MANDATED WAGE FLOORS AND THE WAGE STRUCTURE: ANALYZING THE RIPPLE EFFECTS OF MINIMUM AND PREVAILING WAGE LAWS

A Dissertation Presented

by

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DEDICATION

To my family for their continuous support and inspiration.

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ABSTRACT

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This dissertation empirically investigates the extent of ripple effects associated with changes in mandated wage floors in the United States. Ripple effects are generally theorized to exist because employers provide wage increases beyond those legally required in order to preserve a particular wage hierarchy. This research thus addresses an important policy question: What is the overall impact of mandated wage floors on the wage structure? I examine two types of mandated wage floors: federal and state minimum wage laws and state prevailing wage laws.

I use a semi-parametric approach to estimate the wage effect of state and federal minimum wage changes at fourteen different wage percentiles. I find a limited minimum wage ripple effect. Workers earning up to the 15th wage percentile (within 135 percent of the minimum wage prior to the increase) experience a wage effect from minimum wage changes. Although limited in extent, these estimates imply a ripple effect multiplier of approximately 2.40 to 2.50. This expanded effect modestly improves the target efficiency of minimum wage laws. Also, because the wage growth of these

lower wage percentiles lag the rest of the wage distribution in the absence of minimum wage changes, they appear to comprise a minimum wage contour. A separate analysis of the retail trade industry produces similar results.

To observe prevailing wage law ripple effects, I estimate the wage effect of the repeal of state prevailing wage laws at five different points in the wage distribution using quantile regression. I use mean regression on samples of workers divided by union status and work experience to further specify the location of wage effects. The results suggest that prevailing wage laws produce limited or no ripple effects. The repeal of prevailing wage laws specifically impact the union wage premium of relatively more experienced construction workers and do not appear to spillover significantly to uncovered workers. The pattern of construction premiums across states suggests that prevailing wage laws may substitute as a source of bargaining power for union density.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

INTRODUCTION AND OVERVIEW

1.1 Introduction

The consequences of mandated wage floors, such as the federal minimum wage, have long been a source of debate among economists. The source of controversy is two-fold. One reason is ideologically-based, these mandated wage floors are a prototypical form of market regulation: these laws formalize a minimum level of compensation for paid work that is explicitly determined by social norms. The other reason is their prominence: state and federal minimum wage laws cover the vast majority of wage and salary workers. According to the Bureau of Labor Statistics (BLS), 72 percent of workers are covered by these laws (U.S. Department of Labor 2001).

In the last 10 years, a new type of mandated wage floor—living wage laws—has rejuvenated the debate around these types of laws. The relatively high wage floors (on average, 185 percent of the federal minimum wage (Brenner and Luce 2005) introduced by these living wage laws significantly raise the potential costs and benefits for those affected by these laws. More recent living wage proposals are attempting to increase the coverage of such laws—from workers on city contracts to workers within city limits. This would, of course, magnify their potential economic impact.[1] Lesser-known prevailing wage laws have also produced a stream of research on mandated wage floors. The interest in these laws has largely been fueled by efforts to *repeal* state and federal prevailing wage laws during the late 1970s and early 1980s, as well as, in recent years.

The issue that has generated the largest volume of research connected to minimum wage laws has been the concern about the potential negative employment effects for low-skilled workers (e.g., Brown, Gilroy and Kohen 1982; Card and Krueger 1995, 2000; Neumark and Wascher 1992, 2000). With this issue, a consensus appears to have emerged: State and federal minimum wage laws do not have a large negative employment effect. Disagreement now primarily centers on whether the employment effect is small and negative, neutral, or small and positive (Freeman 1995; Fuchs, Krueger, and Poterba 1998). Other issues, however, have been less extensively researched and thus remain unresolved. Such issues include: (1) *Substitution Effects.* To what extent do employers substitute other inputs for low-wage workers when the minimum wage rises? (2) *Minimum wage careers.* How important are minimum wage jobs as an income source for low-wage workers over the long-term? (3) *Efficiency.* How do minimum wage laws compare to other policies aimed at reducing poverty among the working poor, such as the Earned Income Tax Credit? (4) *Ripple effects.* To what extent do minimum wage laws affect the wages of workers who are not mandated wage increases? These less-examined issues have played prominently in the recent policy debates around mandated wage floors pointing to the need for further research on their economic effects.[2]

This dissertation aims to advance knowledge on one of these less-examined issues, the issue of the ripple effects. Ripple effects refer to the non-mandated change in wages that occur in response to a mandated change in the wage floor. As I will discuss in further detail below, wage differentials that makeup the wage structure may themselves be significant to workers and employers, not just the wage levels. As a result, in order to avoid compressing these wage differentials when a mandated wage floor is increased, employers may adjust the wages of workers whose wages are not required by law to change, particularly those workers earning wages close to, but above, the wage floor.

Both proponents and opponents of mandated wage floors raise the issue of ripple effects in their arguments for or against such laws. Opponents point out that ripple effects may significantly increase the cost of such policy measures with respect to employers' wage bills. If ripple effects are underestimated then the costs to employers are underestimated and the ability of employers to absorb the wage increases is overestimated. On the other hand, if ripple effects are minimal, this may support opponents' critique that minimum wage laws, in particular, impact a narrow group of workers, and thus may not be an effective anti-poverty policy tool. In the case of prevailing wage laws, opponents argue that such laws serve a small interest group and even worse, are effectively racist by primarily benefiting white construction workers. On the other side of the issue, proponents of prevailing and minimum wage laws argue that the effects of mandated wage floors are broad: a rise in the wage floor establishes a higher benchmark for wage norms, and thus requires adjustments up the wage structure. These ripple effects increase the wages for a much larger group of workers than those directly bound by the minimum. Thus, both sides of the debate are particularly concerned with the nature and extent of ripple effects. Beyond these policy-specific debates, the significance of ripple effects is about whether, or to what extent, these public policy tools can potentially alter the shape or position of the wage distribution. To the extent that ripple effects operate, mandated wage floors are a more potent tool than would otherwise be true if there were no ripple effects.

1.2 Theoretical Context

Several economic theories motivate the idea that relative wages are rigid and thus provide the basis for ripple effects. Three of these theories are based on the idea that relative wages are rigid due to emulation: 1) compensating differentials, 2) efficiency wages, and 3) wage norms. Each of these theories provides a reason why workers who earn above a mandated wage floor will seek to emulate changes to the wage floor in order to maintain their wage differentials, and thereby produce ripple effects.

Compensating differentials are wage differentials that exist to equalize the pecuniary and non-pecuniary work conditions across jobs (Rosen 1986). Because jobs have non-pecuniary qualities that may make work more or less appealing, wage differentials arise to compensate for these differences. Jobs with undesirable work conditions such as physical discomfort, risk of injury, or overnight work hours may require a compensating (positive) wage differential between such jobs and other jobs that employ workers of similar quality in order to attract a labor supply. A reduction in the compensating differential, without any change to the non-wage work conditions, may cause workers to quit and eliminate the labor supply for such jobs. Therefore, an increase in minimum wage levels may cause ripple effects as workers in jobs that require compensating differentials emulate the mandated increase.

The theory of efficiency wages proposes another cause for ripple effects. Employees and employers often have conflicting interests over the level of work effort employees provide in the workplace: employers tend to want to obtain greater work effort while workers want to provide less. As a result, employers face the problem of getting their employees to comply with their labor demands. This compliance problem is exacerbated by the fact that monitoring the quantity and quality of labor provided by employees can be difficult. As a result, labor contracts may be imperfectly enforceable. Efficiency wage theory proposes that employers seek ways to get workers to internalize the desire to comply with the employers' demands. One method is to generate involuntary unemployment by offering a wage that is high relative to the market-clearing wage. The rise in involuntary unemployment increases the cost to workers of job loss and thereby provides an internal incentive for worker compliance. This wage is referred to as an efficiency wage. A positive wage differential also serves the same purpose of creating an economic cost to workers for noncompliant behavior. Efficiency wage theory predicts that these relatively high wages reduce shirking and turnover, and attract a higher quality labor force (Bowles and Gintis 1985; Katz 1986; Shapiro and Stiglitz 1984). In sum, positive wage differentials serve as an endogenous enforcement mechanism when exogenous (external) enforcement mechanisms, such as supervision, are inadequate. An increase in a minimum wage will reduce these wage differentials and may diminish their disciplinary affect. As a result, ripple effects may be generated as employers adjust their wage structure to maintain their efficiency wages. In other words, to maintain worker discipline employee wages must emulate the increases to mandated wage floors.

Pre-dating the theoretical formalization of compensating wage differentials and efficiency wages, institutional labor economists such as John Dunlop wrote extensively about the wage-setting influence of relative wages. He used a more universal framework for understanding the role of relative wages based on the concepts of job clusters and wage contours. A job cluster is a set of jobs that are linked by: "(1) technology, (2) administrative organization of the production process, including policies of transfer, layoff, and promotion or (3) social custom" (Dunlop 1964, 16). As a result, the wages of these jobs are viewed in relation to each other by employees and employers and tend to move together, i.e., their wage differentials tend to be maintained. Wage contours are groups of job clusters, across firms. Wage contours are linked together: "(1) by similarity of product markets, (2) by resort to similar sources of labour force, or (3) by common labor market organization (custom) [so] that they have common wage-making characteristics" (Dunlop 1964, 16). Efficiency wages and compensating differentials may be subsumed under this general framework.[3] However, Dunlop's framework also provides for a third concept: wage norms. Wage norms are characteristics of wages which through their regularity, become imbued with social value; these characteristics become social customs. Wage norms are socially constructed compensation standards. In this context, wage differentials are valued by workers because the wage differentials are understood to be fair, in the sense of being appropriate to the overall employment situation at a given work site.

Akerlof and Yellen (1990) model the role that wage norms play in the determination of wages. Their fair wage-effort hypothesis enters the relative wage of workers directly into the wage equation. Their model is based on the idea that workers reciprocate work effort in exchange for a fair wage, where the equity of the wage is dependent on its distance from the wages of other workers (as opposed to, for example, the specific conditions of their job). The fair wage-effort hypothesis is based on a socially constructed sense of fairness that affects the worker's work effort. This theory explicitly identifies the need for employers to adhere to a wage norm to maintain a cooperative workforce.

Wage norms therefore provide a third motivation for ripple effects. If wage differentials are viewed as fair, then changes to these wage differentials are likely to cause conflict between employers and their employees. Workers seek to emulate any increase in a mandated wage floor because workers value their wage differentials on principle.

A fourth theoretical explanation for ripple effects does not rely on interpersonal comparisons. Instead, ripple effects may occur as employers substitute more productive labor (skilled) for less productive (unskilled) labor when a the mandated wage floor increases. In this case, the channel for the ripple effect is through an increase in demand for skilled labor. As the price of unskilled workers increases due to an increase in the wage floor—assuming that skilled workers are good substitutes for unskilled workers—the demand for skilled workers increases, putting upward pressure on their wages. Workers earning between the old and new minimum wage are assumed to be less skilled and therefore less productive than those workers earning wages at or above the new minimum wage level. As a consequence, after a minimum wage increase, the wages of workers who had been making at or above the new minimum wage level experience an increase in their wages, producing a ripple effect.

Thus, several theories predict that changes in mandated wage floors will cause a ripple effect. With the exception of substitution effects, each of these theoretical concepts—compensating differentials, efficiency wages, and wage norms—are similar in that the wage differential is maintained because workers will otherwise retaliate. In the case of compensating differentials, workers quit. In the case of efficiency wages and wage norms, workers are uncooperative on the job, or they quit. As such, the bargaining power of workers plays an important role in these theories, where bargaining power is defined to be the ability to effectively inflict meaningful consequences on the employer who does not comply with workers' wishes. More or less bargaining power will determine the degree to which employees are able to motivate their employers—through the channels identified by the theories discussed above—to maintain wage differentials. Thus, ripple effects depend not only on whether workers *value* their relative wage, but also on whether workers are *able* to maintain their relative wage. Ripple effects through these channels are dependent on the political economic environment in which the mandated wage floor changes occur. As such, determining the outcome of ripple effects is an empirical matter.

This dissertation aims to empirically investigate the existence of ripple effects associated with changes in mandated wage floors in the United States. I examine two types of mandated wage floors. The first is the most prominent mandated wage floor in the United States: the federal minimum wage enacted by the 1938 Fair Labor Standards Act (FLSA) and its state-level counterparts. The second is a lesser known mandated wage floor that requires compensation minima for construction workers employed on publicly-funded or assisted construction projects known as prevailing wage laws. This type of law also exists at the state and federal level. The federal-level prevailing wage law was established by the Davis-Bacon Act (enacted in 1931) and state-level prevailing wage laws are often referred to as "little Davis-Bacons."

These two types of mandated wage floors differ from each other in important ways. The federal and state minimum wage laws cover the vast majority of workers, but provide relatively small increases to the wage floor (8 percent, on average, from 1983 to 2002[4]). Large data resources exist for the study of these laws because of their long history and breadth of coverage. As a result, a detailed description of their wage effects is possible. Prevailing wage laws, on the other hand, cover a small fraction of workers (roughly 20 percent of construction workers or 1 percent of all workers[5]) but imply a much larger change to the wage floor (past research suggests raises on the order of 30 percent, see table 3.3). These qualities make prevailing wage laws resemble more closely living wage laws. By studying two qualitatively different mandated wage floors, I hope to gain insight on how ripple effects vary given different parameters. Studying the ripple effects of living wage laws directly is, unfortunately, constrained by the limited survey data available on affected workers.[6]

This study of ripple effects has a major limitation: I use non-experimental data to estimate ripple effects. Measuring a policy effect using non-experimental data can only approximate the preciseness of a controlled experiment. The challenge faced by the researcher is to identify and control for spurious relationships. These controls, however, are never perfect. In the end, because economic policy takes place in an ever-changing environment that is affected by a wide range of factors, any single empirical analysis of a policy effect, no matter how carefully constructed and observed, is inevitably socially and historically contingent. Recognizing this, empirical exercises such as the ones presented in this dissertation produce provisional answers. To explore the robustness of the estimates in this study I vary factors such as the time frame analyzed, as well as the types of workers that comprise the "control" and "treatment" groups. These additional analyses strengthen confidence in the overall findings.

1.3 Overview

The dissertation is organized as follows. Chapters two and three present the empirical research conducted on each type of mandated wage floor. Each of these two chapters is divided into four parts: background and literature review, methodology, results and discussion. Below I provide a preview of each chapter and the main findings.

In chapter two, I review previous empirical research on the range of wage effects of federal and state minimum wage laws, and identify ways to improve the methodology of this prior research. I present a semi-parametric methodology that involves estimating a first-difference model which relates standard wage determinants to state and federal minimum wages at fourteen different wage percentiles.

The results suggest a limited minimum wage ripple effect: workers earning up to the 15th wage percentile—typically within 12 percent of the current minimum wage—experience a wage effect from federal and state minimum wage changes. In other words, employers raise the wages of workers earning 12 percent above the new minimum wage floor. While this ripple effect is limited in extent, its size is large relative to the mandated wage effects. Specifically, the ripple effect multiplies the total change in wage income by a factor of 2.40 to 2.50. That is, the sum of the additional wage income received by workers whose wages rise due to the ripple effect is 140 to 150 percent of the additional wage income received by workers whose wages are mandated to increase. For example, when the federal minimum wage increased from

$3.35 to $3.80, the total change to wage income caused by the mandated wage increases is estimated to be $20.6 million (assuming hours and employment are constant). Workers earning just above the new minimum wage level, who also received wage raises due to the ripple effect, experienced a total gain of $28.5 million in additional wage income (again, assuming hours and employment are constant), thus more than doubling the overall change in the wage bill caused by mandated raises alone.

This expanded effect modestly improves the target efficiency of state and federal minimum wages laws by decreasing the proportion of teenagers and students (traditionally-aged) among affected workers and increasing the proportion of adult workers. Using 2000 data, I find that the proportion of adult wage earners with modest family incomes among affected workers increased from 49 percent to 56 percent when ripple effects are taken into account.

Finally, I find that state and federal minimum wage laws are, perhaps, more important to the wage *levels* of minimum and near-minimum wage workers. Without the compression caused by minimum wage increases, the lower wage percentiles lag behind the rest of the wage distribution in wage growth.

Chapter three begins with a survey of prior research related to the magnitude of the mandated raises associated with state and federal prevailing wage laws, as well as the scope of coverage. Research is sparse on the magnitude and extent of ripple effects of prevailing wage laws. To estimate prevailing wage ripple effects, I again use a semi-parametric approach by estimating a difference-in-difference-in-difference (DDD) model using quantile regression at five different points in the wage distribution separately. This approach takes advantage of the fact that a subset of states repealed their state prevailing wage laws over a relatively short period of time. I produce a second set of results to further specify the location of wage effects by estimating the DDD model on samples of workers divided by union status and work experience using a mean regression technique.

Past research has found that the construction union premium is significantly reduced with the repeal of prevailing wage laws. This research confirms this earlier finding but adds some new observations.

First, the major finding in this chapter is that prevailing wage laws affect a limited group of workers thus indicating limited or no ripple effects. Approximately, 12 percent of construction workers, all who earned above average wages with prevailing wage laws, experience a significant decline in their wages when the laws are repealed. Because this figure is well within the generally accepted estimates of covered workers, this finding suggests that covered workers primarily experience wage declines, not workers in general. Further, because the subgroup of workers that experience wage declines are relatively more experienced union workers (workers with a potential labor force experience of 15 years or more), it appears that prevailing wage laws significantly increase the union wage premium of covered workers, not union workers in general. In other words, the effect of repeals does not appear to spillover to uncovered workers.

This first observation leads to this second finding: prevailing wage laws play a crucial role in determining the wage premiums of covered union construction workers. The large wage premium associated with the construction industry is not experienced evenly across construction union workers. The pattern of construction premiums suggests that prevailing wage laws may substitute as a source of bargaining power for union density. The construction premium obtained by more experienced construction union workers in moderately-unionized states that repealed their laws approach the construction premiums obtained by those found in strongly unionized states that kept their prevailing wage laws. In contrast, less experienced union workers have significantly lower premiums as compared to their counterparts in highly unionized states.

Chapter four provides concluding remarks that synthesize the major findings of the previous chapters and discusses their implications for how ripple effects operate more generally. I conclude with suggestions for future research.

Notes

[1] Examples of such living wage laws include: a 2003 living wage law applicable to firms operating in the city of Santa Fe and a 2000 living wage law applicable to the Coastal Zone of Santa Monica (an area including Santa Monica's downtown tourist area which was, in part, developed with public funds) proposed in 2000.

[2] Examples of some recent research on these questions include Fairris (2005) on substitution; Andersson, Holzer, and Lane (2005) on minimum wage careers; Neumark and Wascher (2001) on the relative merit of EITC.

[3] Eichner (1991) does so explicitly in his extensions of Dunlop's (1964) concepts.

[4] Author's calculations based on data described in section 2.3.1.

[5] This 20 percent estimate is based on the proportion of the value of construction projects that was publicly-owned during the late 1980s to late 1990s. See table 3.2. The one percent estimate is based on author's calculations using data described in section 3.2.1.

[6] Some studies, however, have recently collected survey data for the specific purpose of measuring living wage law effects. I discuss two of these studies (Brenner and Luce 2005 and Reich, Hall, and Jacobs 2005) in chapter four.

CHAPTER 2

ANALYSIS OF MINIMUM WAGE RIPPLE EFFECT

2.1 Background

States enacted the first minimum wage laws in the United States during the early 1900s. These initial state-level mandated wage floors provided specific protection against exploitative working conditions for those viewed to be the most vulnerable members of society: women and children. At the inception of these minimum wage laws, there was a fairly broad consensus among economists that minimum wage laws,[1] on principle, were uncontroversial, and the question up for debate was the level at which minimum wages should be set. According to economic historian Robert E. Prasch, the opinions of economists of minimum wage laws ranged from "cautious to open enthusiasm" (Prasch 1998, 161).

Economists who advocated for minimum wage laws provided several justifications, all centered on the idea that employers may need to be compelled by forces *outside* the market to behave responsibly with regard to the welfare of society, as well as, the welfare of their workers. Arguments for minimum wage laws included the following. First, unequal differences in economic and political power between employers and workers disadvantaged workers in negotiating their working conditions. Therefore, the most vulnerable in society (women and children), in particular, needed the safeguard of minimum wage laws. Second, employers should, on moral grounds, pay a "living wage." Because wage outcomes were not always sufficient to support the worker and his/her dependents, minimum wage laws had to compel errant employers to do so. Third, low wages degraded the morality and health of workers. As a result, low wages harmed the public good that workers provide—a productive, healthy (morally and physically) society. Minimum wage laws were seen as a way to protect this public good. And finally, employers also needed to be compelled to seek and implement efficient means of producing their goods and services. Without minimum wage laws, advocates of minimum wage laws argued that some employers would choose to compete with other firms primarily through low labor costs in place of developing new production technologies, organizational structures, or skilled workers. Worse, such low-efficiency firms may push firms that do invest in such improvements out of the market. In sum, minimum wage laws were conceived to provide a binding wage floor for employers in order to discourage undesirable behavior.

Given the original intentions of minimum wage laws, it is unclear whether minimum wage increases are likely to effect significant wage changes via the ripple effect. If minimum wage laws are intended to protect the most vulnerable workers or to counter abuse by nefarious employers, why should any raises beyond those mandated occur? Low wages may indicate an inability of workers to negotiate higher wages and/or an employer choosing to base his/her competitive strength on low labor costs. As a consequence, such workers or employers seem unlikely to try to maintain relative wages after a minimum wage increase. Studying mandated wage floors in this context highlights the question of whether workers are *able* to maintain their relative wage position. From a theoretical standpoint, how this form of mandated wage floor is able to act as a wage norm or otherwise play a role in defining meaningful wage differentials—particularly for those workers earning wages nearest to the minimum—is unclear.

Two factors, however, may provide a basis for minimum wage laws to play such a role. First, a more universal net of minimum wage laws developed over time. The Fair Labor Standards Act (FLSA) of 1938 set a national minimum wage for employees of businesses engaged in interstate commerce or in the production of goods for interstate commerce. Over the decades, the coverage of the FLSA expanded. In particular, amendments to the FLSA in 1961 and 1966 added coverage based on industry and occupation categories (e.g., retail, service, local transit, construction and gasoline service station employees) so that in 1999, the vast majority of wage earners—approximately 72 percent—were covered by the FLSA (U.S. Department of Labor 2001). Second, since 1938 the real value of the minimum wage increased substantially, and in this way, raised its potential impact on labor markets and thus its prominence as a labor market institution (see figure 2.1). During the 1950s-1960s, the federal minimum wage increased from its initial value of $3.35 (in 2004 dollars)[2] to its peak value of $8.69 in 1968, an increase of over 250 percent. In recent decades, however, the real value of the federal minimum has declined significantly—dropping 41 percent to $5.15 by 2004 (in 2004 dollars). During this period of decline, some states took over the role of raising the wage floor for their workforce by setting state minimums above the federal level. From 1983 to 1989, 14 raised their state minimums to values above the federal.[3] These two factors—the increased coverage rate of minimum wage laws and real value of minimum wage levels (albeit with fluctuations)—likely contribute to the potential for these mandated wage floors to act as a general reference point for wage earners, and thus, a basis for ripple effects.

In any case, as discussed above, whether and to what extent minimum wage increases produce ripple effects frequently enters policy debates over the merits of minimum wage laws. Such debates have inspired research aimed at answering this question. In the section that follows, I provide a review of the past empirical research on ripple effects from the state and federal minimum wages.

2.2 Review of Empirical Research on Minimum Wage Ripple Effects

In this literature review I survey past research on the existence and magnitude of minimum wage ripple effects. The purpose of this review is to: 1) present previous empirical research on the contours of the minimum wage ripple effect and 2) discuss some of the methodological issues and pitfalls involved in these efforts. I conclude this section with a brief summary of the findings and the gaps that exist in the research.

Gramlich (1976) published one of the earliest empirical studies of the ripple effect of the federal minimum wage. In his study, Gramlich aims to detect the presence of a ripple effect by assessing whether increases in the average non-agricultural hourly wage that accompany federal minimum wage increases exceed the increases expected if workers only received legally mandated raises. His model is a variation on the most commonly used approach to estimating the minimum wage ripple effect. This approach involves regressing a measure of wages on a measure of the minimum wage; in effect, treating the minimum wage as a wage determinant. Such models use either micro-level data and some variation on the Mincerian wage equation, or macro-level data (Gramlich’s approach) and some variation on the Phillips curve equation.

Gramlich’s study provides preliminary empirical evidence of a minimum wage ripple effect. He reports an estimated Phillips-curve equation on quarterly data having the form (t-statistics are in parentheses):[4]

(2.1) $$\frac{\partial W}{W_{-1}} = \underset{(1.40)}{-0.18} + \underset{(7.40)}{3.08}\ \mathrm{UGAP} + \underset{(10.0)}{0.95} \sum v_i \frac{\partial P_{-i}}{P_{-i-1}} + \underset{(3.70)}{0.027} \frac{\partial Wc}{Wc_{-1}} + \underset{(0.90)}{0.005} \frac{\partial Wc_{-1}}{Wc_{-2}} +$$

$$\underset{(0.20)}{-0.002} \sum v_i \frac{\partial Wc_{-i}}{Wc_{-i-1}}$$

This equation models the growth in average nonagricultural hourly wage (W) to be determined by the unemployment rate (UGAP), an Almon lag of the growth in prices (P), contemporaneous and lagged growth in the minimum wage (Wc), including an Almon lagged minimum wage variable. Thus, Gramlich estimates a statistically significant (at conventional levels) 0.027 wage elasticity for nonagricultural average wages with respect to the minimum wage. In other words, a 10 percent increase in the minimum wage is associated with a 0.27 percent increase in the average nonagricultural hourly wage, after controlling for changes in price level and unemployment. Gramlich uses this wage elasticity to approximate the size of ripple effect raises relative to mandated raises. He finds that his wage elasticity suggests that the non-mandated, ripple effect raises associated with the minimum wage roughly doubles the increase to the wage bill attributed to mandated raises. Because of the thinness of the wage distribution at the minimum wage it is, perhaps, unsurprising that the impact on the wage bill of mandated raises is relatively modest compared to the raises received by workers just above the minimum wage, if there is any ripple effect at all. In any case, this multiplier of two calls attention to the importance of examining the broader effect of the minimum wage when assessing it as a policy tool.

In terms of the minimum wage's impact on the overall wage distribution, however, Gramlich characterizes the size of the multiplier as quite small due to the fact that its size is not great enough to maintain the original wage structure. Assuming that the wage effects are limited to the bottom of the wage distribution, he concludes that the estimated size of the ripple effect would compress the wage distribution. To see this, consider the extremes: On the one hand, an extensive ripple effect would cause the entire wage distribution to shift toward higher wages as wages at all levels are adjusted to maintain their wage position relative to the wage floor. Such an effect would produce no meaningful change in wage inequality. On the other hand, no ripple effect would cause the lower left tail of the wage distribution to be swept up into a spike at the minimum wage each time the minimum wage increases causing the entire wage distribution to contract. As a result wage inequality is reduced. The absence of an extensive ripple effect leaves the vast majority of the wage distribution unaffected while bringing the lowest wages into closer proximity to the highest. Summarizing, Gramlich offers this assessment of the magnitude of the minimum wage ripple effect, "The multiplier [of the minimum wage ripple effect] is high or low, depending upon the implications one is examining" (Gramlich 1976, 429).

Other studies refine Gramlich's analysis in two general ways. First, researchers use a finer-grained approach to estimating the minimum wage ripple effect. Instead of examining the impact of minimum wage changes on a central tendency measure, such as the mean, later studies try to discern how the impact of the minimum wage varies depending on the proximity of wages to the minimum wage. The second refinement is an econometric one. Gramlich's initial approach depends on time series data which tend to be vulnerable to estimating spurious relationships. The difficultly of accounting for the varied economic changes that take place, simultaneously, over time creates this commonly recognized weakness in time series data. Consequently, the affects of a mixture of economic factors may be captured by a single regressor that also changes over time, such as the minimum wage. A difference-in-difference approach, discussed below, seeks to address this problem. These two refinements will be discussed in turn.

Researchers have employed various ways to conduct a finer-grained analysis of the extent of the minimum wage ripple effect. The basic strategy is to create aggregations of their unit of analysis (e.g., firm, individual, or union contract) by their relative wage position and examine whether the impact of minimum wage changes as relative wages rise. For example, Easton and King (2000) examine whether the impact of the minimum wage is different for workers with only a high school degree versus workers who have obtained some college education. Educational attainment can be seen as a proxy for wage levels, as higher levels of educational attainment are correlated with higher wages. Variations on this analytic strategy include measuring the differential impact of the minimum wage on: union contracts that have base rates at varying distances from the minimum wage (Farber 1981; Swidinsky and Wilton 1982), occupations with different average wages (Grossman 1983), firms with and without minimum wage workers (Card and Krueger 1995), direct wage measures that vary in distance from the minimum (Card and Krueger 1995; Converse, Coe, and Corcoran 1981; Neumark, Schweitzer, and Wascher 2004; Palley 2000; Pollin and Brenner 2001; Pollin, Brenner, and Wicks-Lim 2004; Reich and Hall 2001).

A subset of these studies provides the groundwork for the more detailed analyses discussed below. In contrast to Gramlich's study, these studies directly estimate the range of the minimum wage ripple effect. These studies conclude that the minimum wage ripple effect dissipates quickly as wages rise and consistently find that proximity to the minimum wage is a key determinant in whether minimum wage changes impact wages. Table 2.1 summarizes the findings of these studies.

A survey conducted by Converse, Coe, and Corcoran (1981) provides high-end estimates of the range of the ripple effect. They surveyed approximately 1,400 establishments over the 1979 and 1980 federal minimum wage increases (from $2.90 to $3.10 on January 1, 1979; from $3.10 to $3.35 on January 1, 1980). These establishments varied in industry, occupation, and whether they employed minimum wage workers. Among employers that reported that they gave non-mandated raises in response to the federal minimum wage increase, the overwhelming majority (93 percent in 1979 and 91 percent in 1980) limited these non-mandated raises to workers earning less than 226 percent of the old minimum wage level (roughly less than 200 percent of the new minimum wage level). Similar proportions of employers with directly affected workers (workers who received mandated raises) reported giving non-mandated raises within the same range. However, because wide wage intervals were used to define the response categories of these questions, the ripple effects reported by these employers may have ended anywhere between approximately 140 percent to 225 percent of the old minimum wage (or 130 percent to 200 percent of the new minimum wage). In any case, this study provides upper-end estimates on the range of wages affected by minimum wage increases.

Estimates from other studies suggest that the range is limited to about 150 percent of the new or old minimum wage. Through an inspection of the shifts in teenagers' wage distributions before and after the federal minimum wage increases of the early 1990s, Card and Krueger (1995) conclude that the effect of these increases extend, at most, to 134 percent of the old minimum or 106 percent of the new minimum. In a survey of Texas fast food restaurants, Katz and Krueger (1992) asked employers directly whether they provided non-mandated raises in response to the federal minimum wage increase to $4.25 in 1991. Consistent with Card and Krueger's findings, the survey responses indicate that wage ripple effects extend at least to $4.50 (or 115 percent of the old minimum wage, 106 percent of the new minimum wage). Van Giezen (1994) also uses survey data of fast food establishments to examine the effects of this federal minimum wage increase. He finds that workers upwards of 115 percent to 130 percent of the federal minimum wage in 1990 to 1991 experienced wage increases when the federal minimum wage rose. Reich and Hall (2001) analyze the relatively dramatic rise—35 percent—in California's state minimum wage from 1995 to 1999. Their analysis suggests that the extent of the minimum wage ripple effect reaches up to 153 percent of the initial minimum wage floor (113 percent of the new minimum wage).

The basic findings of these studies confirm Gramlich's initial observation—that the minimum wage ripple effect results in wage compression—and refines this observation by placing a limit on the minimum wage ripple effect. Given that during 1983-2002 states' median wages range from a minimum of 153 percent to 311 percent of states' prevailing minimum wages (i.e., the greater of the federal and state minimum wage)[5], these studies limit the minimum wage ripple effect to the bottom half of the wage distribution.

The studies discussed thus far provide general contours of the behavior of minimum wage ripple effects but do not provide detailed lines of the magnitude, or how the magnitude varies over the range of affected wages. The next set of studies I discuss address these aspects of the ripple effect. Out of these studies evolves the second refinement of Gramlich's preliminary estimates based on time-series data. Specifically, these studies use some variation of the difference-in-difference approach (described in detail below) in order to exclude spurious relationships that tend to affect data with a time-series dimension.

Card and Krueger (1995) provide a clear statement of the methodological issue facing studies using time-series data to estimate the effects of policy measures such as the minimum wage. I repeat their critique at length below, given as part of their review of minimum wage employment effect studies:

> The time-series approach has major disadvantages, as well. First and foremost, the counterfactual is not clear. The aggregate time-series approach implicitly compares employment in years during which the minimum wage is relatively high with employment in years during which it is relatively low. Many things change over time, however. The problem is that it is difficult to distinguish the effect of the minimum wage on employment from the many other factors that are occurring simultaneously. Although time-series studies attempt to control for the effect of change in some exogenous variables (for example, the state of the business cycle), one can never be certain whether the controls are adequate. The implicit assumption is that, controlling for the other explanatory variables, employment would be the same over time if the minimum wage were constant. Unfortunately, there is no way to test this assumption, because the aggregate time-series studies do not try to identify groups that are unaffected by the minimum wage. (Card and Krueger 1995, 183)

With regard to Gramlich's original approach, his estimation of a Phillips curve wage equation (see equation 2.1) relies on time-series data set that does not provide an appropriate counterfactual to occurrences of minimum wage increases. That is, his analysis compares the wage changes that take place when an increase in the minimum wage occurs to the wage changes that take place when the minimum wage is constant. With time-series data, these occurrences take place at different points in time and in order to draw conclusions about the affect of the minimum wage, he has to make the strong assumption that other factors that may affect wage changes are constant or properly controlled for in his model. Consequently, Gramlich's estimate of the affect of the minimum wage on the average wage of hourly non-supervisory workers is open to critique.

The findings of a study by Palley (2000) are similarly open to critique, particularly given that his results depart markedly from the other research surveyed above. His estimates suggest that, "For male workers the impact [of the minimum wage] extends robustly throughout the wage distribution" (Palley 2000, 1). In this study, Palley analyzes national-level time-series (annual) data from 1972-2000 to estimate the following model, separately for men and women at ten different wage percentiles:

$$\text{(2.2)} \quad \Delta\log(\text{qth percentile}_t) = \alpha + \beta_1\Delta\log(\text{minimum wage}_t) + \beta_2\Delta\log(\text{minimum wage}_{t-1}) + \beta_3\Delta\text{unemployment rate}_t + \varepsilon_t$$

where all dollar values are in constant 2000 dollars, and the unemployment rate is measured for men or women as appropriate. He also estimates a variation on this model to estimate long-run effects. It is hard to know for certain, but because male workers wages tended to move with the business cycle peaks and troughs (see Mishel, Bernstein, and Boushey 2003) and the federal minimum wage increases took place during business cycle upswings during the decades of his study (these include the federal minimum wage increases in 1974, 1978, 1979, 1980, 1981, 1990, 1996, 1997; the 1991 federal minimum wage increase took place in the midst of a recession) it is likely that the intermittent wage gains (or smaller losses) of male workers would, statistically, appear to have some relationship with the minimum wage. On the one hand, minimum wage increases may be correctly identified as the causal factor for male workers' wage increases if all other relevant factors are properly controlled for. On the other hand, the coincidence of the upswings in the business cycles during this period with minimum wage increases raise the concern that, as Card and Krueger argue, "…one can never be certain whether the controls are adequate" (Card and Krueger 1995, 183). Because there is no counterfactual of male workers earning wages equal to a particular wage percentile who do not experience a minimum wage increase *at the same point in time* to compare to the wage increases of male workers earning wages equal to the same wage percentile and who do experience a minimum wage increase, it is difficult to conclude whether it is, in fact, the minimum wage that is the causal factor in increasing these workers' wages.[6] Both the pattern in the magnitude of minimum wage effects and the extent of the minimum wage effect on male workers' wages suggest that there is some spurious relationship (i.e., omitted variable correlated with the minimum wage) that is biasing Palley's regression coefficient estimates.[7]

As implied by quotation from Card and Krueger above, one method of more comprehensively controlling for other changes occurring simultaneously with minimum wage increases is to find a subset of workers who provide an appropriate counterfactual.[8] An appropriate counterfactual is a subset of workers that can be observed over the same time period who do not experience a change in the minimum wage who, in all other relevant aspects, are similar to workers who do experience a change in their minimum wage. A commonly used approach to finding such a counterfactual is the difference-in-difference approach used extensively in Card and Krueger's 1995 minimum wage research. Such an approach uses panel data (time-series cross-sections) that can be used to "difference out" spurious trends. The difference-in-difference approach compares the change (the difference over time) in wages of one group of workers, identified as subject to a change in the minimum wage (the experimental group) to another group of workers identified as not being subject to a change in the minimum wage (the control group). The difference between the two groups' wage differences over time identifies the effect of the minimum wage. Thus, the name difference-in-differences. To date, at least three studies have employed variations in this approach in estimating the magnitude of the minimum wage ripple effect, that is, the wage elasticity with respect to changes in the minimum wage of workers who receive ripple effect raises (see table 2.2).

Card and Krueger (1995) employ two different techniques to employ this difference-in-difference approach. Their first technique relies on descriptive statistics.[9] For example, they examine the movement of the 5th and 10th wage percentiles in states that have a high concentration of near-minimum and minimum wage workers among teenagers over 1989 to 1991, assuming that these states are likely to be significantly impacted by the 1990 to 1991 federal minimum wage changes. Their counterfactual is the movement of the 5th and 10th wage percentiles in states that have a relatively low concentration of near-minimum and minimum wage workers among teenagers. These states are expected to experience a low-level of impact from the federal minimum wage, and thus are expected to exhibit trends experienced by low-wage workers in general. They formalize this analysis with their second technique: they use regression analysis to measure the difference-in-differences between high- and low-impact states. They estimate the following models (Card and Krueger 1995, 296) as having the form (standard errors are in parentheses):

$$\text{(2.3)} \quad \Delta\log(\text{5th wage percentile}_s) = \underset{(0.16)}{1.18}\ (\text{Fraction of affected workers}_s) + \underset{(0.50)}{0.27}\ (\Delta\text{Employment Rate}_s)$$

$$\text{(2.4)} \quad \Delta\log(\text{10th wage percentile}_s) = \underset{(0.14)}{0.69}\ (\text{Fraction of affected workers}_s) + \underset{(0.42)}{0.46}\ (\Delta\text{Employment Rate}_s)$$

where the unit of observation is state, "Fraction of affected workers" is the proportion of workers earning between \$3.35 and \$4.24 in April to December 1989, and "ΔEmployment Rate" is measured by the change in employment to population ratio (by state) from 1989 to 1991. Both techniques suggest that there is at least an effect up to the 10th wage percentile, but not as far as the 25th wage percentile. Using their estimate of the percent of affected workers (8.7 percent) before the April 1990 increase, their regression results suggest that the 5th wage percentile, on average, experiences a 10 percent increase given a 27 percent increase in the federal minimum wage (from \$3.35 to \$4.25 in 1991); in other words, a wage elasticity of 0.38. The 10th wage percentile, on average, experiences a 6 percent increase given a 27 percent increase in the federal minimum wage or roughly a 0.22 wage elasticity.

Pollin, Brenner, and Wicks-Lim (2004) conclude that the ripple effect extends no further than the 15th wage percentile based on their estimates of a similar model using regression analysis, this time examining the change in wage percentiles over the years 1991 to 2000. Their model exploits the variation in both state and federal minimum wage laws, as well as the varying concentration of low-wage workers across states at different points in time. Because the context of this study is an examination of a proposed 19.5 percent increase in Florida's prevailing minimum wage, they provide the following wage elasticities for Florida specifically: 0.30 for the 5th wage percentile (6.3 percent increase in the 5th wage percentile given a 19.5 percent increase in the minimum wage) and 0.11 for the 10th wage percentile (2.3 percent increase in the 10th wage percentile given a 19.5 percent increase in the minimum wage).

These two studies flesh out a bit further details about the behavior of minimum wage ripple effects. While consistent with the studies discussed above, these more fine-grained analyses narrow the approximate range of the minimum wage ripple effect to the bottom quarter of the wage distribution. Their results also provide more detail about the rate at which the ripple effect dissipates: non-mandated raises are consistently a fraction of the mandated raises.

A primary weakness of these studies is that the models do not control for factors that are traditionally considered important in wage determination: demographic characteristics. For example, the rapid fall in union density that occurred during the 1980s (the decline continued in the 1990s, but the steepness of the decline is more pronounced in the 1980s, see Mishel, Bernstein, and Boushey 2003) may cause wages to fall for two reasons: 1) because individual workers change their union status, and 2) because lower union density are associated with lower union premiums. If wages were falling (not increasing or increasing slowly) during the 1980s, when minimum wages were generally stagnant, due to this decline in union density, then wage increases attributed to the minimum wage at and after the end of the 1980s may overstate the impact of minimum wage increases on wages. A rigorous accounting of minimum wage effects should include controls for demographic trends.

One study that takes a comprehensive approach to modeling wage growth associated with minimum wage changes using panel data is Neumark, Schweitzer, and Wascher (2004). Because of the important role this study plays in motivating the methodology used in this research project, I provide here a relatively in-depth dicussion of their results and methodology with regard to the minimum wage ripple effect.

The basic approach of this study is to use micro-data from the Current Population Survey outgoing rotation groups, from 1979 to 1997, to regress individuals' one-year wage growth on one-year changes in the minimum wage. Different from the studies discussed above, this model accounts for demographic characteristics (although surprisingly absent is union status), uses a full set of state-year indicator variables to control for macroeconomic or state-level effects on wage growth, and systematically attempts to measure how the minimum wage effect varies across the entire wage distribution. To do this, they interact this measure of minimum wage changes with a set of 12 indicator variables that correlate with different regions of the wage distribution. A simplified version of their model is presented here to facilitate this discussion:

$$(2.5) \quad \frac{Wage_{i2} - Wage_{i1}}{Wage_{i1}} = \alpha + \sum_j \beta_j \frac{MW_{i2} - MW_{i1}}{MW_{i1}} \text{ x } R(Wage_{i1}, MW_1) + \sum_j \eta_j R(Wage_{i1}, MW_1) + \varepsilon_i$$

Where i denotes individuals, the subscripts 1 and 2 denote year 1 or year 2 observations, MW is the prevailing minimum wage, and R is the region of the wage distribution a worker is in during year 1. Specifically, these regions are defined by their distance from the prevailing minimum wage. Workers are categorized into these regions by their wages in the initial year ($Wage_{i1}$) of the two-year interval they are observed. For example, workers who earn below the prevailing minimum wage minus $0.10 in the first of the two years that they are observed all have the value 1 for the indicator variable R_1, and all other workers have a value of 0. Workers who earn between the prevailing minimum wage minus $0.10 and the prevailing minimum wage plus $0.10 in year 1 all have the value 1 for the indicator variable R_2, and all other workers have a value of 0, and so on (up to j regions). In this way, Neumark, Schweitzer, and Wascher are able to calculate separate wage elasticities, with respect to changes in the minimum wage, for wages across the wage distribution. The model includes interactions terms of these indicator variables with a lagged minimum wage change variable to account for longer-term effects (not presented in the equation above).

Neumark, Schweitzer, and Wascher's comprehensive set of estimates of contemporaneous minimum wage effects fall in line with others studies: the positive wage effect is strongest near the minimum wage level and dissipates quickly, effects becoming small and/or statistically insignificant by the time wages reach 200 percent of the minimum wage in year 1. Their study however, also produces some curiously different results: A small but statistically significant effect is reported for wages five to eight times the minimum wage in year 1. In 1997, these wages would correspond with hourly rates roughly between $29.00 and $38.00. While they argue that supply shifts or relative demand shifts could produce an indirect effect on such high-wage workers, these findings diverge from the findings of past studies, with the exception of Palley's (2000) results based on time-series data.

As a matter of course, it is useful to test the robustness of results across different sets of years, given the potential for time-varying factors, even in the case of panel data, to create spurious results. To see how panel data can still produce spurious results, note that there are potentially two ways in which panel data sets provide counterfactuals to minimum wage changes in this type of model: 1) by providing different panels that do not experience minimum wage changes at the same point in time and 2) by providing panels that do not experience minimum wage changes at different points in time. For example, imagine that only one state increases its minimum wage in year t. In this case, workers who reside in the remaining 49 states provide counterfactuals taking place at that same time. Now consider what happens when three years of data from the 50 states are pooled. Assume that no other minimum wage increases take place. Now, the counterfactuals include the wage changes of workers who reside in the other 49 states in year t and the wage changes of workers all 50 states in year t+1, and year t+2. This example illustrates that while the difference-in-difference approach provides some controls to counter spurious results due to insufficient controls for time-varying factors, multiple time-series data or panels pooled over relatively large numbers of years (as in the case of Neumark, Schweitzer, and Wascher's 2004 study) are still vulnerable to them. Panel data increases sample size and allows for a more rigorous model (e.g., controls for state-level fixed effects or demographic characteristics) but re-introduces the issue of time-varying omitted variables.

The importance of the potential for conflating other factors with the minimum wage effect is made clearer when one considers the pattern in the state and federal minimum wage changes during the years that Neumark, Schweitzer, and Wascher study. States' minimum wages increased above the federal minimum primarily during the late 1980s and again in the late 1990s. Federal minimum wage increases, similarly, took place in 1990-1991 and in 1996-1997. Thus increases in minimum wages generally tended to occur during macroeconomic upswings resulting in a heavy representation of "affected" or "experimental" observations near business cycle peaks. The confluence of these two factors will likely cause their effect to be conflated in any analysis. So that, even with the use of panel data, the patterns of the federal and state minimum wage increases challenge the researcher to find a way to properly identify the minimum wage effect. One substitute for providing adequate controls directly in a model is to test the robustness of one's results over different time periods. By doing this, the researcher may hope to break the correlation underlying spurious relationships that may be sensitive to which years are included in one's analysis. This is a particularly hopeful approach in the context of analyzing minimum wage effects given that there was a federal minimum wage change that took place in the midst of a recession (April 1, 1991). This may allow the researcher to vary the degree to which one's sample is weighted toward years in which minimum wages increases coincided with expansionary economic times. Such robustness checks on Neumark, Schweitzer, and Wascher's results may provide some insight into the appearance of positive, statistically significant minimum wage ripple effects for the highest-paid workers. Note, too, that the constraint of estimating the relationship between various labor market characteristics and wage changes in a single equation, for both high-wage and low-wage workers, may itself make regression estimates more vulnerable to producing spurious results.

Another curious result is the consistently negative effect of their lagged minimum wage variable (a one-year lag). Their regression estimates suggest a strong and sizable negative effect for wages near the minimum in year 1, wiping out as much as 75 percent of the minimum wage's initial positive effect for workers earning within $0.10 of the minimum wage. This negative, lagged affect can be understood to be measuring the fact that employers implicitly require workers to "give back" their raises by withholding raises they would have received over the next year if the minimum wage hadn't risen. In other words, employers "…take advantage of inflation in subsequent years to realign wages, partly undoing the effects of legislated nominal wage increases for low-wage workers" (Neumark, Schweitzer, and Wascher 2004, 438). In other words, the minimum wage raises only temporarily increase the real value of workers' wages. Given that real wages for low-wage workers generally do stagnate or fall between federal minimum wage increases (see below for further discussion; also see Mishel, Bernstein, and Boushey 2003, 131-133) during this time period, these results are quite plausible. This negative effect, however, extends throughout the wage distribution, up to the highest wage interval (600 percent to 800 percent of the minimum wage in year 1), so that a 10 percent increase in the minimum wage, one year prior, would result in 2 percent decline in real wages for these high wage workers.

It is theoretically unclear whether the minimum wage has a lagged effect. If the minimum wage produces a ripple effect due to the rigidity of the wage structure, the impact of the minimum wage may be immediate: as soon as the wage floor rises, other wages are raised in tandem to maintain workers' relative wage positions. However, these adjustments may be implemented over time precisely because they are not mandated. If ripple effects are caused by employers substituting toward higher skilled labor, a lagged effect is more likely as adjustments to the workforce require more time than adjustments to the wage schedule (Grossman 1983; Baker 1999). Lagged effects may occur for reasons unrelated to ripple effect, as noted by Neumark, Schweitzer, and Wascher, as employers may allow wages to stagnate after a minimum wage increase. The existence of lagged effects, then, is an empirical one. Other studies have produced conflicting estimates that point to positive or no lagged effects. Grossman (1983), discussed above, finds limited evidence of generally positive lagged effects,[10] whereas Gramlich's (1976) findings suggest no lagged effect. Palley (2000) finds evidence of a positive lagged effect (one year lag) for some wage percentiles using time series data, but not others, and with no discernible pattern. Neumark, Schweitzer, and Wascher's estimates of a consistent and extensive negative one year lag effect of a minimum wage change stands in contrast to these other studies.

A closer examination of how Neumark, Schweitzer, and Wascher measure the lagged minimum wage effect raises a question about whether their estimates are capturing some other effect. As described above, Neumark, Schweitzer, and Wascher use a set of indicator variables to separately measure the lagged minimum wage effect on different regions of the wage distribution. A problem arises because the regions are defined relative to the minimum wage (e.g., 110 percent of the minimum wage to 120 percent of the minimum wage); the region itself shifts, with respect to the wage distribution, after a minimum wage change. Some wage trends are presented in figure 2.2 to illustrate. Among workers in 1989, eight years after the 1981 federal minimum wage change, the subset of workers earning within $0.10 of the federal minimum wage (one of Neumark, Schweitzer, and Wascher's wage regions) earn wages in the extremity of the lower left tail of the wage distribution, with the 5th wage percentile hovering at the upper end of the minimum wage ±$0.10 interval. Then look at the same interval of wages (minimum wage ±$0.10) after April 1991, one year after the federal minimum wage increase in April 1990. The 5th percentile now sits toward the bottom of this wage interval (this is exacerbated by having two consecutive increases in the federal minimum wage). Also note how the wage interval is in much closer proximity to the 10th wage percentile. So that, which portion of the wage distribution that this particular indicator variable isolates is sensitive to whether a minimum wage increase has just occurred.

In the example just described, the wages included in the minimum wage ±$0.10 interval changes from the lowest 5 percent of the working population to wages above, roughly, the lowest 5 percent. Figure 2.2 presents similar trends for another region used in Neumark, Schweitzer, and Wascher's analysis: 130 percent of the minimum wage to 150 percent of the minimum wage. Again, note how the region of the wage distribution identified by this indicator variable shifts from wages below the 20th percentile to wages above the 20th percentile one year after the April 1990 minimum wage change. Two years after the 1991 minimum wage increase, the 20th percentile creeps upward within the wage interval.

