EMPLOYMENT EFFECTS OF PREVAILING WAGE LAWS

by

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ABSTRACT

State prevailing wage laws require that construction workers hired by private contractors on public works projects be paid minimum wages equal to corresponding market wages within a given locality. This analysis uses individual-level data from the Current Population Survey and state-level data from the Bureau of Economic Analysis and Bureau of Labor Statistics to estimate effects of prevailing wage laws on construction worker employment. The empirical strategy exploits the repeal of ten states' prevailing wage laws from 1979 – 1995, incorporating both contemporaneous and lagged effects. Findings include a five to nine percentage point probability increase in construction worker employment associated with repeal of prevailing wage laws. Extended analysis includes estimates of differential effects for groups of construction workers by skill-level and race. Findings suggest a strong positive employment effect for construction workers in general, with little evidence to suggest that sub-groups of construction workers are differentially affected by race or skill-level. Exceptions include a five percentage point decrease in probability of construction employment associated with Hispanic workers after three or more years, and an eleven percentage point increase in probably of employment associated with laborers within the first two years of repeal.

INTRODUCTION

Prevailing wages laws require minimum wages for private construction workers on public works projects. These wages are intended to reflect commensurate market construction wages within a given locality. Prevailing wage laws exist at both the state and federal level, and require that wages for public construction jobs be set according to geographic area, project type, and job classification. Ten states repealed their prevailing wage laws between the years 1979 and 1995 (listed in Table 1). The goal of this analysis is to estimate the effect of the state-level repeal of prevailing wage laws on employment in construction.

Social scientists and policy makers are often interested in how labor policy affects workers, particularly workers on the margin of being employed or of earning a "fair" wage. One argument for prevailing wage laws is that the federal and local governments may have monopsony power in the construction industry, forcing wages downwards as contractors compete for lucrative public projects. In addition, prevailing wages are intended to protect the wages of local workers from being undercut by contractors using outside labor to competitively bid on contracts. One argument against prevailing wage laws is that they raise public construction costs, negatively impacting government budgets and taxpayers. Prevailing wage laws have been receiving attention recently in state legislatures considering repeal of their state-level prevailing wage law, with Indiana repealing its law in July of 2015 and West Virginia repealing in February of 2016.

One important question is what effect do wage floors have on employment? This question has been the main focus of the minimum wage literature. Minimum wage laws

are separate policies from prevailing wages. Minimum wages are enacted with the intention of raising the welfare of low-skill workers by mandating wage minimums that employers must pay their employees. The long history of minimum wage laws, dating back to the Fair Labor Standards Act of 1938, has provided ample opportunity for economists to investigate the empirical effects of wage floors on the labor force. The bulk of the prevailing wage literature has not addressed the question of employment effects. Because employment effects of minimum wage laws are well-documented, the theoretical motivations and methodological strategies used in the minimum wage literature inform this analysis.

The conventional competitive theory of the labor market suggests that wage floors are associated with unemployment and disemployment. It is not readily apparent how employment effects of prevailing wage floors might manifest in construction. Is overall employment in the construction industry affected, relative to other industries? Are certain groups of construction workers within the industry affected more than others? The goal of this analysis is to build models to answer these questions by estimating employment effects of prevailing wage laws.

Table 1: States That Repealed Their Prevailing Wage Laws (1979-2016)

State	Year Enacted	Year Repealed	State	Year Enacted	Year Repealed
AL	1941	1980	MT	1931	
AK	1931		NE	1923	
AZ	1912	1980	NV	1937	
AR	1955		NH	1941	1985
CA	1931		NJ	1913	
CO	1933	1985	NM	1937	
CT	1935		NY	1894	
DE	1962		NC	*n/a*	
D.C.	1931		ND	*n/a*	
FL	1933	1979	OH	1931	
GA	*n/a*		OK	1909	1995
HI	1955		OR	1959	
ID	1911	1985	PA	1961	
IL	1931		RI	1935	
IN	1935	2015	SC	*n/a*	
IA	*n/a*		SD	*n/a*	
KS	1891	1987	TN	1935	
KY	1940		TX	1933	
LA	1968	1988	UT	1933	1981
ME	1933		VT	*n/a*	
MY	1945		VA	*n/a*	
MA	1914		WA	1945	
MI	1965		WV	1933	2016
MN	1973		WI	1931	
MS	*n/a*		WY	1967	
MO	1957				

Source: Department of Labor Wage and Hour Division

BACKGROUND

Prevailing wages were established at the federal level by the Davis - Bacon Act of 1931. The purpose of the Act is to set wages for workers on publicly funded construction projects that reflect the corresponding market wages for that locality, thus "[discouraging] nonlocal contractors from successfully bidding on Government projects by hiring cheap labor from outside the project area, [and] disrupting the prevailing local wage structure" (Government Accountability Office, 1979). Prevailing wages are required "to be no less than the locally prevailing wages and fringe benefits for corresponding work on similar projects in the area" as determined by the Department of Labor.[1] States have adopted “mini Davis-Bacon Acts” for projects funded at the state and local levels. Each state's Department of Labor is responsible for setting and enforcing wage rates, where "locality" is usually defined by either county or Metropolitan Statistical Area (MSA), and sometimes both.[2]

In general, the federal and state prevailing wage laws are statutorily similar. However, applying prevailing wage laws in practice is a complicated process at the state level. Because of this, accessing actual state-level prevailing wage rates is infeasible. Minimum wage laws specify singular wage rates across the whole nation or by state. In contrast, prevailing wage rates vary by state, county (or MSA), project classification, and job description. Some states publish annual prevailing wage rates, but only keep records for the most recent years. Other states require that contractors submit wage requests for

[1] Department of Labor Wage and Hour Division
[2] “Davis-Bacon Surveys”, U.S. Department of Labor Prevailing Wage Resource Book, p.1

each project, so the wages are not publicly available. [3] State laws are left to the discretion of each state, and thus vary widely in the nature of coverage and degree to which they bind. Prevailing wages can be defined customarily rather than statutorily, or they may simply reflect collective bargaining wages (Kessler and Katz, 2001).

Prevailing wages also vary by project classification. The Department of Labor breaks construction down into four main project categories: building, highway, heavy, and residential. “Building” includes construction of sheltered enclosures with walk-in access for the purpose of housing persons, machinery, equipment or supplies; all construction of such structures; the installation of utilities and of equipment, both above and below grade levels; as well as incidental grading, utilities and paving. Such structures need not be "habitable" to be building construction. Also, the installation of heavy machinery and/or equipment does not generally change the project's character as a building. State and local government funding of new buildings from 1965-1996 are estimated to be between 80-90 percent of total publicly funded buildings.[4]

“Heavy” includes those projects that are not properly classified as either "building," "highway," or "residential." Unlike these classifications, heavy construction is not a homogenous classification. Because of this catch-all nature, projects within the heavy classification may sometimes be distinguished on the basis of their particular

[3] For example, the Connecticut Department of Labor website lists monthly prevailing wage rate schedule requests by project type from the years 2011 – 2014. On January 26, 2011 a company called Newfield Construction requested prevailing wage rates for a project described as “Window Replacement and PCB Abatement at Columbus Elementary School” in the town of Bridgeport. This project is classified as “building” out of the four options “building, “heavy,” “residential,” and “service.” “Davis-Bacon Surveys”, U.S. Department of Labor Prevailing Wage Resource Book., p. 5

[4] U.S. Congress CBO Report (1983), U.S. Department of Commerce Current Construction Reports

project characteristics, and separate schedules may be issued for dredging projects, water and sewer line projects, dams, major bridges, and flood control projects.

"Highway" includes construction, alteration or repair of roads, streets, highways, runways, taxiways, alleys, trails, paths, parking areas, and other similar projects not incidental to building or heavy construction. State and local government funding of newly highway construction are estimated to be between 95-99 percent of total publicly funded highway construction from 1965-1996.[5] Finally, "residential" is construction, alteration or repair of single-family houses and apartment buildings of no more than four stories in height. This includes all incidental items such as site work, parking areas, utilities, streets, and sidewalks.

Prevailing wages also vary by job description. The federal Davis-Bacon Act (1931) is statutorily specific to "laborers and mechanics," but no further specificity is given by the law.[6] The Illinois law, for example, is worded almost identically to the federal law, applying to "laborers, workers, and mechanics."[7] Washington is broader, saying that contractors must pay their employees for any work paid for by public funds.[8] The Montana law specifies both construction, defined as heavy, highway, and building, and also non-construction services as falling under the purview of their "mini Davis-

[5] U.S. Congress CBO Report (1983), U.S. Department of Commerce Current Construction Reports
[6] This is problematic because what constitutes the correct "prevailing" wage is unclear, unless at least half of the workers of any one group earn exactly the same wage to the penny. If that is the case, that wage is taken as the prevailing wage for all workers in that classification. If no such majority rate exists, the average rate is used, weighted by the total employed in that group. Until 1983, the controversial "30 percent rule" was used when no majority existed. The prevailing rate was defined as the rate paid to the largest proportion above 30 percent of the workers in that classification and location (U.S. Congress, CBO 1982). The federal DOL uses a survey process to calculate wages that can take anywhere from 4 – 18 months to implement.
[7] Illinois Department of Labor
[8] Washington Department of Labor

Bacon."[9] States like Washington and Montana will have more complex wage data than their federal counterparts due to this variation in applicability by occupation and project type. In general, the broader the legal definition, the more complex and varied is the wage data by occupation and project type. Because the "mini Davis-Bacons" are fashioned after the federal law, they apply to construction laborers and mechanics in general.

