zero, as most states do not experience important changes in the investment profile of their pension funds form one year to another. However, there are enough observations in the tails such that I am able to estimate the changes in this profile as a function of the electoral calendar.

(Figure 3.4 found at the end of this chapter)

3.5 Empirical Analysis

Plan vs State level analysis

I identified two possible ways of performing the analysis of political pension contribution cycles: at the state, and at the plan level. I focus on the analysis using data aggregated by state. Analysis using plan level data can be found in the appendix. Working at the plan level has the advantage of having much more observations for the analysis (close to 2,000 as opposed to around 600 in state level), and the unobserved invariant characteristics of the unit of observation (fixed effects) are much more specific at the plan level as opposed to the state. Rules, governance, membership, and benefits are better defined at the plan in comparison to the state level. However, working at the plan level also has disadvantages. One of these is that there is large disparity between the number of plans each state has. For example, California has fifteen plans in the sample, Illinois and Texas eight, and New York and Minnesota seven. Meanwhile, there are six states with only one plan (see Table 3.5). By making estimates using plan level data as is, I would be biasing the results towards states that have more plans. Another disadvantage of using plan level data

is that, in principle, there are more budgetary incentives to under fund larger plans, since small plans within the state exert much less pressure in the public finances. In order to eliminate the disadvantages of working with plan level data, I aggregated all plans within the state in order to have one observation per state per year.

Analysis at the State Level

In order to perform the analysis at the state level, it is necessary to aggregate all individual plans in a state into one observation. To do this, I calculated the state's total employer contributions to the state plans as the sum of all contributions from the employers and state government to each individual plan in the state. I also calculated the total required contribution of the state as the sum of required contributions of all plans. Finally, the state's percentage ARC paid as the state's total employer contributions over the state's required contribution.

ARC

My estimations of the changes in percentage ARC contributed as the function of the electoral calendar can be found in Table 3.1. As we can see in column (1) during the year preceding the election there is a decrease on average of 8.7 percentage points of the ARC. During all other years of the electoral calendar we observe similar levels of contributions of ARC. This result is robust to including the controls (see column 2). In column (3) we can observe that this pattern of decreasing the ARC in the pre-electoral year is more prevalent when we have a Democrat governor. I do not include state fixed effects when I incorporate partisanship of the governor columns (2) and (3) since the vast majority of states did not experience party change in the analyzed period. Other controls do not reach statistical significance in my estimations.

(Table 3.1 found at the end of this chapter)

Using changes in the percentage ARC paid has the advantage of estimating the magnitude of the pension under-contribution during pre-electoral years. However, it has the disadvantage of overweighting outliers in the estimates. In order to account for this, I propose an alternative specification of the model using a dichotomous dependent variable in my estimations. Following the definition of Thom and Randazzo (2015) I define a dichotomous variable "$CompleteARC_t$" as 1 if the employers contributed at least 95% of the required ARC in the fiscal year t, and 0 otherwise. I use two different models to estimate the probability of contributing the *Complete ARC*: a linear probability model, and a conditional logit model. The conditional logit model allows us to incorporate state fixed effects. The results of this alternative setup of the model can be found in Table 3.2. The results of the previous estimation are consistent with the results presented in Table 3.1. I am able to see that it is less likely to observe a complete ARC contribution in the pre-electoral year. I estimate this decrease in probability in 9.3% for the linear model (see column 1). This estimation is robust to including changes in unemployment, state revenue, and partisanship of the governor (column 2). As we saw in the previous results, it is more likely to observe the decrease in contribution in states which have a Democrat governor, although it does not reach statistical significance. These results are consistent when I used the conditional logit model (See Table 3.2 columns 4-6). As for the controls, increases in union membership make it less likely to contribute the full ARC, which goes in the expected direction, but it is not robust when incorporating other controls. Investment return variables go in the opposite direction I expected. Changes in unemployment and revenue do not reach statistical significance.

(Table 3.2 found at the end of this chapter)

Investment in Equities

As I mentioned before, percentage ARC contributed is a direct way to measure the political pension contribution cycle. However, there are factors in the specification of pension's funding requirements that can be modified in order to change this contribution. One of these is the percentage of funds that are invested in equity. Equities have higher return assumptions than bonds, but incur greater risk. In this section I investigate whether the electoral calendar is associated with changes in the composition of the pension fund portfolio measured with changes in the percentage invested in equity. I have to be careful about how I incorporate this percentage of equity in the estimation, since market movements change the value of equity, and therefore its share of the value of the pension fund. To mitigate this problem, I will only focus on relatively large equity changes. For this, I define two dichotomous variables: $DecreaseEquity_t$ as 1 if the state's percentage invested in equity in year t was 5 percentage points smaller than in period $t-1$; analogously I define the variable $IncreaseEquity_{it}$ as 1 if the state's percentage invested in equity in year t was 5 percentage points greater than in period $t-1$. The results of my estimations can be found in Tables 3.3 and 3.4.

(Table 3.3 found at the end of this chapter)

Regarding the decrease in equity (Table 3.3), I observe that all the regression coefficients for the electoral calendar are negative. Recall that the omitted category in the analysis is the second year after the election, that is, in the middle of the electoral cycle. Therefore, we can conclude that during this year we observe a decrease in percentage invested in equity. Reducing the share of equity can be interpreted as taking a "safe" investment choice. The expected returns of the plan decrease, and therefore the required contributions should be larger. What my

estimates suggest is that conservative investment decisions are more likely to be observed in non-electoral years. These results are robust to the addition of the control variables. The direction of these controls are consistent to what I expected: when the economy is having problems (reflected in high unemployment, decrease in revenue, lower investment returns), the equity as percentage of the pension fund decreases. Partisanship does not appear to be correlated with large decreases in equity.

(Table 3.4 found at the end of this chapter)

For large increases in equity (Table 3.4), partisanship becomes very important in order to interpret the political pension contribution cycle. When I do not directly incorporate partisanship into the analysis of the cycle I get results that are not expected. In columns (1), (2), (4), and (5), we observe that it is during the post-electoral year when we observe increases in the percentage of equity in the pension fund. However, when we incorporate partisanship the results completely change. In columns (3) and (6) we are able to observe that for states which have Democrat governors, the share of equity of the pension fund significantly increases during the pre-electoral year. I am not able to identify any other large increase in equity during other periods of the electoral cycle. This means that for states with Democratic governors, there is an increase likelihood to observe a change towards a more risky portfolio during the pre-electoral year. As for the controls, the variables related to the economy do not appear to be associated with large increases in equity. Meanwhile, the strength of labor unions appear to be natively correlated. States with stronger labor unions are less likely to greatly increase investments in equity from one year to another.

3.6 Conclusions, Policy Implications, and Further Research

In this chapter I extend the literature of the political budget cycles at the subnational level to include pension funding. I explore the relationship between state pension funding and gubernatorial elections in the United States for the period 2001-2014. I show that it is more likely to observe undercontributions to pension funds during pre-electoral years. I estimate a decrease of 8.7% of the ARC paid to state pension funds during the gubernatorial pre-electoral year compared to other years in the electoral cycle.

I also identify changes in the risk profile of the pension fund as a function of the electoral calendar. The percentage of the pension fund invested in equity is more likely to decrease in non-electoral years. That is, more conservative investment decisions, which in turn increase the annual required contribution, are more likely to be observed when the election is still far away. I estimate an increase in probability of 7% of significantly reducing the share of equities in the pension fund in the year that is in the middle of the electoral cycle. The analog for higher risk is also true, but conditional on the partisanship of the incumbent governor. I observe that for states with Democratic governors there is an increased likelihood in significantly increasing the risk profile of the pension fund in the state during the pre-electoral year.

The principal policy implication of this chapter is the need to measure the political budget cycle using accrual-based accounting instead of cash-based accounting, which is currently the common practice (Irwin, 2015). Using accrual-based accounting will make more transparent the off-balance sheet liabilites incurred by a state in order to finance the deficit. The incorporation of pensions into the analysis takes one step in this direction. Changing the contributions and investment profile of the pension fund modifies expenditure not only for the current period, but also

for the duration of the promised benefits, and its effects are not easily observed by the population. The political budget cycles documented in the literature could thus be significantly underestimated.

This chapter can be extended to also analyze the changes in benefits of the pension plan. It is possible that increases in benefits are more likely to be observed when electoral gains for incumbent officials are greater, further deteriorating the sustainability of the system. Analogously, reductions in benefits for future workers could also be timed as a function of the electoral calendar. The same can be done for changes in the assumptions used for actuarial studies of pension plans. Factors such as an increase in the estimated return, years worked by the employee, and life expectancy can dramatically change the funding status of the plan, and therefore the required contributions of the government and taxpayers.

3.7 Tables

Table 3.1: Estimations, State Level - Changes in ARC

	Dependent variable:		
	Change in % of ARC Paid at State Level		
	(1)	(2)	(3)
Election − 1	−0.087*	−0.073	−0.015
	(0.052)	(0.048)	(0.066)
Election	−0.044	−0.035	−0.013
	(0.047)	(0.045)	(0.061)
Election + 1	−0.028	−0.014	0.013
	(0.051)	(0.048)	(0.065)
Δ Unemployment		0.034	0.034
		(0.022)	(0.022)
Δ Revenue PC		−0.010	−0.010
		(0.016)	(0.016)
Election − 1 × Democrat			−0.125
			(0.092)
Election × Democrat			−0.057
			(0.087)
Election + 1 × Democrat			−0.060
			(0.092)
Democrat		−0.008	0.047
		(0.024)	(0.059)
S & P 500	0.141	0.352	0.374
	(0.198)	(0.238)	(0.239)
T. Bond (10 year)	0.152	0.439	0.486
	(0.447)	(0.478)	(0.484)
S & P 500 − 1	0.077	0.312	0.318
	(0.194)	(0.243)	(0.244)
T. Bond (10 year) − 1	0.044	0.249	0.286
	(0.398)	(0.448)	(0.449)
Union Membership		0.041	0.043
		(0.051)	(0.052)
Δ Union Membership	0.193	0.111	0.108
	(0.557)	(0.539)	(0.540)
Constant		−0.088	−0.122
		(0.081)	(0.087)
State Fixed Effects	Yes	No	No
Observations	623	623	623
Adjusted R^2	0.008	0.013	0.016
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3.2: Estimations, State Level - Probability of Full ARC

	Dependent variable:					
	Dichotomous ARC ≥ 95% at State Level					
	linear			*conditional logit*		
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−0.093*	−0.099**	−0.052	-0.880*	-0.913*	-0.626
	(0.047)	(0.048)	(0.063)	(0.373)	(0.363)	(0.472)
Election	0.040	−0.036	−0.034	-0.167	-0.225	-0.425
	(0.043)	(0.043)	(0.058)	(0.337)	(0.341)	(0.466)
Election + 1	−0.028	−0.037	−0.053	-0.355	-0.413	-0.624
	(0.047)	(0.047)	(0.063)	(0.357)	(0.353)	(0.477)
Δ Unemployment		0.003	0.004		-0.166	-0.152
		(0.020)	(0.020)		(0.145)	(0.148)
Δ Revenue PC		0.010	0.010		0.0297	0.0249
		(0.015)	(0.015)		(0.100)	(0.102)
Election − 1 × Democrat			−0.096			-0.618
			(0.087)			(0.659)
Election × Democrat			0.005			0.376
			(0.082)			(0.609)
Election + 1 × Democrat			0.029			0.366
			(0.086)			(0.641)
Democrat		−0.033	−0.020		-0.198	-0.268
		(0.038)	(0.064)		(0.283)	(0.469)
S & P 500	−0.527***	−0.548**	−0.519**	-2.884*	-3.651*	-3.329*
	(0.180)	(0.222)	(0.223)	(1.349)	(1.497)	(1.520)
T. Bond (10 year)	−0.705*	−0.721	−0.626	-3.222	-4.374	-3.349
	(0.406)	(0.444)	(0.449)	(3.190)	(3.299)	(3.379)
S & P 500 − 1	−0.204	−0.211	−0.219	-4.208***	-5.280***	-5.501***
	(0.176)	(0.227)	(0.228)	(1.193)	(1.470)	(1.496)
T. Bond (10 year) − 1	−0.001	0.067	0.088	-5.616*	-6.563*	-6.720*
	(0.362)	(0.417)	(0.418)	(2.661)	(3.007)	(3.072)
Union Membership		−0.251	−0.255		-1.885	-1.901
		(0.243)	(0.243)		(1.794)	(1.805)
Δ Union Membership	−0.906*	−0.783	−0.798	-6.392	-5.662	-5.871
	(0.506)	(0.527)	(0.528)	(3.829)	(3.908)	(3.937)
Constant		0.710***	0.697***		2.451**	2.444**
		(0.118)	(0.121)		(0.823)	(0.856)
State Fixed Effects	Yes	No	No	Yes	No	No
Observations	623	6243	623	507	623	623
Adjusted R^2	0.047	0.052	0.055			

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.3: Estimations, State Level - Decrease in Equity

	Dependent variable:					
	Dichotomous decrease of 5 points or more of equity at State Level					
	linear			*conditional logit*		
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−0.068*	−0.065*	−0.097*	-0.472	-0.457	-0.976
	(0.038)	(0.037)	(0.049)	(0.415)	(0.434)	(0.668)
Election	−0.070**	−0.071**	−0.034	-0.472	-0.255	0.467
	(0.035)	(0.034)	(0.046)	(0.390)	(0.412)	(0.539)
Election + 1	−0.072*	−0.060	−0.028	-0.495	-0.396	0.0747
	(0.038)	(0.037)	(0.049)	(0.431)	(0.433)	(0.561)
Δ Unemployment		0.012	0.011		0.374*	0.384*
		(0.016)	(0.016)		(0.163)	(0.167)
Δ Revenue PC		−0.021*	−0.020*		-0.136	-0.134
		(0.012)	(0.012)		(0.130)	(0.136)
Election − 1 × Democrat			0.062			0.757
			(0.069)			(0.786)
Election × Democrat			−0.080			-1.723*
			(0.065)			(0.843)
Election + 1 × Democrat			−0.065			-0.940
			(0.068)			(0.817)
Democrat		−0.013	0.011		-0.0995	0.276
		(0.024)	(0.047)		(0.278)	(0.455)
S & P 500	−0.671***	−0.544***	−0.571***	-5.443***	-3.072	-3.536
	(0.145)	(0.177)	(0.177)	(1.617)	(2.028)	(2.045)
T. Bond (10 year)	−0.491	−0.316	−0.419	-7.488	-2.770	-4.549
	(0.327)	(0.354)	(0.357)	(4.026)	(4.823)	(4.864)
S & P 500 − 1	−0.571***	−0.497***	−0.487***	0.216	2.061	2.847
	(0.142)	(0.180)	(0.180)	(1.349)	(1.705)	(1.785)
T. Bond (10 year) − 1	−0.658**	−0.749**	−0.748**	6.049	6.142	7.540
	(0.291)	(0.331)	(0.331)	(3.208)	(3.656)	(3.872)
Union Membership		0.013	0.012		-0.0554	-0.0463
		(0.064)	(0.064)		(0.767)	(0.770)
Δ Union Membership	0.089	−0.008	0.039	0.886	0.828	1.644
	(0.407)	(0.406)	(0.406)	(4.824)	(4.710)	(4.769)
Constant		0.311***	0.307***		-2.145***	-2.365***
		(0.064)	(0.067)		(0.645)	(0.702)
State Fixed Effects	Yes	No	No	Yes	No	No
Observations	622	622	622	532	622	622
Adjusted R^2	0.102	0.108	0.115			
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 3.4: Estimations, State Level - Increase in Equity

	Dependent variable:					
	Dichotomous increase of 5 points or more of Equity at State Level					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−0.011	−0.010	−0.073*	-0.147	-0.136	-1.519
	(0.034)	(0.033)	(0.044)	(0.526)	(0.520)	(0.846)
Election	−0.001	0.0002	−0.021	0.0538	0.107	-0.219
	(0.030)	(0.030)	(0.040)	(0.494)	(0.497)	(0.632)
Election + 1	0.073**	0.075**	0.071	0.885*	0.938*	0.723
	(0.033)	(0.033)	(0.044)	(0.442)	(0.441)	(0.547)
Δ Unemployment		0.010	0.010		0.166	0.179
		(0.014)	(0.014)		(0.220)	(0.220)
Δ Revenue PC		0.001	0.001		0.0794	0.103
		(0.011)	(0.010)		(0.176)	(0.179)
Election − 1 × Democrat			0.129**			2.809*
			(0.061)			(1.163)
Election × Democrat			0.043			0.913
			(0.058)			(0.996)
Election + 1 × Democrat			0.015			0.837
			(0.061)			(0.928)
Democrat		0.002	−0.042		0.0853	-0.954
		(0.023)	(0.042)		(0.332)	(0.794)
S & P 500	0.163	0.216	0.182	3.955	4.245	4.074
	(0.128)	(0.156)	(0.156)	(2.191)	(2.559)	(2.627)
T. Bond (10 year)	0.060	0.133	0.046	2.787	3.064	1.995
	(0.289)	(0.313)	(0.315)	(3.926)	(4.260)	(4.335)
S & P 500 − 1	0.109	0.179	0.179	1.935	3.014	3.180
	(0.125)	(0.160)	(0.159)	(1.688)	(2.229)	(2.264)
T. Bond (10 year) − 1	0.024	0.117	0.079	1.286	3.073	2.673
	(0.257)	(0.293)	(0.293)	(3.539)	(4.302)	(4.416)
Union Membership		−0.175***	−0.176**		-2.894**	-3.038**
		(0.067)	(0.069)		(1.055)	(1.076)
Δ Union Membership	−0.053	−0.008	−0.002	-1.051	-0.163	0.0725
	(0.359)	(0.360)	(0.360)	(5.510)	(5.380)	(5.445)
Constant		0.071	0.102*		-3.094***	-2.656**
		(0.058)	(0.061)		(0.860)	(0.895)
State Fixed Effects	Yes	No	No	Yes	No	No
Observations	622	622	622			
Adjusted R^2	0.026	0.038	0.045			

