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ESSAYS ON WAGE DETERMINATION, MOBILITY AND EMPLOYMENT CONTRACTS

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ABSTRACT OF THE DISSERTATION

Essays on Wage Determination, Mobility and Employment Contracts

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This dissertation studies the impacts of employment contracts as well as legal and economic environments on labor market outcomes. Chapter 2 examines the effects of residency laws on public teachers' wages and residential choices using the natural experiment created by the repeal of the residency laws for teachers in Pittsburgh and Philadelphia in 2001. The findings suggest that the residency laws decrease public teachers' wages by ten percent. The data also indicates a significant outflow of residents after the repeal. In order to further investigate whether there is a relocation of teachers due to residency laws, I look at where public teachers are sending their children for schooling. I find that the private school enrolment probability decreases when residency laws are lifted. Chapter 3 studies the factors that determine the enactment of collective bargaining laws for public school teachers. I look at the effects of state specific demographic, political, and economic variables on the decision of a state to adopt collective bargaining laws. I find that state-level school resources such as studentteacher ratio and per pupil expenditures are significant drivers of collective bargaining laws. Furthermore, the political leanings of state governments do not seem to have an important effect on the decision that a state will enact collective bargaining. Finally, state economic conditions appear to have significant effects on bargaining law changes. Chapter 4 focuses on determining whether a spot market or an implicit contracting model better explains the wage determination process over the business cycle. Real business cycle theory employs a spot market model in which current wages are determined by contemporaneous labor market conditions. In contrast, the implicit contract model allows for the history of labor market conditions to determine current wages. I replicate the Beaudry & DiNardo (1991) and extend their investigation along gender, race and regional lines to understand whether labor markets can be explained well or only certain submarkets can be described by one of the models. The results suggest that an implicit contracting model explains the wage determination over the business cycle better than a spot market model for almost all specifications.

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Dedication

To my mother.

Table of Contents

Abstract								
Acknowledgements								
Dedication								
List of Tables								
List of Figures								
1.	Intr	oduction	1					
2.	The	e Effects of Residency Laws on Teachers' Wages and Residential Choice	4					
	2.1.	Introduction	4					
	2.2.	History and Controversy	6					
		2.2.1. Controversy	7					
	2.3.	Literature Review	9					
		2.3.1. Models on Residency Laws	9					
		2.3.2. Empirical Studies on Residency Laws	12					
	2.4.	The Model and Data	16					
	2.5.	Descriptive Statistics	20					
	2.6.	Results	23					
	2.7.	Choice of Residence	26					
	2.8.	Conclusion	30					
3.	Det	erminants of Teachers' Collective Bargaining Rights	33					
	3.1.	An Overview of Collective Bargaining Rights	36					
	3.2.	Literature Review	39					

	3.3.	Non-parametric Survival Estimation	48		
	3.4.	Parametric Survival Anlysis and Data	52		
	3.5.	Dynamic Logit Model	59		
	3.6.	Conclusion	67		
4.	Wag	ge Determination over the Business Cycle	70		
	4.1.	Introduction	70		
	4.2.	Theory of Beaudry and DiNardo 1991	72		
	4.3.	Literature Review	75		
	4.4.	Model and Data	78		
	4.5.	Results	82		
	4.6.	Conclusion	86		
A. Tables and Figures of Chapter 2					
	A.1.	Tables	89		
	A.2.	Figures	98		
в.	Tab	les and Figures of Chapter 3	100		
	B.1.	Tables	100		
	B.2.	Figures	108		
C.	Tab	les and Figures of Chapter 4	110		
	C.1.	Tables	110		
	C.2.	Figures	117		
Re	efere	nces	118		

List of Tables

A.1. List of residency requirements pros and cons.	89
A.2. Descriptive Statistics for public teachers before and after the law change.	90
A.3. Public teachers' age distribution before and after the law change	91
A.4. Percentage of public teachers who reside within the city before and after the	
law change	91
A.5. DID Estimates of the Effects of the Repeal of Residency Laws on Teachers'	
Log of Hourly Wages and Annual Wages.	92
A.6. DID Estimates of the Effects of the Repeal of Residency Laws on Teachers'	
Log Wages for Different Subsamples	93
A.7. DID Estimates of the Effects of the Repeal of Residency Laws on Teachers'	
Log Wages for Different Control Groups and City Fixed Effects.	94
A.8. Estimation Results for Private Teachers	95
A.9. Descriptive statistic for the children of the public teachers	95
A.10.Probit estimation results.	96
A.11. The effect of the interaction term "after \times interaction"	97
A.12. The effect of the interaction term.	97
B.1. Survival Times	100
B.2. Summary Statistics	101
B.3. Collective Bargaining	102
B.4. Exponential Model Coefficient Estimates	103
B.5. The Exponential Model: Hazard Ratios	104
B.6. Estimation results of the dynamic logit model	105

B.7.	Marginal effects of selected variables for three quantiles; low, medium, and	
	high	106
B.8.	Estimation results of dynamic logit model with year dummies. \ldots .	107
C.1.	Summary statistics	110
C.2.	Estimation results for males over the period 1996-2012	111
C.3.	Estimation results for females over the period 1996-2012	112
C.4.	Estimation results for both males and females over the period 1996-2012	113
C.5.	Estimation results for males and females by race over the period 1996-2012	114
C.6.	Estimation results for males and females by union status over the period	
	1996-2012	115
C.7.	Estimation results by region over the period 1996-2012	116

List of Figures

A.1.	Interaction effect as a function of the predicted probability	98
A.2.	t-statistic as a function of the predicted probability. \ldots \ldots \ldots \ldots	98
A.3.	Marginal Effects of the interaction term "treatment \times after".	
		98
A.4.	Marginal Effects of the interaction term "treatment $\times after$ " for different age	
	groups	98
A.5.	Marginal Effects of the interaction term "treatment $\times after$ " for different levels	
	of family income. The variable on the x-axis is the logarithm of real family	
	income	99
B.1.	Kaplan-Meier Survival Function	108
B.2.	Kaplan-Meier Survival Function by South	108
B.3.	Kaplan-Meier Survival Function by Party of the Governor	109
B.4.	Cummulative Hazard Fnct	109
B.5.	Smoothed Hazard Fnc	109
C.1.	Unemployment rate for males overtime	117
C.2.	Unemployment rate for females overtime	117

Chapter 1

Introduction

This dissertation consists of three chapters studying the impacts of employment contracts as well as legal and economic environments on labor market outcomes. The first chapter of my dissertation focuses on how residency laws affect teachers' wages and their residential choice. The second chapter focuses on the impact of state-level economic and political conditions on the likelihood of adoption of mandatory collective bargaining for public teachers. The third chapter investigates whether employment contracts are an important determinant in the movement of the wages in the labor markets over the business cycle. The main motivation behind this dissertation is to evaluate the existing policies and provide some empirical basis for thinking about improvement of these policies and possible alternatives.

The first chapter examines the impact of residency laws on teachers' wages and residential choice using the natural experiment created by the repeal of the residency laws for teachers in Pennsylvania's large cities of Philadelphia and Pittsburgh in 2001. I find that the repeal of residency requirements increased public school teachers' wages by approximately ten percent on average and had varying effects across different demographic groups. Some demographic groups seem to have been more affected than others; the law change appears to have had a greater impact on married teachers and teachers who do not hold a postgraduate degree. Furthermore, residency laws did not seem to affect private school teachers' wages.

The data also indicate a significant outflow of residents after the repeal. In order to further investigate whether there is a relocation of teachers due to residency laws, I look at where public school teachers are sending their children for schooling. Residency laws might in fact induce public school teachers to enroll their children in private schools, as they restrict teachers from moving to an area with a preferred public schooling system. In order to investigate how residency requirements affect families' residential choices, I look at whether the residency requirements increased the probability that public school teachers enrolled their children in private school. The results suggest that the probability that public school teachers will enroll their children in private school decreased by 0.15 of a percentage point after the repeal of the residency law.

The second chapter focuses on the political and economic drivers that result in the enactment of collective bargaining laws using two approaches. The first approach is a survival analysis that studies the timing of the likelihood of abandoning a strict collective bargaining law policy during the time period of 1964 to 1996. The second approach is a dynamic logit model that analyzes the impact of state-level economic and political conditions on the likelihood of adoption of mandatory collective bargaining for public teachers. Even though these models differ in structure and interpretation, essentially both models provide useful insights that are consistent with each other. Empirical results suggest that a state's political environment is not an important factor in the decision to adopt collective bargaining and that the effects and significance of a state's economic conditions are mixed in importance.

The survival analysis predicts that those states that have higher per pupil expenditures and higher student-teacher ratio are associated with lower durations of strict collective bargaining regimes. The results also suggest that the states with higher per capita income and higher the percent of state government expenditures to states' total income experience longer durations of strict bargaining regimes. Similarly, the dynamic logit estimation, the results suggest that per capita income has a significant negative effect on the likelihood of adopting collective bargaining, but the percent of state government expenditures to states total personal income has no significant effect. Instead, this study reveals certain characteristics of a state's public school system that are more important factors affecting the likelihood of collective bargaining. The findings suggest that states with higher student-teacher ratios and higher per pupil expenditures, are more likely to enact collective bargaining laws.

The third chapter investigates whether employment contracts are an important determinant of the movement of wages in labor markets over the business cycle. Two particular views are prominent in the literature. Real business cycle theory generally employs a spot market model in which current wages are determined by contemporaneous labor market conditions. In contrast, the implicit contract model allows for the history of labor market conditions to determine current wages. I replicate the Beaudry & DiNardo (1991) findings using more recent data and also extend their investigation along gender, race and regional lines to understand whether labor markets can be well explained or if only certain submarkets can be described by one of the models. For almost all subgroups the results are consistent with the Beaudry & DiNardo findings, suggesting that the effect of the minimum unemployment rate during a job spell on current wages dominates the effects of the contemporaneous unemployment rate and the unemployment rate at the time a worker starts a job in almost all subgroups. Therefore, this study also supports that the implicit contract model explains the wage determination over the business cycle better than the spot market model over the period 1996 to 2012. The results are robust with respect to almost all of these demographics, and the findings are also consistent across the midwest and northeast regions of the U.S..

Chapter 2

The Effects of Residency Laws on Teachers' Wages and Residential Choice

2.1 Introduction

Laws that require public employees to reside within the jurisdiction they work have been in effect since the political machine era of the 19th century. Residency requirements are intended to provide public jobs to local residents and to keep the income effects of public salaries within the jurisdiction. These requirements have been seen as a way of keeping middle class families in cities, making them the taxpayers and consumers of the city from which they receive their salaries.

Despite their long history, these laws have always provoked intense debates. The opposition has mainly centered around the idea that residency laws shrink the labor pool of municipal employees and decrease the quality of workers. Over two-thirds of U.S. cities had residency laws in 1980 (Eisinger 1983), but by 1997, after decades of controversy, residency requirements remained in effect in only twenty four percent of cities in the U.S. (Duncan 2005). After losing some popularity in the late 20th century, there is now a renewed interest in these laws. Local government officials in Detroit are fighting to bring back a residency requirement for police officers that was lifted in 1999. Lawmakers in Washington D.C. proposed three bills in 2011 relating to residency requirements for their government employees, over half of whom currently commute from neighboring states. New Jersey Governor, Chris Christie, also signed legislation in 2011 that requires all newly hired public employees to be residents of the state.

Even though these laws seem to always return to the legislative tables, past literature has

paid little attention to the implications of residency laws on public worker labor market outcomes. The several models that have been proposed in the literature have yielded conflicting predictions about the effects of these requirements on workers' wages and employment. This paper examines the effects of teachers' residency laws on wages and residential choices. I use the repeal of residency requirements for teachers in Philadelphia and Pittsburgh in 2001 as a natural experiment and employ the method of difference-in-differences, DID, in order to estimate the effect of this legal change on public teachers.

This study differs from the previous studies in three main ways. First, the other studies use aggregate level data; I use micro level data to analyze the effects of the law. The microdata allow me to enter a full set of individual characteristics and better control for several compositional differences that may arise across cities. Second, I employ the methods of difference-in-differences, DID, in order to estimate the effects of residency laws on public teachers. This strategy can identify the true impact of residency requirements on wages. purging out other confounding factors such as time trends and fixed effects. Third, I examine the effects on teachers, a group that may be affected differently than other municipal workers and has not been studied in the literature. While police and firefighters do not have an option to seek this line of work elsewhere within the city, public teachers do have other options to work within the city and avoid the residency requirement through employment in the private sector. Hence, they might be differently affected by these laws. Furthermore, having a pool of high quality teachers is especially important in a community trying to foster its human capital development. Teachers are a particularly interesting yet understudied group. I find that the repeal of residency requirements increases public teachers' wages by approximately ten percent on average and has varying effects across different demographic groups.

In order to investigate how residency requirements affect families' residential choices, I look at whether the residency requirements increase the probability that public teachers will enroll their children in private school. According to the Tiebout (1956) hypothesis, when families with similar economic status decide their residence, they often search for similar public services, which cluster them around the same communities. Public schools are an example of a highly demanded public service, therefore looking at how residency requirements affect enrollment rates is a way of determining whether the residency law is preventing this sorting in the Tiebout sense. In fact, I find that the probability that public teachers will enroll their children in private school decreases after the repeal of the residency law.

The rest of the paper is organized as follows: The next section presents the history and controversy of residency laws; Section 3 reviews the theoretical and empirical literature; Section 4 describes the model and data; Section 5 presents descriptive statistics; Section 6 displays the results; Section 7 provides a model to test the residential choice of the public teachers and the results of the estimation; and Section 8 concludes.

2.2 History and Controversy

Residency requirements have taken many shapes throughout the history of society. Residency requirements can be imposed by state law, municipal charter, or administrative regulation. Jurisdictions may adopt different types of residency requirements (Notes 1975); The most common residency restriction requires that all municipal employees reside within the boundaries of the city for the duration of their employment. Some cities require municipal employees to reside within the county in which the city is located, and some require workers to live within the city's county or in the bordering counties. The law may also require public workers to reside within a specified distance or travel time from his or her place of employment. Residency requirements may apply to all public employees or only to particular types of employees (e.g. policemen and firefighters).

Residency requirements have existed for centuries, as far back, and probably long before, 1413 England, where there is record of a restriction on members of Parliament (LeClercq 1976). In American history, residency requirements came first in the political machine era of the late 19th century and then again in the 1970s (Eisinger 1983). During both periods, these laws were used as a staple of the *"spoils system"* (Anderson 1925 in Johnson et al. 1997), or to award public employment to those who had contributed past service. They may have also resulted from the role of city aldermen who commonly appointed local residents to staff the police force of their respective wards (a practice that can actually be traced back to the mid 17th century), (Johnson et al. 1997). The reform movement of the early 1900s heavily opposed residency requirements, arguing that they hindered the ability to hire the most qualified individual for public positions. By the 1920s most cities favored the *"merit principle"* (Mosher and Kingsley 1936 as quoted by Eisinger 1983, p.86) over the *"spoils system"*. The majority of cities did away with their residency requirements around this time by abandoning the enforcement of these laws (Eisinger 1983).

For the next half century, residency laws remained out of fashion. However, the 1970s saw a massive revival of residency requirements. One explanation of this may be the high unemployment rates of inner city minorities during this period. Similar stories are used to explain the sudden but brief embrace of residency requirements during the Great Depression (Powell 1956 in Eisinger 1983). Eisinger (1983, p.88) states that *"residency laws are assumed to help central city minority job seekers by eliminating overwhelmingly white metropolitan workers from the competition for municipal civil service jobs"*. The majority of cities with a population over 250,000 in 1980 enforced some form of residency requirement, most of which were passed in the preceding decade (Eisinger 1983).

2.2.1 Controversy

The resurgence of residency requirements in the 1970s met with numerous legal challenges. In fact, the legality and constitutionality of residency requirements have been disputed for over a century in the courts, dating back to 1901 (Hirsch and Rufolo 1985). Since then, there have been numerous cases nationwide with a variety of arguments opposing the laws accompanied by a variety of outcomes.

It is not unconstitutional for state and local governments to stipulate qualifications for public employment (Eisinger 1983). The unconstitutionality of residency laws mostly encompasses the Equal Protection and the Due Process Clauses of the 14th amendment and the implied freedom to travel (Hirsch & Rufolo 1985). The Commerce Clause as well as the Privileges and Immunities Clause have also entered the judicial discussion, however the Supreme Court rejected the contention that residency laws violate the Commerce Clause when applied to employment by saying that the cities are not trying to regulate the market but are acting as market participants and therefore not violating the Commerce Clause¹ (Hirsch 1992 p.10). The law itself is primarily aimed to protect states from ordaining means of discrimination against nonresidents for purposes of economic protectionism (Hirsch 1990).

Toomer v. Witsell² in 1948 developed a "substantial reason" test that has since been applied to cases debating the violation of the Privileges and Immunities Clause by residency laws (Hirsch 1990). It basically consisted of two assertions; first that there be a substantial reason for discriminating against nonresidents, and second that there be "a reasonable relationship between the danger represented by the noncitizens as a class, and the discrimination practiced upon them" (Toomer v. Witsell case noted in Hirsch 1990). Similar arguments forced a number of cities to lift their residency requirements if they could not show a "compelling government interest" to restrict the right to travel³(Heinz 1974⁴ p. 414).

A number of rationales for government interest have been tried. Such justifications include reducing the energy used by city employees on their commutes to and from work (Eisinger 1983), providing quick response from firemen and police⁵ (Heinz 1974), and the promotion of police-community relations⁶ (Heinz 1974). Another argument made that is now known as the "public coffer" theory which suggests that public employees should live within the jurisdiction for which they work in order to redistribute their salaries back to the city through taxes and spending. Although the public coffer defense worked in favor of the residency requirements in some cases⁷ (Goldstein 1972), none of these justifications were found to be an unfailing way to defend residency laws. A summary of Pros and Cons

 $^{^1\}mathrm{White}$ v. Massachusetts Council of Construction Employers 460 U.S. 204 (1983) as noted in Hirsch (1992)

²Toomer v. Witsell, 334 U.S. 385 (1948) as noted in Hirsch (1990)

 $^{^{3}}$ Shapiro v. Thompson, 394 U.S. 618 (1969) as noted in Heinz (1974)

⁴Heinz (1974) Constitutional Law-City Employment Residency Requirements, 7 Urb. L. Ann. 414 available at http://openscholarship.wustl.edu/cgi/viewcontent.cgi?article=1766&context=law_ urbanlaw

 $^{^5\}mathrm{Marabuto}$ v. Town of Emeryville, 183 Cal. App. 2d 406, 410, 6
 Cal. Rptr. 690, 692-93 (Dist. Ct. App. 1960) as noted in Heinz 1974

⁶Krzewinski v. Kugler, 338 F. Supp. 492, 499 (D.N.J.1972) as noted in Heinz (1974)

 $^{^7{\}rm Kennedy}$ v. City of Newark, 29 N.J. 178, 148 A.2
d 473 (1959) as noted in Goldstein (1972)

can be found in Table A.1.

Through all of this, the residency requirement controversy continues. For the many employees who remain up in arms over residency requirements, a strong argument must be found in order to attempt to reverse the laws in court. When such an argument, such as restriction of constitutional rights, is identified, it is then on the burden of the city to find a compelling government interest for the law and to prove that the law helps promote these interests. It is essential for local governments to find a strong benefit, economic or otherwise, to the city in order to sustain a residency law.

2.3 Literature Review

2.3.1 Models on Residency Laws

The effects of residency requirements on labor market outcomes depend on their effects on supply and demand; the direction and magnitude of these effects as well as the elasticity of the supply and demand determine the net effects on public employees' compensation and employment (Gonzalez et al. 1991). Several models discuss the effects of these requirements on labor supply and labor demand in the literature. Each model concludes a different effect of the law on workers' pay and employment.

Standard Supply and Demand Model: The standard supply demand analysis suggests that residency requirements shrink the labor pool due to the restrictions the requirements put on the residential choices of employees. The employees who do not want to lock themselves into living within the city for a long period of time and those who do not want to relocate into the city consider alternative jobs without such requirements (Gonzalez et al. 1991). Therefore, the model predicts a decrease in labor supply.

As discussed earlier, residency laws are expected to improve labor productivity by increasing employee knowledge of the community and also increasing availability in case of emergencies. Smith (1980) finds evidence that the performance of police officers increases as the percentage of police officers living within a city increases. Furthermore, Hirsch & Rufolo (1985) argue that the inelastic nature and the relatively fixed input mix of the public goods imply that an increase in productivity reduces the labor demand ⁸. In particular, improved productivity increases output per worker. Therefore, to produce the same level of output, the city needs fewer workers which in turn leads to a decrease in labor demand (Hirsch and Rufolo 1986)⁹.

To sum up, the standard supply-demand model suggests that residency requirements decrease both the supply of and the demand for labor. Consequently, these requirements decrease employment, but their effect on wages is ambiguous. The change in wages depends on the relative change in supply and demand curves (Gonzalez et al. 1991.)

O'Brien (1997) suggests that there is not much contention over whether these laws are negative job attributes, and he further disputes the productivity increase argument. He points out that residency in a jurisdiction does not necessarily guarantee less travel time to the site of emergencies, nor does it provide greater familiarity with the community because police and firefighters will most likely reside in middle income areas and work in less wealthy neighborhoods.

The Nonstandard Model: Hirsch & Rufolo (1985, 1986) discuss the effects of residency requirements on labor supply from a different point of view. They argue that public employees who are also residents are less inclined to strike against their own community since their families and neighbors would be adversely affected by the disruption of public services. Thus their bargaining power decreases, which in turn decreases their reservation wage. Decreased reservation wages increases the labor supply.

On the demand side, the nonstandard model suggests that the demand for labor decreases due to a productivity increase as discussed in the standard model. Therefore, the nonstandard model suggests that residency requirements increase supply of and decrease

⁸Hirsch and Rufolo (1986) discuss that, if a city offers higher wages in order to keep the same quantity and quality of workers that it employed in the absence of the residency law, then labor costs increase, which in turn increases the cost of producing the public good, all else equal. The model also predicts a decrease in demand in that case.

⁹Hirsch and Rufolo (1986) point out that improved productivity decreases the unit costs of production, and the city might decide to increase the output or might consider to substitute this now relatively cheaper labor input for another input. If the city is able to do so, then labor demand may increase. Although, they note that public services are mostly essential goods, and due to the highly inelastic demand and the relatively fixed input mix features of public services, increase in labor demand is not very likely. Therefore, under the standard labor supply theory, demand should decrease.

demand for labor. As opposed to the standard model, this model predicts a decrease in wages and an ambiguous effect on labor demand.

Gonzalez et al. (1991) criticize the idea of weakening the bargaining power and suggest that the supply might increase because of the improvement in job attributes. They argue, for instance, that living within the city limits improves the relationship between police officers and the community. Therefore, we can think that the working conditions of police officers improve, or their job attributes increases. In addition to rejecting the familiarity factor, another criticism made by O'Brien (1997) is that safety workers such as police and fire fighters mostly do not have a right to strike.

The Political Demand Model: The political demand model agrees with the supply decrease from residency requirements as described in the standard model, due to a decrease in the labor pool. Hirsch & Rufolo (1986) propose that residency requirements can help the local government politically. First, public employees are an important fraction of a city's electorate, and they have been shown to vote more frequently than other citizens (Bennet & Orzechhowski 1983; Borcherding et al. 1977 in Gonzalez et al. 1991). Secondly, there are studies proposing that public employees support expansionary budget policies (Gramlich & Rubinfield 1982). Therefore, they are more likely to support higher taxes and higher public expenditures than the rest of the city residents.

As a result, an increased number of public employees in a city through residency requirements tends to increase demand for public goods, thus increasing derived demand for labor. Mehay & Siedan (1986) argue that labor demand generated by public employees voting for higher public expenditures outweighs the decline in labor demand due to productivity improvement. Consequently, the political demand model suggests that residency requirements decrease the supply and increase the demand for labor. This model therefore predicts that residency laws increase wages and an ambiguous effect on employment.

The Bargaining Model: The bargaining model, also developed by Hirsch & Rufolo (1985, 1986), adopts the supply increase seen in the nonstandard model and the demand increase seen in the political demand model. More specifically, they argue that residency requirements decrease public employees' bargaining power because the laws create fidelity

between the worker and the city which makes public employees less aggressive in the bargaining process about strikes or work stoppages. Weak bargaining power results in lower reservation wages which implies a shift of the supply curve to the right. On the demand side, they consider an increase in demand with regard to the public employees voting for higher public expenditures (Gonzalez et al. 1991). According to the bargaining model, residency requirements increase both labor supply and labor demand. Thus, this model predicts an increase in employment and an ambiguous effect on wages.

The Disequilibrium/Bureaucracy Model: Gonzalez et al. (1991) offer an alternative way of looking at the effects of residency requirements on public sector labor market. Rather than an equilibrium model, they suggest that the local public sector market operates under a disequilibrium model where local government pays above equilibrium wages which results in excess supply (Gonzalez & Mehay 1985; Mehay & Gonzalez 1986 in O'Brien 1997).

The disequilibrium/bureaucracy model suggests that residency requirements decrease labor supply due to limitation on residential choice. The key of this model is that the reduction in supply is equal to the existing excess supply assumed by the disequilibrium condition (O'Brien 1997). Therefore, there is no effect on wages due to such laws. The model also suggests that these laws increase labor demand due to the voting power of public employees, as discussed in the Political Demand model.

2.3.2 Empirical Studies on Residency Laws

The theoretical models proposed do not reach an agreement about the effects of residency requirements on public sector labor market outcomes. In the literature, the models are tested empirically, generally using data on protective workers. Empirical studies also do not agree on the effects of these laws on municipal workers' wages and employment. Therefore, which theoretical model is supported by the data is unclear. Some studies support the nonstandard model and some the disequilibrium/bureaucracy model. Below, I discuss in detail the studies and their results in chronological order.

Getz (1979) analyzes the wage determination process for fire fighters, controlling for

residency requirements. His main focus is how unions affect salaries and fringe benefits as well as working hours through collective bargaining. He therefore estimates his model for different subsamples: cities with collective bargaining, cities without collective bargaining, and cities with a combination of the two. His results suggest that residency requirements have no effect on wages for all subsamples.

Using city level data on policemen, Hirsch & Rufolo (1985) find that residency requirements tend to decrease wages. They estimate supply and demand equations using simultaneous equations method. Hirsch & Rufolo (1985) assume that residency laws are endogenous, and they use the rate of population change as an IV. Their estimation suggests that labor supply and labor demand increase in the presence of residency requirements. These findings are the opposite of those predicted by the standard and political demand models. They propose the bargaining model and hypothesize that the results might be because these laws reduce the bargaining power of unions and that this is why the laws are adopted by municipalities¹⁰. They, therefore, support the nonstandard model. Hirsch & Rufolo (1986) further examine the effects of residency laws on labor market outcomes, this time for firefighters using city level data. In this paper, they again assume that the residency laws are endogenous, and to address the simultaneity problem, they instrument the probability of enacting residence requirements using population growth and non-white population share differentials between urban and suburban neighborhoods. They again find that residency laws lower wages. Therefore, their findings for firefighters support the nonstandard model as well.