Finally, states vary in how they treat projects jointly financed by federal, state, and local governments. Some states preempt federal prevailing wage rates and others defer to the federal rates. The federal Davis-Bacon Act statutorily applies only to construction contracts directly purchased by the federal government. This is complicated by two factors: additional federal labor laws that potentially apply to construction (Davis-Bacon is the most important law affecting prevailing wages), and the "extensive involvement of the federal government in the U.S. economy" (U.S. Congress, CBO 1982). The statutory nature of the Davis-Bacon and related Federal Acts, as well as the state-level laws, make it infeasible to categorically distinguish between projects purely or jointly financed by local versus federal governments with precision. From 1965-1996, the Department of Commerce estimates that state and local governments fund about fifteen to twenty-six percent of all construction, relative to only about two to five percent funded by the federal government (see Figure 1).

However, because states have their own prevailing wage laws in construction, "federally assisted or local construction projects may be covered by both the state statute and the Davis-Bacon Act. When these prevailing wage determinations differ, the higher

[9] Montana Department of Labor

of the two is used" (U.S. Congress, CBO 1982, p.2). State-level inquiries into the question of joint-financing provide no additional clarification in general. The Connecticut law uses language of "directness" of federal agencies contracting out projects with respect to funding, for instance.[10] One Washington state study suggests that on jointly funded projects, state laws may have only a minor effect, but in cases where the federal government is not involved the cost savings from repeal would be significant.[11] Federal government measures say that "publicly financed" construction is potentially covered by Davis-Bacon, estimated at about twenty-five percent of total construction from 1965-1996 (see Figure 2).[12] This ambiguity, combined with the Department of Commerce estimate of two to three percent, suggests that even if state laws are conservatively assumed to only cover purely local projects and the federal government has wider-reaching influence, state laws are likely to still have a significant impact in the construction market.[13]

This analysis exploits the state-level repeal of prevailing wage laws to estimate employment effects and does not address the federal law any further. Despite the complexity of the interaction between the federal Davis-Bacon Act and state prevailing wage laws, previous findings suggest that state laws bind and have effects in the economy. Those findings are summarized in the next chapter.

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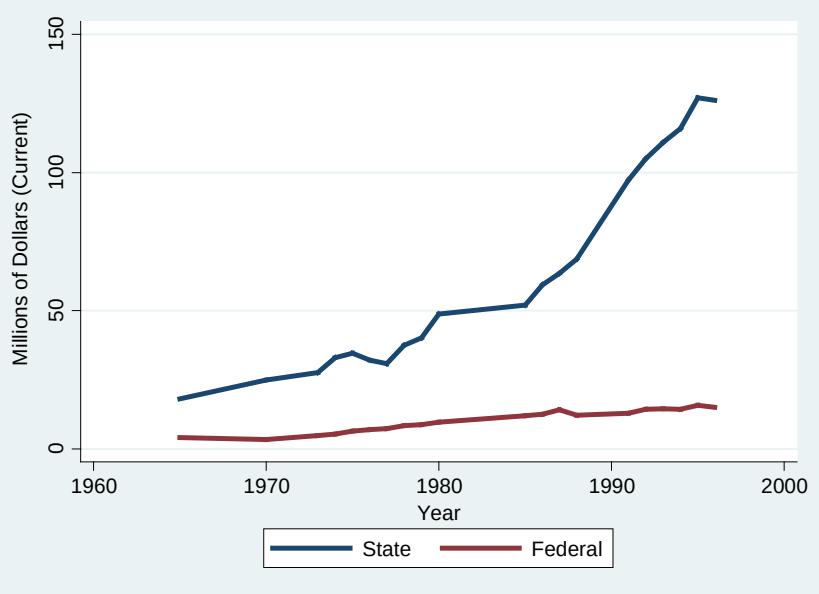
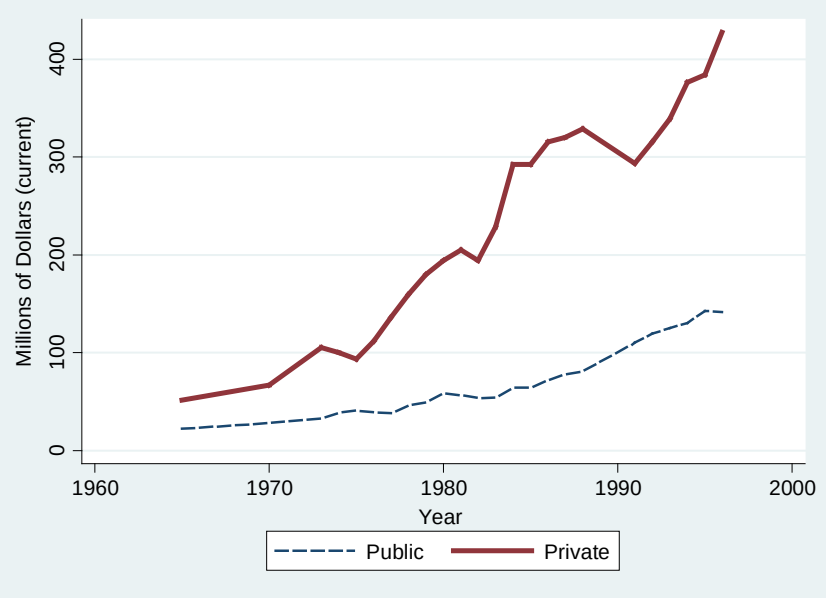
[10] Connecticut Department of Labor
[11] "Prevailing Wage Laws Mandate Excessive Costs," Washington Research Council (1999)
[12] U.S. Congress CBO Report (1983), U.S. Department of Commerce Current Construction Reports
[13] CBO figures include only estimated new construction. Maintenance, alteration, and repairs are also covered under prevailing wage laws.

Figure 1: Public Spending on Construction (1965 – 1996)



Source: U.S. Department of Commerce Current Construction Reports

Figure 2: Public and Private Spending on Construction (1965 – 1996)



Source: U.S. Department of Commerce Current Construction Reports

LITERATURE REVIEW

The prevailing wage literature focuses primarily on the prevailing wage premium and public construction costs. There is very little discussion of employment or labor force effects in general. This review briefly summarizes prevailing wage studies in combination with methodological insights from the minimum wage literature for estimating employment effects.

The literature has established a significant prevailing wage premium above corresponding market wages (Clark 2005, Fogel and Lewin 1974, Keller and Hartman 2001, Kessler and Katz 2001, Petersen 2000, Wicks-Lim 2005).[14] Inflated wages may lead to higher project costs, whereas non-inflated wages (prevailing wage rates lower than market rates) render the law non-binding. There is a debate concerning whether inflated construction labor wages actually increase public project costs.[15] Studies show conflicting evidence regarding whether prevailing wages increase costs or have no inflationary effects, depending on confounding factors such as project type and location

[14] Prevailing wages are calculated and set at the discretion of the Department of Labor of each state. See the background section for more institutional details on prevailing wage laws. Findings suggest that the methodology used by the DOL at the federal and state levels is statistically flawed and does not reflect market wages (Allen 1983, Goldfarb and Morrall 1981). A Government Accountability Office report from 1979, arguing that the Davis-Bacon Act should be repealed, cited inconsistent survey methodology resulting in inaccurate wage determinations and as one of the reasons. The report said that "when surveys were made, the data collection and compilation practices were varied and inconsistent within and among regions...Further, Labor deleted, added, and changed the wage data received without adequate reason or rationale." (GAO, 1979, iii). Such arbitrariness led the GAO to conclude that there could be no assurance that Davis-Bacon rates reflected locally prevailing market rates, and that the high occurrence of arbitrarily inflated wages led to inflated Federal construction costs. Goldfarb and Morrall (1981) utilize the GAO report in their analysis comparing various wage determination methods, and conclude that an arithmetic mean is more appropriate than using a modal rate.

[15] See papers by Azari-Rad, Duncan, Philips, and Prus from 2003-2012 showing no evidence that prevailing wages increase public construction costs. See Fraundorf, Farrell, and Mason (1984) and Thieblot (1996) for opposite evidence.

of specific projects. Kessler and Katz (2001) find significant effects of the state-level repeal of prevailing wage laws on construction worker wages. Petersen (2001) finds that the laws enhance total compensation by sixteen and a half percent, with 66.7 percent of that increase being attributed to pensions. Wicks-Lim (2005) uses quantile regression at five different points in the wage distribution to find "ripple effects" of the state law repeal onto more highly paid laborers and mechanics.

One explanation for why overall project costs may not be affected by the prevailing wage premium is that contractors may substitute higher-skilled labor for lower-skilled labor in response to wage floors. There is little to no peer-reviewed evidence of the effects of prevailing wage laws on employment in general or the skill composition of the construction workforce in particular. However, the minimum wage literature has long investigated employment effects of wage floors, and so provides insight into an analysis of prevailing wage employment effects. This paper contributes to the literature by investigating state-level effects of prevailing wage laws on employment and skill-levels. This section is an overview of prevailing wage literature, as well as highlighting a few of the most notable methodological insights from the minimum wage literature.

Prevailing wages paid to private construction workers hired for publicly funded projects tend to be significantly higher than the corresponding wages for privately funded projects.[16] Often this occurs because prevailing wage rates are determined by or commensurate with union rates.[17] O'Connell (1978) estimates the percentage change in

[16] Petersen (2000), Clark (2005), Fogel & Lewin (1974), Keller & Hartman (2001)
[17] Allen (1983), Golfarb & Morrall (1981), Gujarati (1967), Reynolds (1982)

the expected value of Davis-Bacon wages relative to market wages in 1978. He finds a weighted average of 13.3 to 32.5 percent, with wage premiums for laborers ranging from 19.04 to 35.21, using a normality assumption and a Gamma density function. Estimates for wage reduction as a result of state-level repeal range from two to sixteen percent (Kessler & Katz 2001, Petersen 2000). Kessler and Katz (2001) use Current Population Survey data to estimate the effect of the state-level repeal of prevailing wage laws on construction wages. Kessler and Katz (2001) find that the lower end estimates of overall wage reduction mask larger reductions in the union wage premium (about six to eleven percent) and the black/non-black wage differential (about four to seven percent). Thieblot (1996) uses BLS data to show that construction earnings are higher in states with prevailing wage laws despite a downward trend in wages from the 1976-1979 to 1990-1993 period (pre-repeal and post-repeal).