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.5: Number of Pension Plans by State

State	Plans	State	Plans
Alabama	2	Montana	2
Alaska	2	Nebraska	2
Arizona	4	Nevada	2
Arkansas	2	New Hampshire	1
California	15	New Jersey	3
Colorado	5	New Mexico	2
Connecticut	3	New York	7
Delaware	1	North Carolina	2
Florida	2	North Dakota	2
Georgia	3	Ohio	5
Hawaii	1	Oklahoma	3
Idaho	1	Oregon	2
Illinois	8	Pennsylvania	4
Indiana	2	Rhode Island	2
Iowa	2	South Carolina	2
Kansas	1	South Dakota	1
Kentucky	3	Tennessee	3
Louisiana	6	Texas	8
Maine	2	Utah	2
Maryland	3	Vermont	2
Massachusetts	2	Virginia	2
Michigan	4	Washington	5
Minnesota	7	West Virginia	2
Mississippi	1	Wisconsin	2
Missouri	6	Wyoming	1

Table 3.6: Average ARC paid by State 2001-2014

State	Average ARC Paid	State	Average ARC Paid
Oregon	155%	Iowa	93%
West Virginia	116%	Alaska	93%
Wyoming	112%	Michigan	91%
Tennessee	110%	Indiana	91%
Mississippi	105%	New York	90%
Florida	103%	Hawaii	90%
Idaho	103%	Texas	90%
Georgia	102%	Kentucky	90%
North Carolina	102%	Louisiana	89%
Rhode Island	101%	Montana	88%
Wisconsin	101%	New Mexico	87%
Arkansas	101%	Minnesota	86%
Arizona	100%	Kansas	84%
Alabama	100%	Maryland	83%
Delaware	100%	Ohio	83%
Utah	100%	Washington	81%
Maine	99%	Virginia	80%
South Carolina	98%	Colorado	79%
South Dakota	98%	North Dakota	76%
Nebraska	96%	Oklahoma	74%
California	96%	Massachusetts	71%
Connecticut	94%	Illinois	70%
Nevada	94%	Pennsylvania	64%
Missouri	93%	New Jersey	42%

[3]Source: Own calculations based on the Public Plans Database (2017)

Table 3.7: Funding Percentage of Pension Plans by State 2014

State	Funding	State	Funding
South Dakota	100%	Nevada	72%
Wisconsin	100%	Montana	70%
North Carolina	96%	Virginia	70%
Idaho	94%	New Mexico	70%
Delaware	92%	Maryland	69%
Oregon	92%	Arizona	69%
Washington	92%	Alabama	67%
Tennessee	90%	South Carolina	64%
Florida	86%	North Dakota	63%
Utah	84%	Indiana	63%
Maine	83%	Colorado	63%
Iowa	82%	Michigan	62%
Nebraska	81%	Massachusetts	62%
Texas	81%	Kansas	62%
New York	81%	Rhode Island	62%
Missouri	81%	Louisiana	62%
Georgia	80%	Hawaii	61%
Wyoming	79%	New Jersey	61%
Arkansas	77%	Mississippi	61%
Minnesota	76%	Pennsylvania	61%
Ohio	75%	Alaska	58%
California	75%	Connecticut	52%
Oklahoma	73%	Kentucky	49%
West Virginia	73%	Illinois	48%

[3]Source: Own calculations based on the Public Plans Database (2017)

Table 3.8: Variable Distribution (Mean and S.D.)

Variable	**State Level**	**Plan Level**
% ARC Paid	0.93 (0.38)	0.94 (0.53)
Change in % ARC Paid	0 (0.42)	0 (0.66)
% Invested in Equities	0.54 (0.1)	0.52 (0.15)
Change % Invested in Equities	0 (0.06)	0 (0.1)
Unemployment (%)	6.11 (2.04)	6.4 (2.04)
Change in Unemployment (%)	0 (1.27)	0 (1.28)
Number of Democrats (observations)	307	1,304
Observations	672	2,170

California Public Employees' Retirement System (CalPERS, 2016)

Table 3.9: CalPERS' Investment Regime

Asset Class	**Target**	**Range**
Growth	59%	52-66%
Global Equity	47%	40-54
Private Equity	12%	8-16%
Income - Global Fixed Income	19%	14-24%
Real Assets	14%	9-19%
Real Estate	11%	6-16%
Infrastructure & Forestland	3%	1-5%
Inflation	6%	3-9%
Liquidity	2%	+/-6
Total Fund	100%	

Table 3.10: CalPERS' Board

Represented Party	**#**	**Appointed by**
All members	2	All members
Active State Members	1	Active State Members
Active School Members	1	Active School Members
Public Agency Members	1	Public Agency Members
Retirees	1	Retirees
Governor (Official of a Local Govt)	1	Governor
Governor (Official of Life Insurer)	1	Governor
Public Representative	1	Jointly by speaker of the Assembly and the Senate
State Treasurer	1	Ex Officio
State Controller	1	Ex Officio
Director of California Human Resources	1	Ex Officio
Representative of State Personnel Board	1	Ex Officio

California State Teachers' Retirement System (CalSTRS, 2016)

Table 3.11: CalSTRS' Investment Regime

Asset Class	**Target**	**Range**
Global Equity	47%	41-53%
Private Equity	13%	10-16%
Real Estate	13%	10-16%
Total Equity	73%	
Inflation Sensitive	4%	1-7%
Fixed Income	12%	9-15%
Cash	2%	+/- 3%
Risk Mitigating Strategies	9%	6-12%
Innovative Strategies	0%	+/- 3%
Total Diversifying	27%	
Total	100%	

Table 3.12: CalSTRS' Board

Represented Party	**#**	**Appointed by**
Current Educators	3	Members
Retired CalSTRS	1	Governor, approved by Senate
Public Servants	3	Governor, approved by Senate
School Board	1	Governor, approved by Senate
Director of Finance	1	Ex Officio
State Controller	1	Ex Officio
State Superintendent	1	Ex Officio
State Treasurer	1	Ex Officio

Florida Retirement System (FRS, 2014)

Table 3.13: FRS' Investment Regime

Asset Class	**Target**	**Range**
Global Equity	53%	45 - 65%
Fixed Income	18%	10 - 26%
Real Estate	10%	4 - 16%
Private Equity	6%	2- 9%
Strategic Investments	12%	0 - 16%
Cash Equivalents	1%	0.75 - 5%
Total Fund	100%	

Table 3.14: FRS' Board

Represented Party	**#**	**Appointed by**
Governor	1	Constitution
Chief Financial Officer	1	Constitution
Attorney General	1	Constitution

Teacher Retirement System of Texas (TRS, 2016)

Table 3.15: TRS's Investment Regime

Asset Class	**Target**	**Range**
Global Equity	57%	50 -68%
Stable Value	16%	11-23%
Real Return	22%	17-27%
Risk Parity	5%	0-10%
Total	100%	

Table 3.16: TRS' Board

Represented Party	**#**	**Appointed by**
Governor	3	Governor
Board of Education	2	Governor from a list of the Board
Active members (Teachers)	3	Governor from nominations of school districts
Higher Education Teachers	1	Governor from nominatios of higher education
Retirees	1	Governor from list of retirees

New York State Teachers' Retirement System (NYSTRS, 2016)

Table 3.17: NYSTRS' Investment Regime

Asset Class	**Target**	**Range**
Domestic Equity	35%	31 - 39%
International Equity	18%	14 - 22%
Domestic Fixed Income	16%	12 - 20%
Real Estate	11%	6 - 16%
Real Estate Debt	8%	4 - 12%
Private Equity	8%	3 - 13%
Global Bonds	2%	0 - 3%
High Yield Bonds	1%	0 - 3%
Short Term Investments (Cash Equivalents)	1%	0 - 4%
Total	100%	

Table 3.18: NYSTRS' Board

Represented Party	**#**	**Appointed by**
Teachers	3	Teachers delegates
Retirees	1	Retirees
School Administrators	2	Commissioner of Education
School Board	2	School Board
Bank Executive	1	Board of Regents
State Comptroller	1	Statutory

3.8 School Employees' Retirement System of Ohio (OHSERS, 2015)

Table 3.19: OHSERS' Investment Regime

Asset Class	**Target**	**Range**
Equity	55%	45 - 65%
Global Equities	45%	35 - 55%
Global Private Equity	10%	5 - 15%
Income	35%	30 – 40%
Global Bonds	19%	12 - 26%
Global Real Assets	15%	10 - 20%
Cash Equivalents	1%	0 - 5%
STRATEGY		
Multi-Asset Strategies	10%	5 - 15%
Opportunistic Investments	0%	0 – 5%
Total	100%	

Table 3.20: OHSERS' Board

Represented Party	**#**	**Appointed by**
Employee (Teachers)	4	Employees (Teachers)
Retiree	2	Retirees
Investment Expert	1	Governor
Investment Expert	1	Treasurer
Investment Expert	1	House & Senate

3.9 Ohio Public Employees Retirement System (OPERS, 2017)

Table 3.21: OPERS' investment regime

Asset Class	Target	Range
Public Equity	39%	31 to 47%
U.S. Equity	**	+/- 5%
Non-U.S. Equity	**	+/- 5%
Fixed Income	23%	16 to 30%
Core Fixed	9	6 to 12
Emerging Markets Debt	7	3 to 9
Floating Rate Debt	0	0 to 2
Securitized Debt	1	0 to 2
TIPS	2	1 to 3
High Yield	3	0 to 5
U.S. Treasury	1	0 to 2
Alternatives	31%	22 to 40%
Private Equity	10	5 to 15
Real Estate	10	5 to 15
Hedge Funds	8	4 to 12
Opportunistic	2	0 to 4
Commodities	1	0 to 2
Risk Parity	5%	2 to 8%
GTAA	2%	0 to 4%
Operating Cash	0%	0 to 3%
Total	100%	

Table 3.22: OPERS' Board

Represented Party	#	Appointed by
Retirees	2	Retirees
Non-teaching College/University Employees	1	Non-teaching College/ University Employees
Treasurer-Appointed Investment Expert	1	Treasurer
General Assembly Appointed Investment Expert	1	General Assembly
Ohio Department of Administrative Services	1	Statutory Member
Miscellaneous Employees	1	Miscellaneous Employees
State Employees	1	State Employees
County Employees	1	County Employees
Municipal Employees	1	Municipal Employees
Governor	1	Governor

3.10 Figures

Figure 3.1: Distribution of % ARC Paid by Employer at State Level

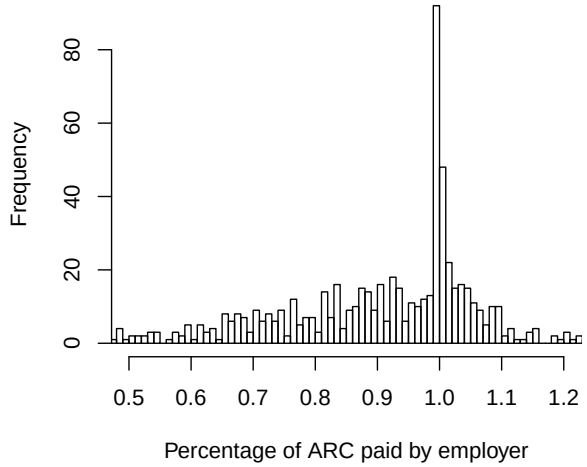


Figure 3.2: Distribution of Changes of % ARC Paid by Employer at State Level

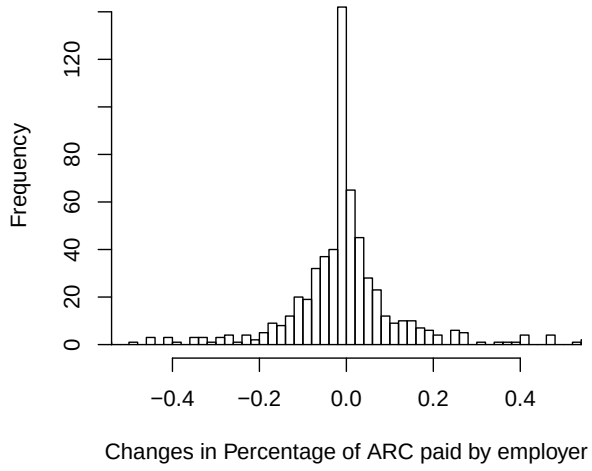


Figure 3.3: Distribution of % Invested in Equities at State Level

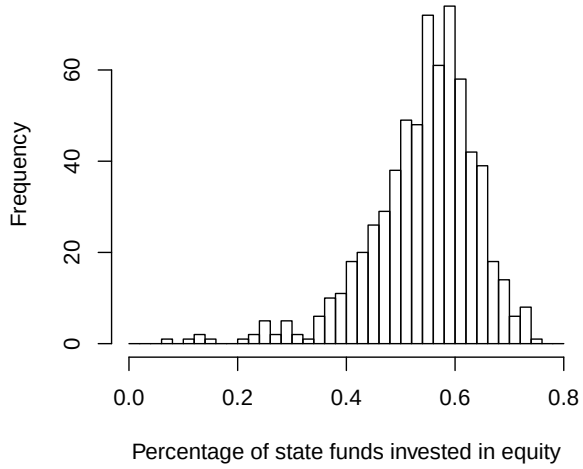
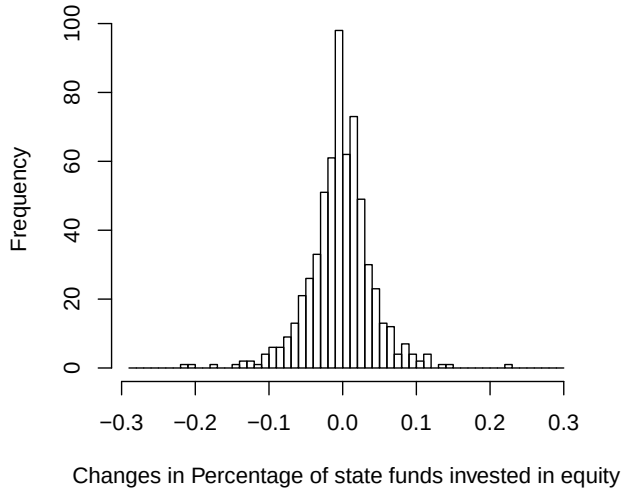


Figure 3.4: Distribution of Changes of % Invested in Equities at State Level



3.11 Appendices

Plan Level Analysis

Analysis at the Plan Level

In this appendix I use individual plans as units of observation. As I did for state level analysis, I explore the relationship of both percentage ARC contributed, and the percentage equity has as the total value of the pension fund as a function of the electoral calendar. Having similar results as the state level would indicate that the dynamics of pension funds are more or less independent of their size and importance in the state. Meanwhile, if results are attenuated towards zero it would indicate that the cycle is much more prevalent in large plans, and meanwhile there are fewer incentives to modify funding decisions for smaller plans.

I present the distribution of ARC contributions, percentage invested in equity, and their respective changes in Figures 3.5-3.8. The characteristics of these are very similar to their state counterparts. However, I identified two important differences. Most plans contribute full ARC transfers (and their received contributions do not change from one year to the next), so the large changes in contributions we observed at the state level are being driven by a relatively small number of plans. The second difference is that there is a (small) number of plans which do not invest in equity at all (Figure 3.7). The pattern we observed at the state level of the continuity of the same investment profile is also reflected at the plan level (Figure 3.8).

(Figures 3.5-3.8 found at the end of this chapter)

ARC

My estimations of the changes in percentage ARC contributed can be found in Table 3.23. As we can see in column (1) the relationship found when exploring

the state level disappears. I do not find this relationship when I include changes in unemployment, changes in revenue, and partisanship of the governor in the regression (see column 2). As I mentioned before, this provides evidence in favor of the hypothesis that we observe larger cycles the larger the plan is within the state.

(Table 3.23 found at the end of this chapter)

Only when I include the interaction of changes in ARC with the partisanship of the governor are we able to observe a pattern. In column (3) we see that, on average, plans in states with a Democrat governor are receiving fewer employer contributions as percentage of the ARC during the preelectoral year, making the reduction of contribution in such states more uniform despite their size more prevalent in pre-electoral years. [4]

(Table 3.24 found at the end of this chapter)

In order to test the robustness of the results I also use the dichotomous dependent variable specification. Again I follow Thom and Randazzo (2015) to define the dummy variable "$CompleteARC_t$" as 1 if the employers contributed at least 95% of the required ARC in the fiscal year t, and 0 otherwise. As it was the case with the state level, I use both a linear probability model and a conditional logit model. The results of this alternative setup of the model can be found in Table 3.24. The conclusions I get from this setup align with the results of the previous case with some exceptions. In the baseline model (columns (1) and (4)) I do not observe either the political pension contribution cycle, providing evidence in favor of the mitigation for smaller plans. When I control for other variables, only in the linear

[4] As it was the case with states as units of observation, I do not include state fixed effects in columns (2) and (3) since the vast majority of states did not experience party change in the analyzed period.

probability model (column 2), I observe a decrease in probability of contributing the full ARC, but even in this case the likelihood of observing full payment of ARC decreases to less than half (4.2%) compared to aggregating by state (11.1%). In conclusion, we can say that for changes in ARC contributed, and also for observing the political pension contribution cycle, it is better to use data aggregated at the state level and not use data at the individual plan level, since the most important changes are observed in the largest plans.