Gonzalez et al. (1991) developed a new approach to these studies by introducing a disequilibrium model discussed in the previous section, but they consider a supply and demand analysis to test their model on the labor market for policemen using city level data. According to their results, residency requirements increase labor demand and cause a reduction in supply, although this decrease is not statistically significant. They conclude

¹⁰Hirsch & Rufolo (1985) also run a logit model where residency requirements for police is dependent variable with some controls to understand what type of cities are more likely to be willing to pass these laws. They find that the percentage of revenue raised locally lowers the likelihood of passing the law. Moreover, they find that the higher the compensation, the higher the probability of residency requirements. They also conclude that local governments of cities that are relatively poor, that experience low population growth and that are less educated are more willing to enact residency laws.

that residency laws have a negative but insignificant effect on compensation, and they suggest that their findings are close to the bargaining model among equilibrium models but are most consistent with their disequilibrium model. They also argue that the approach of Hirsch and Rufolo (1985, 1986) cannot precisely differentiate between the nonstandard and bargaining models because the nonstandard model predicts a decrease in wages, and bargaining model predicts an indeterminate effect in wages while both models suggests an increase in supply. Even though both Hirsch & Rufolo (1985) and Gonzalez et al. (1991) use policemen, they find different effects.

The most recent study is O'Brien (1997), which examines the effects of these laws on protective services. His analysis suggests that residency laws do not affect either supply or demand¹¹. O'Brien (1997) concludes that residency requirements have no effect on public sector labor market, which is consistent with the results of Getz (1979) for firefighters. O'Brien (1997) argues that a possible explanation for his results is that there may be a high degree of non-compliance. In addition, different coverages of residency laws in different cities might lead to different outcomes. For example, in some cities only newly hired employees are affected; in others only officers above a certain rank or workers hired after a certain date are affected (O'Brien 1997).

Furthermore, he discusses the possible policy implications of these results. Although his findings predict no productivity increase due to residency laws, if these laws also do not increase wages, local governments can use the law to help them increase the city's revenue by increasing the number of residents without increasing expenditures. He also suggests that local governments might be interested in hiring less qualified workers so that they could pay lower wages. Lastly, public worker voting power might mean greater support for local government.

As is seen from the empirical studies, how residency laws affect public sector labor markets is still an unresolved issue. So far, Hirsch & Rufolo (1985, 1986) find lower wages and no effect on employment; Gonzalez et al. (1991) find no effect on wages and positive effect on employment; O'Brien (1997) and Getz (1979) find no effect on both wages and employment.

¹¹O'Brien (1997) also treats the law as exogenous; he notes that he replicated his analysis also correcting for endogeneity, but the results were not affected.

To sum up, these empirical findings show that Hirsch & Rufolo (1985, 1986) supports the nonstandard model and Gonzalez et al. (1991) supports the disequilibrium/bureaucracy model. The differences in the groups of municipal employees, the time periods and the cities analyzed in each study in the literature may lead to the varying results. There are far more areas in this field to explore. In this study, I examine the effects of residency laws, on teachers, a group that has not been studied in the literature, using more recent data and a different method of estimation.

A number of differences, as well as a few similarities, arise between the method of estimation in my work and other works. First, as mentioned, among all the studies in the literature, only Hirsch & Rufolo (1985, 1986) do not assume the exogeneity of residency laws. All the other papers, mine included, assume exogeneity of these laws. How reasonable is this assumption? In studies that compare city-level data, residency laws should be randomly assigned across cities once relevant variables have been controlled for. This assumption is questionable, as residency laws are in fact determined by city legislation. While I am making the same assumption, I am comparing individual outcomes in the same city over time. This allows me to control for time-invariant city characteristics that might affect the enactment of residency laws (e.g., time invariant political preference, lobbying activity, etc). Furthermore, as long as each individual does not directly affect residency laws, the assumption of exogeneity seems more realistic in my context. In addition, one might worry how strong the instruments of Hirsch & Rufolo (1985, 1986) (the rate of population change and the difference of nonwhite make-up of the population within and outside the city) are since it is hard to identify variables that determine the passage of these laws and do not affect labor supply and demand.

Second, the methods in previous studies aim to disentangle the employment and wage effect of residency laws by estimating supply and demand equations, while I focus on a reduced form model. While recovering the separate effects on prices and quantities is very interesting, the econometric identification of such models requires credible exclusion restrictions to predict demand and supply. To avoid this layer of complexity, I have chosen to estimate a reduced form model.

Third, I examine the effects of the residency laws on teachers' wages using the natural experiment created by the repeal of these laws for public school teachers in Philadelphia and Pittsburgh in 2001. In particular, the major difference between my study and the others, arises from the following. All the previous works have compared a cross-section of cities. Even assuming residency law exogeneity, as I do, the parameter of residency laws will be correctly identified only if the cities with the law are observationally similar to the cities without the law. This might be questionable in a sample of cities that span through the country and in regression models that do not even control for state fixed effects; (None of the studies mentioned above use state fixed effects). My strategy compares outcomes of individuals in the same city over time, exploiting the fact that some cities repealed the law. In addition, as a simple before-after comparison could lead to biased results if residency requirements are correlated with time trends, I further control for outcomes of individuals living in cities that are observationally similar to the treated cities. The difference-indifferences strategy therefore should reduce the number of confounding factors that might be otherwise present in the rest of the literature. The model and data used in this study is explained in the next section.

2.4 The Model and Data

I aim, in this paper, to determine the impact of residency laws on teachers' wages. In order to determine this, I focus on the most common type of residency laws: those that require public employees to live within the borders of the city for which they work.

In 2001, the Senate of Pennsylvania approved legislation that would repeal the residency requirements for teachers in Philadelphia and Pittsburgh school districts, which had been in effect for decades¹². Proponents of the amendment in Philadelphia, which had a teacher

¹²This "omnibus education bill" also specified wider retirement plan choices for employees of the State System of Higher Education and directed certain studies and assessments of public schools as well as student progress, among other provisions (Senate Democratic Wrap-Up 2001-2002).

Section 9 of The General Assembly of Pennsylvania, Senate Bill 485 entitled "An act amending the act of March 10, 1949 (P.L.30, No.14), entitled "An act relating to the public school system, including certain provisions applicable as well to private and parochial schools; amending, revising, consolidating and changing the laws relating thereto, further providing for teachers' and employees' retirement plans," amended section 1106 and removed a clause allowing "the board of school directors and the bargaining unit representing the professional employees" to have the option to agree upon residency requirements (The General Assembly of

residency policy that required newly hired teachers to move into the city within three years (Useem & Neild 2001), hoped the change would decrease the city's teacher shortage. The bill was signed into law on June 22 and took effect on June 30, 2001¹³.

Advantageously, this repeal of residency requirements in Philadelphia and Pittsburgh can be used as a natural experiment. I employ the difference-in-differences (DID) estimation in order to test the effects of this law change on teachers' wages.

The DID estimation is a common method of evaluating policy changes, such as changes in legislation or administrative government rules. This method compares outcomes from two different groups before and after the policy change under examination: the treatment group is the group that is affected by the policy change and the control group is a group that would not be affected. One assumption that the model imposes is that the treatment and control groups would be comparable over time in the absence of the treatment.

In my study, assuming that the law change is exogenous, I define the treatment group as being public teachers who work in Philadelphia and Pittsburgh and the control group as being public teachers who work in Rochester, Syracuse and Yonkers. These cities are chosen as the control group because they did not have residency requirements over the study period and are close geographically to the treatment group but not so close as to be affected by the law change.

Before discussing the details of the data I use, I specify the DID model. The data I use contains heteroskedasticity, therefore I estimate the model using feasible Generalized Least Squares (FGLS)¹⁴. The DID estimator can be found by the following regression equation.

$$\log(wage) = \beta_0 + \beta_1 after + \beta_2 treatment + \beta_3 after \times treatment + \mathbf{X} \beta_4 + u \quad (2.1)$$

Pennsylvania, House Bill No. 628 Session of 1997), instead ending the section stating that no school district "shall require an employee to reside within the school district as a condition for appointment or continued employment." (The General Assembly of Pennsylvania, Senate Bill No. 485 Session of 2001)

¹³Senate Democratic Wrap-Up 2001-2002. The General Assembly of Pennsylvania, Senate Bill No. 485 Session of 2001

¹⁴The general form of the regression equation I consider is $y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_1 \times x_2 + \beta_4 x_3 + \dots + \beta_{k+1} x_k + u$ where $Var(u|x) = \sigma^2 s(x)$. Note that I assume $s(x) = exp(\delta_0 + \delta_1 x_1 + \delta_2 x_2 + \dots + \delta_k x_k)$. I use the GLS procedure accordingly to estimate the equation 2.1.

where $u = s(x)\varepsilon$. The variable *after* is a dummy variable equal to one for the period after the repeal of the teachers' residency law and zero for the period before the law change. The variable *treatment* is a dummy variable equal to one if a public teacher is working in Pittsburgh or Philadelphia and is equal to zero if a public teacher is working in Rochester, Syracuse or Yonkers. The interaction term, $(after \times treatment)$, is a dummy variable for public teachers in the treatment group after the repeal of the law. In other words, $(after \times treatment) = 1$ if treatment = 1 and after = 1, and zero otherwise. Finally, s(x)take the form; $s(x) = exp(\delta_0 + \delta_1 x_1 + \delta_2 x_2 + ... + \delta_k x_k)$, and ε is the error term which has zero-mean and constant variance $(E[\varepsilon] = 0$ and $E[\varepsilon]^2 = \sigma^2$). The identification assumption is that $E[\varepsilon|(aftertreatment)] = 0$, i.e. β_3 would be zero in the absence of the treatment (Meyer 1995).

The intercept term in the regression, β_0 , is the average wage of public teachers in the control group before the law change. The coefficient on year dummy, β_1 , captures the change in public teachers' wages over time. The parameter, β_2 , captures the location effect on the public teachers' wages independent of the residency requirements. Finally the parameter on the interaction term, β_3 , captures the effect of the repeal of the residency law on public teachers' wages. To control for possible systematic changes in the distribution of individual characteristics over time, I include teachers' characteristics in the regression. The variable **X** is a vector of control variables which include the teachers' characteristics such as age, age squared, education, sex, race, marital status and grade level taught.¹⁵ Lastly, I use the logarithm of real hourly wage as the dependent variable.

Before continuing, I note that, because of the nature of the residency law, the law coverage does not depend on teachers' individual characteristics. Therefore, the treatment should not depend on these characteristics. However, the model might still suffer bias

¹⁵The information on union status is not available in the data. There are some studies proposing that the bargaining for public employees turns more around the axis of benefits and fringes rather than pay because taxpayers cannot observe the benefits directly (Inchniowski 1980, Freeman (1981)). Also, the influence of unionization and laws on public employees' wage determination process is different for different occupations. Wage determination for teachers seems to be less influenced by unionization. The survey of Ehrenberg and Schwartz (1986) on public employees summarizes the wage differentials for union and nonunion members, and these differences for teachers are very small in most studies. In addition, the study of Gyourco and Tracey (1989, 1991) suggests that there is a positive relationship between teachers' wages and the strength of union bargaining rights when the teachers' characteristics are not included. This effect, though, becomes insignificant when teachers' characteristics are included (Gregory and Borland 1999).

because of partial compliance and since the law allows for waivers and grandfathering. I cannot determine the teachers who are grandfathered and/or exempted by the school district. The reasons for the exemptions may be correlated with the employees' age or spouses' workplace (which is related to marital status). If there were a large number of waivers, this could introduce bias.

For the analysis, I use 2000 as the period before the law change with data from a 5% census sample. The sample is random and weighted to represent the entire population. In addition, I supplement the census data with the American Community Survey, ACS, a weighted 1% sample. The ACS questionnaires are almost identical to the 2000 census, with the exception of geographic variables ¹⁶. The 2000-2004 ACS samples contain no geographic information below the state level. It is important at this point to recall that the residency laws were abolished in 2001 for the treatment group. The 2000 census allows me to identify the individuals' cities of employment and residency. Since this information is not available in the 2000-2004 ACS samples, I pool the 2005, 2006 and 2007 ACS samples, which contains information on the city of residence and workplace, for the period after the law exempted public teachers from the residency requirement. This time gap, from 2001-2005, also allows the labor market to have adjusted to the law change. I pool three years in order to increase my sample size. Therefore, one should note that I have a series of cross sections rather than panel data.

In the data, there are teachers who report having only a high school degree or less. I drop such individuals from the study. There are also teachers in the category of "teachers not elsewhere categorized, n.e.c." who have some college education (these teachers might be substitute teachers or teachers in training). I include this group in my sample. I therefore limit my sample to those teachers who have at least some college education.

In this study, all the teachers "except post secondary" are considered as teacher, as defined under 1990 occupation categories in the data. These categories include kindergarten

¹⁶Integrated Public Use Microdata Series (IPUMS), Steven Ruggles, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2010.

and pre-school teachers, primary school teachers, secondary school teachers, special education teachers, teachers n.e.c., and vocational and educational counselors. I identify public school teachers as those individuals who have reported themselves as both being a teacher and a local government employee (city, county, municipal, etc.). I limit my sample to ages between 23 and 65 since the average age of new college graduates is 22. My sample consists of public school teachers who identify their place of employment as within the sample city borders.

The previous studies mainly discuss public workers such as police and firefighters. If a city enacts a residency law, these two groups of workers do not have an option to seek this line of work elsewhere within the city. However, public teachers do have other options to work within the city and avoid the residency requirement through the private sector. In the last table of this study, I include private school teachers to my sample to explore if the private teachers' labor market is indirectly affected by a change in residency laws. Private school teachers are identified to be teachers (as defined in the previous paragraph) who have reported themselves as private sector employees.

It should be noted that the ACS and census data ask individual earnings from the previous year. Therefore, the ACS data pooled from 2005-07 is more accurately data from 2004-06. Similarly, the 2000 census actually reports 1999 earnings. I use hourly wages in the sample in 2006 dollars deflated by regional consumer price indexes that are published by U.S. Bureau of Labor Statistics¹⁷. Hourly wages below the federal minimum wage and above 100 dollars are dropped from the sample.

2.5 Descriptive Statistics

The data summary can be found in Table B.2. According to the summary statistics, the share of black teachers significantly decreases in the treatment group after abolishing the residency law. All other demographics tend to slightly increase. Table B.2 also shows that

 $^{^{17}\}mathrm{Both}$ data sources report each respondent's total pre-tax wage and salary income - that is, money received as an employee - for the previous year. In order to find the hourly wage, I first multiply "usual hours worked per week" with "weeks worked last year" and then divide their wages for the previous year by that amount.

the share of public teachers with postgraduate degrees increases after the repeal of the residency laws, but the increase is only significant in the control group.

In the last two columns of Table B.2, I look at the public teachers' demographic characteristics of the two groups before and after the repeal of the residency law. In the period after the abolishment of the law, the only significant differences are found in race and education. The proportion of white public teachers in the treatment group under the presence of the law is 18 percent lower than in the control group. After the repeal of the law, the difference decreases by almost half, but remains significant. The share of public teachers who hold postgraduate degrees increases in both groups after the repeal of the law, but this increase is significantly higher in the control group. To address these differences, I control for race and education in my regression. Another point is that the mean age of teachers in the before period is significantly higher in the treatment group than in the control group, and this difference becomes insignificant in the after period. This might be a signal of an effect of the law on teachers' age composition.

The age distributions of public teachers are presented in Table A.3. The share of the two oldest cohorts seems to be very much affected by the law change, whereas the share of the public teachers in the age group 23 to 29 did not change much in both regions. The percentage of public teachers aged between 30 and 39 increases by 4.1 percent in the treatment group, though the same group's share decreases by 2.19 percent in the control group. The next older age cohort, which includes teachers age 40 to 49, seems to be the most affected by the law change. The share of the public teachers in this age group drops by 13.5 percentage point in the treatment group after the abolishment of the residency requirements, and it rises by 2.3 percentage point in the control group over the same period. This is the only group that is observed to decrease its percentage when teachers are not obligated by the law to reside within the city limits. The oldest age cohort, which includes teachers at age 50 and older, seems to also be quite affected by the law change. The percentage increase of this cohort is 9.2 in the treatment group, while the same age group's share increases by only 1.5 percentage points in the control group.

Even though the average teacher age does not change significantly before and after the

law change, abolishing the law seems to decrease the likelihood of middle aged teachers being employed in the city and attract teachers younger than 40 and older than 50. One reason for this trend may be that people between the ages of 40 and 50 might be more willing to settle down for a long period of time, while younger teachers have more opportunity to look elsewhere for jobs that offer greater freedom of mobility.

The next table delves deeper into the residency location of public school teachers before and after the law change. In the control group, there is no significant change in the share residing within city limits. According to Table A.4, the percentage of public teachers living within the treatment city significantly decreases by almost 8 percentage points when residency requirements are lifted.

The table also indicates that, in almost all subsamples, the abolishment of residency laws lowers the percentage of public teachers that live in the city. However the change in probabilities is only significant for married teachers. In the presence of residency requirements, the share of married teachers living in the city is much higher than that observed for cities in the control. The share of city residents among married public teachers becomes more equal in the control and treatment groups after the residency requirements are removed from the treatment cities. It is possible that married couples have a greater stake in providing for their family and are more likely to take a good job within a city that has a residency requirement. At the same time, if that city lifts the requirement, married teachers may opt to live outside the city, which might offer more spacious housing, a quieter atmosphere and a safer environment for families.

The data summary indicates a significant outflow of residents, and reallocation seems to be happening especially among married teachers. However, because I do not have a panel, I cannot track individuals, and therefore it is not certain whether this change is driven by hiring decisions and turnover or current teachers moving from the city. A concern among residency law proponents, that repeal will induce large outflows of the population, deems further investigation in section 8. According to the Tiebout (1956) hypothesis, when families with similar economic status decide their residence, they often search for similar public services, such as public schools, which cluster them in the same communities. To determine if this Tiebout sorting is prevented by residency laws, I examine how the private school enrollment of public school teachers children is affected.

2.6 Results

I first estimate the feasible GLS model (equation 2.1), which includes treatment group dummy, dummy for the period after the repeal of the residency law and the interaction of the two along with the following individual characteristics: the teachers' age, age squared, gender, marital status, race, whether the teacher holds a post graduate degree and the grade level taught. The results are presented in the first column of Table A.5.

The results predict that the repeal of residency requirements increases wages by almost 10 percent. The coefficient on the interaction term is statistically significant at the 5 percent level. The model also suggests that wages are decreasing over time by approximately 7 percent. In the third column, I replicate the regression to see whether the result are consistent when I use the logarithm of annual wages as the dependent variable. In this regression, I add an explanatory variable for hours of work. This model also suggests that the relaxation of the residency requirements increases teachers' wages, this time by approximately 9 percent again at 5 percent significance level. Overall, the DID estimates suggest that the repeal of residency laws increases teachers' wages by approximately 10 percent. This result is consistent with Hirsch & Ruffolo (1985,86).

These results suggest that the cities certainly do not offer higher wages in return for the residential restrictions they put upon their employees. It is possible that, instead of simply paying lower wages, the cities are actually recruiting a group of employees who are generally lower-paid; a labor pool that could be less qualified or could be a group that is discriminated against. The results appear to support the fear of residency requirement opponents that these laws decrease the quality of the workers so that the city pays lower wages. I believe these results prove that cities need to be very careful about the implications of these laws before enacting them. If the wages are lower under such requirements, the reasons for the lower wages should be deliberately examined. The change in the structure of the labor market and the quality measures of the workers should be followed after the adaptation of

such laws. If the aim of the city is to offer jobs to decrease the local unemployment rate, it should acknowledge the reasons why its residents are not chosen in the absence of residency restrictions.

The findings suggests that the wages are significantly higher for teachers who hold a postgraduate degree and who are married. The findings also suggest that the wages are lower for African American teachers but the estimated coefficient is not significant. The coefficient on male indicates that men earn one percent less than women when the dependent variable is the log of hourly wage and that men earn two percent more when the dependent variable is log of annual wage. But the coefficient on the gender dummy is not significant in the model. The estimates reveal a concave age-wage behavior as expected.

I further investigate if the laws affect teacher demographics different demographic groups differently. I separate my sample by marital status, race, gender, and education. The findings predict an increase in wages after the removal of the residency requirements in each subsample (Table A.6) with the exception of the nonwhite subsample. However, the results are not always significant.

One of the most intriguing finding is that wages for the teachers who do not hold a post graduate degree rise almost 20 percent after the repeal. This result actually supports the previous argument mentioned, that the residency laws decrease the quality of workers. Teachers who hold post graduate degrees do not seem to be affected by the law change, so one can infer that they are paid for their level of education. However, the wages for less qualified workers, in terms of education, are more subject to variation. I find some evidence that by shrinking the labor pool, the cities are recruiting less qualified workers for lower wages.

Another important effect is on the subsample for teachers who are married. The estimation results predict a striking 15 percent wage increase after rescinding the residency laws. As discussed in the descriptive statistics, there is a significant 10 percent decrease of married teachers living within the city after the repeal of these laws. It may not be that the married teachers are moving out of the city after the repeal, but rather the hiring of a new group of married teachers. One can expect that married people are less inclined to relocate due to having a spouse who is also working and may not be in favor of moving, the consideration of their children's schooling, or a desire for more spacious housing and a safer environment for their family. With an increased group of married teachers who are available to work within the city after the law repeal, it is possible that these include a more qualified, and probably more experienced, set of teachers. The results suggest that females are significantly affected by the law change, and the model predicts an almost 10 percent increase in their wages, whereas wages for men do not seem to be significantly affected by this law change.

In the fourth column of Table A.6, the group for nonwhite teachers, the coefficient on the interaction term is about -0.02, which signals that the repeal of the residency requirements decreases teachers' pay, but this effect is not statistically significant, and the sample is small. As mentioned before, there has been a discussion in court cases and in the literature that one of the motivations behind these laws is to protect minorities. Since the effect is insignificant and the sample size is small, the evidence I have is not enough to support this idea.

One concern might be that either Pittsburgh or Philadelphia may be driving the results. To address this, I look at city fixed effects. In the next table, (Table A.7), the results predict an 8 percent statistically significant increase in wages in Philadelphia. On the other hand, no significant effect is found in Pittsburgh. Perhaps this is not very surprising, because the main reason behind the repeal of the law was the concern regarding the teacher shortage in Philadelphia. It was in Philadelphia that legislators really wanted to do away with their residency law, whereas in Pittsburgh, the local government was not inclined to repeal the law. This would explain why the results are significant for only Philadelphia.

So far in this analysis, I have considered a control group that includes cities in which the public teachers were never subject to residency laws. In the second column of Table A.7, I instead use a control group in which public teachers were restricted by such laws, Buffalo. The residency law in Buffalo was lifted in March, 2011. The results still predict that the removal of residency laws increases teachers' wages, this time by about 15 percent.

Finally, I look at whether private teachers are indirectly affected by a change in residency
laws. In the next table, I limit my sample to private teachers. As is seen from Table A.8, no effect of the residency laws is detected. Hence, the law does not appear to have a spillover effect on the private sector.

2.7 Choice of Residence

According to the descriptive statistics of the data for public teachers, it seems that the composition of the teachers living inside and outside of the city limits is different after the repeal of the law. It looks like public teachers are moving out of the city after the residency requirements are lifted, however one might be concerned that I do not actually have panel data to confirm the idea. In order to further investigate whether there is a relocation of teachers due to residency laws, I look at where public teachers are sending their children for schooling. If a public teacher would rather send their child to a better school outside of the city for which they work but are restricted from moving to an area with a better public school, they will choose to enroll their child in a better choice that is available to them, i.e. a private school. In particular, I analyze how the probability that public teachers will enroll their children in private school changes after the repeal of the residency law.

Duncan (2005) also inquires how the residency requirements affect municipal workers, other than teachers, families' residential choices by looking at private school enrollment. He suggests that a residency requirement interrupts families' mobility and therefore ruins the families' Tiebout behavior. According to the Tiebout (1956) hypothesis, people move from community to community considering the public good produced in each community and choose the one which maximizes their utility. The Tiebout (1956) hypothesis, in this way, explains that families with similar economic status are often searching for similar public services which cluster them around the same communities. When a family decides their residence, they consider not only the cost of living but also school quality in a region. Duncan (2005) uses public schools as an example of a highly demanded public service for families and finds that more municipal workers tend to enroll their children in private schools rather than public schools in the presence of residency requirements. In other words, this is a way of understanding if a residency law prevents Tiebout sorting of the public employees.

Duncan compares the rates of private school enrollment of children of municipal-employed families to those of non-municipal workers using difference-in-differences estimation. He conducts his analysis for each city separately. Duncan stresses that the residency requirements are effective only if the law is accompanied with a municipal wage premium policy. His findings suggest that, among the cities that have residency laws, those that offer a municipal wage premium are more likely to experience significantly higher private school enrollments of the children of municipal workers.

I use the same data sources described in the data section of this paper. I consider again the individual level data, this time using the children of the public teachers between 3 and 18 years of age as observations. The descriptive statistics of the children can be found in Table A.9. Almost half of the children are male and 65 percent are white. Most of the children in the data have married parents, and they are more or less divided equally among elementary, middle and high school children. I run the following probit model.

$$E(y|after, treatment, \mathbf{X}) = Prob(y = 1|after, treatment, \mathbf{X})$$

$$= \Phi(\beta_1 \ after + \beta_2 \ treatment + \beta_{12} \ after \times treatment + \delta \mathbf{X})$$
(2.2)

where $\Phi(\cdot)$ is the standard normal cdf. The variable y is a binary variable equal to 1 if the public teacher's child is enrolled in the private school and equal to 0 otherwise. The variable *after* is 1 for the period after the repeal of the residency law and 0 for the period before this change. The variable treatment is a group dummy indicating whether the child is coming from the treatment group (=1) or from the control group (=0). The interaction term is defined as the product of these two dummy variables. The X is the vector of individual and family characteristics. I control for the child's race, age and the grade level. I also control for the marital status of the parents and total family income.