In addition, the GAO's review of 30 Federal or federally assisted projects found wage costs averaging 37 percent higher than comparable wage costs in twelve localities (ranging from 5 percent to 123 percent higher), inflating overall construction costs an average of three and a half percent. In absolute terms, these costs were estimated to be $191.6 million and $189.1 million for 1976 and 1977, respectively, including additional administrative and enforcement costs in the tens of millions.[18] Some findings suggest that

[18] A study by the Beacon Hill Institute at Suffolk University (Glassman, Bachman, Head, Tuerck, 2008) compares wage methodologies used by the Bureau of Labor Statistics (BLS) and the Wage and Hour Division (WHD) of the Division of Labor, finding that the WHD inflates wages by 22 percent on average relative to the BLS. The Davis-Bacon Act delegates the task of setting prevailing wage rates to the WHD, not the BLS.

often, states either reflect or even explicitly adopt union wage rates as prevailing wage rates (Gujarati 1967, Johnson 1961, Reynolds 1982, Thieblot 1997).[19]

There is far less consensus about what happens to overall project costs in the public construction market as a result of inflated prevailing wage rates. Multi-dimensional heterogeneity across project types, localities, and data availability are just a few reasons why the preponderance of analyses are localized case studies. Studies that find evidence of an increase in overall project costs are a result of the prevailing wage premium. Fraundorf, Farrell and Mason (1984) use total construction costs data from interviews on randomly selected public buildings, with cost as a function of the physical building characteristics and location. They find a 26.1 percent increase in project costs resulting from the Davis-Bacon law. Thieblot (1996) uses Utah construction data to evaluate Philips (1995) and show that both the federal and state governments could save money from repeal since the repeal lowers wages towards market level, but not below.

Studies that show no change in project costs as a result of repeal posit adjusting contractor behavior as the explanation. Azari-Rad, H., Philips, P., and Prus, M. (2002, 2003) write two papers using F.W. Dodge data on contract bid prices for new schools from the years 1991-1999. They find no statistically significant difference between the cost of public schools built under prevailing wages and those built in areas where prevailing wages do not apply. Duncan, Philips, and Prus (2006, 2009, 2012) use a school

[19] Gujarati uses a sample of 300 counties from fifty-two areas during 1960-61 and finds that counties carry out-of-county union rates about 67-98 percent of the time. Johnson (1961) found that seven states explicitly enacted laws setting collective bargaining wage rates as prevailing wage rates. Allen (1983) uses a switching regression model to estimate DOL wage determination bias in answer to the concern that DOL/market wage differentials are endogenous since the latter is partially a function of the former.

dataset from British Columbia and among their findings is the result that project cost efficiency increases over time. The implication is that contractors adjust their behavior to the presence of prevailing wages and that cost inefficiency, if it exists at all as a result of the law, is short-lived.

The bidding process is often cited as the offsetting mechanism that keeps overall project costs low in the face of prevailing wages. When faced with higher labor costs, contractors will adjust their behavior by finding other ways to keep their overall costs from rising in order to win the bid (Azari-Rad, et. al, 2012).[20] The most commonly discussed example is that contractors substitute more highly-skilled labor for lower-skilled labor when forced to pay higher wages, and that this more highly-skilled labor is better managed and more productive. The minimum wage literature discusses similar behavior, saying that the "principal response to a minimum wage is likely substitution away from lower-skilled workers toward higher-skilled, higher-wage workers" (Grant and Hamermesh, 1981).

Kessler and Katz (2001) use CPS and Census individual survey data from 1970-1993 to assess the effect of repeal of state prevailing wage laws on construction labor markets. They find that overall construction wages decline slightly though significantly after repeal, masking larger differential effects among groups. Repeal is associated with a reduced union wage premium and a narrower black/non-black wage differential.[21] The

[20] Azari-Rad (2002) also argues that purchasers of construction services, such as school districts, can plan the timing of their purchases to avoid "overheated" construction markets and keep costs low.

[21] Only a handful of peer-reviewed studies address effects of the Davis-Bacon Act and its state-level derivatives on the labor force - wages, employment, differential effects among groups, etc. Thieblot (1999) uses 1990 Census data to find an inverse correlation between black employment and strength of states' prevailing wage laws, or degree to which the law binds in a particular state. Azari-Rad and Philips (2003)

data and empirical specification used by Kessler and Katz are very similar to the approach used in this paper. They use a difference-in-difference-in-difference (DDD) strategy to estimate the difference between the change over time in the relative blue-collar construction/non-construction wage in repeal states versus non-repeal states before and after repeal. They focus on "blue-collar" construction in particular "to control for other, unobserved time-varying factors that affect all blue-collar labor markets and may be correlated with the status of labor market regulation" (263). The control group is therefore blue-collar non-construction workers in non-repeal states, and the treatment is blue-collar construction workers in repeal states. Kessler and Katz argue that because public construction accounts for as much as one-fourth of all construction, construction labor markets could be broadly affected (260).

$$ln(wage_{ist}) = \beta_1 C_{ist} * A_{st} * L_s + \beta_2 A_{st} * L_s + \beta_3 X_{ist} + V_s + U_t + V_s * C_{ist} + U_t * C_{ist} + \varepsilon_{ist} \quad (1)$$

The basic DDD specification used in their study is shown by Equation 1 where $C_{ist} = 1$ if employed in construction, $A_{st} = 1$ if repeal occurred before year t, and $L_s = 1$ if the state repealed their law. They use a vector of occupational and demographic controls X_{ist}, state and year fixed effects (V_s and U_t), as well as state and year fixed effects interacted with the construction industry. In Equation 1, β_1 is coefficient of interest. Kessler and Katz extend this specification to a DDDD model by including interaction terms for black and union workers, separately. They also use yearly CPS data to include lagged repeal policy variables in all specifications.

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use similar data to critique this result, finding a relative abundance of black workers in states that would eventually repeal their prevailing wage laws.

Because prevailing wage laws could be considered a kind of specialized wage floor, a natural question to ask is whether employment in the construction industry is affected. Studies that find that the prevailing wage premium does not affect overall public construction costs suggest that in the bidding process for government projects, private contractors will somehow adjust their mix of skill-level or labor inputs to keep project costs what they would have been in the absence of the law (Azari-Rad, et. al, 2012).

The question of employment effects is also motivated by the fact that it has already been researched extensively in the minimum wage literature over the past several decades. That body of work can provide empirical guidance as to how to proceed. An extensive review, "Minimum Wages and Employment: A Review of Evidence from the New Minimum Wage Research," by David Neumark and William Wascher (2006) is used to summarize the findings.

A sizable portion of Neumark and Wascher's review is allotted to the "new minimum wage research" of the 1990s, stimulated in part by an increasing number of states raising their minimum wage levels above the federal level during the 1980s. Minimum wage research also resurged in response to the conventional view of labor markets, summarized in a review by Brown, Gilroy, and Kohen (1982), that "time-series studies typically find that a 10 percent increase in the minimum wage reduces teenage employment by one to three percent" (p. 524). The review of the new minimum wage research focuses on four studies in particular. These include Neumark and Wascher (1992), Card (1992a), Card (1992b), and Katz and Kreuger (1992). The basic specification used among the majority of the new minimum wage studies is

$$Y_{st} = \alpha MW_{st} + R_{st}\beta + \varepsilon_{ist} \quad (2)$$

where Y_{st} is state-year observations on employment regressed on a minimum wage variable MW_s and a vector of controls $R_{st,}$ sometimes including state and time fixed effects depending on whether it is a panel data study or case study.

Neumark and Wascher (2006) find a wide-range of estimates and lack of consensus in this literature on the effects of increasing the minimum wage on low-wage employment. However, after comprehensive methodological analysis, the conclude that a majority of the most credible recent studies find negative employment effects of minimum wage laws. For studies that predict positive employment effects, which tend to be case studies based on narrow survey data, they cite serious identification and data problems. The authors also find strong evidence of the greatest disemployment effects taking place among the least-skilled groups. Relevant difficulties they discuss are discussed below.

Sample Time Period

Nuemark and Wascher emphasize that the use of state and time series variation in employment and wages over a sufficiently long sample period is important for drawing broad conclusions about employment effects. This is in contrast to using only regional variation surrounding a particular wage increase at a particular point in time, as seen in studies by Katz and Krueger (1992), Card (1992b), Spriggs and Klein (1994) and Card

and Krueger (1994). [22] This analysis addresses this issue by including variation across 42 states and 23 years, from 1977 – 2000.

Appropriate Measure of Minimum Wage Coverage and Employment Rates

One key question is who makes up the relevant treatment group and how does the data capture or fail to capture that group? Each study constructs their *MW* slightly differently, but all include changes in actual wage rates before and after laws are passed. A "fraction affected" variable (Card, 1992a), "wage-gap" variable (Spriggs and Klein, 1994), and Kaitz index (Neumark and Wascher, 1992) are the most commonly used examples for measuring minimum wage coverage.[23] This problem is simplified in the prevailing wage analysis by the use of a state-level dummy variable that equals one when a state repeals their law and equals zero if it does not.