The problem that these indicator variables introduce is that the counterfactuals for the lagged minimum wage effect are not appropriate. In effect, Neumark, Schweitzer, and Wascher's model measures the lagged minimum wage effect by comparing wage changes of one set of workers (the control group) to another set with relatively higher wages (the experimental group). While this is problematic is general, this can cause problems particularly for measuring effects at the low and high end of the wage distribution. To see this, consider wages at the extremes of the wage distribution. These wages are more likely to be affected by measurement error, and thus exhibit regression to the mean, when examining individuals' wage changes.[11] Thus, workers with initial wages at the low end will tend to have greater wage changes (all else equal) compared to other workers because of measurement error. Conversely, workers with initial wages at the high end will tend to have smaller wage changes (all else equal) compared to other workers because of measurement error. In fact, Neumark, Schweitzer, and Wascher note that their regression estimates of the indicator variables (Rj) indicate, "…substantial regression to the mean. That is, estimated wage and earnings growth is very strong in the bottom part of the distribution, and weak or negative at the top" (Neumark, Schweitzer, and Wascher 2004, 431). Now consider the effect of how the regions examined by Neumark, Schweitzer, and Wascher shift when there is a lagged minimum wage change. These shifts result in comparing workers with relatively higher initial wages to workers with relatively lower initial wages when there is a lagged minimum wage change. Given that workers with relatively higher wages are more likely to experience reductions (or smaller increases) in their wage change as a result of measurement error compared to workers with lower wages, finding negative lagged minimum wage effects is unsurprising, particularly at the extremes of the wage distribution.[12] These observations suggest that while Neumark, Schweitzer, and Wascher's wage intervals have the appealing quality of methodically detailing how minimum wage changes may affect discrete regions of the wage distribution, the technique they use likely exacerbates the measurement error in the data in a way that produces biased estimates for the lagged minimum wage variable. This in turn has substantial consequences for their overall estimates of the minimum wage effect throughout the wage distribution which incorporate the large, negative, lagged effects. [13] In particular, they conclude that:

> The inclusion of lagged minimum wage effects is critical in arriving at the conclusion that low-wage workers are adversely affected, as we find that contemporaneous effects overstate the wage gains and understate the hours and income losses experienced by low-wage workers when minimum wages rise. (Neumark, Schweitzer, and Wascher 2004, 449)

This detailed review of past studies, and Neumark, Schweitzer, and Wascher's 2004 study in particular, provides an introduction to the empirical findings and methodological challenges that shaped the approach discussed in the next section. To summarize, the following conclusions about past research on the minimum wage ripple effect can be drawn: First, the ripple effect is not extensive: its reach is limited within the bottom half of the wage distribution. A corollary to this conclusion is that the magnitude of the ripple effect drops quickly as wages rise. Second, a significant methodological challenge facing this research project is finding a way to distinguish between the effects of minimum wage changes and the effects of economic trends spuriously correlated with minimum wage changes. Third, a proper, rigorous, and detailed estimation of the minimum wage effect across the wage distribution has yet to be offered.

2.3 Data and Methodology

2.3.1 Data

The primary data source for the analyses in this chapter is the 1983 to 2002 outgoing rotation groups of the Current Population Survey (CPS) prepared by the Bureau of the Census for the BLS. These data files are commonly referred to as the CPS-ORG files. The CPS surveys approximately 50,000 households monthly and asks one-quarter of this sample detailed earnings questions. All of the following minimum wage analyses are based on a sample limited to members of the civilian labor force who are at least 15 years old, employed in the public or private sector, and have positive wage earnings within the range of $0.50 and $100.00 (in 1989 dollars). The last qualifier is used to minimize measurement error in the wage variable. Self-employed workers are excluded because CPS surveys from 1993 and earlier do not collect earnings data for self-employed workers. Also, given that the occurrence of ripple effects is largely thought of as a response by an employer to the expectations of workers, the appropriateness of including the wage-response of self-employed workers is unclear. Sampling weights provided by the BLS make the sample nationally representative and are used when deriving estimates from individual-level data.

Hourly earnings data are either 1) taken as reported by hourly-wage workers or 2) imputed by dividing weekly earnings by usual weekly hours. There is the option of imputing the wage of hourly-paid workers based on weekly earnings, in order to include any overtime, tips or commission. However, as documented by Mishel, Bernstein, and

Boushey (2003), data on overtime pay, tips and commission appear to be unreliable and are likely to introduce a significant amount of measurement error in the wage measure. Also note that starting in 1994, the CPS allowed respondents to report that their usual hours vary. As a result, hourly earnings could not be calculated for non-hourly paid workers that report varying hours. These respondents were excluded from the sample. This exclusion reduces the sample by roughly 2 to 3 percent according to the analysis of 1995 to 1997 data by Mishel, Bernstein, and Boushey (2003).

The sample begins in 1983 because an important demographic variable, union membership, was not asked of all the outgoing rotation groups before 1983. As a result, only data from 1983 on provide sufficient samples sizes to carry out the analyses with the union status variable. The dramatic decline in union membership alongside a stagnating federal minimum wage requires accounting for union status in any minimum wage analysis. The sample ends in 2002 because of the comprehensive change in industry classification systems from the Standard Industrial Classification (SIC) system to the North American Industry Classification System (NAICS). This change precludes creating a consistent data series on the industry variables used in the analysis.

The years covered in this analysis roughly cover two business cycles. 1983 to 2002 cover most of the 1980s business cycle (1982 to 1990) and the entire 1991 to 2000 cycle.[14] This timeframe includes two recessions that are preceded by two long expansionary periods. As a result, the sample is predominately composed of years in the upswing of a business cycle.

Apart from the issue of data availability, these years are particularly useful for studying the effect of minimum wage changes because a number of states set their own state-level minimum wages at higher levels than the federal minimum in the mid-to-late 1980s and in the late 1990s. These state-level minimums have the appealing quality of increasing the variability of the prevailing minimum wage measure at a particular point in time. The federal minimum wage increased four times during these years: 1990 (from $3.35 to $3.80), 1991 ($3.80 to $4.25), 1996 ($4.25 to $4.75), and 1997 ($4.75 to $5.15).

2.3.2 Methodology

The basic analytic strategy follows in the tradition of previous studies while addressing the weaknesses discussed in section 2.2. At its most basic, the model used here is a modification of the usual Mincer human capital wage equation to include the minimum wage as a wage determinant. Wages, then, are modeled as outcomes of individual attributes and labor market institutions. Two other major modifications are made to the human capital wage equation. First, I estimate the wage equation as a semi-parametric function of the minimum wage by generating estimates separately for thirteen different points in the wage distribution. Second, the wage equation is transformed into a first-difference model. Each of these aspects of the model will be discussed in turn.

As described in the previous chapter, past research stresses the importance of the proximity of a worker's wage to the minimum wage—his/her wage relative to the minimum—as a factor in whether a worker's wage is affected by changes in the minimum wage. One of the first puzzles for the researcher to solve when using regression analysis to estimate ripple effects is forming an econometrically sound methodology to estimate wage effects for well-defined points in (or regions of) the wage distribution. Past studies have used different methods: by dividing workers according to a covariate of wages and estimating wage equations for each group separately, by examining wage changes and assigning individuals to wage ranges according to their initial wage, or by using wage percentiles as their outcome variable (see discussion in section 2.2).

The disadvantage of using covariates to identify regions of the wage distribution is that the researcher can only imprecisely observe the worker's relative position to the minimum wage. Because the proximity of wages is a key factor in characterizing the ripple effect of the minimum wage, creating sub-samples of workers based on a covariate (e.g., educational attainment), rather than a direct measure of wages, substantially reduces the ability of the researcher to link a minimum wage effect to a particular point in the wage distribution, and thus to characterize the extent and magnitude of the ripple effect.

Why not divide the sample by a direct measure of wages? Dividing the sample by a direct measure of wages has the well-documented problem of potentially biasing regression estimates (Koenker and Hallock 2001). To see this, consider using ordinary least squares (OLS) regression to estimate the bivariate relationship between the minimum wage and wages. Say that the minimum wage and wages vary directly. Also, assume that we are able to estimate this positive relationship correctly and that the errors are normally distributed with a mean of 0. Such a relationship based on hypothetical data is displayed in panel A of figure 2.3. Note the positive coefficient of 1.02 on the minimum wage variable. At low levels of the minimum wage, workers who earn more, the same, and less than the expected wage, as predicted by the regression model, would be observed. Now assume I use wages to delimit a specific portion of the wage distribution. In this example, I focus on the wage interval of $2.00 to $6.00. If in the true relationship wages increase with the minimum wage, more workers with negative errors will be excluded at low values of the minimum wage than at high values of the minimum wage. This is made apparent in panel B of figure 2.3, where observations with wages outside the wage interval are excluded from the sample. Likewise, at high values of the minimum wage, there will be more workers with positive errors that will be excluded. Therefore, when these observations are excluded the error term will vary inversely with the minimum wage. That is, the error term is likely to be correlated with the minimum wage, a violation of one of the basic assumptions necessary for unbiased OLS coefficients. In fact, the (biased) regression coefficient on the minimum wage has fallen to 0.52, about half the magnitude of the original estimate.

An alternative is to use a measure of wage *change* as the outcome variable and then to assign individuals to particular wage ranges using only their wage at time 1. In this case, individuals' wages at time 2 are left unrestricted (i.e., the dependent variable is no longer truncated). This approach circumvents the issue of bias due to sample truncation. Unfortunately, qualities of the CPS survey generate a different set of problems for such an approach.[15] Aside from the issues raised with the Neumark, Schweitzer, and Wascher (2004) study discussed above, another methodological concern raised by the use of individually-matched CPS data is how qualities of the sample are changed by the matching process.

Because the detailed wage data in the CPS are only collected among the outgoing rotation groups, using individual-level data matched over time restricts one's sample to individuals that remain in the same residence over a one-year period.[16] The importance of these non-matches and the role of residential mobility as a source of non-matching is documented by Madrian and Lefgren (1999). Examining matching rates over one year, Madrian and Lefgren find that approximately 29 percent of individuals cannot be merged properly using the identification variables provided in the CPS, a significant proportion. Of those 29 percent, over half of the non-matches are due to residential mobility. If there is no meaningful difference related to how the minimum wage may impact wages between transient, low-wage workers and low-wage workers who maintain stable residences then the attrition bias will be limited. However, one can imagine that maintaining a stable residence is likely to be related to the ability of individuals to obtain moderate wage jobs relatively quickly in one's work career. In fact, Madrian and Lefgren find that non-matches tend to have lower family incomes, lower individual incomes, and a greater likelihood to have moved the year prior (Madrian and Lefgren 1999, 39). These differences are likely to be exacerbated when studying wage changes, because observations on wage changes additionally require that individuals are employed at both points in time. Therefore, it is plausible that transient (with regard to residency and employment) individuals are more likely to work in minimum and near-minimum jobs for extended periods of time relative to low-wage individuals who are more stable. There are a couple reasons why these differences between transient and other low-wage workers may influence the impact of the minimum wage. On the one hand, I expect that workers who remain in low-wage jobs for an extended period of time would be most likely to perceive relative wage changes as the minimum wage changes. Workers who move out of minimum-wage jobs quickly are less likely to feel a need to preserve their relative wage since they expect their wage position to significantly improve via an experience premium or job change. Therefore, if there are rigid wage norms among low-wage workers, this sample would tend to understate the impact of a ripple effect. On the other hand, workers whose job opportunities are limited to minimum wage jobs are also least likely to have the resources to advocate for the preservation of their relative wage position. Therefore, excluding transient workers may overstate the ripple effect.

Another drawback to using individually-matched data over time is the issue of measurement error, as discussed above. A consequence of using individual-level data in calculating wage changes is the potential for wage changes to reflect a substantial amount of regression to the mean, inflating wage changes for workers at the low-end of the wage distribution and deflating wage changes for workers at the high-end of the wage distribution. Summary measures such as sample means or sample percentiles used to create aggregate units, on the other hand, moderate the influence of the large individual changes due to regression to the mean.

Given these problems associated analyzing ripple effects using individual-level data from the CPS, I chose to rely on summary measures of individual-level data for my outcome variable. More specifically, I estimate wage percentiles at the state-level and use these state-level observations for my dependent variable. To summarize the discussion above, these state-level measures have the following attractive qualities. First, they retain the appealing aspect of allowing the researcher to methodically estimate minimum wage effects at different points in the wage distribution. Second, when used to construct wage changes, the state wage percentiles retain information from all respondents with valid wage data at one point in time. Third, they are more robust to measurement error. Fourth, state wage percentiles improve on Neumark, Schweitzer, and Wascher's relative wage measure because wage percentiles are not linked mechanically to the minimum wage. As a result, estimated relationships between wage percentiles and the minimum wage should be confined to economic relationships rather than constructed by the structure of the model.

One other estimation technique that deserves mention here is quantile regression. This estimation technique uses a different minimization criterion than OLS (for an introduction to quantile regression see Koenker and Hallock 2001) that provides a way to estimate the effects of a set of covariates on a dependent variable at different points of the dependent variable's conditional distribution. At first glance, quantile regression may appear to produce similar results to those estimated by using OLS with wage percentiles as the dependent variables. This is true in special cases (this is discussed further in chapter three). The important aspect to keep in mind is that quantile regression fits the regression line through percentiles of the *conditional* wage distribution which may not be appropriate. Because I am interested in estimating how the wage effect of the minimum wage changes for wages that are close to the minimum wage and upward, it is important that the percentiles of the conditional wage distribution are somewhat uniform in their relative position to the minimum wage. This fact limits the ability of the researcher to control for many important factors.

For example, say that I wanted to estimate the effect of the minimum wage at the 5th percentile of the conditional wage distribution and say the regression equation includes an indicator variable for union status. The quantile regression would estimate the wage effect of the minimum wage based on the movement of the 5th wage percentile of nonunion workers, as well as, the 5th wage percentile of union workers. Given that union workers earned, on average, 12 percent more than nonunion workers in 2001 (Mishel, Bernstein, and Boushey 2003) the estimated wage elasticity produced by quantile regression would be based on a relatively large range of relative wages (relative to the minimum wage, that is). Consequently, the results would produce ambiguous estimates of the reach and magnitude of ripple effects.

To get relatively precise results, the percentiles of the conditional wage distribution need to track the proximity of the workers' wages relative to the prevailing minimum wage rather than relative to a specified reference group. Thus there is a trade-off between including controls in one's model and the precision of ripple effect estimates when using quantile regression. Because of this trade-off, I chose not to use this technique for this analysis. While the relative position of wage percentiles of states' unconditional wage distributions to the states' prevailing minimum wages also varies (across states and across time), the degree of variation is significantly more limited than that caused by using wage distributions conditional on the demographic variables typically included in the human capital wage equation.

Using state-level wage percentiles as the outcome variable, I estimate minimum wage effects for the following thirteen different wage percentiles, separately: 5th, 10th, 15th, 20th, 25th, 30th, 35th, 40th, 50th, 60th, 70th, 80th, 90th. By taking this semi-parametric approach, each of the variables included in the model (presented below)—including the minimum wage—may flexibly vary in effect for each wage percentile.

Aggregating individuals units into state units requires the construction of summary measures for most of the other variables in the wage equation besides the dependent variable. To facilitate the description of how I construct these aggregate measures I present a wage equation (below) that is analogous to the one ultimately used in this study. Starting with this analogous form allows me to demonstrate the direct correspondence between the basic model of this study and the Mincer wage equation, which might otherwise be unclear.

$$
\begin{aligned}
(2.6)\ \ln(\text{wage}_{ist}) = \quad & \alpha + \beta_1 \ln(\text{min}_{st}) + \beta_2 \ln(\text{min}_{s\ t-1}) + \beta_3(\text{female}_{ist}) + \beta_4(\text{nonwhite}_{ist}) \\
& + \beta_5(\text{experience}_{ist}) + \beta_6(\text{experience}_{ist}^2) + \beta_7(\text{hsgrad}_{ist}) \\
& + \beta_8(\text{some college}_{ist}) + \beta_9(\text{collgrad}_{ist}) + \beta_{10}(\text{union}_{ist}) \\
& + \beta_{11}(\text{FT status}_{ist}) + \sum_I \beta_I(\text{Industry}_{ist}) \\
& + \sum_O \beta_O(\text{Occupation}_{ist}) + \sum_Y \beta_Y(\text{Year}_t) + \varepsilon_{ist}
\end{aligned}
$$

where the subscript i denotes the individual, the subscript s denotes the state and the subscript t denotes the point in time. This model follows the general form of a Mincer wage equation: the wage (logged) is regressed on a set of human capital measures including dummy variables for the following categories: female, nonwhite, high school graduate (and no further degree), college-educated without a BA, college graduate with BA or higher degree, union member, and full-time worker. Added to the standard human capital measures, I include twenty-one dummy variables for each major industry group and thirteen dummy variables for each major occupation group.[17] I also include the variables of interest, the contemporaneous and lagged values of the prevailing minimum wage. The final set of terms is a set of dummy variables for each year included in the analysis. The basic differences between equation 2.6 and the basic model (equation 2.8) of this study is that the unit of analysis of equation 2.6 is individuals rather than states, and the variables are in levels rather than differences.

State averages (means) of the individual-level observations at a point in time convert these individual-level observations to state-level observations. In the case of indicator variables, the averages amount to proportions. For example, the indicator variable for female is transformed into the proportion of the state sample that is female. The indicator variables for industry affiliation are transformed into measures of each state's industry composition. States' prevailing minimum wages and year indicator variables are uniform within state thus they are unchanged at any given point in time. Thus equation 2.6 is transformed into:

$$\begin{aligned}
(2.7)\ \ln(\text{wage percentile}) = \quad & \alpha + \beta_1 \ln(\text{min}_{st}) + \beta_2 \ln(\text{min}_{s\,t-1}) + \beta_3(\text{proportion female}_{st}) \\
& + \beta_4(\text{proportion nonwhite}_{st}) + \beta_5(\text{average experience}_{st}) \\
& + \beta_6(\text{average experience}_{st}{}^2) + \beta_7(\text{proportion hsgrad}_{st}) \\
& + \beta_8(\text{proportion some college}_{st}) + \beta_9(\text{proportion collgrad}_{st}) \\
& + \beta_{10}(\text{proportion union}_{ist}) + \beta_{11}(\text{proportion FT status}_{ist}) \\
& + \sum_I \beta_I(\text{proportion in industry}_{st}) \\
& + \sum_O \beta_O(\text{proportion in occupation}_{st}) \\
& + \sum_Y \beta_Y(\text{Year}_t) + \varepsilon_{st}
\end{aligned}$$

To reflect the demographic characteristics of workers earning wages at a particular wage percentile, the proportions are estimated for regions of the wage distribution centered on each wage percentile. For example, for the analysis of the 10th wage percentile, the "proportion female" variable is estimated from the sub-sample of workers earning between each state's 5th and 15th wage percentiles. In this way, the changes in demographic characteristics that are observed with this "proportion female" variable are specific to the wage percentile being analyzed.

Given the monthly administration of the CPS survey, state-level observations can potentially be constructed as frequently as monthly. The monthly sample sizes, by state, range from 86 to 1,405 observations. As such, in order to reliably estimate the demographic characteristics in the manner described above, monthly state samples were pooled. In order to insure that all states were retained for the analysis and that pooled monthly samples used for the proportion measures have 30 individual observations at minimum, six months of data were pooled. More specifically, the demographic variables were estimated each year on data from January to June and on data from July to December for each state. To make the other variables consistent with the time frame of the demographic variables, the minimum wage and wage percentile variables were averaged over the corresponding six months. In the end, 2,000 state-level observations were created (2 x 20 years x 50 states). To control for possible seasonality effects a dummy variable "Half1" was added to the model where Half1=1 if the observation is taken from the months of January to June and 0 otherwise.

Aggregating individuals observations into state units effectively transforms the collection of monthly CPS data sets from a data set of pooled repeated cross-sections into a panel data set. Each of the 50 states generates a panel with observations spanning over 20 years.[18] Because of the relatively large number of time periods (40), relative to the number of panels (50), this panel data set is more appropriately thought of as a multiple time series data set (see Wooldridge 2002, 175-176). In such cases, where the number of time periods is of the same order as the number of panels, issues of temporal persistence (e.g., autocorrelation and non-stationarity) have to be carefully considered.

To investigate whether there are any series in the model that are non-stationary Im-Pesaran-Shin (IPS) panel unit root tests were performed on each variable.[19] For all variables, with the exception of the minimum wage variables, the null hypothesis of non-stationarity of the IPS test was rejected at conventional levels of statistical significance. The IPS tests for the minimum wage variable,[20] on the other hand, indicate that non-stationarity cannot be rejected (see top panel of table 2.3). The nonstationarity of the minimum wage variable is expected given that, in nominal terms, the minimum wage tends to rise in discrete steps (exceptions to this include the recent changes in Florida, Washington and Oregon state minimum wage laws which index their minimum wage levels to a measure of the price level). Therefore, the source of nonstationarity is a series of structural breaks as opposed to a unit root process. Still, because the minimum wage variable jumps upward over time, not due to a deterministic trend, it is clearly behaves as a nonstationary series and thus may produce spurious results (Enders 1995). To address this issue, I chose the usual approach of first-differencing to remove non-stationarity from the minimum wage variable. Thus, equation 2.7 is transformed into:

$$
\begin{aligned}
(2.8)\ \Delta\ln(\text{wage percentile}_{st}) = {} & \alpha + \beta_1\Delta\ln(\text{min}_{st}) + \beta_2\Delta\ln(\text{min}_{s\ t-1}) \\
& + \beta_3(\Delta\text{proportion female}_{st}) + \beta_4(\Delta\text{proportion nonwhite}_{st}) \\
& + \beta_5(\Delta\text{average experience}_{st}) + \beta_6(\Delta\text{average experience}_{st}^{2}) \\
& + \beta_7(\Delta\text{proportion hsgrad}_{st}) + \beta_8(\Delta\text{proportion some college}_{st}) \\
& + \beta_9(\Delta\text{proportion collgrad}_{st}) + \beta_{10}(\Delta\text{proportion union}_{ist}) \\
& + \beta_{11}(\Delta\text{proportion FT status}_{ist}) + \beta_{12}(\text{Half1}_{t}) \\
& + \sum_{I} \beta_I(\Delta\text{proportion in industry}_{st}) + \sum_{O} \beta_O(\Delta\text{proportion in occupation}_{st}) \\
& + \sum_{Y} \beta_Y(\text{Year}_t) + \varepsilon_{st}
\end{aligned}
$$

The IPS tests were performed on the transformed contemporaneous minimum wage variable. The results are presented in the bottom panel of table 2.3. The t test statistics now exceed (in absolute value) the critical value of -2.32. The null hypothesis of non-stationarity can be rejected.

Equation 2.8 is the basic model used in the minimum wage analysis. Two other aspects of this specification motivate this final form. First, note that first-differencing each variable has the appealing result of removing the biasing effect of any time-invariant omitted variables (Wooldridge 2002). A potential source of such bias may be some unobserved quality of a state's labor market environment that may be correlated with the value at which the state sets its minimum wage. Omitting such a variable would produce biased estimates of the effect of the minimum wage.

Second, the biennial character of the data allows the specification of equation 2.8 to include year dummy variables in the regression analysis without eliminating the minimum wage variation that arises from federal minimum wage changes. Past research using annual data has been criticized by Burkhauser, Couch, and Wittenberg (2000) for including such year dummy variables to control for macroeconomic conditions. As they note, if much of the variation in the minimum wage is not allowed to identify the effect of the minimum wage the regression may not be able to produce precise results (i.e., large standard errors). Also, if much of the movement in the minimum wage is due to the federal minimum wage and this variation is captured by year dummy variables then the estimated minimum wage effect may not provide a result that can be generalized. In other words, the estimated effect may be specific to the subset of states that raise their state minimums above the federal level. To see this, it is useful specifically identify the source of variation in the minimum wage that allows the regression equation to identify the effect of the minimum wage. Examine the grid of prevailing minimum wage changes, based on annual averages, provided in table 2.4 for the years 1983 to 2001[21].

In the grid, the columns are years and the rows are states, and each entry of "X" represents an increase in the prevailing minimum wage due to a change in the federal or state minimum wage. As is evident in this grid there are two main sources of variation. One is across states at a given point in time. Only a subset of states experience increases in their state minimum wages that exceed the federal minimum wage, therefore variation in the minimum wage at any given point in time will come from this subset of states. These states are primarily located in the Northeast and the West, however a handful of states are outside these two regions, including Delaware, Minnesota, New Jersey, North Dakota, Pennsylvania and Wisconsin. Note also that, as mentioned above, the years that these states experience state minimum wage changes are clustered around the late 1980s and the late 1990s to early 2000s. As a result, this cross-state variation is limited to certain years and certain states. This feature of the minimum wage change variable will become important when examining the potential for generating spurious results.

The other main source of variation is within states across time. All states experience some combination of federal and/or state minimum wage changes during the time period under analysis. However, the majority of states experience minimum wage changes at the same time: when the federal minimum wage changes. As a result, including year dummy variables, as done in equation 2.8, may at first appear to subsume this variation in the minimum wage variable since any changes in the prevailing minimum wage that coincides with a change in year will be captured by the corresponding year dummy variable. Biennial observations avoid this problem. To see this, consider the minimum wage changes experienced by states that do not have state minimum wages that exceed the federal minimum during the years of 1983-2001.[22] These changes are presented in table 2.5. In column 2 each federal minimum wage change is listed, along with the date of its implementation. The 6-month average federal minimum wage is presented in column 4 with the 6-month interval specified in column 3. The values for the minimum wage change variable over one year are presented in the last column. The entries in the last column show how the biennial nature of the data allows the minimum wage change variable to vary within years so that year dummy variables do not subsume all the minimum wage variation from the federal minimum wage change.

In the remaining sections I will discuss two refinements to equation 2.8. Because of the important role that the presence of low-wage workers has in inducing a minimum wage ripple effect—either by establishing a clearly-defined wage norm or by providing a gauge of how wage differentials change when the minimum wage changes—estimating the basic model (equation 2.8) for the whole economy may not characterize well the minimum wage effect for sub-sectors of the economy. Particular sub-sectors (e.g., wage contours, industries, geographic regions) with low numbers of minimum wage workers may dilute the estimated impact experienced by sub-sectors with high concentrations of minimum wage workers. If this is the case, the average effect estimated by equation 2.8 may be accurate but not very informative. The following alternative specification is estimated to address this.

Another measure is added to equation 2.8 to take into account how such differences across sub-sectors may affect the extent and magnitude of the minimum wage ripple effect. This variable is the proportion of workers (within state and averaged over six months, as with other variables) who earn wages between 100 percent-120 percent of the prevailing minimum wage: that is, the proportion of workers who either earn the minimum wage or sit directly above it. This variable, "proportion of directly affected workers" or PROPDAW, roughly measures the degree to which a given minimum wage change is likely to "bite" (i.e., directly affect a state's wage distribution) given that minimum wage changes are always less than 27 percent during the time period covered in this analysis. The proportion of directly affected workers is interacted with (i.e., multiplied by) the minimum wage variables to capture how the impact of the minimum wage change may be mediated by the presence of minimum wage workers in each state's economy. That is, the impact of a change in the minimum wage is allowed to vary depending on the proportion of directly affected workers. This model is presented below:

$$
\begin{aligned}
(2.9)\ \Delta \ln(\text{wage percentile}_{st}) = {} & \alpha + \beta_1 \Delta \ln(\text{min}_{st}) + \beta_2 \Delta \ln(\text{min}_{s\,t-1}) \\
& + \beta_3\, [\Delta \ln(\text{min}_{st}) \times (\text{PROPDAW}_{st})] + \beta_4\, [\Delta \ln(\text{min}_{s\,t-1}) \times (\text{PROPDAW}_{st})] \\
& + \beta_5(\text{PROPDAW}_{st}) \\
& + \beta_6(\Delta \text{proportion female}_{st}) + \beta_7(\Delta \text{proportion nonwhite}_{st}) \\
& + \beta_8(\Delta \text{average experience}_{st}) + \beta_9(\Delta \text{average experience}_{st}^{2}) \\
& + \beta_{10}(\Delta \text{proportion hsgrad}_{st}) + \beta_{11}(\Delta \text{proportion some college}_{st}) \\
& + \beta_{12}(\Delta \text{proportion collgrad}_{st}) + \beta_{13}(\Delta \text{proportion union}_{st}) \\
& + \beta_{14}(\Delta \text{proportion FT status}_{st}) + \beta_{15}(\text{Half1}_{t}) \\
& + \sum_{I} \beta_I (\Delta \text{proportion in industry}_{st}) + \sum_{O} \beta_O (\Delta \text{proportion in occupation}_{st}) \\
& + \sum_{Y} \beta_Y (\text{Year}_t) + \varepsilon_{st}
\end{aligned}
$$

Note that the proportion of directly affected workers is not first-differenced. This is because the measure is, in effect, differentiating states on the basis of the "degree of treatment" experienced by each state at a given point in time. With such variables, the "degree of treatment" value is always 0 at time 1, and thus, simply the level value in time 2.

I also estimate these specifications for the retail trade industry alone. That is, all the variables in equations 2.8 and 2.9 are derived from CPS individual-level data sets that only include workers who report working in retail (CPS major industry code 10). The retail trade industry is singled out because it consistently has the highest concentration of minimum wage workers. In fact, across the years of 1983 to 2002, 50 percent of minimum wage workers are in the retail trade industry (see table 2.6). Nondurable manufacturing and education services are tied for the next highest concentration of minimum wage workers: each has 6.8 percent of all minimum wage workers. Minimum wage workers, then, are overwhelmingly concentrated in the retail trade industry.

The prominence of minimum wage workers in this industry provides an indication of how important the minimum wage is in forming the retail trade wage structure. How this affects the magnitude and extent of the ripple effect is unclear. On the one hand, minimum wage effects should be large and extensive in the retail trade industry if employers react by substituting away from their usual labor sources toward workers who typically earn higher wages or if workers are able to effectively maintain their wage differentials. On the other hand, the concentration of minimum wage workers may indicate that workers are not able to effectively negotiate their wages, and thus are also unable to effectively maintain their wage differentials, and thus minimum wage effects may be limited to mandated wage raises.

One modification had to be made to the basic methodology described above. For this part of the analysis, the demographic variables are not estimated from segments of each state's wage distribution. Instead, these variables are estimated from each state's entire wage distribution of workers in the retail trade industry. Small sample sizes prohibited estimating the demographic variables from segments of each state's retail trade industry wage distribution. Additionally, because of the smaller samples sizes available for the retail trade sector, the 5th wage percentile cannot be reliably estimated, and therefore the regression estimates start at the 10th wage percentile.

2.4 Results

2.4.1 Analysis of the Total Economy

Before presenting the regression estimates of equations 2.8 and 2.9, descriptive statistics are presented in table 2.7. These descriptive statistics provide demographic profiles of the workforce at different points in the wage distribution. In order to facilitate a comparison between each region of the wage distribution and the overall sample, characteristics for the total workforce are presented in the bottom row.[23] Female and nonwhite workers are overrepresented in the bottom half of the wage distribution. Female workers make up roughly 60 percent of the bottom quarter of wage workers. Workers in the bottom half of the wage distribution are also disproportionately nonwhite, part-time, and nonunion. These workers also tend to have less work experience and are less likely to have a bachelor's degree relative to the total sample. Each of these traits generally increase or decrease monotonically in the expected direction as wages rise. The minimum wage typically intersects the wage distribution at the 5th wage percentile, as the average ratio between the 5th wage percentile and the prevailing minimum wage is equal to 1.00. Workers at the other end of the wage distribution (the 70th wage percentile and up) typically earn at least 300 percent of the minimum wage.

As reported above, the retail trade industry (see table 2.8) is by far the largest employer of workers at the low end of the wage distribution. Among workers earning below the 10th wage percentile, over 40 percent are employed in the retail trade industry. The private household services industry has the next highest percentage of workers at 7 percent, followed by education and personal services (excluding private household services) at 7 percent and 5 percent respectively. These low-wage workers are overwhelmingly and disproportionately concentrated in the service sector of the economy. This fact is also made clear by the occupational distribution of workers by wage percentile presented in table 2.9. The majority of workers at the low end of the wage distribution are disproportionately concentrated in service and sales occupations. In contrast, workers at the higher wage percentiles are more dispersed across the industries with the highest concentrations in durable manufacturing and education, at 13 percent and 11 percent, respectively. Interestingly, these high-wage workers (workers around the 70th wage percentile and up) are significantly underrepresented in the industries that low-wage workers are overrepresented in, with the exception of education. The majority of the high-wage workers are concentrated in the executive, administrative and managerial occupations, professional specialty occupations, and precision production, craft and repair occupations.

I turn now to the wage effects of the minimum wage estimated for each of the thirteen wage percentiles. I will first provide an overview of the contours of the minimum wage ripple effect and then examine the magnitudes of the effect more carefully. Coefficients of the minimum wage variables produced by regression estimates of equation 2.8 are presented in table 2.10 and figure 2.4.[24] Overall, the results are consistent with past research: the effect of the minimum wage is greatest on wages closest to the minimum. The ripple effect is also limited in its extent: these results indicate that the ripple effect is limited primarily to those earning wages around the 15th wage percentile and below. The effect diminishes quickly as wages rise. As shown in table 2.7, the 15th wage percentile is typically 23 percent greater than the prevailing minimum wage and the 20th wage percentile is typically 35 percent greater than the prevailing minimum wage. Therefore, these results provide evidence of a ripple effect up to 135 percent of the prevailing minimum wage at the time of the minimum wage increase. To put this into context, on average, workers who are currently bound by the federal minimum wage ($5.15) and earn less than $6.95 can expect to experience a wage increase if the federal minimum wage increases. Regression estimates for higher wage percentiles produce minimum wage effects that are too small to distinguish from zero given the estimated standard errors.

Contrary to expectations, there appears to be a lagged minimum wage effect on the highest wage percentiles (70th to 90th). Although the coefficients are modest in size—none surpass 0.10—they are statistically significant. Given that the wages in this part of the wage distribution are quite high relative to the minimum (on average, greater than three times the minimum wage) and that these workers are also typically concentrated in the industries that minimum wage workers are not, the meaning of this anomalous result is unclear.

A spurious correlation caused by the timing of the minimum wage increases may explain these incongruous results that indicate a minimum wage effect at the top of the wage distribution. As discussed above, many minimum wage changes occur near or at the peak of the two business cycles included in this analysis and many of the state minimum wage changes that did occur are clustered among a small set of states. To examine whether these results may be reflecting the effects of a concurrent macroeconomic trend, the model is estimated using the following three samples: 1) a sample excluding states in the New England region,[25] 2) a sample excluding observations from the 1980s, and 3) a sample including observations only from the 1980s. The rationale behind using these modified samples is to observe to what degree the original estimates may be due to the particular set of state and year combinations that are included in the analysis. Estimating the minimum wage effect without the presence of the New England states may remove the observations that potentially spuriously correlate increases in the minimum wage with wage increases due to fluctuations of the business cycle. The New England states are singled out because their state minimum wage increases coincide closely with the business cycle peaks (see table 2.4). Note that immediately preceding the 1990 business cycle peak, all six states of the region increased their state minimum wages at least twice. In the years immediately preceding the 2001 business cycle peak, four of the six raised their state minimums. A sample excluding the 1980s is used because it is during the 1980s, in particular, that state and federal minimum wage changes are clustered around the peak of a business cycle. In contrast, the state and federal minimum wage increases during the 1990s and later occurred at various points in the business cycle: The 1990 federal minimum wage increase took place roughly at the peak of the business cycle, the 1991 federal minimum wage increase took place during a recession, the following federal minimum wage changes in 1996 and 1997 took place during the upswing of a business cycle but not at its peak, and various state minimum wage changes in the late 1990s and early 2000s are spread across the business cycle peak of March 2001 and the recession of November 2001. Given these patterns, problems with spurious correlation are likely to be exacerbated when estimates are generated using the 1980s only sample and improved when estimates are generated using the sample excluding the 1980s.

Figure 2.5 presents the minimum wage coefficients estimated from these three different samples. Excluding the New England states (panel A) or excluding the 1980s (panel B) has the effect of reducing the magnitude of the minimum wage effect across all the wage percentiles, indicating that wage growth across the wage distribution was stronger for New England states, in particular, and for all states in the 1980s when minimum wages changed. Assuming that the minimum wage has a differential impact across the wage percentiles, the fact that the coefficients were reduced across the wage distribution suggests that a portion of the minimum wage estimates reflect wage growth due to factors other than the minimum wage. Note that the coefficients for the top wage percentiles are closer to zero when the New England states are excluded; also, these coefficients are no longer statistically significant at conventional levels. The minimum wage effect on the 70th and 80th wage percentiles is likewise reduced and no longer statistically different from zero. The coefficients based on the sample excluding the 1980s are also substantially reduced for each of the 70th, 80th and 90th wage percentiles. Coefficients on these wage percentiles are also no longer statistically significant, with the exception of the 90th percentile.

Finally, estimates based on the 1980s only are presented in the bottom panel of figure 2.5. Now, the minimum wage appears to have a large, positive effect on all wages with a particularly strong contemporaneous effect at the 90th wage percentile. In fact, the combined minimum wage effect at the 90th wage percentile appears to be larger than the minimum wage effect at the 15th wage percentile. In any case, the whole wage distribution appears to be moved by a minimum wage change. Given that this extensive effect is only produced when examining the years that are most likely to produce spurious results, the soundness of these estimates is questionable. These implausible results also provide evidence that it is difficult for regression analysis to parse out effects of the minimum wage from other economic trends taking place in the economy given the pattern of minimum wage changes. One could, of course, argue that the minimum wage has a particularly strong effect in the 1980s *because* the minimum wage changes took place at the peak of the business cycle. This may not be definitively contradicted given the simultaneity of minimum wage changes and business cycle peaks. However, given that the extent of the minimum wage effect appears to be limited to the lower 15 percentiles or so when these years are excluded (or when just the New England states are excluded) this argument would require an implausibly large increase in the minimum wage effect during the mid-to-late 1980s.

I will now examine more closely the results from the sample excluding the New England states. Note that the coefficients based on the sample excluding the New England states and the coefficients based on the sample excluding the 1980s are substantively the same: they follow the same pattern, they are similarly smaller in magnitude relative to the coefficients based on the total sample, and they are similar to each other in magnitude. The wage elasticity of 0.47 (contemporaneous and lagged effects combined) for the 5th wage percentile indicates that a 10 percent increase in the minimum wage is associated with a 4.7 percent increase in the 5th wage percentile. One might have expected the wage elasticity at the 5th wage percentile to be equal to 1.00 given that the average value of the 5th wage percentile coincides closely with the minimum. However, since the coefficient is the effect of a 10 percent increase in the minimum wage, *on average*, this coefficient will reflect the changes in 5th wage percentiles that cover a range around the minimum wage. Note that if the 5th percentile is already 5 percent greater than the minimum wage, the expected increase in the 5th percentile would be only an additional (roughly) 5 percent when the minimum wage increases by 10 percent. In this case, the elasticity would be 0.50, not 1.00. To the degree that 5th percentiles are slightly above the minimum wage, the elasticity will be diminished. Likewise, if the 5th wage percentile is substantially less than the minimum wage then the 5th percentile may be gauging the wage changes of workers that are not covered by the minimum wage laws, earn a portion of their wages through tips (and may have an unchanged lower applicable minimum) or are employed by noncompliant employers.[26] In such cases, the change in the 5th percentile may be substantially lower than the change in the minimum wage. The impact of these observations will also tend to diminish the average measure of the 5th percentile wage elasticity. As discussed below, controlling for the range of the wage percentiles with respect to their proximity to the minimum will provide evidence of such variation in the wage elasticities for wages around the minimum wage. In any case, the magnitude of the minimum wage effect is substantively large at the 5th wage percentile and is larger than that of all the other wage percentiles, as would be expected given that it is closest in proximity to the minimum wage.

The next wage elasticity is estimated for workers who are typically just above the minimum wage: 11 percent above the minimum on average. Workers at this point in the wage distribution receive, on average, a 2.8 percent raise when the minimum wage rises by 10 percent. The proximity of the 10th wage percentile to the minimum wage suggests that raises associated with minimum wage changes are largely comprised of ripple wage raises. For example, consider the federal minimum wage increase in 1996: from $4.25 to $4.75, a 12 percent increase. In 1996, the 10th wage percentile was, on average, 13.8 percent above the minimum wage. Applying the estimated wage elasticity of 0.28, a 12 percent minimum wage increase would result in the 10th wage percentile rising 3.3 percent. This 3.3 percent increase represents non-mandated wage increases, given that the 10th wage percentile is already above the new federal minimum wage level. However, note that the 10th wage percentile will be situated considerably closer to the 5th wage percentile and the minimum wage after the minimum wage increase: no longer 13.8 percent above the minimum, the 3.3 percent increase puts the 10th wage percentile at 6 percent above the minimum ([1.135*1.033]-1.12=0.06). The 15th wage elasticity drops to 0.18. Repeating the same exercise using a 12 percent increase in the federal minimum, this wage elasticity suggests that the 15th wage percentile would rise 2.2 percent, moving the 15th wage percentile from 23 percent above the minimum to 14 percent above the minimum ([1.23*1.022]-1.12=.14). These calculations illustrate a minimum wage effect that, because of its limited magnitude and extent, results in significant compression of the left-tail of the wage distribution. On the other hand, even though the ripple effect wage increases are not large enough or extensive enough to realign the wage structure after a minimum wage increase, they do provide economically meaningful raises for the workers who receive them.

I now turn to equation 2.9 which introduces the variable “Proportion of Directly Affected Workers” or PROPDAW to refine the minimum wage measure. This model was estimated using the sample excluding New England states to diminish the potential for obtaining spurious results. The regression coefficients are presented in table 2.11 and the wage elasticities are displayed in figure 2.6. In this figure, two sets of wage elasticities are presented. Wage elasticities associated with low concentrations of near minimum wage workers (top panel) and wage elasticities associated with high concentrations of near minimum wage workers (bottom panel). As discussed earlier, PROPDAW is introduced as a mediating factor of the minimum wage effect to allow the regression to differentiate between states that are more and less bound by their minimum wage. Or, put another way, the model allows the minimum wage effect to vary according to the concentration of low-wage workers in a particular state. Large concentrations of low-wage workers may increase the prominence of the minimum wage as a reference point for workers, more so than when there are few low-wage workers in a particular state at a particular point time. This may increase the size of the wage elasticity or extend the effect of the minimum wage to higher wage percentiles. Alternatively, large concentrations may cause employers to provide less extensive or smaller nonmandated raises because of the greater cost associated with such raises when relatively many workers sit just above the minimum wage. Note that observing how minimum wage increases may vary depending on its "bite" provides some insight into how ripple effects of living wage laws may operate since these laws typically enact substantially higher mandated wage floors, or minimum wages with a large "bite."

The wage elasticities presented in figure 2.6 indicate that the former case applies. That is, the wage elasticities increase with relatively large concentrations of near minimum wage workers. However, the extent of the minimum wage effect is not much altered when there is a relatively high concentration of workers – a statistically significant effect extends only to the 20th percentile. The magnitude of the wage elasticities do not vary dramatically when considering a wage distribution with relatively few low-wage workers versus considering a wage distribution with relatively many low-wage workers. While the magnitudes of the wage elasticities are higher at each wage percentile when there are relatively many low-wage workers, this likely reflects the fact that the interaction terms of equation 2.9 effectively controls for the range in the relative position of the wage percentiles with respect to the minimum. For example, the greater the density of a state's wage distribution near the minimum wage (the greater the "bite" of the minimum wage), the closer the 5th wage percentile is to the minimum wage. Therefore, the minimum wage will increase wages at the 5th percentile of states which have a large concentration of workers earning wages near the minimum more than in states which have a small concentration of workers earning wages near the minimum. On the whole, it appears that the minimum wage does not have a noticeably different affect on states with large concentrations versus states with low concentrations of low-wage workers.

To illustrate this point further, consider the wage effects caused by the 1996 federal minimum wage increase predicted by the estimates in Table 2.11 for two states: Michigan (a relatively high-wage state) and Mississippi (a relatively low-wage state). Both states' prevailing minimum wage is equivalent to the federal minimum wage throughout the time period studied. In the latter half of 1995 (July to December), the proportion of workers between the minimum wage and 120 percent of the minimum wage was 8.2 percent in Michigan and 16.1 percent in Mississippi. Applying the regression results, the 5th wage percentile in Michigan is estimated to rise by 4.7 percent and the 5th wage percentile in Mississippi is expected to rise 7.2 percent (given the 12 percent increase in their prevailing minimums).[27] The 10th wage percentile is expected to rise 2.5 percent and 5.0 percent for Michigan and Mississippi, respectively.[28] In this example, it appears that Mississippi's 10th wage percentile behaves more like Michigan's 5th wage percentile because its wage distribution sits more closely to its prevailing minimum wage. These calculations simply demonstrate that the position of the wage distributions of different states, relative to the minimum, varies significantly enough to measurably affect the magnitude of the minimum wage effect at the lower percentiles. Therefore, the results from the model characterized by equation 2.9 improve on those of equation 2.8 by allowing for a more nuanced measure. However, these results do not indicate that there is a qualitatively different minimum wage ripple effect across states with large and small concentrations of low-wage workers.

The one exception to this pattern occurs at the 90th wage percentile. A statistically significant, positive lagged effect for the 90th wage percentile appears for states with a high value of PROPDAW. This result is difficult to interpret because the effect is isolated at the far end of the wage distribution (wages at this percentile are, on average, 450 percent of the minimum wage), is not part of a discernible pattern of effects, and is of a relatively small magnitude (although broadly consistent with the findings of Neumark, Schweitzer, and Wascher (2004). Also, although the semi-parametric approach of estimating the same wage equation for wage percentiles across the wage distribution has the appealing quality of estimating in a detailed way the impact of minimum wage increases, it has the drawback of estimating a "one-size-fits-all" wage equation. The highest wages are likely to be governed by somewhat different influences than the lowest wages, so that the wage equation will do a better job at explaining wages at one end or the other. Given that the wage equation has smaller R^2 statistics for the higher wage percentiles relative to other wage percentiles, it appears that the wage equation used is better able to explain movements in lower wage percentiles and thus raises the concern discussed above—that estimates for the higher wage percentiles may be more vulnerable to capturing spurious relationships. Therefore, drawing conclusions from this positive, lagged effect at the top of the wage distribution is problematic.

In sum, the ripple effect of the minimum wage appears to be limited to those workers earning wages around the 15th wage percentile and below (workers below, roughly, 135 percent of the minimum wage). This may vary slightly depending on how closely the particular wage distribution sits relative to the minimum wage. This ripple effect diminishes quickly; workers above the 15th wage percentile experience little or no ripple effect.

2.4.2 Analysis of the Retail Trade Industry

The overarching question in examining this sector separately is whether there is a change in the quality of the minimum wage effect when looking at this sub-sector of the economy where roughly 50 percent of minimum wage workers are employed. To answer this question, equations 2.8 and 2.9 were estimated based on observations from workers in the retail trade industry only. Before presenting the regression results, demographic profiles of the retail trade industry are provided in table 2.12. Because of sample size limitations, these characteristics are not presented by wage percentile.

The demographic profile of the retail trade sector resembles closely that of the lower half of the wage distribution of the whole economy. This aspect of the retail trade industry is reflected in its occupational composition. The large majority (73 percent) of workers in the retail trade industry are in occupations that are associated with low wages: sales, service (except protective and household), and handlers, equipment cleaners, helpers, laborers. Compared to the workforce across industries (see table 2.7), the retail sector has a higher concentration of female workers (54 percent versus 48 percent), lower average years of potential labor force experience (13.8 versus 18.1), a smaller proportion of college graduates (10.4 percent versus 24 percent), union members (5.1 percent versus 14.4 percent), and full-time workers (59.3 percent versus 75.9 percent). Only with respect to the proportion of nonwhite workers is the retail trade industry similar to the workforce across industries (both at 19 percent). This implies that nonwhite, low-wage workers are more concentrated in industries outside retail since the vast majority of retail trade workers earn wages below the 50th percentile of the entire employed workforce and the proportion of nonwhite workers is greater than 19 percent among such workers.

The 50th wage percentile of the retail trade industry is, on average, 38 percent greater than the prevailing minimum wage. This approximately coincides with relative wage position of the 20th wage percentile of the entire workforce. If the extent of the minimum wage ripple effect is similar to what is observed in the entire economy, the minimum wage effect should not extend past the 50th wage percentile of the retail trade industry.

I present the regression results for equation 2.8 based on retail trade workers only in table 2.13 and the wage elasticities in figure 2.7. As with the previous analysis, I present the results for the sample excluding New England states.

The regression results for the retail industry are consistent with those of the total economy, rather than producing qualitatively different results. First, the minimum wage effect extends to the 40th wage percentile, but not beyond the 50th percentile. As with the results based on workers across industries, the minimum wage effect is limited to wages below, roughly, 135 percent of the minimum wage. The magnitudes of the wage elasticities are also largely consistent with the previous results. For example, for workers earning approximately 125 percent of the minimum wage, the wage elasticity is 0.2 regardless of whether one is looking exclusively at the retail trade industry or across industries. While this result may seem unsurprising because so many low-wage workers are employed in the retail trade sector, this consistency in results indicates that the *concentration* of directly affected workers is not a significant factor in determining the extent and magnitude of minimum wage ripple effects.

Instead, the large concentration of low-wage workers in the retail trade industry allows the wage percentiles used in these regressions to provide greater detail on the minimum wage effect. For example, the as shown in Table 2.12, the 10th wage percentile of the retail trade industry is, on average, 99 percent of the minimum wage. The wage elasticity for the 10th wage percentile is roughly 0.70, significantly greater than the wage elasticity (0.47) found for the 5th wage percentile using the sample of workers across industries which is, on average, 100 percent of the minimum wage. This appears to be due to the fact that when the sample is restricted to the retail industry alone, the range of each particular wage percentile's relative position to the minimum narrows. In effect, the wage elasticity estimated for the 5th wage percentile of the workers across industries is a combination of the effects observed for the 10th, 15th and 20th wage percentiles of the retail trade industry. As suggested above, controlling the range of the wage percentiles with respect to their proximity to the minimum parses out some of the variation in the wage elasticities for wages around the minimum wage.

Regression results for equation 2.9 are presented in table 2.14 and figure 2.8. As before, the coefficients on the contemporaneous minimum wage variable and/or the interaction term with PROPDAW are consistently positive and statistically significant up to the 40th wage percentile. The coefficients on the lagged minimum wage variable and PROPDAW are not statistically significant at the conventional levels, with the exception of the coefficients for the 20th wage percentile, and vary in sign. This negative effect around the 20th wage percentile may result from the fact that the greater the density of the wage distribution at that point (just above the minimum wage, or 107 percent of the minimum, on average) may dampen the minimum wage effect simply because many more workers' wages need to be affected in order to move the 20th wage percentile. If the lagged positive minimum wage effect is already small, then the large concentration of workers just above the minimum that tends to form just after the minimum wage has increased will attenuate the movement of the wage percentiles and may result in slower wage growth for those wage percentiles.

The wage elasticities estimated for the 50th wage percentile and higher are consistently small in magnitude and are not statistically significant. The magnitude of the contemporaneous minimum wage effect, in particular, drops sharply after the 40th wage percentile. A distinctly different pattern of minimum wage effects appears at the top end of the wage distribution (the 90th wage percentile). The negative contemporaneous minimum wage effect followed by a positive lagged minimum wage effect appears to suggest that the top wage earners in the retail trade industry at first experience a decline in their wage growth when the minimum wage increases, but then largely make up this loss in the following year. This may be evidence that employers adjust their wage bill by reducing the wage growth of workers at the high end to accommodate greater wage growth of workers at the low end. However, given that these results are not statistically significant and the magnitudes of the effects are relatively small, such evidence is not conclusive of a more extensive minimum wage ripple effect in the retail trade industry.

The results from the retail trade industry reinforce the earlier findings. The positive wage effect of minimum wages extends to workers earning below 135 percent of the minimum wage. This ripple effect diminishes quickly: in the retail industry the wage elasticity for workers earning 125 percent of the minimum wage is 0.2, less than one-third the size of that measured for workers most likely to received mandated wage increases (the 10th wage percentile). Estimates based on workers across industries produce the same wage elasticity for workers at 125 percent of the minimum wage and also indicate that the minimum wage effect dissipates for workers at higher wage levels.

2.5 Discussion

To understand the economic significance of the estimated minimum wage effects, I provide several contexts for assessing them. First, I calculate a minimum wage multiplier that provides a summary measure of the magnitude of the added wage effect introduced by the ripple effect. Second, I explore whether considering workers who receive nonmandated raises via the ripple effect as part of the beneficiaries of minimum wage laws changes the target efficiency of minimum wage laws. Third, I place the impact of the minimum wage ripple effect in the context of overall wage trends to understand better how the minimum wage influences the shape of the wage distribution over time.