Before presenting the results, it is worthwhile to reserve a small discussion on the interaction effect of nonlinear models. There has been a debate on how to present the probit estimation results of DID method. The discussion revolves around the fact that the interaction effect of the independent variables, say x_1 and x_2 , is the cross-derivative of the expected value of y^{18} . Ai & Norton (2003) stress the fact that an interaction effect could be nonzero even if $\beta_{12} = 0$ and that it is conditional on the independent variables. They point out that it is possible that the magnitude and the sign of the interaction effect and the statistical significance may vary by observation. They suggest that "the statistical significance of the interaction term cannot be tested with a simple t-test on the coefficient of the interaction term" (Ai & Norton (2003) p.124). Therefore, they suggest to plot the predicted probabilities and interaction effects and also to plot z-statistic of the interaction term effect and its statistical significance.

Another important paper, Greene (2010), argues that the process of statistical testing about partial effects, and interaction terms in particular, produces generally uninformative and sometimes contradictory and misleading results mainly because of the fact that the unit change in the relevant variable may itself be unreasonable. Hence, he says that the "common practice of testing hypotheses about partial effects is less informative than one might hope, and could usefully be omitted from empirical analyses" (Greene 2010 p. 291). He proposes that the analysis should be presented in two steps: First, statistical testing should be done on the model coefficients. Then, one should present the partial effects and predictions. No hypothesis testing needs to be done at this step. Rather, graphical presentations can be more informative than a numerical statistical demonstration of the estimation results.

Table A.10 presents the estimation results of the probit model (equation 2.2). In this table, I present the estimation results along with the marginal effects of the variables in which I look at the statistical significance of both coefficients and marginal effects. According to the table, the probability that public teachers will enroll their children in private schools decreases by 0.17 percentage points after the repeal of the residency requirements. This decrease is statistically significant at the one percent level. The table also shows that the private school enrollment seems to be significantly larger in the treatment cities. The

$$\frac{\Delta^2 E(y|x_1, x_2, X)}{\Delta x_1 \Delta x_2} = \left(\Phi(\beta_1 + \beta_2 + \beta_{12} + \delta X) - \Phi(\beta_2 + \delta X)\right) - \left(\Phi(\beta_1 + \delta X) - \Phi(\delta X)\right)$$

¹⁸The interaction effect for two dummy variables is

marginal effect of the interaction term is negative and statistically significant, but as discussed earlier, the common practice of testing the coefficient and/or marginal effect is a poor way of explaining the effect. Table A.11 shows the discrete double difference of the interaction term as the interaction effect along with the standard error and z-statistic¹⁹. The results here suggest that the probability that public teachers will enroll their children in private schools decreases after the repeal of the residency requirements by 0.15 in the treatment group. The increase in the enrollment probabilities to private school is reversed with the repeal of the law. Perhaps one way of looking at this situation is that the trend of increasing the likelihood of private school enrollment is not followed by the treatment group because they would not enroll their children in private school unless their residential choices were restricted.

Figure A.1 plots the predicted probabilities of private school enrollment and the interaction effects, which are the effects of the repeal of the residency law. The figure shows that the interaction effect is always negative for all observations and that the effect ranges between -0.25 and -0.05. The interaction effect has the same sign for different values of the covariates, which, if we recall, is the main concern of Ai and Norton; to find different signs for different observations. Figure A.2 depicts t-statistics as a function of the predicted probabilities. The figure indicates that z-statistics do not have much variance for different predicted probabilities and seem to be significant in this range.

For a more detailed investigation, I use a graphical approach. Table A.12 shows the interaction effect for all possible periods and groups (Note that one can reach the interaction effect in Table A.11 by calculating difference in differences using this table). The effects are plotted in Figure A.3. The figure shows that, in the existence of the law, the treatment teachers are more likely to enroll their children in private schools compared to the control group. This difference almost completely disappears after the repeal of the law.

The next couple of figures, Figure A.4 and Figure A.5, look at the interaction effect for different age groups and family income respectively. The interaction effect is larger for younger children (Figure A.4). It makes sense that the repeal of the law especially affects

 $^{^{19}\}mathrm{Norton}$ et al. (2004) provides a detailed discussion on the interaction effects and Stata code for the analysis.

the enrollment probabilities of younger kids, as families are more protective of younger ones and therefore try to enroll them in a school that is as close to their residence as possible and also look to give them the best start possible in their schooling. It can be seen in Figure A.5 that the effect of the law change on the probability of private school enrollment does not appear to vary depending on the family income. One should note that the last two figures suggest that I should be careful in interpreting the findings; the results appear to be driven by the changes in the control group. For instance, according to Figure A.3, the enrollment probabilities stay almost the same before and after the repeal in the treatment group but increase in the control group after residency requirements are lifted. This change can be seen in Figure A.4 and Figure A.5 as well across different age groups and income levels. To conclude, suggestive evidence shows that this decrease could be driven by some change in the control group.

2.8 Conclusion

In this paper, I analyze the impacts of residency laws on teachers' wages. This study differs from other studies in the literature in three ways; it focuses on teachers rather than protective workers; it uses individual level data instead of aggregate level data; and it employs a different estimation method, difference-in-differences. The findings suggest that the repeal of residency laws increases wages by approximately 10 percent. This effect is stable among almost all of the different specifications discussed in the previous section. Some demographic groups seem to be more affected than others; the law change appears to have a greater impact on married teachers and teachers who do not hold a postgraduate degree.

These results are consistent with the Hirsch and Rufolo (1985, 1986) findings on police and firefighters, but contrast the findings of Getz (1979), Gonzalez et al. (1991), and O'Brien (1997). The different results might be driven by differences in data (such as different data sizes, different groups of municipal employees, different coverage of residency laws in different cities) and methods applied in each study. The assumption that residency laws are exogenous is more realistic in my study since I consider individuals, and individuals do not directly influence the enactment of the residency laws. On the other hand, previous studies consider cities as observations, and cities have legislative power on the passage of such laws. It therefore seems less likely that the exogeneity assumption will hold in their setting. Additionally, previous studies compare cities with residency laws to cities without these laws. These cities, in fact, might be very different even after controlling for some characteristics. Using individual level data allows me to enter a full set of individual characteristics and better control for several compositional differences that may arise across cities. In addition, difference-in-differences strategy removes fixed group effects and time effects which should reduce the number of confounding factors that might otherwise be driving wage determination. The results of this paper are consistent with the non-standard model's²⁰ prediction of lowering wages, although it may also fit to the standard model, which predicts ambiguous effect on wages.

Moreover, evidence suggests that the residency requirements also affect the residential choices of families. This being one of the arguments of residency law proponents, it is important to examine whether the repeal of residency laws will lead to a migration of teachers from the city. Teachers with children who would want to move out of a city without the restrictions of a residency law would be likely to enroll their children in private schools. In examining private school enrollment, the findings show that the probability that public teachers will enroll their children in private schools decreases when residency laws are lifted. Furthermore, the difference in enrollment probabilities between the control and treatment groups seems to disappear in the absence of these requirements.

Possible implications of residency requirements on the labor market relating to the public coffer theory and productivity effects are as follows: Looking at the effects directly related to labor market functioning and labor market outcomes, prior results suggest that residency requirements can be used to help cities in financial turmoil. Since public teachers' wages are lower in the presence of the residency laws, city legislators can apply these requirements

²⁰Unlike Hirsch and Rufolo (1985, 1986), this study suggests a decrease in labor supply with residency laws, which is compatible with the standard model. But we can impute the increase in supply due to positive job attributes (Gonzalez et al. 1991) and the decrease in derived demand for labor due to a decrease in demand for public schools because of the quality change. The decreased quality in public schools may increase the demand for its substitute, private school. Therefore, the findings may support the Non-standard model.

to decrease labor costs. When this effect is combined with the well known public coffer theory, which suggests the recirculation of public salaries back to the city through taxes and spending, these laws can be used as a tool to help the city budget, especially in times of recession.

However, a potential explanation for the increase in wages after the repeal of residency laws could be an increase in quality. Opening up the market for a larger number of teaching candidates by abolishing residency requirements may help cities to choose the most qualified teachers. The common idea that residency laws increase productivity of workers through proximity and familiarity factors may not apply to teachers. The proximity factor is irrelevant for teachers, and a familiarity bond does not necessarily increase productivity, though it can create positive job attributes.

To conclude, residency requirements for teachers appear to impact wages and might have broader implications, not only in the labor market (through changes in productivity and quality), but also on the city community as a whole (through differential impact on spending). Given the importance of teachers in the development of the human capital, understanding regulations in their market remains an important area for policy debate and academic research.

Chapter 3

Determinants of Teachers' Collective Bargaining Rights

This study focuses on the political and economic drivers that result in enactment of collective bargaining laws. I conduct a survival analysis to better understand the timing of the likelihood of abandoning a more restrictive collective bargaining rights policy during the time period of 1964 to 1996. I also analyze the impact of state-level economic and political conditions on the likelihood of adoption of mandatory collective bargaining for public teachers using a dynamic logit model. To the best of my knowledge, this paper represents one of the most comprehensive attempts to date to investigate the determinants of collective bargaining rights in the public sector.

Collective bargaining laws have always been controversial. One of the most intense debates began in September 1968 when public classrooms all over New York City remained empty. Instead, the streets were filled with teachers carrying signs protesting the decentralization of the city's schools. Decentralization was seen by the United Federation of Teachers (UFT) as an attempt to thwart teachers' bargaining positions (Podair 2002). The result was a teachers' strike that lasted for months. Around one million children in the largest city in the U.S. were affected, and collective bargaining agreements for teachers, among other issues surrounding the strikes, came into view of the public eye (Podair 2002). Despite controversy, a series of laws regarding collective bargaining came into effect since then in almost all states.

Over 40 years later, the nation's eyes turned again to collective bargaining rights for teachers in Chicago and Wisconsin, where proposed restrictions of public employee collective bargaining resulted in protests and strikes throughout 2011 and 2012. The extreme backlash from the successful attacks on collective bargaining in Wisconsin sparked senatorial recall elections in 2011, and gubernatorial and senatorial recall elections in 2012. Some argue that collective bargaining rights for public employees undermine local elected governments' control over their operations and hinder government officials' efforts to manage budgets, while others argue that these laws actually help local governments to better organize public sector labor markets and improve the education system (Freeman & Han 2013). The debate regarding collective bargaining rights remains unresolved and encompasses economic, legal, and political issues.

Past literature has focused primarily on the effects of collective bargaining on the labor market. Some studies have examined effects on compensation and unemployment among teachers. Others have examined impacts on teachers' quality by looking at student performance. Despite the debates and controversy, there have been few studies investigating the factors that determine collective bargaining laws.

The unique aspects of public employee collective bargaining laws draw attention to the need for specific and detailed studies of these laws. Mainly, local governments face a dilemma; budget pressures require aggressive management of expenses, including among other things, teachers' compensation, while residential property values generally depend on the quality of local schools. In this paper, I study an array of characteristics to identify which combination of factors prevail in enactment of collective bargaining laws.

On the political side, I use several different ideological measures in order to control for possible connections between enactment of collective bargaining laws and political power differences between Democratic and Republican parties: First is Ranney's competition index which evaluates the political ideology of a state by looking at the Democrat Party's dominance in state government. An alternative is based on the notion that political party ideology can vary greatly across states (the Republican Party in NJ is likely more liberal than the Republican Party in Alabama, or maybe even the Democratic Party in Alabama). Berry et al. (1998) measure government ideology using weighted average of the ideological position of major actors in state government: the governor, and two major parties in each of the legislative chambers. Therefore, this second index measures a state's conservative or liberal leanings without consideration of party lines, other than their position as a majority or minority in the legislature. I use their government ideology index to see whether it gives better information than the Ranney index. I also use a measure of government control, veto proof, which represents whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto. Using these indexes should capture the political ideology of a state.

On the economic side, I investigate the possible effects of states' public school resources in the previous year on the enactment of collective bargaining laws during the next period, by controlling for per pupil spending and student-teacher ratios in my analysis. Some literature argues that budget pressures in states and/or in the school districts might influence the enactment of collective bargaining laws. Some legislative histories of collective bargaining and anecdotal evidence, e.g. from New York City during the 1960s, indicate that wages, working conditions, and school funding have been motivating factors for adoption of collective bargaining rights for public school teachers. Accordingly, one would think that such economic variables as well as states' school budget pressures influence the enactment of such laws.

Additionally, I include local economic conditions in an effort to capture changes in labor markets due to budget reallocations among public services. I control for states' per capita income and state government total expenditures as a percent of state total personal income to capture such effects. Finally, to control for local conditions, I use the ratio of public workers out of the workforce, states' school age population and state level crime rates.

The purpose of this paper is to investigate the impact of state and school district specific demographic, political, and economic variables on the decision of enacting collective bargaining. One approach in this study is to use a model that examines the relative impacts of states' economic and political conditions on the duration of strict collective regimes. Another approach is to employ a dynamic logit model to account for history dependence (the decision to enact stronger collective bargaining laws might be closely related to the law of a previous year). In particular, I examine the effects of state and school district specific demographic, political, and economic variables on the likelihood of enacting collective bargaining rights.

The survival analysis predicts that those states that have higher per pupil expenditures

and lower per capita income seem to experience longer durations of strict collective bargaining regimes. The results suggest that the states that have higher per pupil expenditures and a higher student-teacher ratio experience lower duration of strict bargaining regimes. The results also suggest that higher per capita income and higher percent of states' expenditure to total income are associated with longer durations of strict bargaining regimes. Lastly, political conditions in a state do not seem to have an important effect on the duration of strict collective bargaining regimes.

Similarly, the empirical results of the dynamic logit model suggest that a state's political environment is not an important factor in the decision to adopt collective bargaining and that the effects and significance of a state's economic conditions are mixed in importance. In particular, per capita income has a significant negative effect on the likelihood of adopting collective bargaining, but the percent of states' total expenditures to states' total personal income has no significant effect. Instead, this study reveals certain characteristics of a state's public school system that are more important factors affecting the likelihood of collective bargaining. In particular, the findings predict a positive relationship between the probability that a state will adapt bargaining laws and the state's per pupil expenditures as well as student-teacher ratio.

The remainder of the paper proceeds as follows: Section 2 presents an overview of collective bargaining rights; Section 3 reviews the literature; Section 4 describes non-parametric survival analysis and discusses the results; Section 5; Section 6 describes the dynamic logit model and discusses the results; Section 7 concludes.

3.1 An Overview of Collective Bargaining Rights

Collective bargaining rights, both as to their nature and effects of their application, have a long history and continue to evolve today. While generally employees have a constitutional right of association including organization into unions, applicable law and regulation differs for the private sector from the public sector. One of the main goals of collective bargaining in the private sector is to maintain "industrial democracy" in order to balance the power that employees hold over employees. This leveling of the playing field in the public sector context raises the issue of whether collective bargaining amounts to illegal delegation of government power to non-elected workers (Wellington & Winter 1969). To preserve the "sovereignty of public employers", collective bargaining was very limited in the public sector in the early twentieth century.

Gradually, demands of public employees to have the same rights as private sector employees led governments to grant collective bargaining rights to public employees. Some local governments were sympathetic to public employees' demands regarding collective bargaining rights due to the belief that the public sector would benefit from these rights. Wellington & Winter (1969) argue that in, the private sector, collective bargaining laws promote understanding of issues from both parties' perspectives, allow workers to participate in their governance, help unions give the worker community greater say in the political arena, and balance the power between employer and employees. The first three of these arguments could be justified in the public sector, but the fourth argument is harder to justify since it conflicts with the notion of sovereignty of government.

Therefore, public sector employer and employee negotiations are much more complicated than in the private sector, and the outcome of these negotiations have greater impact on communities. Government services such as education, protection, waste removal services, etc., are essential for every community, and their quality and quantity are particularly important to taxpayers. However, the valuation and cost analysis of these services are very difficult since the government procurement process can be costly and inefficient. Hence, from constituents' perspectives, if public employee union negotiations result in increased costs of their services there is no "market" in which consumer demand can shift, the only recourse being a political election process response or moving out of the jurisdiction. Consequently, in the public sector, when costs increase, it is more difficult to raise taxes than it is to raise prices in the private sector. Accordingly, governments' increased labor expenses will more likely be offset by reductions elsewhere in government services, potentially lowering the quality or quantity of the offset services. Additionally, with taxpayer demand for government services being more inelastic, since there are fewer alternatives as compared to the private sector, extended disputes resulting in public employee strikes are much more damaging to taxpayers than private employee strikes are to consumers.

In this work, I focus on the public school teachers who have both similarities to and differences from other public and private sector employees: Public school teachers may be subject to different qualification standards and teaching performance standards than private school teachers, but they compete in the same labor markets to some extent. In this labor market competition aspect, public school teachers differ from many other public employees, e.g. police, corrections, fire, emergency workers, in that there are no competing private markets for such other public workers. The nature of services provided by public school teachers also differs from such other public workers in that education services' benefits are more long-term and are more difficult to value, than for example police, fire, and sanitation workers' services. All of these differences contribute to, or at least partly explain, the different sides of the debate regarding collective bargaining rights for public employees.

Collective bargaining rights for public school teachers result in relationships, duties, and impacts among several parties or groups: public school teachers, unions representing public school teachers, school district officials (employers), state and local policymakers (and perhaps federal policymakers), and consumers (taxpayers) including public school students. Indirectly, other types of public sector employees, and private sector teachers and their employers are affected by public school teachers' collective bargaining rights and outcomes.

Lindy (2011) effectively lays out the arguments for and against collective bargaining laws for public school teachers: Proponents take the view that collective bargaining laws (1) preserve/protect the "basic level of dignity" for teachers, i.e. more professionalism resulting in higher "level of care and responsibility", increasing teaching quality, which benefits the educational system; (2) improve working conditions, encouraging teachers to work better and to stay in their jobs longer, gaining greater experience, which benefits the educational system; and (3) aid teachers to identify and push for improvements in education, reforms that otherwise would less likely be considered by district officials. On the other hand, Lindy (2011) points out that opponents of collective bargaining claim that collective bargaining is a more expensive process, restricts government's flexibility to administer necessary changes, and results in higher taxpayer costs, all with unclear benefits to students. For example, government may be unable to reward great teachers or remove bad ones or hire good new teachers. Collective bargaining also "distorts democratic accountability of public school systems" (Lindy 2011, page 1141). Unlike the private sector, public school teachers' unions can use political pressure to influence government officials' decisions and actions.

Furthermore, Lewin et al. (2012) argue that demand for public sector workers is inelastic and unions would exploit this in negotiations to overpay public sector workers compared to private sector workers (This argument is a two way street. From supporters' point of view, through collective bargaining, public school teachers are more able to secure better wages and benefits). An additional argument is that collective bargaining rights hamper governments' flexibility in making adjustments in response to financial crises and recessions (Freeman & Han 2013).

3.2 Literature Review

The economic literature on collective bargaining laws focuses for the most part on the impact of collective bargaining laws on labor market outcomes. Most of the studies investigate how collective bargaining rights are related to public employees' pay, benefits, and employment, and some of them look at the effects of these laws on school resources (such as teacherpupil ratio and per pupil expenditures), and teachers' productivity by looking at students' performance (Hoxby 1996; Lovenheim 2009; Lindy 2011; Freeman & Han 2013; Brunner & Squires 2013; Frandsen 2015). The literature also discusses union wage differentials between public and private sectors. The rest of the literature concentrates on the effects of private sector unionization. Although there is a huge literature looking at the outcomes of collective bargaining laws there is a handful of studies examining the drivers of collective bargaining laws. Below, a summary is provided of results from that literature.

The literature introduces two theoretical approaches for the enactment of collective bargaining laws coming from two prominent papers: Farber (1988), and Waters & Moore (1990). Farber uses a cost-benefit approach, Waters and Moore uses a supply-demand approach. In particular, Farber (1988) investigates the evolution of public sector bargaining laws for the period 1955-1984. He suggests that the decision to enact collective bargaining laws depends on the "costs of policy change" and the state's "intensity of preferences for public sector unionization". The intensity of preferences for public sector unionization represents the attitude of a state toward public sector unionism. That is the more favorable a state is to unionism, the more likely that state will enact laws that provide greater collective bargaining rights. The *cost* is the cost of enacting legislation. Farber argues that a state will enact a new policy if the value of intensity of preferences associated with the new policy is greater than the value of the existing policy plus the cost of passing the law. He then examines the factors that determine the costs and intensity of preferences for three employee groups (state employees, police, and teachers) using a Markov transition model. He considers the number of days the state legislature meets, the number of bills enacted by state government, a dummy variable representing whether the legislature and the governorship are controlled by the same party, and a time trend as the factors that determine the "costs". Moreover, he considers the COPE (the Committee on Political Education) score of the state's congressional delegation, the extent of private sector unionization, a dummy variable representing whether the state is in the South, per capita income, the ratio of state and government expenditures to income, and a time trend as the factors that determine the "intensity of preferences". Although his results suggest that the cost variables are not significant in explaining a policy change, the variables used to define the "intensity of preferences" are significant with the exception of extent of private sector unionization.

On the other hand, Waters & Moore (1990) suggest a demand and supply analysis to explain the enactment of collective bargaining laws. They employ the following reduced form model

$$L = L(IG, OG, TG, T, C, EC)$$

$$(3.1)$$

where L is the legal status of collective bargaining rights for public employees. On the demand side they consider four factors: (1) interest group (IG) which by definition is the group that benefits the most from the legislation, (2) opposition group (OG) on the other hand represents the group that will be negatively affected by the legislation, (3) third party group (TG) represents the group that will indirectly benefit from the legislation, and (4) taste variables (T) which include attitudes of public employees and voters in a state. On the supply side they consider a number of political cost variables (PC) to capture the role of politicians in determining public policies under pressure of voters (including the dynamics between opposing and supporting interest groups) and also economic cost variables (EC) to capture the cost of introducing and enacting legislation.

Waters & Moore (1990) consider the time period from 1970 to 1980 in their analysis. They categorize bargaining rights into three groups to create a categorical variable as their dependent variable: The first category includes the states that have no bargaining provisions or those that prohibit bargaining; the second category includes the states that have mandatory meet and confer law; the third category includes the states with mandatory bargaining laws. On the demand side, they use percent of public sector unionization (to capture IG); the number of unfair labor practice cases charged against an employer (to capture OG); percent of private sector unionization (to capture TG); nonwhite population and average salary in the state (to capture tastes). On the supply side, they use COPE ratings, the proportion of neighboring states which had passed mandatory bargaining laws to capture the effects of political cost variables. Furthermore, they include the ratio of legislators from the majority party to the total number of legislators to capture the effects of economic cost variables.

Their results suggest that collective bargaining legislation is significantly and positively affected by the extent of public sector unionization, the percent of nonwhite population and salaries of public employees. Moreover, it is negatively affected by the number of unfair labor practice charges against employers. They find that the higher the politicians' positive attitudes toward unionization, and competition between political parties, the higher the likelihood of enacting collective bargaining laws. Finally, they suggest that the analysis of bargaining laws should be treated endogenously. The authors also examine whether some measures for federal revenue shares of a state have a significant effect on the determination of collective bargaining laws but they do not find a significant effect.

An early work by Hunt & White (1983) studies the socioeconomic characteristics of populations that affect the extent of collective bargaining laws for public school teachers. They argue that state legislation is a mirror of the community and its subgroups, and that the legislation causes wealth redistribution. Therefore, they investigate the groups who benefit and those who lose from teachers' collective bargaining laws. The authors reason that working conditions affect the demand for bargaining which in turn affects the demand for bargaining legislation. As far as working conditions go, they further argue that salary, pay differences between males and females as well as between primary and secondary school teachers, classroom size, and layoffs may be important determinants of demand for bargaining. Regarding wealth redistribution, they argue that legislators aim to maximize their votes by enacting policies to satisfy the special interest groups representing the largest segments of the population.

They estimate the status of collective bargaining laws on a scale from 1 to 4, the higher values representing more favorable collective bargaining laws, based on various characteristics using data from 95 SMSA in 35 states. They use the number of school districts relative to population and the average wage of teachers relative to median male income to capture conditions of employment that affect demand for and benefits gained from lobbying for pro-union laws. They include state and local spending on education as a percent of state per capita income, and note that these variables may negatively or positively impact enactment of collective bargaining, depending on whether educational funding legislation is independent from or complementary to collective bargaining legislation. They argue that, where there is high financial support, the demand for bargaining is likely to be relatively lower. Hence state funding is an important factor.

Also, they point out a possible spillover effect among public employees, if a group of public employees benefit from the bargaining law, it will spread to other public employee groups. This spillover effect may apply to the private sector indirectly because of fairness concerns of the legislators to the other groups in the workforce. They argue that the large number of health and government workers affects labor organization which in turn, through spillover effects, has a positive impact on the legislation of teachers' collective bargaining. They aim to capture the spillover effect by including percent of private sector unionization, percent of non-practitioners employed in the health profession, and percent of government employees. Additionally, they consider possible negative attitudes toward union power, and therefore control for the number of unfair labor practice charges per representation election in their analysis.

As expected, one of the groups probably most affected by teachers' bargaining laws are parents of school age children, who would support any proposal that would improve their state's education system. To that end, Hunt & White (1983) argue that parents support any policy that would improve school districts including bargaining rights. So the authors control for the percent of school age population to account for this in their analysis. On the other hand, taxpayers' incentives are to lower the tax burdens that may result from collective bargaining legislation, but their opposition would be less if residency or employment in the district is temporary. Thus the authors include estimated labor force separation rate as a control variable.

They use median male education level as a proxy for income, arguing that it will have a positive impact on bargaining legislation, assuming education is a normal good and bargaining improves educational services. Lastly, they include percent of teachers in the labor force to represent the size of the most related interest group for bargaining laws and percent of male teachers as a proxy for income.

Their results suggest that bargaining legislation is positively associated with the percent of male teachers, state and local spending on education as a percent of state per capita income, percent of non-practitioners employed in the health profession, extent of private sector unionization, and labor force separation rate. Their results also indicate that bargaining legislation is negatively associated with number of unfair labor practice cases charged against an employer and percent of teachers in the labor force. They conclude that working conditions, monopsony power, salary differentials, and size of student population do not have a significant impact on teachers' bargaining legislation.

Another important study, Saltzman (1985), examined the relationship between public sector collective bargaining laws and the growth in teachers' unionism (between 1959 and 1978), and concluded that enactment of such collective bargaining laws was the most important factor in the unionism growth rather than the reverse, contrary to the common view in the literature. Moreover, he found that the increase in teachers' unionism had a weak effect on the adoption of collective bargaining laws. In fact, he showed that the extent of political patronage in a state and the bargaining laws adopted in neighboring states had a significant effect on the adoption of these laws.