An appropriate measure of employment is a similar concern. Employment-to-population ratios among specific age or demographic groups, particularly teens, are typically used. The measure of employment largely depends on how the "fraction affected" is specified in the *MW* variable. Construction workers on prevailing wage projects cannot be distinguished from construction workers on non-prevailing wage projects. The following prevailing wage analysis uses three different employment

[22] For example, the review critiques the “fraction affected” variable used by Card (1992a), which measures the percentage of teenagers earning an amount between the old minimum wage and the new minimum wage, just prior to the implementation of the new wage. Critics say that while this captures regional variation, it ignores some of the cross-state variation in the effective size of federal minimum wage increases (Wessels, forthcoming). Therefore it doesn’t account for the fact that an erosion in the real/effective value of the minimum wage will reduce its effect on employment. This variable might be useful for case studies, but it is less useful for longer time periods because it excludes variation in the real minimum wage resulting from inflation and other aggregate factors.

[23] See studies for explanations of how these variables are constructed.

measures to begin to address this problem: employment in the construction industry overall, employment among groups within the construction industry, and probability of employment among individual construction workers.

Lagged Effects of the Minimum Wage

The question of when policy enactments or repeals will actually take effect is an empirical one. One possibility is that effects happen quickly due to high turnover rates in low-wage industries. This could also occur if policy changes are announced months before they are actually enacted. Another possibility is that full adjustment could take time: adjustment of non-labor inputs could slow adjustment of other inputs like labor. Lagged effects could capture long-run substitution between labor and capital, and properly scale effects of the law. The authors say that the evidence must include both lagged and contemporaneous responses in order to be convincing.

This analysis addresses timing uncertainty by incorporating parametric and non-parametric lag specifications in addition to contemporaneous specifications.

Underlying Trends

Underlying trends in employment growth across states is a concern because this could lend itself to endogeneity in the question of the effect of the minimum wage on employment. Separating the employment effects of the federal and state laws is also a concern in both the minimum and prevailing wage literatures. The following analysis

includes graphs of employment trends in a discussion of potential bias in the estimated effects due to underlying trends.

The literature has established that prevailing wage laws do not achieve their stated end of reflecting local market wages, but instead tend to inflate construction wages. The bulk of ensuing empirical discussion has focused on analyzing the wage premium impact on public project costs and government expenditures. This paper aims to utilize the empirical insights and concerns from the minimum wage literature in order to address an unanswered question in the prevailing wage literature: how do prevailing wages affect employment in the construction industry?

THEORY

Prevailing wages paid to private construction workers hired for publicly funded projects tend to be significantly higher than corresponding wages on privately funded projects.[24] Often this occurs because prevailing wage rates are determined by or commensurate with union wages.[25] Because prevailing wages act as a wage floor, a natural question that arises is whether prevailing wage laws affect construction worker employment. If there are theoretical motivations for seeing an effect of minimum wage laws, can similar motivations be used for prevailing wage laws? If so, will there be clear or ambiguous empirical effects? Both the minimum wage and prevailing wage literatures are called upon to consider the theoretical motivation.

Competitive Versus Monopsony Models

There are two general labor market theories of wages: the traditional neoclassical theory and the monopsony theory. In Figure 3, the traditional competitive model assumes that labor markets are competitive in setting wages. A wage floor (W_F) results in an unemployment gap where the quantity of labor supplied (B) exceeds the quantity demanded (A). In the monopsony model in Figure 4, which assumes a single employer of labor services, wages (W_M) are determined by the optimality condition that marginal revenue product (MRP) is equal to marginal factor cost (MFC), as opposed to a competitive market equilibrium where the quantity supplied of labor equal quantity

[24] Petersen (2000), Clark (2005), Fogel & Lewin (1974), Keller & Hartman (2001)
[25] Allen (1983), Golfarb & Morrall (1981), Gujarati (1967), Reynolds (1982). See literature review section for more details.

demanded to set the competitive market wage (W_C). The monopsony model predicts that a wage floor leads to an increase in employment within a certain range, from L_{F1} to the intersection of the MRP and supply curves. The wage floor above that intersection (W_{F2}) does lead to an increase in employment, from the market quantity L_m to L_{F2}^{D}, but an unemployment gap also occurs between points L_{F2}^{S} and L_{F2}^{D}, because the quantity of labor supplied at that wage level is L_{F2}^{S}. If the wage level lies above the optimality condition at point A, disemployment and unemployment occur as in the competitive model.

One important implication of this is that the least-skilled workers will be the workers for whom the relative wage floor will more likely lie above MRP = MFC. The wage floor is likely to be effective at a high level like W_{F3}, relative to higher-skilled workers whose effective wage might be W_{F2} or W_{F1}. Wage rates for the least-skilled workers would rise, and they would be most likely to face the greatest unemployment as a result of wage floors in both the competitive and monopsony models.[26]

In their review of the minimum wage literature, Neumark and Wascher (2007) also point out that the competitive model does not necessarily predict disemployment in particular industries as a consequence of a general minimum wage increase. If disemployment does not occur in response to a minimum wage increase for a particular sample of establishments, this should not necessarily be viewed as a contradiction of the neoclassical model of the labor market. Neumark and Wascher cite Welch's (1995) explanation that employment effects in a particular sector depend on relative factor intensities. Welch gives an example of substitute goods produced by a sandwich shop and

[26] Analysis summarized from Hirshleifer, Glazer, and Hirshleifer (2005).

a fast-food restaurant, where the former might be more low-wage labor intensive relative to the fast-food restaurant. Greater demand, and thus an increase in employment, could be induced for the fast-food restaurant if a wage floor increases prices at the low-wage intensive sandwich shop.

In addition, the minimum wage may have no effect or a positive effect on employment in certain industries at certain times, depending on how quickly labor markets clear. Some labor markets are in shortage and some are in surplus at any given time. If employers do not respond immediately by adjusting wages, then a policy increase in the minimum wage level could be associated with an increase in the quantity supplied of labor, and therefore employment, if a labor market is in shortage.

There are two major ways in which prevailing wage laws differ institutionally from federal or state minimum wage laws. First, minimum wage laws clearly specify singular wage rates, either across the whole nation or by state. In contrast, prevailing wage laws specify wage rates by trade/classification, project type, and county. The second major difference between prevailing and minimum wage laws is that prevailing wage laws apply, in general, only to private construction workers on publicly funded construction projects. Whether the competitive model can make any predictions in the construction industry is unclear, because as Neumark and Wascher (2007) point out above, the competitive model does not assume predictions for particular industries or sub-sectors. Any employment effects found might be ambiguous and thus unrelated to theoretical predictions. Alternatively, the competitive model could be relevant since prevailing wage laws determine wage rates exclusively for construction workers. It is not

a general wage law applied to a narrow industry, but an industry-specific wage law applied to an industry.

Predictions

The competitive model is used as a basic theoretical framework for effects of prevailing wages for two reasons. First, because public spending only accounts for about a quarter of construction spending, there is no clear evidence to suggest that governments have monopsony power in construction labor markets. Second, the competitive and monopsony models predict similar effects for least-skilled workers, who are the focus of this analysis. What are the expected results repealing prevailing wage laws on employment? Potential outcomes for overall employment in construction, lesser-skilled subgroups, and individual industries are considered.

General Employment

The competitive labor market model predicts that the repeal of prevailing wage laws is associated with an increase in employment in the construction industry due to the removal of wage floors. A positive employment effect is expected if the data sufficiently replicates an ideal dataset showing which individual workers work on prevailing wage projects and which do not. Given imperfect survey data and confounding factors like changes in industry composition, this effect could be obscured. For instance, if there is significant displacement of employment from public to private projects as a result of prevailing wage laws, then an employment effect may not be visible in the construction industry overall. If contractors adjust the input mix of skill-level or labor inputs to keep

costs low, overall employment might not be affected, but different groups within the labor force might be.

Employment of Least-Skilled

The competitive and monopsony labor market models both predict that the repeal of prevailing wage laws is most likely to increase employment of low-skill workers relative to higher-skill workers. This occurs if employers substitute high-skill for low-skill workers when a wage floor is in place. Employers may utilize labor-capital substitution in addition to labor-labor substitution. In his comparison of prevailing wage rates and union wages, Allen (1983) indicates that labor cost increases are tempered by substitution between labor and capital, so that high "union rates are chosen only when the percent union is quite high and for some kinds of construction much higher than that stated in [Davis-Bacon]" (O'Connell, 1986). Fraundorf et. al (1984) challenges this when they find significant cost increases without much evidence of alternative input substitution.

A positive employment prediction for the least-skilled again assumes ideal data which is able to accurately measure skill-level. Two imperfect proxies are used to measure skill-level in the survey data: if the worker is a high school dropout or a laborer. If employment rates among these groups change as a result of anything other than labor-labor or labor-capital substitution under prevailing wages, the estimates will be biased. Positive employment effects as a result of repeal could be obscured.

Employment in a Particular Industry

Neither the competitive nor monopsony model of the labor market is intended to predict the effect of broad wage laws on particular industries or sub-sectors. Employment effects in particular industries depend on the local conditions of that industry, such as relative factor intensities of competing firms (Welch, 1995). A priori employment predictions for particular industries are the most ambiguous. In addition, industry-specific labor markets may clear faster or slower than competing labor markets. If a labor market is in shortage, so that the quantity demanded exceeds the quantity supplied and wages are below equilibrium, then a wage floor could be associated with an increase in employment. Therefore, repeal would be associated with a decrease in employment.

The neoclassical labor market theory of wages predicts that employment in general will increase as a result of an increase in supply or an increase in demand, where wages rise in the former and fall in the latter scenario. In the presence of a wage floor, a labor surplus will arise causing both disemployment and additional unemployment of new workers who want to work at that higher wage. In contrast, the monopsony model predicts that a wage floor could cause employment to rise within a specific range. Both models predict that a wage floor will result in unemployment among low-skilled workers, within a certain range. Low-skill workers will tend to be disproportionately affected because they tend to have the lowest wages, so a wage floor will have the most binding effect on them. High-skilled workers, or workers that already earn an above-minimum wage, should not be directly adversely affected by minimum wages because the law has no direct effect on them. However, neither model makes clear predictions about

employment in a particular industry, because employment in any given industry or sub-sector closely depends on relative factor intensities. Finally, employment in any particular industry also depends on how quickly individual labor markets clear.