Investment in Equities

As I did in the state case, I also analyzed changes in percentage of equity in the pension fund as the function of the electoral calendar. Refer to section 3.5 for a description of the variables used and their implications. Regarding the decrease in equity (Table 3.25) at the plan level, and consistent with the results we have at the state level, we observe that all the regression coefficients for the electoral calendar are negative. Therefore, I can conclude that during the middle of the electoral cycle we observe a decrease in percentage invested in equity. The interpretation is the same as section 3.5: more conservative investment decisions are generally made in non electoral years. The interesting result here, and consistent with my findings regarding ARC contributions (Section 3.11), is that this change of more conservative portfolios is larger at the state than at the plan level. That is, larger plans experience larger changes in portfolio composition compared to smaller plans. These results are robust to including economic variables in the estimation such as changes in unemployment and state revenue, as well as the partisanship of the governor.

(Table 3.25 found at the end of this chapter)

For large increases in equity (Table 3.26), I get very similar estimations as for the state level. Partisanship is very important in order to interpret the political pension contribution cycle. When I do not directly incorporate partisanship into the analysis of the cycle I get results that are not expected. In columns (1), (2), (4), and (5), we observe that is during the post electoral year when we see increases in the percentage of equity in the pension fund, which go against what I expected. Meanwhile, when I incorporate partisanship we are able to observe that for states with Democrat governors incur in the change in equity. However, there is an important difference in these estimations compared to ARC contribution and decrease in equities. I do not observe a decrease in the cycle when I use the plan level compared to the state level. This suggests that large increases in equity are more prevalent in smaller plans as opposed to larger plans, and this goes against what I expected. The rationalization of this pattern is open for discussion.

(Table 3.26 found at the end of this chapter)

Table 3.23: Estimations, Plan Level - Changes in ARC

	Dependent variable:		
	Change in % of ARC Paid at Plan Level		
	(1)	(2)	(3)
Election − 1	0.011	−0.002	0.049
	(0.051)	(0.036)	(0.056)
Election	0.067	0.064	0.020
	(0.047)	(0.044)	(0.058)
Election + 1	0.010	−0.007	−0.037
	(0.050)	(0.037)	(0.056)
Δ Unemployment		0.004	0.003
		(0.018)	(0.018)
Δ Revenue PC		−0.009	−0.010
		(0.015)	(0.015)
Election − 1 × Democrat			−0.185**
			(0.073)
Election × Democrat			0.168***
			(0.061)
Election + 1 × Democrat			0.113
			(0.070)
Democrat		−0.005	−0.012
		(0.010)	(0.048)
S & P 500	−0.251	−0.212	−0.183
	(0.195)	(0.210)	(0.210)
T. Bond (10 year)	−0.234	−0.080	0.011
	(0.447)	(0.463)	(0.465)
S & P 500 − 1	0.291	0.223	0.183
	(0.186)	(0.198)	(0.200)
T. Bond (10 year) − 1	0.449	0.212	0.170
	(0.386)	(0.430)	(0.433)
Union Membership		0.009	0.014
		(0.010)	(0.009)
Δ Union Membership	−0.784	−0.262	−0.228
	(0.555)	(0.370)	(0.336)
Constant		−0.033	−0.019
		(0.061)	(0.066)
Plan Fixed Effects	Y	N	N
Observations	1,926	1,927	1,927
Adjusted R^2	0.008	0.008	0.009

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.24: Estimations, Plan Level - Probability of Full ARC

	Dependent variable:					
	Dichotomous ARC ≥ 95% at Plan Level					
	linear			*conditional logit*		
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−0.022	−0.047*	−0.054	-0.381	-0.438	-0.510
	(0.025)	(0.025)	(0.033)	(0.223)	(0.224)	(0.292)
Election	0.046**	−0.016	−0.003	-0.0155	-0.0986	-0.0429
	(0.023)	(0.025)	(0.034)	(0.226)	(0.230)	(0.301)
Election + 1	0.022	0.0002	0.020	0.0695	0.0400	0.178
	(0.025)	(0.025)	(0.034)	(0.224)	(0.225)	(0.304)
Δ Unemployment		−0.021**	−0.021**		-0.174*	-0.172*
		(0.010)	(0.010)		(0.0871)	(0.0871)
Δ Revenue PC		−0.005	−0.005		-0.0412	-0.0434
		(0.008)	(0.008)		(0.0685)	(0.0686)
Election − 1 × Democrat			0.014			0.156
			(0.044)			(0.399)
Election × Democrat			−0.024			-0.108
			(0.043)			(0.394)
Election + 1 × Democrat			−0.035			-0.248
			(0.044)			(0.398)
Democrat		−0.004	0.007		0.0673	0.118
		(0.019)	(0.033)		(0.174)	(0.299)
S & P 500	−0.519***	−0.506***	−0.513***	-4.142***	-4.628***	-4.705***
	(0.095)	(0.111)	(0.112)	(0.876)	(0.993)	(0.999)
T. Bond (10 year)	−0.743***	−0.627***	−0.653***	-4.894*	-5.631**	-5.845**
	(0.219)	(0.239)	(0.242)	(2.012)	(2.133)	(2.150)
S & P 500 − 1	−0.028	−0.507***	−0.497***	-3.341***	-4.571***	-4.502***
	(0.091)	(0.103)	(0.104)	(0.752)	(0.950)	(0.957)
T. Bond (10 year) − 1	0.139	−0.782***	−0.777***	-5.231**	-7.182***	-7.218***
	(0.189)	(0.223)	(0.226)	(1.672)	(1.987)	(1.999)
Union Membership		−0.028	−0.038		-0.577	-0.543
		(0.141)	(0.136)		(1.290)	(1.293)
Δ Union Membership	−0.363	−0.393	−0.386	-2.906	-3.037	-3.075
	(0.271)	(0.281)	(0.283)	(2.321)	(2.449)	(2.454)
Constant		0.784***	0.783***		2.786***	2.761***
		(0.068)	(0.068)		(0.622)	(0.635)
Plan Fixed Effects	Yes	No	No	Yes	No	No
Observations	1,930	1,931	1,931	1211	1931	1931
Adjusted R^2	0.032	0.043	0.044			

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.25: Estimations, Plan Level - Decrease in Equity

	Dependent variable:					
	Dichotomous decrease of 5 points or more of Equity at Plan Level					
	linear			*conditional logit*		
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−0.044*	−0.011	−0.009	-0.148	-0.0730	-0.0304
	(0.023)	(0.023)	(0.031)	(0.212)	(0.221)	(0.302)
Election	−0.084***	−0.028	0.011	-0.439*	-0.282	0.134
	(0.021)	(0.023)	(0.031)	(0.208)	(0.221)	(0.294)
Election + 1	−0.078***	−0.043*	−0.011	-0.425	-0.359	-0.0869
	(0.023)	(0.023)	(0.032)	(0.218)	(0.226)	(0.307)
Δ Unemployment		0.047***	0.048***		0.370***	0.374***
		(0.009)	(0.009)		(0.0862)	(0.0865)
Δ Revenue PC		−0.016**	−0.015**		-0.142	-0.142
		(0.007)	(0.007)		(0.0753)	(0.0757)
Election − 1 × Democrat			−0.006			-0.0849
			(0.041)			(0.381)
Election × Democrat			−0.075*			-0.877*
			(0.039)			(0.405)
Election + 1 × Democrat			−0.059			-0.496
			(0.041)			(0.404)
Democrat		0.004	0.041		-0.00721	0.313
		(0.015)	(0.029)		(0.150)	(0.252)
S & P 500	−0.552***	−0.209**	−0.218**	-3.526***	-1.020	-1.124
	(0.088)	(0.100)	(0.100)	(0.816)	(1.016)	(1.016)
T. Bond (10 year)	−0.068	0.122	0.081	-2.392	2.388	1.862
	(0.202)	(0.219)	(0.220)	(2.014)	(2.373)	(2.379)
S & P 500 − 1	−0.685***	0.217**	0.242**	-0.314	1.585	1.899*
	(0.085)	(0.096)	(0.097)	(0.707)	(0.907)	(0.926)
T. Bond (10 year) − 1	−0.840***	0.523***	0.559***	4.425**	4.852*	5.328**
	(0.174)	(0.202)	(0.203)	(1.629)	(1.904)	(1.941)
Union Membership		−0.059	−0.059		-0.734	-0.727
		(0.043)	(0.043)		(0.437)	(0.437)
Δ Union Membership	0.090	0.079	0.105	0.578	0.814	1.208
	(0.248)	(0.251)	(0.251)	(2.554)	(2.499)	(2.512)
Constant		0.137***	0.117***		-2.160***	-2.348***
		(0.036)	(0.039)		(0.348)	(0.375)
Plan Fixed Effects	Yes	No	No	Yes	No	No
Observations	2,014	2,015	2,015	1664	2015	2015
Adjusted R^2	0.091	0.080	0.083			
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 3.26: Estimations, Plan Level - Increase in Equity

	Dependent variable:					
	Dichotomous increase of 5 points or more of Equity at Plan Level					
	(1)	(2)	(3)	(4)	(5)	(6)
Election − 1	−0.040*	−0.033	−0.078***	-0.441	-0.338	-0.855**
	(0.022)	(0.022)	(0.029)	(0.251)	(0.245)	(0.320)
Election	−0.005	0.002	−0.032	-0.115	0.0386	-0.318
	(0.020)	(0.022)	(0.029)	(0.252)	(0.244)	(0.309)
Election + 1	0.034	0.036*	0.019	0.325	0.349	0.128
	(0.022)	(0.022)	(0.030)	(0.229)	(0.224)	(0.286)
Δ Unemployment		0.023***	0.023***		0.248**	0.256**
		(0.009)	(0.009)		(0.0912)	(0.0910)
Δ Revenue PC		0.006	0.006		0.0815	0.0749
		(0.007)	(0.007)		(0.0803)	(0.0801)
Election − 1 × Democrat			0.092**			1.211**
			(0.039)			(0.466)
Election × Democrat			0.066*			0.829
			(0.037)			(0.443)
Election + 1 × Democrat			0.036			0.607
			(0.039)			(0.427)
Democrat		−0.015	−0.063**		-0.171	-0.831*
		(0.014)	(0.027)		(0.154)	(0.334)
S & P 500	0.086	0.156	0.147	1.305	2.166	2.145
	(0.084)	(0.095)	(0.095)	(0.963)	(1.107)	(1.116)
T. Bond (10 year)	−0.172	−0.029	−0.038	-1.519	0.309	0.351
	(0.192)	(0.207)	(0.207)	(2.110)	(2.195)	(2.214)
S & P 500 − 1	−0.017	0.099	0.083	-0.450	1.366	1.228
	(0.081)	(0.091)	(0.091)	(0.805)	(1.019)	(1.027)
T. Bond (10 year) − 1	0.002	0.189	0.139	-0.897	2.539	1.984
	(0.166)	(0.190)	(0.192)	(1.863)	(2.141)	(2.168)
Union Membership		−0.131***	−0.132***		-1.458***	-1.498***
		(0.038)	(0.038)		(0.421)	(0.424)
Δ Union Membership	−0.103	−0.096	−0.123	-1.438	-1.204	-1.464
	(0.236)	(0.236)	(0.237)	(2.617)	(2.591)	(2.614)
Constant		0.128***	0.157***		-2.134***	-1.822***
		(0.033)	(0.036)		(0.375)	(0.392)
Plan Fixed Effects	Yes	No	No	Yes	No	No
Observations	2,014	2,015	2,015	1534	2015	2015
Adjusted R^2	0.014	0.025	0.028			
Note:						*p<0.1; **p<0.05; ***p<0.01

Figure 3.5: Distribution of % ARC Paid by Employer at Plan Level

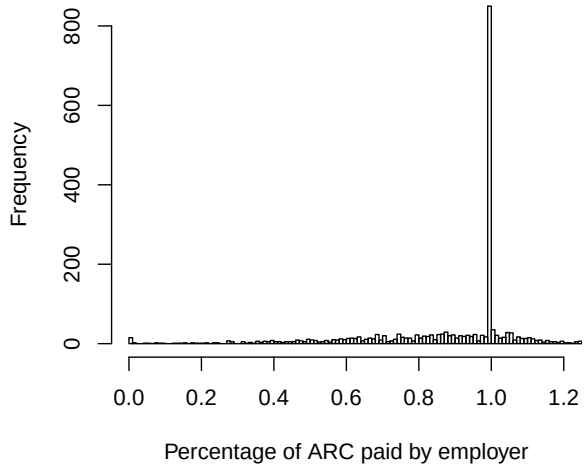


Figure 3.6: Distribution of Changes of % ARC Paid by Employer at Plan Level

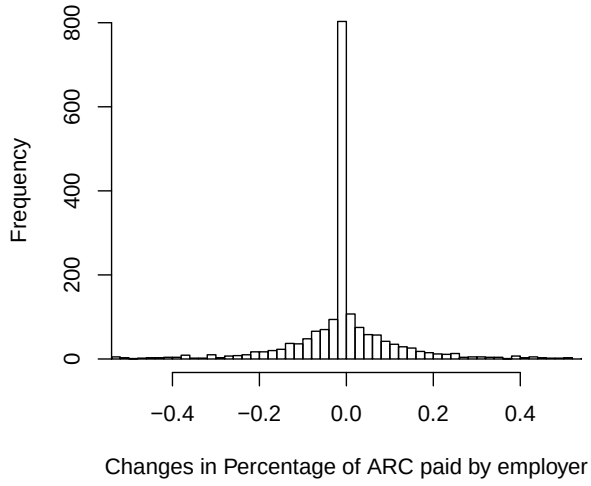


Figure 3.7: Distribution of % Invested in Equities at Plan Level

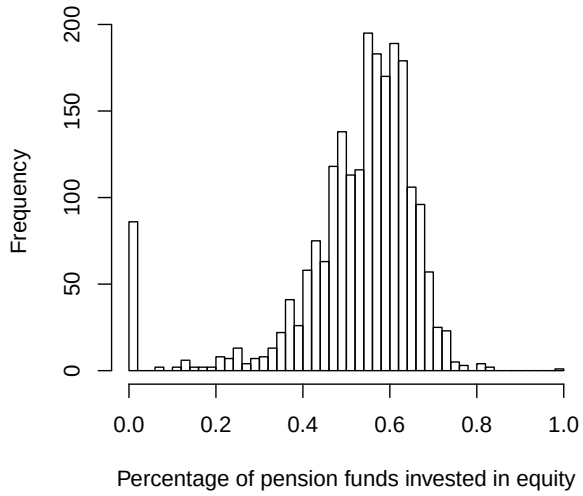
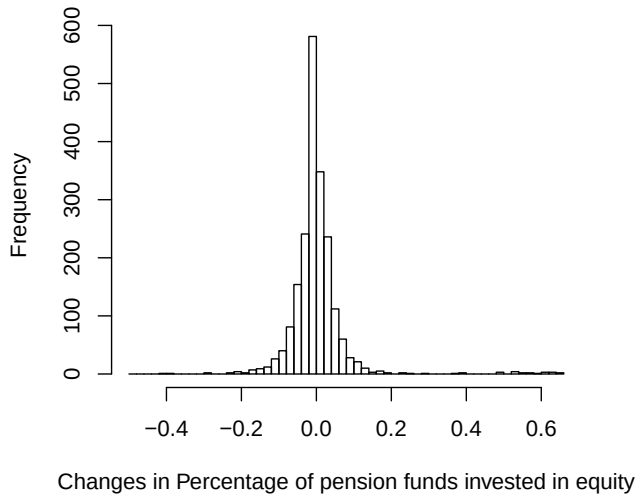
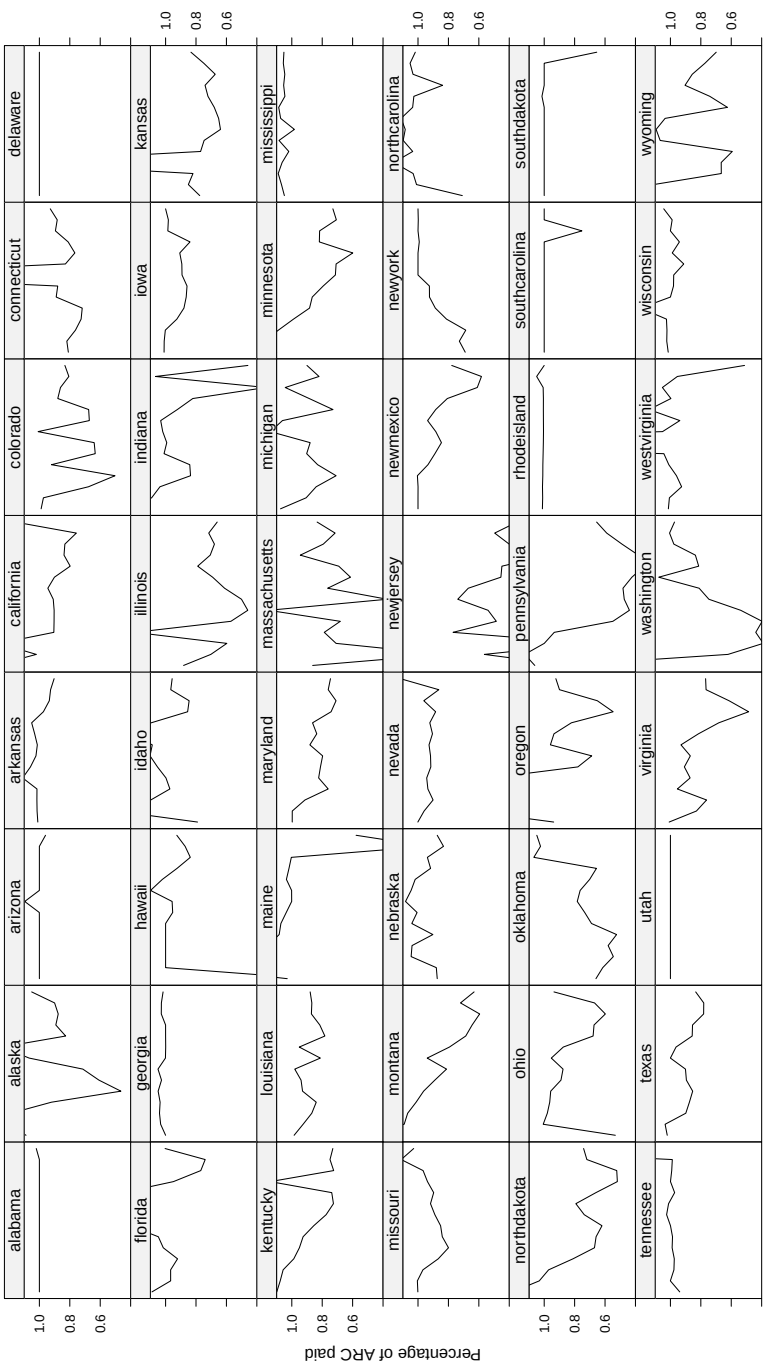