2.5.1 Estimating the Ripple Effect Multiplier

The above discussion makes clear that mandated raises alone do not sufficiently capture the total change in the wage bill due to minimum wage increases. To gauge the importance of ripple effects as a consequence of minimum wage increases, I calculate a summary measure of its impact on the wage bill; I estimate a ripple effect multiplier. The multiplier is, in this case, the factor by which the wage bill is increased over and above mandated wage increases.

To approximate the ripple effect multiplier I construct estimates of the mandated and ripple effect raises that take place over the federal minimum wage increases in 1990, 1991, 1996, and 1997. I first use the regression results to estimate the increase in the mean wage of each affected wage percentile due to mandated effects (the wage increases required to bring wages up to the new minimum) and the increase in the mean wage due to ripple effects (the wage increases that bring wages above the minimum).[29] I then multiply these wage increases by the average number of hours worked per week (mean hours multiplied by number of workers) for each wage group to calculate the change in the wage bill. The multiplier is the ratio of the total increase in the wage bill to the mandated increase. For the April 1990 federal minimum wage increase, I examine the change in wage percentiles from the second half (July to December) of 1989 to the second half of 1990. For the April 1991 federal minimum wage increase, I examine the change in wage percentiles from the second half of 1990 to the second half of 1991. For the October 1996 federal minimum wage increase, I examine the change in wage percentiles from the first half (January to June) of 1996 to the first half of 1997. For the September 1997 federal minimum wage increase, I examine the change in wage percentiles from the first half of 1997 to the first half of 1998. Table 2.15 presents the figures used for this calculation.

The estimates of the minimum wage multiplier range from 2.4 to 2.9. That is, ripple effects add an additional 140 percent to 190 percent over and above the mandated wage effects of a minimum wage increase. This range of the estimated multiplier indicates that the ripple effect, in overall dollar value, is greater than that of the mandated wage increases even though the per hour increase due to the ripple effect is smaller. This results from the greater density of the wage distribution immediately above the minimum as compared to just at or below the minimum. As a consequence, even though the ripple effects calculated in table 2.15 are expected to be limited to wages within a dollar and a quarter of the minimum wage prior to the increase, these raises multiplied by the number of worker-hours adds a significant amount to the wage bill.

Note that the somewhat wide range in the multiplier (50 percentage points) is due to the multiplier estimated from the 1997 federal minimum wage increase. Excepting that, the multiplier ranges between 2.40 and 2.50. It is interesting to note why the 1997 federal minimum wage change generates such a large multiplier. The larger size in the 1997 multiplier is not due to a larger (level) change in the wage bill due to the ripple effect. Relative to the other years, the change in the 1997 wage bill is relatively modest (compare $29.9 million in 1997 to $34.8 million in 1991and $34.0 million in 1996), especially given their nominal values. Rather, the difference is due to the fact that the direct raises—the mandated wage increases—are relatively modest, because of the significantly smaller number of workers earning close to the minimum wage at that time. The late 1990s are widely noted as a period of historically low unemployment rates, causing even the low-wage labor market to tighten. As such, a relatively small number of workers are observed earning wages right around the minimum wage in the first half of 1997. This fact magnifies the multiplier effect by reducing the direct effect. Interestingly, my estimates of the ripple effect—excepting the 1997 estimate—are close to Gramlich's (1976) multiplier of 2.00, which was derived using a different methodology and different time period (1954-1975). Possible causes for the smaller ripple effect estimated by Gramlich is the lower coverage rates of the minimum wage laws during those years as well as a smaller concentration of workers at the bottom of the wage distribution.[30]

The variation presented above in the multiplier highlights how the point at which the wage floor enters the wage distribution influences the relative significance of ripple effects. This aspect is important to keep in mind when considering the potential ripple effect of other wage floors such as living wages. Wage elasticity estimates using equation 2.9 suggest that where minimum wage levels enter the wage distribution does not significantly change the extent or magnitude of ripple effects. As such, greater densities of workers near the minimum wage lower the relative size of ripple effect raises to mandated raises, as many more workers receive mandated raises when there is a high concentration of workers near the minimum versus when there is a low concentration of workers near the minimum. Extrapolating from these observations on the wage effects of minimum wage laws, these results suggest that living wage laws will have significantly smaller multipliers with regard to ripple effects. As noted above, living wage laws typically call for mandated wage floors that are 185 percent of the federal minimum wage, roughly equal to the 40th wage percentile (see table 2.5). This undoubtably indicates a marked increase in the proportion of workers receiving mandated raises relative to the average 11 percent of workers (see table 2.11) that are directly affected by state and federal minimum wage changes.

2.5.2 Evaluating the Impact of Ripple Effects on the Target Efficiency of Minimum Wage Laws

The multiplier indicates that there is a significant proportion of the value of the minimum wage effect that falls on those workers who earn wages above the minimum wage. More specifically, there is an economically significant positive wage effect for those workers earning wages up to, roughly, 135 percent of the minimum wage (prior to the minimum wage change). This is an important observation for the debate over the target efficiency of minimum wage laws.

Although the impact of minimum wage increases is strongest for those who receive mandated wage raises, as noted in the previous section, more than half of the total value of the wage increase is obtained by those who receive ripple effect raises. As a result, a corollary effect of the ripple effect wage increases may be to change the composition of the beneficiaries of minimum wage increases.

A longstanding critique of using minimum wage laws to reduce poverty is that its benefits are not well targeted (e.g., Burkhauser and Finegan 1989). A primary source of this target inefficiency is associated with the presence of secondary earners (i.e., wage earners in the family that do not contribute a significant share to the family's overall income) among low-wage workers. As a result, the relationship between low-wages and poverty is argued to be weak. The assessment of other analyses focus on the larger presence of adults among minimum wage workers to argue that minimum wage laws are sufficiently well-targeted to redistribute income toward poor families (Mishel, Bernstein, and Boushey 2003). Both sides of the debate, however, agree that the significant proportion of teenagers and/or students (assumed to be secondary earners) among minimum wage workers is a primary source of minimum wage target inefficiency. Therefore, the assessments of how useful minimum wage laws are in reducing poverty has depended, in part, on the degree to which one weighs the merit of circumscribing benefits to low-income families against the merit of providing benefits disproportionately to low-income families. The presence of ripple effects adds to this debate by expanding the pool of beneficiaries of minimum wage increases, and potentially improving the target efficiency of minimum wage laws. In this section, I explore the target efficiency of the minimum wage and whether it improves or worsens when considering workers up to the 15th wage percentile.

Because the position of the wage distribution changes over time, relative to the minimum, I chose to examine workers in a year that is neither very close nor very distant from a federal minimum wage change. Because of the degree of compression that is suggested by the estimates discussed in the previous sections, examining workers from a year that immediately follows a national minimum wage change may qualitatively change the demographic profiles of workers at the lower wage percentiles to appear more like workers that are typically at higher wage percentiles, as the wage differences between the wage percentiles narrows. Conversely, examining workers from a year that is many years past a national minimum wage change may change the demographic profiles of low-wage workers to appear even more different from workers that are typically at higher wage percentiles, as the distance between the wage percentiles widens. I chose to focus on the year of 2000 which follows the September 1997 federal minimum wage increase by approximately three years.

To examine the demographic characteristics of workers at different points in the wage distribution, I use the 2000 March Annual Demographic Supplement to the CPS which includes information about family income, poverty status, as well as other family characteristics. I examine the demographic characteristics of workers earning wages within two wage intervals. The first wage interval is the federal minimum wage in March 2000 ($5.15) to $5.85, an interval evenly distributed across the 5th wage percentile in March 2000 ($5.50, averaged across states). Following the pattern of mandated and ripple effect raises calculated in table 2.15, I assume that these workers would primarily receive mandated raises if the federal minimum wage had increased by a typical amount in 2000. Two other categories of low-wage workers are examined: workers earning between $5.85 and $6.55, and workers earning between $6.55 and $7.15. These two categories are centered on the 10th wage percentile ($6.18) and 15th wage percentiles ($6.83), respectively. I present the demographic characteristics of workers earning wages within these wage intervals in Table 2.16.

Table 2.16 presents the demographic characteristics for the total sample as well as each subset of workers. As found in past research, teenagers and students make up a significant proportion of low-wage workers: Approximately 41 percent of workers earning near the 5th wage percentile are students between the ages of 16 and 24 and/or teenagers. This proportion is sizeable – over four times that found in the total sample. However, even at wages near the federal minimum in 2000, the majority of workers do not fit in the category of workers who are typically considered to be working for pocket money. This proportion declines substantially as wages rise: to 17 percent among workers earning wages between \$6.55 and \$7.15 per hour. The average age across these groups of workers reflect this trend. The average age rises from 29 years old in the lowest wage category to 35 years old in the highest wage category. Clearly, on the basis of age and student status, the composition of affected workers changes when one considers ripple effects as part of the impact of minimum wage laws.

As noted before, women and minorities are overrepresented among these low-wage categories, as well as those with only a high school degree.[31] These characteristics do not vary much across wage groups.

The poverty status of workers' families is indicated by two measures. The first is the percentage of workers who live in families with incomes below the federal poverty threshold.[32] The second is the percentage of workers who live in families within incomes below 200 percent of the federal poverty threshold. Both of these measures are provided because researchers of poverty in the United States widely view the federal poverty threshold as a poor measure of poverty-level income. In fact, the National Research Council's Panel on Poverty and Family Assistance produced a comprehensive critique (1995) detailing how the federal poverty measure is inadequate for the task of determining poverty-level incomes. On the basis of improved methods, researchers suggest that 124 percent to 153 percent of the federal poverty-level income threshold more accurately reflects the poverty status of families. Accordingly, the proportion of workers living in families with incomes below the federal poverty income thresholds should be viewed not only as the lower bound on the poverty rate measure, but also an indicator of the proportion of workers who live in families that are experiencing severe poverty. The proportion of workers living in families with incomes twice the federal poverty income thresholds should be viewed as a more inclusive measure, including families that are considered poor and/or low-income. Low-wage workers are disproportionately poor or low-income, at least twice the rate found in the total working population. Even so, a significant proportion of these workers are not poor according to these measures.

To get a clearer picture of what kind of income levels the remaining roughly 60 percent of nonpoor low-wage workers come from a third measure is provided: the percentage of workers whose family income falls below a "middle-class" income. This income threshold is 400 percent of the federal povery threshold and approximates the median family income, as demonstrated by the fact that 51 percent of all families (with at least one wage earner) have incomes below the middle-class threshold. Looking now at the three low-wage groups, the large majority of low-wage workers are from families with incomes below the middle-income threshold, ranging from 72 percent to 76 percent.

A comparison of the median and mean values of the remaining characteristics present important differences among workers within each wage category, causing the mean to vary from the median. The fact that the difference between the median and mean values of several of the characteristics is greatest in the lowest wage category and decreases as wages rise suggests that low-wage teenager/student workers are distinctly different from low-wage adult workers. For example, the mean value of a worker's contribution to family earnings, which provides an indicator of whether the worker is a primary or secondary earner, is 43 percent compared to its median value of 27 percent in the lowest wage category. The former value suggests that workers in this wage group are essential wage earners in their families, whereas the latter suggests these workers are less so. Among workers in the highest wage category ($6.55-$7.15), both the mean (54 percent) and median (44 percent) indicate that these workers are one of the primary earners in the family. This observation suggests that these wage groups are comprised of two distinct subsets of workers. While this may be unsurprising, it is often overlooked in discussions of the low-wage workforce. Therefore, to provide a more accurate picture of these workers, I present demographic profiles of these two groups, adult workers and teenager/student workers, separately in table 2.17. For reference, summary statistics are provided for all adult workers and all teenager/student workers, also.

What is striking about presenting the demographic profiles of these low-wage workers this way is how clearly they describe two very different groups of workers. First, the percentages of nonwhite and female workers are noticeably higher among adult low-wage workers than among teenager/student workers. In fact, among the higher two wage groups, nonwhite workers are underrepresented among teenager/student workers. Interestingly, female workers are overrepresented across wage groups and across subsets of workers. Second, median and mean usual hours worked are now closely aligned for teenager/student workers, reflecting that these workers are predominantly part-time; whereas the median and mean usual hours worked still reflect a range among adults.

Third, adult low-wage workers are clearly important contributors to their family incomes and earnings, significantly more than indicated in table 2.16. Among adult workers in the low-wage categories, the average percentage of family earnings contributed by the worker is now nearly 60 percent as opposed to the average of 43 percent to 54 percent in table 2.16. Correspondingly, teenager/student workers are typically secondary, nonessential workers, contributing an average of 21 percent to 26 percent of family earnings. While their average contribution is nontrivial, it is substantially less than that of adult workers. Finally, poverty rates among low-wage adult workers are markedly higher than among low-wage teenager/student workers. For example, among workers in the highest wage group, the 40 percent of adult workers live in families with incomes below twice the poverty level (i.e., low-income), whereas teenage/students workers in this wage group have poverty rates that are in line with the general working population.

This examination of the demographic characteristics of low-wage workers demonstrates how both sides of the debate on the target efficiency of minimum wage laws are partly correct. Those who criticize minimum wage laws accurately identify a subset of workers who benefit from minimum wage laws who are not necessarily the intended beneficiaries of these laws: teenager and student low-wage workers are not, on average, the working poor, primary wage earners in their families nor are they among the traditionally disenfranchised (nonwhite). On the other hand, those who advocate for minimum wage increases to boost the incomes of the working poor accurately point out the larger proportion of adult low-wage workers who are primary wage earners, largely poor, and nonwhite. Incorporating the ripple effect wage raises of minimum wage expands the pool of beneficiaries to include an even greater proportion of these adult low-wage workers. The vast majority (more than 80 percent) of workers earning around the 10th and 15th wage percentiles are neither teenagers nor traditionally-aged students. And, a substantial (although not a majority) of these adult low-wage workers are low-income, these workers typically work full-time hours, and provide at least half of his/her family's earnings or nearly half of his/her family's income. The overwhelming majority (77 percent to 80 percent) come from families with incomes at or below what may be considered "middle-class." If workers from middle-class income families are considered appropriate recipients of the benefits of minimum wage laws, then the target efficieny of minimum wage laws may be viewed as adequate as 74 percent of all low-wage workers come from such families.

In any case, the target efficiency is modestly improved when ripple effects are included. Among low-wage workers likely to receive mandated raises from a minimum wage increase, 49 percent are adult workers from families with middle-class incomes or less. When low-wage workers likely to ripple effect raises are added to this population, this proportion is increased to 56 percent. So that among all workers likely to be affected by a minimum wage increase (workers earning from \$5.15 to \$7.15), 56 percent are middle-class (or less) income adult workers.

2.5.3 Evidence of a Minimum Wage Contour

As noted earlier, if minimum wage increases result in limited ripple effects then the overall wage distribution compresses, so that wage inequality is reduced. That is, if only workers earning low wages experience an increase in their wage when the mandated wage floor rises, then the bottom of the wage distribution is pushed closer to the top of the wage distribution. In this section, I consider the question of whether this wage compression persists in the long-run in order to evaluate whether a benefit from minimum wage laws is to reduce overall wage inequality (if, of course, one views reducing wage inequality as desirable). To answer this question, I examine how the short-run wage effects (including a one-year lagged effect) estimated above interact with the long-run patterns in wage growth at different points in the wage distribution.

If wages at the low-end of the wage distribution, in general, follow similar patterns of wage growth as the rest of the wage distribution, then minimum wage increases could potentially cause a persistent contraction of the wage distribution and thus gradually reduce wage inequality over time. However, if wages at the low-end of the wage distribution experience slower rates of wage growth then minimum wage increases may be used to address deficits in the wage growth. This is, as discussed above, one of the original reasons for minimum wage laws—to discourage employers from using a low-road strategy of competing on the basis of the lowest wages possible. Such employer behavior causes the low-end of the wage distribution to follow closely the movements in the wage floor.

Past research has suggested this strong link between the wages of low-wage workers and the minimum wage. Rather than the minimum wage simply boosting a set of wages when the floor is raised, it has been argued that a subset of workers' wages fall in real value as the minimum wage falls in real value. Research by Spriggs and Klein (1994) and Rodgers, Spriggs, and Klein (2001) describes such workers as being part of a minimum wage contour—a set of workers whose wages are predominantly determined by the minimum wage.[33] Research findings linking the minimum wage and wage inequality of DiNardo, Fortin, and Lemieux (1996) are consistent with the existence of a minimum wage contour. They examine the role of the falling real value of the minimum wage in increasing wage inequality during the 1980s and found that wages in the bottom segment of the wage distribution tended to fall relative to the rest of the wage distribution as the minimum wage fell, suggesting that a segment of wages were tied to the minimum wage level. More specifically, Lee (1999) found that the 10th to 25th wage percentile differential, as well as the 25th to 50th wage percentile differential, increased due to the erosion of the minimum wage during the 1980s. This indicates that roughly the bottom 25 percent of the wage distribution effectively responds to the minimum wage rather than the overall wage trends of the rest of the wage distribution.

The conclusions of this previous research suggest that those workers affected by the minimum wage may be better described as being anchored to the minimum wage, rather than simply lifted by the minimum wage. Minimum wage increases appear to be important in raising the wages of low-wage workers. This stands in contrast to the findings of Neumark, Schweitzer, and Wascher (2004) discussed earlier. Based on a negative lagged minimum wage effect, they find that the wage raises that workers obtain in the short-run are "given back" as wage growth in the year following is reduced in response to minimum wage increases. As a result, they conclude that the wage benefits of minimum wage increases are much less than the immediate wage increases suggest because they substitute for wage growth that would have otherwise occurred in the following year. The difference between these findings is somewhat subtle, but they are distinct and opposing in their position on whether the wage increases that low-wage workers experience when the minimum wage increases produce positive consequences for low-wage workers. To summarize: Researchers of the minimum wage contour argue that workers only receive raises if the minimum wage increases, and therefore serve an important function in assisting low-wage workers to maintain the real value of their wages. Neumark, Schweitzer, and Wascher, on the other hand, argue that the raises associated with the minimum wage only serve to substitute for wage growth that would have occurred, and therefore provide limited benefit to low-wage workers with regard to the real value of their wages.

To investigate the accuracy of these contending scenarios, I compare the patterns of wage growth of the lower wage percentiles with the wage growth of other wage percentiles. More specifically, I examine the data to answer the question: Does the wage growth of the 5^{th} to 15th wage percentiles, the wage percentiles that are affected by minimum wage changes, differ from the rest of the wage distribution? An answer in the affirmative reinforces the impression of a minimum wage contour so that the wage effects of minimum wage increases are required to drive the wage growth of low-wage workers. An answer in the negative reinforces the impression that the minimum wage increases do not play an important role in maintaining the real value of the wages of low-wage workers.

A potentially instructive yet simple method of exploring whether particular wage percentiles are more closely linked to the minimum wage as opposed to changes in the overall economy, is to plot the trend of wage percentiles, over time, along side a measure of the minimum wage and a measure of the price level (CPI-U). Figure 2.9 displays the trends of the 5th to 60th wage percentiles, averaged across states over 6-month periods (to mirror data used in the above analysis) from 1983-2001. To facilitate comparisons between the three series, each is indexed to 1 for the first time period (January-June, 1983). Two patterns emerge: First, there is an apparent relationship between the mean prevailing minimum wage and the mean 5th, 10th, and 15th wage percentiles; such a relationship is less apparent in the percentiles beyond the 15th. These wage percentiles do not move in tandem with the price level, but rather dip below the price level between the 1990-1991 and 1996-1997 federal minimum wage increases. The link between the minimum wage and the lower wage percentiles indicates an erosion of their real values along with the minimum wage over this time period. Second, the wage trends of the 20^{th} to 60th wage percentiles appear to follow a path that is more or less, in tandem with the changes in the price level. Although this relationship is not consistently one-to-one, the trends suggest a strong correlation.

Looking more closely at the lower wage percentiles, it appears that during the 1980s, when the federal minimum wage remained unchanged from 1981 to 1990 (various state minimum wages increased in the late 1980s, but the effect on the average minimum wage level is slight) the wages of the lowest waged workers (5th-15th wage percentiles) stagnated along with the minimum wage. In the 1990s, when the federal minimum wage change twice (each in two stages), the wages of the lowest paid workers increased along with the minimum wage increases. Not until the late 1990s, did their wage trends increase at a rate at least as fast, and sometimes faster, than the CPI-U. Comparing the wage trends in these lower wage percentiles to the trend in the CPI-U clearly reveals that these wage trends did not keep apace with inflation in the absence of minimum wage increases, with the exception of the last few years of the extended expansionary period of the late 1990s. What this pattern implies is that only after prolonged periods of low unemployment, do the wages of the lowest paid workers ever gain in real value without minimum wage increases. These figures suggest that during “normal” economic conditions (i.e., during expansion and contractions that are not longer than average), the wages of the lowest paid workers move with the minimum wage. Workers who benefit from increases in the minimum wage appear to also be tied down to the minimum wage when it does not increase. These patterns are consistent with the existence of a narrow minimum wage contour, i.e., a segment of the workforce for which the minimum wage acts as the key rate.

The separate estimations of the wage equations (i.e, equation 2.8) at different points of the wage distribution provide a way to look at the wage determination process at different levels in the wage hierarchy. In particular, the year dummies in the model measure how each of the “rungs” in the wage hierarchy move over time, after controlling for movements associated with changes in demographic characteristics, industry and occupation composition, and minimum wage levels. These year dummies, then, capture how various points in the wage structure move in response to macroeconomic changes. Plotted over time, these coefficients trace out the ups and downs of business cycles. As seen in figure 2.10, the coefficients move in opposite direction of the national annual unemployment rate.[34] To facilitate a comparison across wage percentiles, figure 2.10 presents averages of these year coefficients by wage percentile.

Several different averages of the year coefficients by wage percentile are presented in figure 2.11 to illustrate how different points in the wage distribution have fared under different macroeconomic environments. Averages for the entire period, the 1980s, and the late 1990s are presented, excluding the years of the federal minimum wage increases.[35] These figures demonstrate more systematically the differences in wage growth across the wage distribution illustrated by the wage trends in figure 2.9. For reference, the average rate of inflation—3.2 percent for the years 1983 to 2001—based on the CPI-U is also plotted.

Note first that the growth rates of the 5th and 10th wage percentiles are consistently lower than the other wage percentiles. Over time, these lower wage percentiles lag behind the rest of the wage distribution, thereby increasing wage inequality. The 15th wage percentile also tends to lag in wage growth relative to other wage percentiles, however less consistently. The coefficients averaged for the 1980s reflect the well-documented increase in wage inequality that took place during this time period, attributed largely to the wage declines experienced by low-wage workers. For example, at the growth rates indicated by the 1980s coefficients (again, controlling for changes in minimum wage, demographic, occupational and industrial mix), the 5th wage percentile would be roughly 20 percent greater in 10 years, as compared to the 50th wage percentile which would be roughly 40 percent greater in 10 years.

The slower relative wage growth for these lower wage percentiles has another important economic consequence beyond that of increasing wage inequality. The growth rates of the 5th and 10th wage percentiles are also below the average rate of inflation. In other words, the wage growth of these lower wage percentiles is not sufficient to maintain their real value. Only in the late 1990s does the growth of the 10th wage percentile exceed that period's inflation rate of 2.5 percent, producing real wage gains for those workers. The 5th wage percentile, on the other hand, achieves growth rates just fast enough to maintain its real value. Again, however, relative to the rest of the wage distribution the 5th wage percentile loses ground during this period–as all other wage percentiles made *gains* in their real value.

These patterns demonstrate how the lower wage percentiles, those that tend to move with the minimum wage also do not tend to grow at sufficient rates to maintain their position relative to the rest of the wage distribution *or* their real value. The ripple effect, put in this context, is a reflection of the dependency these workers have on minimum wage laws to raise their wages. In other words, workers who earn wages at the lower extremity of the wage distribution do not appear to have effective means for raising their wages in absence of legislatively mandated wage raises. These observations are consistent with the existence of a minimum wage contour. In other words, these results suggest that minimum wage increases play an important role in buoying the real value of the wages of low-wage workers rather than the limited role suggested by Neumark et al.'s (2004) analysis. Employers appear to be more than immediately "taking back" wage raises caused by minimum wage increases. Rather, it appears that employers *withhold* wage raises excepting the occasions when the minimum wage increases.

This has a particular implication for the role of the minimum wage in reducing inequality. If wages at the low end of the wage distribution experienced wage increases due to the minimum wage *and* kept apace with the wage growth of the rest of the wage distribution, then the minimum wage would cause a persistent contraction of the wage distribution; it would effectively reduce wage inequality over time. Instead, the wage distribution moves in an accordion-like fashion over time: The real values of wages in the bottom 15 wage percentiles or so are pulled downward when the real value of the minimum wage sinks, and pushed upward when the minimum wage rises. The minimum wage restrains rather than reduces, wage inequality over time.

2.6 Conclusions

The main objective of this chapter was to identify and describe whether minimum wage increases produced wage increases beyond those that are mandated. Previous research suggests that they do. Most of these studies, however, do not differentiate the effect across the wage distribution in a detailed way. The most detailed estimates of minimum wage ripple effects provided by Neumark, Schweitzer, and Wascher (2004) have significant weaknesses in their methodological approach, discussed in detail above. My study uses an alternative methodological approach which produces a more reliable estimate of the minimum wage ripple effect. First, this study uses pooled repeated cross-sections that allow virtually all workers to be included in the sample. The methodology used by Neumark et al., based on one-year longitudinal data available from the CPS, limits their sample to workers that are employed and residing at the same address at both endpoints of one year.[36] As a result, worker who are marginally-attached to the workforce and transient workers are excluded from their sample and thus likely produce sample selection bias. Second, the relative wage position of the counterfactual wages and the "treated" wages are inconsistent in Neumark et al.'s study. Consequently, their study produces question estimates. Empirical wage percentiles, used in this study, provide more approporate counterfactuals by holding constant the workers' wage position (relative to the overall wage distribution). Finally, I explore the robustness of my estimates by varying the state and years included in the analysis. This exercise indicates that despite controlling for demographic characteristics, occupation and industry composition, and state- and year- effects, the pattern of minimum wage changes in the United States likely cause a portion of the estimated minimum wage effect to be spurious.

The methodology developed for this study produce the following results. Workers earning, on average, 135 percent of the minimum wage (wages around the 15th percentile or less) experience wage increases when the minimum wage rises. The size of the wage increase drops quickly as wages rise so that the effect drops by over one-third from the 5th to 10th wage percentile, and another one-third from the 10th to the 15th wage percentile. The outcome of these changes is a large degree of wage compression at the lower end of the wage distribution.

Further, these minimum wage ripple effects do not appear to be qualitatively different when there are high or low proportions of workers who receive mandated raises when the minimum wage increases. As a result, using an interaction term with the minimum wage change variable and the proportion of directly affected workers appears to be important in refining the results of the basic model (equation 2.8) rather than altering the overall conclusions. In other words, minimum wage effects do not appear to be significantly different when the minimum wage increase has a large or small "bite."

The results of a separate analysis based on the retail trade industry workers only do not alter these conclusions. The retail trade sector results provide more detailed, rather than qualitatively different, estimates for the low-end of the wage distribution for the entire economy. Note that this is consistent with the conclusion that varying the "bite" of the minimum wage does not appear to produce qualitatively different results. Since the retail trade sector has the largest concentration of minimum wage workers, it is a sector of the economy in which the minimum wage has the greatest potential to act as a reference wage. Thus, if the "bite" of the minimum wage (the degree of its direct effect) changed the extent or magnitude of the ripple effect, I would expect to observe this among workers in the retail trade industry. Because I do not observe qualitatively different effects when the degree of impact of minimum wage increases is varied, this suggests that mandated wage floors such as living wage laws or citywide minimums will produce similarly limited ripple effects.

The fine-grained analysis of minimum wage ripple effects also helps to inform the debate around the overall impact of minimum wage increases. I gauge the significance of ripple effects by estimating their value relative to mandated wage increases associated with minimum wage increases. This multiplier is estimated to be roughly 2.40 to 2.50. Therefore, in the context of minimum wage increases, the value of ripple effect raises is greater than the value of mandated raises. This finding underscores the importance the significane of these effects when considering the impact of minimum wage changes.

These estimates also allow for a more complete picure of the target efficiency of minimum wages. The target efficiency is modestly improved with the inclusion of workers who receive ripple effect raises in the targeted population. Adding such workers increases the proportion of moderate- to low-income adults among all workers receiving wage raises due to a minimum wage increase from 49 percent to 56 percent.

Finally, estimating wage equations separately for different points in the wage distribution provides insight on how wage inequality is affected by minimum wage laws in the long-run. I find that the wage growth among workers affected by minimum wage increases (via mandated and/or ripple effect raises), excluding wage growth cause by minimum wage increases, tends to lag behind those of the rest of the wage distribution, a finding that is consistent with the existence of a minimum wage contour. As a result, increases to the minimum wage restrains, rather than reduces, wage inequality over time.

Notes

[1] This conclusion is drawn by Prasch (1998) after surveying economic journals published during 1912 to 1923.
[2] Nominal values are adjusted with the national CPI-U from the Bureau of Labor Statistics.
[3] These states include: Alaska, California, Connecticut, Hawaii, Maine, Massachusetts, Minnesota, New Hampshire, North Dakota, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, and Wisconsin.
[4] See Equation 5.3 in Table 5, p. 428 of Gramlich (1976).
[5] Derived from author's analysis of Current Population Survey outgoing rotation groups (CPS-ORG) from 1983 to 2002.
[6] In fact, in the survey conducted by Converse, Coe, and Corcoran (1981), employers were asked to distinguish between non-mandated raises that workers would receive after a federal minimum wage increase *regardless* of the change in the federal minimum wage and non-mandated raises that workers would receive *because* of the change in the federal miimum wage. A large majority of employers (roughly 75 percent) who reported non-mandated raises after a federal minimum wage increase also report that these raises are *unrelated* to the federal minimum wage increase.
[7] Note that there is the additional complication that accompanies the inclusion of measures of macroeconomic trends directly. If such trends are endogenous (e.g., if changes in price level or unemployment are, in part, caused by changes in the minimum wage) this raises the problem of bias due to using endogenously-determined regressors.
[8] Note also that cross-sectional data has its own weakness well documented by the econometrics literature (Wooldridge 2002). If minimum wage levels are themselves partially determined by some aspect of the cross-section (i.e., the minimum wage cannot be treated as an exogenous variable) then regression coefficients estimating the effect of minimum wage levels on wages will be biased as noted by Easton and King (2000). In their study, for example, they consider that some quality of the metropolitan areas they study may be correlated with the level at which minimum wages are set. In this case, differences in wages across metropolitan areas may be caused by this omitted factor rather than the minimum wage level. Fixed effects models with panel data are commonly used to address this issue.
[9] This approach produced the estimates on the range of the minimum wage ripple effect discussed above, and presented in table 2.1.
[10] Her regression estimates of a model including a quarterly lag structure of her minimum wage change variable do include some negative coefficients for these lags, however, they are not statistically significant at the conventional levels (Grossman 1983, 371-372).
[11] If a respondent erroneously reports an unusually high wage in time t, it is likely that s/he will report a lower wage in time t+1 due to the tendency of data to regress to the mean. In other words, if the respondent reported her/his wage erroneously high in time t chances are greater that s/he will report a wage closer to average (less high) in time t+1. As a result, the wage change for workers who report very high wages will have some

negative component due to measurement error, and thus, all else equal, the wage change will be relatively smaller compared to other workers whose initial wages are lower.

[12] Note also that the negative lagged effect is greatest in absolute value at the bottom of the wage distribution, declines in absolute value towards the middle of the wage distribution, and then increases in absolute value at the top of the wage distribution. This pattern of negative effects fits what would be expected if the indicator variables Rj are, in part, capturing measurement error.

[13] Note that the measurement error issue that affects wages likely also affects their measure of hours. This is due to the fact that their hourly wage is calculated using data on usual hours for non-hourly wage workers. Therefore, non-hourly wage workers may have very low wages because they report very high hours. To the degree that this is true, the problem associated with the Rj variables and measurement error in wages will have a similar affect on their measure of hours. That is, the lagged effect of minimum wage changes on hours will be negatively-biased by measurement error for workers with low wages.

[14] Business cycle dates are taken from those published on the National Bureau of Economic Research website, "Business Cycle Expansions and Contractions," <http://www.nber.org/cycles.html/> (8 June 2005).

[15] As noted by Neumark, Schweitzer, and Wascher (2004) alternative micro-data sets such as those provided by the Survey of Income Program Participation (SIPP) survey do not provide adequate sample sizes to estimate models with relatively comprehensive sets of control variables.

[16] Neumark, Schweitzer, and Wascher (2004) take this approach. To adjust for this bias in their sample, they adjust their sample weights to account for patterns of attrition. As they note, nonrandom matching that is not taken into account by their weighting scheme and that is correlated with changes in outcome or any other variable of interest may create problems in their estimates.

[17] There is a substantial literature that documents industry and occupational differences unexplained by human capital differences (England 1992; Fogel 1979; Freedman 1976; Krueger and Summers 1987; Mason 1994). I use the major industry and major occupation categories provided in the CPS to define the dummy variables.

[18] Because Washington, DC does not have a uniform district-wide minimum wage prior to 1993, it is excluded from the data set.

[19] Augmented Dickey-Fuller tests were conducted first, separately for each state and each variable, to determine the appropriate number of lags to be allowed in the IPS test. The IPS tests were repeated for a range of lags also. This process follows the method recommended by Campbell and Perron (1991). The IPS tests were also done with and without a time trend, and with the elimination of common time effects.

[20] Tests were only performed on the contemporaneous minimum wage variable since tests on the lagged minimum wage variable would produce virtually identical results. Because the minimum wage has a visibly apparent upward trend over time, the IPS test was conducted with the assumption of a time trend. The series was also cross-sectionally demeaned to eliminate common time trends. This specification seemed be reasonable

since the basic model includes year dummies to control for trends that may be common across the cross-sections.

[21] The year of 2002 is not included because the changes are from Year t to Year t+1 and 2003 is not included in this analysis.

[22] These states include: Alabama, Arizona, Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Michigan, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, New York, North Carolina, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, West Virginia, and Wyoming.

[23] These averages are based on the state-level measures used in the regression analysis and are described in section 2.3. Thus, state averages are generated from subsets of workers earning between the wage percentile indicated plus or minus 5 percentiles during a particular 6-month interval. For example, the subset of workers earning around the 5th wage percentile includes workers earning below the 10th wage percentile, the subset of workers earning around the 10th wage percentile includes workers earning between the 5th and 15th wage percentiles, and so on. These state averages are then averaged across states to produce the data presented in the table.

[24] Equation 2.5 is estimated using Prais-Winsten regression with panel corrected standard errors. The panel corrected standard errors are estimated with the assumption of first-order panel-specific autocorrelation and panel heteroskedastic errors with contemporaneous cross-correlated errors. Standard errors that are robust to within-panel heteroskedasticity are not used because to reliably estimate such standard errors, the number of panels (clusters) should be large relative to the number of parameters estimated.

[25] New England states include: Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, and Connecticut.

[26] Workers who earn a portion of the wages through tips are generally treated differently under minimum wage laws. In particular, a portion of their tips is credited toward a worker's wage in determining whether a worker is earning the mandatory minimum. At the federal level, prior to 1996, the tip credit was 50% of federal minimum wage. However, since 1996 the tip credit has been fixed at a dollar amount $2.13. As such, federal minimum increases do not, after 1996, administratively increase the mandated wage floor for tipped workers and as a result, federal minimum wage increases do not mandate raises for tipped workers. States which allow for tip credit (35 of 50) vary in how they determine tip credits. Some states fix the minimum level of direct wages as a proportion of their state minimum wage, others set an absolute level.

[27] For Michigan: (0.19)(0.12) + (2.32)(0.12)(0.082) + (-0.02)(0.12) + (0.34)(0.12)(0.082) = 0.047; for Mississippi: (0.19)(0.12) + (2.32)(0.12)(0.161) + (-0.02)(0.12) + (0.34)(0.12)(0.161) = 0.072.

[28] For Michigan: (-0.11)(0.12) + (3.19)(0.12)(0.082) + (0.11)(0.12) + (-0.61)(0.12)(0.082) = 0.025; for Mississippi: (-0.11)(0.12) + (3.19)(0.12)(0.161) + (0.11)(0.12) + (-0.61)(0.12)(0.161) = 0.050.

[29] See appendix C for details. Note that my estimates ignore potential employment effects that may result from minimum wage increases. Because past research on employment

effects has suggested that negative employment effects are either small, non-existent, or slightly positive, I conclude that disregarding employments effects is justified. However, to the extent that employment effects are present, these estimates will be incorrect. How the multiplier will be affected will depend on how the employment effect is distributed across the lower wage percentiles.

[30] For a clear illustration of this see kernel density estimates in Dinardo, Fortin, and Lemieux (1996).

[31] Note that the first wage category probably has a smaller percentage of high school graduates because of the large percentage of teenage and student workers among this group.

[32] Note that family poverty status, as well as family earnings and income measures, are based on the income obtained by the family in the year prior. While the March ADS provides information on worker's annual earnings and hours from the year prior which can then be used to calculate wages, I chose to categorize workers according to their contemporaneous wage measure (provided in the March ORG data). Although this creates a time inconsistency between the wage measure and the income measures, I chose this method because the calculated wages based on the March ADS tends to underestimate the proportion of teenagers and students for the lowest percentile range. This is due to the fact that there is greater measurement error in variables used to construct wage measure from the March ADS which require respondents to recollect their earnings and hours from the year prior. As a result, the composition of workers at the low end of the wage distribution tend to be older workers with higher overall incomes, suggesting that their low wages reflect measurement error in the measures of hours worked and weeks worked, in particular. A comparison of demographic variables between samples based on the March ADS and the March ORG reveal that these differences are greatest in the lowest wage category used in this analysis but are greatly reduced in the higher wage categories.

[33] Spriggs and Klein (1994) conceive of the minimum wage contour as wages that are determined by a common set of factors, primarily the minimum wage, so that changes among these wages are interrelated.

[34] National annual unemployment rate is the 12-month average of the BLS national, monthly estimates based on the Current Population Survey (Bureau of Labor Statistics, 2005).

[35] Even though the minimum wage effects should largely be captured by the minimum wage variable of the model, there is still the possibility that a portion of the effect of the federal minimum wage change will be absorbed by the year coefficients. For instance, looking back at table 2.5, in 1990 and 1996 the magnitudes of the six-month changes of the federal minimum wage are almost equal. As a result, because the federal minimum wage change in virtually the same across the whole year for the majority of the states, the year dummies for 1990 and 1996 ar likely to reflect at least some of the impact of these minimum wage increases on wages.

[36] Additionally, my outcome variable is a summary measure (percentiles) rather than individual observations on wage changes which, as Neumark et al. (2004) note, contain large amounts of measurement error.

CHAPTER 3

ANALYSIS OF PREVAILING WAGE RIPPLE EFFECT

3.1 Background and Literature Review

The repeal of state-level prevailing wage laws provide another case in which ripple effects can be examined. In general, prevailing wage laws stipulate minimum wage and benefits package levels for construction workers working on publicly-funded or financially-assisted projects that are based on the concept of "customary" compensation.[1] Federally-supported projects are covered by the Davis-Bacon Act (passed in 1931) while state prevailing wage laws (the first passed in Kansas in 1891) cover public projects that are funded or financially-assisted by a state or municipality.[2]

Prevailing wage laws originated as a way for Congress to intervene in the labor market given that, at the time, the U.S. Supreme Court ruled that government could not regulate private contracts between individuals. By regulating its own construction contracts, Congress aimed to affect the labor standards more broadly by setting an example. U.S. Senator John Conness of California articulated this view in reference to the National Eight Hour Day Act–the original prevailing wage law adopted in 1868:[3]

> I know that the passage of this bill cannot control in the labor of the country; but the example to be set by the Government…I know that labor in the main, like every other commodity, must depend upon the demand and supply. But, sir, I for one will be glad, a thousand times glad, when the industry of the country shall become accommodated to a reduced number of hours in the performance of labor. (U.S. Congress 1868, 27)

Thus, at least some legislators explicitly intended the effects of prevailing wage laws to ripple across the labor market.

Another motivation behind prevailing wage laws was a desire for the federal government to *not* intervene in the local labor market. More specifically, Robert L. Bacon, the Republican representative who first introduced the Davis-Bacon Act in 1927, worried that federal government work could distort local labor market standards given its large buying power:

> The Government is engaged in building in my district a Veteran's Bureau hospital. Bids were asked for. Several New York contractors bid, and in their bids, of course, they had to take into consideration the high labor standards prevailing in the State of New York…The bid, however, was let to a firm form Alabama who had brought some thousand non-union laborers from Alabama into Long Island, N.Y.; into my district. They were herded onto this job, they were housed in shacks, they were paid a very low wage, and the work proceeded … It seemed to me that the federal Government should not engage in construction work in any state and undermine the labor conditions and the labor wages paid in that State … The least the federal Government can do is comply with the local standards of wages and labor prevailing in the locality where the building construction is to take place. (U.S. Congress 1927, 2)

Prevailing wage laws, then, were intended not only to maintain the customs of the local labor market, but to do so that high labor market standards prevailed. The strategy of this legislation was to limit the channels through which competition in the construction industry operated by codifying local standards.

Prevailing wage laws (state and federal) vary in what aspects of work are regulated and what determines coverage. More specifically, as described by the U.S. Department of Labor, the federal law stipulates that:

> …each contract over $2,000 to which the United States or the District of Columbia is a party for the construction, alteration, or repair of public buildings or public works shall contain a clause setting forth the minimum wages to be paid to various classes of laborers and mechanics employed under the contract. Under the provisions of the Act, contractors or their subcontractors are to pay workers employed directly upon the site of the work no less than the locally prevailing wages and fringe benefits paid on projects of a similar character. The Davis-Bacon Act directs the Secretary of Labor to determine such local prevailing wage rates. (U.S. Department of Labor 2005)

The rules governing Davis-Bacon prevailing wage rates[4] have varied over time but have substantively remained the same: the prevailing wage rates are to be the current modal or mean wage defined within geographic location, occupation, and construction type. Until 1985, the federal rule for setting the Davis-Bacon wage rate (which many state laws adopt for their rates) was know as the “30 percent rule.” This stipulated that the rates were to be set at the wage level received by 30 percent of workers in a particular occupation, region, and construction-type. If no such “modal” rate existed, then the average was to be used. After 1985, the rule was changed to the “50 percent” rule. Under the Reagan administration, the 30 percent qualification was increased to 50 percent so that 50 percent of workers had to receive a particular wage rate in order to determine the prevailing wage rate, else the average was to be used. This change was seen as weakening the role of unions since previously they only had to achieve 30 percent coverage to set the prevailing wage rate to their contract rates.

Some state laws are modeled after the Davis-Bacon Act but most differ in some significant way. Coverage may be limited to contracts that exceed a specified threshold, a set of occupation categories, and/or certain types of construction. State laws also vary in their treatment of projects which are funded by municipalities and projects which are jointly funded with the federal government. And, state laws vary in the method they use to determine prevailing wage rates: some states conduct surveys and use the majority, average, or modal rate; some states use an unspecified or ad hoc process, some use collectively bargained rates. The diversity among state prevailing wage laws is great. Despite this, these laws have the common intent of the Davis-Bacon Act—to support locally-determined “good” wages by mandating public works to pay such wages. These state-level prevailing wage laws are approriately referred to as “little Davis-Bacons.”

Because the intended consequence of the law is to intervene in the marketplace to support a higher level of wages than would likely exist in the absence of such laws (as with minimum wage laws), prevailing wage laws have been controversial. This is evidenced by the various repeals of the state-level prevailing wage laws that have occurred. Ten states have repealed their state prevailing wage laws since the late 1970s, giving way to arguments that such laws are inflationary and also discriminatory against traditionally nonunion workers (i.e., minority construction workers) (Azari, Yeagle, and Phillips 1994).[5] Eight states, located in the South and Midwest, have never had state-level prevailing wage laws. By the end of 2005, 31 states will have state prevailing wage laws, a marked decline from the 1973 peak of 42 (see table 3.1).[6] Beyond the political controversy over state prevailing wages laws, their importance may be indicated by two factors: 1) the proportion of construction work that is tied to public funding and 2) the size of the mandated wage increases.

While no empirical estimate exists for the percent of construction workers covered by state and federal prevailing wage laws separately, publicly-funded construction has comprised over one-fifth of the total value of construction projects during the late 1980s and 1990s (see table 3.2). State and local government-owned construction projects comprised approximately three-quarters of this one-fifth. In other words, the large majority of publicly-funded construction projects involve local funds. However, because the Davis-Bacon Act covers many projects that are only partially funded by federal funds, the Congressional Budget Office (U.S. Congress C.B.O, 1983) estimated in 1982 that 20 to 25 percent (as opposed to 4 to 6 percent) of all construction is covered by the Davis-Bacon Act. The extent of state prevailing wage coverage will be increased beyond its 15 to 20 percent of total construction value to the extent that state and local funds only partially fund projects and decreased to the degree that the Davis-Bacon Act preempts state coverage. Also, Bloch (2003) argues that the value of public work projects tends to exceed similar private projects. As a result, the proportion of covered employment may be less than the proportion of the value of publicly-owned construction. Despite the lack of an estimate of the percentage of workers covered exclusively by state prevailing wage laws, the above estimates suggest that the state-level laws play a significant role in the construction labor market.

Prevailing wage rates are meant to reflect wage norms within occupation, construction-type, and local area. Therefore, legally, prevailing wage laws increase the mandated wage floor to a central tendency measure of a particular wage distribution of construction workers from the minimum wage. However, in practice, the mandated wage increase is more accurately characterized as an increase from a counterfactual wage: the wages that covered workers would have received if no state prevailing wage law existed. Because neither measure—the prevailing wage rate nor the appropriate counterfactual wage—is easily identified, determining the magnitude of the mandated wage increase is difficult.

First, identifying the actual value of prevailing wage rates is not easily done. Given the complexity of prevailing wage laws (as mentioned above, wage rates are specified within area, construction-type, and occupation, at a particular point in time) and the diversity of rate determination processes across states—including some states that do not specify a process for determining prevailing wage rates (Thieblot and Burns 1986)—using actual prevailing wage rates in an analysis can quickly become prohibitive. For example, Thieblot's 2005 study of Pennsylvania's Davis-Bacon wage rates tabulates all the wage determinations for the state. In total, there were 2,027 different rates in February 2004. As a result, past studies have relied either on case studies or have generated estimates of the mandated wage increase from overall wage effect estimates and coverage estimates (Bloch 2003).

Summary statistics of a particular wage distribution would seem to provide an alternative to identifying prevailing wage rates, since the rates are supposed to reflect the modal or average wage rates. However, whether prevailing wage rates actually do has been a source of debate. The focal point of the debate is whether prevailing wage rates are biased toward union rates (Allen 1983; Bloch (2003); Gujarti 1967; Thieblot 1976, 2005). There are two reasons why prevailing wage rates might be set at union rates: 1) because union contracts facilitate wage uniformity within occupation/construction-type/area, union wage rates are likely to meet the modal requirement for prevailing wage rates when union density in an area is high, and 2) even when union rates do not reflect the modal or average rate of a particular occupation/construction-type/area, union wages may be chosen to bypass the administrative burden of alternative determinations or because of political pressure coming from unions (or union supporters) to do so. Allen (1983) partially resolves this debate by presenting evidence that union density is correlated with prevailing wage rates being set at union rates, as would be expected if prevailing wage rates were being set according to the "modal or average" rule. While this observation does not rule out a bias of prevailing wage rates toward union rates, it does demonstrate that such a bias is not absolute.

Second, the appropriate counterfactual wage is not easily identified. The counterfactual wage should be derived from the wages workers would receive in the absence of a prevailing wage law. Simple comparisons between the prevailing wage and average wage do not take into account the fact that the average wage is itself a function of prevailing wage rates. To the extent that prevailing wage rates influence wages beyond those of covered workers, the greater the average wage rate will be biased away from a true, counterfactual average wage.

Given these difficulties and the differences across prevailing wage laws, past research has produced a range of estimates of the mandated wage increases associated with prevailing wage laws (see table 3.3). The empirical research of Goldfarb and Morrall (1981) provides lower-bound estimates of the mandated wage increases from the prevailing wage laws of 4 to 9 percent. These estimates are based on 1972 wage differentials between Davis-Bacon prevailing wage rates and average wage rates by occupation in nineteen cities. These estimates are not likely to apply broadly given that, as Allen (1983) points out, these cities had unusually high union density rates (80-84 percent). Thieblot (1975) calculates wage differentials of greater magnitude based on a set of case studies in which he presents differences between Davis-Bacon prevailing wage rates and average wage rates from the private sector during the 1970s.[7] Averaging his measures by case study, Thieblot generates estimates that range between approximately 30 percent to 50 percent.

An upper bound estimate may be derived from the average union-nonunion wage differential in the construction industry, assuming that covered workers are all nonunion and prevailing wage rates are set at union rates. This upper-bound estimate, based on the construction union premium during 1980 to 1984—the pre-repeal period analyzed in this paper—is approximately 52 percent.[8] This however, is likely an overstatement of the mandated wage increase given that union workers tend to be more skilled and thus gives employers the incentive to fill jobs on covered projects with union workers (see Azari-Rad, Yeagle, Philips 1994).

Intermediate estimates are provided by O'Connell (1986) and Kessler and Katz (2001). Using 1978 BLS area wage surveys that provide data on which workers are employed in covered construction projects as well as data on union and nonunion wage rates, O'Connell generates counterfactual average wages by assigning covered nonunion workers average nonunion rates (which presumably are lower than their prevailing wage rates). He then combines these counterfactual wage rates with an assumption about the shape of the counterfactual wage distribution to produce estimates of wage changes by occupation. His estimates, averaged across occupations, range between 13 to 33 percent.

Kessler and Katz estimate mandated wage increases of 12 to 20 percent based on their regression estimates of the overall wage decline due to the nine state prevailing wage law repeals that occurred from 1979 to 1988.[9] These estimates have the advantage of using the change in average wage rates before and after prevailing wage laws have been repealed. This analytic strategy somewhat circumvents the need to make assumptions about the counterfactual wage distribution because states provide their own "counterfactual" after their state prevailing wage law is repealed. The disadvantage to this strategy, of course, is that the mandated wage increase is estimated by comparing wage measures at two different points in time, opening up the possibility of correlating spurious trends to the law repeals (this study will be discussed in more detail below).[10]

Generally then, estimates of the mandated wage increase may be said to be within the range of 12 percent to 53 percent. Using the midpoint of this range, the mandated wage increase associated with prevailing wage laws may be estimated to be roughly 30 percent, a sizable increase. Also, the higher end estimates indicate mandated wage increases that are substantially larger than any minimum wage increase (federal minimum wage increases are typically 11 percent; two-step increases in 1990-1991 and 1996-1997 increased the federal minimum by 26 percent and 21 percent, respectively).

While past studies have varied in terms of the estimated magnitude of the mandated wage increase, they have been consistent in finding measurable positive wage effects associated with the presence of prevailing wage laws, both at the federal and state level. Such results should be unsurprising given the intent of these laws. However, the fact that state prevailing wage laws exclusively cover only a subset of workers (i.e., cover workers that are not also covered by the Davis-Bacon Act) may obscure the wage effect associated with the state prevailing wage laws. Additionally, if the prevailing wage rates are set low relative to the construction wage industry, it would be possible that prevailing wage laws would have no positive affect on wages.