Figure 3: A Competitive Model of Wages in the Labor Market

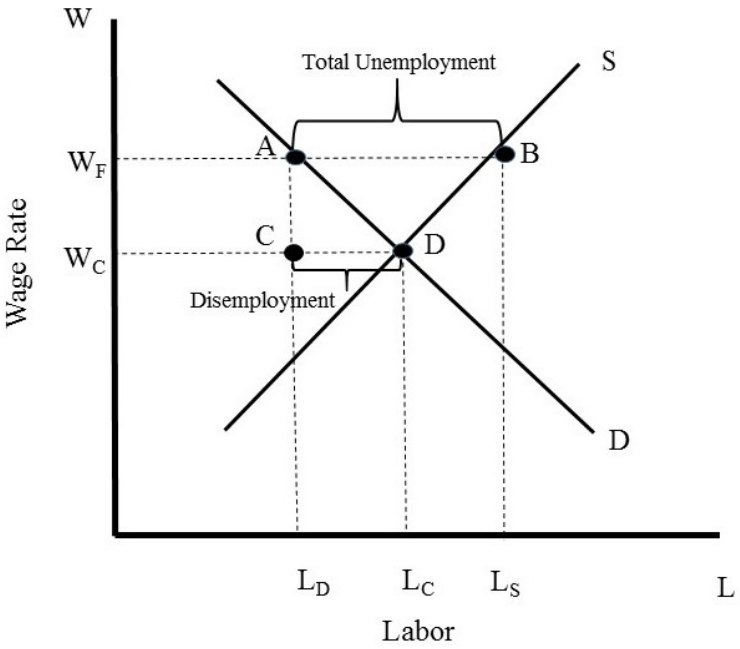
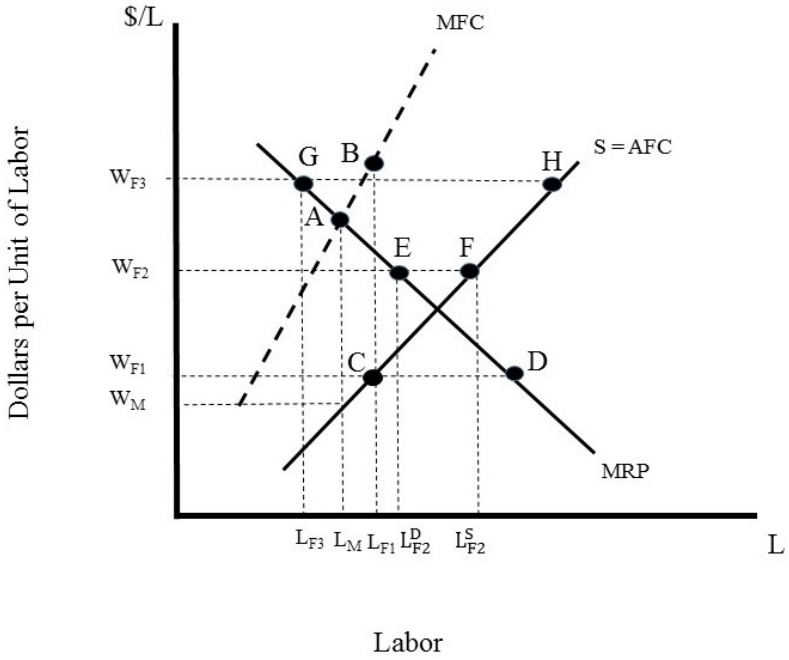


Figure 4: A Monopsony Model of Wages in the Labor Market



DATA

The first state to repeal its prevailing wage law was Florida in 1979, and the most recent were Indiana in July of 2015 and West Virginia in February of 2016 (these two policy changes are excluded from the analysis because they are still in the process of enacting their laws). States that never had a prevailing wage law are not included in the analysis. Of the 43 states that had a prevailing wage law, ten repealed. Nine states never had the law. Information on the current status of state prevailing wage laws and dates of repeal are taken from the Wage and Hour Division of the Department of Labor (DOL) website. The policy repeal variable is a dummy that equals one for the year after repeal in states that repealed their prevailing wage laws. The policy variable equals zero if it is the year before repeal or for states that never repealed.

These data are combined with annual micro-data from the Current Population Survey (CPS) for the years 1976-2000. The CPS is a monthly individual and household survey that began in the 1940s after the Great Depression. The Integrated Public Use Microdata Series (IPUMS) integrates and harmonizes the variables over time, starting in 1962, in order to make cross-time analyses possible. The CPS ASEC yearly files are used in this analysis.

The CPS data include individual-level demographic, education, and employment variables. The variables used are: year, state, age (restricted to 18 – 64), sex, race, marital status, educational attainment, employment status, labor force status, occupation, and industry. "Employed" is a binary variable equal to one for employed and equal to zero for unemployed or not in the labor force. Due to the complexity of the race/ethnicity

variable, a separate Hispanic variable is created that does not overlap with dummies for "white" and "black." A high-school dropout dummy variable is created from the overall educational status variable.

One problem with the CPS survey data is that some unemployed workers have an occupation associated with them. Not all construction workers in the data are employed. In the survey, people who are not working are asked about their last job. [27] The binary "employed" variable includes both "at work" workers and those who "have a job, but not at work last week." All others are categorized as unemployed. The documentation for the occupation variable says that the universe of respondents includes civilians age 15+ who: "were currently employed; or had previously worked and were looking for work; or were not currently in the labor force but had worked in the preceding 5 years" (IPUMS CPS). This occupation variable was chosen because it is standardizes the census-based coding scheme changes from the year 1970 to 2000. The CPS categorical method mirrors that of the U.S. Census Bureau.[28]

Table 2 displays summary statistics for all workers in all 42 states in column 1, all workers in repeal states in column 2, and all workers in non-repeal states in column 3. Means in column 1 are tested for statistical significance using a one-sample t-test. Means in columns 1 and 2 are tested for significance using a two-sample t-test to assess whether the means between repeal and non-repeal states are statistically different. Standard deviations are reported in parentheses below the means.

[27] IPUMS-CPS
[28] Ibid.

The overall employment rate in Table 2 column 1 is about 69 percent, with a slight increase in repeal states relative to non-repeal states. Males make up a slightly smaller percentage of the sample than females, at about 46 percent. Blacks are nine percent of the sample, staying fairly consistent between repeal and non-repeal states. Hispanics make up the largest minority of the sample at about thirteen percent, which slightly increases to fourteen percent in repeal states. These minority groups are of empirical interest since, together, they make up well over twenty percent of both the blue-collar and laborer groups. High-school dropouts make up 37 percent of the group of dropouts and high-school graduates, staying steady across repeal and non-repeal states. Laborers are small percentage of the sample, at about three percent.

Figure 5 graphs the percentage of states that repeal their prevailing wage law, as a percentage of all states, over time. There is a clear upward trend in the percentage of states repealing their laws beginning in the base year of 1979. Figure 6 graphs the mean employment rate in repeal and non-repeal states. Repeal states are divided into three groups corresponding to early, middle, and late repeal between the years 1979 – 1995. Figure 7 graphs the mean of the % Construction dependent variable (p.33) in Equation 2 in the following chapter. There is no clear trend in the % Construction variable. If construction employment were increasing over time concurrently with the policy repeal, this might lead to positively biased estimates of the law's repeal.

A separate, state-level data set is constructed using CPS data. This state-level data set is then merged onto the main CPS micro data set. Several state-level dependent variables are constructed from the CPS data including an employment rate measure for

construction overall, *% Construction,* and an employment rate for each subgroup in the construction industry, *% Group*. The subgroups include: high-school dropouts, Hispanics, blacks, and laborers.

1. "*% Construction*" = the ratio of the total employed in construction to the total employed in all industries.
2. "*% Group*" = the ratio of all workers in the group employed in construction to the total employed in construction.

Independent variables include annual state-level unemployment rates and labor force numbers are taken from the BLS. Annual state-level personal income per capita and state population data is taken from the BEA. Additional state-level controls are generated using the CPS data, including age and racial composition. Due to the questions of interest, weighting these variables is likely not a necessary concern if the repeal of the law is not expected to be correlated with differences in the demographics these variables represent.

Because more occupations are linked to the construction industry than are of interest in this analysis, one question is which occupations should be included. For instance, even though there are nineteen "construction trades" explicitly specified in the occupational coding, 268 different occupations are actually linked to the construction industry when all other industries are dropped from the dataset.[29] These include potentially miscellaneous or inappropriate occupations such as dental hygienists or

[29] Some of the construction trades are not in the construction industry. Laborer is defined by occupation, so that it can be used to compare construction laborers to non-construction laborers, where the difference is picked up by the construction industry dummy variable.

various kinds of scientists and engineers. Since this research is concerned with identifying the policy effect on construction workers, particularly construction laborers, the analysis is done in three parts.

First, effects are estimated without dropping any occupations. This referred to as "all" workers within the construction industry throughout the analysis. Second, the sample is restricted to "blue-collar" occupations in all specifications, because these are the construction workers most likely to be affected by prevailing wage coverage and repeal.[30] This sample is referred to as "blue-collar" workers within construction throughout the analysis. The non-blue collar occupations dropped are listed in Appendix A. Third, a separate dummy variable for construction laborers is created to include the fourteen occupations that describe labor-related tasks. Two of these occupations are from the "construction trades" occupational categories, chosen because they had the two lowest wage levels out of the fourteen. The rest are taken from the blue-collar occupations linked to the construction industry. The fourteen "laborer" occupations chosen for the construction laborer dummy variable are listed in Appendix A.