Saltzman (1985) summarizes the factors that may affect the extent of teacher bargaining which I briefly describe next. He argues that the percent of male teachers and average school size are important school characteristics. His reasoning is that male teachers are more likely to unionize, but he noted that the women's movement would reduce this gender disparity. Regarding school size he argues that in more crowded school districts, teachers are more aggressive and stronger, giving them more power because they can pressure policy makers with the prospect of a strike. He also mentions that in large school systems, it is easier for school systems to bargain collectively with teachers rather than individually. As to the variables regarding labor market conditions, he argues that lower wages became the main stimulator for teachers to go on strike even when the wages were relatively better around 1960, mainly because even though their wages went up, their wages were still lower than for many blue-collar workers. Therefore, in order to control for labor market conditions, he uses unemployment rate from the previous year, teachers' earnings relative to earnings of workers in manufacturing, and growth rate of teachers' employment.

He addresses the effect of membership in other unions, arguing that a strong labor movement in a state would encourage teacher bargaining rights as well. He includes union membership and also the fraction of non educational public workers who are members of the American Federation of State, County, and Municipal Employees (AFSCME). Thus he needed to control for attitudes toward unionism and he uses COPE voting scores for this purpose. He further explores this effect by including income and a dummy variable for states in the South. The author argues that states in the South are less developed and more willing to attract industry, thereby seeing unions as obstacles. In particular, public sector unionism was believed to drive wages up, which would in turn increase taxes, which would lead to a decrease in investment. Lastly, he includes a dummy variable for percent of population living in urban areas. He argues further that the expected change in extent of bargaining laws may depend on the extent of bargaining laws in the past, and he includes variables representing different enactments of bargaining laws in different time periods in the past.

Next, he lays out the factors that may influence the nature of *teacher bargaining laws*: First, he considers union membership. On one hand, politicians would be more responsive to public employees with a more powerful union, thereby increasing the likelihood of enacting more favorable collective bargaining laws. On the other hand, politicians may want to keep public employees' strikes or other demands under control by not providing them bargaining rights. He uses the lagged proportion of teachers covered by collective bargaining agreements and the lagged change in that proportion, as well as AFSCME membership and total union membership. Attitudes toward unions is controlled by COPE score, urbanization and income measures.

He reasons further that in states that adopt a merit system, politicians lean toward better bargaining laws as opposed to states where public workers were awarded jobs after working on political campaigns. A variable that measures the percent of public employees protected by a civil service merit system is therefore used in his analysis. Another point he makes concerns regional patterns. He argues that state governments pass similar laws to those of neighboring states because they consult each other about local policies. He uses four variables to control for such regional patterns: the fractions of states that prohibit bargaining, that mandate bargaining, that have a more pro-union law, and that have a less pro-union law.

He further suggests that teacher bargaining laws began to be more common as the Democratic Party started winning more elections. He measures the changes in Democratic strength using the percentage change of Democratic party seats in the house of legislature with a subordinate Democratic Party and the change in the number of Democratic majorities held throughout a state's governmental bodies. He also points out that a Democratic majority does not necessarily show a liberal state legislature. For instance, he mentions that in the South, most legislators, both liberal or conservative, were affiliated with the Democrats. Lastly he adds six dummy variables representing the bargaining law categories in the previous period.

In his bargaining law model, he considers the years biennially from 1959-60 to 1977-78. He employs a logit model in which he uses Probability of Change to Higher Law Category as the dependent variable. The model predicts that the *expected probability that a state's bargaining law will change to a more pro-union category in a given biennium* is weakly related to the extent of the bargaining law. The probability is significantly related to union membership, real per capita income, Democratic Party control, state jobs not subject to patronage, dummy variables representing the fractions of states with various types of bargaining rights, and dummies representing previous periods' categories of legislation.

A prominent study, Freeman and Valletta (1988) investigated how labor laws across states affect collective bargaining status, wages and employment in the U.S. for teachers, police and firefighters, and local workers. They find that state public sector labor laws have a significant effect on the probability that municipal workers are covered by collective bargaining contracts. To capture the scope of the collective bargaining laws they prepared a dataset, "NBER Public Sector Collective Bargaining Law Data Set", which tracks the status over time of state collective bargaining laws from 1955 to 1985. Their dataset has been used in the literature either directly or as a starting point to create a new measure of the scope of collective bargaining rights. I use an updated version of their data and the details are given in the Data section of this paper.

Hill & Waters (1995) is also an important paper in understanding the coverage of public teachers by unions. In their study, they examine the relationship between the extent of teacher bargaining and states' legal environments using a simultaneous equation model. They measure the extent of teacher bargaining by the proportion of public school teachers who are covered by collective bargaining contracts (*teacher bargaining*), which depends on the presence of bargaining laws as well as public toward teacher bargaining and unionism. Presence of bargaining laws is represented by dummy variables derived from the three categories as used commonly in the literature: whether the state has no law or prohibits collective bargaining, whether the state has a "permissive" collective bargaining laws (i.e. employer authorized but not obligated to bargain with the union, and union has right to present proposals and to meet and confer), and whether the state has mandatory collective bargaining. Furthermore, they include average teachers' wage, arguing that union representation is a normal good and should affect bargaining positively; percent of agricultural sector in the state's labor force as a proxy for union organizational costs; a binary variable indicating whether the state is in the South, as a proxy for negative attitudes toward unionism; percent of private sector unionism, as a proxy for pro-union sentiment; per capita income, arguing that education is a normal good and therefore as demand for education goes up so does support for teachers bargaining rights; proportion of neighboring states that enacted mandatory bargaining rights in the past, arguing that legislators consult with neighboring states when considering new legislation (as proposed by Saltzman 1985).

In their model, public sentiment toward collective bargaining and unionism are simultaneously determined and depend on *teacher bargaining*, presence of bargaining laws (as explained above), extent of private sector unionization, percentage of classroom teachers in the civilian labor force (to represent their interest group), and ratio of classroom teachers to number of school districts (to capture the effect that larger school districts have more union organization). They also include the number of unfair labor practice cases charged against employers in the prior year which may have a positive or negative effect: Since the number of such cases represents the opposing group, it would create negative sentiments, but it may also lead to a more organized private sector labor movement. Additionally they include wage, arguing that it can either have a positive effect due to the positive relationship between demand for education and demand for bargaining rights, or have a negative effect due to damaging sentiment that may result from low earnings.

Their results suggest that the dummy variable indicating whether a state is in the South, percent of agricultural workforce, and proportion of neighboring states that enacted mandatory bargaining rights in the prior period have a significant positive effect in determining the proportion of teachers covered by a collective bargaining law. Also, they find that the variables wage, percentage of classroom teachers in the civilian labor force, ratio of classroom teachers to school districts, and unfair labor practice cases have significant effects on sentiments toward collective bargaining.

3.3 Non-parametric Survival Estimation

This paper aims to understand why some states adopt more or less restrictive collective bargaining regimes than others, and particularly tries to find out the main determinants of the decision of enacting collective bargaining laws. In order to understand how likely it is that a state will change its laws over time, I conduct a survival analysis of collective bargaining in which the failure is defined to be "moving from a prohibited collective bargaining regime to bargaining regime". More specifically, the event is whether the state changes its policy, and failure is when a state adopts collective bargaining law (i.e. policy switches collective bargaining laws from a "prohibited" to a "permitted" collective bargaining regime).

The categorization of collective bargaining laws in this study comes from a dataset originally prepared by Valletta & Freeman (1988). In their study Freeman and Valetta (1988), they constructed an index for state collective bargaining laws by classifying collective bargaining rights depending on the laws' provisions and dispute resolution mechanisms using that data set. Valletta & Freeman (1988) define seven categories ranging from least to most "favorable" towards collective bargaining rights; no provision for bargaining, bargaining prohibited, bargaining authorized but not required, right to "present proposals", right to "meet and confer", duty to bargain implied, and duty to bargain explicit. Most of the studies in the literature use this dataset and/or use their categorization as a starting point.

In the "bargaining prohibited" category, public employees have no right to negotiate terms and conditions of employment collectively through a union. The "no provision for bargaining" category also means that public employees have no collective bargaining right, because such a right is not specifically conferred by law. The "bargaining permitted" category allows bargaining but the employer has no obligation to negotiate. The "meet and confer" or "present proposals" category gives an option for unions to share their demands/concerns over terms of employment, but in this case also, the employer has no obligation to negotiate. Freeman & Valletta (1988) explain that the states that fall into categories "right to present proposals" and "right to meet and confer," are those in which public employers can determine the terms and conditions of employment without bargaining. The states in categories "duty to bargain implied" and "duty to bargain explicit" must attempt to bargain. While the states in the category "duty to bargain implied" are not by law obligated to come to an agreement, the states in category "duty to bargain explicit" are required to come to a written agreement. The "duty to bargain" category requires employers and employees to meet at the bargaining table and negotiate. In case of disagreements, states may adopt different dispute resolution mechanisms¹.

Their dataset, "The National Bureau of Economic Research (NBER) Public Sector Collective Bargaining Law Data Set", is prepared by Valletta & Freeman $(1988)^2$, which includes the status of state bargaining laws from 1955 to 1985. The dataset has been extended to 1996 by Kim Rueben³ which follows 50 states from 1955 to 1996 for five government employee groups (state employee, local police, local firefighters, local teachers, and other local employees). In this study, I use the extended version of the dataset for the local teachers group.

Another study, Lindy (2011), breaks collective bargaining laws for public school teachers into three categories: "mandatory" in which collective bargaining between public school districts and recognized unions is required, "permissive" where each school district can decide if they want to participate in collective bargaining, and "right-to-work" in which a state bans collective bargaining in their public school districts. My model uses a similar method to Lindy's categorization of collective bargaining laws, but essentially uses an ordering method based on Freeman and Valletta's "favorableness" criterion. Specifically, I categorize collective bargaining laws into two groups: *prohibited* and *permitted* (which includes "authorized" and "mandatory"). The category "*prohibited*" includes states with no provision and states where collective bargaining is prohibited. The category "authorized" includes states in which employers are authorized but not required to bargain with unions, states with right to present proposals, and states with right to meet and confer. The other category, "mandatory", includes states where duty to bargain is implied and duty to bargain

¹Three such mechanisms are exercising teachers' right to strike, required arbitration (which means that a neutral third party settles impasses), and required fact finding and mediation (which means the parties are required to settle the dispute themselves, without a neutral 3rd party who enforces the settlement).

²Valletta R. & Freeman R. B. (1988), Appendix B: NBER Public Sector Collective Bargaining Law Data Set. In Freeman R. B., and Ichniowski C. editors, *When Public Sector Workers Unionize (1988)*. University of Chicago Press.

³Available at http://www.nber.org/publaw/lastaccessed9/29/2015

is explicit. Thus, my model assumes that *permitted* includes states in which government has a duty to bargain with teachers' unions under "explicit" and "implied" statutory provisions, is permitted but not required, and is authorized to bargain with unions under "right to present proposals" and "right to meet and confer"

The analysis time of this study is 32-years, starting from 1964 to 1996. All states enter the data at the same time, but collective bargaining status for five states are not available in the datasets after 1990. So five states are lost within the observation window (i.e. right censored).

The Table B.1 describes the time of failure (the number of states that experience a switch in policy) and survival rates. The first column of Table B.1 shows the time of failure; the second column is the total number of states at risk of failure; the third column is the number of states that switched to a bargaining regime at each time; the fourth column is the number of states censored. Because 10 states had already adopted bargaining laws by the beginning of the study (they failed already), the number of states at risk of failure at time 0 is 40. The results indicate that at time t = 2, we observe 4 states moving to collective bargaining laws. The biggest failure seems to correspond to time 6 (going from 1969 to 1970). At the end of the 7 years (the end of 1971), more than half of the states had switched to permitted bargaining laws. At the end of 32 years, there are 5 states that did not experience a failure and 5 states are lost after 27 years. The estimates of the survivor function along with its standard errors and 95% confidence interval are shown in the rest of the table. The survivor function seems to be a decreasing function in time, and the statuses of states do not seem to change after 15 years. The survival function is calculated using the Kaplan-Meier survivor estimator, S(t)

$$\widehat{S}(t) = \prod_{i|t_j \le t} \left(\frac{n_i - d_i}{n_i}\right) \tag{3.2}$$

where n_i is the number of states at risk at time t_i , d_i is the number of failures at time t_i and the product is over all observed failure times less than or equal to t. Figure B.1 depicts the survival function. The number of states at risk is shown for each reported time underneath the figure. At the end of the study, approximately 12 percent of the sample has not experienced any change in their policy. The estimated median survival time is 6, which

means that 50% of states are expected to survive after 6 years and the (restricted) mean is about 10 years.

Recent discussions on public sector collective bargaining rights, especially in Wisconsin and Ohio, have revealed differences of opinion between Republicans and Democrats. Republicans argue that these laws undermine elected governments and hinder efficiency in the public sector, while in contrast, Democrats argue that these laws actually help local governments to better organize public sector labor markets (Freeman and Han 2013). Another example of the differences among political parties, is the study of Lindy (2011) whose paper examines a unique natural experiment in New Mexico between 1993 and 2003. The state legislature enacted a collective bargaining law in 1992 along with a sunset clause, which essentially made collective bargaining a requirement for public school districts effective from April 1, 1993 to July 1, 1999, at which time the legislature would either reauthorize or let the law lapse. Although the legislature attempted to reauthorize the law in 1999, the Republican governor of New Mexico at the time vetoed the reauthorization and the law was allowed to lapse. In 2003, a Democratic governor took office and the state enacted a new version of the 1993 collective bargaining law. In order to investigate whether party affiliation have an effect on the durations of "prohibited" bargaining regimes I look at the survival times depending on the party of the governor. Particularly, Figure B.3 shows survival times by whether the party of the governor is Democratic or not. The survival probabilities seem to fluctuate but do not seem to be very different from each other until the last half of the analysis time period. Furthermore, a log-rank test did not detect a significant difference between the survival functions.

Another discussion in the literature is that Southern states are less likely to adopt collective bargaining rights. For instance, Saltzman (1985) argues that because Southern states are less developed compared to other states, they are less inclined to unionism (and therefore collective bargaining) because it may hinder industrial development. Hill & Waters (1995) and Farber (1988) also use dummy variable representing states in the South as a proxy for negative attitudes toward unionism/bargaining, arguing that private sector unionism is lower in these states. Figure B.2 shows survival times for the South to see whether or not regional differences occurred over time. The figure shows that states in the South are more likely to survive (meaning that they did not switch to permitted bargaining policy) compared to other states until the period 12 and the difference seems to fade out over time. In fact, after time period 16, states in the South were less likely to survive, but I found no significant difference between the survival functions.

Cumulative hazard function and the smoothed hazard function are depicted in Figures B.4 and B.5. Figure B.4 illustrates a non-decreasing cumulative hazard function. Figure B.5 shows that the hazard ratio first increases and then decreases. In other words, as time goes on, states are less likely to experience the event or switch to a collective bargaining law. It appears that, after time period 11 years from the start of the study, the likelihood of policy change gets smaller.

3.4 Parametric Survival Anlysis and Data

In order to further investigate whether state and school specific demographic, political, and economic variables are related to survival times, I perform a parametric approach which may give us a better idea of how changes in explanatory variables affect the failure rate, given that there is no failure up to a particular time. The model is as follows

$$t_j = x_j \beta_x + \epsilon_j, \quad \epsilon_j \sim N(0, \sigma^2) \tag{3.3}$$

I consider Exponential Model which assumes that the baseline hazard function is constant.

$$h(t|x_j) = h_0(t) \exp(x_j\beta_x)$$

= $\exp(\beta_0) \exp(x_j\beta_x)$ (3.4)
= $\exp(\beta_0 + x_j\beta_x)$

The cumulative hazard and survival functions are $H(t|x_j) = exp(\beta_0 + x_j\beta_x)t$ and $S(t|x_j) = exp\{-exp(\beta_0 + x_j\beta_x)t\}$ respectively.

The explanatory variables include states' political, economic and demographic variables such as an ideology index (as a measure of a state government's ideology), natural logarithm of real per capita income, states' expenditures as a percent of state personal income, percent of public employees in work force, school age population, states' property crime rates, and a binary variable indicating whether the state is in the South; measures of states' public school resources from the previous period such as per pupil expenditures and student-teacher ratio. I combine various state level datasets from 1964 to 1996.

State Political Variables: I consider several ideological measures to control for possible connections between elected party and enactment of collective bargaining laws. The literature also uses variables to represent state government ideology in oder to capture sentiments toward unionism. For instance, Hirsch et al. (2013) and Babcock and Engberg (1999) use indices, relating Democrat and Republican Party affiliations in support of certain legislative bills (e.g. supporting or not supporting unions). Specifically, Hirsch et al. (2013) use the percentage of votes by each legislator in a given year on selected bills and construct an index measuring the percentage of "right" votes for each state's delegation averaged over the years 1965 and 1967-1975 as an instrumental variable. Babcock and Engberg (1999) use political voting percentages in their analysis to account for the idea that Democrats tend to be more supportive of unions than Republicans. As can be seen from these examples, some studies in the literature imply that the more Democratic a government's political view is, the more supportive the government is of union power. Similar some studies use COPE voting index in the literature as discussed in the previous studies section in detail. This is consistent with the idea of creating and/or using indices representing how politically Democratic governments are, to describe the political spectrum on labor issues like collective bargaining.

The first index is Ranney's competitiveness index (which is a widely used state level competition index, available in the "Other Scholars' Competitiveness Measures" dataset prepared by Klarner $(2013a)^4$. This index is the average of the percentage of a state's Democratic gubernatorial candidates' votes, the percentage of Democratic party seats in the state senate, the percentage of Democratic party seats in the state House of Representatives, and the percentage of terms in which the governor, Senate and House experience

⁴Klarner, Carl, 2013a, "Other Scholars' Competitiveness Measures", http://hdl.handle.net/1902.1/ 22519, Harvard Dataverse, V1

Democrat party dominance (Ranney 1976). The index ranges between 0 and 1, where 1 is representative of full Democratic control.

Since political party ideology can vary greatly across states, to better represent a state's political view, measurements of government ideology using weighted voting scores (rather than solely political party affiliation) have been developed. Berry et al. (1998) calculate state government ideology as "the weighted average of the ideological position of each of five institutional actors (i) the Democratic delegation in the state house, (ii) the Republican delegation in the state house, (iii) the Democratic delegation in the state senate, (iv) the Republican delegation in the state senate, and (v) the governor where the ideological position of a legislative party delegation is defined as the central tendency of the ideology of its members" (Berry et al. 2013 p.165). Ideology rating scores range between 0 and 100, with 100 being the most liberal. Thus, this measurement indicates the state's conservative or liberal leanings without consideration of party lines, other than their position as a majority or minority in the legislature. Their ideology index is being updated in "Updated Citizen and Government Ideology Data, 1960-2013 ⁵.

Furthermore, I consider a "veto proof" variable as a measure of government control. Particularly, democratic veto proof is a dummy variable representing whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto. Besides that, another measure I also consider is a general veto proof variable which represents whether some party has enough legislators in both chambers of the legislature to override a gubernatorial veto. The variable veto proof is similar to the unified government measure discussed in Farber's (1988) as part of the cost of collective bargaining. He uses a dummy variable representing whether there is unified control of the government (whether the state legislative is controlled by the same party as the governor). He argues that under unified government control, it is easier for a state to pass any type of legislation according to their political tendencies toward bargaining. Both veto proof variables are taken from

⁵Berry, William D., Evan J. Ringquist, Richard C. Fording and Russell L. Hanson. 1998. "Measuring Citizen and Government Ideology in the American States, 1960-93." American Journal of Political Science 42:327-48", which can be found at https://rcfording.wordpress.com/state-ideology-data/

State Partisan Balance Data collected by Klarner (2013c).⁶

Finally, I simply use party of the governor (= 1 if Democrat Party; = 0 otherwise) as a measure of the government ideology using the State Partisan Balance Data mentioned above. Note that federal or national election results do not always reflect the political leanings of state and local results. Democratic and Republican party platforms usually focus on national issues and principles that do not always apply to state and local issues. For that reason, these indices probably will give us better information about state governments' political views.

Berry et al. (1998) also calculate *citizen ideology* (in the same dataset described earlier) by using weighted average ideology scores for each congressional district's incumbent and challenger for major political parties, as well as election results, to describe the ideological positions of the electorate. They use an unweighted average of the districts to compute the citizen ideology index for the state. I also use citizen ideology as a measure of the political environment. This variable can give us an idea about whether the party affiliations of the state government matters in the decision to enact collective bargaining laws or the state government simply follows the sentiments of its communities toward collective bargaining.

State Economic Variables: Economic variables such as natural logarithm of real per capita income (in dollars) and state government expenditures as a percent of states' total personal income are included in the analysis. These variables are taken from "State Economic Data", again collected by Klarner (2013b).⁷ The monetary values in this dataset are based on 2007 prices using the Berry, Fording and Hanson cost of living index (at the state level). Please note that all monetary values used in this study are also in 2007 prices and corrected for inflation using the same index.

Regarding states' finances, public sector employer and employee negotiations are complicated in the public sector and the outcome of these negotiations has greater impact on

⁶Klarner, Carl, 2013c, "State Partisan Balance Data, 1937 - 2011", http://hdl.handle.net/1902.1/ 20403, Harvard Dataverse, V1

⁷Klarner, Carl, 2013b, "State Economic Data", http://hdl.handle.net/1902.1/20404, Harvard Dataverse, V1

communities. Consumers do not pay directly for government services from which they benefit. Instead, government allocates tax revenue to pay for public services on the constituent consumers' behalf. Government services such as education, fire and police protection, waste removal services, etc., are essential for every community, and their quality and quantity are particularly important to taxpayers. Furthermore, from constituents' perspectives, when taxpayer costs increase, there is no market in which consumer demand can shift when costs increase. And it is difficult to raise taxes. Accordingly, a government's increased labor expenses will more likely be offset by reductions elsewhere in government services, potentially lowering the quality or quantity of the offset services. Hence, for states that have trouble to generate enough resources or meet states' expenses it is possible that local governments will be more likely to preserve their power and flexibility to administer necessary changes in their economy (Lindy 2011). To state it differently, poor states (i.e. states with low revenue or states with high expenditures) may be less willing to enact laws that are more favorable to teachers' collective bargaining rights. Therefore, I also include these local economic conditions in an effort to capture changes in labor markets due to budget reallocations among public employees.

The literature also uses similar variables. For instance, Farber (1988) also controls for proportion of state and local government expenditures to income of the state to represent the size of the state's government, again as a proxy for a more powerful public sector. Furthermore, Farber (1988) argues that high per capita income calls for high demand for public services which supports the public sector, making that sector more powerful. A more powerful sector may lead to a state with a positive attitude toward public sector bargaining. Saltzman (1985) also uses per capita income as a positive indicator of support for unionism, arguing that a high level of economic development leads to a society that is less concerned about tax increases and therefore more supportive of bargaining rights.

States' Public School Resources: I consider per pupil expenditures (in 2007 prices) and student-teacher ratio to capture possible effects of a states' public school resources in the previous period on the enactment of collective bargaining laws in the state during the next period. The working conditions for public school teachers may affect the demand

for bargaining rights. The argument is that fewer resources implies worse equipment and supplies (both in quantity and quality), which implies worse or more difficult working conditions for teachers and worse learning conditions for students. Therefore, public school teachers would more likely seek bargaining rights if working conditions in their schools were worse. Also, they are more likely to be heard both because it is harder to replace them in these areas and their working conditions would be harder to deal with. Since public schools are a highly demanded public service, local governments would be more willing to help the school system through hearing the needs from the best informed group, i.e. public teachers, since they would be the ones who can best pin down the need of the school system.

One may expect that higher student teacher ratio will more likely increase the demand for bargaining rights. Similarly, a lower per pupil expenditure may indicate a worse working condition and/or low teachers' wages which may in turn stimulate teachers to seek more bargaining rights. On one hand, a low expenditure may also stimulate state government to be more inclined toward bargaining rights to aid the school system. On the other hand, expenditures on education may also be related to state government financial concerns depending on the states' financial status.

Briefly, teachers' influences on district voters' preferences, and possible connections between teachers' performance and union demands give rise to the question of whether school allocated resources affect the enactment of collective bargaining laws I use Teachers' Digest Annual Reports to collect information on per pupil expenditures and student-teacher ratio. Per pupil expenditures is the average annual per pupil expenditures in public school system in 2007 prices. Student teacher ratio is calculated as the number of full time and part time classroom teachers in public elementary and secondary day schools divided by the enrollment in full time public elementary and secondary day schools.

Other State Variables: I also use the Current Population Survey $(CPS)^8$ to calculate the percent of public workers and percent of school age children in the population

⁸The CPS dataset is taken from IPUMS: Sarah Flood, Miriam King, Steven Ruggles, and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 4.0. [Machine-readable database]. Minneapolis: University of Minnesota, 2015. Available at https://cps.ipums.org/cps/

(ages from 5 to 17). I include percent of public workers to capture the size of the government. One of the groups probably most affected by teachers' bargaining laws are parents of school age children, who would support any proposal that would improve their state's education system. To that end, Hunt & White (1983) argue that parents will support teachers' bargaining, thinking that it would add to the quality of teachers and school districts. Accordingly, I include percent of school age population as a proxy for the size (power) of the interest group that Hunt & White (1983) discusses. Lastly, I include crime rates⁹ as explanatory variables as a proxy for quality of life.

One should also note that I could not include private sector unionism because data on this is unavailable at the state-level for the entire period considered in this study. An organized and strong private sector unionism in a state may have a spill-over effect on the public sector unionism. Even though the argument on the inclusion of this variable is strong, some studies in the literature i.e. Farber (1988), Waters & Moore (1990) do not find a significant effect of private sector unionization on the decision to enact bargaining laws. Therefore, the results in this paper hopefully are not dramatically affected by not including that measure in the analysis.

The means and standard deviations (s.d.) of explanatory variables can be found in Table B.2. Data summary shows that the average per pupil expenditures is \$6724, student teacher ratio is about 19; proportion of states' total expenditures to personal income is about 12.6; logarithm of per capita income is 10.17; proportion of school age population is 0.20 and public workers to workforce is 0.16. The average crime rates per 100,000 inhabitants is 4082. and the data includes 33% of souther states. The data also shows 33% of states have Democratic party control; 39% of states have some party control; the average leanings of states' government and citizens are almost half Democratic and non Democratic.

Table B.3 shows the evolution of collective bargaining regimes for states over the analysis time. The changes in the table are not necessarily the exact years because the table is constructed after the observations with missing values have been dropped. The behavior of bargaining regimes over time shows that there is only one state that is moving from

⁹Crime rates per 100,000 population reported by the FBI on "http://www.ucrdatatool.gov/Search/Crime/State/StatebyState.cfm"

"permitted" to "prohibited" bargaining laws. That state is Virginia, which switches its regime to a more strict collective bargaining regime in 1978. Also, Georgia first switches from "prohibited" to "permitted" and then back again to "prohibited".