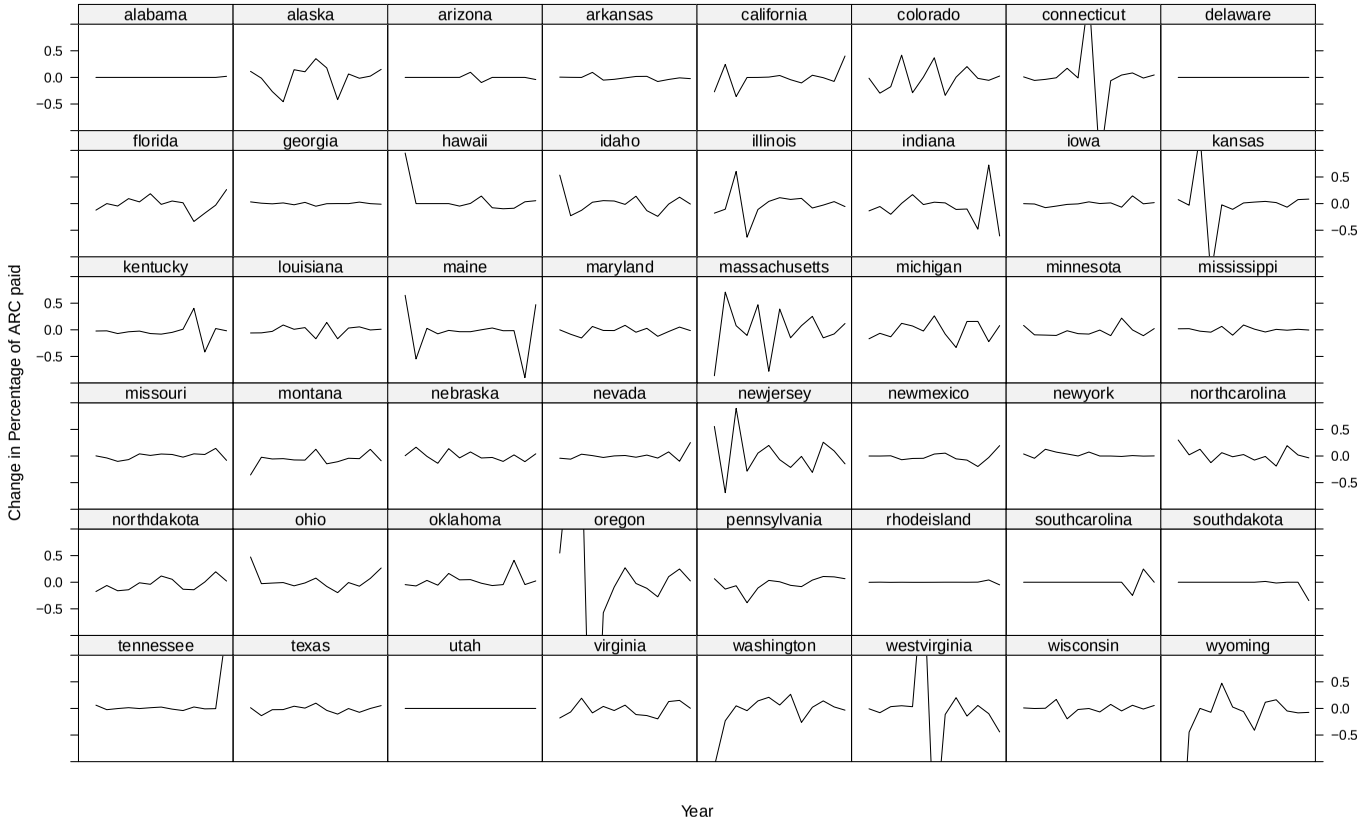
Figure 3.8: Distribution of Changes of % Invested in Equities at Plan Level



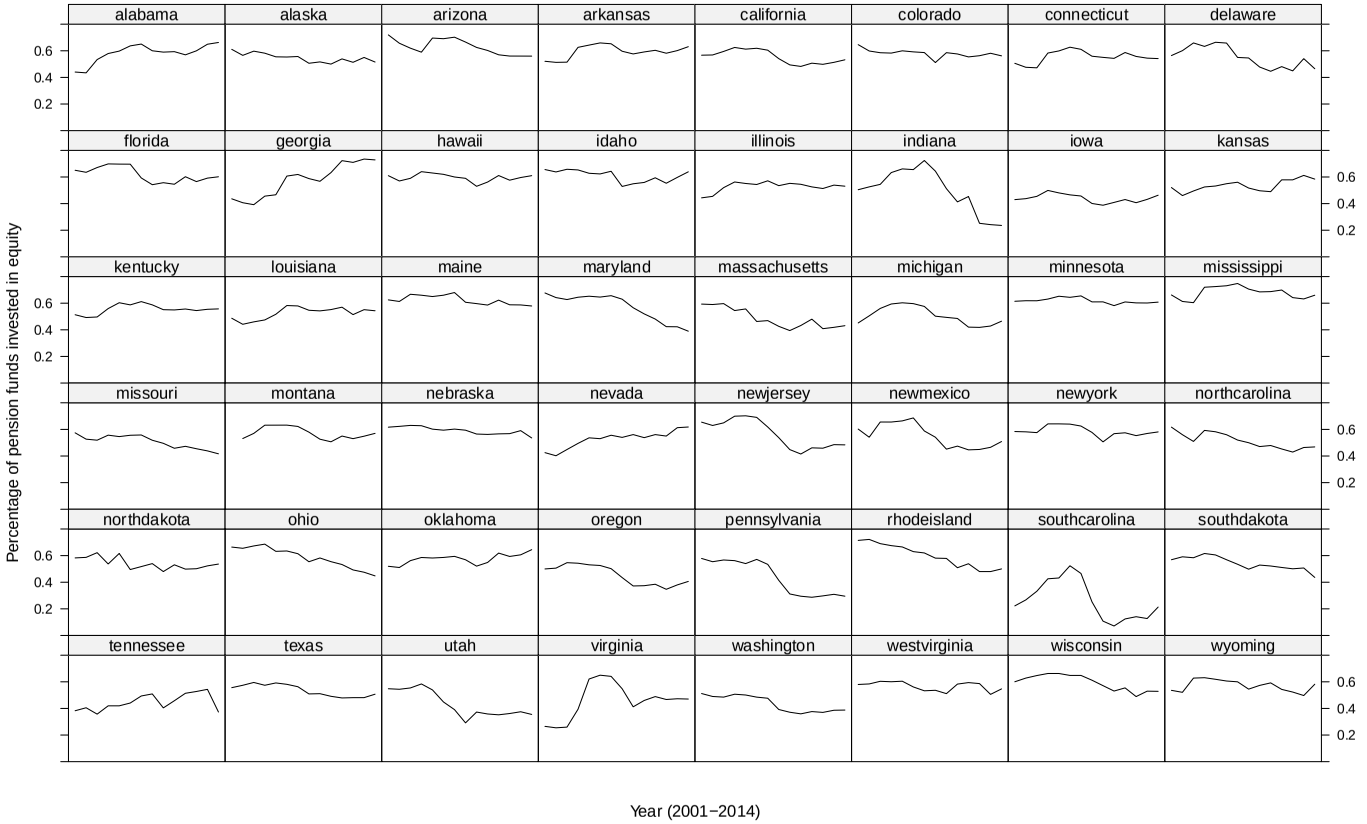
Evolution of Percentage ARC paid in the Sample



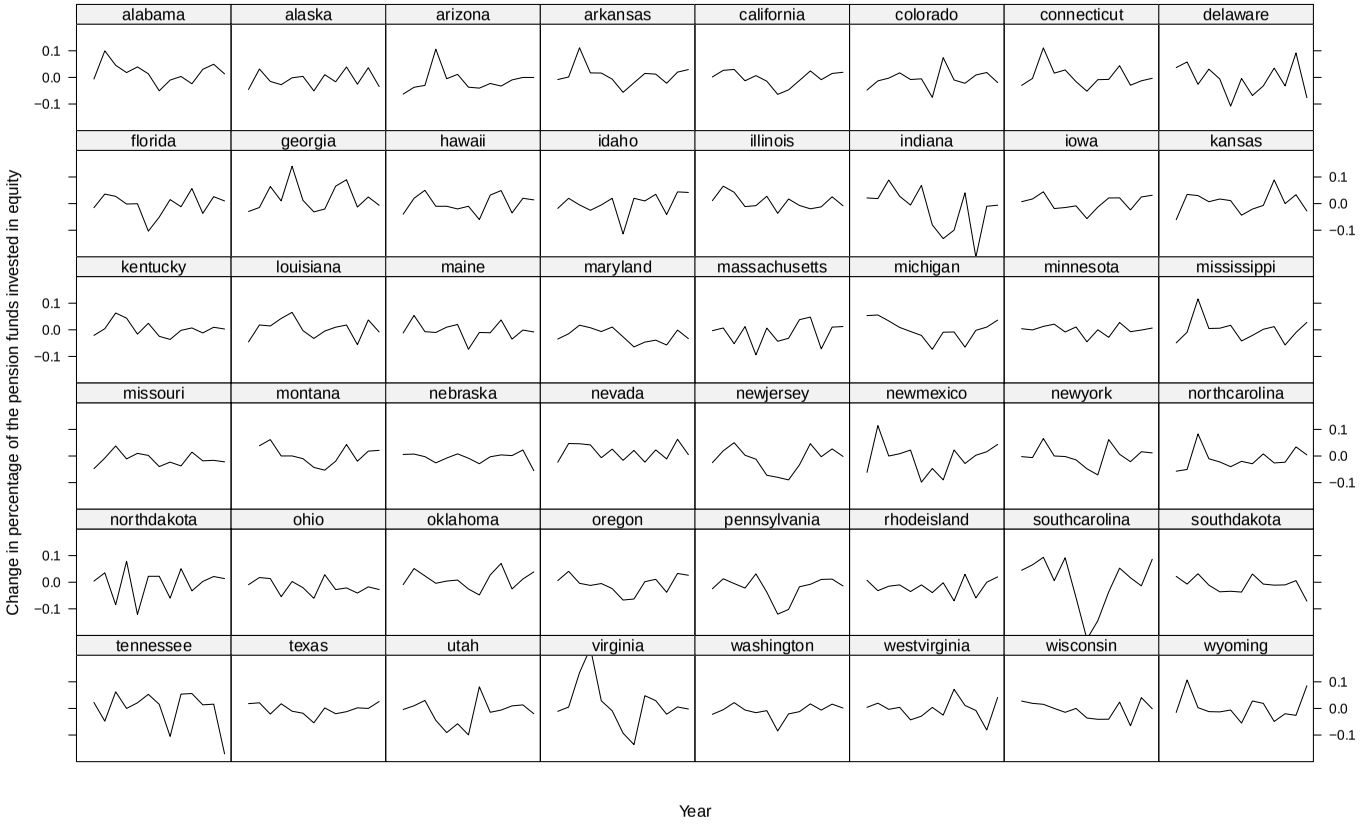
Evolution of Change in Percentage ARC paid in the Sample



Evolution of percentage of state pension funds invested in equity



Evolution of change in percentage of state pension funds invested in equity



Chapter 4

POLITICAL CREDIT RATING CYCLES: EVIDENCE FROM GUBERNATORIAL ELECTIONS IN MEXICO

4.1 Abstract

Do credit rating agencies adjust the timing of their rating announcements as a function of the electoral calendar? I develop a formal model of the political credit rating game, collect a novel database using credit ratings of Mexican states, and estimate panel models for gubernatorial elections in Mexico for 2005-2015. Both formal and empirical results indicate that credit rating agencies delay announcing rating downgrades until after elections, especially when elections are very competitive.

4.2 Introduction

> "To judge from their behavior, all the rating agencies worried about was maximizing the number of deals they rated for Wall Street investment banks, and the fees they collected from them."
>
> Michael Lewis - The Big Short (2011)

This chapter investigates the dynamics of credit rating announcements for governments during electoral periods. Credit Rating Agencies (CRAs) have become a key player in the democratic process. The agencies use financial, economic, and legal information to evaluate the performance of public financial entities. Since a negative evaluation from the CRAs implies problems with the administration of public resources, it is important to determine if CRAs are doing their job properly. If so, they promote electoral accountability and responsible use of the public finances. If not, they do not.

Information about incumbent officials is often incomplete, imprecise, and costly to acquire. Many voters thus rely on shortcuts, or cues, to make decisions (Sniderman, Brody, and Tetlock, 1993). One of the most important cues is the state of the economy. Tufte (1980) shows that economic performance in the months preceding an election can tip the political balance. Voters reward incumbents for prosperity and punish them for recession. Given this, incumbent officials have strong incentives to make sure that voters perceive the economy as prosperous before elections. As a result, as Nordhaus (1975) first showed, within an incumbents' term in office there is a predictable pattern in policy in which there is austerity in early years of the term and more flexibility before the elections.

Political budget cycles create a bias between how the economy appears to be at present and how it is actually going to perform in the long term. But it is not easy for voters to figure out that difference. In fact, the incumbent government can take actions that are not welfare-optimal, but send signals that look good to voters.

For more than 35 years, the mainstream approach to macroeconomic stability has been to follow the guidelines summarized in the Washington Consensus by Williamson (1990). These guidelines place a heavy emphasis on restricting the government's ability to manipulate the economy, on promoting fiscal discipline, and on insuring that that public expenditure directed towards investments, privatization of state enterprises, and deregulation.

Limiting the capacity of the government to intervene in the economy is not a new objective. After the Glorious Revolution, the creation of the Bank of England in 1694 greatly limited the ability of the British crown to renege on its obligations (North and Weingast, 1989). The concept of an independent central bank has persisted, and many countries rely on an independent central bank to hopefully separate the political and the economic objectives of the government. Controlling government spending

has followed a different strategy. Here the focus is on transparency. In democracies all around the world, governments are required to publish regular financial data reports. However, in many instances governments have the capacity to manipulate financial statements, and it would be extremely costly for individual citizens to verify the veracity of the published balance sheet and make predictions about the status of current and future public finances. To fulfill these roles, independent monitors such as financial auditors and CRAs are included. Financial auditors verify the accuracy of financial statements (AICPA, 2015), while CRAs evaluate and make inferences about the public finances.

CRAs provide information about a debtor's ability to meet their financial obligations through their credit ratings. A credit rating is an assessment of the creditworthiness of a debt instrument or obligor, based on analytical models, assumptions, and expectations (SEC, Investor Education, and Advocacy, 2013). Credit ratings are particularly useful when there is relevant, non-verifiable information (Parlour and Rajan, 2015). Because their ratings are forward looking, CRAs provide a signal of the government's long-term economic performance.

There is a problem, however. The problem with CRAs as supervising agents is that they are paid by the same entity they are supposed to rate, and different agencies compete for the same business. It has been shown that CRAs do not always behave in a way that maximizes information disclosing.

A number of previous studies have investigated how competition in the rating agency market affects ratings. These studies all focus on the private sector, i.e., on the corporate bond market. Becker and Milbourne (2011) find that the introduction and subsequent penetration of Fitch Ratings into the US market changed the dynamics of the bond rating market. They find that with increased competition, ratings went up, the correlations between bond yield and rating fell, and the ability of ratings to

accurate predict defaults deteriorated. Indeed, Flynn and Ghent (2017) show that new entrants in the rating agency market are more likely to assign higher ratings, indicating that they compete in ratings. Bolton, Freixas, and Shapiro (2012) use a formal model to show that because of rating shopping and fee renegotiation, rating efficiency may be higher under a monopoly CRA than under a duopoly despite the potential for the increased informativeness of two ratings. They find that having only one rating agency reflects risk more accurately compared to two. Griffin et al. (2013) suggest that the inflation in ratings is more likely derived from rating catering than rating shopping: in rating shopping firms only keep the highest, while in rating catering firms selectively disclose their rating from one agency or another. This pushes ratings up, and they find that the quality of ratings is lower for bonds that are rated by two CRAs compared to only one.