One of the ways in which this CPS survey dataset deviates from an ideal dataset is it is unclear exactly which workers are covered under the prevailing wage law and thus affected by repeal. The treatment of interest, the effect of prevailing wage laws and their repeal, is not randomly assigned. Ideally, the data would specify exactly who worked on

[30] In an analysis of public versus private sector wages, Fogel and Lewin (1974) find that public sector wages tend to have a more equalitarian distribution: public wages are comparatively higher than private wages for blue-collar jobs than they are for managerial positions. Though not necessarily directly linked to state prevailing wage laws, this insight potentially carries greater relevance for prevailing wages because they mainly apply to the construction industry, which is predominantly blue-collar.

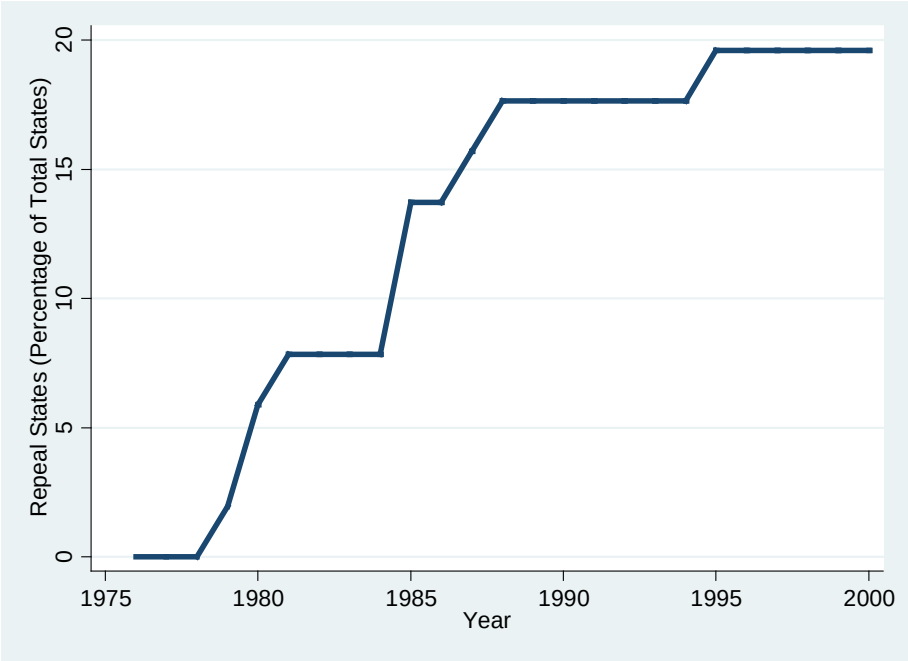
construction projects covered by prevailing wage laws and who did not. The sampling of all workers, blue-collar workers, and laborers is done partially as a robustness check against the categorical ambiguity and uncertainty in the data, and partially as an attempt to isolate the effect on the workers most expected to be affected by prevailing wages and wage repeal: blue-collar construction laborers and mechanics. In addition, using individual-level CPS data helps mitigate the non-random treatment effect by providing increased variation in employment outcomes.

Table 2: Summary Statistics for All Workers

	All States	Repeal States	Non-Repeal States
Repeal	0.12***	--	--
	(0.32)	--	--
Employed	0.69***	0.71***	0.69***
	(0.46)	(0.46)	(0.46)
Labor Force	0.74***	0.75***	0.74***
	(0.44)	(0.43)	(0.44)
Age	37.82***	38.17***	37.77***
	(13.00)	(12.96)	(13.01)
Male	0.46***	0.46**	0.46***
	(0.50)	(0.50)	(0.50)
Female	0.54***	0.540**	0.54***
	(0.50)	(0.50)	(0.50)
White	0.75***	0.74***	0.75***
	(0.44)	(0.44)	(0.43)
Black	0.09***	0.10***	0.084***
	(0.28)	(0.29)	(0.28)
Hispanic	0.13***	0.15***	0.13***
	(0.34)	(0.35)	(0.34)
Married	0.39***	0.38***	0.39***
	(0.49)	(0.49)	(0.49)
High School Dropout	0.37***	0.36***	0.37***
	(0.48)	(0.48)	(0.48)
High School Graduate	0.63***	0.65***	0.63***
	(0.48)	(0.48)	(0.48)
Laborer	0.03***	0.02***	0.03***
	(0.16)	(0.15)	(0.16)
N	1787432	206255	1581177

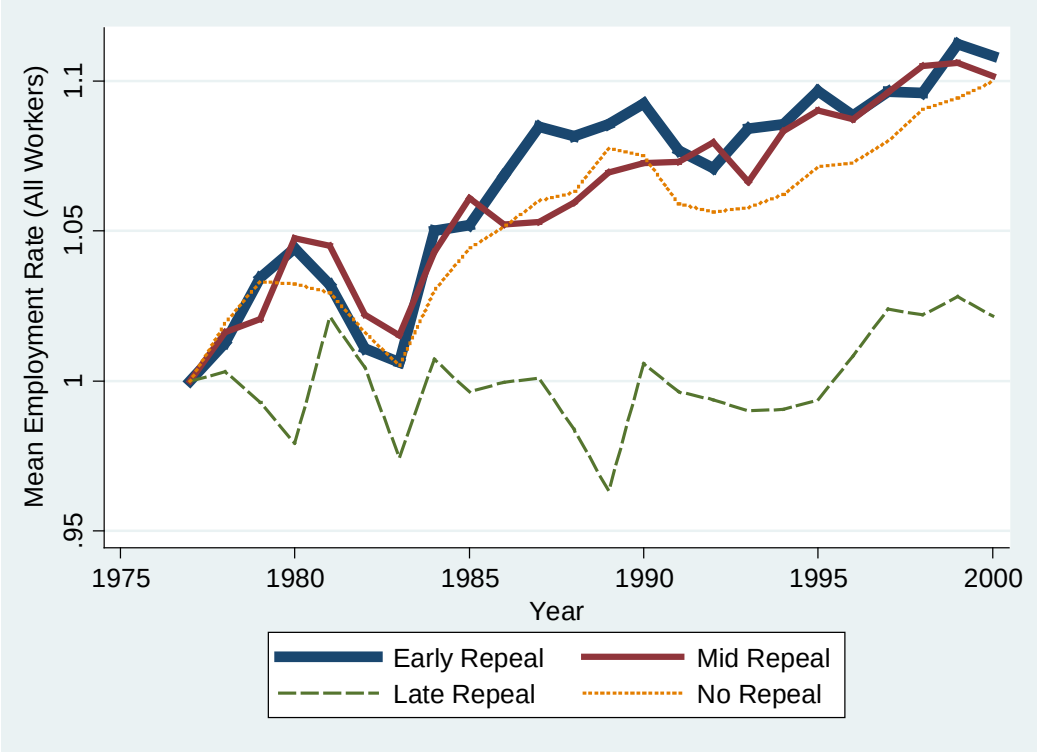
Source: IPUMS Current Population Survey. Standard deviations reported in parentheses. The first column for all states is a one-sample t-test for statistical significance. The 2nd and 3rd columns are a two sample t-test that the means in repeal versus non-repeal states are significantly different. ***p<0.01 **p<0.05 *p<0.1

Figure 5: Percentage of Repeal States Over Time (1976 – 2000)



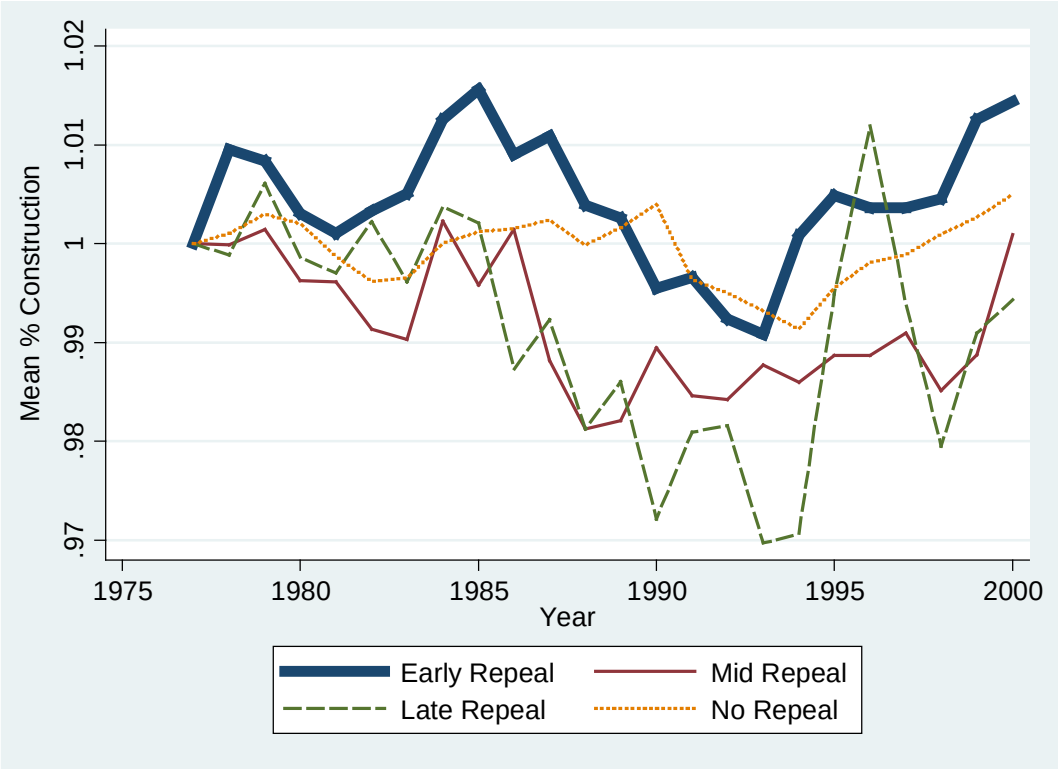
Source: U.S. Department of Labor

Figure 6: Employment Rates in Repeal and Non-Repeal States



Source: CPS Early Repeal (1979-1981): FL, AL, AZ, UT Mid-Repeal (1985-1988): CO, NH, ID, KS, LA
Late Repeal: UT (1995)

Figure 7: Construction Employment Rates in Repeal and Non-Repeal States



Source: CPS Early Repeal (1979-1981): FL, AL, AZ, UT Mid-Repeal (1985-1988): CO, NH, ID, KS, LA
Late Repeal: UT (1995)

EMPIRICAL SPECIFICATION

Recall that the goal of this analysis to estimate the effect of the repeal of state prevailing wage laws on employment in the construction industry. Because prevailing wages are wage floors, it is expected that repeal is associated with positive employment effects. However, it is unclear how these positive effects might manifest due to the complexity of prevailing wages and their statutory specificity to the construction industry. Perhaps the construction industry overall is affected, perhaps groups with the construction industry are affected, or perhaps it is only possible to observes changes in employment at the individual level.