Whereas many studies have focused on measuring the size of mandated wage increases associated with prevailing wage laws, few studies have examined the impact of ripple effects. As with the minimum wage, opponents of prevailing wage laws cite ripple effects as a key component of the economic cost of prevailing wage laws (e.g., Philips, Mangum, Waitzman, and Yeagle 1995; Herzenberg and Price, 2003).[11] However, to the best of my knowledge, only two studies address this issue in a substantive way. I review these two studies below.

A set of interviews conducted by Bourdon and Levitt (1980) provides qualitative data on the incidence of ripple effects. In 1976, they interviewed 240 construction contractors in eight metropolitan areas.[12] As part of the interview, they asked questions to directly assess whether prevailing wage rates influenced the wage rates set for workers that are not covered by prevailing wage laws. Specifically, they asked private, open-shop contractors who engaged in some public work how they dealt with the potential wage differential between nonunion workers who work on publicly-assisted contracts and nonunion workers working on private contracts. As the researchers point out, these private, open-shop contractors are most likely to be affected by ripple effects because the potential wage differential between the two types of contracts is greatest for their work crews. What the researchers found was that the contractors engaged in a wide variety of wage-setting strategies that aimed to isolate the effect of prevailing wage rates to covered contracts only. These strategies include: creating two separate work crews with distinct wage scales, avoiding public work altogether, or rotating workers through covered jobs. Only a few contractors indicated a ripple effect, responding that they paid higher wages for all work, not only covered work.

O'Connell (1986) examines the incidence of ripple effects from a different perspective. His study asks the question: Do prevailing wage rates determine, in part, the union wage premium in construction? In contrast to the usual question of whether union wage rates in construction determine the levels at which prevailing wage rates are set (as discussed above), this question asks whether the level of the wage floor provides increased bargaining power for construction unions, and thereby produce greater union premiums. If the wage floor is set below the union rates, union contractors may be placed at a cost disadvantage, increasing the pressure for union workers to concede to lower wages at the bargaining table. If, on the other hand, prevailing wage rates are set at union wage levels, then unions are better able to maintain or increase their wage rates, i.e, increase or maintain their union wage premiums. O'Connell's basic question is whether the positive wage effect associated with prevailing wage laws ripple across the construction union sector more broadly. O'Connell finds a statistically significant, positive relationship between construction union premiums and the use of union wage rates in prevailing wage determinations despite controlling for union density (he also controls for local labor market conditions). He concludes from this finding that the effect of prevailing wage laws is broader than the mandated wage increases required by such laws.

O'Connell's conclusion is based on estimates of the effect of using union rates for prevailing wage rates on the average union premium. However, an increase in the average union premium can be produced a variety of ways: by an increase in union premiums among a subset of union workers, an increase in union premiums among union workers in general, or a combination of increases in union premiums of varying sizes. To discern whether a ripple effect is produced by prevailing wage laws, one must distinguish between these different situations.

For example, it may be the case that as the wage floor increases, employers have a greater incentive to increase the labor productivity of their workforce, either by increasing the skill level of their workers or the capital content of the work process. While this may increase the bargaining power of high-skilled workers, this may not translate more broadly across the the union sector. In order to achieve the latter, union bargaining power (and unity) would have to be sufficient to negotiate high wages across skill levels. An increase in the union premiums of a subset of union workers alone will raise the average union premium. If increases in union premiums are circumscribed to a subset of union workers, then this would indicate limited or no ripple effects. O'Connell, however, does not analyze the scope of the effect he observes.

A gap exists in the research examining the impact of prevailing wage laws. Ripple effects have not been directly estimated even though they are an important component to understanding the way in which the wage floor intervenes in the wage-setting process. The myriad of wage levels assigned by prevailing wage laws as wage floors, as well as the limited scope of coverage creates particular challenges to studying any ripple effect associated with prevailing wage laws. As mentioned above, the manner in which prevailing wage rates vary will quickly make the use of actual prevailing wage rates prohibitive for any study that attempts to go beyond the scope of case studies. In addition, the focused coverage on construction projects financially assisted by public funds may limit the ability of statistical analyses to detect effects unless they are relatively broad.

However, there are several aspects of prevailing wage laws that are likely to make their effect distinctly different from that of the minimum wage and therefore make it useful to study. By studying two qualitatively different mandated wage floors, insight may be gained on how ripple effects vary given different parameters. In particular, because several aspects of prevailing wage laws make them resemble living wage laws more closely, the results of this analysis may provide insight into how the ripple effects of living wage laws operate as well. First, as noted above, the potential magnitude of the wage floor change associated with prevailing wage laws is significantly greater than that associated with minimum wage laws (that is, in the context of comparing labor markets with and without state prevailing wage laws). The range of estimates presented in table 3.3 suggests increases in the mandated wage floor on the order of 30 percent. Second, even though prevailing wage laws only cover one part of the construction industry, prevailing wage laws insert a wage floor into a dense part of the wage distribution thereby increasing the potential relative wage effect because of the proximity of many more workers to the wage floor. Unlike minimum wage increases, which make small adjustments to the bottom of the wage distribution, prevailing wage laws set a wage floor in the middle, roughly, of the wage distribution. In this way, similar to living wage laws, prevailing wage laws have greater potential to disrupt the wage hierarchy if ripple effects do not occur. If relative wages that make up the wage hierarchy play an important role in maintaining worker discipline, high levels of productivity, and/or a low turnover rate or if changes in relative wages cause substitution effects, as suggested by economic theory, then the insertion of a mandated wage floor in the middle of the wage distribution should produce significant ripple effects vertically (i.e., among workers who earn wages above and below the wage floor) and horizontally (i.e., among workers outside the covered sector) who may see the wage floor as a reference wage. Finally, because prevailing wage laws define wage floors by occupation, a wide variety of workers is affected. Whereas minimum wages figure primarily in the wages of workers in the retail trade sector, and only among workers that earn very low wages relative to the rest of the wage distribution, prevailing wage laws (as well as living wage laws) may have increased potential to cause ripple effects if the maintenance of relative wages depends, in part, on bargaining power. As such, an analysis of prevailing wage laws may approximate more closely the way in which living wage laws affect the wage structure of the municipalities in which they are enacted than minimum wage laws.

The other significant difference between this study of ripple effects, based on the prevailing wage law, and the minimum wage analysis presented in chapter 2 is that the ripple effects are observed in the context of the removal of a mandated wage floor. A substantial literature in macroeconomics exists on the concept that nominal wages do not tend to fall—that is, nominal wages are theorized to be "downward sticky." If this phenomenon applies to the construction industry in the context of this public policy change, the wage effects of a decrease in the wage floor will not simply reverse the wage effects of an increase in the wage floor. For example, workers who earn high wages when the prevailing wage law is in effect may sustain their high wages (and potentially increasing their relative wage) when the prevailing wage law is repealed due to their resistance to changing their wage *level*. Examining ripple effects of the repeal of prevailing wage laws in this context, then, may underestimate the overall wage effect: no prevailing wage effect and wage inertia produce the same outcome. On the other hand, wage inertia after the repeal of a prevailing wage law suggests that those wages levels are maintained by some other mechanism—so that the prevailing wage law is not the essential determinant of such wages.

3.2 Data and Methodology

3.2.1. Data

The data source on wages, union status, and occupation for this analysis is the same as that used in the minimum wage analysis: individual-level data from the CPS-ORG files, unless otherwise noted (for further details about the CPS-ORG data see section 2.3.1). Sampling weights are used throughout the analysis to make the data nationally representative. All dollar values are expressed in constant 1982 dollars unless otherwise noted (nominal wages are adjusted using the Current Price Index – Urban Consumers).

The one difference from the minimum wage analysis, in terms of the criteria for inclusion in the data set, is that concerning extremely high and extremely low wages. For the quantile regression analysis discussed below, excluding such observations is unnecessary given that quantile regression estimates are robust to extreme values (Koenker and Hallock 2001) as long as the conditional quantiles are not themselves in the extreme tails of the conditional wage distribution.

A few variables used in this analysis were modified in the CPS survey over the time period analyzed: 1980 to 1992. The first two are the 3-digit occupation and industry codes. The two occupations that are analyzed here, carpenters and laborers, are consistent across the change that occurred from 1982 to 1983, as is the industry category of construction. The aggregated category of blue-collar workers also does not appear to be affected by the coding change. I use the BLS definition of blue collar workers which includes the following major occupational groups: (1) precision production, craft, and repair, (2) machine operators, assemblers, and inspectors, (3) transportation and material moving, and (4) handlers, equipment cleaners, helpers, and laborers (U.S. Department of Labor 2005).

The third is the variable measuring the level of education attained. This variable is used in creating the Mincerian potential labor force experience measure (i.e., age – 6 – years of education). The change from 1991 to 1992 involved a switch from one-year categories of attained education to grosser categories based on credentials obtained (e.g., high school diploma). As a result, a consistent variable of number of years of education had to be created.[13]

Data on state prevailing wage policies are taken primarily from Thieblot and Burns' extensive 1986 study, Prevailing Wage Legislation: The Davis-Bacon Act, State "Little Davis-Bacon" Acts, the Walsh-Healey Act, and the Service Contract Act, of federal and state prevailing wage legislation. This study provides the most comprehensive source to-date of state-level detail on the state prevailing wage laws complied in one source.[14]

3.2.2 Methodology

The basic analytic strategy is a difference-in-difference approach (DD), similar to that used in the minimum wage analysis and that used by Kessler and Katz (2001) to study the impact of state prevailing wage law repeals on the average wages of construction workers. As with minimum wage laws, because a subset of states have changed their state-level prevailing wage policy status over time, differences across states over time may be exploited to observe prevailing wage law effects in the DD framework. More specifically, differences in wages over time (the first difference) within states that repealed their state-level prevailing wage policies are compared to the differences in wages over time within states that do not have a change in their state-level prevailing wage policy (the second difference). Thus, having a subset of states that repealed their prevailing wage laws at the same time that other states did not change the status of their state prevailing wage laws potentially allows the researcher to "difference out" economic trends not related to prevailing wage laws occurring simultaneously. Kessler and Katz add a third "difference" by examining construction wage premiums (construction wages relative to blue collar wages) instead of wages levels, thus referring to this analytic strategy as a difference-in-difference-in-difference approach (DDD). To clarify, this approach compares how the construction premium within repeal states (the first D) changes over time (the second D) to how the construction premium within non-repeal states changes over time (the third D). This allows the researcher to additionally "difference out" within-state economic trends, along with across-state economic trends.

To assess the presence of ripple effects, I attempt to measure the impact of prevailing wage laws throughout the wage distribution. If the wage floor falls, and it was binding (i.e, the prevailing wage rates were above the wage rates on privately-funded construction projects), the elimination of the wage floor should cause wages at the prevailing wage rates to fall. To the extent that higher wage levels are determined by their relative distance above the wage floor set by prevailing wage laws, those higher wage rates should also fall. To the extent that the impact of prevailing wage laws ripple outside public works, an extensive wage impact across the construction sector should be observed below, at, and/or above the prevailing wage rate levels. Therefore, the ripple effect is observed by measuring the extent of the negative effect of state repeals on wage rates across the wage distribution as well as across the construction sector.

Analyzing the effect of state prevailing wage laws has different data requirements than the ripple effect analysis of state minimum wage laws. The breadth of coverage and uniformity (within state) of federal and state minimum wage laws allowed a detailed accounting of wage effects in chapter two by using virtually the entire CPS-ORG sample of employed workers. Prevailing wage laws, on the other hand, mandate wage floors within publicly-funded or financially-assisted construction projects that vary by occupation and geographic region (often county). A much smaller proportion of the CPS-ORG data is available to describe these wage distributions. This data requirement drives the major difference between the estimation strategy used in the minimum wage analysis and the prevailing wage analysis. To estimate the impact of state prevailing wage law repeals on wages, I use quantile regression on individual-level data. The need to estimate low (10th) and high (90th) percentiles conditioned on occupation, industry, geographic region and time prohibits the use of mean regression techniques on aggregated, state-level observations. That is, because estimating percentiles in the tails of the wage distribution conditioned on occupation, industry, geographic region and time requires a large number of individual observations, individual observations have to be pooled across states and across years. In the case of the prevailing wage analysis, then, I cannot construct a data set comprised of state-level half-year observations as in the minimum wage analysis. Rather, I have to construct a data set from observations aggregated over multiple states and years. Such aggregations produce insufficient observations for mean regression.[15]

An alternative technique would be to evaluate the statistical difference between the following two quantities for various wage percentiles: 1) the ratio between the change in wage percentile among construction workers to the change in the wage percentile of blue collar workers within states that repealed their prevailing wage laws over the time they repealed their laws and 2) the same ratio within states that did not repeal their prevailing wage laws, over the same time period. Statistically significant differences between these ratios would indicate movement in wages specifically associated with the repeal of the state prevailing wage laws. Quantile regression provides an elegant presentation of, substantively, this statistical comparison.

Note the importance of analyzing the wage impacts by occupation. Because occupations vary in their position in the overall construction industry wage distribution (e.g., laborers are situated at the lower end and carpenters at the higher end), workers' wage position relative to their respective wage floor will differ from their wage position relative to the bottom of the construction industry wage distribution. Therefore analyzing wage effects at different points in the construction industry wage distribution across occupations will not reveal how the law repeals affect workers near or far from their respective wage floor. The occupations carpenters and laborers are singled out primarily because of their predominance in the industry and thus, relatively large sample sizes. However, analyzing these two occupations also have the appealing quality of differing in average wage level. Within the construction industry, as well as across blue collar occupations, carpenters have relatively high wages and laborers relatively low wages.

Thus, I will be able to examine how prevailing wage law repeals affect both low-wage and high-wage occupations.

One similarity between these two occupations, however, is that both are unlicensed occupations. As a result, the entry of workers into these occupations is likely to be easier than licensed occupations in construction, such as plumbers and electricians. Recall that the theories that predict ripple effects from changes in the mandated wage floor depend, in part, on the ability of workers to resist changes in the size of their relative wages. If the supply of labor is limited to an occupation due to licensing requirements, the ability for workers to resist such changes increases. To the degree that labor supply is readily available for unlicensed occupations, the ability of workers to resist such changes decreases. Similarly, if the cause of ripple effects is the substitution of higher-skilled workers for lower-skilled workers when the mandated wage floor is increased, then a limited labor supply of higher-skilled workers will cause their wages to increase more than if the labor supply is plentiful. Thus, the relative ease of entry into these occupations may dampen the potential for ripple effects.

I use quantile regression to estimate a simple DDD model. Before proceeding, I briefly present the basic model below to facilitate the remainder of this discussion:

$$
\begin{aligned}
(3.1)\quad \ln(\text{wage}_{ist}) = \alpha \quad & + \beta_1(\text{construction}_{ist}) + \beta_2(\text{repeal state}_s) + \beta_3(\text{after repeal}_t) \\
& + \beta_4(\text{construction}_{ist} \text{ x repeal state}_s) \\
& + \beta_5(\text{construction}_{ist} \text{ x after repeal}_t) \\
& + \beta_6(\text{repeal state}_s \text{ x after repeal}_t) \\
& + \beta_7(\text{construction}_{ist} \text{ x repeal state}_s \text{ x after repeal}_t) + \varepsilon_{ist}
\end{aligned}
$$

where the subscripts i, s, and t denotes the individual, state, and time, respectively. Each of the variables in this model is an indicator variable, where construction=1 if a worker is a construction workers, 0 otherwise; repeal state=1 if a state repealed its state prevailing wage law, 0 otherwise; after repeal=1 if year is 1988 or later, 0 otherwise.

The sample is limited to blue collar workers. The model is estimated separately for a sample of blue collar workers which excludes all construction occupations except carpenters and a sample blue collar workers which excludes all construction occupations except laborers.

In order to control carefully for time-specific trends, only states that repealed their prevailing wage laws roughly around the same time are included in this analysis. As a result, five repeal states were used in this analysis: Colorado, Idaho, Kansas, Louisiana, and New Hampshire repealed their laws between 1985 and 1988. Because Alabama, Arizona, Florida, and Utah repealed their prevailing wage laws during 1979 to 1981 they were excluded from the sample of repeal states. Alabama and Utah were also excluded from the sample of control states (states that experience no change in the prevailing wage laws over 1985 to 1988) because their state prevailing wage laws were repealed during the “before” period (1980 and 1981, respectively). Also, because Alabama’s prevailing wage law was actually a mandated wage ceiling rather than a wage floor it is inappropriate to use in an analysis of changing wage floors. Because past research (Kessler and Katz 2001; Petersen and Godtland 2005) indicates that the wage effects from the state repeals may lag the repeal by three to five years, Arizona and Florida were also excluded from the sample of control states as both repealed their state prevailing wage laws in 1979.

Two sets of time intervals are used in the analysis. The first interval includes the years 1980 to 1984 and 1988 to 1992, where 1988 to 1992 make up the “after treatment” years (see table 3.1 for the dates of state repeals). A second time interval is also examined. While the “before” years remain the same (1980 to 1984), the after years are shifted one year to 1989 to 1993. Two time intervals are used for the following reasons. The primary purpose of using two different sets of “after” years is that the second time interval allows for the effect of prevailing wage law repeals to lag their enactment by an additional year. For state such as Kansas and Louisiana, the second time period allows for a lag of 2 to 5 years and 1 to 4 years respectively (the other states will have a lag of up to 4 to 8 years). Research by Petersen and Gotland (2005) suggests that the most significant negative wage effects caused by state repeals take place five years after their enactment. Shifting the “after” period by one year will allow for a stronger lagged effect. Shifting the after period also provides a modest robustness check for the regression coefficients. If the results are sensitive to the years used, this may suggest that the regression coefficients are picking up the affect of some other factor, as opposed to the change in policy status.

As with the minimum wage analysis, the repeal effect is estimated at various points in the wage distribution: the 10th, 25th, 50th, 75th and 90th percentiles of the wage distribution conditioned on the covariates listed above. Note that because there are only two time periods, the multiple time series issues that arose for the minimum wage analysis are not present here.

As mentioned above, to reliably measure wage percentiles of wage distributions conditioned on industry, occupation, geographic region, and time, these wage distributions must be based on observations pooled across states and across years. The indicator variables described above pool the data into aggregate groups.[16] The minimum number of observations used to estimate the wage percentiles is 319.

The quantile regression technique, and its limitations, was introduced briefly in section 2.3.2. The primary disadvantage of using this technique, as discussed above, is the trade-off between controlling for various demographic characteristics and precisely estimating ripple effects. To review, the quantile regression fits the regression line through percentiles of the *conditional* wage distribution. Because I am interested in observing whether the wage effect of the prevailing wage law repeals depends on the relative wage position of workers to the prevailing wage floor (i.e., the minimum wage rates mandated by the prevailing wage laws), it is important that the percentiles of the conditional wage distribution used in the analysis are consistent in their position relative to the prevailing wage floor. The percentiles of wage distributions conditioned on demographic characteristics (e.g., union status), unfortunately, are not uniform in their position relative to the wage floor. To repeat the example above, the difference between the 5th wage percentile of union workers and the prevailing wage floor set by prevailing wage laws is not likely to be the same as the difference between the 5th wage percentile of nonunion workers and the prevailing wage floor. In fact, to precisely estimate ripple effects, the only covariates that should be included in the quantile regression are those which are used to define the wage floor (e.g., state, industry, occupation, time). This way, the percentiles of the conditional wage distribution are consistent across workers with respect to the workers' wage position relative to the wage floor. Fortunately, this shortcoming can be dealt with in a couple of ways.

First, while individual-level demographic controls are not included, analyzing state-level prevailing wage law repeals makes it possible to control for within state demographic changes that are unrelated to changes in state prevailing wage law changes: a control for within-state labor market trends among non-construction blue collar workers can be included because only construction workers are covered by the state prevailing wage laws. This type of control variable was not available in the minimum wage analysis because of the virtually universal coverage of state minimum wage laws.

Second, the controls that are included in the model are flexibly specified. The construction variable controls for differences, across state and time, between the construction industry and other industries in which blue collar workers are employed. The after-repeal variable controls for macroeconomic trends experienced by blue collar workers over this time period. The repeal state variable controls for labor market differences between states that repeal their prevailing wage laws and those that do not. To allow the indicator variables to account for these differences correctly, the model is estimated separately for a sample with observations from repeal states and states that consistently have state prevailing wage laws and a sample with observations from repeal states and states that consistently do not have state prevailing wage laws. So that all in all, four samples are used to estimate this model (see table 3.4).

The interaction terms provide further flexibility for each of these variables to capture wage trends specific to repeal states or differences between local construction labor markets. The interaction term "construction x repeal state" accounts for differences between the construction industry in repeal states versus the construction industry in other states. The interaction term "repeal state x after repeal" accounts for differences over time that are specific to the blue collar labor market in repeal states. The interaction term "construction x after repeal" captures wage trends over time within the construction industry nationally. The last variable is the variable of interest: the coefficient β_7 on the interaction term "construction x repeal state x after repeal" captures the change over time in construction wage premiums of construction workers in repeal states versus construction workers in other states.

A couple final comments should be made with regard to the methodology used in this chapter. First, because the wage floor of state prevailing wage laws is varied across occupations, counties, and type of construction (heavy, highway, building, or residential) measuring precisely the extent and size is more difficult than in the case of the minimum wage. This is due to the fact that the prevailing wage rates are not available in the data so the location of the wage floor is not known precisely. The varied nature of the wage floor levels and prevailing wage determination rules discussed above requires that the researcher have the exact prevailing wage rate schedules in order to identify their location in the wage distribution. This data requirement is prohibitive for a national study. Additionally, data on actual coverage (that is employment on covered construction contracts) is not available in nationally-representative data such as the CPS. This is in contrast to state and federal minimum wage laws which, as noted earlier, one can assume universal coverage of minimum wage laws. As a result, this examination of how the wage distribution changes when the state-level prevailing wage floor is removed will provide general contours of its overall impact—possibly including a ripple effect—but will produce less precise observations on its extent and size compared to the minimum wage analysis in chapter two.

Second, it is important to be clear about what the quantile regression is estimating in this DDD model. The estimates of the model generated by quantile regression are essentially estimates of quantile treatment effects, where the treatment is "removing a state prevailing wage law." Quantile treatment effects, however, are different from treatment effects. In the context of state prevailing wage law repeals, the quantile treatment effect is the difference between the median wage, for example, of states with state prevailing wage laws and the median wage of states without state prevailing wage laws. The model above simply adds controls for national, state, and industry trends and state and industry levels, to account for spurious effects. Treatment effects, on the other hand, are the difference between outcomes for a given individual.

The importance of this difference can be illustrated with two simple examples presented in table 3.5. First, take a set of 10 workers with the following distribution of wages: two workers earn \$5 per hour, two earn \$7 per hour, two earn \$10 per hour, two earn \$12 per hour and the final two earn \$15 per hour. In this set of workers, the 75th percentile wage is \$12 per hour. If the two \$12 per hour workers experience wage decreases to \$7 per hour then the 75th percentile falls to \$10 per hour and the 50th percentile falls to $7 per hour (see column 3). The *wage structure* changes such that the 75th percentile declines by $2 and the 50th percentile falls by $3. That is, the array of wages offered by employers changes such that the upper wages are somewhat lower than before, but do not reflect the individual workers' larger negative wage changes – a decline of $5 per hour. While the range of the negative wage effect reflects the individual wage effects (fall in wages from the former 75th percentile to the new 50th percentile, or $12 to $7) , this scenario cannot be distinguished from a scenario where the individual workers earning $12 per hour received a reduction in wages to $10 per hour and workers earning $10 per hour received a reduction in wages to $7 per hour. In other words, the magnitude of the treatment effect cannot be directly observed from the magnitude of the quantile treatment effect.

Consider a second scenario in which the $12 per hour workers again see a decline in their wages to $7 per hour (column 4). At the same time, say that workers earning $5 per hour see an increase in employers' demand for their labor. In this case then, the 75th and 50th percentiles fall as before, but the 25th percentile now rises to $7 per hour from $5 per hour. In this example, positive wage movements do not offset negative wage movements. This example is relevant to the repeal of state prevailing wage laws because workers who earned wages less than that received by workers covered by the prevailing wage laws may see their wage rise after the repeals due to substitution effects. However, these lower-wage workers are unlikely to see their wages exceed the new, lower wage rates of previously covered workers. This scenario is unlikely given that it would imply that employers are now willing to pay higher wages, *relative to formerly covered workers*, to workers that they had previously paid lower wages to. Employers are more likely to increase their demand for "less-skilled" labor up to the point that their wages equal the wages of formerly covered workers (depending on the degree to which there are skill differences between the two groups). As a result, in examining the wage effects of the state prevailing wage law repeals, I can assume that negative individual wage effects will be detected by declines in the wage percentiles even though the magnitude of the individual wage effects cannot be directly observed.

To provide a more systematic account of the difference between quantile treatment effects and treatment effects, I reproduce parts of Bitler, Gelbach, and Hoynes' (2003) discussion on this topic. Let $Y_i(T)$ be the outcome variable I am interested in where T=1 if individual i is treated and T=0 if individual i is not treated. A treatment effect is defined to be: $\delta_i \equiv Y_i(0) - Y_i(1)$ for a given individual i. Treatment effects are observed with the joint distribution of $Y_i(0)$ and $Y_i(1)$. In the context of the repeal of state prevailing wages, given this joint distribution I would be able to generate a direct view of how the prevailing wage law repeal impacts the wages of workers according to their relative wage position before treatment. I would do this by sorting the treatment effects according to the distribution of $Y_i(O)$. However, in general, only marginal distributions can be observed, $F_0(y)$ and $F_1(y)$, where $F_T(y) \equiv \Pr[Y_i(T) \leq y]$. Quantile treatment effects are based on these marginal distributions. Define the qth quantile of distribution $F_T(Y)$ to be $y_q(T) \equiv \inf\{y: F_T(y) \geq q\}$. The qth quantile treatment effect then is: $\Delta_q = y_q(1) - y_q(0)$.

In the absence of direct knowledge of the joint distribution, the characteristics of the marginal distributions can be used to make observations about treatment effects. Bitler, Gelbach, and Hoynes (2003) note two special cases when the marginal distributions would be sufficient to describe the joint distribution: (1) If the treatment effect is constant across the distribution of observations, then $\Delta_q = \delta_q$ and (2) If the rank of observations is preserved after treatment then $\Delta_q = \delta_q$. As they explain:

> Under rank preservation [as in both cases above], any person whose outcome in the counterfactual control distribution is the qth quantile will also have an outcome that is the qth quantile in the counterfactual treated distribution. It then follows that Δ_q and δ_q are equal. (Bitler, Gelbach, and Hoynes 2003, 17)

Therefore, if there is rank preservation, the quantile regression estimates of the above model will provide estimates of not only the quantile treatment effects but the treatment effects also.

In the case of minimum wages, it is likely that rank preservation holds true because of its universal coverage and small changes in the wage floor. Therefore, quantile treatment effects are likely to be equivalent to treatment effects. In the case of prevailing wage laws, however, rank preservation is unlikely. If the state law repeals result in 20 percent of construction workers in repeal states experiencing wage decreases on the order of 30 percent (the estimated magnitude of the mandated wage raises caused by prevailing wage laws), rank preservation is clearly improbable.

In the more general case where the assumption of rank preservation does not hold, only general contours of the treatment effects can be observed: (1) Fix a quantile q*. The minimum treatment effect δ_q for all $q \geq q^*$ is no larger than the smallest quantile treatment effect Δ_q for all $q \geq q^*$. Thus if any quantile treatment effect is negative, at least one treatment effect is negative. (2) The logical inversion also holds: Fix a quantile q*. The maximum treatment effect δ_q for all $q \leq q^*$ is no smaller than the maximum quantile treatment effect Δ_q for all $q \leq q^*$. Thus if any quantile treatment effect is positive, at least one treatment effect is positive. In other words, if negative quantile treatment effects are observed then there are negative treatment effects. For example, if a negative quantile treatment effect is estimated for the 75th wage percentile of construction laborers due to the state repeals, then I can conclude that at least one construction laborer that is positioned at or above the 75th wage percentile before the repeals occur, experienced a negative wage effect of at least the magnitude of the estimated quantile treatment effect.

While some information about treatment effects, therefore, can be gleaned from the quantile regression results without rank preservation, further assumptions need to be made to make conclusions about their actual size and specific location (that is, the affected workers' prior relative wage position).[17] Given this, it is important to underscore the fact that the quantile regression results are estimates of changes to the wage structure rather than estimates of the size and location of treatment effects. In other words, quantile regression estimates are limited to identifying what parts of the wage *structure* are affected as opposed to measuring the impact of the state repeals on individual workers. However, as discussed above, the assumption that employers will not increase the wages of workers who earned relatively low wages prior to treatment beyond the wage levels of workers who earned relatively high wages prior to treatment allows me to narrow the potential range of the size and location of treatment effects. Therefore, contingent on this assumption, the quantile regression estimates provide the contours of the size and location of treatment effects.

An alternative technique to identify the size and location of treatment effects is to approximate individual treatment effects by estimating wage effects for subsamples of workers that vary by wage level (assuming that treatment effects depend largely on wage level). These subsamples can then be used to estimate *group* treatment effects. Recall that wage effects cannot be properly estimated by mean regression on samples divided by wages without introducing bias (see section 2.3.2). Thus, the subsamples cannot be defined by wages directly, wage covariates must be used instead. I use union status and potential labor force experience to divide the sample. I define four subsamples with these two characteristics. Figure 3.1 illustrates the way that union status and experience level divide workers by wage level. Average wages are presented for three groups of states, divided into the four subgroups used in the mean regression analysis. The top panel presents average wages for states that kept their prevailing wage laws (PWL states), the middle panel presents average wages for states that repealed their prevailing wage laws (Repeal states), and the bottom panel presents average wages for states that did not have their own prevailing wage laws (No PWL states) during the "before" years. Also, the 95 percent confidence intervals are indicated in these figures by the brackets that overlay each bar. Moving from left to right (excluding the averages for all workers), the average wages rise and the difference in average wages are statistically significant for all groups except for the more experienced union workers in No PWL states. Therefore, these covariates of wages, union status and potential labor force experience, provide a way to divide up the sample by relative wage position without causing a sample truncation problem. However, as noted earlier, using covariates to divide the sample produces less precise estimates of the location of wage effects in the wage distribution because the covariates are not a direct measure of wages.

If the magnitude of the repeal effect on workers' wages is primarily contingent on workers' relative wage level, the mean regression estimates of the repeal effect should be roughly equivalent to the magnitude of the effect multiplied by the proportion of workers affected. As a result, two other aspects of the repeal effect can be deduced from the mean regression estimates. If an assumption can be made about the proportion of workers affected within a particular subgroup, the magnitude of the treatment effect can be deduced from the mean regression coefficients. Alternatively, if an assumption can be made about the magnitude of the repeal effect, the proportion of affected workers within a subgroup can be deduced from the mean regression coefficients.

This analytic strategy depends on the assumption that union status and potential work experience—the characteristics that define the subgroups— largely determine individual wages. To the extent that this is not the case, the estimated treatment effects by union/experience subgroup may be caused by individual-level variations in other characteristics related to wages. For example, consider individuals who move out of the union sector after the state repeals. These workers may have received more formal training than individuals who are consistently in the nonunion sector, even though they may have similar levels of work experience. In this case, as former union workers enter the nonunion sector, wages in the nonunion sector may actually rise after the state repeals – even if the wages of workers that are consistently nonunion experience a wage decline. Thus, the mean regression effectively estimates "subgroup" treatment effects rather than the negative individual treatment effects experienced by workers that are consistently nonunion or that have left the union sector. However, union workers tend to have greater potential labor force experience (see table 3.8) indicating that if wage differences between union workers and nonunion workers are due to other unobserved skill differences, experience level may act well as a proxy for these skill differences. In other words, if workers change union status, their skill differences will continue to differentiate former union workers from workers who are consistently nonunion (i.e., by categorizing former union workers in the highly experienced subgroup of nonunion workers).

Use of union status introduces two major modifications to the data used in the analysis. First, because union status is only available for all outgoing rotation groups beginning in 1983, the years used in this analysis are restricted to 1983 to 1984 for the "before" period. To construct a comparable two year "after" period, I used the years 1988 to 1989. These years have the appealing quality of preceding the federal minimum wage increases that take place in 1990 and 1991, the effects of which may be difficult to isolate because the subgroups of workers analyzed are likely to be affected differently by minimum wage changes (e.g., construction laborers in states that repeal state prevailing wage laws are likely to experience a greater positive impact from the increased federal minimum wage than blue collar workers within those states). Although the same argument could be made for the fall in real value of the federal minimum wage over the late 1980s, the relatively short time interval between 1984 and 1988 minimizes this effect. Also, the last federal minimum wage increase was two years prior to 1983 so that some of the decline in real value of federal minimum wage already occurred. In order to provide a robustness check as well as to allow for a greater lagged effect (as discussed above), a second two-year interval is also used for the "after" period: 1991 to 1992. Second, the limited availability of data on union status reduces sample sizes significantly. As a result, observations are pooled across construction occupations as opposed to estimating the model separately for carpenters and construction laborers. Using mean regression on these smaller samples requires fewer observations than quantile regression to produce relatively robust estimates.

As mentioned above, previous work done by Kessler and Katz (as well as, O'Connell) suggests a strong link between union status and the impact of the repeal of state prevailing wage laws. In fact, they find that the repeals reduce the construction union premiums by approximately one-half, a reduction of roughly 10 percentage points (Kessler and Katz 2001, 261). The question about whether this effect is relatively large in scope, rippling across the union sector, or limited to a subset of workers has yet to be answered. I attempt to answer this question by estimating the average treatment effect for subgroups within the union sector.

3.3 Results

3.3.1 Descriptive Analysis

I begin with a descriptive analysis of the workers in the sample to provide some context for the regression results that follow. In particular, I present statistics by state prevailing wage law status to explore other differences, besides prevailing wage laws, between these states' labor markets. In table 3.6, I present demographic characteristics for construction workers by state group, where the state groups are defined by state prevailing wage policy status. The state groups are referred to as Repeal states, PWL states, and No PWL states for states that repealed their state prevailing wage laws during 1980 to 1992, states that have state prevailing wage laws throughout 1980 to 1992, and states that did not have state prevailing wage laws throughout 1980 to 1992, respectively.[18]

What is immediately striking is that this grouping is economically meaningful with regard to wages. Looking across the first row, the median construction wages by state group reveal that construction workers earn lower wages in states without state prevailing wage laws compared to states with state prevailing wage laws. The wages of workers in Repeal states, on the other hand, fall somewhere between those of No PWL states and PWL states.

Several characteristics of the construction workforce may contribute to these differences in wage level. First, varying levels of skill may play a role. Note that construction workers in PWL and Repeal states are more likely to have graduated from high school than workers in No PWL states. Also, union density is highest in PWL states and lowest in No PWL states. Finally, to the extent that discrimination plays a role in lowering wages, the greater than average proportion of nonwhite workers in No PWL states (the majority of which are located in the South) may contribute to the relatively low construction wages in No PWL states.

Differences in wage levels are also linked to differences in the more general blue collar labor market within each state group. In fact, the state grouping is broadly consistent with constellations of labor market institutions that are generally viewed as more or less “labor-friendly.” These labor market institutions include Right-to-Work laws,[19] state minimum wage laws, and union density (see table 3.7). Among the PWL states, only 6 (out of 33) have Right-to-Work laws, 14 have had state minimums that exceed the federal minimum wage level, and the average unionization rate is the highest among the three state groups at 34 percent. Correspondingly, the median blue collar wage in PWL states—$6.97—is also the highest among the three state groups. In contrast, all of the No PWL states have Right-to-Work laws,[20] only two out of the eight states have had state minimums that exceed the federal minimum wage level, and No PWL states have the lowest average unionization rate (15 percent) and the lowest median blue collar wage ($5.62). Repeal states fall somewhere between PWL states and No PWL states on all the dimensions except the state minimum wage, in which case, none of the Repeal states enacted a state minimum that exceeded the federal during these years. State prevailing wage laws, then, exist alongside a set of labor market institutions that tend to raise workers' wages. Wage differences among construction workers (see table 3.6), who comprise a significant proportion (14 percent) of the blue collar workforce, tend to correspond to these differences in labor market institutions.

The basic demographic characteristics presented in table 3.6 are presented for union and nonunion construction workers separately in table 3.8 to examine demographic differences across union status. Among these characteristics—race, gender, potential labor force experience, and education—union construction workers possess more wage-enhancing qualities. While larger differences exist along the lines of experience and education, union construction workers also have lower proportions of nonwhite workers and lower proportions of female workers (although the construction industry is overwhelming male across states). This comparison of demographic characteristics across state groups illustrates how union construction workers are different from nonunion construction workers and that skilled workers are concentrated in the union sector. Therefore the variation in average wage levels across these state groups (which vary in union density) may also be, in part, due to differences in labor market institutions that influence the type of workers that are employed in the construction industry.

Important differences also exist between the occupations. Table 3.9 provides some details about how the two construction occupations focused on in this study differ.

Laborers and carpenters are situated at different parts of the construction industry wage distribution. Construction laborers sit at the low-end of the construction industry's wage distribution with a median wage ($6.03) well below the median wage for all construction workers ($7.75). Similar to low-wage occupations outside construction, construction laborers have an overrepresentation—relative to the total construction industry—of minority workers and female workers. In contrast, carpenters have a median wage ($7.57) close to that of the median construction wage and minority and female workers are underrepresented—again, with respect to total construction industry—among carpenters. Union density, however, varies more by state group than by occupation.

These descriptions of the construction industry labor markets by occupation and state prevailing wage law status provide the backdrop against which to consider the effects of the prevailing wage law repeals presented below. These details also indicate that the labor markets of both the No PWL states and the PWL states differ in important ways from the Repeal states. In this way, neither group of states serves as an ideal control group. However, because No PWL states and PWL are quite distinct from each other, using each group as a control will potentially account for a wide variety of labor market trends that are different from, but take place at the same time as, the state prevailing wage law repeals. The PWL states, for example, may control well for the decline in wages among union workers in general, whereas the No PWL states will not. No PWL states, on the other hand, may control well for real wage changes associated with changes in the federal minimum wage (which declines during the 1980s as well as increases in the early 1990s).

I present a final set of descriptive analysis to anticipate the regression results that follow. In figure 3.2 I provide kernel density estimates of the wage distributions of laborers, carpenters, and blue collar workers (outside construction) from Repeal states. Wage distributions from the years before and after the state law repeals are displayed. These kernel density figures provide preliminary observations of how the wage distributions of construction workers in Repeal states appear to be affected by the removal of their state prevailing wage floor.

In the top panel of figure 3.2, kernel density figures are presented for the time periods "Before Repeal" (1983 to 1984) and "After Repeal" (1988 to 1989). More specifically, the following wage distributions of Repeal state workers are displayed: (1) all construction laborers, (2) nonunion construction laborers, and (3) union construction laborers.[21] The analogous figures of carpenters are presented in the middle panel. To put these figures in context of overall trends occurring simultaneously, the bottom panel provides figures for non-construction blue-collar workers in Repeal states.

Starting with the bottom panel, the overall wage distribution of blue collar workers does not experience a visually striking change over this time period but does shifts toward lower wages. Each of the subgroups (union and nonunion blue-collar workers) also experience some wage decline, however, this occurs primarily among low-wage nonunion workers. Union workers, amassed in the top half of the wage distribution, appear to decline in overall density rather than experiencing a marked shift toward lower wages. The blue collar labor market in the Repeal states thus exhibit a decline in real wages due to the fall in real wages among low-wage workers (likely a product of the falling real value of minimum wages) and a decline in wages at the high end of the wage distribution due to a decline in union density.

Looking now at the construction laborers, some similarities with the blue-collar wage trends arise. The overall wage distribution shifts toward lower wages, but this appears to occur across union status. There is again a decline in wages at the top of the wage distribution, primarily due to the fall in union density among construction laborers. This shift is also, however, due to a shift in the concentration of union wages toward lower wages indicating that higher-than-average union wages, in particular, fall over this time period. Interestingly, the density of nonunion workers increases in the second time period at the top end of the wage distribution, filling in some of the gap caused by the declines in the union sector (however, even this segment of the nonunion wage distribution represents lower wages with respect to the union wage distribution prior to the repeal).

The pattern described above is suggestive of union laborers leaving the union sector but remaining in construction (i.e., highly skilled union workers work take jobs with nonunion contractors and thus appear in the high end of the nonunion wage distribution). This trend is consistent with qualitative data collected by Azari-Rad et al. (1994), on Utah construction workers after Utah repealed its state prevailing wage laws in 1981. Quoting from an interview with a construction contractor, they observed that, "

> …there were a lot of union workers that carried their card in their shoe. They worked open shop until a union job came available. A lot of folks, all of a sudden started to find homes over there [in the open shop] and never came back.

The authors then conclude:

> Consequently, contractors that remained union did not have a significant labor productivity advantage over many of the newly nonunion contractors. This

> effectively forced remaining union contractors out of much of the construction market. (Azari-Rad, Yeagle, and Philips 1994, 210)

Such a trend reinforces the impression that union workers, in particular, benefit from prevailing wage laws and, correspondingly, suffer from prevailing wage law repeals.

Carpenters provide a striking example of the negative impact of the state law repeals. While the wage distribution of all construction carpenters loosely follows the pattern of blue-collar workers in Repeal states more generally, there is a marked shift in the wage distribution of union carpenters. In the initial time period their wage distribution is skewed to the left, so that a greater proportion of wages are above average. In the second time period, the situation is reversed: their wage distribution is skewed to the right so that a greater proportion of wages are below average. This departs from the trends among blue collar workers.

These kernel density figures point to significant negative wage effects resulting for union construction workers when states repeal their prevailing wage laws. In particular, wages at the higher end of the union wage distribution appear to fall. Because the intent of prevailing wage laws is to uphold a relatively high-wage norm, this pattern of effects seems likely to be caused by the prevailing wage law repeals, as opposed to some other economic trend.

3.3.2 Quantile Regression Estimates of State Repeal Effects on the Construction Industry Wage Structure

Figures 3.3 and 3.4 present the quantile regression estimates of the impact that state prevailing wage law repeals have on the 10th, 25th, 50th, 75th and 90th wage percentiles for laborers and carpenters separately, and over the two different time intervals (1980 to 1984 and 1988 to1992 in figure 3.3 and 1980 to1984 and 1989 to 1993 in figure 3.4) described above. In each figure, the top panel presents estimates using PWL states as the control states and the bottom panel presents estimates using No PWL states as the control states. To put the estimates of β_7 in context, an additional model was estimated:

$$(3.2)\ \ln(\text{wage}_{is}) = \gamma + \delta_1(\text{construction}_{is}) + \delta_2(\text{repeal state}_s) + \delta_3(\text{construction}_{is} \text{ x repeal state}_s) + \varepsilon_{is}$$

This model was estimated separately with observations from the "before" years and the "after" years. The estimates of δ_3 provide a measure of the level differences between the construction premiums of Repeal states and other states before and after the state repeals. The β_7 coefficients, on the other hand, measures how these relative level differences change over time. Each panel presents, in this order, estimates of $\delta_{3,\text{Before}}$, $\delta_{3,\text{After}}$ and β_7. Note that β_7 is equivalent to $(\delta_{3,\text{ After}} - \delta_{3,\text{ Before}})$. Tables 3.10 and 3.11 present these estimates along with bootstrapped standard errors.[22]

Looking at the results for construction laborers first, the dark grey bars representing the estimates for $\delta_{3,\text{ Before}}$ indicate that the construction premiums for the 75th and 90th percentiles, in particular, are lower in Repeal states when compared to PWL states, whereas the construction premiums for the lower percentiles are relatively similar. This pattern is also reflected when comparing the wage distribution of laborers in Repeal states to the wage distribution of laborers in No PWL states. The differences in construction premiums at the 75th and 90th wage percentiles stand out as being relatively large but with the opposite sign. These patterns largely reflect that, after controlling for blue collar labor market differences, much of the difference between construction wages across these state groups is specific to top portion of the wage distribution. That is, whereas differences in construction wages at the 10th through the 50th wage percentiles across states are accounted for by differences between states' blue collar labor markets, differences between the highest construction wages are specific to the construction industry. Because union workers are concentrated in this high wage range (see table 3.8), these differences in construction premiums reflect, in part, differences in construction premiums obtained by union workers in particular. In other words, the negative difference in construction wage premiums between the high-wage laborers in Repeal states and PWL states are likely due to the greater construction premiums obtained specifically by union construction workers in PWL states. Analogously, the positive difference in construction wage premiums between the high-wage laborers in Repeal states and No PWL states are likely due to the greater premiums obtained specifically by the union construction workers in Repeal states. I explore this further below.

The quantile regression results consistently associate a negative wage effect on laborers' wages at the 75th wage percentile with the repeal of states' prevailing wage laws. The estimated magnitude of this effect ranges between -0.05 to -0.13, so that the 75th wage percentile falls by 5 to 12 percent after the prevailing wage laws are repealed. Or, put another way, the presence of state prevailing wage laws raises the 75th wage percentile by roughly 5 to 12 percent.

These magnitudes fall at the low-end of past research estimates of the wage raises associated with prevailing wage laws (see table 3.3). This is unsurprising given that the wage effects on the wage structure are likely to be smaller than the wage effects experienced by individual workers. While the estimates are consistently negative across time periods and control groups, only the estimates using the 1989-1993 “after” time period are statistically significant at conventional levels. The negative effect may be stronger using the 1989-1993 “after” time period because of the increased lag time allowed. These consistently negative wage effects – across control groups and across time periods – are mildly supportive of the hypothesis that the repeal of state prevailing wage laws have some negative wage effects. More specifically, these negative wage effects are primarily experienced by workers in the top quartile of the wage distribution.

The regression estimates produce another consistent pattern of effects across the construction laborers’ wage distributions. Note that while wages at the 90th wage percentile of Repeal state construction laborers varies from the 90th wage percentile of No PWL laborers and PWL laborers for reasons other than differences between their blue collar labor markets, these wage levels in the wage structure appear to be unaffected by the state repeals. In other words, the top of the wage structure does not shift with the removal of the prevailing wage floor. This result implies that having a state prevailing wage law does not impact the wages at the top of the wage distribution.

Finally, there is a positive wage effect measured at the 25th wage percentile of laborers (figure 3.4). Such an effect is consistent with the substitution of low-wage workers for high-wage workers in previously covered jobs that is more likely to lag the enactment of the repeals. Though these effects are measured too imprecisely to be statistically significant, they do appear across state control groups when using the 1989-1993 “after” time period.

Turning now to construction carpenters, a similar pattern of differences between construction workers across the three state groups appears. First, the top wages of construction carpenters in Repeal states are substantially (and statistically) lower than those among construction carpenters in PWL states. Compared to No PWL construction carpenters, Repeal construction carpenters have higher wages, statistically, but this time the construction premiums are higher across the entire wage distribution indicating a more generalized difference between the labor market of Repeal state carpenters and that of No PWL state carpenters.

The quantile regression estimates of the repeal effect on the top wage earners in Repeal states are consistently negative across the control groups and across time periods, and range between -0.06 to -0.14 (a decline in the wage percentiles of 6 to 13 percent). The negative effect associated with the state repeals is observed at both the 75th and 90th wage percentiles. The state repeals roughly doubles the size of the gap between the top wage earners in Repeal states as compared to their counterparts in PWL states, based on either time intervals. Relative to No PWL states, the positive difference in construction premiums is at least halved. An example of the magnitude of the effect in terms of levels illustrates the economic significance of these changes: A 13 percent decline in the 90th wage percentile for construction carpenters in Repeal states represents an hourly rate reduction of $1.58, from the counterfactual level of $12.41 to $10.83 after the state repeals. Again, given that union carpenters are concentrated in these top wage percentiles, this negative effect likely reflects a reduction in the union construction premium in particular.

Although the pattern of effects is consistent, these estimates cannot be statistically differentiated from zero due to the imprecision of these estimates. Also, the negative impact on 90th conditional percentile wages may be overstated when using the 1988 to 1992 “after” time period. These estimates decrease with the alternative “after” time period or 1989 to 1993 suggesting that the wages of carpenters in Repeal states were able to rebound slightly or that the larger estimated negative impact of the repeals reflect other concurrent wage effects during 1988 to 1992. This stands in contrast to the estimates for construction laborers who experience a greater decline in wages associated with the repeals when the additional one-year lag is allowed.

There also appears to be a negative effect at the bottom of the wage distribution. The estimate of β_7, though also not statistically significant, is a sizable -0.07 when using the 1988-1992 “after” time period and the No PWL states as the control states. This negative effect decreases in magnitude somewhat when using the 1989-1993 “after” time period and is inconsistent with the estimates produced when using the PWL states as the control states. The inconsistency of this negative effect across control groups suggests that a trend specific to the construction industry in No PWL states may be causing an increase in the construction premium among workers in such states that is not experienced by either the construction industry in the Repeal states or the PWL states.

Overall, these estimates of the impact on the overall wage structure are surprisingly focused given the potentially varied nature of the wage floors mandated by state prevailing wage laws. In particular, negative wage effects are limited to the top quartile of wages. The effects are observed at the 75th and 90th wage percentiles of construction carpenters and the 75th wage percentiles of construction laborers. Despite the consistency of the results, they provide only weak evidence of such patterns of effect due to the relatively large bootstrapped standard errors relative to the coefficient magnitudes.

3.3.3 Mean Regression Estimates of State Repeal Effects on the Construction Industry Wage Structure

I turn now to the mean regression results which provide estimates of the state prevailing wage law repeals for subsets of workers, divided by union status and potential labor force experience (see figures 3.5-3.6 and tables 3.12-3.13). As described above, I estimate the impact of state prevailing wage repeals separately for two different potential labor force experience groups and by union status. The panels in the figures present regression coefficients for $\delta_{3,Before}$, $\delta_{3,After}$ and β_7 as in the quantile regression analysis, but this time the coefficients are estimated using mean regression.[23] The first set of bars in each panel presents the coefficients estimated across experience levels. The next two sets of bars are estimated for two subgroups of workers based on their level of potential labor force experience (15 years or less and greater than 15 years). The top panels present coefficients for the entire sample. The middle panels present coefficients for nonunion workers only and the bottom panels presents coefficients for union workers only. As before, the control groups vary by state type as well as by time interval. Two sets of time intervals used here are 1983 to 1984 and 1988 to 1989 (presented in figures 3.5 and 3.6 and table 3.12) and 1983 to 1984 and 1991 to 1992 (presented in figures 3.7 and 3.8 and table 3.13).

At first glance, the pattern across the panels reveals that the construction premiums for nonunion workers vary little across state types. Given that unconditional medians for construction workers by state type do vary (see table 3.6), it is interesting to note that construction wages premiums among nonunion workers—construction wages *relative to* blue-collar wages—within each state grouping are fairly consistent before the state repeals. This is evidenced by the small magnitudes of the estimates for $\delta_{3,\text{Before}}$ across the middle panel (none exceed 0.05). That is, construction wage premiums for nonunion workers differ by less than five percentage points between state groups. This pattern can be interpreted as indicating that nonunion construction workers across state types hold similar relative wage positions (as defined by their position within the local blue collar labor market).

The estimates for this same coefficient based on union workers only, on the other hand, indicate greater construction premiums for workers in PWL states and smaller construction premiums for workers in No PWL states. The construction premium for workers in Repeal states is ten percentage points less than that of workers in PWL states, on average, before the state repeals (see bottom panel, figure 3.5). Relative to workers in No PWL states, the construction premium is 11 percentage points greater, on average (see bottom panel of figure 3.6). Therefore, after controlling for differences between blue collar labor market conditions, the differences in construction wage levels appear to be strongly associated with union workers.

Effects of state prevailing wage laws are strongly associated with more experienced union workers. Large negative wage effects associated with the state repeals consistently appear strongest among the more experienced union construction workers. Given that the more experienced union construction workers also tend to earn the highest wages among construction workers, on average, these negative effects are concentrated at the high end of the construction wage distribution. The magnitudes of the coefficients vary from -0.22 to -0.16, and are statistically significant across both types of control states and both time periods. In other words, if the state prevailing wage laws had not been repealed the average wage of these construction workers would be 15 percent to 20 percent greater.