A challenge to all these accounts of competition and rating inflation is that CRAs would presumably not risk their reputation by not accurately reporting their assessments. However, Mathis, McAndrews, and Rochet (2009) show formally, and with data, that reputation incentives are not enough to make CRAs accurately report ratings. Skreta et al. (2009) show that rating inflation is more prevalent in markets where the assets to be rated are more complex. When assets are simple, agencies' ratings are similar and the incentive to ratings shop is low. When assets are sufficiently complex, ratings differ enough that an incentive to shop emerges. There are alternative explanations about why competition reduces rating quality. The most prevailing is the aforementioned rating shopping/catering one. However, there is an alternative explanation by Kashyap and Kovrijnykh (2013), who endogeneize the quality of the signal the rating agency receives depending on the level of effort it invests. More competition produce lower fees, leading to lower efforts and less accurate ratings. This theory explains a decrease in the accuracy of the signal, but does not explain the bias towards higher grades that is observed. All these explanations

differ in their causation of rating inaccuracy, but have in common the existence of a bias towards higher ratings, which I address in this chapter.

The behavior of CRAs with respect to the public sector has received far less scrutiny. What literature there is focuses exclusively on the national level. Block and Vaaler (2004) analyze whether the political budget cycle is relevant to foreign investors, and see that it is more likely to observe a downgrade 60 days before an election. Vaaler, Schrage, and Block (2006) complement and expand on this study by pointing out that not only do elections matter, but also if the party in power is likely to change. Right-wing governments are preferred by CRAs, but are penalized in electoral years even when they are safe in power. The political budget cycle undermines creditworthiness for more fiscally disciplined governments. Finally, Hanuusch and Vaaler (2013) find that CRAs can extend their influence via ratings and reduce the size of the budget deficit in electoral years.

While these papers study the relationship between the budget cycle and credit ratings, they do not examine the possibility that CRAs might have incentives to delay rating downgrades during electoral periods. The government is the client of the CRA, and they might well demand high ratings during electoral periods to be reelected. Incumbent officeholders would correspondingly want to avoid rating downgrades during electoral periods, because this information sends the signal of low quality of their governance.

This chapter fills this gap in previous research by studying the *political credit rating cycle* itself. I define a *political credit rating cycle* as a predictable change in the credit rating assigned to a governmental entity, or its debt, by a CRA as a function of the electoral calendar. This is independent of changes attributable to the budget cycle (for example budget balance, changes in debt) and the business cycle (i.e. growth or contraction of the economy as a function of the election).

To perform the analysis I develop a theoretical model that incorporates the incentives of governments and rating agencies. I then test the predictions generated by model using data on debt ratings for states in Mexico. In general, subnational data is extremely useful in comparative analysis. As Alt and Rose (2007) argue, restricting the domain of analysis to the subnational level holds relatively constant a wide range of socio-economic, political, and cultural characteristics that might otherwise confound the analysis. Additionally, states are unable to expand the monetary base. In Mexico, the state ratings market has the additional appealing characteristic of having ratings assigned to the state itself and not to particular individual bond emissions. This removes another potential source of identification problems.

4.3 The Ratings Agency Market

To understand the credit ratings market we need to consider the size of this market, what is at stake with a rating adjustment, and the incentives (and deterrents) to adjust the timing of these change in ratings. As Table 4.1 shows, the top three agencies, S&P, Moody's, and Fitch, account for more than 95% of the global market.

(Table 4.1 found at the end of this chapter)

At first glance, one might imagine that the central offices in New York or London understand that their reputations are at stake, and work to ensure that ratings are fair and timely. Indeed, the three largest CRAs have sizable resources: S&P Rating Services had 2.45 billion in revenue in 2014; during the same year Moody's reported revenue of $3.3 billion, while Fitch Group's various businesses generated total revenues of $1.1 billion. These three companies employ more than 29,000 people worldwide. If investors were to desert unreliable companies, a significant proportion of this revenue might be lost. Mexico is a tiny percentage of the global activity of any CRA, accounting for about 1% of the number of assigned ratings for

Moody's and S&P, and 6% for Fitch (See Table 4.2). One might well think that a CRA would not want to take a reputational risk in a market of the size of Mexico.

(Table 4.2 found at the end of this chapter)

While this may be the view of the home office, it is not the case for the Mexico City office. For the analysts that work in Mexico and are responsible for the ratings I analyze in this chapter, Mexican paper is the core of their business. Moreover, these offices are small: Fitch, S&P, and Moody's Mexico only have 57, 51, and 28 analysts respectively (and less than 20 are the public finance division in any firm). When a local office loses a client, it loses in two fronts. That client ceases to pay the continuation fees for the rating, and it can no longer sell the analysis to potential investors. Indeed, once a client decides to no longer be rated (the rating is withdrawn) all information regarding the client is considered confidential. The performance and survival of the whole public finance branch in Mexico thus depends on keeping a small number of clients satisfied. Clearly, there is a principal-agent problem within the firm. The parent companies very well might want to keep their reputation intact, but their local analysts want to maximize the number of clients.

If private solutions to principal agent problems in credit ratings are threatened by the importance of local knowledge and firm decentralization, one might think that regulation can take care of them. Since the three main CRAs in Mexico are foreign, they have to comply with both their home and Mexican regulations. Several points in this regulations are directed towards reducing the opportunities of collusion and the incentives to do so: 1) CRAs are required to use all credible and potentially significant information available to assign a rating, including the one received from a source other than the client 2) CRAs are prohibited from issuing or maintaining a credit rating where a person within the CRA who participates in determining or monitoring the credit rating also participates in sales or marketing of a product or

service of the CRA or is influenced by sales or marketing considerations. Sales or marketing is defined as "pitching" the services, offering subscription to the CRA, or providing information about the costs of the CRA. 3) There are limitations on how much revenue a CRA can get from a single client, as it should not exceed 5 to 10% the CRA revenue. Yet these regulations at best mitigate the problem because individual raters do not want to lose clients.

Other regulations, although directed towards increasing efficiency in ratings, have the potential to increase the incentive for collusion. For example, in Mexico CRAs are required to submit their financial information to the regulation office. Because this information is confidential, it cannot be audited by the general public or financial entities. Further, CRAs are required to notify their client when a rating is going to be modified, perhaps to verify the rationale of this change. However, such notice provides additional opportunities for negotiation or collusion between client and the CRA.

Supply side issues in the rating market raise serious questions about the reliability of this informational service, but there are also issues on the demand side. These stem largely from the Mexican gubernatorial political system. There are 31 states in Mexico plus the Federal District (Mexico City). The governor who heads the state's executive is elected by popular vote for a term of at most six years. Each governor may only serve a single term. Nevertheless, the career of a governor does not end with his term. State executives from the three main parties tend to be strong candidates for their parties' presidential nominations, and even politicians who do not run for the nation's highest office can provide critical support for other presidential hopefuls (Langston, 2010). Therefore, there are still strong incentives for a governor to keep his party in power in his state, and to make its financial status appear as favorable as possible.

A rating downgrade has unfavorable consequences for the reelection probability of the incumbent party. First of all, the news of the rating downgrade is published in the mainstream media, with negative references to budget imbalances or increases in public debt. Second, current bank loans of the state have variable interest rates that rise with rating downgrades.[1] Third, banks have restrictions on the riskiness of the assets they are able to buy. When there is a rating downgrade, the pool of potential investors in the state falls. Clearly, a government prefers to avoid ratings downgrades. Assuming that voters associate voting downgrades with either a bad economy, or more debt and more expensive debt service, governors will prefer to avoid downgrades during electoral periods.

Both the CRAs and the state governments, then, might want to control the timing of rating downgrades despite regulations enjoining them from doing so. In order to analyze whether it is indeed the case in gubernatorial elections in Mexico we need to consider the CRAs' own accounts of how ratings are arrived. It could be that the observed changes in ratings just reflect different CRA methodologies. It turns out that all three international CRAs consider similar factors to assign their grades. These factors include: 1) ability to increase taxes and tax base, 2) transparency, control, and monitoring of the budget, 3) liquidity, 4) operating margin, 5) debt burden, 6) share of interest payments as percentage of total expenditure, 7) interest rates, 8) debt maturities, 9) debt purpose, 10) risk controls, 11) evaluation of the local economy, 12) expenditure flexibility and, 13) support from the upper level of government they would receive in case of financial distress.

[1] State debt in Mexico is peculiar since most of it is not sold as bonds to the public, but to banks through private credit agreements. It is customary in these credit agreements to have a variable rate that is a function of a base rate (usually close to inflation), plus a surcharge that is a function of the state rating. These are usually specified in a table in the credit contract. When there is a rating downgrade current interest payments go up, reducing the resources available to perform other governmental activities.

Clearly we will need to incorporate data that proxy for these factors in our analysis. Several of these variables are likely correlated with any political budget cycle that might be present, and so changes in ratings might be responding to these indicators of public finance and not to the electoral calendar by itself. Moreover, as a byproduct of the analysis of the political budget cycles, it is possible to see which indicators the CRAs are responding to in assigning their ratings.

To test my hypotheses concerning political credit rating cycles, I collected ratings data for the three biggest CRAs regarding the non-guaranteed risk of repayment of each state in Mexico. Non-guaranteed obligations are those that are paid from the general budget of the state. In contrast, structured debt stands for the obligations that are guaranteed to be paid by drawing upon revenue from a particular source (i.e. vehicle tax). These revenues flow into a trust, and the trust has the obligation to pay the lenders. Non-guaranteed debt better reflects the current status of the budget of the state since even relatively risky states can build a strong trust to increase the rating of a particular asset. They cannot do so with the non-guaranteed debt. Moreover, most short term debt is non-guaranteed. I also consider the national scale rating to compare the risk of default of the state to the sovereign risk of sovereign default of Mexico. This means that if Mexico as a country increases or decreases its default risk, the default risk of the states will not change. This is necessary to isolate their changes from the aggregate economic environment of the country. The source of these data is the websites of the three international CRAs that operate in Mexico (Fitch, 2015, Moody's, 2015, S&P, 2015).

(Table 4.3 found at the end of this chapter)

Mexican states have a relatively recent history of issuing debt, and thus a relatively recent history of credit ratings. The first two state ratings in Mexico were published in 1999, but diffusion was rapid. By December 2000, 21 states had a rating. Today,

every state is rated by at least one agency. 10 states are rated by only one CRA, 17 are rated by two, and 4 are rated by all three CRAs. If we translate the rating scale linearly (see Table 4.3), the mean and median ratings are A, and has a standard deviation of two notches ($AA-$ to $BBB+$). The lowest rating in the sample was a B+ for Quintana Roo after 2012[2] and the highest AA+ that has been reached by five states in different periods. The evolution of the rating average has decreased slightly in the last decade, from A+ in 2007 to A at the beginning of 2015. The sharpest decrease occurred from May 2010 to June 2012.[3]

(Figure 4.1 found at the end of this chapter)

From 2005 to 2015 there were a total 117 changes in state credit ratings. The number of changes is asymmetric: only four states had a constant rating, and five experienced only one. In contrast, two states had ten changes in ratings.

(Figure 4.2 found at the end of this chapter)

Perhaps the most important component of the rating determination is public debt. The total debt of states in Mexico was about 30 billion dollars at the end of 2014. This amount corresponds to 2.9% of the GDP of Mexico. Although small by some standards, subnational debt is a major topic of concern in Mexico. Debt has increased twofold in six years, and has a growth rate of 12% annually (Diaz, 2015). I include debt levels for the 31 states in Mexico, from the third quarter of 2005 to the first quarter 2015 with data gathered from the Mexican Secretariat of Finance and Public Credit. The Secretariat incorporates information from various sources, including the self-reported debt by the states, banks, and diverse lenders (SHCP, 2015).

[2]There was a default for the state of Jalisco in December 2012 (getting a rating of D by Fitch Ratings), but it was quickly paid, so it did not reach the quarterly cutoff used in this chapter.

[3]Note that this decrease was in a period of growth in Mexico, and oil prices were still high.

In addition to the debt data, I also calculated the yearly budget balance defined as the total revenue minus total expenditure. Chronic deficits in the public finances of a state should increase the risk of default, and generates pressure to increase short and long term public debt. In Mexico only about 3% of total public revenue is generated at the state and municipal level, but about 52% of total public expenditure is made by these levels of government (Diaz, 2015). In other words, almost all resources the state uses for their operation and public investments come from federal transfers (Langston, 2010). Therefore, the largest impact on ratings that is accountable to the state government is on the expenditure side, so it is worthwhile to also track expenditures separately in order to perform an accurate analysis.

I also considered other types of budget measures, including operating balance (which includes only operating expenditures of the government) and balance before public investment (after paying interests and capital). Public investments are a very different type of expenditure from operating expenditure, mainly for two reasons. First, it is expected that public investments can increase government revenue in the future, either directly (construction of a toll road), or indirectly by promoting growth or welfare of the inhabitants of the state. Second, in case of financial distress it is easier to reduce investments than to reduce wages or to fire public employees. The National Institute of Geography and Statistics (INEGI) classifies public expenditure according to its destination, and provides for each state series of public investments that are comparable among them. I will use these series to control for public investments, and normalize it by the size of the state budget.

Another component considered by the CRAs is the amortization schedule of the debt. Although related to the amount of debt the state has, it should be considered by itself. Even if the amount of debt is not large, if it is very short term the risk of default increases. These schedules are not public, but it is possible to use the amount of public expenditure directed toward debt service as a proxy. If the slope of

the schedule is very aggressive, we should see large portions of the state expenditure used to service debt. I incorporate the series of debt expenditure published by INEGI (2015), and normalize the amount of expenditure by revenue.

CRAs also take into account the state of the economy, as a healthy economy is needed in order to derive steady revenue from taxation. I use real change in state GDP and state unemployment, also published by INEGI (2015).

Finally, all CRAs agree that entities without transparency are to be penalized in their rating. Unfortunately there is no official index of transparency, nor do the CRAs publish this qualitative characteristic of the states. However, a well-known consulting company in Mexico, ARegional, has been publishing an index of fiscal transparency for the states in Mexico since 2006. This index is called the *Indice de Transparencia y Disponibilidad de la Informacion Fiscal de las Entidades Federativas* (ITDIF). It is published yearly, and assigns a rating between 0 and 100 to each state (Aregional, 2015).

4.4 Model

Here I propose a parsimonious rating game that incorporates the incentives of the CRAs and the officials discussed in the previous sections. The objective of this model is to illustrate when the government has a credible threat of substituting a CRA, and when the CRA panders and assigns a higher rating than it should during the electoral period.

I model three players: Nature, a CRA, and the Government. The setup and timing of the model goes as follows: (1) Nature assigns high (α) or low ($1-\alpha$) quality to the government. (2) The CRA receives a signal $s \in \{h, \ell\}$ with precision of ρ regarding the quality of the Government, and assigns high (r_h) or low rating (r_ℓ). (3) The Government decides either to keep the rating, or withdraw it and pay $C > 0$ to

another CRA to get a new rating. (4) In case the first rating was withdrawn, a second CRA assigns high rating with probability $\gamma \sim \Gamma(\cdot)$, or low rating with probability $1-\gamma$. (5) Nature reveals (δ), or not ($1-\delta$) the true state of the government. (6) The election is held. If the true state was revealed it is used to determine the outcome of the election. Otherwise the most recent rating is used.

This model has several implicit assumptions: (i) the Government needs to have a rating; (ii) voters consider the face value of the rating, so they don't do rating unraveling, implying that the reelection probability is ceteris paribus higher when having a higher rating; (iii) the Government can withdraw the rating without any electoral penalty, but only has enough time to replace the CRA once before the election; (iv) the Government has the belief that the other CRA has a probability $\gamma \sim \Gamma(\cdot)$ of assigning a high rating, and $1-\gamma$ of assigning a low rating; (v) it is cheaper in monetary terms to maintain a rating than to get a new one ($C > 0$); (vi) all players are risk neutral.

Payoffs

If the CRA doesn't get replaced, it receives the continuation value M. If the CRA lies about the true state of the government and the true state is revealed, it incurs in a reputation cost F.

If the Government gets reelected it gets a payoff of 1, otherwise it receives 0. In both cases, if it replaced the CRA it incurs in the cost C. If the true quality of the Government is revealed by the Nature, the rating is not considered in the reelection probabilities: if the Government is high quality it gets reelected with probability $p(r_h)$. If it is low quality it gets reelected with probability $p(r_\ell)$.

If the true state of the world is not revealed by Nature, the reelection probability depends on the assigned rating. Let $p(r_h)$ be the probability the incumbent gov-

ernment gets reelected given that the CRA assigned a high rating, and let $p(r_\ell)$ be the probability of being reelected given that the CRA assigned the low rating. By assumption (ii), $p(r_h) > p(r_\ell)$.

Theorem

The CRA is more likely to manipulate ratings if its remuneration is high, switching costs are low, the election is competitive and the result can switch with the rating, if the probability of revealing the state[4] is low, the reputation cost is low, and if the size of the difference between the precision of the signal and the prior is small.

Proof

This game can be solved by backward induction. Since the Government plays after the CRA, I will start by solving the Government problem first.