Table B.4 reports the estimated coefficients and Table B.5 shows hazard ratios of the exponential model. The results suggest that states that have higher per pupil expenditures and higher student-teacher ratios experience lower "prohibited" duration. In other words, these states leave the "prohibited" bargaining regime faster among all models that use different political variables. According to the results, higher per capita income is associated with higher duration times, which means that states with high per capita income leave the "prohibited bargaining regimes" slower. The variable per capita income becomes significant when veto proof measure are used. The findings also suggest that the states with higher percent of state government expenditure to income seem to experience higher durations of "prohibited" bargaining regimes, which means these states leave the "prohibited regimes" slower for Models 1 and 5. Furthermore, according to the results, political conditions of states do not seem to have a significant effect on the duration periods. One exception is that veto proof appear to have significant effect on the duration of strict bargaining regime. The states that have a party with enough legislators in both chambers of the legislature to override a gubernatorial veto experience lower durations of strict bargaining regime. Because only one of the political measures is significant at the ten percent level, there is no strong evidence to conclude that the political environment in a state is an important determinant of collective bargaining regimes.

3.5 Dynamic Logit Model

Another approach I use in this study is a dynamic logit model in oder to investigate the determinants of the behavior of collective bargaining regimes. I look at the effects of state and school specific demographic, political, and economic variables on the likelihood of adoption of collective bargaining rights for public school teachers. I employ a dynamic (history dependence) approach by including past years' conditions for collective bargaining as one of the indicators of the current year's effects on the likelihood of its adoption.

Let the latent variable $Y_{i,t}^*$ represent the extent of collective bargaining laws for public school teachers, in state *i* at time *t* (*i* = 1, ..., N; *t* = 1964, ..., 1996). Then the latent variable model is

$$Y_{i,t}^* = \beta_0 + \beta_1 Y_{i,t-1} + \mathbf{X}_{1i,t}' \beta_2 + \mathbf{X}_{2i,t-1}' \beta_3 + \upsilon$$

$$(3.5)$$

where $Y_{i,t-1}$ is a binary variable which represents whether collective bargaining is prohibited (= 0) or not (= 1) in state *i*, at time t - 1, $\mathbf{X_{1i,t}}$ is a vector of variables that includes state specific political and economic variables at time *t* in state *i*, $\mathbf{X_{2i,t-1}}$ is a vector of variables that includes measures of states' public school resources at time t - 1 in state *i*, and *v* is the error term with standard logistic distribution. While we do not observe $Y_{i,t}^*$, we do observe the discrete choice made by the state according to the following choice rule:

$$Y_{i,t} = \begin{cases} 1 & \text{if } Y_{i,t}^* > 0 \\ 0 & \text{if } Y_{i,t}^* \le 0 \end{cases}$$
(3.6)

where $Y_{i,t}$ is a binary variable that is equal to 1 when collective bargaining is *permitted* in state *i* at time *t*, and is equal to 0 when collective bargaining is *prohibited* in state *i* at time *t*. One can recast this discrete choice model as a conditional probability response, given values of the political, economic and resource variables, as follows:

$$P(Y_{i,t} = 1 | Y_{i,t-1}, \mathbf{X}_{1i,t}, \mathbf{X}_{2i,t-1}) = G(\beta_0 + \beta_1 Y_{i,t-1} + \mathbf{X}'_{1i,t} \beta_2 + \mathbf{X}'_{2i,t-1} \beta_3)$$
(3.7)

where the dependent variable is the probability that state *i* will adopt a collective bargaining law at time *t*. The explanatory variable $Y_{i,t-1}$ is the lagged dependent variable; $\mathbf{X}_{1i,t}$ includes states' political, economic and demographic variables at time *t* such as an ideology index (as a measure of a state government's ideology), natural logarithm of real per capita income, states' expenditures as a percent of state personal income, percent of public employees in work force, school age population, states' property crime rates, and a binary variable indicating whether the state is in the South; $\mathbf{X}_{2i,t-1}$ includes measures of state *i*'s public school resources at time t-1 such as per pupil expenditures and student-teacher ratio. Finally, the function *G* is the cumulative distribution function for the logistic distribution for the given variable values.

It is likely that the decision to change collective bargaining laws in period t depends on the form of the collective bargaining regime at time t-1. I include the explanatory variable, $Y_{i,t-1}$, to represent the state of the collective bargaining laws in the previous period. Hence, β_1 measures the state (i.e. history) dependence effect, i.e. the effect that existence of a particular type of collective bargaining laws in the past has on the probability of existence of the same type of collective bargaining laws in the present. In the literature Saltzman (1985) also considers prior category of laws and Hirsch et al. (2013) also control for influence of a state's voters' and workers' past sentiments regarding enactment of collective bargaining laws.

Table B.6 presents results for six models. Each model adopts a different variable to represent the political environment. The first model uses state government's *ideology* index to account for the political circumstances in a state. According to the results, the likelihood of enacting collective bargaining increases with the lag of the dependent variable, which supports the idea that state (i.e. history) dependence does matter for enactment of collective bargaining laws. The results also show that the likelihood that a state will enact collective bargaining laws significantly increases with the student-teacher ratio from the previous period and per pupil expenditures from the previous period. As for states' economic variables, the table shows that state expenditures as a percent of the state's personal income has a negative effect on the probability that a state will enact collective bargaining, but the effect is not significant. However, per capita income has a significant negative effect. An important point of the results is that the likelihood of enacting collective bargaining increases as the ideology index increases, but the effect is not significant. The size of the public work force, percent of school age population, property crime, and whether a state is in the South do not seem to significantly affect the probability of adopting collective bargaining laws.

To be more specific, I looked at three percentile ranges (low, medium and high) for
marginal effects of ideology index, student-teacher ratio, per pupil expenditures, and per capita income (Table B.7, Model 1). The likelihood of adopting collective bargaining increases as a state's ideology gets closer to a more Democratic political view for each range. The marginal effect is increasing from low to high ranges. Particularly, the probability that a state will adopt collective bargaining laws increases by approximately 1.7 percentage points when moving from low to medium and 0.9 of a percentage point from medium to high ranges. The marginal effects of ideology are calculated for a 10-point increase in the ideology index (recalling that the ideology index ranges between 0 and 100). That is, a 10point increase in the ideology index increases the probability that a state enacts collective bargaining by 0.01 for low level of ideology index, by 0.027 for medium, and by 0.037 for high. The results show that the more Democratic states are more likely to adopt collective bargaining. This means that the movement toward a more liberal view in states that fall in the low category (i.e. more Republican) will react less toward bargaining compared to the areas that are presumably more liberal (i.e. middle or high category).

Per pupil expenditures from the previous period seem to have a strongly significant effect on the likelihood of enacting collective bargaining. Table B.7 shows that the marginal effect is strongest for the low expenditure range. Every 100 dollars increase in per pupil expenditures increases the probability by 38 percent for the high ranges, 13 for medium and 12 for low. The marginal effect of student-teacher ratio appear to have an important effect as well. The findings suggest that the effect of student-teacher ratio from the previous period increases more significantly as the range increases: the results show a 0.6 percentage point increase from low to medium and more than double from medium to high, as seen in Table 2. The marginal effects are calculated for an increase in the student-teacher ratio by 5. Therefore, the results suggest that an additional 5 students per teacher increases the probability of enacting collective bargaining by 11 percent for the low range, 17 for medium, and 46 for high. The table also shows that a 10 percent increase in per capita income appears to decrease the collective bargaining probability by .53 for low, .82 for medium and .86 for high. That effect of per capita income is significantly higher in poor areas, and the results do not point to a significant difference in the effects in medium and high income areas.

In summary, there is a slight increase in the likelihood of enacting collective bargaining as a state becomes more Democratic in political view (i.e. the effect is small and not significant). Regarding states' public school variables, such as student-teacher ratio and per pupil expenditures, the results suggest that states with school resources, in terms of higher student-teacher ratio and higher per pupil expenditures, are more likely to enact collective bargaining laws.

The positive relationship between student-teacher ratio and the probability of enacting bargaining laws could be due to harder working conditions in less funded areas, and more lobbying for collective bargaining as a means of remedying such conditions. A high studentteacher ratio may affect the quality of teaching, which in turn may affect students' academic achievement. Families with school age children especially would be a group directly affected by a high student-teacher ratio. Consequently, such families would be more likely to support teachers' bargaining rights with the thinking that their teachers (and staff) are the most knowledgeable about their school districts' needs, and state government officials would be more responsive to teachers' demands. Also, a better public school district is a highly demanded attribute of an area. Accordingly, pressure by parents (as taxpayers and voters) would influence elected government officials to be more sympathetic to collective bargaining, the rationale being that heeding teachers' demands would better serve the community. This may also explain why the marginal effects are strongest in areas with the highest studentteacher ratio.

The positive relationship between per pupil expenditures and probability of enacting bargaining laws is an interesting result. This could be since, from the state government's point of view, high per pupil expenditures reflect good support from the state to the school system. Thus the state government may not be very concerned about budget reallocations among different public services as a means of meeting overall budget constraints. As a result, in such circumstances, state governments that are already spending a lot on education would be more willing to adopt collective bargaining laws. The rationale is that, since public schools are highly demanded public services, the state's residents, who are also taxpayers

and voters, will support legislation favorable to their teachers. In such states where allocations for education are higher, state finances become relatively less important than hearing (e.g. through collective bargaining) about school districts' needs to maintain a high quality education from teachers who are most knowledgeable about such needs. Also in these areas, presumably the bargaining process would not be as contentious as in areas with lower school funding, because the focus would be less on the relative expensiveness of allocations and more on the educational benefits. Another way of looking at the result (assuming teachers' labor market to be symmetric) is that low per pupil expenditures decrease the probability of enacting bargaining laws. That is, low per pupil expenditures may mean that the school system needs more funding, but the state government may be more concerned about its budget and thus want to keep tight control of budget allocations. Accordingly, states whose school resources and budget allocations are limited may be less willing to enact bargaining laws, which would likely put pressure on increasing expenditures in at least two ways: First, collective bargaining is more likely to result in higher teacher compensation as well as higher allocations for school resources. Second, collective bargaining is a time consuming and expensive process, putting even more budget pressure on already financially strapped school districts. State governments may also be concerned about potential disputes that could arise through collective bargaining, which could be expensive to resolve, could alienate teachers and voters (i.e. parents), and could even result in bigger problems for state officials such as strikes and, in the long run, political losses in elections.

Finally, per capita income has a surprisingly important negative effect on the decision to adopt collective bargaining. This could be because the corresponding higher tax revenue would lead to better school resources. This negative relationship contrasts with the studies in the literature (i.e. Farber (1988) finds positive effect), that communities with higher income increase the demand for public services which in turn leads to communities that are more favorable to unionization. One explanation may be that high income families usually choose communities with better school districts and it would be competitive to find a job in these school districts as a teacher, since they provide better working environments. On the other hand, in school districts that have fewer resources (presumably areas with low income families), the state government would be more willing to sit at the bargaining table with teachers' representatives, both to keep the teachers and also to provide better education for the community.

The next three models in Table B.6 consider the same set of independent variables, the only difference among these models being the political index measure. The second model considers the Ranney index, rather than ideology. The third model considers Democratic veto power which is a dummy variable representing whether Democrats have enough legislators in both chambers of the legislature to override a gubernatorial veto. The fourth model considers a dummy variable representing whether the governor is from the Democratic Party or not. The fifth model considers variable veto proof, which represents whether some party has enough legislators in both chambers of the legislature to override a gubernatorial veto. The estimates in Table B.6 show no significant effect for the folded Ranney index, Democratic veto power, party of the governor and veto proof on the probability that a state will enact collective bargaining laws. It may be concluded that the state governments' political circumstances have no significant effect on the probability. The table also shows that the magnitude of the coefficients of the other explanatory variables and their significance are similar to the first model. The only exception is the variable South: the model that considers Democratic veto power predicts that Southern states are significantly less likely to adopt collective bargaining laws in some specifications. In Model 5, I consider the explanatory variable veto proof, which represents whether some party has enough legislators in both chambers of the legislature to override a gubernatorial veto. The results show the findings for government control without the party lines specification and the findings do not seem to be very different. Finally, in the sixth model, I consider citizen ideology as one of the predictors of probability of enacting collective bargaining, and the estimation results show that as a state's citizen ideology gets closer to a more liberal view it is more likely that the state will adopt collective bargaining laws. The rest of the explanatory variables are consistent with the previous results. The marginal effects of the selected variables for these three models can also be found in Table B.7. Overall, according to the results of Table B.6 and B.7, we may conclude that the political leanings of the state government do not have a significant effect on the decision to adopt collective bargaining.

In Table B.8, I replicate the same models shown in Table B.6 and include year dummies. One should note that after dropping missing variables I am left with sometimes 11-12 states in a year. Therefore including year dummies was problematic since I did not have enough variation in a particular year. To overcome this problem, I combined 4 years when constructing year dummies. The table shows a significant effect of ideology on the likelihood of collective bargaining. The results again predicts a significant effect of citizen ideology on the probability of enacting collective bargaining. The findings show that per capita income is no longer significant. On the other hand, per pupil expenditures is significant among all models. The student-teacher ratio is weakly significant for the models that include ideology, Ranney index and citizen ideology, but is not significant for the models that use Democratic veto proof, veto proof and party of the governor. There is also no dramatic difference in the estimated coefficients of the other explanatory variables. In summary, the results suggest that party lines do not matter in the decision to adopt collective bargaining laws. Rather than which party is in control (or whether a party is in control), it is citizens' leaning towards a Democratic view that has a significant effect. This result is a little problematic since one may question whether citizen ideology would determine the government officials' party as well as elections. Therefore, this result should be analyzed further before drawing definitive conclusions.

Summing, there is a slight increase, if anything, in the likelihood of adopting collective bargaining as a state becomes more Democratic in political view. State economic variables, such as the percent of state government expenditures to total personal income do not have a significant effect on the probability of enacting bargaining laws. On the other hand, per capita income does seem to be significant. Also, the percent of revenue to income is weakly significant. However, overall a state's political variables do not appear to have an important effect on the probability of adopting collective bargaining laws. An important point is that the estimation results predict that school resources, as measured by student-teacher ratio and per student expenditures, are important drivers of collective bargaining laws. Another point this study emphasizes is that studies on collective bargaining laws should be analyzed in a dynamic framework.

3.6 Conclusion

The purpose of this paper is to attempt to understand the determinants on the decision of collective bargaining laws. I look at the effects of state and school district specific demographic, political, and economic variables on the likelihood of adoption of collective bargaining. I employ parametric and nonparametric survival estimation in order to analyze the relationship between state-level economic and political conditions, and the duration of strict bargaining regimes. I also use a dynamic (history dependence) approach by including past years' conditions for collective bargaining as one of the indicators of the current year's effects on the likelihood of adoption of collective bargaining.

The survival analysis predicts that those states that have higher per pupil expenditures and higher student teacher ratio are associated with lower durations of strict collective bargaining regimes. The results also suggest that the states with higher per capita income and higher expenditure income ratio experience longer durations of strict bargaining regimes. Findings also suggest that political leanings of the state do not appear to have a significant effect on the duration of strict collective bargaining regimes.

The findings of dynamic logit model suggest that higher student-teacher ratio and per pupil expenditures are associated with higher probabilities of enacting collective bargaining laws. High student-teacher ratio may reflect difficult working conditions, which may in turn lower the quality of education and drive families to be more sympathetic to collective bargaining. Thereof the state government, both to improve the educational system and to respond to voters' demand, is more likely to grant bargaining rights to their teachers.

Furthermore, results from the dynamic approach in my model indicate that there is a significant dependence on a state's historical likelihood of adopting mandatory collective bargaining that affects the current year's likelihood of adopting collective bargaining. That is, there appears to be a strong "state dependence" of likelihood of adopting mandatory collective bargaining when the conditions supporting adoption of collective bargaining have existed in the past. This may simply indicate that adoption of collective bargaining is a complicated process that takes time and deliberation to realize.

While the two approaches I use in this study, a dynamic logit model and a survival

analysis model, differ in structure and interpretation, both models provide useful insights that are consistent with each other. The dynamic logit model is a parametric, essentially Bayesian approach, whereas the survival analysis model uses the Kaplan-Meier estimator, a nonparametric method requiring fewer assumptions. A parametric approach of survival analysis is also employed. One should note that the survival model is potentially more informative than the logit model because the data shows a movement toward collective bargaining was one way.

An important result of this paper is that school resources are significant drivers of collective bargaining. The positive relationship between per pupil expenditures and adopting collective bargaining bargaining could be because state governments that are already spending a significant amount on public education are less concerned about risks of budget reallocations through collective bargaining therefore would be more willing to adopt collective bargaining laws. Accordingly, states whose school resources and budget allocations are limited may be less willing to enact bargaining laws because of potential cost increases that bargaining may induce. Especially for states that want to keep flexibility and control over budget allocation among public services, they will be less favorable to collective bargaining rights because these may result in higher teacher compensation or higher expenditures on physical assets and the bargaining process itself may be a time consuming and expensive process.

Political leanings of state government do not seem to be an important driver of enacting collective bargaining. The implication is that a state's political environment, i.e. whether Democratic or Republican, is not an important factor in the decision to adopt collective bargaining. Although, I find that the citizen ideology does have a significant effect on this probability. In particular, as state citizen ideology gets closer to a liberal view, this seems to increase the likelihood of bargaining.

Interestingly, while educational resources per pupil are important factors, the effects and significance of a state's economic conditions are mixed in importance. In particular, per capita income has a significant negative effect on the likelihood of adopting collective bargaining, but the ratio of states' expenditures to personal income does not appear to have a significant effect on adopting collective bargaining.

The negative effect of per capita income is interesting and may warrant further study. Possible rationales for this result are that, (1) parents with higher incomes may be more likely to support increased allocation of resources for education, without the need for collective bargaining, and (2) when parents cannot afford increased allocation of resources for education and such resources must be funded by government, the government funding allocation process may be more likely to need formal input and discussions from teachers, such as through collective bargaining.

The conclusions in this work, that the relationships between adoption of collective bargaining and public school specific variables are significant, suggest that these relationships should be studied further. Future research identifying more detailed connections between such variables specific to local public education and decision criteria on whether to adopt collective bargaining could provide insight to public policy makers in how to improve public education systems.

Chapter 4

Wage Determination over the Business Cycle

4.1 Introduction

The question of how wages behave over the course of a business cycle has long been debated in the literature, both from a macroeconomic as well as a microeconomic perspective. The mechanisms of wage determination over the business cycle could vary along many economic dimensions and individual characteristics in labor markets.

Three particular views are prominent in the economics literature; the spot market model, the implicit contract model, and the human capital model. In the spot market model, current labor market conditions determine wages. This model implicitly ignores issues of worker mobility, the risk tolerances of workers and employers, and contractual obligations between employers and employees. In the implicit contract model, labor market conditions at the time a worker is hired determine wages. This model implicitly assumes conditions that preclude mobility of workers (e.g. workers are immobile for some reason including being risk averse or that mobility is costly), or obligations in the employer-employee relationship discourage employees from moving (e.g. accrual of certain benefits contingent on staying). In the human capital model, wages are determined by improvements in knowledge and skill for example by means of education, on-job training, and other efforts to improve the worker's productive value.

One basis for the contract model of wage determination is the possibility that transactions costs from participating in market activities encourage long term relationships between employers and workers, making it inefficient to adjust employment terms in each period (Dow 1995). Significant changes in computing and communication technology in recent years have likely decreased these transaction costs, possibly changing the calculus of optimal wage determination. Labor markets may be more sensitive to economic activity now because of the increased labor mobility induced by globalization. These forces suggest that the path dependence of labor market conditions in the evolution of wages may have weakened in recent years. On the other hand, globalization brings greater uncertainty to labor markets, which makes implicit contracts more valuable for workers.

The debate on the behavior of wages is an unresolved question in economics in part because the literature to date has not reached a consensus. Theory cannot precisely tell us whether the spot market or implicit contract model explains wage determination over the business cycle better and some empirical studies show evidence supporting the implicit contract model (Grant 2003), while others support the spot market model (Devereux & Hart 2007; Hagedorn & Manovskii 2013).

Beaudry & DiNardo (1991) study the question of whether wages are affected by the history of labor market conditions consistent with an implicit contract approach or with a spot market approach. They develop a theory that accounts for both approaches and test their theory on data from the Panel Study of Income Dynamics (PSID) for 1976 through 1984 and the Census Bureau's Current Population Survey (CPS) for 1979 and 1983. They conclude that the labor market operates like an implicit contract market model rather than a spot market model, and is very procyclical. Furthermore, the authors show that, for 15 of the 21 industry segments they analyze, industry-specific past labor market conditions have significant effects on corresponding industry wages, even after controlling for current labor market conditions.

This paper aims to evaluate critically Beaudry & DiNardo (1991) findings using more recent CPS data. I extend the investigation of Beaudry & DiNardo (1991) along gender, race and regional lines to understand whether only certain submarkets behave differently from the general labor market and which of the these models better characterizes the labor market.

The results suggest that the minimum unemployment rate during the job spell is consistently significant among different subgroups supporting the contract market model for costlessly mobile workers. Furthermore, the results predict a positive relationship between contemporaneous unemployment rate and wages. The evidence is not enough to support the contract model for costly mobile workers. In summary, the results support Beaudry and DiNardo (1991) findings suggesting that the wage behavior functions more like a contract model rather than a spot market model. The results also back the study of Grant (2003) which argues that there is a tendency towards implicit contracting for women.

The remainder of the paper proceeds as follows: Section 2 summarizes the theory of Beaudrey & DiNardo (1991), Section 3 reviews the literature, Section 4 describes the model and the data. The results are presented in Section 5, and Section 6 concludes.

4.2 Theory of Beaudry and DiNardo 1991

Beaudry & DiNardo 1991 develop and test a model of wage behavior over the course of a business cycle. Their model describes a contract wage in a labor market with costless mobility whereby wage responds to supply and demand market forces. Like an implicit contract model with costly mobility, the authors' model's wage depends on the amount negotiated at the beginning of the contract, but like a spot market model, their model's wage is revised upward in response to tighter labor market conditions.

Beaudry & DiNardo's model assumes an economy that consists of risk neutral employers and risk averse workers. It produces only one unit of a consumption good. A worker's per period utility consumption is given by U(c). All agents in this economy have a discount factor of β and probability of dying equal to $(1 - \mu)$ each period. Employers have access to a technology that requires one worker, and the economy produces an amount of the consumption good equal to $\Phi(t)$ at time t, i.e. labor productivity at t. Productivity is stochastic and is assumed to follow an AR(1) process: $\Phi(t) = (1 - \alpha)\Phi^* + \Phi(t - 1) + \varepsilon(t)$, where $0 < \alpha < 1$ and $\varepsilon(t)$ is i.i.d., and Φ^* is the long term level of labor productivity.

Assuming employers and workers enter into risk sharing employment contracts in a competitive market, market equilibrium is described by the solution to the following optimization problem:

$$\max_{w_{t+1}} \sum_{i=0}^{\infty} (\beta \mu^2)^i E_t[U(w_{t+i})]$$

subj.to
$$\sum_{i=0}^{\infty} (\beta \mu^2)^i E_t [\Phi(t) - w_{t+i}].$$

The solution to this problem describes an optimal employment contract with a fixed wage, conditional on survival of both parties. The wage at time (t + j) in a contract that is negotiated at time t is

$$w(t+j,t) = \Phi^* + \frac{1-\beta\mu^2}{1-\beta\mu^2\alpha} [\Phi(t) - \Phi^*], \qquad \forall t,$$
(4.1)

noting that the wage at time (t + j) does not depend on j.

In this competitive market, the equilibrium condition for employment will adjust in a way that makes the worker indifferent between accepting a job today on one hand, and staying unemployed in the current period and postponing until next period her decision to take a job on the other hand, assuming some employment occurs in every period. That is

$$U(wh(t)) + \beta \mu \ E[\ V(w(t+1,t+1),\Phi(t+1)|\Phi(t)\] = \ V(w(t,t),\Phi(t))$$

where wh(t) is a reservation wage when the worker is unemployed, representing the marginal value of household production at time t, $V(w, \Phi(t))$ is the discounted expected utility at contract wage w and state of technology $\Phi(t)$. Assuming U(.) is of log(.) form, the authors show that the equilibrium relationship between the contract wage and reservation wage is given by

$$\log[w(t+j,t)] = \Omega_1 + \frac{1-\beta\mu^2}{1-\beta\mu^2\alpha}\log[wh(t)],$$
(4.2)

where Ω_1 is essentially a constant (time dependent only in higher order terms).

The authors reason that the reservation wage should be negatively related to the participation rate, $\frac{l(t)}{L(t)}$, so that it changes as given by

$$\ln[wh(t)] - \ln[wh(t-1)] = \frac{(1-\theta)[l(t) - l(t-1)]}{L(t)},$$
(4.3)

where $0 < \theta < 1$. Then substituting this equation into the prior equation gives

$$\log[w(t+j,t)] = \Omega_1 + \Omega_2 [1 - \frac{l(t)}{L(t)}], \qquad (4.4)$$

where $\Omega_2 = -\frac{(1-\theta)(1-\beta\mu^2)}{1-\beta\mu^2\alpha} < 0$, and noting that $\left[1-\frac{l(t)}{L(t)}\right]$ is a measure of the unemployment rate.

Beaudry & DiNardo test equation 4.4 in order to explore the relationship between wages and labor market conditions in the context of an implicit contract model of an economy with immobile workers. This model shows no link between contemporaneous labor market conditions and the contract wage. In contrast, the authors hypothesize that, in a costless job mobility market environment, a worker will only commit to contracts in which other employers have no incentive to bid the worker away. Accordingly, such contracts must be risk sharing contracts in which employers' expected profit is nonpositive.

Harris & Holmstrom (1982) showed that the equilibrium wage in such an optimal risk sharing implicit contract takes the following form

$$W_{t+j}(\Phi^{t+j}) = \max \{W_{t+j-1}(\Phi^{t+j-1}), X(\Phi_{t+j})\} = \max \{X(\Phi_{t+i})\}_{i=1}^{j}$$

where the function $X(\Phi_{t+i})$ is the initial wage negotiated in state Φ_{t+i} and is equal to the average expected productivity conditional on prior states $(\Phi_{t+i}, ..., \Phi_t)$ being below Φ_t (since realizations of Φ_t must yield zero profits). Thus a worker's contract wage is adjusted upward to meet contemporaneous higher contract offers, but is not adjusted downward. The authors show that, in such circumstances, this last equation can be rewritten as follows:

$$W_{t+j}(\Phi^{t+j}) = W(t+j,t) = \max \{k[1 - \frac{l(t+i)}{L(t+i)}]\}_{i=0}^{j}$$

$$\approx k_0 - k_1 \min\{1 - \frac{l(t+i)}{L(t+i)}]\}_{i=0}^{j}, \qquad (4.5)$$

where k'(.) < 0, k_0 is a constant, and $k_1 > 0$. This equation holds that, in a competitive

labor market with mobile risk-averse workers, a worker's wage depends on the most favorable market conditions observed since the worker began her job. This differs from the spot market model in which only the current market condition matters, and differs from the implicit contract model with costly mobility in which only the contract wage at the beginning of the job matters.