Three models are used to estimate the effect of prevailing wage law repeal on employment through each of these possible pathways. The first model estimates the state-level percentage of all workers employed in the construction industry. The second model estimates state-level employment rates of high-school dropouts, laborers, Hispanic, and black workers relative to all workers within the construction industry. Finally, the third model estimates the probability of being employed in construction as an individual worker.

Model 1 is motivated by the question of whether repeal of prevailing wage laws impacts the size of the construction industry overall, as measured by employed construction workers. The competitive theory of the labor market predicts a positive effect of repeal in construction employment overall. If construction employment decreases as a result of the law being in place, then it is expected that employment will increase after repeal, due to the removal of a wage floor. In this case, non-construction

blue-collar jobs could serve as a substitute for construction blue-collar jobs before repeal. Equation 3 regresses the state-level percentage of construction employment, *%Construction*, on the policy repeal variable $Repeal_{st}$ and a vector of controls X_{st}.

$$(\%\ Construction)_{st} = \beta_0 + \delta_1 Repeal_{st} + \beta_1 X_{st} + u_{st} \quad (3)$$

"*% Construction*" is the ratio of the total employed in construction to total employed in all industries, by year and state, as in Equation 4.

$$(\%\ Construction)_{st} = \frac{Total\ employed\ in\ construction}{Total\ employed\ in\ all\ industries} \quad (4)$$

The policy variable $Repeal_{st}$ equals one for repeal states in the first year after repeal and equals zero for repeal states before they repealed or for states that kept their law. The vector of state-level controls, X_{st}, includes a minimum wage dummy variable, the state-wide employment rate, and income per capita. It is important to control for these labor market variables because they are likely correlated with both repeal of prevailing wage laws and construction employment.

One issue in estimating a causal effect of prevailing wages on employment is that there are many remaining omitted variables in the error term u_{st}. Relevant omitted variables are any that are correlated both with the repeal of prevailing wage laws and employment of construction workers. These could include time-varying micro and macroeconomic factors such as work force composition, skill, occupation and anything affecting blue-collar labor markets overall. They could also include unobserved fixed differences between states such as culture or political climate. Some fixed differences, such as differing laws and regulations, are observable but difficult to measure. Equation 3 addresses these concerns by including a state and year fixed effects specification where

$u_{st} = v_s + z_t + \varepsilon_{st}$. State fixed effects are estimated in the vector v_s and year fixed effects are estimated by z_t. For a difference-in-difference approach to be valid, there must not be significantly different trends between the repeal (treatment) and non-repeal (control) states over time. To control for this possibility, state-specific time trends are used throughout the entire analysis.

Another problem with estimating the effect of prevailing wage repeal is that it is unclear when the law will take effect in the labor force. An important critique in the minimum wage literature is that any study of labor market effects of wage laws should include both contemporaneous and lagged effects of the policy change (Neumark and Wascher, 2006). Including both contemporaneous and lagged effects provides a more complete picture of employment effects, particularly if markets respond in the long-run but not the short-run. A slightly modified version of Equation 3, Equation 5, includes lagged effects where the $Repeal_{st}$ policy repeal variable is split into two separate indicators. The first turns on one to two years after repeal and the second turns on three or more years after repeal.

$$(\%\ Construction)_{st} = \beta_0 + \delta_1(1-2\ Years\ After)*Repeal_{st} + \delta_2(3+\ Years\ After)*Repeal_{st} + \beta_1 X_{st} + u_{st} \quad (5)$$

Finally, a remaining problem is possible endogeneity in repealing prevailing wage laws. States could be choosing to repeal their laws in response to factors correlated with employment rates. In their 2001 paper investigating the state-level repeal of prevailing wage laws on construction wages, Kessler and Katz point out that if states repeal the law in response to weak blue-collar labor markets, then resulting estimates could be biased

(262). On the other hand, if states that repeal their prevailing wage laws are already inclined toward higher employment levels for whatever reason, then this could also lead to overestimation of employment effects. This analysis incorporates repeal dates from ten different states staggered across a sixteen-year period to mitigate this bias, but the endogeneity problem remains if all repeal states are choosing to repeal their laws in response to the same blue-collar labor market factors.

Model 2 addresses the question of employment effects within the construction industry. The purpose of Model 2 is to estimate the internal allocation of employment changes among groups in construction, in addition to the overall construction employment rate in Model 1. If no visible effect is observed in Model 1, it could be masking differential effects among groups in Model 2.

The groups included are workers who are high-school dropouts, Hispanic, black, or laborers. High-school dropouts and laborers are interesting groups, because as some of the least-skilled construction workers, they are most likely to face unemployment as a result of a wage floor. High-school dropouts, at relatively low level of education, are used as a proxy for a low level of skill. Another approach is to look at the effect on construction laborers. Studies in both the minimum wage and prevailing wage literatures suggest that employers might react to wage floors by substituting away from lower-skilled workers to higher-skilled.[31]

Hispanics are an interesting minority group because they make up about thirteen percent of the construction workforce sample, about twice as much as blacks. To the

[31] See Grant and Hamermesh (1981), Metzger and Goldfarb (1983), Keyes (1982), etc. in Literature Review.

extent that being in these groups is correlated with employment-related variables such as skill-level or degree of unionization, they might reveal differential employment effects of repeal. Because they represent large proportions of construction workers, these groups are primarily used in response to empirical uncertainty about where internal employment effects might manifest.

To answer the question of employment within the construction industry, Equation 6 regresses the state-level percentage of construction workers who are high-school dropouts, Hispanic, black, or laborers on the policy repeal variable $Repeal_{st}$ and a vector of controls X_{st}.

$$(\%\ Group)_{st} = \beta_0 + \delta_1 Repeal_{st} + \beta_1 X_{st} + u_{st} \quad (6)$$

% Group is a generic variable representing each of these four sub-groups, which are estimated separately. These dependent variables are ratios between the total employed in each of the groups and the total employed in construction.

$$(\%\ Group)_{st} = \frac{Total\ in\ group\ \ employed\ in\ construction}{Total\ employed\ in\ construction} \quad (7)$$

The same concerns about omitted variable bias in Model 1 are also relevant in Model 2. Accordingly, the same vector of state-level control variables X_{st} are again included to control for time-varying work force characteristics. State and year fixed effects are again included in u_{st} to control for unobserved fixed differences between repeal and non-repeal states over time. Finally, the timing of the policy effect is again a concern. As in Model 1, lagged effects are estimated for each of the groups. Equation 8 takes account of lagged effects, similarly to Equation 5.

$$(\%\ Group)_{st} = \beta_{0} + \delta_1(1 - 2\ Years\ After)*Repeal_{st} + \delta_2(3+\ Years\ After)*Repeal_{st} + \beta_1 X_{st} + u_{st} \quad (8)$$

Finally, Model 3 addresses the question of whether an individual construction worker is likely to be employed in construction or not as a result of prevailing wage repeal. It is expected that individuals in these groups will be less likely to be employed in the construction industry. Instead of estimating changes in state-level employment rates of groups, Model 3 allows estimation of differential effects for individuals workers. This gives a sense of the likelihood of being employed in construction instead of aggregate group employment rates. An individual-level equation includes more variation than the state-level equations, allowing for greater statistical power and potentially smaller standard errors.

To answer the question of probability of employment, Equations 9 – 11 build a difference-in-difference-in-difference-in-difference specification in three cumulative steps for ease of interpretation. First, starting with a difference-in-difference (DD) regression, Equation 9 regresses the binary $(Employed)_{ist}$ variable on the $Repeal_{st}$ policy variable and a vector of controls.

$$(Employed)_{ist} = \beta_{0} + \beta_1 Repeal_{st} + \beta_2 X_{ist} + u_{st} \quad (9)$$

The coefficient β_1 measures the difference-in-difference effect of the average change in the probability of being employed in repeal states before and after repeal relative to non-repeal states. The control vector includes a minimum wage law indicator, sex, race, marital status, age and age squared, which are important time-varying

individual worker characteristics to control for. Due to the same omitted variable bias concerns stated in Models 1 and 2, state and year fixed effects are again included.

Second, Equation 10 builds on Equation 9 with a difference-in-difference-in-difference (DDD) approach. Equation 10 is motivated by the question of whether repeal of prevailing wage laws impacts employment of construction workers in general, similarly to Model 1. By comparing the differences in employment over time between treatment and control states, the DDD model is exploiting "within" variation in the treatment and control states (how much the data in each state deviate from the average in that state before and after repeal) then comparing those average differences between the two groups.