Two other consistent patterns appear in the mean regression estimates. First, echoing the distributional shifts illustrated by the kernel density estimates in section 3.3.1, more experienced, nonunion construction workers appear to experience an *increase* in their construction premium after the repeal of the state prevailing wage laws. In other words, these construction workers see their relative wage positions rise after the law repeals. This result is produced across state control groups when using the later time period. This repeats the pattern of effects observed from the quantile regression estimates. That is, a positive wage effect appears when an increased lag period is used among relatively low-wage construction workers (see figure 3.4), strengthening the evidence that some form of substitution takes place. Two plausible causes for this increase in construction premiums are: 1) an increase in demand for more experienced nonunion workers or 2) an increased presence of formerly union construction workers among more experienced nonunion workers due to union construction workers leaving the union sector and entering the nonunion sector, and thus potentially increasing the average productivity among the pool of more experienced, nonunion workers. Average productivity is likely to be increased because of the tendency of union construction workers to have higher levels of formal apprenticeship training.[24] In other words, the positive wage effects suggest that employers substitute away from workers with union status with the repeal of state laws. Second, a large, positive wage effect among less experienced union construction workers is estimated (β_7=0.11 and 0.12) when using the earlier time period. Though this result is neither statistically significant at conventional levels nor robust to the use of the later time period it may indicate some initial substitution of employers toward less experienced, union workers who tend to earn lower wages than more experienced union workers.

One other result is notable: a statistically significant, negative wage effect is estimated for less-experienced nonunion construction workers in Repeal states when PWL states are used as the control states (β_7=-0.03). The magnitude of this estimate is small and is not robust to the use of any of the alternative control groups. For these reasons, I conclude that it is unlikely that this anomalous result reflects wage effects associated with the state prevailing wage laws.

In sum, while controlling for national trends within and outside the construction industry, level differences between the blue collar labor markets and construction premiums of state groups, as well as within state-group blue collar labor markets trends, both the quantile and mean regression estimates consistently indicate negative wage effects at the top of the wage distribution. The mean regression estimates identify relatively more experienced and unionized construction workers as affected by these state repeals. Among these workers, I observe wage declines on the order of 16 to 22 percent. The quantile and mean regression estimates also provide some evidence of substitution effects, particularly in terms of substitution toward nonunion workers when the state prevailing wage laws are repealed. These positive effects hover around a seven percent increase in wages experienced by workers who tend to earn low to moderate level wages (i.e., construction laborers in Repeal states earning wages around the 25th wage percentile or more experienced nonunion construction workers in Repeal states).

3.4 Discussion

On the whole, these estimates of effects at different points of the wage distribution suggests that the state prevailing wage law repeals exert a fairly focused negative effect on the wage structure of construction workers. In this section, I examine more carefully whether these estimates are consistent with a limited or extensive ripple effect. To do this, I first evaluate whether the estimates are consistent with the individual wage effects I would expect to observe if state prevailing wage laws are only accompanied by mandated wage changes (i.e., wage increases to bring covered workers' wages up to the prevailing wage rates). In the context of the removal of a mandated wage floor—the repeal of the state prevailing wage laws—these mandated wage changes are expected to be retracted, causing a reduction in the wages of covered workers.

The outcome of the above exercise reinforces the view that there is a narrowly focused wage effect experienced specifically within the union sector. This finding is consistent with past research. In the second part of this discussion, I explore the implications of the observed wage effects associated with the state prevailing wage law repeals for the bargaining power of union workers.

3.4.1 Assessing the Evidence of a Ripple Effect

Given the limitations of both the quantile and mean regression estimates with regard to observing individual wage effects associated with the state prevailing wage law repeals, I make some assumptions to approximate the contours of the individual wage effects given the regression estimates of the repeal effects on the wage structure. In this section, I evaluate whether the estimated effects on the wage structure are consistent with limited or extensive ripple effects.

Ripple effects potentially occur vertically—up and down the wage distribution, or horizontally—across covered and uncovered workers. I first examine the quantile regression results to assess the extent of ripple effects up and down the wage distribution. At the extreme, if there are extensive ripple effects caused by prevailing wage laws then the entire wage distribution would shift toward higher wages with the enactment of a prevailing wage law. The repeal of prevailing wage laws would, conversely, shift the entire wage distribution toward lower wages. In the context of the repeal of the state prevailing wage laws then, the extent of a ripple effect caused by prevailing wage laws—across the wage distribution—will be reflected by the extent that individual wage effects cause the entire wage distribution to move toward lower wages. Therefore, the extent of the negative wage effect associated with the state prevailing wage law repeals will provide some indication of the extent of the ripple effects.

Recall that the quantile regression estimates identify how the wage structure is affected by the prevailing wage law repeals (or the quantile treatment effects) rather than the individual wage effects (or treatment effects). Changes to the wage structure reflect individual wage effects directly only under specific conditions, as discussed in section 3.2.2. However, from the simple example presented in section 3.2.2 and table 3.5, I illustrate how one can observe the bounds of the magnitude of an individual effect by the range of effects on the wage structure, as long as the assumption that positive wage changes do not offset negative wage changes holds. Thus, the range of the negative wage effects observed through quantile regression analysis provides information about the potential magnitude of the individual effects produced by the law repeals. Discerning this detail is important because if the range of the wage effects exceeds the magnitude of the wage declines expected to be observed from the loss of legally mandated raises, two outcomes that are consistent with ripple effects may have taken place: (1) workers who earn wages below the prevailing wage rates, and thus presumably not covered by the prevailing wage laws, earn higher wages than they would otherwise in the presence of prevailing wage laws and thus experience a decline in wages when these laws are repealed or (2) workers who earn wages above the prevailing wage rates obtain such wages by reference to the mandated wage levels required by prevailing wage laws and thus experience a decline in wages when these laws are repealed. In other words, to the extent that the range of the negative wage effect on the wage structure exceeds that expected from mandated wage changes associated with the state prevailing wage laws, ripple effects are indicated.

Given the above discussion, I approximate the wage range of the negative wage effects to infer the potential magnitude of the wage effects experienced by individual construction workers from the quantile regression estimates. Analogous to the example discussed in 3.2.2, this range is produced by taking the difference between the wage level of the highest and lowest wage percentiles of the conditional wage distribution that experience a negative wage effect, taking into account the reduction of the lower wage percentile by the negative wage effect associated with the law repeals. For laborers, I take the difference between the 90th and 75th wage percentiles, and depending on which coefficient estimate is used to reduce the 75th wage percentile, the wage range varies from $7.79 to $10.42 and $7.13 to $10.42. The potential magnitude of the wage effects thus varies from 37 to 46 percent. For carpenters, I again take the difference between the 90th and 75th wage percentiles. The potential magnitude of the wage effects is estimated to be somewhere between 31 percent ($11.15 to $13.72) to 36 percent ($10.03 to $13.72). Although these approximations fall within the range of mandated wage raises estimated by previous research, they fall within the high end of this range. Thus, the quantile regression estimates are consistent with limited ripple effects. In other words, the wage effects associated with the law repeals appear to be largely due to a retraction of mandated wage changes.

To assess whether the wage effects of state prevailing wage laws extend across covered and uncovered workers (i.e., ripple horizontally across the construction workforce) I turn to the mean regression estimates. As discussed above, the mean regression estimates isolate a negative wage effect among more experienced union workers in Repeal states with the repeal of state prevailing wage laws. This subgroup makes up only 12.1 percent of all construction workers in Repeal states—well below the estimated 20 percent of construction employment covered by federal and state prevailing wage laws. Given the smallness of the affected group and that those contractors who are mandated to pay relatively high wages due to prevailing wage laws are likely to employ the relatively high-skilled union workers on covered contracts, these regression results suggest that there is little ripple effect across the construction industry. Although, it is possible that if, within the other subgroups, one set of workers are covered (and thus experience a negative wage effect) and another set of workers are not covered and may experience a positive wage effect due to substitution effects, this seems unlikely. The reason that this is an unlikely scenario is because the estimates of the level differences between construction premiums among Repeal state workers in the other subgroups *before* the repeals do not provide any indication that these workers have construction premiums that are either notably greater than those obtained by similar workers in states with *no* state prevailing wage laws nor similar to those obtained by workers in states *with* state prevailing wage laws. In other words, the Repeal state workers in the other subgroups do not appear to experience any positive wage effect associated with having a state prevailing wage law. Thus, it seems unlikely that Repeal state workers in these other subgroups would experience negative wage effects associated with having their state prevailing wage law repealed. The mean regression estimates do not indicate extensive ripple effects across covered and uncovered workers.

Further, because it is likely that most of the 12.1 percent of more experienced union construction workers in Repeal states were employed on covered contracts before the state repeals, the mean regression coefficient for this subgroup provides a rough estimate of the magnitude of the individual wage effects. Based on the mean regression coefficients, more experienced union construction workers are estimated to experience a 15 to 20 percent decline in wages when their state laws are repealed. Note that this magnitude is significantly smaller than the individual wage effect deduced from the quantile regression estimates. This is unsurprising since I used the quantile regression estimates to roughly approximate the outer bound values of the individual wage effects.

Based on the above assessment of the mean and quantile regression results, the presence of state prevailing wage laws appears to have a strong, negative effect on the wages of a circumscribed set of construction workers. The magnitude of these wage effects implied by the range of treatment effects derived from the quantile regression results and the fraction of affected workers indicated by the mean regression results point to negative wage effects that are largely limited to the retraction of mandated wage raises associated with state prevailing wage laws rather than reflecting extensive ripple effects.

3.4.2 The Impact of Prevailing Wage Laws on Union Bargaining Power

As illustrated with the descriptive statistics at the beginning section 3.3, the labor markets of the three state groups differ from each other in important ways. Differences in average wages accompany the varying labor market contexts. Blue collar workers tend to fare better in PWL states than in No PWL states, while the average wage of Repeal state blue collar workers falls between these two groups of states. In this section, I focus on the *level* differences between construction premiums across state groups before and after the state repeals in order to put into context the *changes* in construction premiums associated with the repeal of state prevailing wage laws. A comparison of construction premiums among union workers, in particular, across state groups indicate that state prevailing wage laws play an important role in allowing a subset of union workers to obtain relatively high wages.

Recall that the estimates of $\delta_{3,Before}$ presented by the dark grey bars in figures 3.5 and 3.6 estimate the level differences between construction premiums before the state repeals. As discussed earlier, the estimates presented in the top panels of these figures indicate that construction premiums somewhat echo the patterns in blue collar median wages. In contrast, among nonunion workers (middle panels), construction premiums do not vary across state groups. None of the $\delta_{3,Before}$ estimates are statistically significant at conventional levels due to their small magnitudes. This pattern of results suggests that nonunion construction workers are not greatly affected by the presence or absence of state prevailing wage laws. Among union workers, on the other hand, the level differences between construction premiums before the repeals are statistically significant and large in magnitude.

Looking now more closely at union construction workers divided by potential labor force experience, the level differences between construction workers in Repeal states and PWL states before the repeals appears to be limited to union construction workers with lower levels of experience. Among union construction workers with higher levels of experience, the difference between construction premiums is not statistically significant and is small in magnitude. In other words, those construction workers most likely to be affected by the prevailing wage law repeals (more experienced union construction workers) are the same workers who have similar construction premiums as those in PWL states. As noted above, PWL states may be generally characterized as having a more labor-friendly environment than the other states (e.g., a higher unionization rate, few states with Right-to-Work laws), so it is unsurprising that the construction premiums are smaller for less experienced construction workers in Repeal states when compared to their counterparts in PWL states. In fact, less experienced union construction workers may be thought of as a control group for aspects of the construction labor market of Repeal states that are unrelated to prevailing wage laws status since they do not appear to experience any consistent effect from the state repeals (β_7 is estimated to be 0.11 or 0.02, depending on the years examined). In other words, the level differences in construction premiums among less experienced union workers capture differences in construction labor markets unrelated to prevailing wage law status. What is somewhat surprising is that construction premiums among more experienced union workers in Repeal states are not similarly smaller than their counterparts in PWL states. Because the difference in construction premiums among more experienced union workers *after* the state repeal is large, negative, statistically significant, and comparable in size to the difference in construction premiums among less-experienced union workers (-0.21 versus -0.18), it appears that state prevailing wage law status underlies the similarity in construction premiums among more experienced union workers across state groups. When those laws are repealed, the construction premiums among more experienced union workers diverge, and reflect the same degree of difference observed among the less-experienced union workers. In other words, after the state prevailing wage laws were repealed, the disadvantage of being a union construction worker in Repeal states as compared to PWL states emerges.

An analogous, but converse pattern is observed when comparing construction premiums among union workers in Repeal states and No PWL states. Less-experienced union workers in Repeal states are estimated to earn similar, or slightly greater, construction premiums when compared to their counterparts in No PWL states before the state repeals. The estimated difference between the construction premium among more experienced union workers in Repeal states and No PWL states, on the other hand, is large and positive in magnitude. However, given the imprecision of the estimate, the coefficient is not statistically significant at conventional levels. When the state laws are repealed this large difference in construction premiums is eliminated completely. The level difference between construction premiums among the more experienced union workers is estimated to be -0.02 and is not statistically significant. In other words, once the state prevailing wage laws were repealed, the advantage of being a construction union worker in Repeal states as compared to No PWL states was eliminated.

These results suggest that state prevailing wage laws play an important role in establishing substantial wage premiums for union construction workers in Repeal states. Given that the significant construction premiums in Repeal states (before the repeals) approximate the large construction premiums found in PWL states, states which have significantly greater unionization rates among blue collar workers in general and construction workers specifically (see tables 3.6 and 3.7), it appears that state prevailing wage laws provide an alternative source of bargaining power for union workers that may not be available through union membership alone. Put another way, despite the greater unionization rate among PWL states before the state repeals, more experienced union construction workers in Repeal states obtained construction premiums similar to those obtained by similar union construction workers in PWL states, indicating that the prevailing wage laws significantly determine the ability of construction union workers to obtain relatively high wages.

Past research has identified the role of prevailing wage laws in determining the wages of union workers specifically. O'Connell (1986) argues that prevailing wage laws augment the bargaining power of union construction workers and thus raise union premiums. More specifically, when prevailing wage laws limit the degree of wage competition in the local labor market by putting union contractors on a level playing field with nonunion contractors with regard to wages, construction unions have a greater potential to raise their union premiums than would otherwise be the case. The findings of

Kessler and Katz (2001) confirm that prevailing wage laws have a particularly strong and positive effect for union construction workers. This link between union workers' bargaining power and the effect of state prevailing wage laws is broadly consistent with the findings of O'Connell (1986). As discussed above, O'Connell investigates the question: Do prevailing wage rates determine, in part, the union wage premium in construction? O'Connell found a positive correlation between the use of union wages for prevailing wage rates and union wage premiums, concluding that the effect of prevailing wage laws spills over more broadly across the union sector. The results above, however, suggest something slightly different. On the one hand, the presence of state prevailing wage laws appears to determine, in part, the average construction wage premium for union workers specifically. However, this appears to be due to the impact prevailing wage laws have on a subset of union workers, rather than across the union sector, in general.

3.5 Conclusions

The main objective of this analysis was to use the repeal of state prevailing wage laws to assess whethere this type of mandated wage floor generates ripple effects. Toward this end, I used a combination of quantile and mean regression techniques to assess wage effects at different points in the wage distribution. To my knowledge, this is the first attempt to empirically estimate the wage effects of prevailing wage laws across the wage distribution. Past research on the wage effects of prevailing wage laws primarily rely on estimating average effects. Unfortunately, such estimates provide limited insight on whether this type of policy tool extends beyond those workers who are employed on covered construction projects and mandated wage raises. Given that one of the original intents of prevailing wage laws was to intervene in the labor market so as to to uphold relatively high wages generally, understanding the extent to which prevailing wage laws affect wages across the construction industry is important in evaluating prevailing wage laws as a policy.

The results of this research points to a focused, negative wage effect caused by state prevailing wage law repeals. First, there is no evidence of an extensive ripple effect given the changes in the wage structure estimated for carpenters and laborers. While the estimates for laborers are rather imprecise and small in magnitude, the regression estimates consistently point to an effect that is concentrated at the 75th wage percentile and limited to wage levels below the 90th wage percentile. The estimates for construction carpenters, on the other hand, consistently indicate a negative effect on the 75th and 90th wage percentiles. The fact that the impact appears to be limited to the upper quartile of the wage distribution may be due to the fact that the prevailing wage floor is set at a high rate, therefore little room exists at the top of the wage distribution for an extended ripple effect.

Using the regression estimates I estimate that the wage raises associated with the state prevailing wage laws (at least as experienced by construction workers in Repeal states) are reasonably within the range expected to be mandated. I also find that the fraction of affected workers is relatively small (12 percent). As such, I conclude that any ripple effect is likely to be small.

Finally, I find that highly skilled union construction workers in states that repealed their prevailing wage laws are able to enhance their construction wage premium to levels approximating those found in higher union density states (i.e., states with state prevailing wage laws). This enhanced construction union wage premium is largely eliminated with the law repeals. This link suggests that state prevailing wage laws provide alternative channel through which workers are able to gain bargaining power in negotiating their wages. However, this mechanism appears to be confined to the subset of workers actually covered by prevailing wage laws.

Despite the important ways in which prevailing wage laws are distinct from minimum wage laws, the limited extent of the wage effects associated with prevailing wage laws is generally consistent with the findings presented in the minimum wage chapter. Prevailing wage laws are different from minimum wage laws in the following ways: the magnitude of the wage raises associated with prevailing wage laws is greater, the prevailing wage floor is inserted into a dense part of the wage distribution thereby increasing the potential relative wage effect because of the proximity of many more workers to the wage floor, and set a wage floor in the middle of the wage distribution rather than adjusting the lowest wages. Because of these factors, similar to living wage laws, prevailing wage laws have a greater potential to disrupt the wage hierarchy if ripple effects do not occur. Because I do not observe extensive ripple effects as result of prevailing wage laws, this suggests that mandated wage floors such as living wage laws will produce similarly limited ripple effects.

Notes

[1] Coverage is not universal for all publicly-funded or financially-assisted projects. Variations in coverage exist (Thieblot and Burns 1986) and depend on factors such as contract value, occupation, project-type (e.g., school construction), and funding source (some state prevailing wage laws exclude projects funded by local government).

[2] In the case of mixed funding sources, either state prevailing wage laws or the Davis-Bacon Act may supercede the other; this varies by state (Theiblot and Burns 1986).

[3] The National 8-Hour Day Act limited the workday to eight hours *without* a reduction in the daily rate. Given that the working day was customarily 10 hours at the time of its passage, the Act raised *hourly* wage rates by 25 percent.

[4] I use the term prevailing wage rates to refer to the compensation package specified by prevailing wage laws. Not all prevailing wage compensation requirements include benefits, but all stipulate wage levels.

[5] Alabama, which repealed its law in 1980, however, may be better categorized as a state that never had a state-level prevailing wage law because its law set a wage ceiling as opposed to a wage floor.

[6] Oklahoma's state laws were judicially annulled in 1995. Michigan's state law was also judicially annulled in 1995, but this decision was reversed in 1997.

[7] See Thieblot 1975, tables 13, 14, 16, and 20.

[8] Bloch (2003) suggests this upper-bound. His estimate for the union-nonunion differential in construction is 26 percent for the years of 1967 to 1979. The estimate provided in the text is based on the regression analysis presented in section 3.3.3 using CPS ORG data from 1983 to 1984. Specifically, the union premium is estimated for construction workers who lived in states that repealed their state prevailing wage laws between 1985 and 1988 (see table 3.1 for a list of states). Union status data is not available for all outgoing rotation groups in prior to 1983.

[9] As explained by Bloch (2003), Kessler and Katz estimate a negative wage effect associated with the repeal of state prevailing wage laws. They estimate a 2.3 to 3.9 percentage point decrease in the average construction wage. Assuming a coverage rate of 20 percent, this suggests a wage decrease of 12 to 20 percent (0.023/0.20=0.115; 0.039/0.20=0.195). Azari-Rad, Yeagle, and Philips (1994) also estimated the wage effect of state prevailing wage law repeals. Their estimate based on data from 1975 to 1991 is similar to Kessler and Katz's lower estimate. Azari-Rad, Yeagle, and Philips find a 2 percentage point decrease in the construction wage premium over average wages.

[10] This may account for the significantly higher wage effects estimated by Petersen (2000). He also estimates wage effects of the state prevailing wage law repeals (over the period 1982 to 1992). He finds a 10 to 13 percent increase in average wages associated with the presence of a state prevailing wage law. These imply mandated wage increases on the order of 50 to 70 percent (0.10/0.20=0.50; 0.13/0.20=0.70). The likely source of discrepancy is that Petersen does not control for within-state labor market trends occurring outside of construction that may be correlated with the change in prevailing wage policy.

[11] For a compilation of studies which research a variety of topics related to prevailing wage laws, see Azari-Rad, Philips, and Prus (2004). Topics include the impact of prevailing wage laws on unionization rates, benefits, injury rates, training, total construction costs, employment, and productivity.
[12] Atlanta, Baltimore, Boston, Denver, Grand Rapids, Kansas City, Portland (OR), and New Orleans.
[13] For the years of 1979 to 1991, the CPS variable "Highest Grade of School Attained" is used and the following code was used to convert variable values to estimated number of years of education:

If CPS Variable "Highest Grade of School Attained" =	Then "Years of Education" =
0	1
1-4	4
5-6	6
7-8	8
9	9
10	10
11	11
12	12
13	13
14-15	14
16	16
16+	18

For the years of 1992 and later, the CPS variable "Highest Level of School Completed or Degree Received" is used and the following code was used to convert variable values to estimated number of years of education:

If CPS Variable "Highest Level of School Completed or Degree Received" =	Then "Years of Education" =
31	1
32	4
33	6
34	8
35	9
36	10
37	11
38-39	12
40	13
41-42	14
43	16
44+	18

[14] Other sources include the Monthly Labor Review's annual review of state labor legislation published in their January issues and a newsletter for legislators (Dominic, 2005).
[15] Interestingly, Bassett, Tam, and Knight (2002) directly compare results from mean regression on quantile measures to quantile regression and find no meaningful differences.
[16] Note that to achieve sample sizes large enough to reliably analyze percentiles of occupation and industry specific wage distributions, I have to make the following simplifying assumption: I assume that the wage distributions at lower levels of geographic aggregations (e.g., county-level) reflect roughly the wage distribution at the state-level.
[17] For an extensive investigation of the way various assumptions may sharpen estimates of treatment effects, see Heckman, Smith, and Clements (1997).
[18] Repeal states include the following five states: Colorado, Idaho, Kansas, Louisiana, New Hampshire. No PWL states include the following eight states: Georgia, Iowa, Mississippi, North Carolina, North Dakota, South Carolina, South Dakota, and Virginia. PWL states include the remaining states excluding Alabama, Arizona, Florida, and Utah. See discussion of exclusions in methodology section 3.2.2. The state exclusions detailed in the methodology section are applied throughout the analyses.

[19] Right-to-Work laws provide workers with the "right to work" for an employer without joining a union or paying union dues, regardless of whether a union represents the employees of that employer. Unions are required, however, to represent all workers within its bargaining unit regardless of whether all employees of the bargaining unit are union members or pay dues. As such, these laws are viewed as unfavorable to unions.

[20] Right-to-Work laws were enacted in Georgia, Iowa, North Carolina, North Dakota, South Dakota and Virginia in 1947. Right-to-work laws were enacted in South Carolina and Mississippi in 1954 (U.S. Department of Labor, 2004).
[21] As noted in above, examining wages by union status restricts the "before" years that can be used. See section 3.2.2 for details.

[22] The standard errors for these quantile regression coefficients are bootstrapped standard errors, following the recommendations of Koenker and Hallock (2001). Each standard error was produced using the sampling weights provided by the CPS, and took into account the potential non-independence of observations within states. At minimum, 1000 replications were used. I assume that the coefficients have approximately normal distributions. Comparing results from 1000 and 2000 replications suggest that bias-corrected standard errors are not reliable at this level of replications.

[23] More specifically, the estimates are produced using generalized least-squares regression with standard errors that are robust to heteroskedasticity and non-independence within states. CPS provided sampling weights are used.

[24] Many formal apprenticeship programs are administered through a joint union-contractor effort so that union workers are more likely to obtain formal training, as opposed to simply on-the-job training if they are union members (BLS 2005; Azari, Yeagle, and Philips 1994). If this is the primary cause of increased wages among more experienced nonunion workers, then negative ripple effects on workers who were nonunion before and after the repeals may be obscured by the movement of former union workers into the nonunion sector.

CHAPTER 4

CONCLUDING REMARKS

The primary contribution of this research is a careful examination of the impact of mandated wage floors on the entire wage structure. In particular, I aimed to establish whether and to what extent mandated wage floors exert pressure on wages beyond those required to change by law. To this end, I derive empirical estimates of the wage effects of two different mandated wage floors—minimum wages and prevailing wages—at various points across the wage distribution. While this issue has been an important question in policy debates around minimum wage laws, prevailing wage laws, and more recently living wage laws, it has not been rigorously analyzed. The one exception is a study of minimum wage ripple effects by Neumark, Schweitzer and Wascher (2004). However, as described above, due to several weaknesses in their methodological approach I believe their results warrant scrutiny. Ripple effects caused by prevailing wage laws have not, to date, been estimated. As such, this dissertation aims to describe the behavior of ripple effects in order to fill this gap in research.

Because no a priori reason for ripple effects exists, the question of whether ripple effects are produced by changes in mandated wage floors is an empirical one. The most frequently articulated basis for ripple effects is centered on the idea that workers value their relative wage position, not just their wage level. Wage norms are thus generated out of the existing wage structure. As such, a change to the level of a mandated wage floor requires adjustments to wages across the wage structure in order to realign the wage hierarchy to the new wage floor level. Otherwise, the change in the mandated wage floor will cause relative wages to change also. These theories, however, hinge on the assumption that workers are able to effectively resist changes to their relative wage position when mandated wage floors change. Put another way, these theories assume that relative wages are rigid. Instead of finding that relative wages are rigid, I find that relative wages are, in the context of minimum and prevailing wages, fairly flexible.

Estimates of the minimum wage ripple effect suggest a large degree of wage compression accompanies increases in the minimum wage. Ripple effects from minimum wage increases are limited to the bottom 15 wage percentiles and the ripple effect raises are not sufficient to re-establish their former position relative to the wage floor. In other words, the relative wage positions of workers are not realigned after minimum wage levels are increased. To explore whether an increased potential for the minimum wage to disrupt the wage hierarchy causes greater ripple effects, I examine whether ripple effects differ in low-wage states versus high-wage states or for the retail trade industry where a large fraction of minimum wage workers are employed. I conclude that the same degree of wage compression takes place across these contexts.

This result is echoed in the prevailing wage analysis. Even when the mandated wage floor is inserted in a dense part of the wage distribution, and involves a relatively large wage increase—only a fraction—an estimated 12 percent—of construction workers appear to be affected (an estimate well within the 20 percent of construction workers assumed to be covered by prevailing wage laws). This finding—that a fairly circumscribed set of construction workers are negatively affected by the state repeals—suggests that construction wages are not generally set in relation to the prevailing wage rates. In particular, the negative wage effects associated with the law repeals is limited to the union sector. There is no evidence that these wage effects spillover into the nonunion sector. These findings suggest that the effects of prevailing wage laws do not include a strong ripple effect. In other words, adjustments to the relative wage positions of workers outside the covered sector do not appear to have occurred when the prevailing wage laws were repealed.

These fluctuations in the relative wage position of workers indicate that in these contexts, workers have a limited ability to sustain their relative wage position when mandated wage changes occur. In the cases studied here, mandated wage floors appear to be primarily just that: mechanisms that raise wages by fiat, for low-wage workers in the case of minimum wages, and a subset of construction union workers, in the case of prevailing wage laws, as opposed to establishing and enforcing a set of wage norms.

This is different from saying, however, that mandated wage floors have limited economic consequence for the wage structure. Minimum wage increases actually play an important role in *maintaining* the relative wage position of low-wage workers (as opposed to potentially changing the relative wage position). As described in section 2.5.3, over time, between minimum wage increases, the relative wage position of low-wage workers (relative to the rest of the wage distribution) deteriorates unless the minimum wage is increased. Low-wage workers appear to be part of a minimum wage contour so that their wages depend on minimum wage increases to realign their relative wage position. This conclusion contrasts with that of Neumark, Schweitzer and Wascher (2004) who discuss the role of minimum wage increases—outside the context of long-run trends in wage growth—as having transitory effects.

Also, despite the limited extent of the minimum wage ripple effect, workers who earn wages just above the minimum wage floor do experience some wage increases (though not sufficient to maintain their relative wage position) and comprise a large proportion of workers relative to the proportion of workers who earn the minimum. As a result these raises are economically significant – the overall change in the wage bill is more than doubled because of ripple effects. Wage hierarchies are important within a limited range. Finally, prevailing wage laws appear to be crucial in enabling covered construction union workers in Repeal states to obtain large construction wage premiums that union status alone may not provide.

These findings suggest that in the context of living wage laws, which impose larger increases in the wage floor but usually with less coverage than the minimum wage and prevailing wage laws, ripple effects will be similarly limited. Only a handful of studies have collected and examined data on workers covered by living wage laws to examine the impact of living wage laws after their enactment.

Brenner and Luce (2005) provide data on the wage distributions of covered firms in Boston from 1998 to 2001, during which Boston experienced the enactment of their living wage law. Between the years of 1998 and 2001, when living wage levels rose to $9.11, the wage distribution across affected firms indicate that the percent of workers earning below $9.25 decreased dramatically (from 23 percent to 4 percent), while the percent of workers earning below $11.75 remained constant. Therefore, a significant degree of wage compression took place and ripple effects did not extend beyond $11.75 (or 29 percent of the 2001 current living wage level of $9.11). These ripple effects do not appear to extend much further than those associated with minimum wage laws (up to roughly, 25 percent above the current minimum wage level).[1] Thus, their evidence shows that the extent of the ripple effect is limited (i.e., the whole wage distribution within covered firms does not shift) with this large increase in the mandated wage floor (for Boston city contractors, the wage floor effectively increased 57 percent in the first year of Boston's living wage law). In terms of ripple effects across covered and uncovered firms, they estimate that the number of workers who receive wages raises grows by approximately 30 percent when affected workers employed by uncovered firms are included in the number of affected workers. Thus, there appears to be a significant horizontal ripple effect.

Reich, Hall, and Jacobs (2005) studied San Francisco's Quality Standards Program (QSP) which set a living wage rate at $10.00 in 2001 for virtually all low-wage workers at the San Francisco International Airport. This represents an increase in the wage floor for covered airport workers from the state minimum in 2001 of $5.75 to the $10 QSP wage adopted in 2001, a dramatic 74 percent. The extent of coverage of the QSP is also dramatically high (when considering the labor market of the airport alone): approximately one-third of all airport employees were covered by the QSP living wage policy. The evidence from this study indicates somewhat limited vertical ripple effects but extensive horizontal ripple (or spillover) wage effects. In particular, while they observe significant wage compression, wage increases appear to extend up to workers earning up to $13.99 per hour (Reich, Hall, and Jacobs 2005, 117), or 40 percent above the new mandated wage floor. They also find that the percent of workers receiving wage raises associated with QSP climbs from 49 percent of potentially covered workers (workers in ground-based non-managerial employees) to 73 percent, when uncovered workers who receive wage raises (identified by their survey) are added to covered workers who receive wage raises. Similar to the Boston living wage, there appears to be a substantial number of workers who experience horizontal ripple effects.

These two case studies provide some insight into how varying the parameters of mandated wage floors may differ from the minimum wage and prevailing wage laws studied in this paper. How do these case studies compare to the minimum wage and prevailing wage laws? Among the case studies, the much larger increases to the wage floors for a segment of workers are accompanied with limited vertical ripple effects but substantial horizontal ripple (spillover) effects. In terms of vertical ripple effects, the results based on minimum wage and prevailing wage laws also suggest limited effects. In terms of horizontal ripple effects or spillover effects, spillover effects cannot be observed in the case of minimum wage laws because of their near universal coverage. The wage effects of prevailing wage laws, however, did not appear to spillover significantly to uncovered workers.

This variation in spillover effects may be related to the ability of workers to choose their employer and thus their wage. In the case of Boston, the unemployment rate during 1998-2001 was exceptionally low at less than 3 percent during these years. Thus, the ability of workers to quit one job because of unsatisfactory wages and to get another job was enhanced during this period. As a result, workers in Boston may have been more effective at bargaining over their wages, and thus able to maintain their relative wage position among uncovered firms. Reich, Hall, and Jacobs (2005) suggest that this was the case at San Francisco's airport where fully one-third of workers were covered by the QSP and, "…employers not covered by the QSP raised pay at a faster rate than they otherwise would have, in order to keep their employees from leaving for higher-paying jobs covered by the QSP, and to match wage norms" (Reich, Hall, and Jacobs 2005, 119). State prevailing wage laws, in contrast, appear to cover (exclusively) roughly 12 percent of the construction workforce according to the results discussed above. In sum, raising (or creating) mandated wage floors, even dramatically, is unlikely to cause changes up and down the wage distribution. Raising (or creating) mandated wage floors may cause changes across covered and uncovered workers contingent on the ability of uncovered workers to enforce their relative wage position.

4.1 Directions for Future Research

This research was primarily concerned with observing and describing ripple effects. Research exploring other sub-sectors of the economy, and/or different time periods may help to shed light on what factors enhance or dampen ripple effects. For example, to explore further the role of wage norms in producing ripple effects, one might examine how ripple effects may change given different indicators for workers' bargaining power, such as tight labor market conditions or high levels of union density. In the study done by Reich et al. (2005), it appears that tight labor market conditions played a role effecting a large (horizontal) ripple effect. More survey data on workers before and after living wage laws are enacted is needed to study, rigorously, the way various parameters of the laws effect different changes in the wage structure. However, this is a difficult task given the small number of workers typically covered by living wage laws.

Future research that identifies the channels through which ripple effects occur will provide key insights into the process of wage determination. That is, by parsing out the relative roles of more strictly economic factors (e.g., production technology) and more social factors (e.g., bargaining power and wage norms) will provide insight into their relative roles in the wage determination process in general. It is this process, ultimately, that needs to be understood to reasonably anticipate the impact of future variations in mandated wage floors.

Notes

[1] Given the large changes in the wage floor associated with living wage laws, an analogous comparison between the extent of the minimum wage ripple effect (up to 135% of the former wage floor) does not translate well in this context because the living wage levels tend to exceed 135 percent of the state (or federal) minimum wage rate. As a result, I discuss the extent of the minimum wage effect in terms of the current wage floor: The minimum wage ripple effect extends up to roughly 25 percent above the current wage floor (35 percent minus the average minimum wage increase of 8 percent).

Table 2.1: Estimates of the Extent of the Minimum Wage Ripple Effect

Study	Minimum Wage Increase	Upper Limit of Effect: Wage Level	Percent of Old Minimum Wage Level	Percent of New Minimum Wage Level
Converse, Coe, and Corcoran (1981)	Federal minimum wage:			
	$2.65 to $2.90 on January 1, 1979	$4.00-$6.00	226%	207%
	$2.90 to $3.10 on January 1, 1980		207%	194%
Card and Krueger (1995)	Federal mininum wage:			
	$3.35 to $4.25 over the years of 1989-1992	$4.50	134%	106%
Katz and Krueger (1992)	Federal minimum wage:			
	$3.90 to $4.25 on April 1, 1991	$4.50	115%	106%
Van Giezen (1994)	Federal minimum wage:			
	$3.90 to $4.25 on April 1, 1991	$5.40	138%	127%
Reich and Hall (2001)	California minimum wage:			
	$4.25 to $5.75 over the years 1995-1998	$6.50	153%	113%

Sources: Converse, Coe, and Corcoran 1981, tables 29 and 53; Card and Krueger 1995, 164-166; Katz and Krueger 1992, table 3; Van Giezen 1994, 29; Reich and Hall 2001, table 10.

Table 2.2: Wage Elasticity Estimates Among Workers Receiving Ripple Effect Raises

Study	Minimum Wage(s) Increase Analyzed	Wage Level	Wage Elasticity
Card and Krueger (1995)	Federal mininum wage:		
	$3.35 to $4.25 over the years of 1989-1991	5th percentile	0.38
		10th percentile	0.22
Pollin, Brenner, Wicks-Lim (2004)	State and federal minimum wages:		
	1991-2000	5th percentile	0.30[a]
		10th percentile	0.11[a]
Neumark, Schweitzer, and Wascher (2004)	State and federal minimum wages:		
	1979-1997	150%-200% of old minimum	0.16

Notes: [a]These wage elasticities were calculated in the context of a 2004 Florida minimum wage proposal, thus they incorporate features of Florida's 2003 wage distribution. For Card and Krueger (1995) see Table 9.3. For Pollin, Brenner, and Wicks-Lim (2004

Table 2.3: Panel Unit Root Tests on Biennial Time Series of Prevailing Minimum Wage by State, 1983-2002

	Lag Length					
	15	16	17	18	19	20
Variable: Prevailing Minimum Wage$_t$						
t-bar test statistic	-1.93	-1.91	-1.90	-1.89	-1.91	-1.96
10% critical value			-2.32			
Variable: ΔPrevailing Minimum Wage$_t$						
t-bar test statistic	-3.16	-3.22	-3.25	-3.25	-3.28	-3.27
10% critical value			-2.32			
N, T			50, 240			

Notes: Variables were cross-sectionally demeaned and a time trend was assumed. Range of lag lengths are based on ADF tests conducted on each state series separately. ΔPrevailing minimum wage$_t$ = Prevailing minimum wage$_t$ – Prevailing minimum wage$_{t-1}$.

Table 2.4: Incidence of Changes in Prevailing Minimum Wage by State and Year

	Year																		
State	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001
ME		x	x	x		x	x	x	x				x	x	x				x
NH				x	x	x	x	x	x				x	x	x				
VT			x	x	x	x	x	x	x			x	x	x	x	x	x	x	
MA			x	x	x	x	x	x	x				x	x			x	x	
RI			x	x	x	x	x	x	x				x	x		x	x	x	
CT				x	x	x		x	x				x	x	x	x	x	x	x
NY							x	x	x				x	x	x				
NJ							x	x	x	x				x	x				
PA						x	x	x	x				x	x	x				
OH							x	x	x				x	x	x				
IN							x	x	x				x	x	x				
IL							x	x	x				x	x	x				
MI							x	x	x				x	x	x				
WI						x	x	x	x				x	x	x				
MN					x	x	x	x					x	x	x				
IA							x	x	x				x	x	x				
MO							x	x	x				x	x	x				
ND						x	x	x	x				x	x	x				
SD							x	x	x				x	x	x				
NE							x	x	x				x	x	x				
KS							x	x	x				x	x	x				
DE							x	x	x				x	x	x	x	x	x	
MD							x	x	x				x	x	x				
VA							x	x	x				x	x	x				
WV							x	x	x				x	x	x				

Continued, next page

Table 2.4 (cont'd): Incidence of Changes in Prevailing Minimum Wage by State and Year

	Year																		
State	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001
NC							x	x	x				x	x	x				
SC							x	x	x				x	x	x				
GA							x	x	x				x	x	x				
FL							x	x	x				x	x	x				
KY							x	x	x				x	x	x				
TN							x	x	x				x	x	x				
AL							x	x	x				x	x	x				
MS							x	x	x				x	x	x				
AR							x	x	x				x	x	x				
LA							x	x	x				x	x	x				
OK							x	x	x				x	x	x				
TX							x	x	x				x	x	x				
MT							x	x	x				x	x	x				
ID							x	x	x				x	x	x				
WY							x	x	x				x	x	x				
CO							x	x	x				x	x	x				
NM							x	x	x				x	x	x				
AZ							x	x	x				x	x	x				
UT							x	x	x				x	x	x				
NV							x	x	x				x	x	x				
WA						x	x				x			x	x	x	x	x	x
OR						x	x	x						x	x	x			
CA					x	x							x	x	x	x		x	x
AK							x	x	x				x	x	x				
HI					x			x	x	x									x

Note: Entry of "X" indicates a change in the prevailing minimum wage for the corresponding state and year. Prevailing minimum wages changes are based on annual averages.

Table 2.5: Federal Minimum Wage Changes by Six Month Intervals

Year	Changes in Federal Minimum Wage	6-month Time Period	Federal Min. Wage Averaged Over 6-Month Time Period		% Change in 6-Month Average Min.Wage from Year t to Year t+1
1983		Jan.-June	$	3.35	--
		July-Dec.	$	3.35	--
1984		Jan.-June	$	3.35	--
		July-Dec.	$	3.35	--
1985		Jan.-June	$	3.35	--
		July-Dec.	$	3.35	--
1986		Jan.-June	$	3.35	--
		July-Dec.	$	3.35	--
1987		Jan.-June	$	3.35	--
		July-Dec.	$	3.35	--
1988		Jan.-June	$	3.35	--
		July-Dec.	$	3.35	--
1989		Jan.-June	$	3.35	7%
		July-Dec.	$	3.35	13%
1990	*Increased to $3.80 on April 1*	Jan.-June	$	3.58	13%
		July-Dec.	$	3.80	12%
1991	*Increased to $4.25 on April 1*	Jan.-June	$	4.03	6%
		July-Dec.	$	4.25	--
1992		Jan.-June	$	4.25	--
		July-Dec.	$	4.25	--
1993		Jan.-June	$	4.25	--
		July-Dec.	$	4.25	--
1994		Jan.-June	$	4.25	--
		July-Dec.	$	4.25	--
1995		Jan.-June	$	4.25	--
		July-Dec.	$	4.25	6%
1996	*Increased to $4.75 on Oct.1*	Jan.-June	$	4.25	12%
		July-Dec.	$	4.50	11%
1997	*Increased to $5.15 on Sept.1*	Jan.-June	$	4.75	8%
		July-Dec.	$	5.02	3%
1998		Jan.-June	$	5.15	--
		July-Dec.	$	5.15	--
1999		Jan.-June	$	5.15	--
		July-Dec.	$	5.15	--
2000		Jan.-June	$	5.15	--
		July-Dec.	$	5.15	--
2001		Jan.-June	$	5.15	--
		July-Dec.	$	5.15	--
2002		Jan.-June	$	5.15	---
		July-Dec.	$	5.15	---

Table 2.6: Distribution of Minimum Wage Workers Across Industries, 1983-2002

Major Industry Group	% of Minimum Wage Workers
Agriculture	3.1%
Mining	0.1%
Construction manufacturing	1.4%
Manufacturing-durable goods	2.8%
Manufacturing-nondurable goods	6.8%
Transportation	0.9%
Communications	0.3%
Utilities and sanitary services	0.2%
Wholesale trade	1.7%
Retail trade	49.6%
Finance, insurance and real estate services	1.4%
Private household miscellaneous	1.4%
Business and repair	4.8%
Personal services, except private household	4.7%
Entertainment, professional, and related services	3.5%
Hospital	0.9%
Medical, except hospital	3.3%
Educational services	6.8%
Social services	3.7%
Other professional	1.4%
Forestry and fisheries	0.1%
Public administration	1.3%
	100.0%

Note: Minimum wage workers are defined here to be workers earning exactly the applicable prevailing minimum wage.

Table 2.7: Demographic Characteristics by Wage Percentile, 1983-2002

Wage Percentile	Ratio of Wage Percentile to Minimum Wage	Percent Female	Percent Nonwhite (Including Latino)	Average Potential Labor Force Experience	Percent College Graduate	Percent Union	Percent Full-Time Worker
5th	1.00	62.3%	23.9%	13.0	7.4%	3.1%	39.4%
10th	1.12	61.1%	26.1%	12.6	6.2%	3.9%	42.6%
15th	1.23	60.9%	26.1%	14.0	7.3%	4.7%	51.8%
20th	1.35	60.3%	25.6%	15.4	8.8%	5.6%	60.7%
25th	1.47	59.4%	24.9%	16.3	10.1%	6.6%	67.6%
30th	1.59	58.1%	24.1%	16.9	11.3%	7.9%	72.7%
35th	1.72	56.8%	23.0%	17.4	12.7%	9.2%	76.5%
40th	1.86	55.3%	22.0%	17.8	14.4%	10.6%	79.4%
50th	2.16	51.1%	19.9%	18.5	18.2%	14.1%	83.6%
60th	2.52	46.1%	17.9%	19.1	23.1%	18.1%	86.2%
70th	2.97	41.1%	16.1%	19.7	29.4%	22.5%	87.6%
80th	3.57	36.3%	14.5%	20.4	37.0%	26.2%	88.3%
90th	4.55	30.1%	12.0%	21.3	49.1%	24.9%	88.5%
Total Sample		47.9%	19.3%	18.1	23.9%	14.4%	75.9%

Note: These statistics are based on the 6-month state averages used in the analysis. Wage percentile refers to the center point of the wage interval used to define the particular subset of workers. See text for details.

Table 2.8: Industry Composition by Wage Percentile, 1983-2002

Major Industry	Total Sample	Wage Percentile 5th	10th	15th	20th	25th	30th
Agriculture	1.6%	4.4%	3.2%	3.0%	3.0%	2.8%	2.5%
Mining	1.0%	0.1%	0.1%	0.1%	0.2%	0.3%	0.4%
Construction	5.6%	1.6%	1.7%	2.3%	3.1%	4.0%	4.9%
Manufacturing durable goods	9.9%	2.3%	3.2%	4.5%	5.9%	7.1%	8.2%
Manufacturing nondurable goods	7.3%	4.0%	5.0%	5.8%	6.5%	7.1%	7.7%
Transportation	4.4%	1.6%	1.6%	2.0%	2.6%	3.0%	3.5%
Communications and public util.	1.5%	0.3%	0.4%	0.5%	0.6%	0.7%	0.9%
Utilities and sanitary services	1.5%	0.2%	0.3%	0.3%	0.4%	0.5%	0.7%
Wholesale trade	3.7%	1.5%	1.8%	2.3%	2.9%	3.3%	3.9%
Retail trade	17.3%	43.5%	45.2%	39.2%	31.9%	25.8%	21.2%
Finance, insurance and real estate	6.1%	2.3%	2.4%	3.4%	4.8%	5.9%	6.6%
Private household services	1.1%	7.1%	1.8%	1.5%	1.4%	1.2%	1.0%
Business, auto and repair services	4.8%	3.8%	5.0%	5.8%	6.2%	6.3%	6.0%
Personal services, exc. household	2.7%	5.4%	5.5%	5.5%	5.1%	4.5%	3.9%
Entertainment and recreation	1.5%	3.2%	3.3%	2.8%	2.4%	2.0%	1.8%
Hospital	4.5%	1.1%	1.5%	2.0%	2.7%	3.5%	4.0%
Medical, except hospital	4.3%	2.7%	4.0%	5.0%	5.7%	5.9%	5.9%
Educational	9.2%	6.9%	7.0%	6.7%	6.8%	7.3%	7.8%
Social services	2.1%	4.0%	3.5%	3.3%	3.2%	2.9%	2.6%
Other professional	3.8%	2.3%	1.9%	2.1%	2.5%	2.9%	3.1%
Forestry and fisheries	0.2%	0.2%	0.1%	0.1%	0.1%	0.1%	0.1%
Public administration	5.9%	1.6%	1.5%	1.8%	2.2%	2.8%	3.4%

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Table 2.8 (cont'd): Industry Composition by Wage Percentile, 1983-2002

	Wage Percentile						
Major Industry	35th	40th	50th	60th	70th	80th	90th
Agriculture	2.1%	1.7%	1.2%	0.8%	0.6%	0.4%	0.3%
Mining	0.5%	0.6%	0.9%	1.1%	1.4%	1.8%	2.3%
Construction	5.5%	6.1%	6.9%	7.4%	7.5%	7.3%	7.1%
Manufacturing durable goods	9.1%	10.0%	11.5%	12.6%	13.3%	13.3%	12.7%
Manufacturing nondurable goods	8.1%	8.3%	8.3%	8.0%	7.7%	7.4%	7.5%
Transportation	3.8%	4.0%	4.4%	5.0%	5.9%	7.0%	6.0%
Communications and public util.	1.0%	1.0%	1.2%	1.4%	1.9%	2.5%	2.9%
Utilities and sanitary services	0.8%	0.9%	1.2%	1.6%	2.0%	2.5%	3.4%
Wholesale trade	4.3%	4.4%	4.4%	4.5%	4.3%	3.9%	3.8%
Retail trade	17.9%	15.5%	12.1%	10.0%	8.2%	6.2%	4.8%
Finance, insurance and real estate	7.1%	7.4%	7.5%	7.0%	6.2%	6.0%	6.6%
Private household services	0.8%	0.7%	0.5%	0.3%	0.2%	0.1%	0.1%
Business, auto and repair services	5.6%	5.3%	4.7%	4.5%	4.2%	3.9%	4.2%
Personal services, exc. household	3.4%	3.0%	2.3%	1.7%	1.3%	0.9%	0.7%
Entertainment and recreation	1.6%	1.4%	1.1%	1.0%	0.9%	0.7%	0.7%
Hospital	4.5%	4.9%	5.2%	5.1%	5.5%	6.6%	6.7%
Medical, except hospital	5.8%	5.5%	5.0%	4.3%	3.7%	3.3%	3.0%
Educational	8.2%	8.5%	9.2%	10.0%	10.8%	11.4%	11.9%
Social services	2.4%	2.2%	1.9%	1.7%	1.5%	1.2%	0.9%
Other professional	3.3%	3.5%	3.8%	4.1%	4.2%	4.3%	4.9%
Forestry and fisheries	0.1%	0.1%	0.2%	0.2%	0.2%	0.2%	0.2%
Public administration	4.1%	4.9%	6.4%	7.6%	8.5%	9.0%	9.3%

Notes: These statistics are based on the 6-month state averages used in the analysis. Wage percentile refers to the center point of the wage interval used to define the particular subset of workers. See text for details.

Table 2.9: Occupation Composition by Wage Percentile, 1983-2002

Major Occupation	Total Sample	Wage Percentile 5th	10th	15th	20th	25th	30th
Executive, administrative, and managerial	11.5%	2.5%	2.1%	2.8%	3.7%	4.7%	5.5%
Professional specialty	13.9%	4.6%	3.7%	4.2%	4.8%	5.3%	5.9%
Technicians and related support	3.5%	0.7%	0.8%	1.0%	1.3%	1.7%	2.1%
Sales	10.9%	16.5%	21.2%	19.9%	16.9%	13.9%	11.6%
Administrative support, including clerical	16.3%	8.6%	11.2%	14.0%	17.7%	21.2%	23.9%
Private household	0.9%	6.3%	1.4%	1.1%	1.0%	0.9%	0.8%
Protective service	1.8%	1.1%	1.5%	1.7%	1.8%	1.8%	1.6%
Service, except protective and household	12.3%	37.8%	33.2%	28.4%	24.1%	20.5%	17.3%
Precision production, craft and repair	11.1%	2.6%	3.2%	4.3%	5.7%	7.0%	8.2%
Machine operators, assemblers and inspectors	6.8%	4.4%	6.2%	7.3%	8.1%	8.6%	9.0%
Transportation and material moving	4.5%	2.5%	2.8%	3.1%	3.6%	4.1%	4.7%
Handlers, equip. cleaners, helpers, laborers	4.6%	7.0%	8.7%	8.4%	7.7%	7.1%	6.6%
Farming, forestry, and fishing	1.9%	5.4%	4.0%	3.7%	3.6%	3.3%	2.9%

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Table 2.9 (cont'd): Occupation Composition by Wage Percentile, 1983-2002

	Wage Percentile						
Major Occupation	35th	40th	50th	60th	70th	80th	90th
Executive, administrative, and managerial	6.5%	7.7%	10.0%	12.2%	14.6%	17.0%	22.7%
Professional specialty	6.6%	7.4%	9.7%	13.3%	18.4%	24.0%	30.1%
Technicians and related support	2.6%	3.2%	4.4%	5.1%	5.2%	5.3%	5.0%
Sales	10.1%	9.3%	8.3%	8.1%	7.9%	7.3%	7.7%
Administrative support, including clerical	25.3%	25.8%	24.1%	20.2%	15.5%	11.3%	6.7%
Private household	0.6%	0.5%	0.3%	0.2%	0.1%	0.1%	0.0%
Protective service	1.6%	1.7%	1.9%	2.3%	2.5%	2.3%	1.8%
Service, except protective and household	14.5%	12.2%	8.1%	5.1%	3.1%	1.9%	1.2%
Precision production, craft and repair	9.2%	10.3%	12.5%	14.6%	16.3%	17.4%	16.2%
Machine operators, assemblers and inspectors	9.1%	9.0%	8.7%	8.0%	7.0%	5.8%	3.4%
Transportation and material moving	5.2%	5.5%	6.0%	6.1%	5.6%	4.8%	3.6%
Handlers, equip. cleaners, helpers, laborers	6.1%	5.6%	4.5%	3.8%	3.2%	2.3%	1.3%
Farming, forestry, and fishing	2.5%	2.0%	1.5%	1.0%	0.7%	0.5%	0.3%

Note: These statistics are based on the 6-month state averages used in the analysis. Wage percentile refers to the center point of the wage interval used to define the particular subset of workers. See text for details.