Government

The Government observes the assigned rating, and decides to keep or withdraw rating. It is straightforward to see that if it is a high rating, the government will keep it. Therefore I will focus on the case where it received a low rating.

If the government keeps the low rating it gets $\delta p(\cdot) + (1-\delta)p(r_\ell)$. If it withdraws it gets $\delta p(\cdot) + (1-\delta)(\gamma p(r_h) + (1-\gamma)p(r_\ell)) - C$.[5] Therefore, the government will keep the rating if $\gamma < \frac{C}{(1-\delta)(p(r_h)-p(r_\ell))}$. This inequality is intuitive, as the government is more likely to withdraw the rating: (I) The more lenient the second CRA is. (II) $p(r_n) - p(r_\ell)$ is large (marginal electoral benefit of a high rating). This means that if the government knows that is going to win or lose for sure, or the marginal

[4] This can also be interpreted as the probability the CRA is caught lying.

[5] Replace $p(\cdot)$ with $p(r_h)$ or $p(r_\ell)$ depending on the government has high or low quality, the same inequality holds for both cases.

benefit of a higher rating is negligible, then it will not withdraw the rating. (III) The probability the true state of the world (δ) is revealed is small (marginal benefit of having a rating), since the CRA only adds value when the true state is opaque.

CRA

The next step in the backward induction process is to take the rating withdrawal cutoff from the Government, incorporate it in the payoffs of the CRA, and solve for the optimal rating assigning strategy. Recall the precision of the signal (s) the CRA receives about the true state of the world is ρ. That is, $pr(s_w = w) = \rho$, with $w \in (h, \ell)$. Also recall the prior of state h is α, and state ℓ is $1 - \alpha$. Therefore, the posteriors are the following:

$pr(h|s_h) = pr(\ell|s_\ell) = \frac{\alpha\rho}{\alpha\rho+(1-\alpha)(1-\rho)}$ and $pr(h|s_\ell) = pr(\ell|s_h) = \frac{\alpha(1-\rho)}{\alpha(1-\rho)+\rho(1-\alpha)}$

CRA observes high signal

If the CRA gives a high rating it has an expected payoff of:

$$M + \left(\frac{(1-\alpha)(1-\rho)}{\alpha\rho + (1-\alpha)(1-\rho)}\right)(\delta)(-F)$$

If the RA gives low rating the expected payoff is:

$$\Gamma\left(\frac{C}{(p(r_h) - p(r_\ell))(1-\delta)}\right)M + \left(\frac{\rho\alpha}{\alpha\rho + (1-\alpha)(1-\rho)}\right)(-F)\delta$$

Therefore it assigns high rating if

$$M\left(1 - \Gamma\left(\frac{C}{(p(r_h) - p(r_\ell))(1-\delta)}\right)\right) - \delta F\left(\frac{1-\alpha-\rho}{\alpha\rho + (1-\alpha)(1-\rho)}\right) > 0 \qquad (4.1)$$

CRA observes observe low signal

If it assigns high rating its expected payoff is:

$$M - \delta F\left(\frac{\rho(1-\alpha)}{\alpha(1-\rho)+\rho(1-\alpha)}\right)$$

If it assigns low rating its expected payoff is:

$$M\Gamma\left(\frac{C}{(p(r_h)-p(r_\ell))(1-\delta)}\right) - \delta F\left(\frac{\rho(1-\alpha)}{\alpha(1-\rho)+\rho(1-\alpha)}\right)$$

Therefore, the RA will assign a high rating if:

$$M\left(1-\Gamma\left(\frac{C}{(p(r_h)-p(r_\ell))(1-\delta)}\right)\right) - \delta F\left(\frac{\rho-\alpha}{\alpha(1-\rho)+\rho(1-\alpha)}\right) > 0 \qquad (4.2)$$

Results

From equations (4.1) and (4.2) above, I can derive some predictions from the model. The CRA is more likely to manipulate ratings if its remuneration (M) is high, switching costs (C) are low, the election is competitive and the result can switch with different ratings ($p(r_h) - p(r_\ell)$), if the probability of revealing the state[6] (δ) is low, the reputation cost (F) is low, and if the size of the lie ($\rho - \alpha$, how much more precise your information is with respect to the prior) is small. Also, the intuitive conclusion that it is more likely that the CRA assigns a high rating when it receives the high signal holds.

[6]This can also be interpreted as the probability the CRA is caught lying.

Two interesting predictions of the model are testable: only negative ratings will be postponed (positives are not), and the more competitive the election, the more likely we are to observe the postponement of negative ratings until after the election. From this two testable observations I derive the following hypotheses, which will be tested using the data described in the previous sections:

1. CRAs have incentives to delay rating downgrades for governments until the election has passed

2. This delay is more likely to occur if the election is very competitive

4.5 Empirical Analysis

The first indication that ratings announcements might be influenced by the electoral calendar is that we observe a disproportionately large number of rating downgrades after elections, compared to a relative large number of rating upgrades before elections; as we can see in Table 4.4, we observe rating upgrades twice as often in the year preceding the election than afterwards. Meanwhile, we observe more than twice the number of downgrades after the election than before the election.

(Table 4.4 found at the end of this chapter)

In Mexico, gubernatorial elections are not held at the same time in all states. Each state has its own electoral calendar, so it is possible to compare the average rating of states with and without election during the same period. Figure 4.3 considers for each election, state, and CRA its rating for the four quarters before and after the election compared to the average rating of all states in the same period. We can see that ratings are higher before elections. Average ratings between the fourth and second quarter before the election are 2%-3% higher, and this amount monotonically

decreases in time, reaching an average of 3% lower on the fourth quarter after the election.

(Figure 4.3 found at the end of this chapter)

So far the evidence is consistent with the hypothesis of political credit rating cycles. However, it is necessary to incorporate in the analysis the variables that the CRAs consider in their methodology to see if these variables account for this variation in ratings around the elections. Changes in ratings might reflect true changes in the creditworthiness of the state. These changes in creditworthiness can be measured using the indicators of public finance used by the CRAs themselves, and may reflect a political budget cycle.

Econometric analysis

In order to test for the existence of a political credit rating cycle, apart from any political budget cycle that may exist in the Mexican gubernatorial elections, I estimate the following equations:

$$\Delta Rating_{ijt} = \beta_1 preelection_{it} + \beta_2 postelection_{it} + \sum_k \beta_k X_{kit} + \epsilon_{ijt} \qquad (4.3)$$

$$Upgrade_{ijt} = \beta_1 preelection_{it} + \beta_2 postelection_{it} + \sum_k \beta_k X_{kit} + \epsilon_{ijt} \qquad (4.4)$$

$$Downgrade_{ijt} = \beta_1 preelection_{it} + \beta_2 postelection_{it} + \sum_k \beta_k X_{kit} + \epsilon_{ijt} \qquad (4.5)$$

In all equations the subindex i stands for state, the subindex $j \in \{Fitch, Moody's, S\&P\}$ for CRA, and t for the time period. For model 4.3, I use fixed effects model. In the other two equations, I use both a linear probability model and conditional logit model.

The dependent variable $\Delta Rating_{ijt}$ in the first equation is the first difference between the assigned rating to state i by CRA j in period t and period $t-1$. I assume the rating scale is linear, and assign a numerical value between 0 and 15 to each rating, where 15 corresponds to AAA (or Moody's Aaa), 14 is $AA+$ (or Moody's $Aa1$), and the other ratings are coded analogously (See Table 4.3). The rating assigned to period t is the rating the state had the last day of the quarter.

The other two dependent variables are dichotomous and reflect whether there was a rating upgrade or rating downgrade from one quarter to the next. This metric makes the results independent from the assumption of the linear values assigned to the rating scale. The variable $Upgrade_{ijt}$ takes the value of 1 if the rating for state i by CRA j at the end of period t is strictly larger than the rating at the end of period $t-1$, and the variable takes the value of 1, if the rating in period t is strictly lower than the rating in period $t-1$.

The coefficients of interest in these regressions are β_1 and β_2 . The coefficient β_1 estimates the change in the variable of interest attributed to being a pre-electoral period. The variable $preelection_{it}$ is dichotomous, and indicates whether period t corresponds to one of the four quarters preceding a gubernatorial election in state i. Analogously, β_2 estimates the change in the variable of interest attributed to being a post-electoral period. The variable $postelection_{it}$ indicates whether the observation corresponds to one of the four quarters after the gubernatorial election. According to my hypotheses, I expect β_1 to be positive and β_2 to be negative for the models. That is, I expect ratings to be higher before the election (or having an increased

probability of observing an upgrade before the election), and the rating to be lower after the election (or have a lower probability of observing an upgrade after the election). Conversely, I expect the opposite signs for downgrades. If we are going to observe a downgrade, it is more likely to be after the election has occurred.

I include as controls variables that the CRAs use to determine ratings, using variables X_{kit}. These controls include debt over revenue, increase in debt over revenue, percentage change of state GDP, fiscal balance over revenue, expenditure related to public investments over total expenditure, debt expenditure over total expenditure, and transparency. Without these controls we could potentially misattribute the change in ratings to the incentives of the CRA, when they could just be adjusting ratings due to actual changes in the status of public finances.

The expected signs of the parameters attributed to these controls are the ones that reflect the likelihood of the state to have the resources needed to pay its obligations. Thus, we expect increases in ratings to be positively correlated with a positive budget balance, an increase in revenue, a larger fraction of the budget directed toward public investments, and an increase in economic growth. Conversely, we expect that increases in debt, increase in expenditure, and deficits to be negatively correlated with credit ratings.

The final component of the model are fixed effects. I incorporate both $state \times rating$ agency, and time fixed effects. The $state \times rating$ agency fixed effects control for unobserved time invariant characteristics of the states. I am thus allowing each CRA to have a different time invariant rating criteria for each state they grade. The time fixed effects control for the unobserved trends that are common to all states in the country.

Empirical Results

Rating change and the elections

Results for rating changes and their relation to the electoral calendar are reported in Tables 4.6, 4.7, and 4.8. Table 4.6 shows the estimates of the model for first differences in ratings, according to the scale in Table 4.3. Tables 4.7 and 4.8 show the estimates for the other two models which specify the dependent variable as the dichotomous $upgrade$ and $downgrade$, respectively.

In all three tables we observe the same pattern during the pre-electoral period. There is no variation (either up or down) attributed to being in quarters before the election (see row $preelection$ in Tables 4.6, 4.7, and 4.8). All estimates in this period are statistically insignificant for both changes in rating and for the dichotomous models of upgrade and rating downgrade. This result is robust with and without controls, to the non-linearity of the model, and state $\times$ CRA and period fixed effects. This is consistent with what I expected, since there is no incentive to delay the rating when it is going to increase. On the other hand, it would also make sense for the CRA to make an additional effort to have increase in ratings before the election. Perhaps the reason we do not observe a positive estimation for preelectoral upgrades is the low number of upgrades (only ten) around the elections for this sample. Moreover, rating upgrades get less attention in the media compared to rating downgrades, making the marginal benefit of the adjustment in the rating smaller.

I expected to observe less rating downgrades in the preelectoral period compared to non-electoral years. That is, I expected the coefficient of $preelection$ in Table 4.8 to be negative, but the estimates are always indistinguishable from zero. It is possible this is a result of the very small number of rating changes in preelectoral period.

In the post-electoral period the results are strongly consistent with the hypothesis

that rating downgrades are postponed until after the election. The probability of observing a rating downgrade in the quarters after an election is higher than in the non-electoral quarters. In Table 4.6 we see that quarters after an election have lower ratings (.1 less notches) on average compared to non-electoral quarters. This estimation is robust to the introduction of controls and fixed effects. When I use the dichotomous variable *downgrade* instead of rating level I reach the same conclusion. The marginal effect of the variable $postelection$ in Table 4.8 indicates how much more likely it is to see a rating downgrade after an election.This variable is positive and statistically significant in almost all specifications. It is 2.5%-3.5% more likely to observe a rating downgrade for each quarter in the next four quarters afteran election compared to non-electoral quarters. This estimation might look small, but recall that the post-electoral period is defined as four quarters, so the probability compounds to 10%-15% yearly.

The variable $postpostelection$ is dichotomous, and takes the value of 1 if the observation is on the four quarters after a post electoral period, or equivalently, if the observation is on the quarters 5-8 posterior an election. This variable was included to observe whether the downgrade associated with an election was followed by a subsequent increase, suggesting mean reversion. The results do not indicate that this is the case. In almost all models, the variable $postpostelection$ goes in the expected direction of a mean reversion, but it is not sizable and does not reach statistical significance.

CRA methodology control variables

In general, the estimated coefficients for these controls go in the direction expected by the methodology of the CRAs. Increases in debt ($\Delta Debt/Revenue$) are associated with reductions in ratings: a decrease in .45 notches when debt over revenue

increases 100%(Table 4.6, columns 4 and 5). This is consistent when using the variable *upgrade*. The probability of observing a rating upgrade decreases 4% when debt over revenue increases 100% (Table 4.7, columns 4 and 5), and increased likelihood of having a rating decreases: it is 10% more likely to have a ratings downgrade when debt over revenue increases 100% (Table 4.8, columns 4 and 5).

Similarly, positive budget balances ($Balance/Revenue$) are associated with higher ratings (Table 4.6), increased probability of a rating upgrade (Table 4.7), and a decreased probability of a rating downgrade (Table 4.8). Having a larger percentage of total expenditure as public investment ($Investment/Exp$) is positively correlated with rating increases and negatively correlated with rating decreases (Tables 4.7 and 4.8 respectively).

There are also variables related to CRA methodology that do not appear to be significant in determining changes in ratings, upgrades, or downgrades. Change in state GDP ($\Delta\%GDP$) rarely reaches statistical significance in the proposed models. Total debt over disposable revenue ($Debt/Revenue$) is not significant for changes in rating levels and rating downgrades, although it is associated with increased likelihood of receiving a rating upgrade (Table 4.7).

Rating Changes and Election Competitiveness

As indicated above there are also incentives for the CRA to adjust ratings as a function of the competitiveness of the election. The threat of withdrawing a bad rating is much more serious, which means that CRAs face additional pressure to delay ratings downgrades. We thus expect it to be more likely to observe a delay in a downgrade when the election is competitive.

To test this prediction, I partition the sample of elections according to the closeness

of the results.[7] I consider an election as close if the difference between the winner and the runner-up was within 10% of the valid (non-null) votes.[8] Analogously, an election is defined to be a landslide if the difference between the first and second place was more than 10%. The median difference in this sample between the first and second place is 7.32%. As it turns out, my results are robust to the alternative definition of closeness.

We observe the predicted result in the data, even when controlling for variables that could be affected by the political budget cycle, and which could also be affected by the competitiveness of the election. Consider first the model that incorporates the change in rating level as the dependent variable (Table 4.9). Here the variables of interest, $preelection$ and $postelection$, include their interactions with the closeness of the election. The main result is consistent with our hypothesis. We observe more rating decreases in the period after the election when elections are competitive. Moreover, this decrease is robust to the covariates attributed to the political rating cycle, while for landslide elections it is not (Compare $Close * Postelection$ and $Landslide * Postelection$ in columns 4 and 5 in Table 4.9). I estimate that the decrease in rating for the post electoral period for close elections is around .12 notches. This result persists after specifying fixed effects.

The dichotomous variable equations produce similar results. There is an increased likelihood of observing a downgrade in postelectoral periods of close elections. In the linear probability model (Table 4.11, columns 1 and 2) the probability of observing a rating downgrade when the election was close, everything else constant, is 3.7-4.4% higher than in a non-electoral period.

[7] According to my assumptions, a change in ratings might change the electoral result, so it would be better to have poll data to better estimate the incentives of the CRA to adjust the change in ratings. This information is not available, so I use the actual electoral results as a substitute.

[8] Recall that Mexico has a multi-party system.

Competitiveness and CRA methodology as controls

The coefficients estimated here for other covariates do not significantly change from the baseline model, i.e. without electoral results. Increases in debt are associated with decreases in ratings, and, when using the dichotomous variables, there is decreased probability of seeing rating upgrades, and increased probabilities of rating downgrades. More favorable budget balances are associated with increases in ratings and, when using dichotomous variables, increased probabilities of rating upgrades, and decreased probabilities of rating downgrades. Also, if a large percentage of the total expenditure is used to finance public investments it is less likely to observe rating downgrades. The variables that were not correlated with changes in ratings, e.g. change in state GDP, transparency, and expenditure in debt in the baseline model, are still not correlated when I control for competitiveness of the election.

Differences by CRA

The results suggest that there is heterogeneity among CRAs and the political credit rating cycle. As we see in Table 4.12, Moody's and Fitch are more likely to downgrade ratings after an election. Standard & Poor's, in contrast, seems to change its ratings independently of the electoral calendar. Note that for this exercise we have to use random effects, since the fixed effects would be collinear with the CRA variables.

Unexpected change in party, change in risk?

Another possible explanation of the downgrades after the election could be that CRAs might not expect the results of the election, and that they downgrade in reaction to the electoral surprises. In order to test this, I compare the number of rating downgrades after the election when the incumbent party lost the election by a

relatively small margin to the same case when the incumbent party won the election. If the aforementioned hypothesis is correct, we should observe a larger number of rating downgrades when the incumbent party lost the election.

To test this, I use the same bandwidth of the previous section (10%) to define a close election. The distribution of the plurality of elections in the sample can be seen in Figure 4.4. The shaded regions in the figure show the number of quarters with close elections considered to generate Table 4.5.