4.3 Literature Review

There are other studies in the literature that examine whether a contracting model explains wage behavior over the business cycle by testing Beaudry & DiNardo's theory. Below I summarize some of these studies.

McDonald & Worswick (1999) replicate the Beaudry & DiNardo (1991) study using Canadian data, and extend their study to explore differences by age of worker. McDonald & Worswick analyzed 11 years of individual employee data from the period 1981 through 1992, tailoring their data structure to be closely similar to the data used by Beaudry & DiNardo, except that they excluded immigrants. Their results indicate that unemployment rate changes during job tenure most significantly affect wages of young men and men near retirement. While the authors' results overall are consistent with the Beaudry & DiNardo study, they suggest that the evolution of (macroeconomic) unemployment rate at different stages of job tenure may be a more important factor influencing wages than interactions among implicit contract and spot market models.

Seltzer & Merrett (2000) study attributes of an "internal" labor market by analyzing historical data from a large Australian bank whose personnel policies were representative of banks at the time in Australia and the U.K. The data used included comprehensive wage and employment information for individual employees over a period from the early 1850s to the 1940s, including entire careers records for employees entering the bank's work force from 1887 through 1900. The authors supplemented their data with macroeconomic data needed to compute real wages. Their results show that wages increased during job tenure, more in response to time in the job rather than other aspects of experience. The results also suggest that, during the first 25 to 30 years of job tenure, wage growth is determined, to some extent, by unobserved improvements in ability and frequent rotations of positions and work locations. While the authors admit their work, as a case study of one firm, would be difficult to generalize to other firms and industries, their results overall are consistent with an implicit contract model of labor markets.

Grant (2003) also replicates the Beaudry & DiNardo (1991) study using a different source of data, and extends their study to analyze differences in effects between men and women. Grant's data comes from the National Longitudinal Surveys, covering the period 1966 through 1998, the time periods varying somewhat by the 6 age and gender combinations used in the study. His model is the same as that of Beaudry & DiNardo, but includes some fixed effects omitted in the Beaudry & DiNardo study. Grant's results confirm the Beaudry & DiNardo study and, further, he finds that the effects are similar for both men and women.

Devereux & Hart (2007) apply the methodology of Beaudry and DiNardo (1991) on a British panel data over the period 1976-2001. Their results suggest that the spot market model better explains the wage behavior for the British labor market. The authors argue that considering unemployment rates can be misleading because wage changes may be caused by non-contracting reasons. For instance, they argue that promotions are less likely to occur when unemployment rates are higher which affects a workers' wage during her job spell. They further argue that the starting unemployment rate will influence wages regardless of the existence of a contract, i.e. a worker will be more likely to have a higher wage when unemployment rate is low. To address these possible biases, they consider jobbased and employer-based minimum unemployment estimates and suggest some of the wage changes are due to promotions.

Bellou & Kaymak (2012) employ Beaudrey & Dinardo's methodology to a panel dataset set which includes thirteen European countries over the period 1994-2001, and their paper suggests that contracts play a significant role in the determination of wages over the business cycle in Europe. One of the critics to the interpretation of history dependence in the evolution of wages is that the results may be driven by a bad match. In order to address the possible effects of job switches due to match quality, they control for match quality. In addition, they consider the wage behavior of workers who did not change jobs in two consecutive years. After controlling unobserved differences in match quality, their results still show a robust history dependence in the wage determination process. Their results suggest that the type of contracts depends on the unionization and the extent of bargaining process. For instance, they find that current wages are explained by the labor market conditions at the start of a job in countries with more powerful unions and bargaining rights. The authors mention that the contemporaneous unemployment rate also has a significant effect, and they suggest that the effects of current labor market conditions on wage determination should not be ruled out.

In a more recent study, Hagedorn & Manovskii (2013) conducted a theoretical and empirical study in which they develop an on-the-job search model and then test their model using data similar to that used in Beaudry & DiNardo (1991). Hagedorn & Manovskii's data is from Panel Study of Income Dynamics Data (PSID) from 1976 through 1997, supplemented by the National Longitudinal Survey of Youth Data (NLSY) including more than 6,000 individuals residing in the U.S. in 1979 and born during the 1957 through 1964 period. They also use seasonally adjusted unemployment data constructed from Bureau of Labor Statistics and the Current Population Survey databases. The authors' model is that current wages depend only on aggregate (macroeconomic) market conditions (i.e. unemployment rates) and on idiosyncratic worker productivities. An innovative feature of their model is their method of identifying quality of job matches in the course of employee job tenure, and their use of this method to control for unobserved match specific productivity that has been an issue in other similar studies. The authors approximate quality of job matches by expected number of job offers during employee job tenure, which in turn they measure by the sum of market tightness variables (based on lowest weekly unemployment rates during employee tenure).

Their results are that past aggregate labor market conditions have a significant effect on current wages, but the effect depends on the expected number of job offers during employee tenure. For example, they conclude that wage differences arise because employees starting during boom years have higher wages due to the greater number of job offers then, than employees starting during recession years when there are fewer jobs offers. Furthermore, job switchers' wage volatility is higher than for stayers, and workers experiencing lower job match quality achieve higher wage benefits during boom years rather than during recession years. While the authors' results replicate the findings of Beaudry & DiNardo (1991) (and Bils 1985), they reject the history dependence model of wages in favor of their on-the-job search model. They find that, after controlling for unobserved idiosyncratic productivity using their expected number of job offers method, unemployment rates during employee job tenure are insignificant in explaining current wages, and wage volatility differences between job switchers and job stayers disappear. They conclude that the only reason their results replicate those of Beaudry & DiNardo (1991) is that historical unemployment rates during employee tenure are correlated with number of job offers during employee tenure.

4.4 Model and Data

The basic structure of any model of wages over time must account for the business cycle, as well as the variables of interest that are relevant to the study at hand. Thus labor market conditions over the business cycle must be accounted for in the model. I follow common practice in measuring labor market conditions by using the unemployment rate which we denote by U_t at time = t. I examine whether current wages are determined only by the contemporaneous unemployment rate, as the spot market model proposes; or by the unemployment rate at the time that a worker starts his job, as the implicit contract model with immobile workers proposes; or by the minimum unemployment rate since a worker starts a job, as the implicit contract model with mobile workers proposes. I estimate the following model that is developed by Beaudry & DiNardo (1991)

$$log(w(i,t+j,t)) = X_{i,t+j}\Omega_1 + \Omega_2 U(t,j) + \epsilon_{i,t+j}$$

$$(4.6)$$

where w(i, t + j, t) is the wage of individual *i* at time t + j who began the job in period *t* (i.e. *j* =duration of employee tenure), X_i is a vector of variables that include individual characteristics at time t + j, $\epsilon_{i,t+j}$ is the error term. U(t, j) represents various measures of unemployment rates as follows

$$U(t,j) = \begin{cases} U_{t+j}, & \text{contemporaneous unemployment rate} \\ U_t, & \text{unemployment rate at the time a worker starts a job} \\ \min\{U_{t+k}\}_{k=0}^j, & \text{minimum unemployment rate during the job spell} \end{cases}$$
(4.7)

Generally, one would expect a negative relationship between wages and unemployment rate. An intuitive basic principles analysis tells us that, when labor markets become tighter, the supply of prospective employees declines (i.e. the unemployment rate decreases), so that other things held constant, including employer demand, wage rates (and thus wages) will increase. Conversely, as the unemployment rate increases, we would expect wage rates (and wages) to decrease. This observation is also consistent with the negative slope of the classical Phillip's curve relating wage rate changes with unemployment rates. Accordingly, if anything (i.e. if any of the components of U(t, j) above are significant), the coefficient Ω_2 should be negative.

However, the effects of unemployment rate on wages, in particular the magnitudes of the coefficient Ω_2 , may differ for the three models described above. In the spot market model, employees hired during tight labor market conditions (when U_{t+j} is relatively lower) would be expected to earn a higher wage rate than employees hired during higher unemployment conditions. Presumably, wage rates of employees already working would be unaffected by the spot market changes, unless employers perceived the spot market changes as a signal of increased risk of losing employees to other firms offering a higher wage rate. If employers did see changes in the spot market (i.e. change in unemployment rate U_{t+k} for some k > 0) as such a signal, they might increase the wage rates of employees are at such risk of accepting a better wage offer. Assuming not all of a firm's employees are at such risk of moving, the overall wage rate increase might be smaller than for new employees. As long as the job market becomes tighter (i.e. the unemployment rate declines to a minimum value min $\{U_{t+k}\}$ for $0 \le k \le j$, where j is duration of employee tenure), we would expect continued wage rate increases, and the relation between wages and minimum unemployment rate during the job spell is justified for mobile employees. For employees not at risk of accepting better

wage offers, e.g. immobile employees, wage rates might not increase much during their job spells, and the relation between wages and unemployment rate at the time of hire (U_t) is justified for immobile employees. Note that, while wages rates might decrease for newly hired employees during times of higher unemployment, intuitively consistent with increased competition among prospective employees for open positions, so that the coefficient Ω_2 for them would be positive, we would not ordinarily expect a decline in wage rates of already employed workers. Accordingly, a positive coefficient Ω_2 for already employed workers would be implausible.

I estimate the equation 4.6 by OLS (following Beaudry & DiNardo 1991) using the natural logarithm of weekly wages as the dependent variable. The explanatory variables include experience, experience squared, tenure, years of schooling, union status, race, marital status, residence in metropolitan area, four regional dummies, and 20 industry dummies.

Specifically, experience is the potential years of experience calculated as age-schooling-5; tenure is the number of years the individual has worked in his/her current job; union status is controlled by a dummy variable, *union*, indicating whether the individual is a union member or is covered by a collective bargaining agreement (= 1) or not (= 0); race is controlled by a dummy variable, *nonwhite*, indicating whether the individual is white (= 0) or not (= 1); marital status is controlled by a dummy variable, *nonwhite*, indicating whether the individual is whether a person is married (= 1) or not (= 0); residence in metro area is controlled by a dummy variable, *metro*, indicating whether the individual is located in a metropolitan area (= 1) or not (= 0); and the four regional dummies used represent Northeast, Midwest, South, and West. Originally, 22 major industry dummies were constructed (excluding armed forces). However, two groups were dropped because they included only a small number of people. Therefore, I consider the remaining 20 industry dummies.

I use the Current Population Survey (CPS) from 1996 to 2012 obtained through IPUMS-CPS, University of Minnesota. Beaudrey & Dinardo (1991) use individual data from the 1979 and 1983 Pension Supplement of the Census Bureau's May Current Population Survey (CPS) and the Panel Study of Income Dynamics (PSID, 1976-84) in their analysis. They note that PSID is a small dataset and information on tenure is not available for some years. More importantly, they point out that it is not clear whether the PSID tenure variable refers to a person's tenure in the present position or with the present employer. They mention that CPS is better in both senses. To avoid this problem, I only concentrate on the CPS dataset in this study since tenure is an important piece of information that determines unemployment rates that should be considered as labor market conditions change. Furthermore, the authors could only use two years of CPS, which prevented them from including various unemployment rates in one regression. The availability of data over a longer period allows me to include various unemployment rates in the same regression. One down side is that PSID is a panel dataset, so that feature is lost when using cross sectional CPS data.

The CPS date records the number of years the respondent has worked in his/her current job. I construct the tenure variable using this information for worker who were employed starting 1960 and later for subsamples of male and female employees. The unemployment rate at the time that the employee was hired (U_{start}) is the unemployment rate at the beginning of her tenure. For example, if a respondent in 1996 has worked in the current job for 30 years, then this person's U_{start} is the unemployment rate in 1966. The minimum unemployment rate during the job spell (U_{min}) is the lowest unemployment rate throughout the employees tenure. For the same person in the example above, U_{min} would be the best labor market conditions measured by the lowest unemployment rate between 1966 and 1996. The contemporaneous unemployment rate $U_{current}$ is the unemployment rate at the time of the survey. For instance, all individuals surveyed in 1996 would have $U_{current}$ equal to the unemployment rate in 1996. The figures C.1 and C.2 show the movement of males and females unemployment rates over time respectively. The movement of unemployment rates for both groups are congruent. However, the unemployment rate for men appears to be higher than for women in numbers period, especially after 2008.

The sample used in this study is restricted to individuals who are between 21 and 64 years old, are currently employed and are full time workers (individuals who work at least 35 hours a week). I consider the unemployment rate for workers aged 20 years old and over in this study. Finally, wages are in 1996 prices and are deflated using regional CPI values obtained through the Bureau of Labor Statistics website. The individuals with hourly wage

rates below the federal minimum wage are dropped from the sample.

The means and standard deviations of explanatory variables are presented in Table C.1. The first column of the table is the summary statistics for the entire sample, the second column is for males, and the third is for females.

The log of weekly earnings is 6.08 for the entire sample on average. Males seem to have higher wages compared to females. The average years of experience is about 22, with males having slightly less on average than females. Average years of schooling is 12.6, with males slightly lower than females. The percentage of nonwhite workers is 16.3% on average for the entire sample, with the percentage of nonwhite female workers around one half of a percentage point higher than males. Married workers account for 57% of the entire sample, with males being slightly higher than females. The percentage of the sample that are union members is almost 17%, with men being almost 1 percentage point higher than women. The percentage of the sample living in metropolitan areas is over 75% and basically even between male and female. Finally, the average years of tenure is 6.6 years for the sample, with males having slightly longer average tenure than females.

4.5 Results

The estimation result for equation 4.6 for males is presented in Table C.2. The first model in the first column of the table considers the current unemployment rate, $U_{current}$, to represent the contemporaneous labor market conditions as the spot market model proposes. The second model in the second column considers the minimum unemployment rate during a workers' job spell U_{min} to represent the best labor market condition since a worker has started her job as the implicit contract model with costless mobility proposes. The third model in column three employs the unemployment rate at the time a worker is hired, U_{start} , to represent the labor market conditions at the time a worker starts his job. Then, the next three columns show the results from three regressions that considers two out of the three unemployment rates in the same regression. Specifically, the fourth model includes $U_{current}$ and U_{min} in the same regression; model 5 includes $U_{current}$ and U_{start} ; and model 6 includes U_{min} and U_{start} . Finally, the last column shows the results from all three labor market link variables in the same regression. I apply these procedures in estimations along gender, race and regional dimensions.

The results of the first three columns of Table C.2 show that the current unemployment rate, $U_{current}$, is not significant, whereas the minimum unemployment rate during a workers' job spell, U_{min} and the unemployment rate at the time a worker is hired, U_{start} are significant with a negative sign at the one percent level. These results support implicit contracting models rather than the spot market model. The spot market model does not seem to explain wage movements according to Model 1. Furthermore, the next three columns present the models that include the two unemployment rates in one regression. Model 4 in column 4 shows that both $U_{current}$ and U_{min} are significant. An important point is that the $U_{current}$ has a positive effect, i.e. positive coefficient (which is inconsistent with the theory), suggesting that a one percentage point increase in the current unemployment rate increases wages by 0.8 percent. On the other hand, U_{min} appears to have a significant negative effect on wages, suggesting that a one percentage point increase on U_{min} decreases wages by 2 percent. In the next column, Model 5 shows that U_{start} has a significant negative effect on wages whereas $U_{current}$ does not have significant effect. Therefore, Model 5 supports implicit contracting model with costly mobility over the spot market model. Model 6, in column 6, predicts that the effect of U_{min} on wages is significant but the effect of U_{start} is not significant. Hence, Model 6 supports the implicit contract model with costless mobility as opposed to the contract model with costly mobility. Finally, the last model considers all unemployment rates in one regression, and the results predict a positive effect from $U_{current}$ and a negative effect from U_{min} . Model 7 shows that the effect of U_{min} is more than triple the effect of $U_{current}$ (in absolute terms) and the sign of $U_{current}$ is not consistent with the theory.

Therefore, considering the results from Table C.2, one may suggest the effect of minimum unemployment rate during a workers' job spell supersedes the effects of contemporaneous unemployment rate and the unemployment rate at the time a worker is hired. These findings are consistent with Beaudry & DiNardo (1991) and Grant (2003), and supports the implicit contract model with costless mobility. According to the estimation results of Table C.2, experience has a significant concave effect on wages; return to education and tenure are significantly and positively related to wages. Married workers seem to have significantly higher wages compared to single workers. Workers who reside in metropolitan areas also seem to have significantly higher wages compared to workers residing in non metropolitan areas. Furthermore, union members seem to have significantly higher wages compared to nonunion members. Lastly, nonwhite workers appear to have lower wages compared to white workers. All findings here are consistent with labor market literature and these results are quite stable across all of the models in the table.

Table C.3 presents the results for female workers, and the results appear to be similar to those for men. The first three models find that all corresponding unemployment rates are significant but $U_{current}$ still has a positive effect. Furthermore, $U_{current}$ stays significant when other unemployment rates are added, and the last model shows that its magnitude is higher compared to that of the male labor market. The implicit contract model with costly mobility represented by the U_{start} seems to lose its significance compared to the implicit contract model with costless mobility U_{min} . U_{min} appears to be significant in all of specifications and its magnitude in the last model in which all unemployment rates are included is close to the corresponding value for the male labor market. Particularly, a one percentage point increase in U_{min} decreases wages by 2.40% for men and by 2.35% for women.

For the combined sample for men and women, the results do not seem to be very informative (Table C.4). In the table, all unemployment rates are significant in all models. The effect of current unemployment rate on wages is still consistently positive and significant across all models. Similarly, the effect of minimum unemployment rate during a worker's job spell is consistently significant and negative in all models suggesting that wages are adjusted for the best labor market conditions. Therefore, history of labor market conditions has explanatory power on the wage movements. The results predict that a one percentage point increase in U_{min} will lower wages about 3.7%. The coefficient on the unemployment rate at the time a worker is hired, U_{start} , is also significant but the sign of the coefficient of

the rate changes from one model to the other. Perhaps it is not surprising to see that the results are not informative for the combined sample, because circumstances of employment differ for male and female workers. Therefore, these markets should be analyzed separately to see whether these subgroups do behave differently from the general labor market, and which of the models better characterize wage changes.

Next, I separate the sample for men and for women into two groups by race (i.e. white and black) and the results are presented in Table C.5. The results for white men are still in favor of the implicit contract model with costless mobility. Even though there is some evidence of the history dependence of wage behavior, for white women the results are less plausible because the effect of the actual unemployment rate dominates the effect of the minimum unemployment rate for this subgroup. $U_{current}$ still has a positive effect on wages. That being the case, it may suggest that women's wages are more responsive to changes in the labor market. An interesting result is that the findings do not support any of the models for black males, nor for black females. Accordingly, these groups do not seem to be affected by changes in labor market conditions. It is possible that wage determination process do not perform in a certain way depending on the racial composition of the employees.

I perform the same analysis for workers who are union members to understand the effect of unionization on the wage adjustment process for men and for women separately. The results are presented in Table C.6. Male workers who are union members do not seem to be affected by changes in the labor market. Unions, in one sense, protect workers' wages from any fluctuations in the market. Thus, while men in unions are not negatively affected when the labor market is slack, they also do not benefit positively when the labor market is tight. According to the outcomes, the labor market for the rest produce similar results as before, suggesting that there is some evidence for the implicit contract model with history dependence, particularly with costless mobility. Thus unions seem to work well in protecting male workers from economic fluctuations.

The regional separation groups (Table C.7) show an interesting result. The wage behavior in the midwest and northeast regions can be best explained by the contract model with costlessly mobile workers. Wages in the south and west on the other hand appear to be affected by the contemporaneous unemployment rate, but they do not seem to follow a spot market model because the effects seem to be positive.

In summary, the minimum unemployment rate since a worker starts a job, U_{min} , is consistently negative and statistically significant across almost all groups. The coefficients on current unemployment rate are statistically significant in almost all groups but (1) the effect of $U_{current}$ is smaller on average compared to the effect of U_{min} (2) the coefficient on the $U_{current}$ is positive, which contradicts the theory and this should be analyzed further. The results do not seem to show enough evidence to support the implicit contract model with costly mobile workers. Overall, one may conclude that the results suggest that the implicit contract model with costless mobility seems to explain wage behavior best as Beaudrey & DiNardo (1991) proposed.

4.6 Conclusion

The behavior of wages over the business cycle is an important yet unresolved question in economics. A clear understanding of the determination of wages over the business cycle can help us to devise more efficient labor market policies, particularly by identifying those who experience larger variations in wages from changes in the economy. Having a better disaggregated understanding of wage and employment behavior over the business cycle is valuable for efficient targeting of subsidized child care, unemployment insurance, job training for low skilled workers and other support for low wage families.

To this end, this paper replicates the study of Beaudry & DiNardo (1991) using more recent data. I also broaden their investigation along gender, race and regional lines to understand whether labor markets can be explained well or only certain submarkets are described by one of the models. The results are consistent with the Beaudry & DiNardo findings, suggesting that the effect of the minimum unemployment rate during a job spell on current wages dominates the effects of the contemporaneous unemployment rate and the unemployment rate at the time a worker starts a job, in almost all subgroups. Therefore, the results suggest that the behavior of wages is determined by the implicit contracting model with mobile workers. According to racial composition, the labor market for white males appears to function more like a contract model with costless mobile workers. However, the results for other gender and race groups do not seem to show a dominant market behavior. Particularly, there is weak evidence that the black male labor market functions like a spot market model. The market for black females shows some support for a contract model, but the results cannot differentiate between costly or costless mobile workers models. The findings for white females produce mixed results and cannot precisely signal one market. The contract model with costless mobile workers seems to lose its dominance for white females, and current unemployment rate seems to be more effective for this group. But since the effect if positive, it is also not supporting a spot market model. According to the results, it would not be wrong to say that racial decomposition does not seem to signal the dominance of one model over another with the exception of the white male group.

Regarding subgroups of union membership, the results seems to effect males who are union members by isolating them from any fluctuations in the market. In a sense, union membership for men provides job security, making their wages non responsive to the labor market conditions. Interestingly, we do not see this protection for female workers. Nonunion workers, regardless of gender, seem to be dominated by a contract model with costless mobile workers similar to the overall male and female groups.

Finally, the regional subgroups show another interesting point. The estimation results by regions suggest that the wage behavior in the midwest and northeast is explained best again by the contract model with (costlessy) mobile workers, but this is not seen in the south and west regions.

The positive relationship between current wages and the contemporaneous unemployment rate should be analyzed further. One possible explanation could be the effect of the 2008 economic crises on the behavior of wages; it is possible that layoffs and hires during the crises may have affected the labor market differently, and contemporaneous unemployment rates may be affecting wages. In order to analyze this, I have replicated the regression for people who found a job on or after 2008 by considering the data for 2010 and 2012. For the male labor market, I found that both the unemployment rate at the time that the employee was hired and the minimum unemployment rate during a job spell are significant. But since I consider such a short period of time, these two unemployment rates are highly correlated, and when these are considered in the same regression function, the results were not informative. The findings for those who stayed in their jobs throughout the 2008 crises still support implicit contract model. The results for the female labor market were inconclusive. This may be because the female labor markets is different than the male labor markets. For instance, men were more likely to lose their jobs during the most recent economic crises, so they would have been more affected by the changes in the labor market. For future research, this should be analyzed further when more recent data becomes available.

Appendix A

Tables and Figures of Chapter 2

A.1 Tables

Table A.1: List of residency requirements pros and cons.

Pros					
1. Increase the tax base					
2. Decrease the local unemployment rate					
3. Increase "availability of emergency manpower"					
4. Protect the "public coffer"; to recirculate public employee					
salaries back into the city via taxes and spending					
5. Promote equality (lower unemployment in minority groups)					
6. Increase workers' productivity through familiarity					
7. Keep middle class families as city residents					
Cons					
1. Shrink the labor pool					

- 2. Restrict workers' freedoms
- 3. May affect public employee wages
- 4. May negatively effect public workers' quality of life in areas

such as affordable housing, safety and schools

5. Lower the quality of workers; can result in less educated and less skilled employees

Notes (1975) , Eisinger (1983), Hirsch and Rufolo (1985), and Finkenbinder (2009) are excellent sources for detailed discussions of these arguments.

	Treatment Group				Control G	roup	Before	After
	before	after	after-before	before	after	after-before	control-treatment	control-treatment
age	$44.896 \\ (0.428)$	45.260 (0.622)	$0.364 \\ (0.734)$	$\begin{array}{c} 43.074 \\ (0.579) \end{array}$	$\begin{array}{c} 43.721 \\ (0.721) \end{array}$	$0.646 \\ (0.929)$	-1.821^{***} (0.710)	-1.539 (0.968)
postgraduate	$\begin{array}{c} 0.572 \\ (0.021) \end{array}$	$0.625 \\ (0.027)$	$0.053 \\ (0.034)$	$0.655 \\ (0.026)$	$0.759 \\ (0.029)$	0.104^{***} (0.040)	0.083^{***} (0.034)	$\begin{array}{c} 0.134^{***} \\ (0.041) \end{array}$
male	$0.261 \\ (0.019)$	$\begin{array}{c} 0.275 \\ (0.025) \end{array}$	$0.014 \\ (0.031)$	$\begin{array}{c} 0.302 \\ (0.025) \end{array}$	$\begin{array}{c} 0.274 \\ (0.031) \end{array}$	-0.029 (0.040)	$0.041 \\ (0.031)$	-0.001 (0.040)
married	$\begin{array}{c} 0.572 \\ (0.021) \end{array}$	$\begin{array}{c} 0.613 \\ (0.027) \end{array}$	$\begin{array}{c} 0.040 \\ (0.035) \end{array}$	$\begin{array}{c} 0.620\\ (0.026) \end{array}$	$\begin{array}{c} 0.605 \\ (0.034) \end{array}$	-0.014 (0.043)	$0.048 \\ (0.034)$	-0.007 (0.043)
white	$0.663 \\ (0.020)$	$0.708 \\ (0.025)$	$0.045 \\ (0.033)$	$0.840 \\ (0.020)$	$0.808 \\ (0.027)$	-0.032 (0.033)	0.176^{***} (0.030)	0.099^{***} (0.038)
black	$0.296 \\ (0.020)$	$\begin{array}{c} 0.241 \\ (0.024) \end{array}$	-0.055^{*} (0.031)	$\begin{array}{c} 0.124 \\ (0.018) \end{array}$	$\begin{array}{c} 0.125 \\ (0.023) \end{array}$	-0.001 (0.029)	-0.172^{***} (0.029)	-0.116^{***} (0.035)
N	547	323		337	208			

Table A.2: Descriptive Statistics for public teachers before and after the law change.