Table 2.10: Estimated Wage Elasticities by Wage Percentile

	Wage Percentile					
	5th	10th	15th	20th	25th	30th
Total Sample						
$\Delta lnmin_1$	0.39	0.24	0.16	0.05	0.04	0.01
	(11.05)	(7.7)	(4.93)	(1.67)	(1.2)	(0.3)
$\Delta lnmin_0$	0.13	0.11	0.08	0.05	0.02	0.05
	(3.61)	(3.36)	(2.4)	(1.56)	(0.8)	(1.51)
R^2	0.31	0.35	0.31	0.27	0.27	0.26
N	1,900	1,900	1,900	1,900	1,900	1,900
Excluding New England Region						
$\Delta lnmin_1$	0.44	0.23	0.14	0.03	0.00	-0.01
	(11.69)	(7.41)	(4.32)	(0.81)	(0.05)	(-.42)
$\Delta lnmin_0$	0.03	0.05	0.04	0.03	-0.01	0.03
	(0.76)	(1.67)	(1.19)	(0.93)	(-.2)	(0.75)
R^2	0.35	0.39	0.33	0.30	0.28	0.27
N	1,672	1,672	1,672	1,672	1,672	1,672
Years: 1990-2001						
$\Delta lnmin_1$	0.32	0.24	0.14	0.03	0.02	0.01
	(7.41)	(6.43)	(3.64)	(0.93)	(0.64)	(0.19)
$\Delta lnmin_0$	0.14	0.08	0.07	0.02	0.01	0.03
	(3.59)	(2.14)	(1.94)	(0.57)	(0.26)	(0.75)
R^2	0.37	0.38	0.35	0.32	0.33	0.28
N	1,200	1,200	1,200	1,200	1,200	1,200
Years: 1983-1989						
$\Delta lnmin_1$	0.49	0.23	0.16	0.08	0.11	0.02
	(8.92)	(4.25)	(2.74)	(1.77)	(2.53)	(0.42)
$\Delta lnmin_0$	0.16	0.23	0.09	0.20	0.14	0.11
	(2.45)	(3.59)	(1.2)	(3.32)	(2.55)	(1.89)
R^2	0.37	0.45	0.35	0.33	0.32	0.32
N	700	700	700	700	700	700

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Table 2.10 (cont'd): Estimated Wage Elasticities by Wage Percentile

	Wage Percentile						
	35th	40th	50th	60th	70th	80th	90th
Total Sample							
$\Delta \ln min_1$	-0.01	0.02	-0.03	-0.03	-0.02	0.01	0.04
	(-.2)	(0.64)	(-.94)	(-1.06)	(-.87)	(0.42)	(1.21)
$\Delta \ln min_0$	0.01	-0.01	0.04	0.02	0.06	0.07	0.07
	(0.42)	(-.24)	(1.29)	(0.83)	(2.04)	(2.49)	(2.28)
R^2	0.24	0.25	0.24	0.25	0.20	0.21	0.23
N	1,900	1,900	1,900	1,900	1,900	1,900	1,900
Excluding New England Region							
$\Delta \ln min_1$	-0.04	-0.02	-0.03	-0.06	-0.02	0.00	0.03
	(-1.32)	(-.52)	(-1.08)	(-2.03)	(-.49)	(0.13)	(0.88)
$\Delta \ln min_0$	-0.01	-0.04	-0.02	-0.02	0.01	0.03	0.04
	(-.24)	(-1.28)	(-.57)	(-.9)	(0.46)	(1.14)	(1.21)
R^2	0.24	0.25	0.24	0.26	0.21	0.20	0.23
N	1,672	1,672	1,672	1,672	1,672	1,672	1,672
Years: 1990-2001							
$\Delta \ln min_1$	-0.01	-0.02	-0.02	-0.05	-0.04	0.01	-0.01
	(-.19)	(-.56)	(-.46)	(-1.64)	(-1.24)	(0.19)	(-.16)
$\Delta \ln min_0$	0.00	-0.01	0.01	0.01	0.05	0.04	0.06
	(-.13)	(-.27)	(0.23)	(0.2)	(1.41)	(1.27)	(1.86)
R^2	0.27	0.28	0.29	0.30	0.26	0.26	0.26
N	1,200	1,200	1,200	1,200	1,200	1,200	1,200
Years: 1983-1989							
$\Delta \ln min_1$	0.03	0.09	0.01	0.07	0.04	0.04	0.14
	(0.82)	(2.4)	(0.18)	(1.56)	(0.91)	(1.09)	(3.05)
$\Delta \ln min_0$	0.08	0.12	0.16	0.15	0.18	0.14	0.14
	(1.74)	(2.6)	(3.35)	(2.62)	(3.22)	(2.82)	(2.41)
R^2	0.32	0.31	0.27	0.31	0.21	0.27	0.33
N	700	700	700	700	700	700	700

Notes: See text for details. T-statistics are in parentheses.

Table 2.11: Estimated Wage Elasticities by Wage Percentile

	Wage Percentile					
	5th	10th	15th	20th	25th	30th
Excluding New England Region						
$\Delta \ln min_1$	0.19	-0.11	-0.19	-0.14	0.01	-0.06
	(2.22)	(-1.4)	(-2.17)	(-1.63)	(0.11)	(-0.73)
$\Delta \ln min_1$ x PROPDAW	2.32	3.19	3.24	1.72	0.13	0.61
	(3.07)	(4.72)	(4.26)	(2.22)	(0.18)	(0.87)
$\Delta \ln min_0$	-0.02	0.11	0.00	0.08	-0.03	-0.01
	(-0.18)	(1.43)	(-0.01)	(0.82)	(-0.3)	(-0.08)
$\Delta \ln min_0$ x PROPDAW	0.34	-0.61	0.17	-0.49	0.04	0.15
	(0.48)	(-1.0)	(0.25)	(-0.69)	(0.06)	(0.23)
R^2	0.35	0.41	0.35	0.31	0.29	0.28
N	1,672	1,672	1,672	1,672	1,672	1,672
Effect Evaluated for[a]:						
States with Low PROPDAW						
$\Delta \ln min_1$	0.40	0.17	0.09	0.01	0.02	0.00
$\Delta \ln min_0$	0.01	0.06	0.01	0.03	-0.02	0.01
States with High PROPDAW						
$\Delta \ln min_1$	0.55	0.38	0.31	0.12	0.03	0.04
$\Delta \ln min_0$	0.04	0.02	0.03	0.00	-0.02	0.02

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Table 2.11 (cont'd): Estimated Wage Elasticities by Wage Percentile

	Wage Percentile						
	35th	40th	50th	60th	70th	80th	90th
Excluding New England Region							
$\Delta lnmin_1$	-0.04	0.02	-0.03	0.00	-0.06	0.01	0.08
	(-0.48)	(0.31)	(-0.42)	(0.03)	(-0.86)	(0.19)	(1.05)
$\Delta lnmin_1$ x PROPDAW	0.10	-0.26	0.11	-0.43	0.54	-0.05	-0.42
	(0.15)	(-0.38)	(0.15)	(-0.67)	(0.8)	(-0.07)	(-0.65)
$\Delta lnmin_0$	-0.05	-0.03	-0.02	-0.12	-0.01	-0.04	-0.11
	(-0.63)	(-0.35)	(-0.25)	(-1.63)	(-0.14)	(-0.45)	(-1.39)
$\Delta lnmin_0$ x PROPDAW	0.27	-0.14	-0.04	0.72	0.15	0.61	1.27
	(0.43)	(-0.23)	(-0.06)	(1.25)	(0.23)	(0.88)	(2.11)
R^2	0.25	0.26	0.25	0.26	0.21	0.20	0.23
N	1,672	1,672	1,672	1,672	1,672	1,672	1,672
Effect Evaluated for[a]:							
States with Low PROPDAW							
$\Delta lnmin_1$	-0.03	0.00	-0.02	-0.04	-0.02	0.01	0.04
$\Delta lnmin_0$	-0.03	-0.04	-0.02	-0.06	0.00	0.01	0.00
States with High PROPDAW							
$\Delta lnmin_1$	-0.02	-0.02	-0.02	-0.06	0.02	0.01	0.02
$\Delta lnmin_0$	-0.01	-0.05	-0.03	-0.01	0.01	0.05	0.08

Notes: See text for details. T-statistics are in parentheses. [a]The 25th percentile value of PROPDAW, 0.088, is used to evaluate the total effect for states with low PROPDAW.The 75th percentile value of PROPDAW, 0.153, is used to evaluate the total effect for states with high PROPDAW. The mean of PROPDAW is 0.11.

Table 2.12: Demographic Profile of Retail Trade Industry, All Workers

Characteristic	
Female	53.6%
Nonwhite (including Latino)	19.0%
Average years of potential labor force Experience	13.8
Highest educational attainment	
High School Diploma only	41.8%
Bachelor's Degree or higher	10.4%
Union member	5.1%
Full-time worker	59.3%
Major Occupation	
Executive, administrative, and managerial	6.5%
Professional specialty	1.7%
Technicians and related support	0.6%
Sales	39.0%
Administrative support, including clerical	7.8%
Private household	0.0%
Protective service	0.3%
Service, except protective and household	26.1%
Precision production, craft and repair	5.9%
Machine operators, assemblers and inspectors	0.8%
Transportation and material moving	2.8%
Handlers, equip. cleaners, helpers, laborers	8.4%
Farming, forestry, and fishing	0.2%
Ratio of Wage Percentile to the Minimum Wage	
10th	0.99
15th	1.03
20th	1.07
25th	1.11
30th	1.15
35th	1.19
40th	1.25
50th	1.38
60th	1.55
70th	1.81
80th	2.20
90th	2.93

Notes: These statistics are based on the 6-month state averages used in the analysis. See text for details.

Table 2.13: Estimated Wage Elasticities by Wage Percentile, Retail Trade Industry

	Wage Percentile					
	10th	15th	20th	25th	30th	35th
Excluding New England Region						
$\Delta \ln min_1$	0.57	0.52	0.36	0.19	0.16	0.17
	(11.61)	(14.4)	(10.92)	(5.03)	(4.29)	(3.94)
$\Delta \ln min_0$	0.13	0.05	0.08	0.07	0.07	0.03
	(2.54)	(1.39)	(2.27)	(1.81)	(1.89)	(0.73)
R^2	0.27	0.44	0.37	0.34	0.33	0.30
N	1,672	1,672	1,672	1,672	1,672	1,672

	Wage Percentile					
	40th	50th	60th	70th	80th	90th
Excluding New England Region						
$\Delta \ln min_1$	0.11	0.02	-0.03	-0.06	0.04	-0.08
	(2.38)	(0.29)	(-.51)	(-.98)	(0.55)	(-1.14)
$\Delta \ln min_0$	0.06	0.04	0.06	0.07	0.02	0.19
	(1.35)	(0.68)	(1.05)	(1.04)	(0.27)	(2.56)
R^2	0.30	0.31	0.29	0.27	0.25	0.21
N	1,672	1,672	1,672	1,672	1,672	1,672

Notes: See text for details. T-statistics are in parentheses.

Table 2.14: Estimated Wage Elasticities by Wage Percentile, Retail Trade Industry

	Wage Percentile					
	10th	15th	20th	25th	30th	35th
Excluding New England Region						
$\Delta lnmin_1$	0.61	-0.08	-0.20	-0.30	-0.17	-0.05
	(3.95)	(-0.74)	(-1.86)	(-2.76)	(-1.31)	(-0.41)
$\Delta lnmin_1$ x PROPDAW	-0.13	2.11	1.97	1.74	1.26	0.88
	(-0.26)	(6.4)	(5.53)	(4.75)	(2.85)	(2.12)
$\Delta lnmin_0$	-0.07	0.03	0.27	0.22	-0.01	-0.07
	(-0.41)	(0.31)	(2.47)	(1.85)	(-0.05)	(-0.52)
$\Delta lnmin_0$ x PROPDAW	0.66	0.06	-0.68	-0.53	0.23	0.27
	(1.33)	(0.19)	(-1.98)	(-1.44)	(0.5)	(0.65)
R^2	0.27	0.47	0.40	0.36	0.34	0.31
N	1,672	1,672	1,672	1,672	1,672	1,672
Effect Evaluated for[a]:						
States with Low PROPDAW						
$\Delta lnmin_1$	0.57	0.43	0.27	0.11	0.13	0.16
$\Delta lnmin_0$	0.09	0.05	0.11	0.09	0.05	0.00
States with High PROPDAW						
$\Delta lnmin_1$	0.56	0.73	0.55	0.36	0.31	0.28
$\Delta lnmin_0$	0.19	0.06	0.01	0.02	0.08	0.03

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Table 2.14 (cont'd): Estimated Wage Elasticities by Wage Percentile, Retail Trade Industry

	Wage Percentile					
	40th	50th	60th	70th	80th	90th
Excluding New England Region						
$\Delta lnmin_1$	-0.23	0.01	0.02	-0.16	-0.10	0.04
	(-1.71)	(0.06)	(0.11)	(-0.92)	(-0.52)	(0.18)
$\Delta lnmin_1$ x PROPDAW	1.29	0.14	-0.05	0.47	0.54	-0.45
	(2.83)	(0.3)	(-0.1)	(0.79)	(0.88)	(-0.59)
$\Delta lnmin_0$	0.01	-0.13	-0.04	-0.07	0.02	0.25
	(0.07)	(-0.85)	(-0.24)	(-0.35)	(0.11)	(0.98)
$\Delta lnmin_0$ x PROPDAW	0.10	0.48	0.26	0.39	-0.06	-0.19
	(0.22)	(1.01)	(0.48)	(0.63)	(-0.09)	(-0.24)
R^2	0.32	0.32	0.31	0.27	0.26	0.21
N	1,672	1,672	1,672	1,672	1,672	1,672
Effect Evaluated for[a]:						
States with Low PROPDAW						
$\Delta lnmin_1$	0.07	0.04	0.01	-0.05	0.03	-0.07
$\Delta lnmin_0$	0.03	-0.02	0.02	0.02	0.01	0.20
States with High PROPDAW						
$\Delta lnmin_1$	0.26	0.06	0.00	0.01	0.11	-0.13
$\Delta lnmin_0$	0.05	0.05	0.06	0.08	0.00	0.17

Notes: See text for details. T-statistics are in parentheses.[a]The 25th percentile value of PROPDAW, 0.23, is used to evaluate the total effect for states with low PROPDAW.The 75th percentile value of PROPDAW, 0.38, is used to evaluate the total effect for states with high PROPDAW.

Table 2.15: Estimates of the Ripple Effect Multiplier

Federal Minimum Wage Increase	Wage percentile	Mean wage before change[a]	Mean hours worked/ week before change[b]	Wtd N[c]	Mean prevailing minimum wage before change	Raise due to minimum wage increase	Wage level after minimum wage increase	Mean prevailing minimum after minimum wage change	Direct Raises	Indirect Raises
April 1, 1990										
$3.35-$3.80										
	5th	$ 3.61	28.2	4,539,673	$ 3.49	0.16	$ 3.77	$ 3.86	$ 20,587,997	
	10th	$ 4.11	31.5	5,243,769	$ 3.49	0.11	$ 4.22	$ 3.86		$ 17,883,126
	15th	$ 4.57	33.0	4,780,689	$ 3.49	0.07	$ 4.64	$ 3.86		$ 10,656,317
									$ 20,587,997	$ 28,539,443
									Multiplier=	2.39
April 1, 1991										
$3.80-$4.25										
	5th	$ 3.91	29.1	4,408,805	$ 3.86	0.18	$ 4.09	$ 4.27	$ 23,528,929	
	10th	$ 4.38	30.4	5,067,123	$ 3.86	0.13	$ 4.50	$ 4.27		$ 19,348,303
	15th	$ 4.82	33.8	5,294,747	$ 3.86	0.09	$ 4.91	$ 4.27		$ 15,411,990
									$ 23,528,929	$ 34,760,293
									Multiplier=	2.48

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Table 2.15 (cont'd): Estimates of the Ripple Effect Multiplier

Federal Minimum Wage Increase	Wage percentile	Mean wage before change[a]	Mean hours worked/ week before change[b]	Wtd N[c]	Mean prevailing minimum wage before change	Raise due to minimum wage increase	Wage level after minimum wage increase	Mean prevailing minimum after minimum wage change	Direct Raises	Indirect Raises
October 1, 1996 $4.25-$4.75										
	5th	$ 4.54	24.4	4,844,485	$ 4.34	0.21	$ 4.75	$ 4.82	$ 24,889,433	
	10th	$ 5.12	26.4	5,231,344	$ 4.34	0.14	$ 5.26	$ 4.82		$ 19,515,964
	15th	$ 5.64	30.2	5,419,191	$ 4.34	0.09	$ 5.73	$ 4.82		$ 14,462,378
									$ 24,889,433	$ 33,978,343
									Multiplier=	2.37
September 1, 1997 $4.75-$5.15										
	5th	$ 4.83	24.1	3,981,635	$ 4.82	0.16	$ 4.99	$ 5.19	$ 15,442,126	
	10th	$ 5.32	27.5	6,118,138	$ 4.82	0.11	$ 5.43	$ 5.19		$ 18,283,409
	15th	$ 5.87	29.3	5,385,418	$ 4.82	0.07	$ 5.94	$ 5.19		$ 11,648,724
									$ 15,442,126	$ 29,932,133
									Multiplier=	2.94

Notes:[a]Mean wage is the mean of the wage percentile in column 2. [b]Mean hours are estimated from workers earning between the wage percentile ± 2. For example, mean hours per week for the 5th wage percentile is based on workers earning above the 3rd wage percentile and through the 7th wage percentile.[c] As with mean hours, weighted Ns are estimated from workers earning between the wage percentile in column 2 ± 2. See text for further details on samples.

Table 2.16: Demographic Profiles of Workers in 2000

	Total	Wage Group $5.15-$5.85	Wage Group $5.85-$6.55	Wage Group $6.55-$7.00
Individual Characteristics				
Mean Hourly Wage	$14.72	$5.47	$6.20	$6.94
Mean Wage as % of Minimum Wage	85%	3%	16%	27%
Student and 16-24 yrs. old	7.0%	35.4%	26.6%	14.0%
Teenager	5.7%	32.7%	22.6%	11.3%
Student or Teenager	8.6%	40.9%	30.5%	17.3%
Non-white	27.7%	39.8%	39.2%	35.1%
Female	48.3%	59.7%	58.1%	60.5%
High school diploma, only	30.6%	25.9%	32.0%	38.0%
Age				
Mean	37.5	29.0	30.8	34.7
Median	37.0	23.0	25.0	30.0
Usual hours worked/week				
Mean	37.3	25.9	29.4	31.8
Median	40.0	25.0	35.0	40.0
Family Characteristics				
Family Earnings				
Mean	$58,030	$41,068	$40,450	$41,650
Median	$48,000	$29,500	$29,904	$30,000
Family Income				
Mean	$63,874	$46,215	$45,543	$46,767
Median	$52,525	$34,000	$35,360	$37,000
Worker's earnings as % of Family Earnings				
Mean	63.7%	43.1%	47.9%	54.4%
Median	64.3%	27.3%	33.3%	43.9%
Worker's earnings as % of Family Income				
Mean	57.0%	35.3%	39.7%	45.0%
Median	54.7%	21.9%	25.6%	32.6%
Poverty Status				
Severe Poverty	5.2%	19.9%	16.2%	10.9%
Low-Income	18.4%	43.8%	43.4%	38.1%
Middle-Income	51.3%	76.1%	73.6%	72.2%
Weighted N	117,163,588	5,588,639	7,209,054	5,329,748

Source: CPS March Annual Demographic File 2000. Dollar values are in 2000$.

Table 2.17: Demographic Profiles of Workers by Adult and Teenager/Student Status

	Total		Wage Group 5.15 - 5.85		Wage Group 5.85 - 6.55		Wage Group 6.55-7.15	
	Adults	**Teenager /Student**	**Adults**	**Teenager /Student**	**Adults**	**Teenager /Student**	**Adults**	**Teenager /Student**
Individual Characteristics								
Mean Hourly Wage	$15.43	$7.26	$5.50	$5.42	$6.21	$6.18	$6.94	$6.96
Average Wage as % of Minimum Wage	190%	125%	103%	103%	115%	116%	127%	128%
Student and 16-24 yrs. old	0.0%	81.1%	0%	87%	0%	87%	0%	81%
Teenager	0.0%	66.0%	0.0%	79.9%	0.0%	74.0%	0.0%	65.6%
Student or Teenager	0.0%	100.0%	0.0%	100.0%	0.0%	100.0%	0.0%	100.0%
Non-white	27.9%	24.8%	45.2%	31.9%	46.9%	21.8%	38.5%	18.8%
Female	48.1%	50.2%	64.8%	52.3%	59.4%	54.9%	61.4%	56.1%
High school diploma, only	31.9%	16.8%	37.0%	9.9%	37.4%	19.7%	41.3%	22.1%
Age								
Mean	39.4	18.2	37.2	17.3	36.5	17.8	38.2	18.2
Median	39.0	18.0	36.0	17.0	33.0	18.0	36.0	18.0
Usual hours worked/week								
Mean	38.8	22.0	31.3	18.2	33.6	19.9	34.0	21.2
Median	40.0	20.0	38.0	20.0	40.0	20.0	40.0	20.0

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Table 2.17 (cont'd): Demographic Profiles of Workers by Adult and Teenager/Student Status

	Total		Wage Group					
			5.15 - 5.85		5.85 - 6.55		6.55-7.15	
	Adults	Teenager /Student	Adults	Teenager /Student	Adults	Teenager /Student	Adults	Teenager /Student
Family Characteristics								
Family Earnings								
Mean	$58,055	$57,764	$33,605	$51,861	$33,707	$55,824	$36,002	$68,655
Median	$48,000	$48,300	$22,880	$43,050	$25,400	$48,000	$26,560	$58,400
Family Income								
Mean	$63,837	$64,264	$38,360	$57,575	$38,284	$62,092	$40,911	$74,766
Median	$52,300	$55,360	$26,400	$52,560	$29,522	$55,385	$32,503	$64,571
Worker's earnings as % of Family								
Mean	67.0%	28.8%	58.2%	21.4%	57.8%	25.7%	60.7%	24.0%
Median	68.3%	10.7%	55.0%	7.1%	48.2%	8.3%	55.6%	9.8%
Worker's earnings as % of Family								
Mean	60.1%	24.2%	47.6%	17.6%	48.4%	19.8%	50.5%	19.1%
Median	57.9%	9.7%	42.3%	5.9%	38.2%	7.2%	37.9%	9.2%
Poverty Status								
Severe Poverty	4.6%	11.4%	25.4%	12.0%	18.0%	12.0%	11.3%	9.5%
Low-Income	17.6%	27.2%	50.3%	34.4%	50.0%	28.3%	42.2%	18.5%
Middle Income	50.6%	59.7%	82.1%	67.3%	79.6%	60.0%	77.0%	49.1%
Weighted N	107,062,974	10,100,614	3,303,977	2,284,662	5,010,969	2,198,085	4,407,839	921,908

Source: CPS March Annual Demographic File 2000. Dollar values are in 2000$.

Table 3.1: Chronology of State Prevailing Wage Laws through 2005

State	Year of Enactment	Year of Repeal	State	Year of Enactment	Year of Repeal
Northeast			South		
Connecticut	1935		Alabama	1941	1980
Maine	1933		Arkansas	1955	
Massachusetts	1914		Delware	1962	
New Hampsire	1941	1985	Florida	1933	1979
New Jersey	1913		Georgia	--	
New York	1897		Kentucky	1940	
Pennsylvania	1961		Louisiana	1968	1988
Rhode Island	1935		Maryland	1945	
Vermont	1973		Mississippi	--	
			North Carolina	--	
			Oklahoma	1909	1995
			South Carolina	--	
			Tennessee	1953	
			Texas	1933	
			Virginia	--	
			West Virginia	1966	
Midwest			West		
Illinois	1931		Alaska	1931	
Indiana	1935		Arizona	1912	1979
Iowa	--		California	1931	
Kansas	1891	1987	Colorado	1933	1985
Michigan	1965	1995-1997	Hawaii	1955	
Minnesota	1973		Idaho	1911	1985
Missouri	1957		Montana	1931	
Nebraska	1923		Nevada	1937	
North Dakota	--		New Mexico	1937	
Ohio	1931		Oregon	1959	2005
South Dakota	--		Utah	1933	1981
Wisconsin	1931		Washington	1945	
			Wyoming	1967	

Sources: Kessler and Katz (2001), Dominic (2005), Thieblot and Burns (1986)

Table 3.2: Ownership of Construction Projects (in current thousand dollar values)

Ownership	1987 Dollar-Value	1987 % of Total	1992 Dollar-Value	1992 % of Total	1997 Dollar-Value	1997 % of Total
Government-Owned						
Federally-owned	$ 26,434,306	5.3%	$ 30,243,555	5.7%	$ 37,451,064	4.4%
State- and Locally- Owned	$ 71,866,661	14.5%	$ 103,523,652	19.6%	$ 148,713,072	17.6%
Total	$ 98,300,967	19.8%	$ 133,767,207	25.3%	$ 186,164,144	22.0%
Privately-Owned	$ 397,045,344	80.2%	$ 394,338,640	74.7%	$ 659,379,392	78.0%
Total	$ 495,346,312	100.0%	$ 528,105,847	100.0%	$ 845,543,552	100.0%

Sources: U.S. Department of Commerce (1996, 2000).

Table 3.3: Estimates of Wage Raises Due to Prevailing Wage Laws

Study	Prevailing Wage Law	Wage Raise
Goldfarb and Morrall (1981)	Davis-Bacon rates in 1972	4% - 9%
Thieblot (1975)[a]	Various Davis-Bacon rates, 1970s	30% - 50%
O'Connell (1986)	Davis-Bacon rates in 1978	13% - 33%
Kessler and Katz (2001)[b]	State rates, 1979-1993	12% - 20%
Author's analysis[c]	Davis-Bacon or state rates, 1980-1984 (Union wage premium in construction)	52% - 53%

Notes: [a]The wage raise is approximated by averaging over occupations by case study. [b]Kessler and Katz use two different data sources, the 1977-1993 CPS MORG files, and the 1970,1980,1990 Census data to estimate the wage effects of state repeals from 1979 to 1993. [c]Union wage premium based on (mean) regression analysis presented below, see text for details.

Table 3.4: Samples Used in Quantile Regression Analysis

	Treatment Group	Control Group
Sample 1	Construction carpenters in states that repealed their state prevailing wage laws	Construction carpenters and blue collar workers in states *with* state prevailing wage laws and blue collar workers in repeal states
Sample 2	Construction carpenters in states that repealed their state prevailing wage laws	Construction carpenters and blue collar workers in states *without* state prevailing wage laws and blue collar workers in repeal states
Sample 3	Construction laborers in states that repealed their state prevailing wage laws	Construction laborers and blue collar workers in states *with* state prevailing wage laws and blue collar workers in repeal states
Sample 4	Construction laborers in states that repealed their state prevailing wage laws	Construction laborers and blue collar workers in states *without* state prevailing wage laws and blue collar workers in repeal states

Table 3.5: Examples of the Impact of Individual Wage Changes on the Wage Structure

Observations:	**Initial Wage Distribution**	**New Wage Distribution (Scenario 1)**	**New Wage Distribution (Scenario 2)**
Worker 1	$ 15.00	$ 15.00	$ 15.00
Worker 2	$ 15.00	$ 15.00	$ 15.00
Worker 3	$ 12.00	$ 7.00	$ 7.00
Worker 4	$ 12.00	$ 7.00	$ 7.00
Worker 5	$ 10.00	$ 10.00	$ 10.00
Worker 6	$ 10.00	$ 10.00	$ 10.00
Worker 7	$ 7.00	$ 7.00	$ 7.00
Worker 8	$ 7.00	$ 7.00	$ 7.00
Worker 9	$ 5.00	$ 5.00	$ 7.00
Worker 10	$ 5.00	$ 5.00	$ 7.00
Wage Percentile:			
90th	$ 15.00	$ 15.00	$ 15.00
75th	$ 12.00	$ 10.00	$ 10.00
50th	$ 10.00	$ 7.00	$ 7.00
25th	$ 7.00	$ 7.00	$ 7.00
10th	$ 5.00	$ 5.00	$ 7.00

Table 3.6: Construction Worker Characteristics by State Prevailing Wage Law Status

Worker Characteristics	State Group, 1980-1992			
	PWL	Repeal	No PWL	Total
All Construction Workers				
Median wage	$ 8.13	$ 7.57	$ 6.16	$ 7.75
% Union	32.6%	18.7%	8.9%	28.3%
Median potential labor force experience	14.0	13.0	13.0	14.0
% High school graduate	48.5%	47.9%	43.0%	47.7%
% Nonwhite	18.7%	16.3%	21.4%	18.9%
% Female	1.5%	1.9%	1.7%	1.5%
% of employed blue collar workforce	14.0%	18.5%	13.8%	14.2%
Unweighted N	*67,647*	*6,907*	*13,859*	*88,413*

Note: See text for details.

Table 3.7: Labor Market Characteristics by State Prevailing Wage Law Status, 1980-1992

Labor Market Characteristic	State Group			
	PWL	Repeal	No PWL	All States
# of states with Right-to-Work laws by 1980	6	2	8	16
# of states with state minimum>federal minimum (1980-1992)[a]	14	0	2	16
Average Union Density[b]	34%	20%	15%	30%
Median Blue Collar Wage (excluding construction)[c]	$ 6.97	$ 6.64	$ 5.62	$ 6.64
# of states	33	5	8	46

Notes: [a]This number includes all states that had state minimums that exceeded the federal minimum at least one month during 1980-1992.[b]Average union density is taken as the average over individuals. The sample sizes are 300,439; 22,999; and 65,899 for PWL states, Repeal states and No PWL states respectively. Union status data is from 1983-1992. [c]Median blue collar wage based on sample of individual workers across states, the unweighted sample sizes are 412,818; 33,190; and 86,603 for PWL states, Repeal states and No PWL states respectively. See text for further details.

Table 3.8: Construction Worker Characteristics by Union and State Prevailing Wage Law Status

	State Group, 1983-1992			
Worker Characteristics	PWL	Repeal	No PWL	Total
Union Construction Workers				
Median wage	$ 12.07	$ 10.60	$ 9.69	$ 11.87
Median potential labor force experience	19.0	19.0	19.0	19.0
% High school graduate	54.4%	53.4%	52.0%	54.2%
% Nonwhite	15.5%	14.8%	16.5%	15.6%
% Female	1.1%	1.7%	1.3%	1.2%
Unweighted N	*17,062*	*899*	*993*	*18,954*
Nonunion Construction Workers				
Median wage	$ 6.78	$ 6.88	$ 5.91	$ 6.60
Median potential labor force experience	12.0	13.0	13.0	12.0
% High school graduate	46.8%	47.3%	42.9%	46.1%
% Nonwhite	20.9%	16.2%	22.3%	20.8%
% Female	1.7%	1.9%	1.9%	1.7%
Unweighted N	*35,069*	*3,980*	*9,786*	*48,835*

Note: See text for details.

Table 3.9: Construction Worker Characteristics by Occupation and State Prevailing Wage Law Status, 1980-1992

Worker Characteristics	State Group PWL	Repeal	No PWL	Total
Laborers				
Median wage	$ 6.37	$ 5.67	$ 4.78	$ 6.03
% Union	28.7%	16.3%	8.6%	25.6%
Median potential labor force experience	12.0	11.0	10.0	12.0
% High school graduate	43.0%	39.4%	37.4%	42.1%
% Nonwhite	28.1%	28.6%	36.8%	29.1%
% Female	2.6%	3.3%	3.1%	2.7%
% of employed blue collar workforce	2.3%	2.8%	1.8%	2.3%
Unweighted N	*11,025*	*1,086*	*1,909*	*14,020*
Carpenters				
Median wage	$ 7.97	$ 7.57	$ 6.10	$ 7.57
% Union	24.7%	13.5%	5.3%	21.2%
Median potential labor force experience	12.0	11.0	13.0	12.0
% High school graduate	49.7%	46.8%	41.9%	48.4%
% Nonwhite	15.0%	9.0%	14.7%	14.6%
% Female	0.9%	1.0%	0.9%	0.9%
% of employed blue collar workforce	2.3%	2.8%	2.3%	2.3%
Unweighted N	*11,266*	*1,116*	*2,326*	*14,708*

Note: See text for details.

Table 3.10: Wage Effects of State Prevailing Wage Law Repeals, After Years: 1988-1992

	Wage Percentile				
	10th	25th	50th	75th	90th
Control States: PWL					
Laborers					
Before	-0.01	-0.05	-0.06	-0.13	-0.17
	(0.04)	(0.06)	(0.07)	(0.06)	(0.04)
After	-0.04	-0.03	-0.09	-0.18	-0.16
	(0.04)	(0.05)	(0.05)	(0.11)	(0.09)
Change	-0.03	0.02	-0.03	-0.05	0.00
	(0.06)	(0.06)	(0.05)	(0.09)	(0.08)
Carpenters					
Before	0.05	0.08	0.01	-0.06	-0.10
	(0.04)	(0.06)	(0.07)	(0.07)	(0.04)
After	0.07	0.04	0.01	-0.12	-0.24
	(0.06)	(0.07)	(0.05)	(0.06)	(0.09)
Change	0.01	-0.04	0.00	-0.06	-0.14
	(0.06)	(0.05)	(0.05)	(0.09)	(0.09)
Control States: No PWL					
Laborers					
Before	0.02	0.00	0.01	0.10	0.10
	(0.04)	(0.06)	(0.05)	(0.05)	(0.04)
After	-0.02	-0.02	-0.01	0.00	0.09
	(0.05)	(0.04)	(0.05)	(0.11)	(0.1)
Change	-0.04	-0.02	-0.01	-0.10	-0.01
	(0.06)	(0.06)	(0.05)	(0.09)	(0.1)
Carpenters					
Before	0.12	0.10	0.08	0.13	0.14
	(0.04)	(0.05)	(0.05)	(0.07)	(0.05)
After	0.05	0.10	0.08	0.05	0.01
	(0.06)	(0.06)	(0.04)	(0.05)	(0.09)
Change	-0.07	0.01	0.00	-0.08	-0.12
	(0.06)	(0.05)	(0.05)	(0.08)	(0.09)

Notes: Before years are 1980-1984. After years are: 1988-1992. Quantile regression estimates are based on weighted data. Bootstrapped standard errors are in parentheses. Resampling for bootstrapped standard errors take into account nonindependence of within-state observations. See text for details.

Table 3.11: Wage Effects of State Prevailing Wage Law Repeals, After Years: 1989-1993

	10th	25th	50th	75th	90th
Control States: PWL					
Laborers					
Before	-0.01	-0.05	-0.06	-0.13	-0.17
	(0.04)	(0.06)	(0.07)	(0.06)	(0.04)
After	-0.02	0.02	-0.08	-0.20	-0.13
	(0.03)	(0.03)	(0.05)	(0.08)	(0.07)
Change	-0.01	0.07	-0.02	-0.07	0.03
	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)
Carpenters					
Before	0.05	0.08	0.01	-0.06	-0.10
	(0.04)	(0.06)	(0.07)	(0.07)	(0.04)
After	0.08	0.09	0.01	-0.14	-0.18
	(0.06)	(0.08)	(0.06)	(0.06)	(0.09)
Change	0.02	0.01	0.00	-0.07	-0.08
	(0.06)	(0.05)	(0.05)	(0.08)	(0.1)
Control States: No PWL					
Laborers					
Before	0.02	0.00	0.01	0.10	0.10
	(0.04)	(0.06)	(0.05)	(0.05)	(0.04)
After	-0.01	0.06	0.00	-0.02	0.08
	(0.04)	(0.03)	(0.04)	(0.07)	(0.07)
Change	-0.03	0.06	0.00	-0.13	-0.02
	(0.06)	(0.07)	(0.07)	(0.06)	(0.07)
Carpenters					
Before	0.12	0.10	0.08	0.13	0.14
	(0.04)	(0.05)	(0.05)	(0.07)	(0.05)
After	0.08	0.14	0.08	0.03	0.06
	(0.06)	(0.07)	(0.05)	(0.06)	(0.1)
Change	-0.04	0.04	0.00	-0.10	-0.08
	(0.07)	(0.05)	(0.06)	(0.08)	(0.1)

Notes: Before years are 1980-1984. After years are: 1989-1993. Quantile regression estimates are based on weighted data. Bootstrapped standard errors are in parentheses. Resampling for bootstrapped standard errors take into account nonindependence of within-state observations. See text for details.

Table 3.12: Wage Effects of State Prevailing Wage Law Repeals, After Years: 1988-1989

		Years of Potential Labor Force Experience	
	Total Sample	<=15	>15
		Control States: No PWL	
All Workers			
Before	0.08	0.08	0.08
	(0.03)	(0.04)	(0.04)
After	0.05	0.07	0.00
	(0.04)	(0.03)	(0.05)
Change	-0.03	-0.01	-0.08
	(0.04)	(0.05)	(0.04)
Non-Union Workers			
Before	0.03	0.05	0.00
	(0.02)	(0.04)	(0.04)
After	0.04	0.05	0.00
	(0.03)	(0.03)	(0.04)
Change	0.01	0.00	0.00
	(0.02)	(0.04)	(0.03)
Union Workers			
Before	0.10	0.07	0.14
	(0.05)	(0.08)	(0.09)
After	0.05	0.19	-0.08
	(0.06)	(0.07)	(0.05)
Change	-0.06	0.12	-0.22
	(0.06)	(0.11)	(0.11)
		Control States: PWL	
All Workers			
Before	-0.04	-0.03	-0.05
	(0.04)	(0.04)	(0.05)
After	-0.06	-0.05	-0.11
	(0.04)	(0.04)	(0.05)
Change	-0.02	-0.02	-0.06
	(0.02)	(0.02)	(0.04)
Non-Union Workers			
Before	0.01	0.01	-0.01
	(0.02)	(0.03)	(0.03)
After	-0.01	-0.02	-0.03
	(0.04)	(0.03)	(0.04)
Change	-0.02	-0.03	-0.02
	(0.02)	(0.02)	(0.03)
Union Workers			
Before	-0.11	-0.18	-0.04
	(0.03)	(0.07)	(0.07)
After	-0.15	-0.07	-0.21
	(0.03)	(0.06)	(0.04)
Change	-0.05	0.11	-0.17
	(0.04)	(0.09)	(0.09)

Notes: Before years are 1983-1984. After years are: 1988-1989.Standard errors are within parentheses and are robust to heteroskedasticity and non-independence within states.

Table 3.13: Wage Effects of State Prevailing Wage Law Repeals, After Years: 1991-1992

	Total Sample	Years of Potential Labor Force Experience <=15	>15
		Control States: No PWL	
All Workers			
Before	0.08	0.08	0.08
	(0.03)	(0.04)	(0.04)
After	0.04	0.04	0.06
	(0.02)	(0.03)	(0.02)
Change	-0.03	-0.04	-0.02
	(0.03)	(0.06)	(0.05)
Non-Union Workers			
Before	0.03	0.05	0.00
	(0.02)	(0.04)	(0.04)
After	0.05	0.04	0.07
	(0.02)	(0.03)	(0.02)
Change	0.02	-0.01	0.08
	(0.03)	(0.05)	(0.05)
Union Workers			
Before	0.10	0.07	0.14
	(0.05)	(0.08)	(0.09)
After	-0.01	0.03	-0.02
	(0.04)	(0.07)	(0.05)
Change	-0.11	-0.05	-0.16
	(0.07)	(0.11)	(0.1)
		Control States: PWL	
All Workers			
Before	-0.04	-0.03	-0.05
	(0.04)	(0.04)	(0.05)
After	-0.05	-0.05	-0.05
	(0.03)	(0.03)	(0.04)
Change	-0.01	-0.02	0.00
	(0.03)	(0.03)	(0.04)
Non-Union Workers			
Before	0.01	0.01	-0.01
	(0.02)	(0.03)	(0.03)
After	0.03	0.00	0.05
	(0.02)	(0.03)	(0.03)
Change	0.02	-0.01	0.06
	(0.03)	(0.02)	(0.04)
Union Workers			
Before	-0.11	-0.18	-0.04
	(0.03)	(0.07)	(0.07)
After	-0.22	-0.16	-0.26
	(0.03)	(0.04)	(0.04)
Change	-0.12	0.02	-0.22
	(0.04)	(0.08)	(0.05)

Notes: Before years are 1983-1984. After years are: 1991-1992.Standard errors are within parentheses and are robust to heteroskedasticity and non-independence within states.

FIGURES

Figure 2.1: Trends in the Nominal and Real Values of the Federal Minimum Wage, 1938-2004

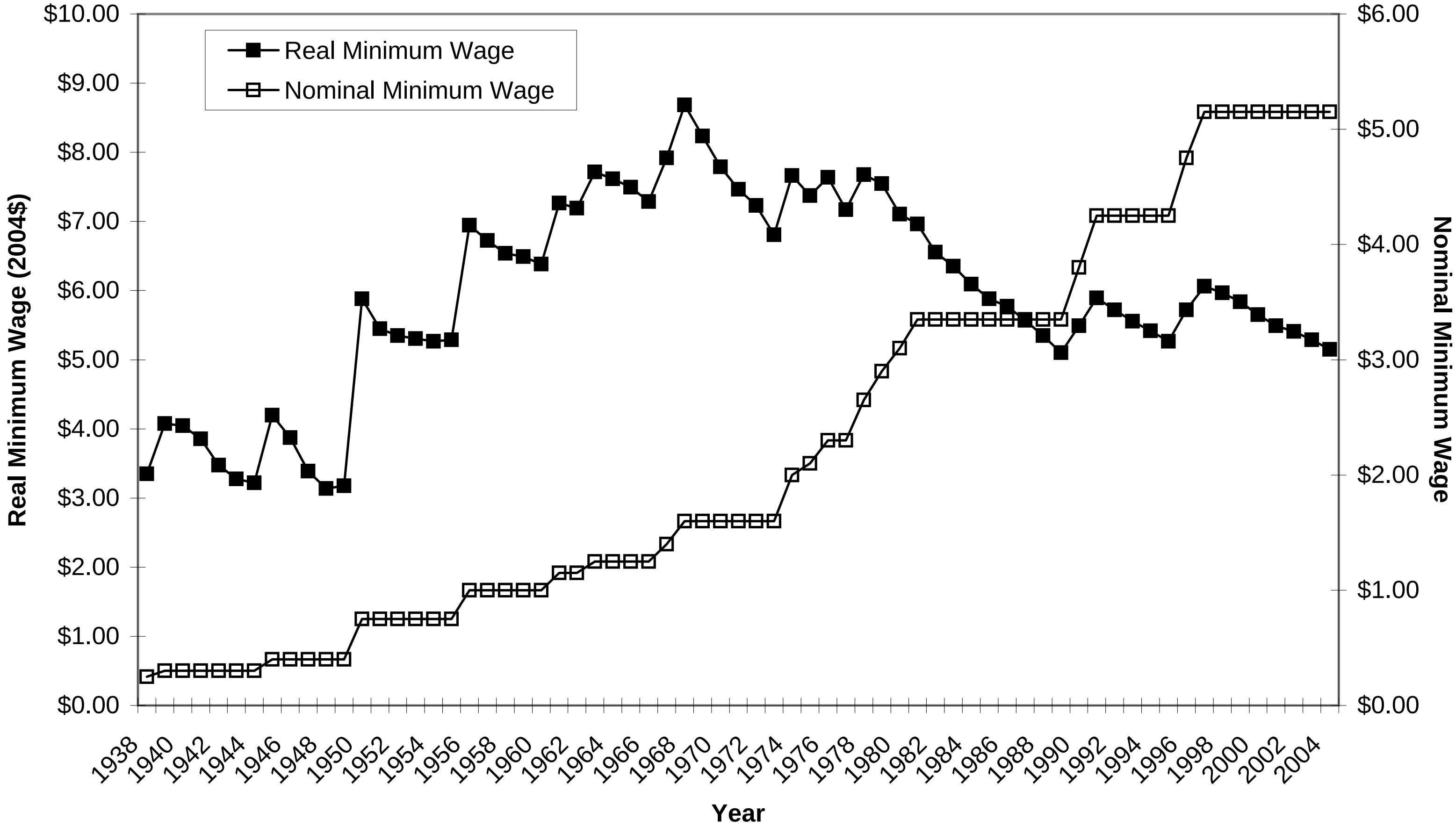

Figure 2.2: Comparing Wage Regions Defined by Neumark, Schweitzer, and Wascher (2004) to Wage Percentiles

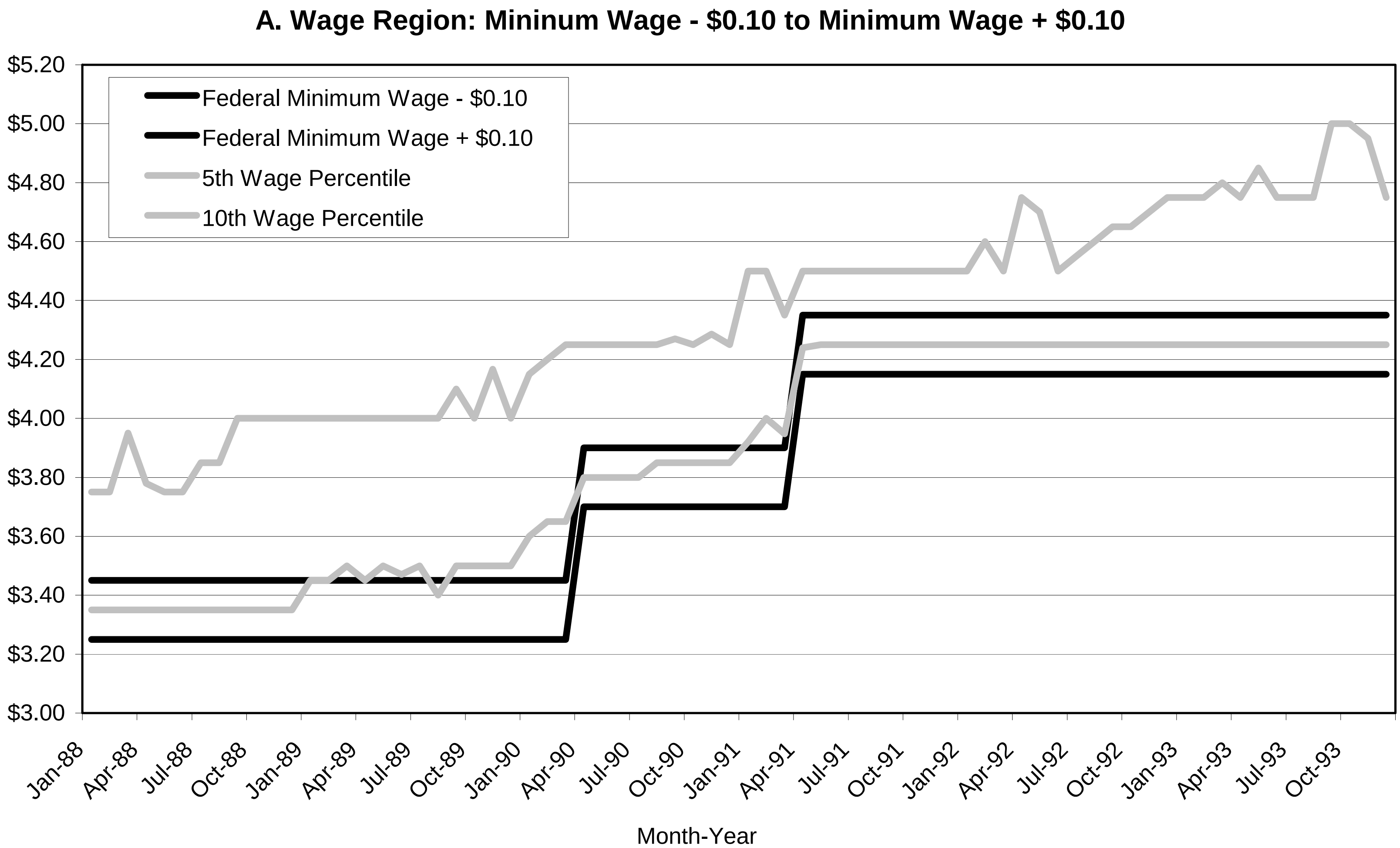

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Figure 2.2 (cont'd): Comparing Wage Regions Defined by Neumark, Schweitzer, and Wascher (2004) to Wage Percentiles