(Figure 4.4 found at the end of this chapter)

As we can see in Table 4.5 changes in party are not driving my results. In fact, only three downgrades in the post electoral period correspond to a change in party for close elections. For most of of the downgrades in competitive elections the incumbent party retained control of the government. Therefore we can conclude that it is not an unexpected change in the party in power what it is triggering rating downgrades after the election.

(Table 4.5 found at the end of this chapter)

4.6 Conclusions, Policy Implications, and Further Research

This chapter extends the literature of the conflict of interest of CRAs to the public finance and political economy domain. I conclude that CRAs are delaying rating downgrades during electoral periods, especially if the election is very competitive.

My empirical results using data from gubernatorial elections in Mexico between 2005 and 2015 suggest that there is an increased probability of rating downgrades after the election, which cannot be explained by the variables mentioned in rating methodologies. I conclude that quarters posterior to an election decrease .1 notches

on average compared to non-electoral quarters. Similarly, I concluded that it is 2.5%-3.5% more likely to observe a rating downgrade for each quarter in the next four quarters posterior to an election compared to non-electoral quarters.

Regarding the competitiveness of the election, my empirical results show that the decrease in rating after the election is more pronounced for close (competitive) elections. I estimate that the decrease in rating for the post electoral period for close elections is around .14 notches, while for landslide elections it is not statistically significant. Similarly, the probability of observing a rating downgrade when the election was close is (everything else constant) about 4% higher than in a non-electoral period.

These results are in line with the hypothesis that there exists a conflict of interest when the CRA rates a government that is paying for its services. The solution to this problem is not easy. Regulations like paying upfront the cost of rating would not prevent hidden arrangements between raters and clients. In corporate finance some authors like Mathis et al. (2009) have proposed the creation of a centralized depository or contract platform, who would be in charge of both hiring the CRAs, and be requested and be paid for a rating. In this case the agency would remove the direct contact between clients and CRAs. The problem with this approach in this case is that the clients are governments themselves, so the centralized depository would itself have the conflict of interest in first place. If the depository were to be run by the federal government, there would be still partisan bias towards governors who are from the same party of the president.

I propose making it mandatory to be rated by all CRAs for all states. In practice, this would be on average incorporating a little more than one agency per state, and the cost of an additional rating is negligible compared to the total state budget. Since it has been shown that the ratings are indeed informative for taking investment

decisions, the potential social benefits would be much larger than the cost. In this case, the threat of removing the CRA would be eliminated, with only the reputation cost to provide accurate ratings remaining.

This research could be extended to other countries at the state or province level, in particular Colombia, Brazil, and Argentina since their subnational governments are rated as well. We expect conflicts of interest to be less monitored than in developed countries. However, developed countries can also be a subject of study; for example in the US. US states have in general very high and stable ratings, so there is not enough variance to apply the same method I did in the Mexican case. However, the lower levels of government have much more variance, and constantly refinance their debt and sell bonds on the open market.

The continuous study of CRAs and governments is necessary to make sure that this relatively new player in the democratic process is fulfilling its role as an independent monitor and it is able to transmit accurate and unbiased information to voters and investors.

4.7 Tables

Table 4.1: Rated Securities by CRA

CRA	Government Securities	Other Securities	Total
S&P	970,200	206,000	1,176,200
Moody's	673,166	168,253	841,419
Fitch	194,086	107,066	301,152
DBRS	16,650	25,552	42,202
KBRA	37	20,340	20,377
EJR	0	19,994	19,994
A.M. Best	0	9,462	9,462
Morningstar	0	5,542	5,542
JCR	399	3,070	3,469
HR Ratings	277	0	277
Total	1,854,815	565,279	2,420,094

Table 4.2: Mexican Rated Securities by CRA

CRA	Government Securities	Other Securities	Total
Fitch	483	1,319	1,802
S&P	135	861	996
Moody's	221	467	688
Total	839	2,647	3,486

Table 4.3: Rating Equivalences

Value	Fitch	Moody'sSP	
15	AAA	Aaa	AAA
14	AA+	Aa1	AA+
13	AA	Aa2	AA
12	AA-	Aa3	AA-
11	A+	A1	A+
10	A	A2	A
9	A-	A3	A-
8	BBB+	Baa1	BBB+
7	BBB	Baa2	BBB
6	BBB-	Baa3	BBB-
5	BB+	Ba1	BB+
4	BB	Ba2	BB
3	BB-	Ba3	BB-
2	B+	B1	B+
1	B	B2	B
0	B-	B3	B-

Table 4.4: Changes in Rating by Period

	Pre-election	**Post-election**
Rating Upgrade	7	3
Rating Downgrade	8	21

Table 4.5: Changes in Rating by Incumbency

	Pre-election	**Post-election**
Incumbent Lost		
Rating Upgrade	6	0
Rating Downgrade	2	3
Incumbent Won		
Rating Upgrade	1	2
Rating Downgrade	4	11

Table 4.6: Estimations - Changes in Rating

	Dependent variable:				
	Δ State Rating				
	(1)	(2)	(3)	(4)	(5)
Preelection	−0.030	−0.031	−0.016	−0.022	−0.015
	(0.026)	(0.026)	(0.028)	(0.028)	(0.030)
Postelection	−0.130***	−0.128***	−0.082***	−0.109***	−0.092***
	(0.027)	(0.028)	(0.030)	(0.030)	(0.032)
Postpostelection	0.007	0.006	0.054*	0.006	0.022
	(0.027)	(0.028)	(0.030)	(0.028)	(0.031)
Debt/Revenue				0.063	0.071*
				(0.040)	(0.041)
ΔDebt/Revenue				−0.453***	−0.474***
				(0.099)	(0.102)
Δ%GDP				−0.430	−1.031*
				(0.346)	(0.592)
Balance/Revenue				1.287***	1.128***
				(0.204)	(0.214)
Investment/Exp				0.298	0.349
				(0.238)	(0.280)
DebtExp/Exp				0.023	0.009
				(0.030)	(0.031)
Transparency				−0.002**	−0.001*
				(0.001)	(0.001)
Observations	2,132	2,132	2,132	1,985	1,985
State*RA FE	N	Y	Y	Y	Y
Period FE	N	N	Y	N	Y
Adjusted R^2 (within)	0.011	0.011	0.007	0.054	0.045
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 4.7: Estimations - Upgrades

	Dependent variable:						
	Upgrade						
			panel linear			*panel conditional logit*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Preelection	−0.007	−0.005	0.001	−0.003	0.001	-0.102	0.633
	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)	(0.453)	(0.581)
Postelection	−0.018*	−0.019*	−0.007	−0.016	−0.007	-0.966	-0.530
	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.655)	(0.817)
Postpostelection	−0.006	−0.008	0.003	−0.012	−0.002	-0.652	-0.408
	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.476)	(0.583)
Debt/Revenue				0.037**	0.039***	1.407**	1.184
				(0.014)	(0.015)	(0.711)	(0.905)
ΔDebt/Revenue				0.016	0.024	1.789	2.087
				(0.036)	(0.037)	(1.455)	(2.078)
Δ%GDP				0.089	−0.097	2.335	-8.005
				(0.126)	(0.215)	(5.590)	(12.728)
Balance/Revenue				0.251***	0.186**	13.222***	14.002 **
				(0.074)	(0.078)	(4.272)	(6.678)
Investment/Exp				0.157*	0.142	8.556 *	12.736 *
				(0.087)	(0.102)	(4.360)	(6.138)
ExpDebt/Exp				0.011	0.006	0.107	0.516
				(0.011)	(0.011)	(0.409)	(1.049)
Observations	2,133	2,133	2,133	1,985	1,985	1,078	1,078
State*RA FE	N	Y	Y	Y	Y	Y	Y
Period FE	N	N	Y	N	Y	N	Y
Adjusted R^2	0.002	0.002	0.0003	0.012	0.009		

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4.8: Estimations - Downgrades

	Dependent variable:						
	Downgrade						
			panel linear			*panel conditional logit*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Preelection	−0.001	−0.001	−0.008	−0.005	−0.006	-0.301	-0.418
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.454)	(0.509)
Postelection	0.046***	0.044***	0.025**	0.035***	0.027**	0.626*	0.313
	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.346)	(0.398)
Postpostelection	0.001	−0.001	−0.019	−0.006	−0.012	-0.364	-0.544
	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.436)	(0.470)
Debt/Revenue				0.005	0.004	-0.459	-0.476
				(0.016)	(0.017)	(0.438)	(0.493)
ΔDebt/Revenue				0.105***	0.111***	0.617	2.208*
				(0.041)	(0.042)	(0.690)	(1.149)
Δ%GDP				0.008	0.148	-1.879	7.232
				(0.142)	(0.244)	(4.647)	(8.260)
Balance/Revenue				−0.471***	−0.417***	-8.016***	-4.844*
				(0.084)	(0.088)	(2.615)	(2.752)
Investment/Exp				−0.176*	−0.255**	-4.482	-10.407**
				(0.098)	(0.116)	(3.436)	(4.709)
ExpDebt/Exp				−0.011	−0.006	-0.325	0.008
				(0.012)	(0.013)	(0.334)	(0.437)
Observations	2,133	2,133	2,133	1,985	1,985	1,049	1,049
State*RA FE	N	Y	Y	Y	Y	Y	Y
Period FE	N	N	Y	N	Y	N	Y
Adjusted R^2	0.009	0.008	0.005	0.038	0.031		

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4.9: Estimations- Changes in Rating by Competitiveness

	Dependent variable:				
	Δ State Rating				
	(1)	(2)	(3)	(4)	(5)
Close*Preelection	−0.008	−0.008	−0.011	−0.015	−0.014
	(0.029)	(0.030)	(0.032)	(0.032)	(0.033)
Landslide*Preelection	−0.094**	−0.093**	−0.069	−0.048	−0.037
	(0.045)	(0.046)	(0.048)	(0.050)	(0.052)
Close*Postelection	−0.119***	−0.115***	−0.093***	−0.124***	−0.114***
	(0.032)	(0.033)	(0.035)	(0.034)	(0.035)
Landslide*Postelection	−0.155***	−0.158***	−0.112**	−0.078	−0.063
	(0.044)	(0.047)	(0.049)	(0.052)	(0.054)
Debt/Revenue				0.061	0.070*
				(0.040)	(0.041)
ΔDebt/Revenue				−0.453***	−0.475***
				(0.099)	(0.102)
Δ%GDP				−0.451	−1.064*
				(0.347)	(0.588)
Balance/Revenue				1.300***	1.160***
				(0.207)	(0.218)
Investment/Exp				0.297	0.341
				(0.238)	(0.282)
DebtExp/Exp				0.023	0.010
				(0.030)	(0.031)
Observations	2,132	2,132	2,132	1,985	1,985
State*RA FE	N	Y	Y	Y	Y
Period FE	N	N	Y	N	Y
Adjusted R^2 (within)	0.013	0.012	0.006	0.054	0.045

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4.10: Estimations- Upgrades by Competitiveness

	Dependent variable:			
	Upgrade			
	panel linear		*conditional logit*	
	(1)	(2)	(3)	(4)
Close*Preelection	0.011	0.012	0.395	0.813
	(0.012)	(0.012)	(0.486)	(0.601)
Landslide*Preelection	−0.028	−0.024	-13.560	-16.591
	(0.018)	(0.019)	(627.130)	(5039.782)
Close*Postelection	−0.013	−0.009	-0.849	-0.800
	(0.012)	(0.013)	(0.782)	(0.913)
Landslide*Postelection	−0.014	−0.006	-0.571	0.870
	(0.019)	(0.020)	(1.102)	(1.271)
Debt/Revenue	0.035**	0.037**	1.283	1.013
	(0.015)	(0.015)	(0.702)	(0.907)
ΔDebt/Revenue	0.018	0.025	1.770	2.477
	(0.036)	(0.037)	(1.427)	(2.148)
Δ%GDP	0.077	−0.107	3.563	-7.029
	(0.127)	(0.214)	(6.054)	(12.973)
Balance/Revenue	0.237***	0.178**	12.699***	14.212**
	(0.076)	(0.079)	(4.268)	(6.691)
Investment/Exp	0.167*	0.153	7.920*	12.229**
	(0.087)	(0.102)	(4.349)	(6.067)
Observations	1,985	1,985	1,078	1,078
State*RA FE	Y	Y	Y	Y
Period FE	N	Y	N	Y
Adjusted R^2 (within)	0.012	0.009		

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4.11: Estimations- Downgrades by Competitiveness

	Dependent variable:			
	Downgrade			
	panel linear		*conditional logit*	
	(1)	(2)	(3)	(4)
Close*Preelection	0.007	0.009	0.127	0.153
	(0.013)	(0.014)	(0.503)	(0.551)
Landslide*Preelection	−0.029	−0.034	-1.055	-1.806
	(0.020)	(0.021)	(0.884)	(1.105)
Close*Postelection	0.044***	0.037**	1.034***	0.913**
	(0.014)	(0.015)	(0.379)	(0.415)
Landslide*Postelection	0.018	0.012	0.037	-0.537
	(0.021)	(0.022)	(0.605)	(0.695)
Debt/Revenue	−0.006	−0.006	-0.451	-0.507
	(0.023)	(0.023)	(0.440)	(0.503)
ΔDebt/Revenue	0.106***	0.114***	0.608	2.315
	(0.041)	(0.042)	(0.691)	(1.166)
Δ%GDP	0.005	0.139	-2.485	5.752
	(0.143)	(0.243)	(4.639)	(8.342)
Balance/Revenue	−0.496***	−0.447***	-8.908***	-7.076**
	(0.085)	(0.090)	(2.658)	(2.951)
Investment/Exp	−0.166*	−0.233**	-3.533	-8.489 *
	(0.098)	(0.116)	(3.492)	(4.745)
Observations	1,985	1,985	1,049	1,049
State*RA FE	Y	Y	Y	Y
Period FE	N	Y	N	Y
Adjusted R^2 (within)	0.038	0.031		

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4.12: Estimations by CRA

	Dependent variable:		
	ΔRating	Upgrade	Downgrade
	(1)	(2)	(3)
Preelection×Fitch	−0.014	−0.012	−0.001
	(0.043)	(0.016)	(0.018)
Preelection×Moodys	0.021	−0.007	−0.009
	(0.043)	(0.016)	(0.018)
Preelection×SP	−0.052	0.022	−0.005
	(0.055)	(0.021)	(0.023)
Postelection×Fitch	−0.181***	−0.016	0.072***
	(0.046)	(0.017)	(0.019)
Postelection×Moodys	−0.086**	0.002	0.042**
	(0.043)	(0.016)	(0.018)
Postelection×SP	−0.013	−0.035	−0.023
	(0.062)	(0.023)	(0.026)
Postpostelection×Fitch	−0.034	−0.017	0.005
	(0.044)	(0.016)	(0.018)
Postpostelection×Moodys	0.069	0.008	−0.004
	(0.042)	(0.016)	(0.018)
Postpostelection×SP	−0.026	−0.009	−0.007
	(0.059)	(0.022)	(0.025)
Observations	1,985	1,985	1,985
Other covariates	Y	Y	Y
FE	N	N	N
R^2	0.098	0.016	0.063
Adjusted R^2	0.097	0.015	0.062
Note:		*p<0.1; **p<0.05; ***p<0.01	

4.8 Figures

Figure 4.1: Rating Distributions

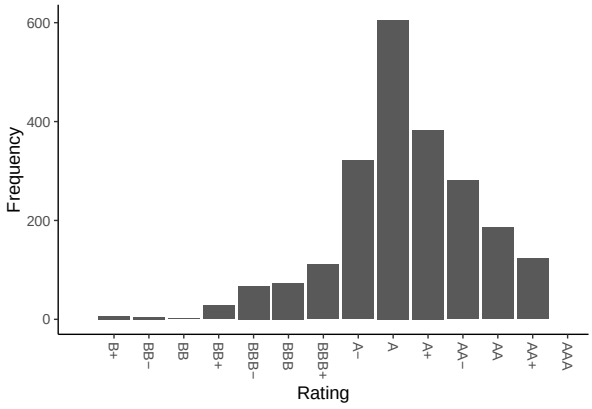


Figure 4.2: Timeline - Rating Average

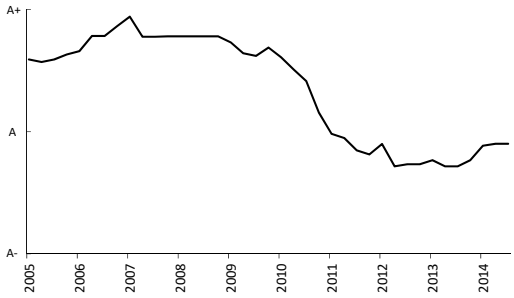


Figure 4.3: Rating Deviations in the Electoral Cycle

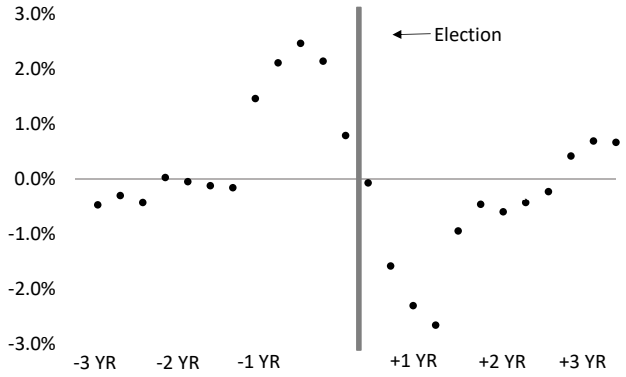
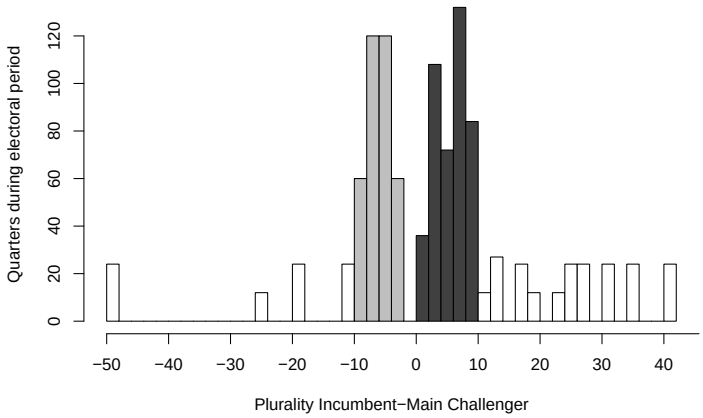


Figure 4.4: Distribution of Electoral Competitiveness



BIBLIOGRAPHY

AICPA American Institute of Certified Public Accountants, Inc (2015). *Overall Objectives of the Independent Auditor and the Conduct of an Audit in Accordance With Generally Accepted Auditing Standards. SAS No. 122; SAS No. 123; SAS No. 128*.