The variable age is years of age, postgraduate is a dummy variable indicating whether an individual holds a post graduate degree (= 1) or not (= 0), male is a dummy variable that is equal to one for men and zero for women, married is a dummy variable indicating whether the individual is married (= 1) or not (= 0), white is a dummy variable representing whether an individual is white (= 1) or not (= 0), black is a dummy variable representing whether an individual is black (= 1) or not (= 0).

Standard errors in parentheses

* (p < 0.10), ** (p < 0.05), *** (p < 0.01)

	Т	reatment	Group	Control Group			
	before	after	after-before	before	after	after-before	
23-29	$0.115 \\ (0.013)$	0.117 (0.017)	0.002 (0.022)	$0.136 \\ (0.018)$	0.120 (0.023)	-0.016 (0.030)	
30-39	$0.166 \\ (0.016)$	0.207 (0.023)	0.041 (0.027)	0.267 (0.024)	$0.245 \\ (0.030)$	-0.022 (0.039)	
40-49	$0.345 \\ (0.020)$	$\begin{array}{c} 0.211 \\ (0.023) \end{array}$	-0.135^{***} (0.032)	$0.246 \\ (0.24)$	$0.269 \\ (0.031)$	0.023 (0.038)	
50-65	$\begin{array}{c} 0.373 \ (0.021) \end{array}$	$0.464 \\ (0.028)$	0.091^{***} (0.034)	$\begin{array}{c} 0.350 \\ (0.026) \end{array}$	$\begin{array}{c} 0.365 \\ (0.033) \end{array}$	0.015 (0.042)	
N	547	323		337	208		

Table A.3: Public teachers' age distribution before and after the law change.

Standard errors in parentheses * p < 0.10, ** p < 0.05, *** p < 0.01

Table A.4: Percentage of public teachers who reside within the city before and after the law change.

	Т	reatment	Group	Control Group			
	before	after	after-before	before	after	after-before	
all teachers	0.581 (0.021)	$0.505 \\ (0.028)$	-0.077^{**} (0.035)	$0.396 \\ (0.027)$	$\begin{array}{c} 0.375 \ (0.034) \end{array}$	0.023 (0.043)	
male	0.503 (0.042)	$\begin{array}{c} 0.449 \\ (0.053) \end{array}$	-0.054 (0.068)	$\begin{array}{c} 0.362 \\ (0.048) \end{array}$	$0.368 \\ (0.064)$	$0.006 \\ (0.080)$	
female	0.503 (0.042)	$\begin{array}{c} 0.449 \\ (0.053) \end{array}$	-0.054 (0.068)	$\begin{array}{c} 0.413 \\ (0.032) \end{array}$	$\begin{array}{c} 0.377 \\ (0.039) \end{array}$	-0.035 (0.051)	
married	$0.482 \\ (0.028)$	$\begin{array}{c} 0.373 \\ (0.034) \end{array}$	-0.109^{**} (0.045)	$\begin{array}{c} 0.335 \\ (0.033) \end{array}$	$\begin{array}{c} 0.333 \\ (0.042) \end{array}$	-0.002 (0.053)	
single	0.713 (0.030)	$\begin{array}{c} 0.712 \\ (0.041) \end{array}$	-0.001 (0.050)	$0.500 \\ (0.044)$	$0.439 \\ (0.055)$	-0.061 (0.071)	
white	0.487 (0.026)	$\begin{array}{c} 0.432 \\ (0.033) \end{array}$	-0.055 (0.042)	$\begin{array}{c} 0.332 \\ (0.028) \end{array}$	$\begin{array}{c} 0.310 \\ (0.036) \end{array}$	-0.022 (0.046)	
nonwhite	$0.766 \\ (0.031)$	$0.681 \\ (0.048)$	-0.085 (0.056)	$\begin{array}{c} 0.741 \\ (0.060) \end{array}$	$0.650 \\ (0.076)$	-0.091 (0.096)	

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

	Hou	rly Wage	Ann	ual Wage
	coefficient	standard error	coefficient	standard error
after	-0.0697*	0.0396	-0.0407	0.0361
treatment	-0.0113	0.0305	-0.0089	0.0274
after \times treatment	0.0987^{**}	0.0486	0.0904^{**}	0.0439
age	0.0475^{***}	0.0100	0.0547^{***}	0.0088
age squared	-0.0004***	0.0001	-0.0005***	0.0001)
male	-0.0138	0.0262	0.0211	0.0239
married	0.0203	0.0235	0.0120	0.0213
black	-0.0128	0.0285	-0.0369	0.0257
postgraduate	0.2170^{***}	0.0274	0.2180^{***}	0.0248
kinder	-0.1420	0.1190	-0.1070	0.1150
primary	0.1130^{**}	0.0535	0.0961^{**}	0.0463
secondary	0.1070^{*}	0.0593	0.0954^{*}	0.0520
special	0.1160^{*}	0.0698	0.0931	0.0628
n.e.c.	-0.3340***	0.0636	-0.3720***	0.0588
weeks			-0.4640***	0.0340
constant	1.8470***	0.2080	5.1690^{***}	0.3150
Ν	1415		1415	

Table A.5: DID Estimates of the Effects of the Repeal of Residency Laws on Teachers' Log of Hourly Wages and Annual Wages.

Standard errors in parentheses * (p < 0.10), ** (p < 0.05), *** (p < 0.01)

	Marital Status		Ra	ce	Gender		Edu	ucation
	Married	Single	White	Nonwhite	Male	Female	Postgraduate	No Postgraduate
after	-0.1060^{**} (0.0512)	-0.0186 (0.0636)	-0.0529 (0.0412)	-0.1230 (0.0975)	-0.0163 (0.0709)	-0.0831^{*} (0.0471)	-0.0484 (0.0448)	-0.1340** (0.0636)
treatment	-0.0544 (0.0421)	0.0357 (0.0488)	-0.0292 (0.0335)	$0.0647 \\ (0.0767)$	-0.0589 (0.0559)	-0.0087 (0.0362)	-0.0020 (0.0355)	-0.0299 (0.0589)
after \times treatment	0.1530^{**} (0.0624)	$\begin{array}{c} 0.0445 \\ (0.0781) \end{array}$	0.1320^{**} (0.0522)	-0.0147 (0.114)	$0.0800 \\ (0.0890)$	0.1010^{*} (0.0573)	$0.0563 \\ (0.0564)$	0.1920^{**} (0.0810)
age	$\begin{array}{c} 0.0398^{***} \\ (0.0141) \end{array}$	$\begin{array}{c} 0.0470^{***} \\ (0.0144) \end{array}$	$\begin{array}{c} 0.0483^{***} \\ (0.0109) \end{array}$	0.0507^{**} (0.0217)	$\begin{array}{c} 0.0492^{***} \\ (0.0180) \end{array}$	0.0500^{***} (0.0119)	0.0621^{***} (0.0133)	$\begin{array}{c} 0.0482^{***} \\ (0.0151) \end{array}$
age squared	-0.0003^{*} (0.0002)	-0.0004^{**} (0.0002)	-0.0004*** (0.0001)	-0.0004 (0.0003)	-0.0004^{*} (0.0002)	-0.0004^{***} (0.0001)	-0.0005^{***} (0.0002)	-0.0005** (0.0002)
married			$\begin{array}{c} 0.0224 \\ (0.0259) \end{array}$	-0.0539 (0.0468)	$\begin{array}{c} 0.0421 \\ (0.0489) \end{array}$	$\begin{array}{c} 0.0149 \\ (0.0273) \end{array}$	$0.0406 \\ (0.0284)$	-0.0295 (0.0403)
black	-0.0340 (0.0387)	$\begin{array}{c} 0.0172 \\ (0.0404) \end{array}$			$0.0059 \\ (0.0601)$	-0.0208 (0.0332)	-0.0004 (0.0348)	-0.0209 (0.0455)
male	-0.0171 (0.0322)	-0.0367 (0.0445)	-0.0299 (0.0293)	$\begin{array}{c} 0.0419 \\ (0.0642) \end{array}$			-0.0435 (0.0326)	0.0935^{**} (0.0446)
postgraduate	$\begin{array}{c} 0.2890^{***} \\ (0.0375) \end{array}$	0.1420^{***} (0.0396)	$\begin{array}{c} 0.2010^{***} \\ (0.0311) \end{array}$	$\begin{array}{c} 0.2440^{***} \\ (0.0541) \end{array}$	0.1360^{***} (0.0516)	$\begin{array}{c} 0.2430^{***} \\ (0.0321) \end{array}$		
constant	$2.1160^{***} \\ (0.3030)$	$1.7490^{***} \\ (0.2950)$	$\frac{1.8660^{***}}{(0.2270)}$	$1.7460^{***} \\ (0.4580)$	$\begin{array}{c} 1.5950^{***} \\ (0.3690) \end{array}$	$\frac{1.8730^{***}}{(0.2450)}$	$1.7680^{***} \\ (0.2830)$	$1.8430^{***} \\ (0.3050)$
grade level taught	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	846	569	1043	372	391	1024	894	521

Table A.6: DID Estimates of the Effects of the Repeal of Residency Laws on Teachers' Log Wages for Different Subsamples.

Standard errors in parentheses * p < 0.1, ** p < 0.05, *** p < 0.01

	Different Control Group	City Fixed Effects
after	-0.122** (0.0539)	-0.0409^{**} (0.0168)
treatment	-0.0769^{*} (0.0397)	
after \times treatment	0.1550^{**} (0.0608)	
philly \times after		0.0829^{**} (0.0339)
pitts \times after		0.0295 (0.0627)
philly		-0.0240 (0.0217)
pitts		-0.0290 (0.0432)
syracuse		-0.0558** (0.0276)
rochester		-0.0759** (0.0312)
age	0.0542^{***} (0.0106)	$\begin{array}{c} 0.0463^{***} \\ (0.00578) \end{array}$
age squared	$\begin{array}{c} -0.0005^{***} \\ (0.0001) \end{array}$	-0.0004*** (0.0001)
male	0.00123 (0.0284)	-0.0113 (0.0163)
married	0.0143 (0.0265)	0.0279^{*} (0.0147)
black	-0.0243 (0.0318)	$0.0104 \\ (0.0172)$
postgraduate	0.2280^{***} (0.0301)	0.1950^{***} (0.0156)
constant	1.7720*** (0.2240)	
grade level taught	Yes	Yes
Ν	1180	3663

Table A.7: DID Estimates of the Effects of the Repeal of Residency Laws on Teachers' Log Wages for Different Control Groups and City Fixed Effects.

Column one is considering a control group that is comprised of public teachers working in Buffalo, a city that enforced residency laws throughout the study period.

	Private Teachers			
	coefficient	standard error		
after	-0.1140*	0.0653		
treatment	-0.0018	0.0561		
after \times treatment	0.0820	0.0830		
age	0.0383***	0.0141		
age squared	-0.000332*	0.000171		
male	0.0956**	0.0450		
married	0.0829^{*}	0.0427		
black	-0.104**	0.0519		
postgraduate	0.169***	0.0467		
constant	1.8270***	0.2680		
grade level taught	Yes	Yes		
N	434			

Table A.8: Estimation Results for Private Teachers.

Standard errors in parentheses

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.9: Descriptive statistic for the children of the public teachers.

variable	mean	variance	sd
male	0.517	0.250	0.500
age	11.386	17.160	4.143
white	0.651	0.227	0.477
middle	0.291	0.207	0.455
high	0.328	0.221	0.470
parents married	0.759	0.183	0.428
family income	96911	464000	$68,\!153$

Table A.10: Probit estimation results.

		Robust		Marginal	Delta	
variable	Coefficient	Std. Err.	t ratio	Effects	Std. Err.	t ratio
after	0.668***	0.169	3.95	0.178^{***}	0.044	4.02
treatment	0.633***	0.133	4.75	0.168^{***}	0.035	4.87
after \times treatment	-0.652***	0.214	-3.06	-0.173^{***}	0.056	-3.09
age	-0.100***	0.030	-3.29	-0.027***	0.008	-3.35
male	0.003	0.099	0.03	0.001	0.026	0.03
white	0.387^{***}	0.114	3.39	0.103^{***}	0.030	3.42
middle	0.293	0.192	1.53	0.078	0.051	1.53
high	0.386	0.301	1.28	0.103	0.080	1.29
parents married	0.010	0.143	0.07	0.003	0.038	0.07
family income	0.007	0.071	0.10	0.002	0.019	0.10
constant	-0.758	0.743	-1.02			
N	900					

* p < 0.1, ** p < 0.05, *** p < 0.01

Variable	Mean	Std. Dev.	Min	Max
Interaction Effect	-0.154	0.045	-0.248	-0.056
Standard Error	0.061	0.013	0.029	0.084
z-statistic	-2.444	0.244	-2.995	-1.691

Table A.11: The effect of the interaction term "after \times interaction"

Interaction effect is the average of the cross partial derivative over all observations in the data set. The interaction effect for two dummy variables is $\Delta^2 E(y|x_1, x_2, X) / \Delta x_1 \Delta x_2 = (\Phi(\beta_1 + \beta_2 + \beta_{12} + \delta X) - \Phi(\beta_2 + \delta X)) - (\Phi(\beta_1 + \delta X) - \Phi(\delta X)).$

		Delta-method			
after $\times {\rm treatment}$	Margin	Std. Err.	\mathbf{Z}	[95% Conf.	Interval]
0 0	0.0995	0.0181	5.50	0.0641	0.1350
01	0.2476	0.0225	11.02	0.2036	0.2917
1 0	0.2585	0.0399	6.48	0.1803	0.3366
11	0.2524	0.0329	7.67	0.1879	0.3169

Table A.12: The effect of the interaction term.
A.2 Figures



Figure A.1: Interaction effect as a function of the predicted probability.



Figure A.2: t-statistic as a function of the predicted probability.



Figure A.3: Marginal Effects of the interaction term "treatment \times after".



Figure A.4: Marginal Effects of the interaction term "treatment \times after" for different age groups.



Figure A.5: Marginal Effects of the interaction term "treatment \times after" for different levels of family income. The variable on the x-axis is the logarithm of real family income.

Appendix B

Tables and Figures of Chapter 3

B.1 Tables

Time	Beg. Total	Fail	Net Lost	Survivor F	Std. Error	[95% C	onf. Int.]
2	40	4	0	0.9	0.0474	0.7551	0.9612
3	36	3	0	0.825	0.0601	0.6677	0.9125
4	33	6	0	0.675	0.0741	0.507	0.7966
5	27	2	0	0.625	0.0765	0.4569	0.7543
6	25	7	0	0.45	0.0787	0.2934	0.5946
7	18	4	0	0.35	0.0754	0.2081	0.4955
8	14	2	0	0.3	0.0725	0.168	0.4437
9	12	2	0	0.25	0.0685	0.1298	0.3901
12	10	2	0	0.2	0.0632	0.0939	0.3345
13	8	1	0	0.175	0.0601	0.077	0.3058
15	7	2	0	0.125	0.0523	0.0458	0.2461
32	5	0	5	0.125	0.0523	0.0458	0.2461

Table B.1: Survival Times

variables	mean	s.d.
lagged per pupil expenditures	6724	1968
lagged student teacher ratio	19.27	3.51
total expenditures_personal income	12.57	4.33
ln(per capita income)	10.17	0.17
south	0.33	0.47
public	0.16	0.04
property crime rates	4082	1418
school age population (5-17)	0.20	0.04
ideology index	50.29	22.75
Ranney index	0.85	0.12
Democratic veto proof	0.33	0.47
Dem party of the governor	0.58	0.49
citizen ideology	46.95	16.30
veto proof	0.39	0.49
N	1229	

Table B.2: Summary Statistics

The variables: lagged per pupil expend is per pupil expenditures from the previous period, laggged student teacher ratio is student-teacher ratio from the previous period, south is a dummy variable indicating whether the state is in the South (= 1) or not (= 0), $\ln(\text{per})$ capita income) is the logarithm of per capita income, total expenditures_personal income is total expenditures as a percent of state personal income, property crime is the state level property crime rates per 100000 inhabitants, public is the ratio of public employees in workforce, school age population is the school age population (ages between 5 and 17), ideology is state government ideology which increases as a state becomes more liberal. Ranney is a competitiveness index, Democratic veto proof is a dummy variable representing whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto, Dem party of the governor is the party of the governor (= 1 if Democrat Party; = 0 otherwise), citizen ideology is states' citizen ideology which increases as citizens become more liberal, veto proof is a dummy variable representing whether some party has enough legislators in both chambers of the legislature to override a gubernatorial veto.

state	start		end	change
Alabama	0		0	
Alaska	1		1	
Arizona	0		1	1978
Arkansas	0		1	1978
California	1		1	
Colorado	0		1	1978
Connecticut	0		1	1966
Delaware	0		1	1978
Florida	0		1	1976
Georgia	0	1	0	1967-1978
Hawaii	0		1	1978
Idaho	1		1	
Illinois	0		1	1967
Indiana	0		1	1970
Iowa	0		1	1978
Kansas	0		1	1978
Kentucky	0		1	1966
Louisiana	0		1	1978
Maine	0		1	1978
Maryland	0		1	1970
Massachusetts	1		1	
Michigan	0		1	1966
Minnesota	0		1	1978
Mississippi	0		0	
Missouri	0		0	
Montana	0		1	1978
Nebraska				
Nevada	0		1	1978
New Hampshire	1		1	
New Jersey	0		1	1969
New Mexico	0		1	1966
New York	0		1	1968
North Carolina	0		0	
North Dakota	0		1	1978
Ohio	0		1	1976
Oklahoma	0		1	1978
Oregon	1		1	
Pennsylvania	0		1	1971
Rhode Island	0		1	1967
South Carolina	0		1	1971
South Dakota	0		1	1978
Tennessee	0		1	1979
Texas	0		1	1968
Utah	1		1	
Vermont	0		1	1978
Virginia	1		0	1978
Washington	0		1	1978
West Virginia	1		1	1010
Wisconsin	1		1	
Wyoming	1 0		0	

 Table B.3: Collective Bargaining

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
lag_per pupil expend	0.0008**	0.0007^{*}	0.0008**	0.0008**	0.0007**	0.0007**
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
lag_st ratio	0.1826^{*}	0.2495^{**}	0.2205^{**}	0.2279^{**}	0.1886^{*}	0.1872^{*}
	(0.1000)	(0.1133)	(0.1081)	(0.1078)	(0.1003)	(0.1006)
south	-0.4083	0.1148	-0.9002	-0.9912^{*}	-0.4091	-0.0513
	(0.5283)	(0.6461)	(0.6566)	(0.5873)	(0.5086)	(0.5709)
ln pc inc	-6.1467^{*}	-5.8808*	-5.4908	-5.2597	-5.9627^{*}	-5.9456^{*}
	(3.3608)	(3.3455)	(3.4619)	(3.4112)	(3.2940)	(3.2275)
expend_income	-0.1831^{*}	-0.1369	-0.1612	-0.1443	-0.1813^{*}	-0.1370
	(0.0995)	(0.0998)	(0.0996)	(0.1011)	(0.0978)	(0.0997)
property crime	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
public	2.6476	3.0596	1.8070	1.6606	3.3217	-0.1422
	(6.3382)	(6.0773)	(6.2803)	(6.242)	(5.9567)	(6.2122)
school pop	0.7775	0.8860	0.4649	-0.7614	0.9046	1.2435
	(3.5690)	(3.6392)	(3.5993)	(3.6806)	(3.5437)	(3.5808)
ideology	0.0020					
	(0.0103)					
Ranney		3.7110				
		(2.6476)				
dem veto_proof			0.6151			
			(0.6042)			
veto_proof				0.9187^{*}		
				(0.5583)		
dem_governor					-0.3041	
					(0.4465)	
citizen						0.0207
						(0.0146)
constant	51.7194	43.9315	43.9305	41.0668	50.0160	48.6029
	(32.4526)	(32.4103)	(33.7489)	(33.2250)	(31.8238)	(31.1392)
Ν	283	279	279	279	283	283

Table B.4: Exponential Model Coefficient Estimates

* p < 0.1, ** p < 0.05, *** p < 0.01

The variables: lag_per pupil expend is per pupil expenditures from the previous period , lag_st ratio is student-teacher ratio from the previous period, south is a dummy variable indicating whether the state is in the South (= 1) or not (= 0), ln pc inc is the logarithm of per capita income, expend_income is total expenditures as a percent of state personal income, property crime is the state level property crime rates per 100000 inhabitants, public is the ratio of public employees in workforce, school pop is the school age population (ages between 5 and 17), ideology is state government ideology which increases as a state becomes more liberal. Ranney is a competitiveness index, dem veto_proof is a dummy variable representing whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto, dem_governor is the party of the governor (= 1 if Democrat Party; = 0 otherwise), citizen is states' citizen ideology which increases as citizens become more liberal, veto_proof is a dummy variable representing whether some party has enough legislators in both chambers of the legislature to override a gubernatorial veto.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
lag_per pupil expend	1.001^{**}	1.001^{*}	1.001^{**}	1.001^{**}	1.001^{**}	1.001^{**}
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
lag_st ratio	1.200^{*}	1.283^{**}	1.247^{**}	1.256^{**}	1.208^{*}	1.206^{*}
	(0.120)	(0.145)	(0.135)	(0.135)	(0.121)	(0.121)
south	0.665	1.122	0.406	0.371^{*}	0.664	0.950
	(0.351)	(0.725)	(0.267)	(0.218)	(0.338)	(0.542)
ln pc inc	0.002^{*}	0.003^{*}	0.004	0.005	0.003^{*}	0.003^{*}
	(0.007)	(0.009)	(0.014)	(0.018)	(0.009)	(0.009)
expend_income	0.833^{*}	0.872	0.851	0.866	0.834^{*}	0.872
	(0.083)	(0.087)	(0.085)	(0.088)	(0.082)	(0.087)
property crime	1.000	1.000	1.000	1.000	1.000	1.000
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
public	14.12	21.32	6.092	5.262	27.71	0.867
	(89.50)	(129.6)	(38.26)	(32.85)	(165.0)	(5.389)
school pop	2.176	2.425	1.592	0.467	2.471	3.468
	(7.745)	(8.827)	(5.730)	(1.719)	(8.757)	(12.42)
ideology	1.002	. ,		. ,		
	(0.010)					
Ranney	. ,	40.90				
U U		(108.3)				
dem veto_proof			1.850			
I.			(1.118)			
veto_proof			()	2.506^{*}		
1				(1.399)		
dem_governor				()	0.738	
					(0.329)	
citizen				1.021	(0.010)	
				(0.015)		
N	283	279	279	279	283	283
						~~

Table B.5: The Exponential Model: Hazard Ratios

 $\boxed{ p < 0.1, ** p < 0.05, *** p < 0.01 }$

The variables: lag_per pupil expend is per pupil expenditures from the previous period, lag_st ratio is student-teacher ratio from the previous period, south is a dummy variable indicating whether the state is in the South (= 1) or not (= 0), ln pc inc is the logarithm of per capita income, expend_income is total expenditures as a percent of state personal income, property crime is the state level property crime rates per 100000 inhabitants, public is the ratio of public employees in workforce, school pop is the school age population (ages between 5 and 17), ideology is state government ideology which increases as a state becomes more liberal. Ranney is a competitiveness index, dem veto_proof is a dummy variable representing whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto, dem_governor is the party of the governor (= 1 if Democrat Party; = 0 otherwise), citizen is states' citizen ideology which increases as citizens become more liberal, veto_proof is a dummy variable representing whether some party has enough legislators in both chambers of the legislators in both chambers of the legislators in both chambers of its a dummy variable representing whether some party has enough legislators in both chambers of the legislators in both

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
lagged policy	10.4975^{***}	10.4777^{***}	10.4093***	10.4603^{***}	10.3734^{***}	10.3975^{***}
	(1.1922)	(1.1839)	(1.1709)	(1.1738)	(1.1664)	(1.1955)
lag_per pupil expend	0.0011^{***}	0.0010^{**}	0.0011^{***}	0.0011^{***}	0.0011^{***}	0.0010^{**}
	(0.0004)	(0.0004)	(0.0004)	(0.0004)		
lag_st ratio	0.3119^{**}	0.3097^{**}	0.2998^{**}	0.3045^{**}	0.3023^{**}	0.3243^{***}
	(0.1227)	(0.1235)	(0.1239)	(0.1200)	(0.1227)	(0.1249)
south	-0.5669	-0.4674	-1.4415^{*}	-0.8570	-1.2274^{*}	-0.0321
	(0.5925)	(0.7336)	(0.7646)	(0.5670)	(0.6596)	(0.6750)
ln pc income	-7.3773**	-7.3450^{**}	-6.6113^{*}	-7.1604*	-6.6450^{*}	-6.7974^{*}
	(3.6559)	(3.6327)	(3.7452)	(3.6609)	(3.6828)	(3.5499)
expend_inc	-0.1209	-0.1502	-0.1666	-0.1650	-0.1527	-0.0652
	(0.1107)	(0.1111)	(0.1117)	(0.1099)	(0.1121)	(0.1150)
property crime	0.0003	0.0004	0.0003	0.0004	0.0004	0.0004
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
public	-5.5158	-1.4410	-3.0822	-2.0111	-2.5999	-7.5555
	(7.6246)	(7.5174)	(7.8040)	(7.6205)	(7.7434)	(7.5416)
school pop	4.5241	3.8860	3.7925	3.6039	2.7155	4.8542
	(4.0590)	(3.9926)	(3.9571)	(3.9516)	(4.0262)	(4.0909)
ideology	0.0201					
	(0.0128)					
Ranney		2.1450				
		(2.9099)				
dem veto_proof			0.8635			
			(0.7222)			
$dem_governor$				0.1325		
				(0.5280)		
veto_proof					0.7097	
					(0.6176)	
citizen						0.0416^{**}
						(0.0188)
constant	57.7853	56.8338	51.6670	56.9296	51.7453	50.4373
	(35.7907)	(35.5422)	(36.9482)	(35.8978)	(36.2731)	(34.6566)
Ν	1252	1229	1229	1252	1229	1252
pseudo R^2	0.906	0.904	0.904	0.905	0.904	0.908
year	no	no	no	no	no	no

Table B.6: Estimation results of the dynamic logit model

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

The dependent variable is the probability that a state will adopt collective bargaining laws. The explanatory variables: lagged policy is the status of bargaining law in the previous period (= 1 if permitted, and = 0 if prohibited), lag_per pupil expend is per pupil expenditures from the previous period , lag_st ratio is student-teacher ratio from the previous period, south is a dummy variable indicating whether the state is in the South (= 1) or not (= 0), ln pc inc is the logarithm of per capita income, expend_income is total expenditures as a percent of state personal income, property crime is the state level property crime rates per 100000 inhabitants, public is the ratio of public employees in workforce, school pop is the school age population (ages between 5 and 17), ideology is state government ideology which increases as a state becomes more liberal, Ranney is a competitiveness index, dem veto_proof is a dummy variable representing whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto, dem_governor is the party of the governor (= 1 if Democrat Party; = 0 otherwise).