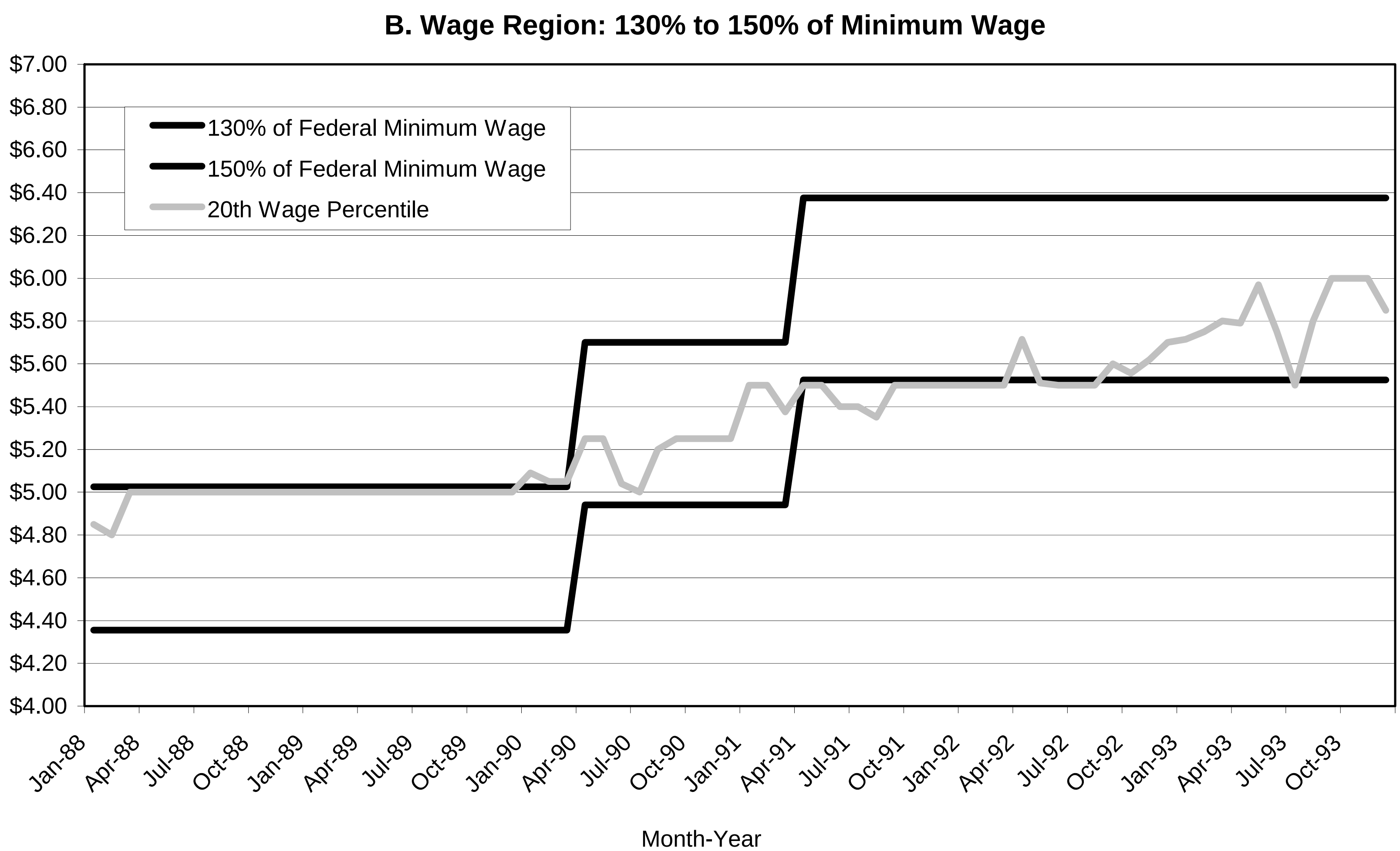
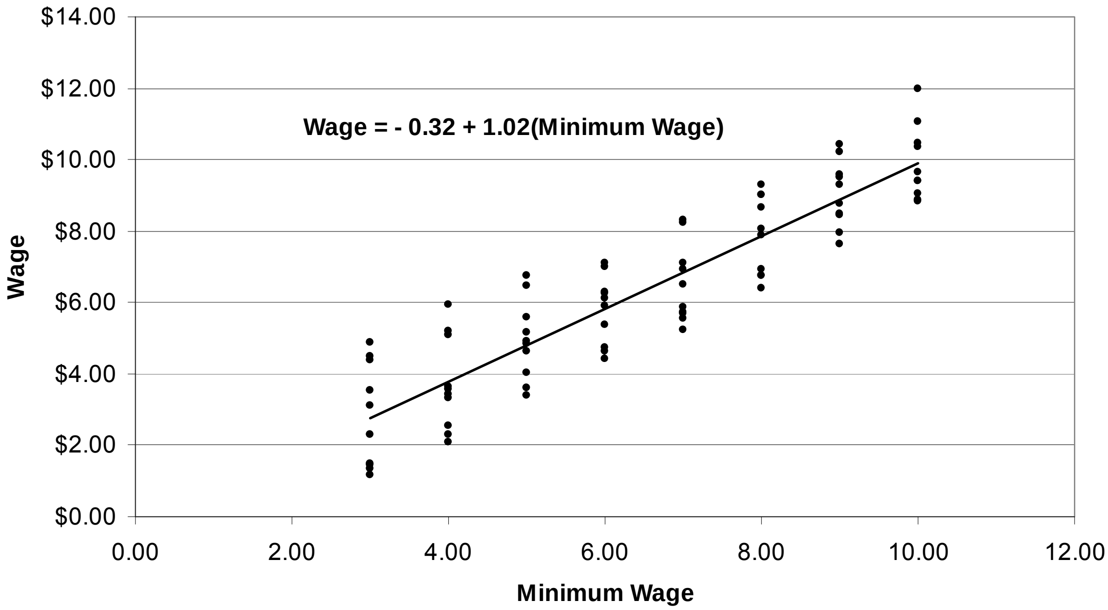


Figure 2.3: Illustrating the Sample Truncation Problem with Hypothetical Wage Data

A. OLS Regression Estimate of: Wage = α + βMinimum Wage + ε
All Data



B. OLS Regression Estimate of: Wage = α + βMinimum Wage + ε
Using Truncated Sample:
Observations within Wage Interval of \$2.00 - \$6.00, only

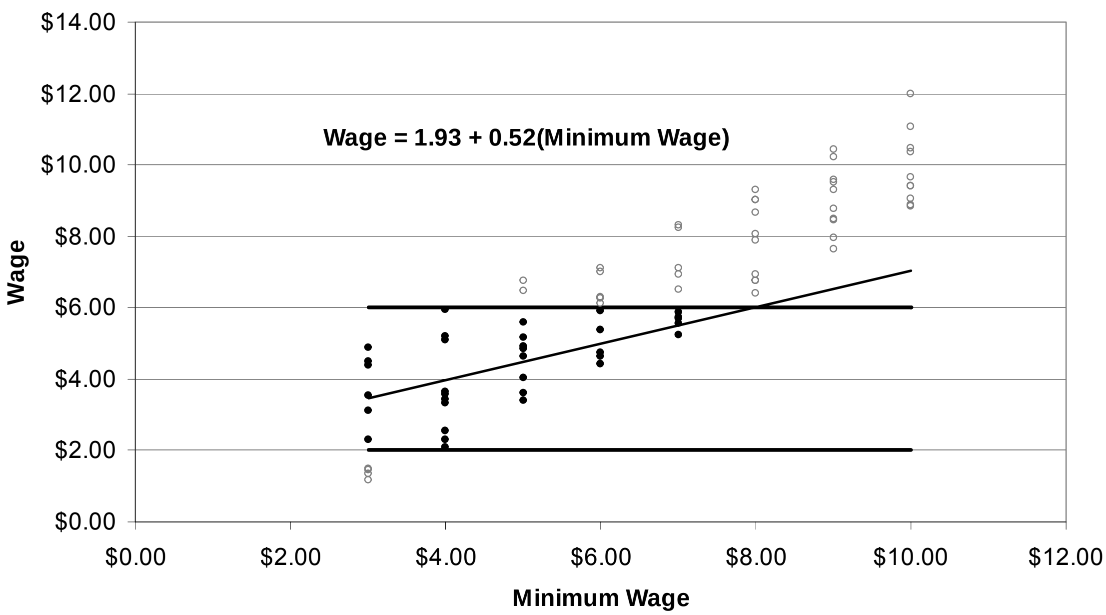


Figure 2.4: Estimated Wage Elasticities by Wage Percentile, Total Sample

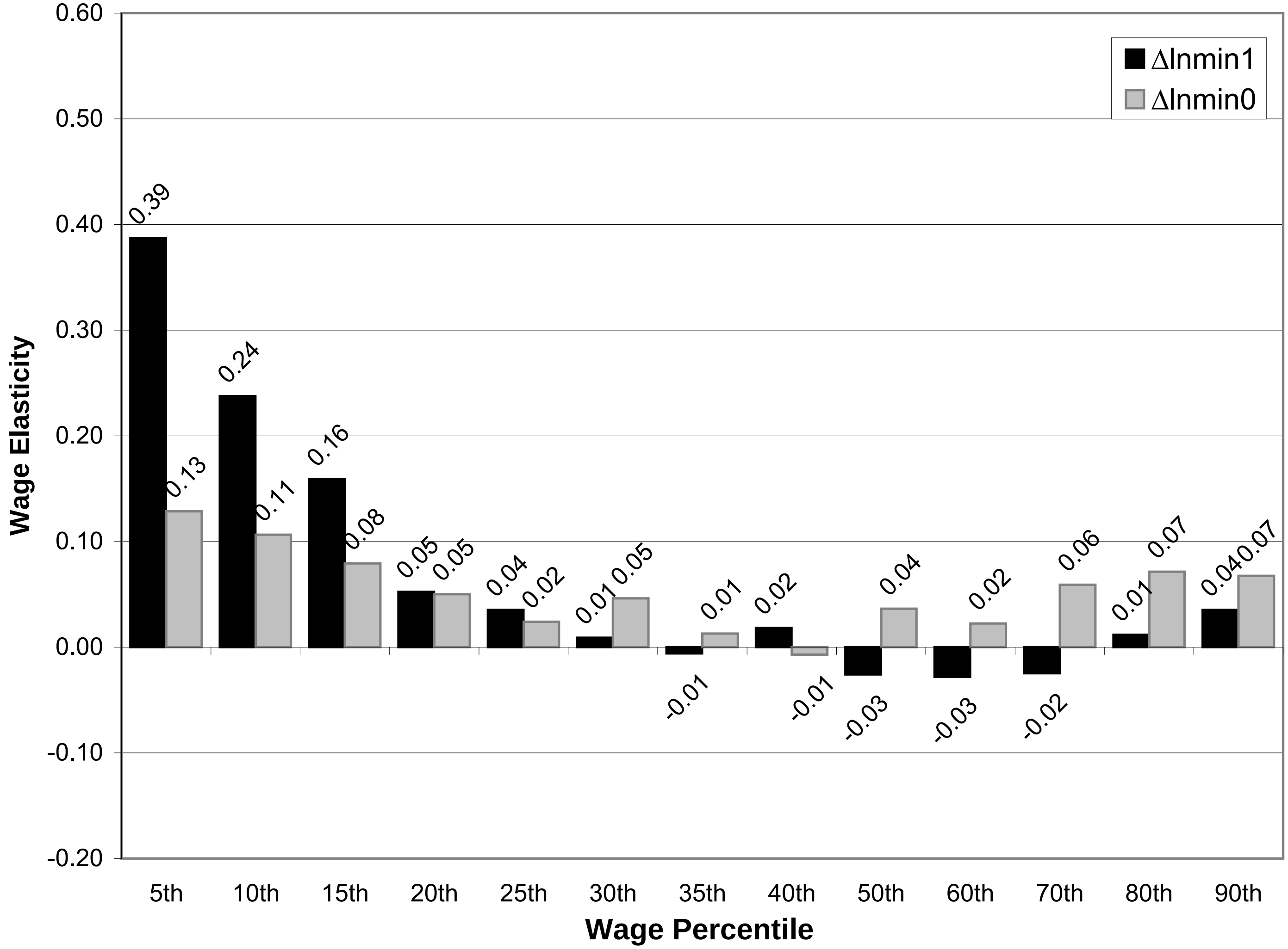


Figure 2.5: Estimated Wage Elasticities by Wage Percentile with Exclusions

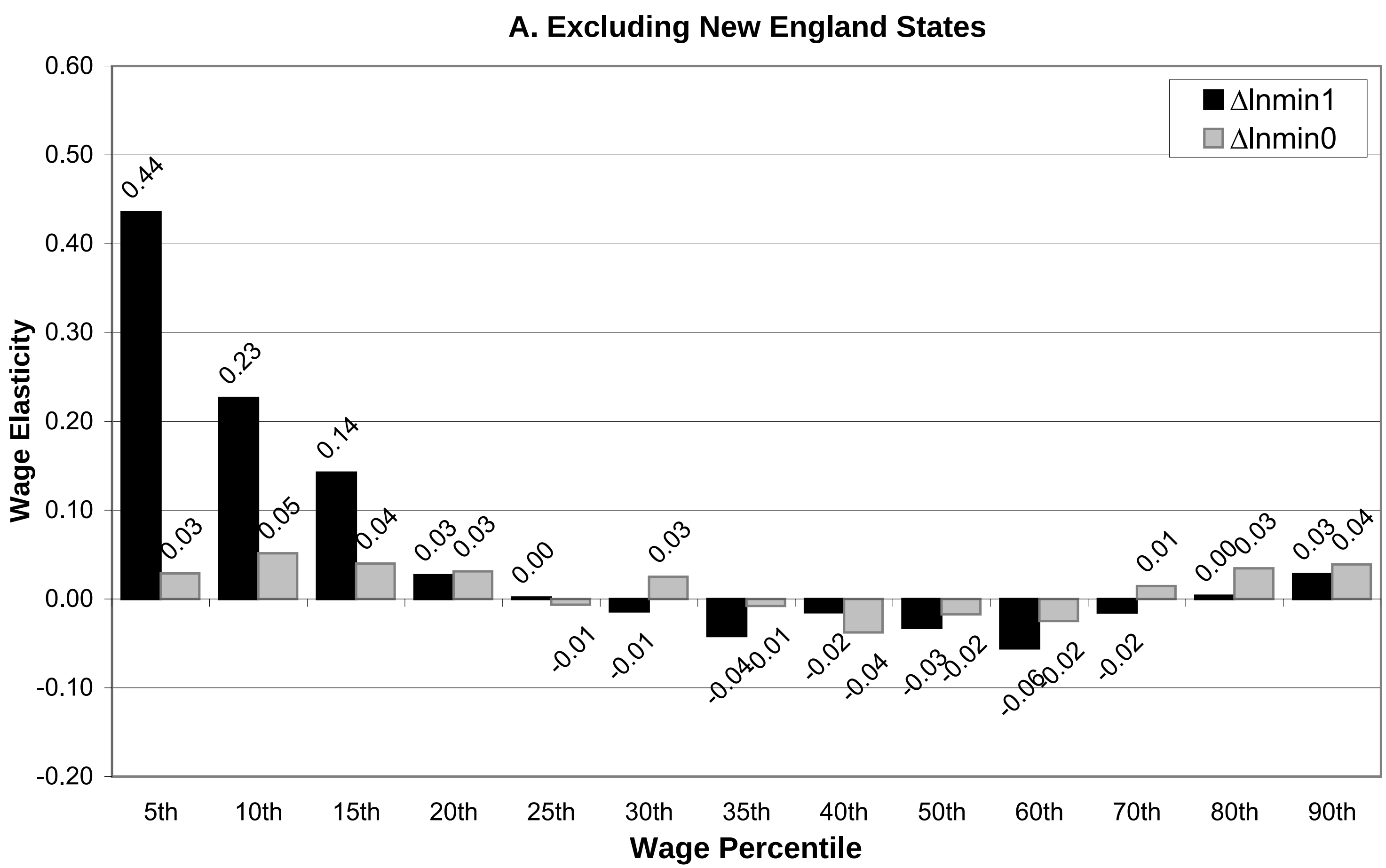


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Figure 2.5 (cont'd): Estimated Wage Elasticities by Wage Percentile with Exclusions

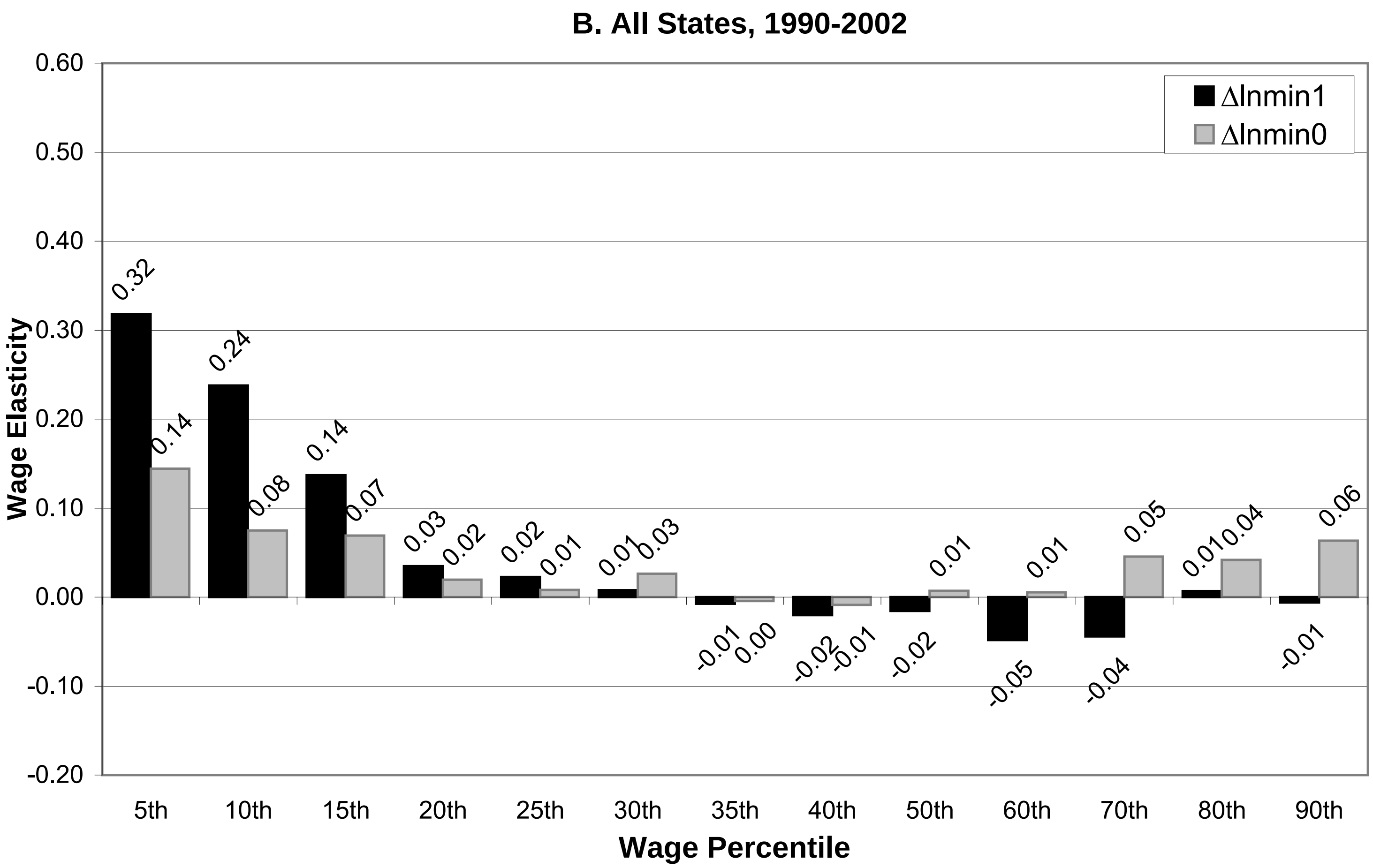

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Figure 2.5 (cont'd): Estimated Wage Elasticities by Wage Percentile with Exclusions

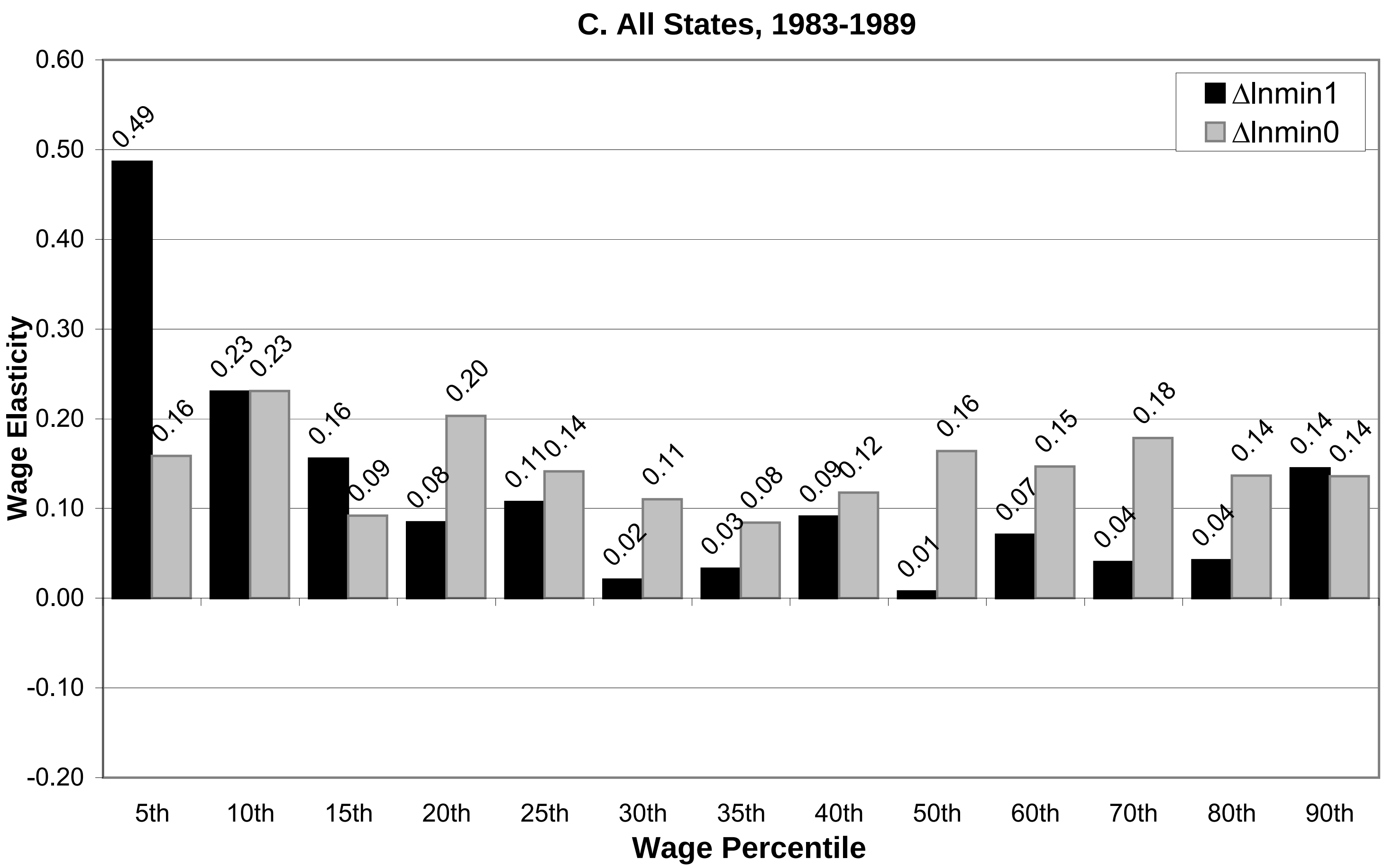

Figure 2.6: Estimated Wage Elasticities by Wage Percentile with PROPDAW
All Years, New England States Excluded

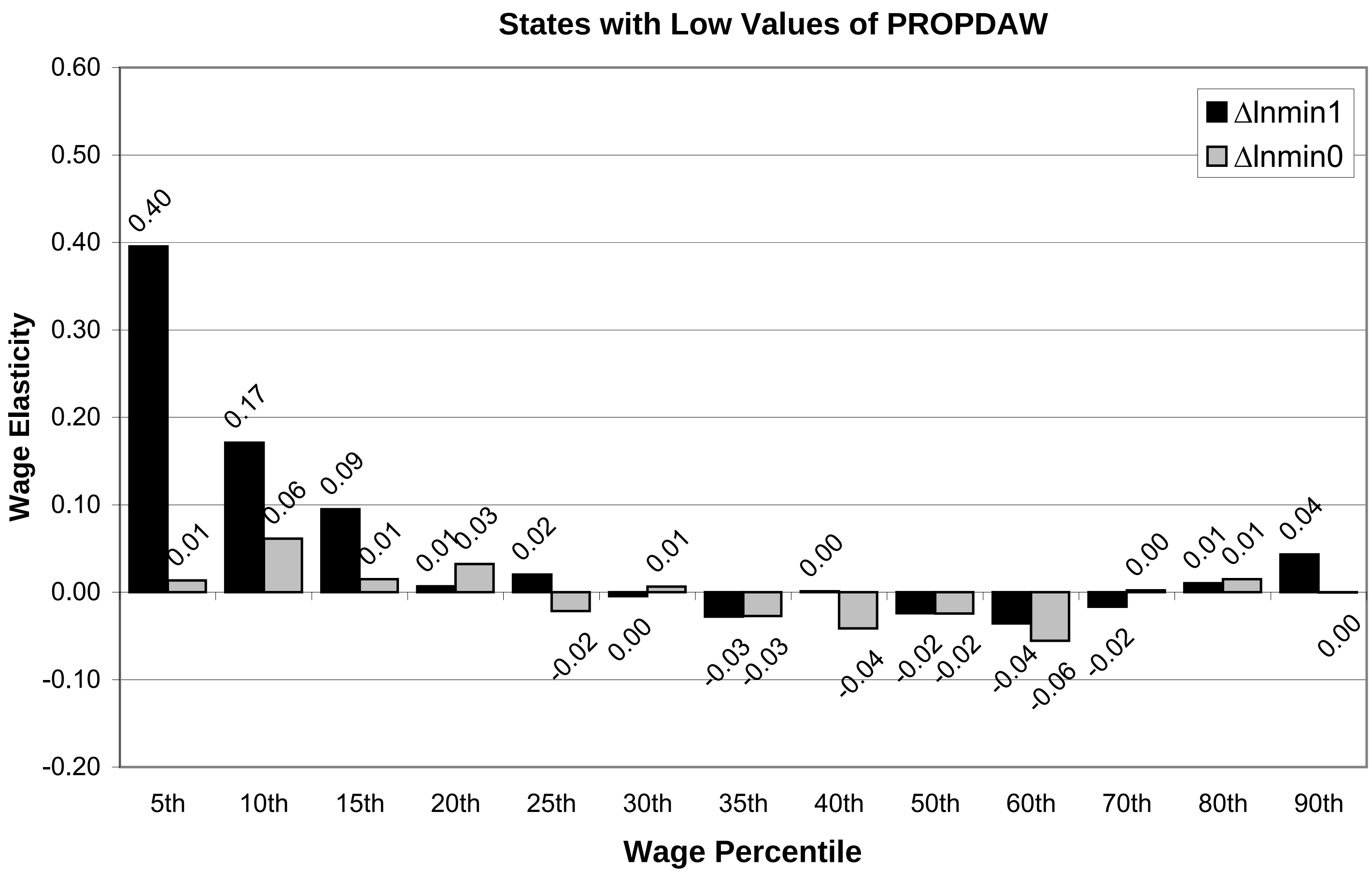

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Figure 2.6 (cont'd): Estimated Wage Elasticities by Wage Percentile with PROPDAW
All Years, New England States Excluded

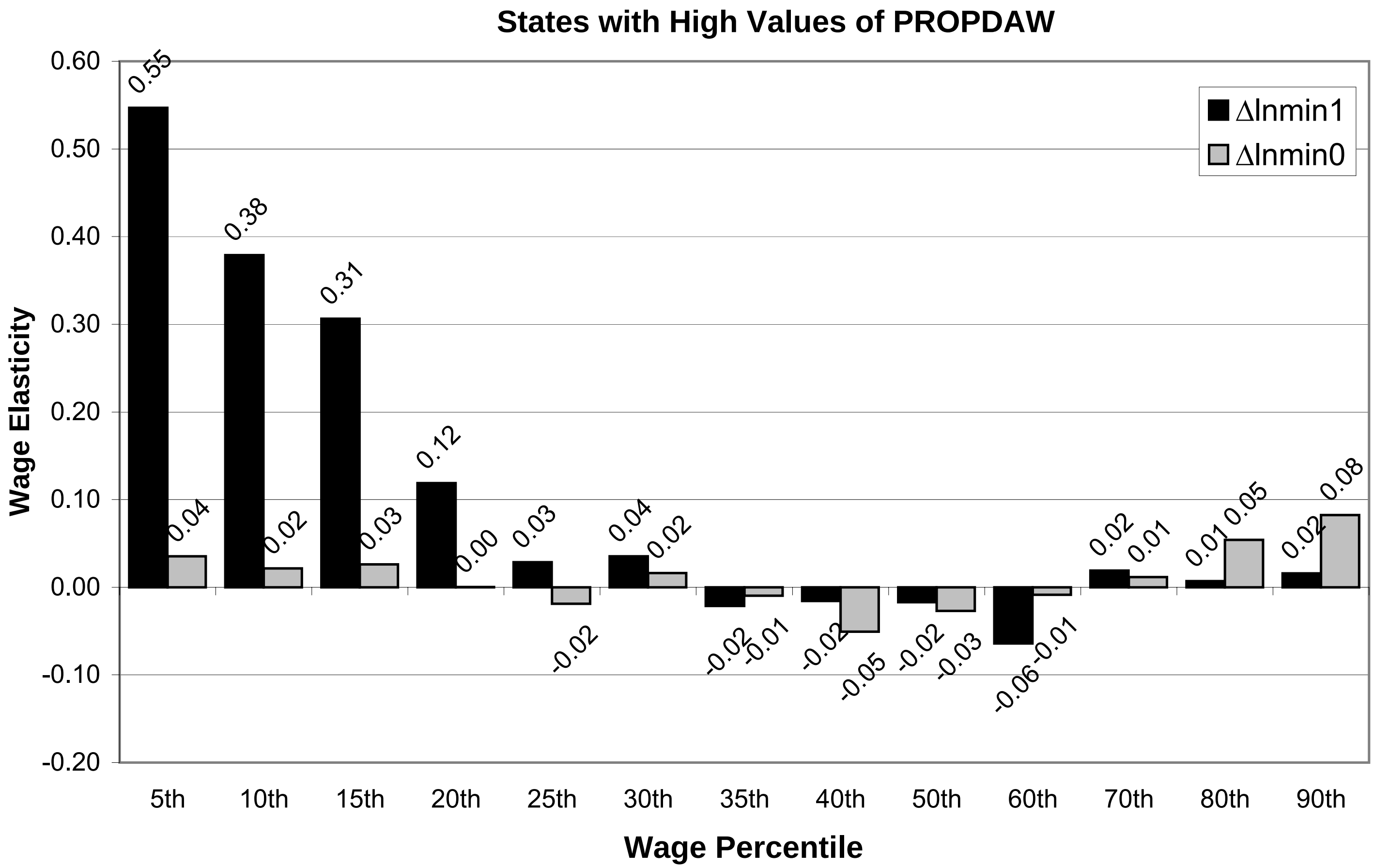

Figure 2.7: Estimated Wage Elasticities by Wage Percentile, Retail Trade Industry

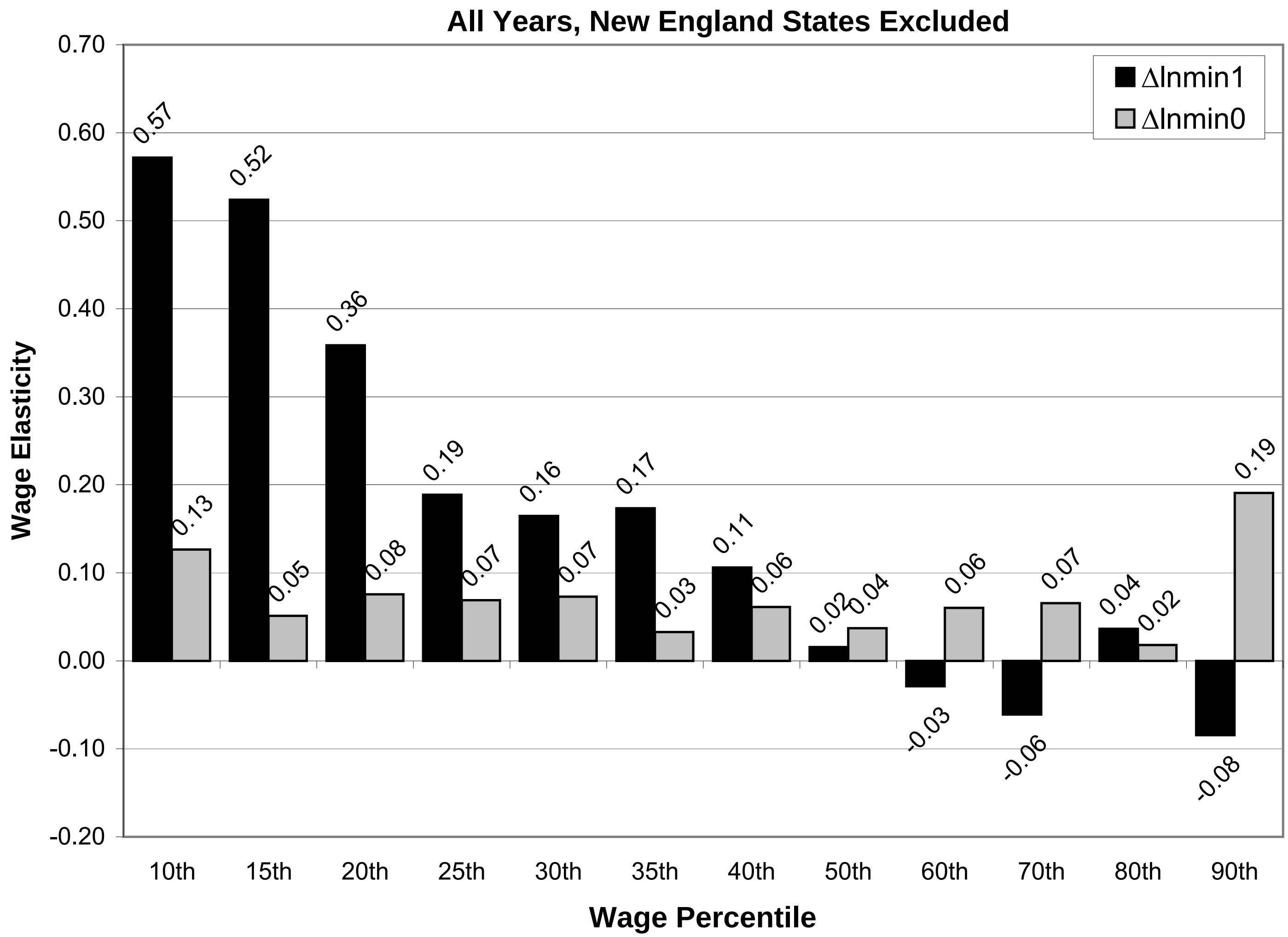

Figure 2.8: Estimated Wage Elasticities by Wage Percentile with PROPDAW, Retail Trade Industry All Years, New England States Excluded

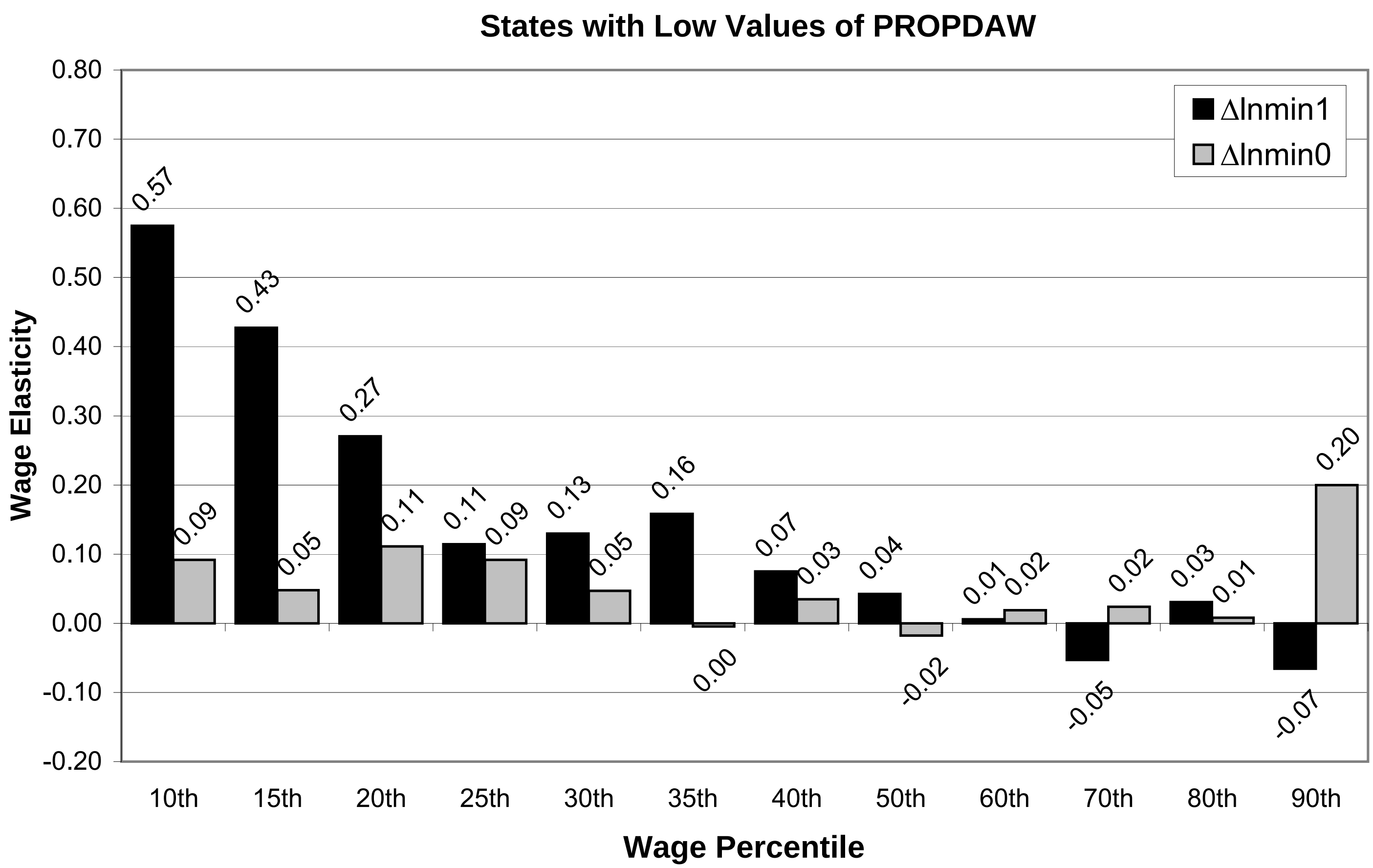

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Figure 2.8 (cont'd): Estimated Wage Elasticities by Wage Percentile with PROPDAW, Retail Trade Industry
All Years, New England States Excluded

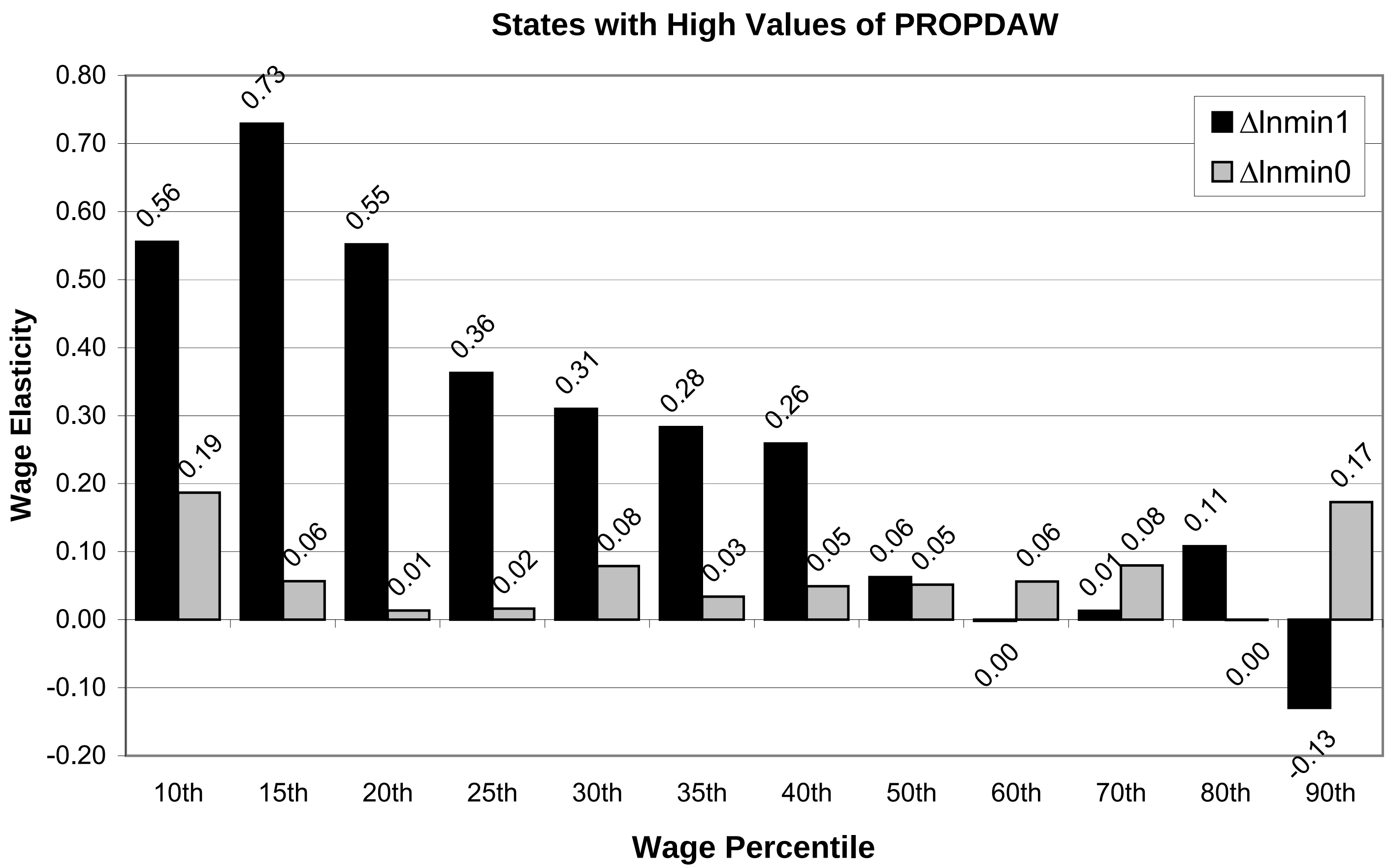

Figure 2.9: Trends in 6-Month Averages of Wage Percentiles, Prevailing Minimum Wages, and Price Level 1983-2001

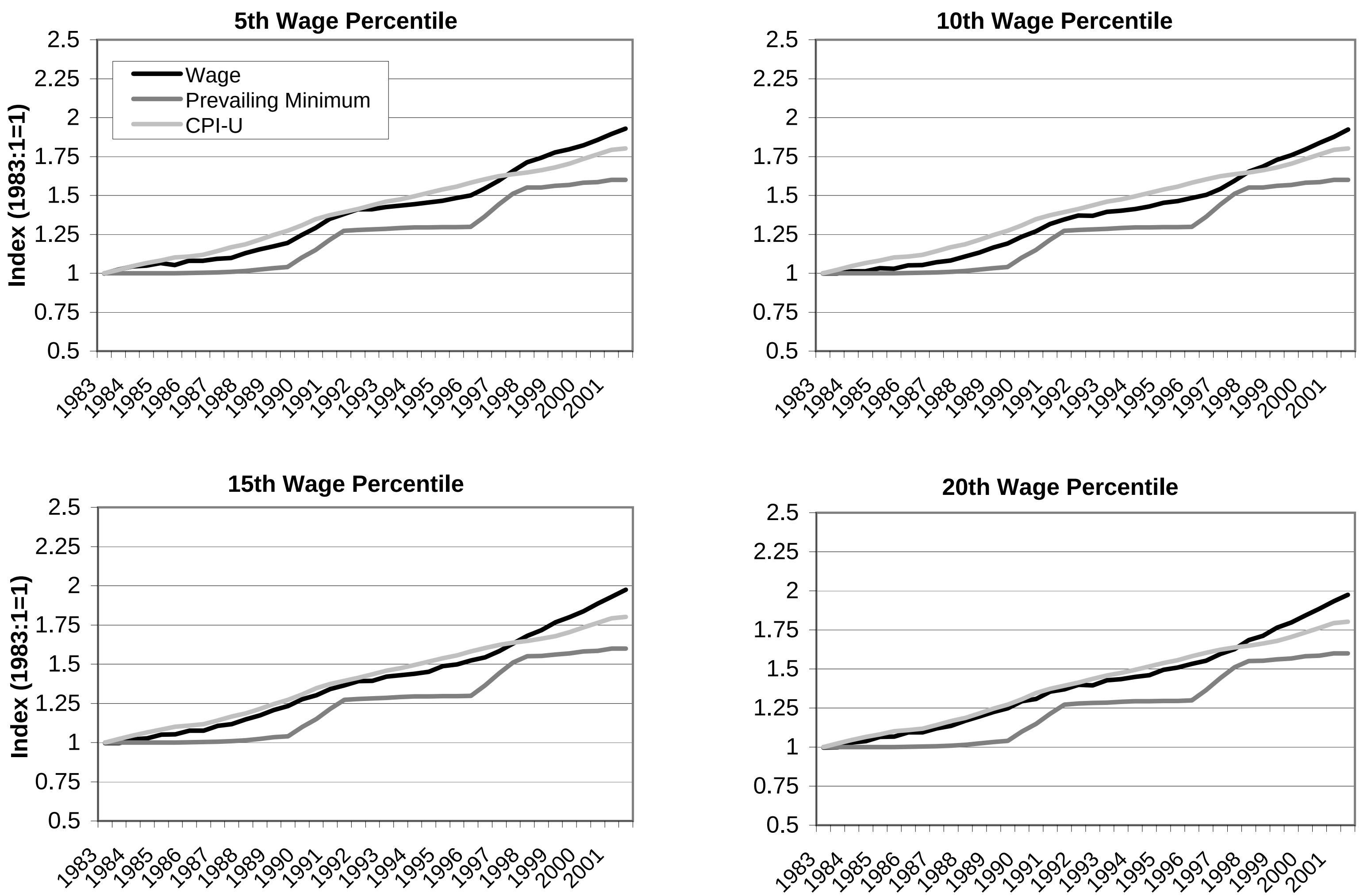

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Figure 2.9 (cont'd): Trends in 6-Month Averages of Wage Percentiles, Prevailing Minimum Wages, and Price Level 1983-2001

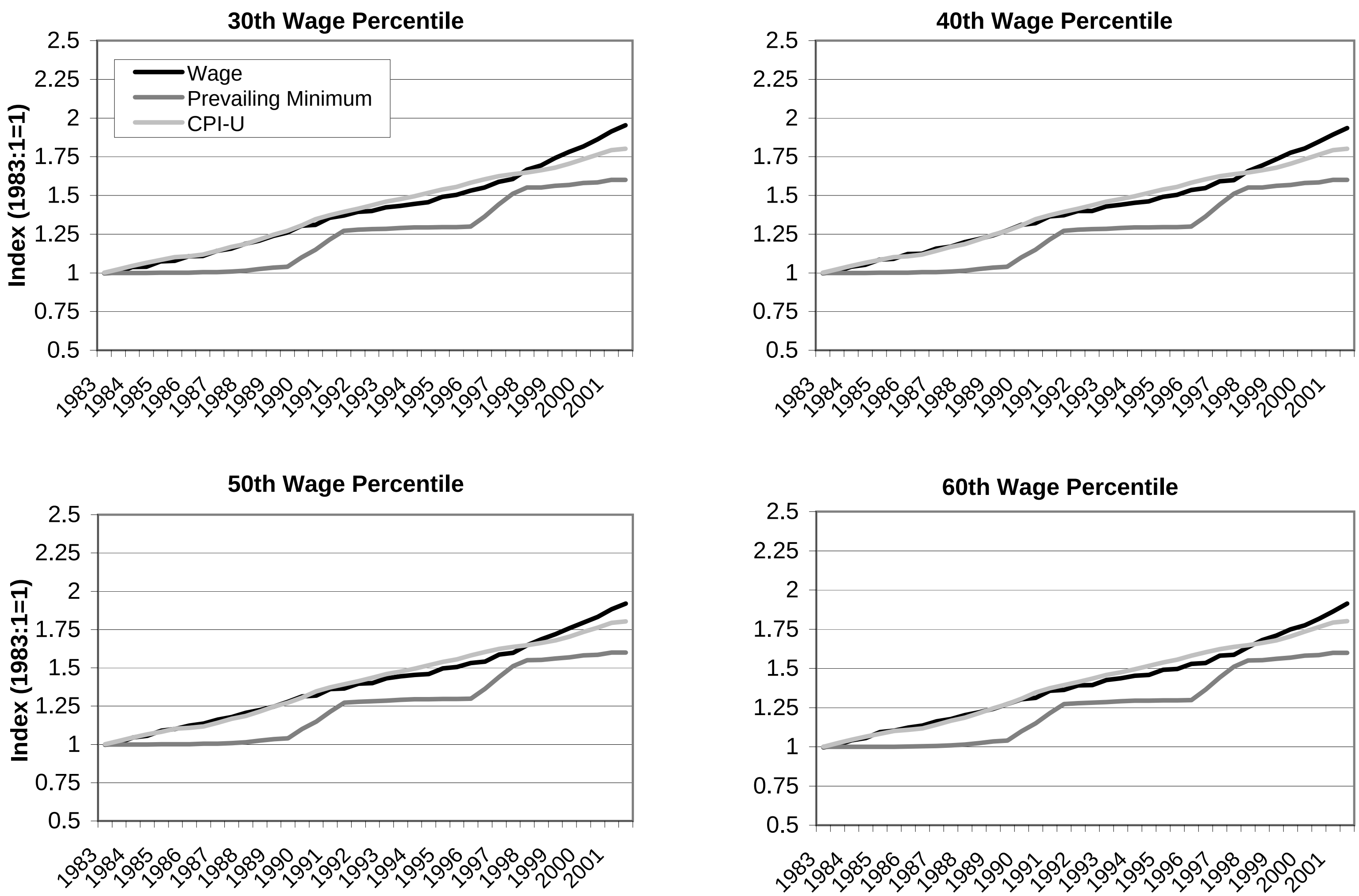
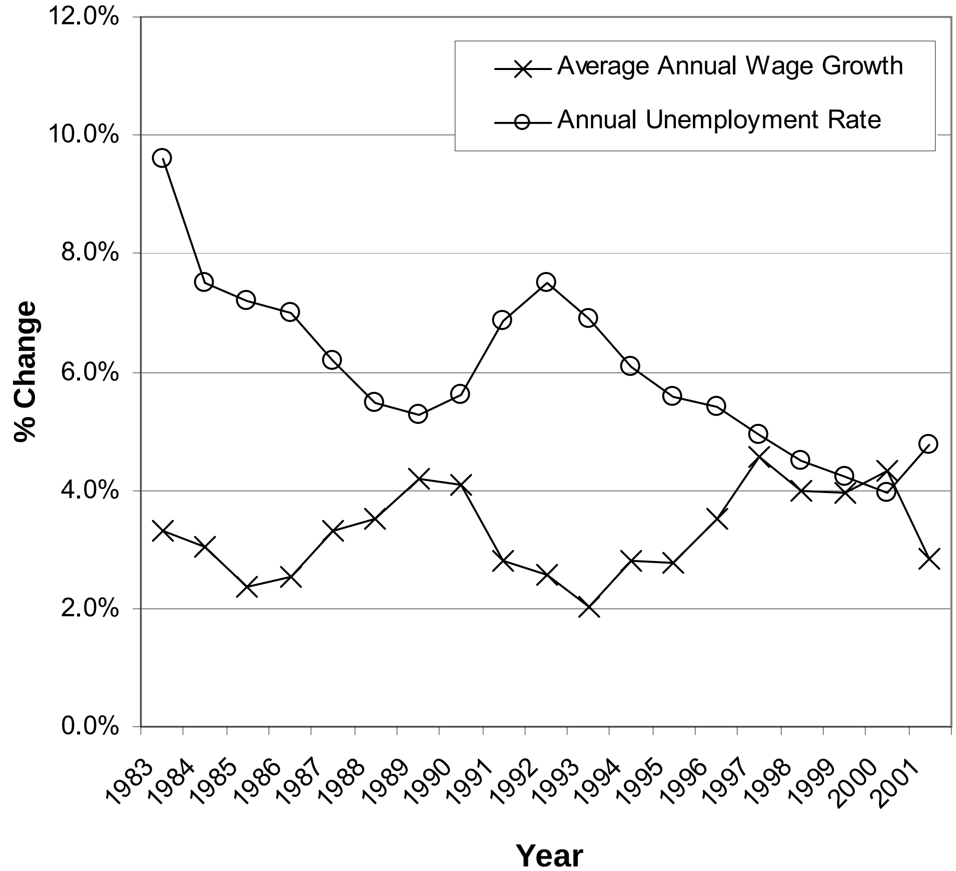


Figure 2.10: Annual Wage Growth Averaged Over Wage Percentiles



Source: 12-month average of BLS national, monthly estimates of unemployment based on CPS. Average annual wage growth based on the regression estimates of year dummies in equation 2.8 using sample excluding New England states.