Alm, James and Mark Evers (1991). "The item veto and state government expenditures". *Public Choice* 68.1, pp. 1–15.

Alt, James, Ethan Bueno de Mesquita, and Shanna Rose (2011). "Disentangling Accountability and Competence in Elections: Evidence from U.S. Term Limits". *The Journal of Politics* 73.1, pp. 171–186.

Alt, James and Shanna Rose (2007). "Context Conditional Political Budget Cycles." In: Boix, Carles and Susan C. Stokes. *The Oxford Handbook of Comparative Politics*. Oxford University Press, pp. 62–97.

Anzia, Sarah F. and Terry M. Moe (2013). "The Politics of Pensions". SSRN Scholarly Paper ID 2300640.

Aregional (2015). *Indice de Transparencia y Disponibilidad de la Informacion Fiscal de las Entidades Federativas: Resumen Ejecutivo*. From Aregional website, http://www.aregional.com/.

Bails, Dale G. (1990). "The Effectiveness of Tax-Expenditure Limitations: A Reevaluation". *American Journal of Economics and Sociology* 49.2, pp. 223–238.

Becker, Bo and Todd Milbourn (2011). "How did increased competition affect credit ratings?" *Journal of Financial Economics* 101.3, pp. 493–514.

Blais, Andre and Richard Nadeau (1992). "The electoral budget cycle". *Public Choice* 74.4, pp. 389–403.

Block, Steven A. and Paul M. Vaaler (2004). "The price of democracy: sovereign risk ratings, bond spreads and political business cycles in developing countries". *Journal of International Money and Finance* 23.6, pp. 917–946.

Bolton, Patrick, Xavier Freixas, and Joel Shapiro (2012). "The Credit Ratings Game". *The Journal of Finance* 67.1, pp. 85–111.

Boylan, Richard T. (2008). "Political distortions in state forecasts". *Public Choice* 136.3, pp. 411–427.

Brown, Jeffrey R., Robert Clark, and Joshua Rauh (2011). "The Economics of State and Local Public Pensions". National Bureau of Economic Research (w16792).

CalPERS, California Public Employees' Retirement System (2016). *Total Fund Investment Policy*. From CalPERS' website https://www.calpers.ca.gov. (visited on 2017-04-01).

CalSTRS, California State Teachers' Retirement System (2016). *Investment Policy and Management Plan*. From CALSTRS' website https://www.calstrs.com/. (visited on 2017-04-01).

Chubb, John E. (1988). "Institutions, the Economy, and the Dynamics of State Elections". *American Political Science Review* 82.1, pp. 133–154.

Clemens, Jeffrey and Stephen Miran (2012). "Fiscal Policy Multipliers on Subnational Government Spending". *American Economic Journal: Economic Policy* 4.2, pp. 46–68.

Cox, James and David Lowery (1990). "The impact of the tax revolt era state fiscal caps". *Social Science Quarterly* 71.3, pp. 492–509.

Damodaran, Aswath (2017). *Historical returns: Stocks, T.Bonds & T.Bills with premiums*. Damodaran Online. URL: http://people.stern.nyu.edu/adamodar/New_Home_Page/datafile/histretSP.html (visited on 05/01/2017).

Diaz, Diego (2015). *Deuda publica en los estados*. IMCO, Instituto Mexicano para la Competitividad AC. URL: http://imco.org.mx/politica_buen_gobierno/deuda-publica-en-los-estados/ (visited on 09/10/2015).

Dobra, Matt and Bruce H. Lubich (2013). "Public Pension Governance and Asset Allocation". SSRN Scholarly Paper ID 2236469.

Drazen, Allan (2001). "The Political Business Cycle After 25 Years". *NBER Macroeconomics Annual* 15, pp. 75–117.

Eaton, Tim V. and John R. Nofsinger (2004). "The effect of financial constraints and political pressure on the management of public pension plans". *Journal of Accounting and Public Policy* 23.3, pp. 161–189.

Elder, Erick M. and Gary A. Wagner (2015). "Political effects on pension underfunding." *Economics & Politics* 27.1, pp. 1–27.

Ellis, Michael A. and D. Eric Schansberg (1999). "The Determinants of State Government Debt Financing". *Public Finance Review* 27.6, pp. 571–587.

Fitch, Ratings Mexico (2015). *Finanzas Publicas, Estados*. URL: http://www.fitchratings.mx/sector/FinanzasPublicas/Edos/default.aspx (visited on 09/10/2015).

Flynn, Sean and Andra Ghent (2017). "Competition and credit ratings after the fall". *Management Science (forthcoming)*.

Frandsen, Brigham R. (2016). "The Effects of Collective Bargaining Rights on Public Employee Compensation: Evidence from Teachers, Firefighters, and Police". *ILR Review* 69.1, pp. 84–112.

Frandsen, Brigham R. and Michael Webb (2017). "Public Employee Pensions and Collective Bargaining Rights: Evidence from State and Local Government Finances". *Journal of Law, Economics & Policy (forthcoming)*.

FRS, Florida Retirement System (2014). *Defined Benefit Plan Investment Policy Statement*. From FRS' webstie https://www.myfrs.com/.

Galli, Emma and Stefania P. S. Rossi (2002). "Political Budget Cycles: The Case of the Western German Lander". *Public Choice* 110.3, pp. 283–303.

Garand, James, Justin Ulrich, and Ping Xu (2013). "Fiscal Policy in the American States". In: Haider-Markel, Donald. *The Oxford Handbook of State and Local Government Politics*, pp. 611–642.

Gilligan, Thomas W. and John G. Matsusaka (2001). "Fiscal Policy, Legislature Size, and Political Parties: Evidence from State and Local Governments in the First Half of the 20th Century". *National Tax Journal* 54.1, pp. 57–82.

Gordon, Tracy (2012). *State and Local Budgets and the Great Recession.* Brookings Institution. URL: https://www.brookings.edu/articles/state-and-local-budgets-and-the-great-recession/ (visited on 08/22/2016).

Griffin, John M., Jordan Nickerson, and Dragon Yongjun Tang (2013). "Rating Shopping or Catering?An Examination of the Response to Competitive Pressure for CDO Credit Ratings". *Review of Financial Studies* 26.9, pp. 2270–2310.

Hanusch, Marek and Paul M. Vaaler (2013). "Credit rating agencies and elections in emerging democracies: Guardians of fiscal discipline?" *Economics Letters* 119.3, pp. 251–254.

Hibbs, Douglas A. Jr. (1977). "Political Parties and Macroeconomic Policy". *American Political Science Review* 71.4, pp. 1467–1487.

Hirsch, Barry and David Macpherson (2017). *Union Membership and Coverage Database from the CPS*. URL: http://www.unionstats.com/ (visited on 05/01/2017).

INEGI, Instituto Nacional de Estadistica y Geografia (2015). *Banco de Informacion Economica (BIE)*. URL: http://www.inegi.org.mx/sistemas/bie/ (visited on 09/11/2015).

Irwin, Timothy C. (2015). "Defining the Government's Debt and Deficit". *Journal of Economic Surveys* 29.4, pp. 711–732.

Kashyap, Anil K. and Natalia Kovrijnykh (2013). *Who Should Pay for Credit Ratings and How?* Working Paper 18923. National Bureau of Economic Research.

Kiewiet, D Roderick (2010). "The day after tomorrow: the politics of public employee retirement benefits". *California Journal of Politics and Policy* 2.3.

Kiewiet, D. Roderick and Mathew D. McCubbins (2014). "State and Local Government Finance: The New Fiscal Ice Age". *Annual Review of Political Science* 17.1, pp. 105–122.

Kiewiet, D. Roderick and Kristin Szakaly (1996). "Constitutional Limitations on Borrowing: An Analysis of State Bonded Indebtedness". *Journal of Law, Economics, and Organization* 12.1, pp. 62–97.

Kneebone, Ronald D. and Kenneth J. McKenzie (2001). "Electoral and Partisan Cycles in Fiscal Policy: An Examination of Canadian Provinces". *International Tax and Public Finance* 8.5, pp. 753–774.

Kousser, Thad, Mathew D. McCubbins, and Ellen Moule (2008). "For Whom the TEL Tolls: Can State Tax and Expenditure Limits Effectively Reduce Spending?" *State Politics & Policy Quarterly* 8.4, pp. 331–361.

Labor, Bureau of Labor Statistics United States Department of (2017). *Local Area Unemployment Statistics*. URL: https://www.bls.gov/web/laus/laumstrk.htm (visited on 05/01/2017).

Langston, Joy (2010). "Governors and "their" deputies: New legislative principals in Mexico". *Legislative Studies Quarterly* 35.2, pp. 235–258.

Leip, David (2017). *US Election Atlas. Gubernatorial General Election Results.* URL: http://uselectionatlas.org (visited on 05/01/2017).

Lowry, Robert C., James E. Alt, and Karen E. Ferree (1998). "Fiscal Policy Outcomes and Electoral Accountability in American States". *American Political Science Review* 92.4, pp. 759–774.

Mathis, Jerome, James McAndrews, and Jean-Charles Rochet (2009). "Rating the raters: Are reputation concerns powerful enough to discipline rating agencies?" *Journal of Monetary Economics* 56.5, pp. 657–674.

McCubbins, Colin H. and Matthew D. McCubbins (2013). "Cheating on Their Taxes: When Are Tax Limitations Effective at Limiting State Taxes, Expenditures, and Budgets". *Tax Law Review* 67, pp. 507–542.

McGuinn, Patrick (2014). *Pension Politics: Public Employee Retirement System Reform in Four States*. Brown Center on Education Policy, Brookings Institution. URL: https://www.teacherpensions.org/resource/pension-politics-public-employee-retirement-system-reform-four-states (visited on 04/21/2017).

Merrifield, John (2000). "State Government Expenditure Determinants and Tax Revenue Determinants Revisited". *Public Choice* 102.1, pp. 25–48.

Minahan, John R. (2014). "Why Is There Debate About Whether Economics Applies to Pensions?" *The Journal of Investing* 23.4, pp. 7–14.

Mitchell, Olivia S. and Robert S. Smith (1994). "Pension Funding in the Public Sector". *The Review of Economics and Statistics* 76.2, pp. 278–290.

Moody's (2015). *Segmento de Mercado - Subsoberanos*. URL: https://www.moodys.com/researchandratings/market-segment/Sub-Sovereign/ (visited on 09/10/2015).

Munell, Alicia H. (2012). *State and Local Pensions: What Now? | Center for Retirement Research*. English. 1st. Brookings Institution Press.

Munell, Alicia H. and Jean-Pierre Aubry (2015). *The Funding of State and Local Pensions: 2014-2018*. Center for Retirement Research. URL: http://crr.bc.edu/briefs/the-funding-of-state-and-local-pensions-2014-2018/ (visited on 04/23/2017).

New, Michael J. (2010). "U.S State Tax and Expenditure Limitations: A Comparative Political Analysis". *State Politics & Policy Quarterly* 10.1, pp. 25–50.

Nice, David C. (1988). "The Item Veto and Expenditure Restraint". *The Journal of Politics* 50.2, pp. 487–499.

Nordhaus, William D. (1975). "The Political Business Cycle". *The Review of Economic Studies* 42.2, pp. 169–190.

North, Douglass C. and Barry R. Weingast (1989). "Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England". *The Journal of Economic History* 49.4, pp. 803–832.

NYSTRS, New York State Teacher's Retirement System (2016). *Investment Policy Manual*. From NYSTRS' website https://www.nystrs.org/.

OHSERS, School Employees' Retirement System of Ohio (2015). *Statement of Investment Policy for the School Employees Retirement System of Ohio*. From OHSERS' website www.ohsers.org/.

OPERS, Ohio Public Employees Retirement System (2017). *Investment Objectives and Asset Allocation Policy - Defined Benefit Fund*. From OPERS' website https://www.opers.org/.

Paler, Laura (2013). "Keeping the Public Purse: An Experiment in Windfalls, Taxes, and the Incentives to Restrain Government". *American Political Science Review* 107.4, pp. 706–725.

Parlour, Christine A. and Uday Rajan (2015). "Contracting on Credit Ratings: Adding Value to Public Information". SSRN: https://ssrn.com/abstract=2516940.

Public Plans Database 2001-2014 (2017). Center for Retirement Research at Boston College, Center for State, Local Government Excellence, and National Association of State Retirement Administrators. URL: http://crr.bc.edu/data/public-plans-database/.

Rogers, Diane Lim and John H. Rogers (2000). "Political Competition and State Government Size: Do Tighter Elections Produce Looser Budgets?" *Public Choice* 105.1, pp. 1–21.

Rose, Shanna (2006). "Do fiscal rules dampen the political business cycle?" *Public Choice* 128.3, pp. 407–431.

– (2010). "Institutions and fiscal sustainability". *National Tax Journal* 63.4, p. 807.

Sabin, Michael J. (2015). "Backtested Pension Math: An Empirical Look at the Causes of CalPERS Underfunding". https://papers.ssrn.com/abstract=2558377.

SEC, Securities, Exchange Commission Office of Investor Education, and Advocacy (2013). *The ABCs of Credit Ratings*. URL: https://www.sec.gov/files/ib_creditratings.pdf.

SHCP, Secretaria de Hacienda y Credito Publico (2015). *Indicadores de Deuda Subnacional y Afectacion de Participaciones*. URL: http://www.shcp.gob.mx/Estados/Deuda_Publica_EFM/Paginas/Presentacion.aspx (visited on 09/10/2015).

Skreta, Vasiliki and Laura Veldkamp (2009). "Ratings shopping and asset complexity: A theory of ratings inflation". *Journal of Monetary Economics* 56.5, pp. 678–695.

Sniderman, Paul M., Richard A. Brody, and Phillip E. Tetlock (1993). *Reasoning and Choice: Explorations in Political Psychology*. Cambridge University Press.

S&P, Standard & Poor's (2015). *Acciones de Calificacion*. URL: https://www.standardandpoors.com/es_LA/web/guest/ratings/ratings-actions (visited on 09/10/2015).

Splinter, David (2017). "State pension contributions and fiscal stress". *Journal of Pension Economics & Finance* 16.1, pp. 65–80.

Strate, John, Harold Wolman, and Alan Melchior (1993). "Are there Election-Driven Tax-and-Expenditure Cycles for Urban Governments?" *Urban Affairs Review* 28.3, pp. 462–479.

Thom, Michael (2013a). "All of the Above: How Fiscal, Political, and Workforce Traits Affect Pension Funding". *State and Local Government Review* 45.3, pp. 163–171.

– (2013b). "Politics, Fiscal Necessity, or Both? Factors Driving the Enactment of Defined Contribution Accounts for Public Employees". *Public Administration Review* 73.3, pp. 480–489.

Thom, Michael and Anthony Randazzo (2015). "Underfunding Annual Pension Contributions: Examining the Factors Behind an Ongoing Fiscal Phenomenon". *State and Local Government Review* 47.1, pp. 35–44.

Triest, Robert K. and Bo Zhao (2013). "The Role of Economic, Fiscal, and Financial Shocks in the Evolution of Public Sector Pension Funding". https://papers.ssrn.com/abstract=2433796.

TRS, Teacher Retirement System of Texas (2016). *Investment Policy Statement*. From TRS' website https://www.trs.texas.gov/.

Tufte, Edward R. (1980). *Political Control of the Economy*. Princeton University Press.

United States Census Bureau, Population and Housing Unit Estimates (2016). *State Population Totals Datasets*. URL: http://www.census.gov/popest/ (visited on 08/22/2016).

United States Census Bureau, State & Local Government Finance (2016). *State and Local Government Finances by Level of Government and by State*. URL: http://www.census.gov/govs/state/ (visited on 08/22/2016).

Vaaler, Paul M., Burkhard N. Schrage, and Steven A. Block (2006). "Elections, Opportunism, Partisanship and Sovereign Ratings in Developing Countries". *Review of Development Economics* 10.1, pp. 154–170.

Veiga, Linda Goncalves and Francisco Jose Veiga (2006). "Political business cycles at the municipal level". *Public Choice* 131.1, pp. 45–64.

Williamson, John (1990). *Latin American adjustment: how much has happened?* Institute for International Economics.