		Model 1			Model 2			Model 3	
Variables	Low	Medium	High	Low	Medium	High	Low	Medium	High
lag_per pupil expend	0.3820	0.1375	0.1208	0.3668	0.1382	0.1228	0.3880	0.1421	0.1228
lag_st ratio	0.1147	0.1721	0.4586	0.1197	0.1718	0.4606	0.1129	0.1686	0.4601
ln pc income	-0.5389	-0.8232	-0.8612	-0.5369	-0.8235	-0.8579	-0.5379	-0.8222	-0.8584
ideology	0.0105	0.0274	0.0368						
Ranney				0.0455	0.0591	0.0520			
dem veto_proof							0.0127	0.0127	0.0127
		Model 4			Model 5			Model 6	
Variables	Low	Medium	High	Low	Medium	High	Low	Medium	High
lag_per pupil expend	0.3860	0.1371	0.1208	0.3887	0.1425	0.1229	0.3518	0.1326	0.1202
lag_st ratio	0.1137	0.1712	0.4588	0.1149	0.1697	0.4604	0.1165	0.1753	0.4585
ln pc income	-0.5391	-0.8234	-0.8609	-0.5381	-0.8219	-0.8584	-0.5381	-0.8240	-0.8613
$dem_governor$	0.0018	0.0018	0.0018						
veto proof				0.0098	0.0098	0.0098			
citizen							0.0408	0.0531	0.0770

Table B.7: Marginal effects of selected variables for three quantiles; low, medium, and high

I looked at three percentile ranges (low, medium and high) for marginal effects of selected variables such as student teacher ratio from the previous period, per pupil expenditures from the previous period, logarithm of per capita income, and political environment variables. The marginal effects are calculated for a 5-point increase in the student teacher ratio, a 10 percent increase in the per pupil expenditures, a 10 percent increase in the per capita income, and a 10-point increase in the citizen (recall that the ideology index and citizen index ranges between 0 and 100).

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
lagged policy	11.2900***	11.1795***	11.2559***	11.2229***	11.1717***	11.1704***
00 1 0	(1.4201)	(1.3907)	(1.4084)	(1.3994)	(1.3984)	(1.4246)
lag_per pupil expend	0.0009**	0.0009**	0.0009*	0.0009**	0.0009**	0.0009*
	(0.0005)	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
lag_st ratio	0.2628*	0.2782^{*}	0.2022	0.2524	0.2211	0.2818*
	(0.1584)	(0.1645)	(0.1680)	(0.1567)	(0.1629)	(0.1610)
south	-0.5287	-0.3350	-1.5604^{*}	-0.8114	-1.1568*	0.1029
	(0.6119)	(0.7455)	(0.8093)	(0.6064)	(0.6824)	(0.6932)
ln pc income	-4.9109	-5.8491	-4.0451	-5.3940	-4.6145	-4.3470
	(4.2015)	(4.1394)	(4.3035)	(4.2244)	(4.2139)	(4.0536)
expend_income	-0.0875	-0.1391	-0.1302	-0.1421	-0.1169	-0.0417
	(0.1353)	(0.1340)	(0.1375)	(0.1344)	(0.1394)	(0.1380)
property crime	0.0005	0.0004	0.0004	0.0005	0.0005	0.0005
	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
public	-8.6693	-2.9637	-6.6613	-3.9955	-5.1873	-11.3392
	(8.0600)	(7.9799)	(8.5594)	(8.0887)	(8.3163)	(8.1050)
school pop	19.8086	14.0682	14.9692	13.5035	11.8955	21.4419
	(16.5141)	(15.8956)	(17.1013)	(16.3608)	(16.7794)	(16.1620)
ideology	0.0237^{*}					
	(0.0127)					
Ranney		2.6485				
		(2.9897)				
dem veto_proof			1.2302			
			(0.7825)			
dem_governor				0.2821		
				(0.5432)		
veto_proof					0.8396	
					(0.6465)	
citizen						0.0483^{**}
						(0.0201)
constant	31.9165	41.6188	27.2428	39.7095	32.5896	24.2503
	(42.4653)	(41.2961)	(43.2446)	(42.4835)	(42.2654)	(40.9054)
Ν	1252	1229	1229	1252	1229	1252
pseudo R^2	0.912	0.908	0.910	0.909	0.909	0.913
year	yes	yes	yes	yes	yes	yes

Table B.8: Estimation results of dynamic logit model with year dummies.

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

The dependent variable is the probability that a state will adopt collective bargaining laws. The explanatory variables: lagged policy is the status of bargaining law in the previous period (= 1 if permitted, and = 0 if prohibited), lag_per pupil expend is per pupil expenditures from the previous period , lag_st ratio is student-teacher ratio from the previous period, south is a dummy variable indicating whether the state is in the South (= 1) or not (= 0), ln pc inc is the logarithm of per capita income, expend_income is total expenditures as a percent of state personal income, property crime is the state level property crime rates per 100000 inhabitants, public is the ratio of public employees in workforce, school pop is the school age population (ages between 5 and 17), ideology is state government ideology which increases as a state becomes more liberal, Ranney is a competitiveness index, dem veto_proof is a dummy variable representing whether the Democratic Party has enough legislators in both chambers of the legislature to override a gubernatorial veto, dem_governor is the party of the governor (= 1 if Democrat Party; = 0 otherwise).

B.2 Figures



Figure B.1: Kaplan-Meier Survival Function



Figure B.2: Kaplan-Meier Survival Function by South



Figure B.3: Kaplan-Meier Survival Function by Party of the Governor



Figure B.4: Cummulative Hazard Fnct



Figure B.5: Smoothed Hazard Fnc

Appendix C

Tables and Figures of Chapter 4

C.1 Tables

	All	Male	Female
ln(weekly earn)	6.0823	6.1758	5.9752
	(0.4520)	(0.4612)	(0.4161)
experience	22.1513	21.8675	22.4764
	(11.5695)	(11.3984)	(11.7545)
schooling	12.6340	12.4060	12.8952
	(2.1694)	(2.2389)	(2.0563)
nonwhite	0.1636	0.1460	0.1838
	(0.3700)	(0.3531)	(0.3874)
married	0.5711	0.5959	0.5428
	(0.4949)	(0.4907)	(0.4982)
union	0.1690	0.2102	0.1220
	(0.3748)	(0.4075)	(0.3273)
metro	0.7525	0.7545	0.7501
	(0.4316)	(0.4304)	(0.4330)
tenure	6.6858	7.0016	6.3240
	(7.5053)	(7.8847)	(7.0284)

Table C.1: Summary statistics

The variable *experience* is the years of experience (age - schooling - 5), schooling is years of schooling, tenure is the number of years the individual has worked in his/her current job, nonwhite a dummy variable indicating whether the individual is white (= 0) or not (= 1), union is a dummy variable indicating whether the individual is a union member or is covered by a collective bargaining agreement (= 1) or not (= 0), married is a dummy variable indicating whether a person is married (= 1) or not (= 0), metro is a dummy variable indicating whether the individual is located in a metropolitan area (= 1) or not (= 0).

Standard deviations in parentheses.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
experience	0.0206^{***}	0.0203^{***}	0.0207^{***}	0.0203^{***}	0.0207^{***}	0.0203^{***}	0.0202^{***}
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
$experience^2$	-0.0004^{***}	-0.0003***	-0.0004^{***}	-0.0003***	-0.0004^{***}	-0.0003***	-0.0003***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
schooling	0.0576^{***}	0.0579^{***}	0.0578^{***}	0.0578^{***}	0.0577^{***}	0.0579^{***}	0.0578^{***}
	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)
nonwhite	-0.0873^{***}	-0.0865^{***}	-0.0870***	-0.0876^{***}	-0.0875^{***}	-0.0865^{***}	-0.0876^{***}
	(0.0089)	(0.0089)	(0.0089)	(0.0089)	(0.0089)	(0.0089)	(0.0089)
married	0.0846^{***}	0.0839^{***}	0.0847^{***}	0.0846^{***}	0.0850^{***}	0.0839^{***}	0.0845^{***}
	(0.0066)	(0.0066)	(0.0066)	(0.0066)	(0.0066)	(0.0066)	(0.0066)
union	0.1755^{***}	0.1747^{***}	0.1748^{***}	0.1761^{***}	0.1753^{***}	0.1747^{***}	0.1763^{***}
	(0.0082)	(0.0082)	(0.0082)	(0.0082)	(0.0082)	(0.0082)	(0.0082)
metro	0.0559^{***}	0.0562^{***}	0.0558^{***}	0.0577^{***}	0.0563^{***}	0.0562^{***}	0.0579^{***}
	(0.0071)	(0.0071)	(0.0071)	(0.0071)	(0.0071)	(0.0071)	(0.0071)
tenure	0.0088^{***}	0.0079^{***}	0.0089^{***}	0.0075^{***}	0.0089^{***}	0.0078^{***}	0.0072^{***}
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0006)
$U_{current}$	0.0006			0.0073^{***}	0.0027		0.0076^{***}
	(0.0018)			(0.0021)	(0.0019)		(0.0021)
U_{min}		-0.0149^{***}		-0.0209***		-0.0160***	-0.0240^{***}
		(0.0028)		(0.0033)		(0.0041)	(0.0046)
U_{start}			-0.0078^{***}		-0.0087^{***}	0.0011	0.0029
			(0.0021)		(0.0022)	(0.0030)	(0.0030)
$\operatorname{constant}$	4.8954^{***}	4.9653^{***}	4.9347^{***}	4.9559^{***}	4.9257^{***}	4.9650^{***}	4.9547^{***}
	(0.0301)	(0.0315)	(0.0303)	(0.0316)	(0.0310)	(0.0315)	(0.0316)
Ν	15948	15938	15948	15938	15948	15938	15938
R^2	0.312	0.313	0.313	0.314	0.313	0.313	0.314
region	YES						
industry	YES						

Table C.2: Estimation results for males over the period 1996-2012

The dependent variable is the log of weekly wages in 1996 prices. Explanatory variable *experience* is the years of experience (age - schooling - 5), *experience*² is experience squared, *schooling* is years of schooling, *tenure* is the number of years the individual has worked in his/her current job, *nonwhite* a dummy variable indicating whether the individual is white (= 0) or not (= 1), *union* is a dummy variable indicating whether the individual is a collective bargaining agreement (= 1) or not (= 0), *married* is a dummy variable indicating whether a person is married (= 1) or not (= 0), *metro* is a dummy variable indicating whether the individual is located in a metropolitan area (= 1) or not (= 0). Industry dummies, and regional dummies (four regions: Northeast, Midwest, South, and West) are included in the estimation.

 $\begin{array}{ll} \mbox{Robust standard errors in parentheses.} \\ * \ p < 0.10, \, ** \ p < 0.05, \, *** \, p < 0.01 \end{array}$

Annual national civilian unemployment rates for ages of 20 years old and over for men are used.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
experience	0.0136^{***}	0.0135^{***}	0.0135^{***}	0.0134^{***}	0.0135^{***}	0.0135^{***}	0.0133^{***}
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)
$experience^2$	-0.0002***	-0.0002***	-0.0002***	-0.0002***	-0.0002***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
schooling	0.0714^{***}	0.0717^{***}	0.0717^{***}	0.0715^{***}	0.0714^{***}	0.0717^{***}	0.0715^{***}
	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)
nonwhite	-0.0268^{***}	-0.0260***	-0.0261^{***}	-0.0271^{***}	-0.0268^{***}	-0.0260***	-0.0272^{***}
	(0.0081)	(0.0081)	(0.0081)	(0.0081)	(0.0081)	(0.0081)	(0.0081)
married	0.0245^{***}	0.0232^{***}	0.0235^{***}	0.0241^{***}	0.0244^{***}	0.0232^{***}	0.0240^{***}
	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0061)
union	0.1206^{***}	0.1195^{***}	0.1196^{***}	0.1209^{***}	0.1206^{***}	0.1195^{***}	0.1210^{***}
	(0.0096)	(0.0096)	(0.0096)	(0.0096)	(0.0096)	(0.0096)	(0.0096)
metro	0.0915^{***}	0.0904^{***}	0.0904^{***}	0.0929^{***}	0.0920^{***}	0.0904^{***}	0.0930^{***}
	(0.0067)	(0.0067)	(0.0067)	(0.0067)	(0.0067)	(0.0067)	(0.0067)
tenure	0.0124^{***}	0.0121^{***}	0.0127^{***}	0.0116^{***}	0.0128^{***}	0.0122^{***}	0.0109^{***}
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0007)	(0.0007)
$U_{current}$	0.0070^{***}			0.0124^{***}	0.0083^{***}		0.0134^{***}
	(0.0023)			(0.0026)	(0.0024)		(0.0028)
U_{min}		-0.0075^{**}		-0.0168^{***}		-0.0069	-0.0235^{***}
		(0.0036)		(0.0041)		(0.0056)	(0.0066)
U_{start}			-0.0050^{*}		-0.0073**	-0.0006	0.0061
			(0.0029)		(0.0029)	(0.0045)	(0.0047)
constant	4.6247^{***}	4.6925^{***}	4.6815^{***}	4.6728^{***}	4.6508^{***}	4.6927^{***}	4.6699^{***}
	(0.0373)	(0.0388)	(0.0377)	(0.0392)	(0.0387)	(0.0388)	(0.0392)
N	13872	13872	13872	13872	13872	13872	13872
R^2	0.316	0.316	0.316	0.317	0.317	0.316	0.317
region	YES	YES	YES	YES	YES	YES	YES
industry	YES	YES	YES	YES	YES	YES	YES

Table C.3: Estimation results for females over the period 1996-2012

The dependent variable is the log of weekly wages in 1996 prices. Explanatory variable *experience* is the years of experience (age - schooling - 5), *experience*² is experience squared, *schooling* is years of schooling, *tenure* is the number of years the individual has worked in his/her current job, *nonwhite* a dummy variable indicating whether the individual is white (= 0) or not (= 1), *union* is a dummy variable indicating whether the individual is a collective bargaining agreement (= 1) or not (= 0), *married* is a dummy variable indicating whether a person is married (= 1) or not (= 0), *metro* is a dummy variable indicating whether the individual is located in a metropolitan area (= 1) or not (= 0). Industry dummies, and regional dummies (four regions: Northeast, Midwest, South, and West) are included in the estimation.

Robust standard errors in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

Annual national civilian unemployment rates for ages of 20 years old and over for women are used.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
experience	0.0164***	0.0162^{***}	0.0164^{***}	0.0161***	0.0165***	0.0161***	0.0159***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
$experience^2$	-0.0003***	-0.0003***	-0.0003***	-0.0003***	-0.0003***	-0.0003***	-0.0003***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
schooling	0.0621^{***}	0.0626^{***}	0.0624^{***}	0.0623^{***}	0.0622^{***}	0.0626^{***}	0.0623^{***}
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
nonwhite	-0.0600***	-0.0588^{***}	-0.0592^{***}	-0.0603***	-0.0600***	-0.0588***	-0.0604^{***}
	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0061)	(0.0061)
married	0.0643^{***}	0.0629^{***}	0.0636^{***}	0.0638^{***}	0.0644^{***}	0.0626^{***}	0.0635^{***}
	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)
union	0.1723^{***}	0.1706^{***}	0.1709^{***}	0.1724^{***}	0.1719^{***}	0.1708^{***}	0.1729^{***}
	(0.0064)	(0.0064)	(0.0064)	(0.0064)	(0.0064)	(0.0064)	(0.0064)
metro	0.0774^{***}	0.0767^{***}	0.0764^{***}	0.0794^{***}	0.0778^{***}	0.0767^{***}	0.0797^{***}
	(0.0051)	(0.0051)	(0.0051)	(0.0051)	(0.0051)	(0.0050)	(0.0051)
tenure	0.0109^{***}	0.0101^{***}	0.0111^{***}	0.0095^{***}	0.0111^{***}	0.0096^{***}	0.0084^{***}
	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
$U_{current}$	0.0055^{***}			0.0135^{***}	0.0072^{***}		0.0149^{***}
	(0.0014)			(0.0017)	(0.0015)		(0.0017)
U_{min}		-0.0144^{***}		-0.0251^{***}		-0.0209^{***}	-0.0370***
		(0.0023)		(0.0026)		(0.0033)	(0.0038)
U_{start}			-0.0055^{***}		-0.0078^{***}	0.0066^{***}	0.0111^{***}
			(0.0017)		(0.0018)	(0.0025)	(0.0026)
$\operatorname{constant}$	4.8268^{***}	4.9188^{***}	4.8796^{***}	4.8993^{***}	4.8544^{***}	4.9170^{***}	4.8944^{***}
	(0.0241)	(0.0252)	(0.0243)	(0.0253)	(0.0249)	(0.0252)	(0.0253)
N	29820	29810	29820	29810	29820	29810	29810
R^2	0.313	0.314	0.313	0.315	0.314	0.314	0.316
region	YES	YES	YES	YES	YES	YES	YES
industry	YES	YES	YES	YES	YES	YES	YES

Table C.4: Estimation results for both males and females over the period 1996-2012

The dependent variable is the log of weekly wages in 1996 prices. Explanatory variable *experience* is the years of experience (age - schooling - 5), $experience^2$ is experience squared, schooling is years of schooling, *tenure* is the number of years the individual has worked in his/her current job, *nonwhite* a dummy variable indicating whether the individual is white (=0) or not (=1), *union* is a dummy variable indicating whether the individual is a collective bargaining agreement (=1) or not (=0), *married* is a dummy variable indicating whether a person is married (=1) or not (=0), *metro* is a dummy variable indicating whether the individual is located in a metropolitan area (=1) or not (=0). Industry dummies, and regional dummies (four regions: Northeast, Midwest, South, and West) are included in the estimation.

Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Annual national civilian unemployment rates for ages of 20 years old and over for women are used for females. Annual national civilian unemployment rates for ages of 20 years old and over for men are used for males.

A White Male								
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
$U_{current}$	-0.0002			0.0067^{**}	0.0019		0.0064^{**}	
	(0.0021)			(0.0026)	(0.0022)		(0.0027)	
U_{min}	. ,	-0.0151^{***}		-0.0207***	. ,	-0.0108**	-0.0190***	
		(0.0033)		(0.0040)		(0.0052)	(0.0061)	
U_{start}			-0.0110^{***}		-0.0117***	-0.0043	-0.0015	
			(0.0026)		(0.0027)	(0.0040)	(0.0042)	
B. Black Male								
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
$U_{current}$	0.0044			0.0078^{*}	0.0062^{*}		0.0069	
	(0.0034)			(0.0042)	(0.0036)		(0.0044)	
U_{min}		-0.0019		-0.0084		0.0068	-0.0029	
		(0.0052)		(0.0063)		(0.0087)	(0.0107)	
U_{start}			-0.0042		-0.0061	-0.0084	-0.0045	
			(0.0040)		(0.0042)	(0.0067)	(0.0071)	
C. White I	Female							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
$U_{current}$	0.0093***			0.0159^{***}	0.0115^{***}		0.0159^{***}	
	(0.0027)			(0.0033)	(0.0029)		(0.0037)	
U_{min}		-0.0052		-0.0188^{***}		0.0076	-0.0185^{*}	
		(0.0043)		(0.0052)		(0.0076)	(0.0097)	
U_{start}			-0.0077**		-0.0113^{***}	-0.0128^{**}	-0.0003	
			(0.0035)		(0.0036)	(0.0062)	(0.0068)	
D. Black Female								
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
$U_{current}$	-0.0019			0.0027	-0.0009		0.0018	
	(0.0045)			(0.0050)	(0.0045)		(0.0057)	
U_{min}		-0.0130^{*}		-0.0145^{**}		-0.0084	-0.0105	
		(0.0067)		(0.0074)		(0.0099)	(0.0124)	
U_{start}			-0.0097^{*}		-0.0096*	-0.0047	-0.0036	
			(0.0051)		(0.0051)	(0.0075)	(0.0085)	

Table C.5: Estimation results for males and females by race over the period 1996-2012

The dependent variable is the log of weekly wages in 1996 prices. Explanatory variable *experience* is the years of experience (age - schooling - 5), *experience*² is experience squared, *schooling* is years of schooling, *tenure* is the number of years the individual has worked in his/her current job, *union* is a dummy variable indicating whether the individual is a union member or is covered by a collective bargaining agreement (= 1) or not (= 0), *married* is a dummy variable indicating whether a person is married (= 1) or not (= 0), *metro* is a dummy variable indicating whether a person is married (= 1) or not (= 0). Industry dummies, and regional dummies (four regions: Northeast, Midwest, South, and West) are included in the estimation. Robust standard errors in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

Annual national civilian unemployment rates for ages of 20 years old and over for white men is used for the white males. Annual national civilian unemployment rates for ages of 20 years old and over for black men is used for the black males. Annual national civilian unemployment rates for ages of 20 years old and over for white women is used for the white women. Annual national civilian unemployment rates for ages of 20 years old and over for black women is used for the black women.

A. Union, Male								
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
$U_{current}$	0.0052			0.0053	0.0051		0.0054	
	(0.0042)			(0.0045)	(0.0042)		(0.0045)	
U_{min}		0.0028		-0.0010		0.0017	-0.0026	
		(0.0083)		(0.0089)		(0.0097)	(0.0104)	
U_{start}			0.0017		0.0009	0.0012	0.0017	
			(0.0046)		(0.0046)	(0.0054)	(0.0054)	
B. Non-Uni	ion, Male							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
Ucurrent	-0.0008			0.0069***	0.0023		0.0070***	
	(0.0020)			(0.0024)	(0.0021)		(0.0024)	
U_{min}		-0.0159^{***}		-0.0217^{***}		-0.0140^{***}	-0.0224***	
		(0.0029)		(0.0036)		(0.0047)	(0.0055)	
U_{start}			-0.0106^{***}		-0.0115^{***}	-0.0019	0.0006	
			(0.0023)		(0.0025)	(0.0037)	(0.0038)	
C. Union, I	Female							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
$U_{current}$	0.0207***			0.0235***	0.0206***		0.0261***	
	(0.0070)			(0.0072)	(0.0070)		(0.0074)	
U_{min}		-0.0021		-0.0165		-0.0094	-0.0336*	
		(0.0148)		(0.0150)		(0.0176)	(0.0185)	
U_{start}			0.0042		0.0036	0.0079	0.0166	
			(0.0085)		(0.0084)	(0.0101)	(0.0104)	
D. Non-Union. Female								
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	
Ucurrent	0.0055**			0.0111***	0.0072***		0.0116***	
	(0.0024)			(0.0028)	(0.0025)		(0.0030)	
U_{min}		-0.0081**		-0.0166***		-0.0050	-0.0201***	
		(0.0037)		(0.0043)		(0.0061)	(0.0073)	
U_{start}			-0.0065**		-0.0088***	-0.0032	0.0032	
			(0.0031)		(0.0032)	(0.0051)	(0.0053)	

Table C.6: Estimation results for males and females by union status over the period 1996-2012

The dependent variable is the log of weekly wages in 1996 prices. Explanatory variable *experience* is the years of experience (age - schooling - 5), $experience^2$ is experience squared, schooling is years of schooling, *tenure* is the number of years the individual has worked in his/her current job, *nonwhite* a dummy variable indicating whether the individual is white (= 0) or not (= 1), *married* is a dummy variable indicating whether a person is married (= 1) or not (= 0), *metro* is a dummy variable indicating whether a person is married (= 1) or not (= 0). Industry dummies, and regional dummies (four regions: Northeast, Midwest, South, and West) are included in the estimation. Robust standard errors in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

Annual national civilian unemployment rates for ages of 20 years old and over for women are used for females. Annual national civilian unemployment rates for ages of 20 years old and over for men are used for males.

A. West							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
$U_{current}$	0.0028			0.0069**	0.0044^{*}		0.0065^{**}
	(0.0025)			(0.0029)	(0.0026)		(0.0030)
U_{min}		-0.0099***		-0.0151^{***}		-0.0035	-0.0118
		(0.0037)		(0.0044)		(0.0065)	(0.0074)
U_{start}			-0.0089***		-0.0104^{***}	-0.0065	-0.0031
			(0.0031)		(0.0033)	(0.0054)	(0.0056)
B. Midwes	st						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Ucurrent	0.0081***			0.0141***	0.0091***		0.0138***
	(0.0025)			(0.0031)	(0.0026)		(0.0035)
U_{min}		-0.0038		-0.0167^{***}		0.0055	-0.0153^{**}
		(0.0039)		(0.0048)		(0.0057)	(0.0077)
U_{start}			-0.0059**		-0.0080***	-0.0088**	-0.0010
			(0.0028)		(0.0028)	(0.0041)	(0.0045)
C. South							
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
$U_{current}$	0.0057^{**}			0.0103***	0.0070**		0.0096***
	(0.0028)			(0.0033)	(0.0029)		(0.0035)
U_{min}		-0.0086**		-0.0166^{***}		0.0009	-0.0122
		(0.0043)		(0.0050)		(0.0071)	(0.0085)
U_{start}			-0.0089^{***}		-0.0111^{***}	-0.0096^{*}	-0.0039
			(0.0034)		(0.0035)	(0.0057)	(0.0060)
D. Northe	ast						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Ucurrent	-0.0026			0.0048	-0.0017		0.0076
	(0.0037)			(0.0045)	(0.0038)		(0.0047)
U_{min}		-0.0155^{***}		-0.0193^{***}		-0.0242^{***}	-0.0330***
		(0.0057)		(0.0068)		(0.0076)	(0.0093)
U_{start}			-0.0023		-0.0020	0.0094^{*}	0.0121^{**}
			(0.0040)		(0.0041)	(0.0053)	(0.0056)

Table C.7: Estimation results by region over the period 1996-2012

The dependent variable is the log of weekly wages in 1996 prices. Explanatory variable experience is the years of experience (age - schooling - 5), $experience^2$ is experience squared, schooling is years of schooling, tenure is the number of years the individual has worked in his/her current job, nonwhite a dummy variable indicating whether the individual is white (= 0) or not (= 1), union is a dummy variable indicating whether the individual is a union member or is covered by a collective bargaining agreement (= 1) or not (= 0), married is a dummy variable indicating whether a person is married (= 1)or not (=0), metro is a dummy variable indicating whether the individual is located in a metropolitan area (=1) or not (=0). Industry dummies are included in the estimation.

Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

C.2 Figures



Figure C.1: Unemployment rate for males overtime



Figure C.2: Unemployment rate for females overtime

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