Figure 2.11: Average Annual Wage Growth by Wage Percentile

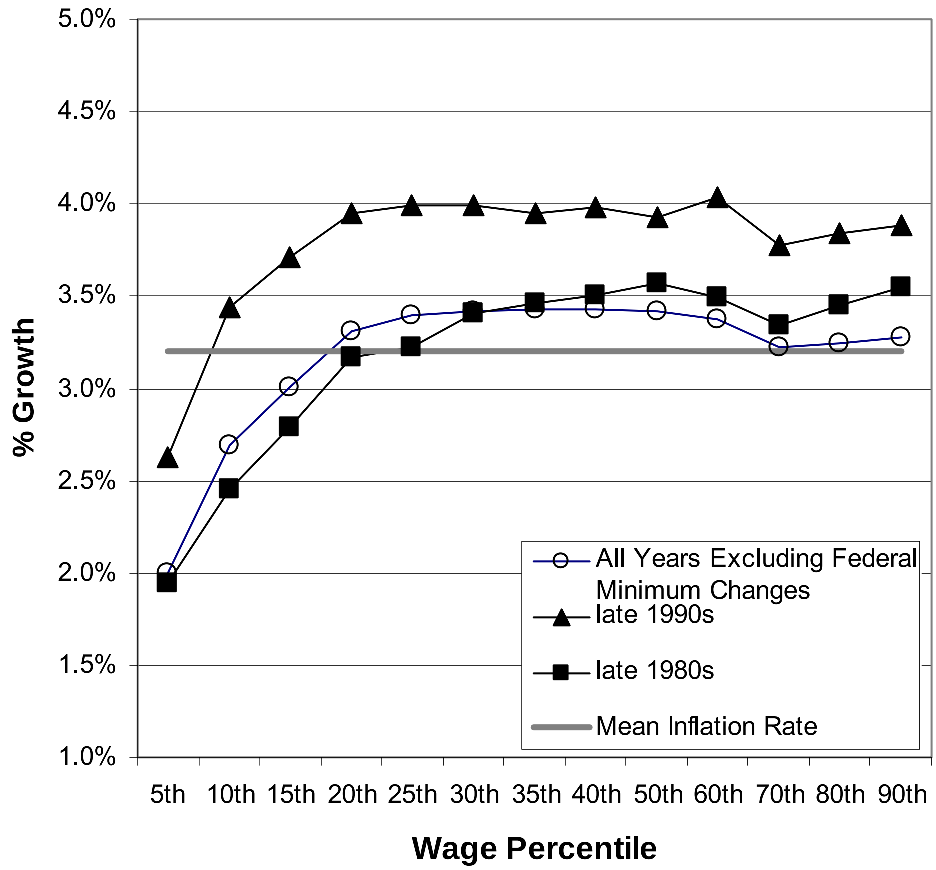


Figure 3.1: Wage Levels of Construction Workers by Union Status and Potential Labor Force Experience

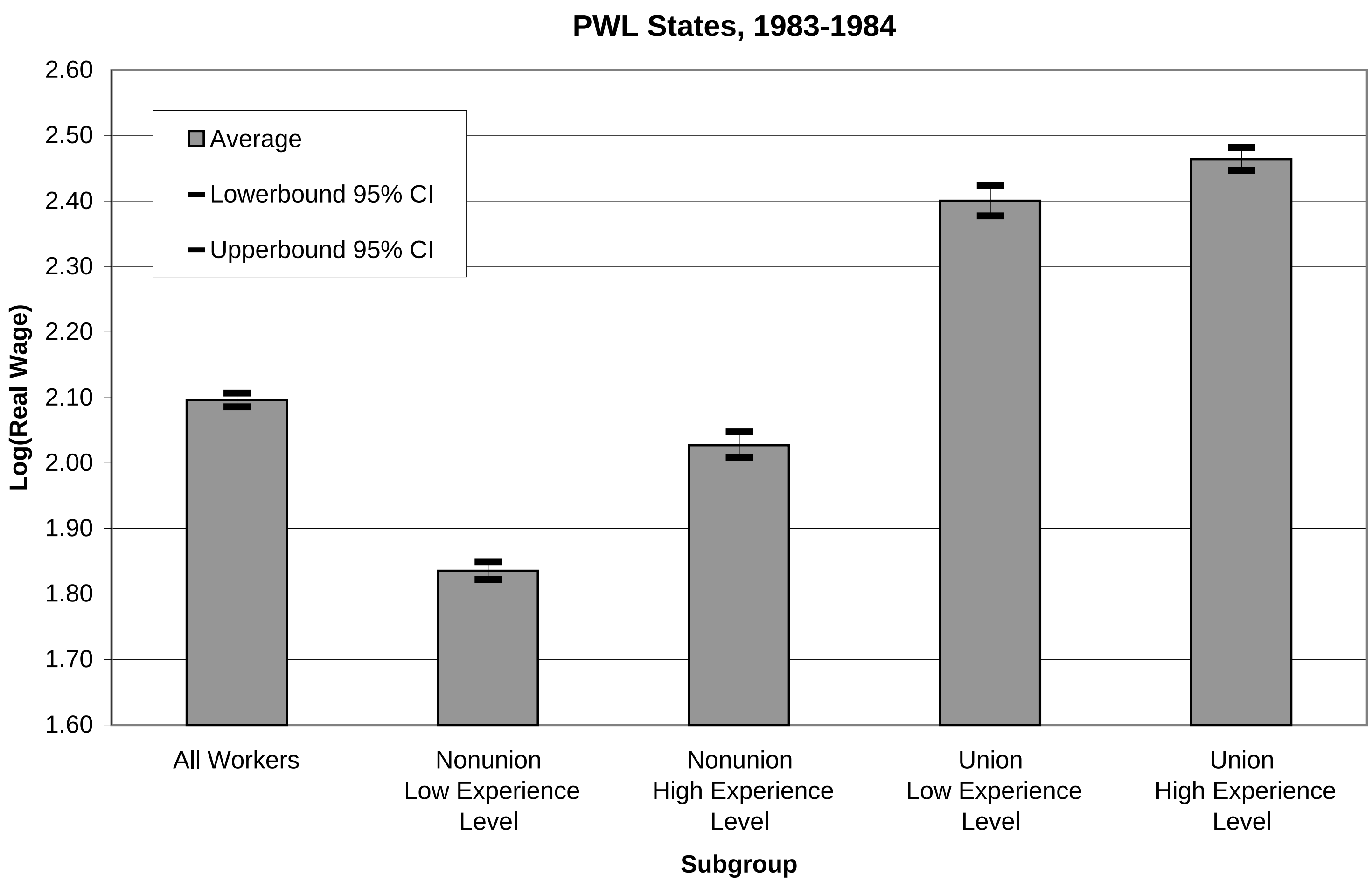


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Figure 3.1 (cont'd): Wage Levels of Construction Workers by Union Status and Potential Labor Force Experience

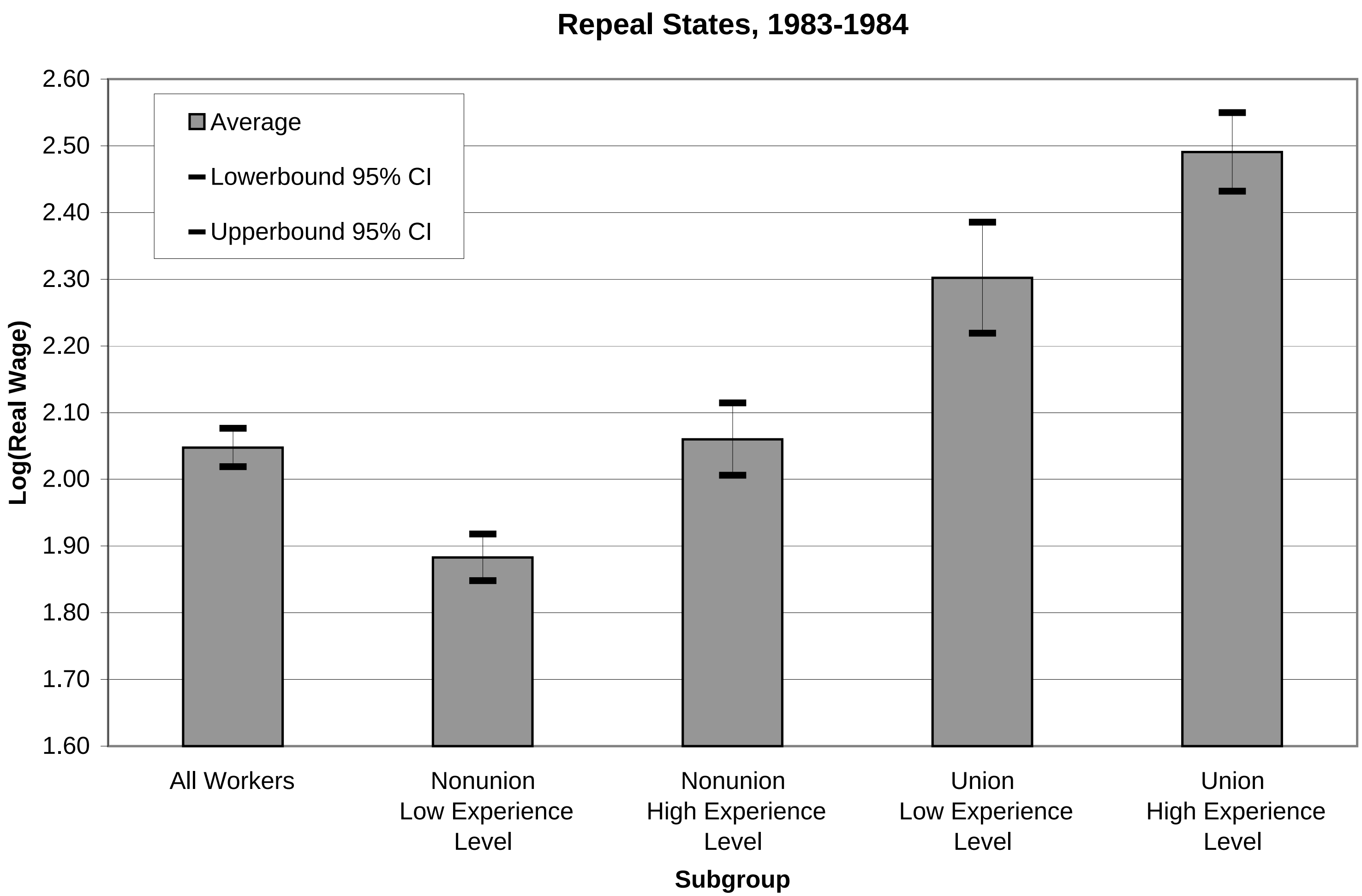

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Figure 3.1 (cont'd): Wage Levels of Construction Workers by Union Status and Potential Labor Force Experience

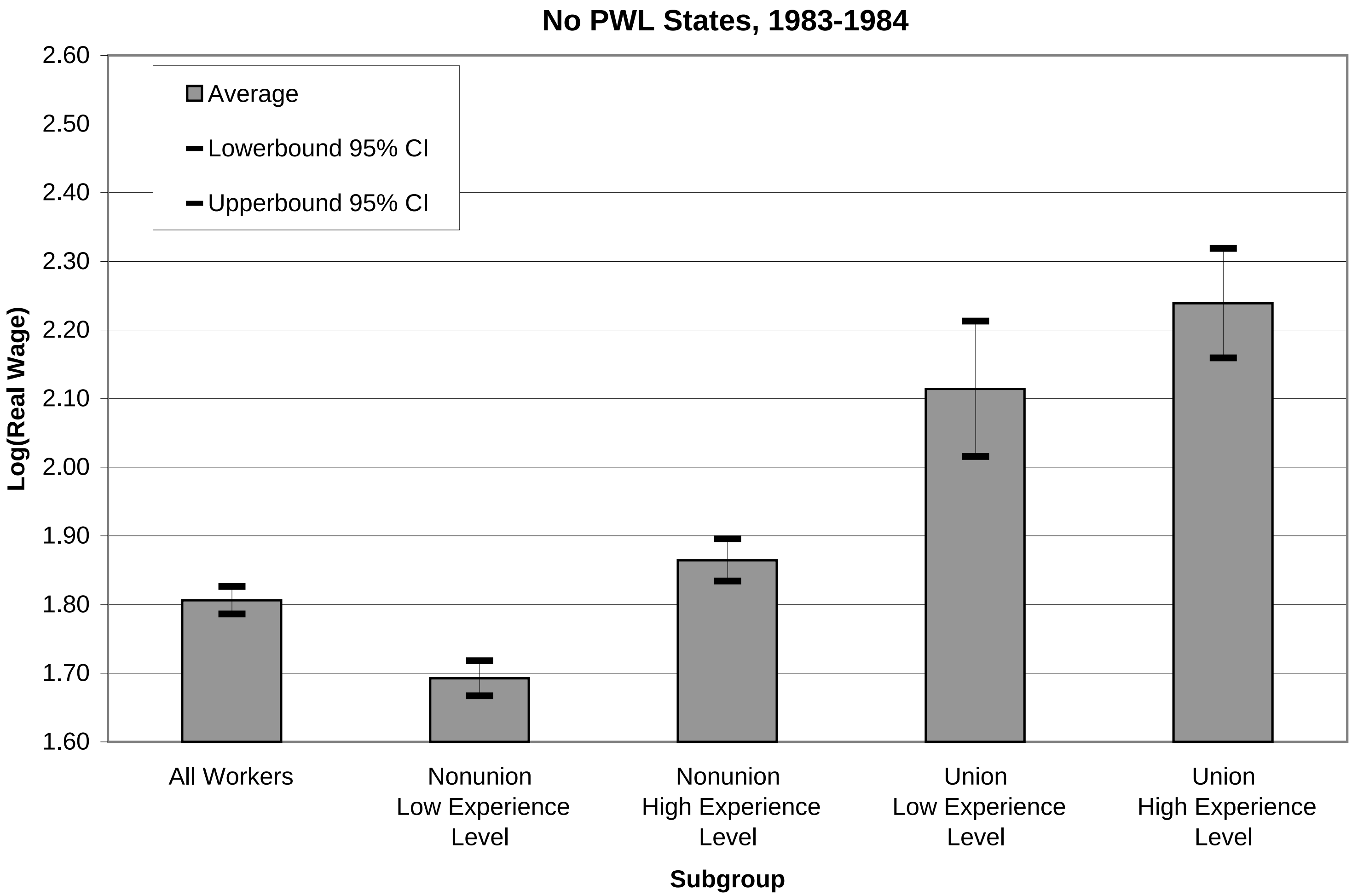

Figure 3.2: Kernel Density Estimates of Real Wages in Repeal States by Occupation and Union Status, Before and After State Prevailing Wage Law Repeals (Before Repeal Years: 1983-1984, After Repeal Years: 1988-1989)

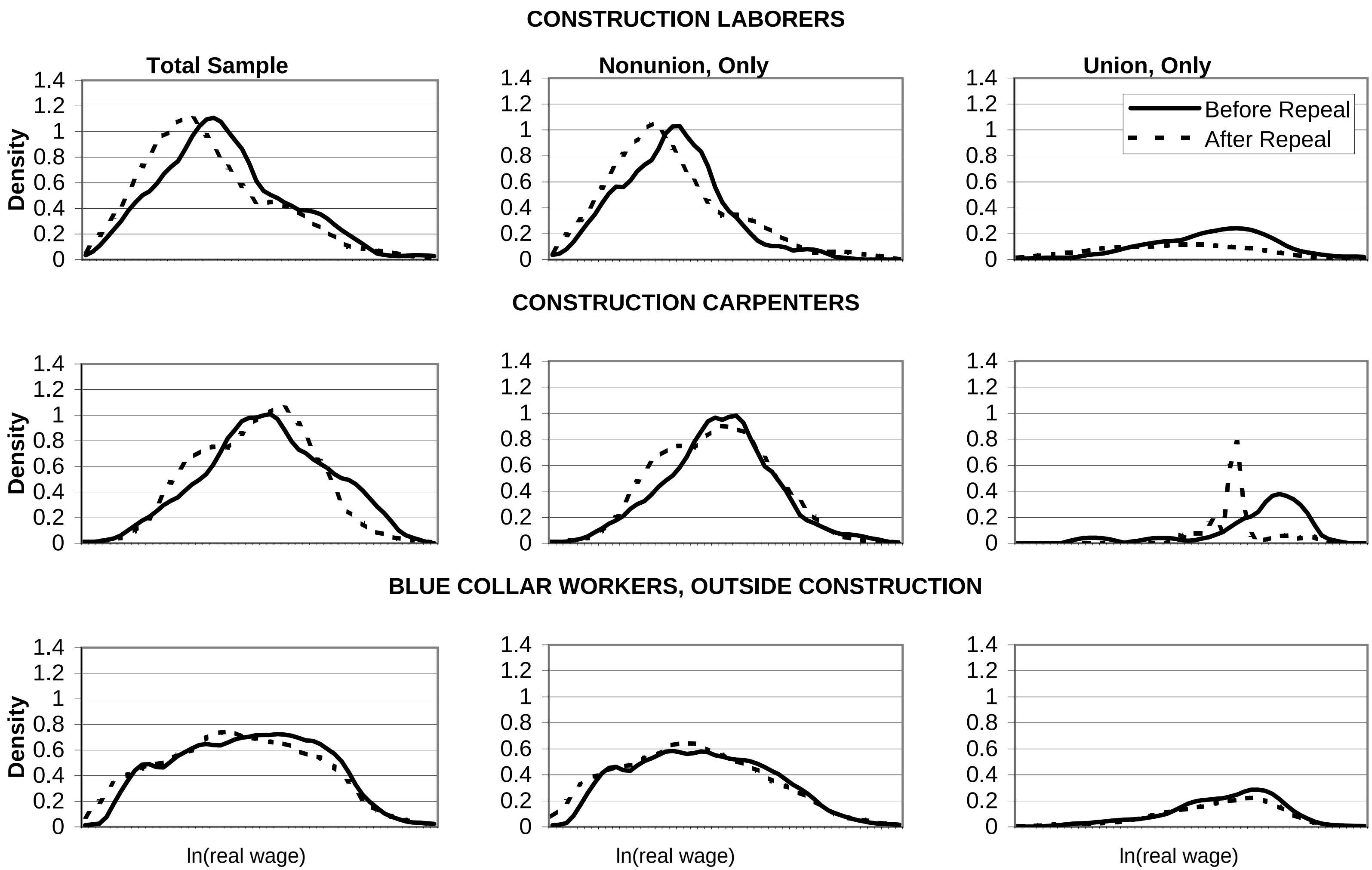

Figure 3.3: Wage Effects of State Prevailing Wage Law Repeals

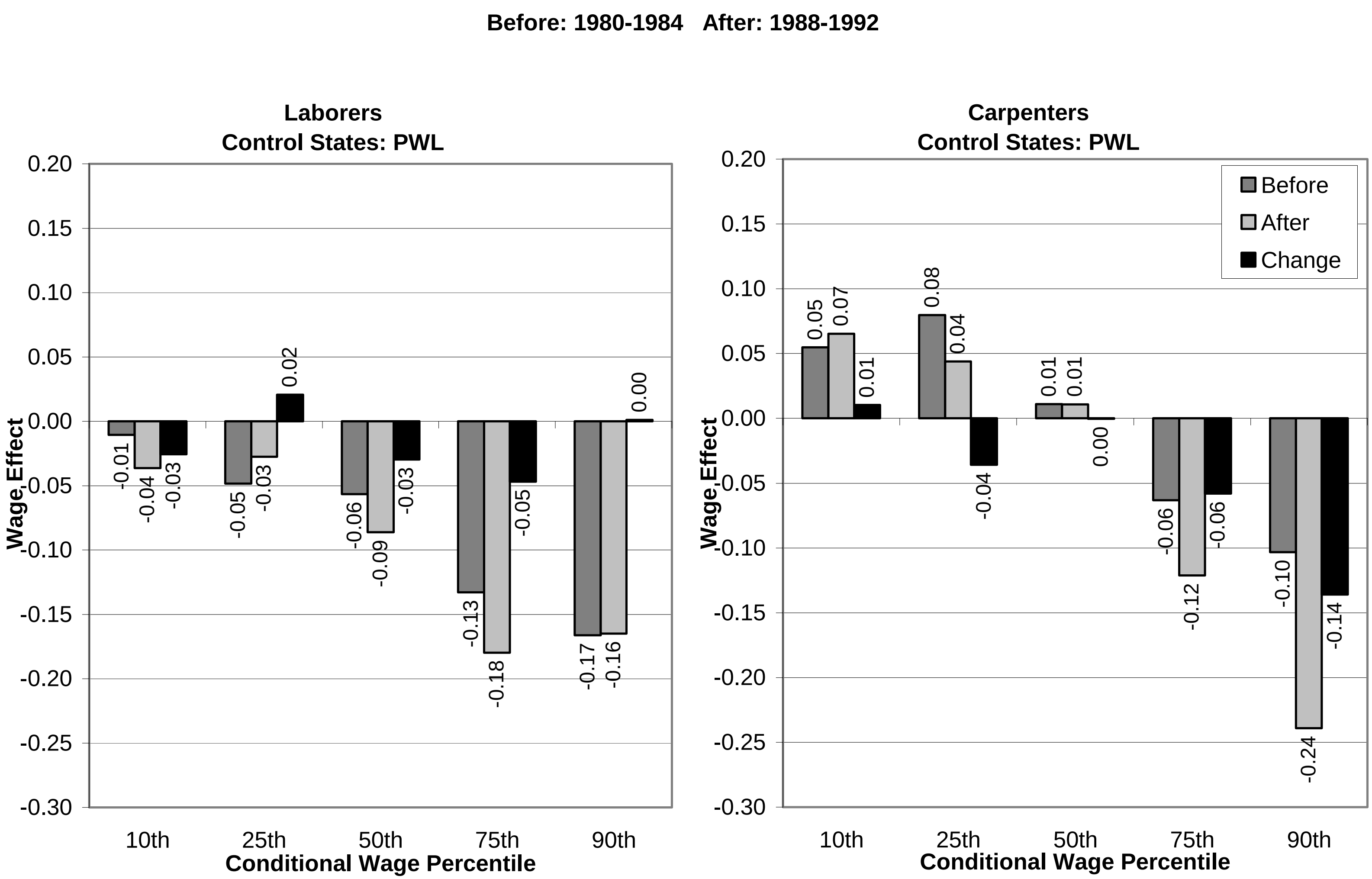

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Figure 3.3 (cont'd): Wage Effects of State Prevailing Wage Law Repeals

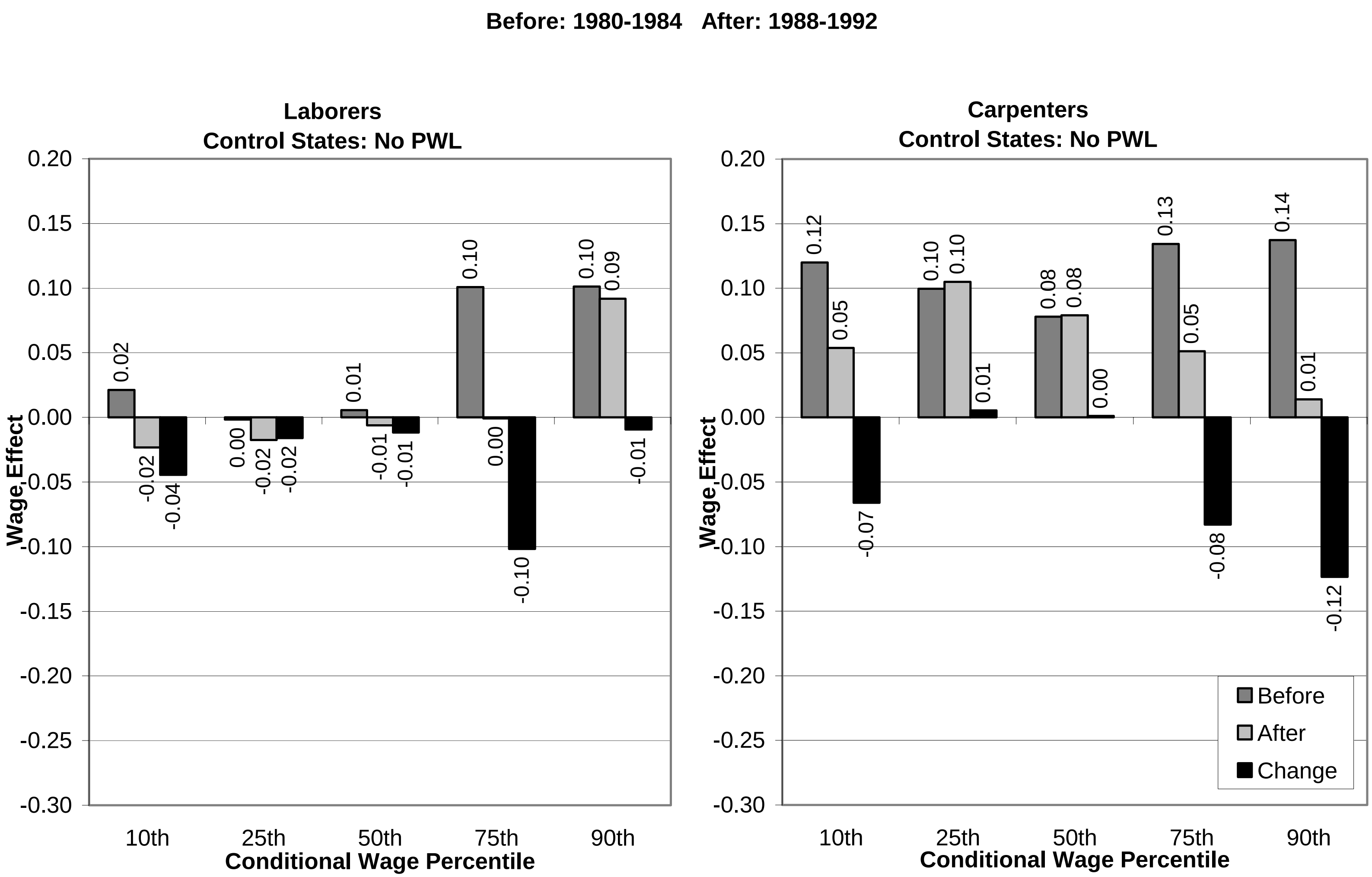

Figure 3.4: Wage Effects of State Prevailing Wage Law Repeals

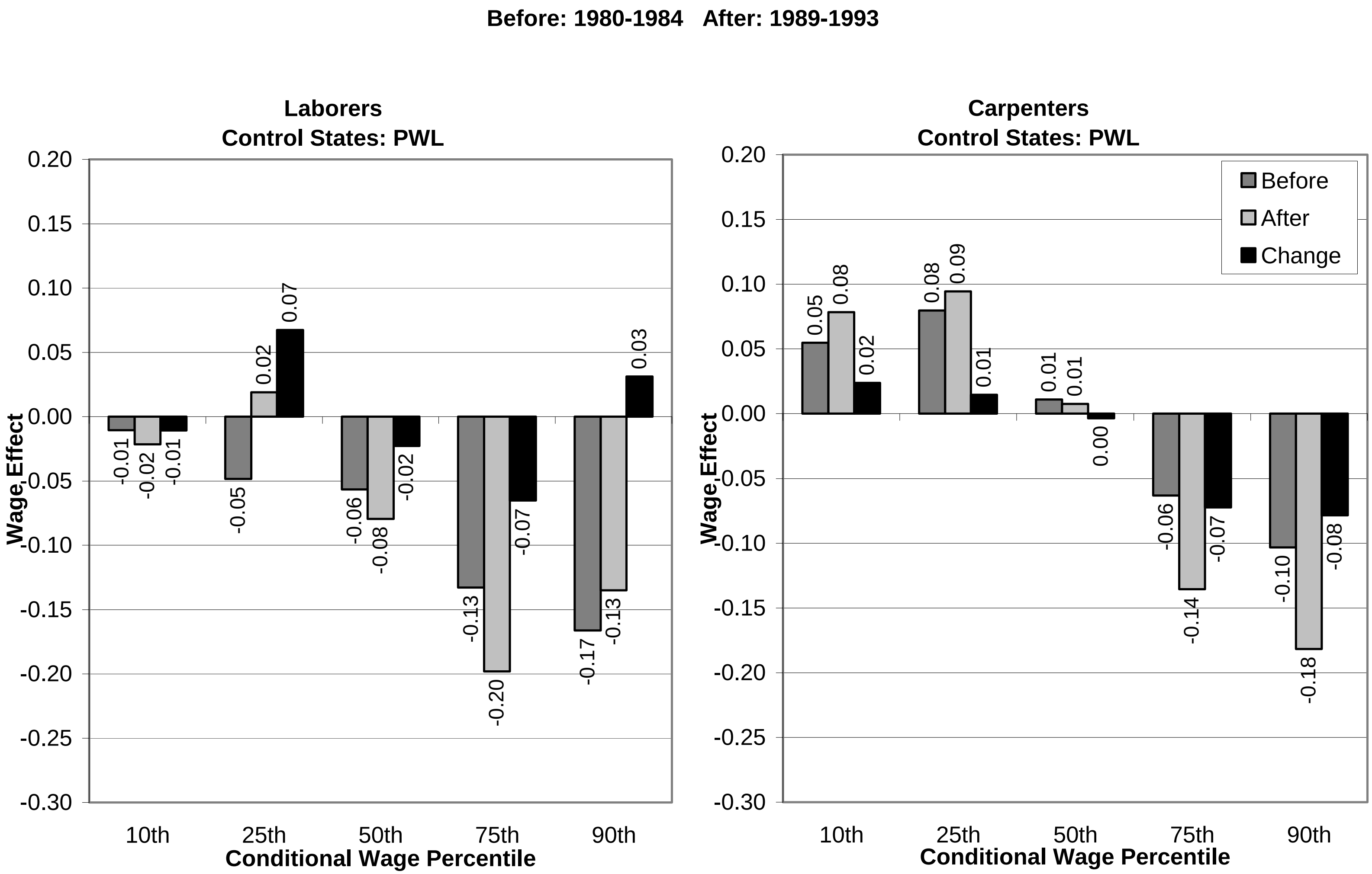

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Figure 3.4 (cont'd): Wage Effects of State Prevailing Wage Law Repeals

Before: 1980-1984 After: 1989-1993

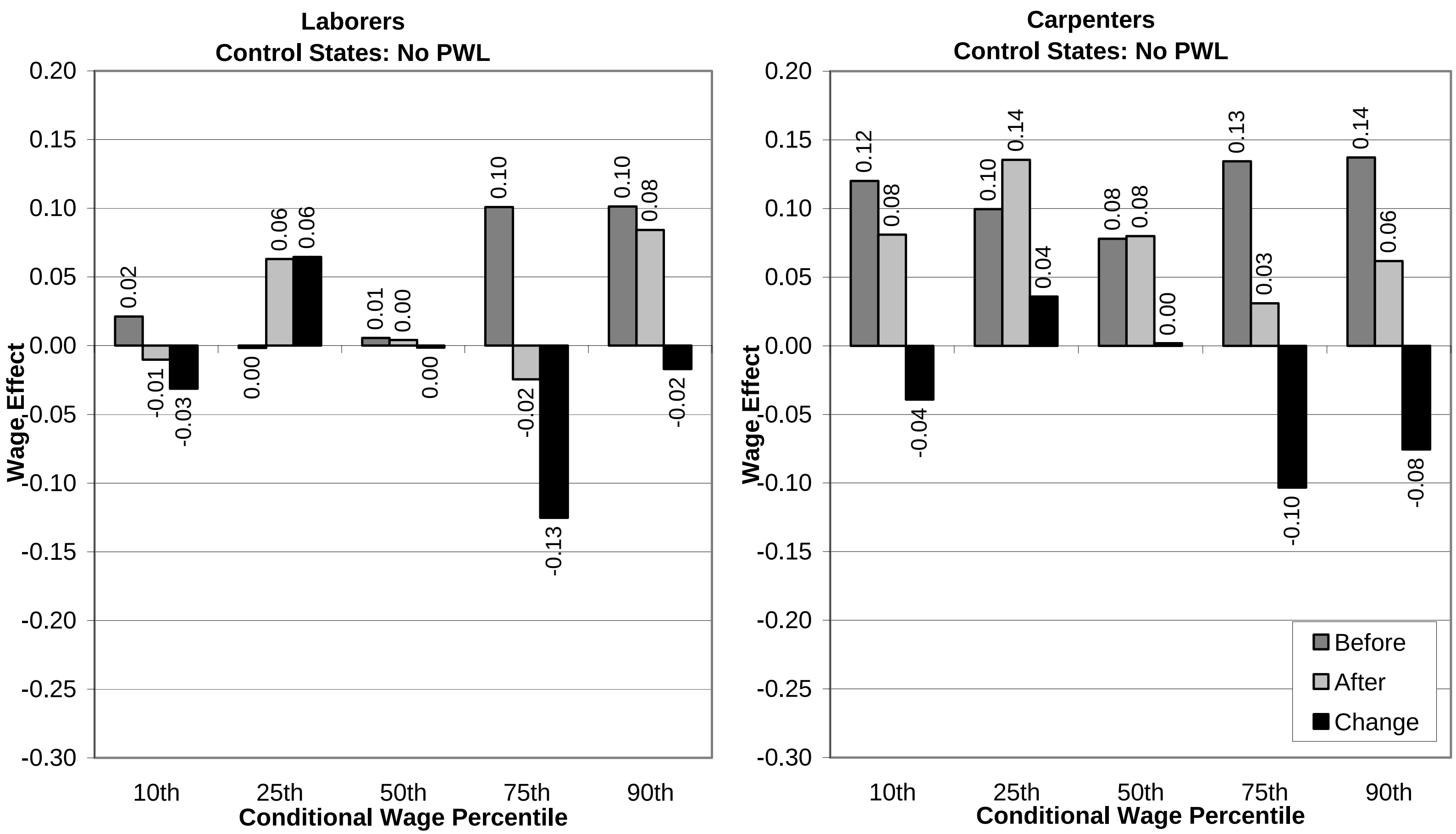


Figure 3.5: Wage Effects by Union Status and Experience Level, After Years: 1988-1989, Control States: PWL States

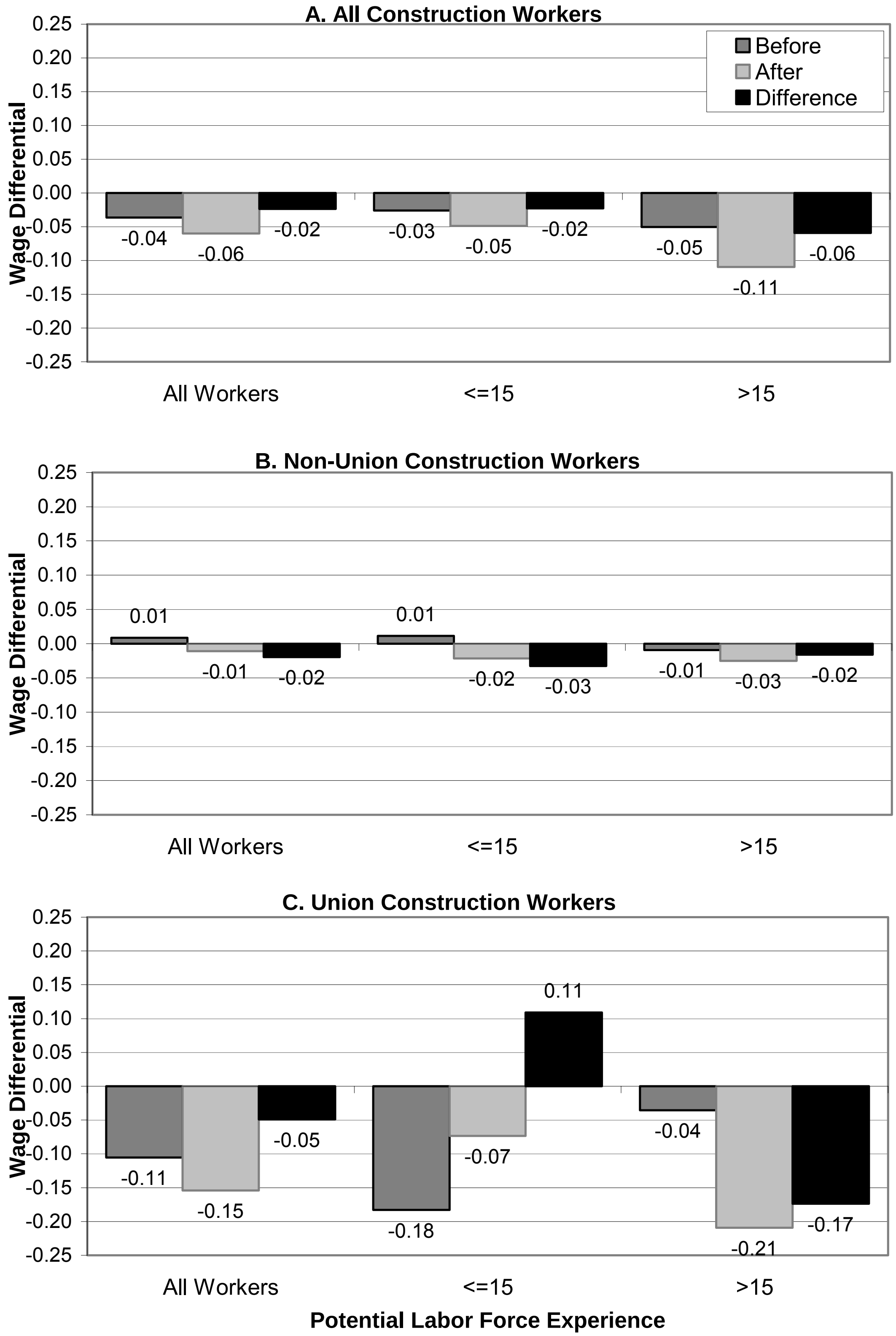


Figure 3.6: Wage Effects by Union Status and Experience Level, After Years: 1988-1989, Control States: No PWL States

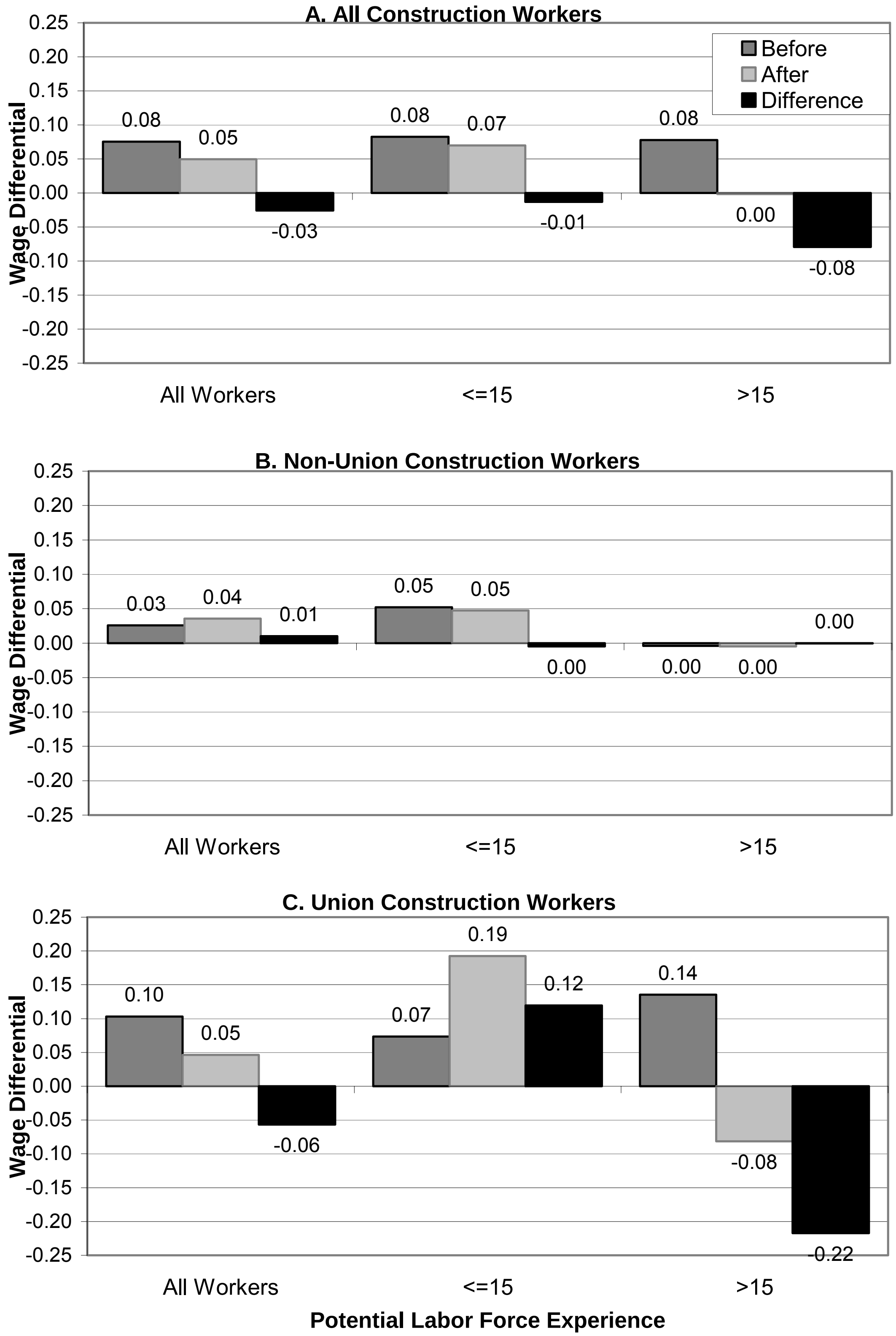


Figure 3.7: Wage Effects by Union Status and Experience Level, After Years: 1991-1992, Control States: PWL States

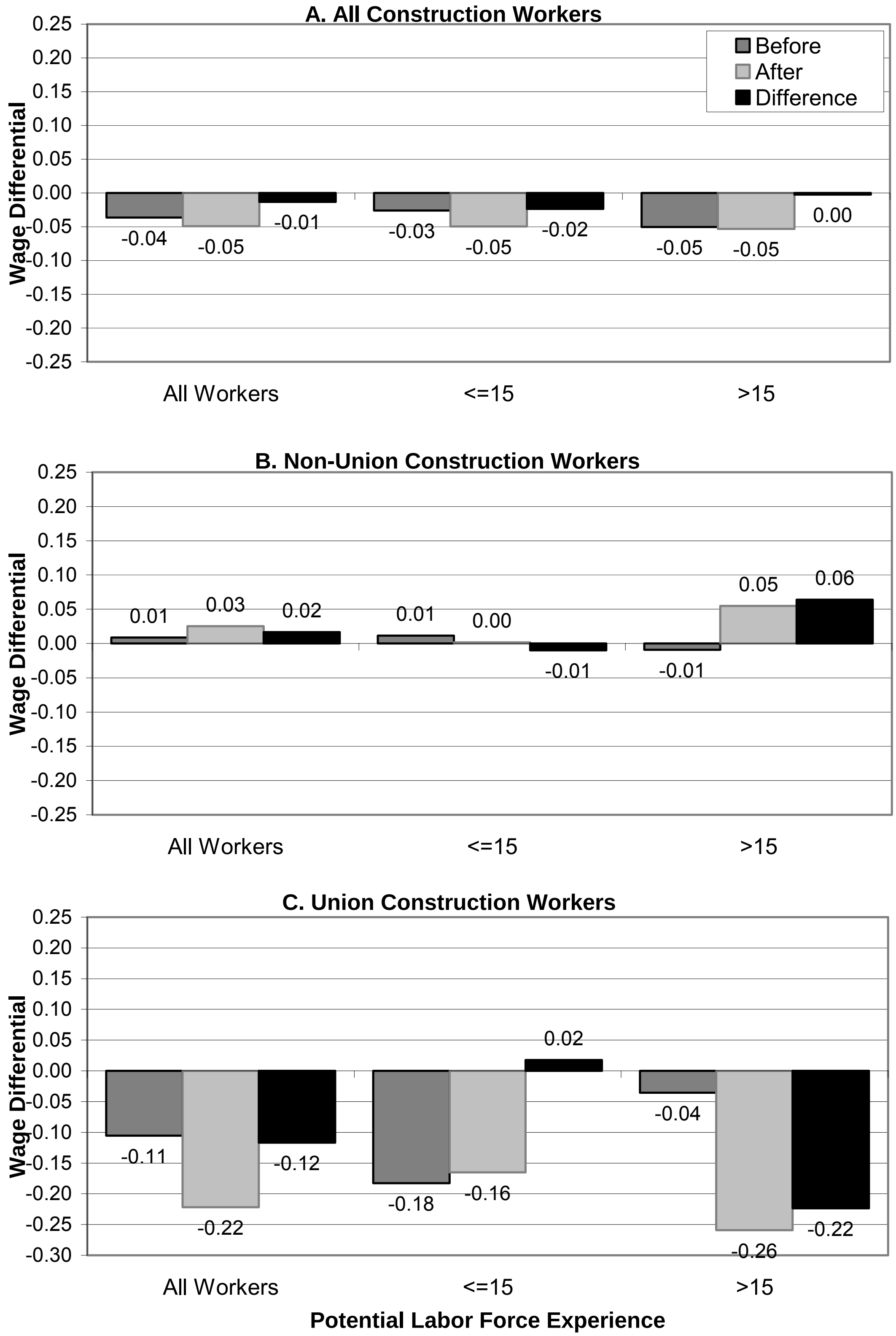
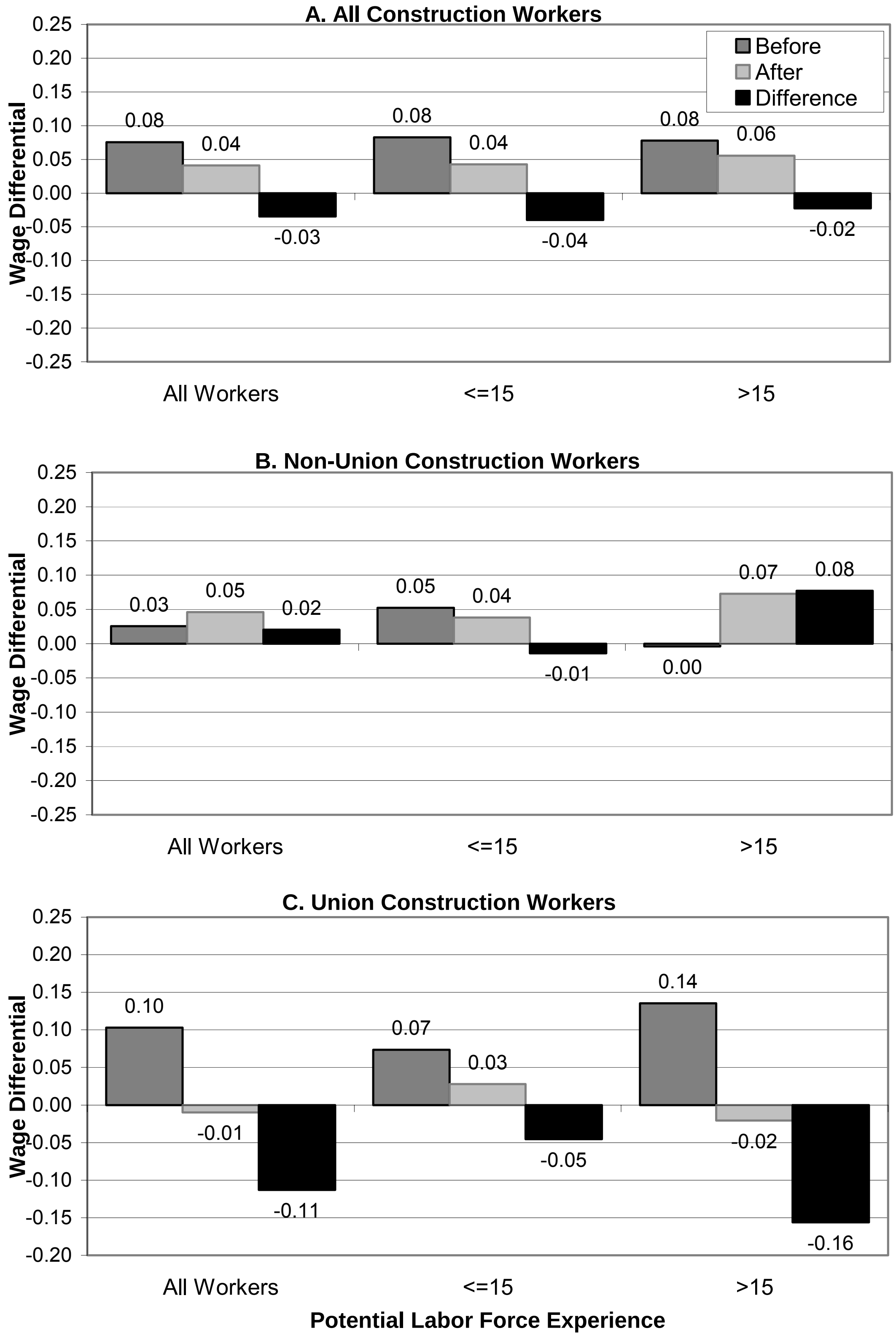


Figure 3.8: Wage Effects by Union Status and Experience Level, After Years: 1991-1992, Control States: No PWL States



APPENDIX

CALCULATIONS FOR THE RIPPLE EFFECT MULTIPLIER

To calculate the wage raises due to the federal minimum wage increase presented in table 2.15, I performed the following calculations:

1) Estimated wage elasticities are taken from table 2.11. These coefficients used in the calculation are:

For the 5th wage percentile:

$$0.19(\Delta \text{lnmin}_1) + 2.32(\Delta \text{lnmin}_1 \text{ x PROPDAW}) +$$
$$-0.02(\Delta \text{lnmin}_0) + 0.34(\Delta \text{lnmin}_0 \text{ x PROPDAW})$$

For the 10th wage percentile:

$$-0.11 (\Delta \text{lnmin}_1) + 3.19 (\Delta \text{lnmin}_1 \text{ x PROPDAW}) +$$
$$0.11 (\Delta \text{lnmin}_0) + -0.61 (\Delta \text{lnmin}_0 \text{ x PROPDAW})$$

For the 15th wage percentile:

$$-0.19 (\Delta \text{lnmin}_1) + 3.24 (\Delta \text{lnmin}_1 \text{ x PROPDAW}) +$$
$$0.00 (\Delta \text{lnmin}_0) + 0.17 (\Delta \text{lnmin}_0 \text{ x PROPDAW})$$

2) I used the following values for Δlnmin_1 and Δlnmin_0 and PROPDAW. Note that the minimum wage change is the average change in the prevailing minimum wage, rather than the change in the federal minimum wage and that the same value is used for $\ln(\text{min}_1)$ and $\ln(\text{min}_0)$ to calculate the total effect (over time) of a particular minimum wage change:

Minimum Wage Change	$\Delta\ln(\text{min})$	PROPDAW
April 1, 1990	0.102	0.098
April 1, 1991	0.102	0.107
October 1, 1996	0.106	0.099
September 1, 1997	0.073	0.106

3) Inserting the values above into the expressions in 1) produces the following percentage change in wage percentiles:

Minimum Wage Change	Percent Change in Wage Percentile
April 1, 1990	4.4%
April 1, 1991	4.7%
October 1, 1996	4.6%
September 1, 1997	3.3%

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