Equation 10 regresses the binary $(Employed)_{ist}$ variable on the $Repeal_{st}$ policy variable, a binary indicator for the construction industry $Const_{ist}$, an interaction term, and a vector of controls in a difference-in-difference-in-difference (DDD) specification.

$$(Employed)_{ist} = \beta_0 + \beta_1 Repeal_{st} + \beta_2 Const_{ist} + \beta_3 Repeal_{st} * Const_{ist} + \\ + \beta_4 X_{ist} + u_{st} \quad (10)$$

The coefficient of interest β_3 measures the effect for construction workers in repeal states and can be interpreted two ways. The first interpretation is the estimated effect for construction workers in repeal states relative to construction workers in non-repeal states. The second interpretation is the estimated effect for construction workers relative to non-construction workers within a repeal state. The final coefficient on the $Repeal_{st}$ policy variable of interest is the average change from ten repeal dates over a

twenty-one year period. This means the differences between states, or the "between" variation, has been removed.

Finally, Equation 11 is a difference-in-difference-in-difference-in-difference (DDDD) individual-level equation estimating the likelihood that high-school dropouts, Hispanics, blacks and laborers are employed in the construction industry after repeal. The DDDD coefficient, β_7 in Equation 11, extends the analysis to estimate a fourth differential effect for construction workers in each of the sub-groups in repeal states after repeal.

Is there a noticeable effect for construction workers who are dropouts or not, or a laborer, Hispanic, or black? The goal of estimating effects for construction workers who are high-school dropouts or laborers is to address the question of labor-labor or labor-capital substitution by employers in response to higher labor costs. If they adjust their mix of labor by skill-level, the least-skilled workers are more likely to be displaced by higher-skilled workers or capital. The goal of estimating effects for Hispanic and black construction workers is to search for evidence of shifting workforce composition in response to prevailing wage repeal. Hispanics and blacks are the largest minorities in construction, at 13 percent and 6 percent of the sample respectively.

In Equation 11, the individual-level "employed" dummy variable is regressed on the repeal variable, a set of interactions, and a vector of individual-level demographic controls. High-school dropouts, Hispanics, blacks, and laborers are interacted with the repeal variable. Though the dependent employment variables in question involve varying kinds of individual construction workers, the treatment effect is at the state-level. The state-level averages in employment across various groups over time provide the variation

for observing the treatment effect. As before, "*Group*" in Equation 10 corresponds to each of these four sub-groups in construction.

$$(Employed)_{ist} = \beta_0 + \beta_1 Repeal_{st} + \beta_2 Const_{ist} + \beta_3 Groupt_{ist} + \beta_4 Repeal_{st}*Const_{ist} + \beta_5 Repeal_{st}*Group_{ist} + \beta_6 Const_{st}*Group_{ist} + \beta_7 Repeal_{st}*Const_{ist}*Group_{ist} + \beta_8 X_{ist} + u_{st} \quad (11)$$

The coefficients of interest in Equation 11 are β_4 and β_7. Including β_4 creates the third difference as in Equation 10 whereby it measures the average effect for construction workers in repeal states. The final interaction for β_7 measures the average effect for the groups, for example high-school dropouts, in construction in repeal states after repeal (and similarly for Hispanics, blacks, and laborers).

Finally, lagged effects are once again estimated for Model 3, using slightly modified versions of Equation 9-11, where "*Group*" indicates each of the sub-groups.

$$(Employed)_{ist} = \beta_0 + \beta_1(1-2\ Years\ After)*Repeal_{st} + \beta_2(3+\ Years\ After)*Repeal_{st} + \beta_3 Const_{ist} + \beta_4 Group_{ist} + \beta_5(1-2\ Years\ After)*Repeal_{st}*Const_{ist} + \beta_6(3+\ Years\ After)*Repeal_{st}*Const_{ist} + \beta_7(1-2\ Years\ After)**Repeal_{st}*Group_{ist} + \beta_8(3+\ Years\ After)**Repeal_{st}*Group_{ist} + \beta_9\ Const_{ist}*Group_{ist} + \beta_{10}(1-2\ Years\ After)**Repeal_{st}*Const_{ist}*Group_{ist} + \beta_{11}(3+\ Years\ After)**Repeal_{st}*Const_{ist}*Group_{ist} + \beta_{12} X_{ist} + u_{st} \quad (12)$$

RESULTS

What is the effect of the repeal of state prevailing wage laws on employment in construction? This section presents results from the estimating equations in Models 1, 2, and 3. There is strong contemporaneous and lagged evidence for an increase in the probability of being employed in construction as a result of prevailing wage repeal for construction workers in general, but little evidence to suggest differential effects among groups of construction workers by race or skill-level.

Table 3 reports results for Model 1, which estimates the effect of prevailing wage law repeal on the proportion employed over time in the construction industry relative to all other industries. Columns 1 and 3 of Table 3 report the coefficient on the policy repeal variable, $\delta_{1,}$ in Equation 3, sampling all workers and blue-collar workers respectively. Columns 1 and 3 report that the employment rate in the construction industry (*% Construction)* increases 0.31 and 0.36 percentage points in states that repealed their laws compared to those that kept them, for all workers and only blue-collar workers respectively. However, the effects are insignificantly different from zero, and small in magnitude relative to a standard deviation of 2 percentage points for the dependent variable *% Construction*.

Model 1 also controls for the presence of a minimum wage law, the state-level employment rate (also scaled from 0 to 100, and state-level income per capita (in thousands of dollars.) The coefficients on the state-level employment rate are highly significant at the one percent level, indicating that a one percentage point increase in the overall employment rate is associated with an increase of about thirteen to eighteen

percentage points in construction employment. It is important to account for both contemporaneous and lagged effects of policy changes. Columns 2 and 4 of Table 3 estimate coefficients δ_1 and δ_2 of Equation 5. When the policy repeal is lagged, the results are again statistically insignificant from zero. These findings suggest that lagged adjustment does not change the null contemporaneous findings in Columns 1 and 3.

Tables 4 and 5 report estimates for Model 2, using Equations 6 and 8 respectively. Model 2 is used to estimate the effect of prevailing wage repeal on groups of workers within the construction industry. Table 4 reports estimates from the state-level employment effects among the four subgroups relative to blue-collar construction workers. The construction of the dependent variables in these two tables are shown in Equation 7. Table 5 re-estimates the same employment effects using a lagged version of the model in Equation 8.

Tables 4 and 5 show few significant results of repeal for these different groups, measured at the state-level as a proportion of total employed construction workers. In Table 4, the first row estimates the coefficients on repeal, δ_1, in Equation 6 sampling only blue-collar construction workers. There is a 2.30 percentage point decrease in employment of black construction workers associated with repeal, significant at the five percent level. These results are mirrored in Table 5, which examines a lagged effect of prevailing wage repeal on the four groups as in Equation 8, again sampling all only blue-collar construction workers. The lagged results are again statistically indistinguishable from zero with the exception of a negative employment effect of black construction

workers of 1.98 percentage points within the first two years. This effect disappears after three years.

The state-level effects in Models 1 and 2 are generally not statistically different from zero, with the exception of a negative employment effect for black workers which disappears after three years. In contrast to the state-level equations above, Model 3 uses an individual-level approach to estimate the probability of construction workers being employed as a result of repeal. Estimating the probability of being employed at the individual level allows for more statistical power and a potentially clearer picture of differential effects for various kinds of construction workers. The dependent employed variable is binary variable equal to one if the worker is employed in construction, and equal to 0 if not employed in construction, which means they could be unemployed or employed in a different industry.

Tables 6 – 9 report results from Model 3 corresponding to Equations 9 – 11 which are used to estimate the effect of the probability of being employed in construction. The DDDD model takes the comparison of time trends one step further by estimating the differential effect on high-school dropouts, laborers, Hispanics, and blacks in construction in repeal states. The coefficient of interest on construction workers in repeal states who are high-school dropouts, laborers, etc. estimates a differential effect of repeal on construction workers within these groups, and not just construction workers in general.

Table 6 reports DD and DDD results from Equations 9 – 10 while sampling all workers, blue-collar workers, and laborers in each of the three panels respectively. The

coefficient of interest in this model is β_3 which gives the effect associated with construction workers in repeal states. Prevailing wage repeal is associated with a 5 – 9 percent increase in the probability of being employed for construction workers in repeal states, highly significant at the one percent level (row 3). These results are reflected in the lagged version of the equation in Table 7, which shows the same effects appearing three or more years after repeal (row 5). When sampling laborers, Table 7 reports a fifteen percentage point increase in the first two years and a ten percentage point increase after three years (row 4).

Table 8 reports results for the DDDD model in Equation 11, which adds interactions between construction workers in repeal states and each sub-group to control for a differential group effect. Extending the DDD model to a DDDD does not change the employment effect of construction workers associated with repeal. The results from the coefficient of interest, β_7, are listed in the seventh row of Table 8. They are statistically indistinguishable from zero with the exception of a negative five percentage point employment effect associated with Hispanic construction workers, signification at the five percent level (row 7). Table 9 reports results for the lagged version in Equation 12, and the results do not change, with the exception of an eleven percentage point increase in employment appearing for laborers within the first two years only, significant at the five percent level (row 10). The negative effect for Hispanics appears three or more years later and not the in the first two years (row 12).

Throughout the analysis, standard errors are clustered at the state level to account for the repeal of prevailing wage laws varying at the state level. A state-specific error

component exists because there are unobserved variables likely correlated with repeal (addressed with the fixed effects specification). Failure to cluster the standard errors would result in too much variation, which is really endogenous, and standard errors that are too small. Each specification is run with state-specific time trends.

Remaining omitted factors correlated with both the repeal of prevailing wages and employment rates could be introducing bias into these estimates. For example, degree of unionization is not available in these data and is therefore not estimated in the analysis. If states that choose to repeal their prevailing wage laws do so in part because they are more heavily unionized, this could positively bias the employment effects of repeal. A greater union presence in repeal states means that the higher wages associated with unionization might have disemployment effects before repeal, enhancing the positive employment effect after repeal.

In sum, there is strong contemporaneous and lagged evidence for an increase in the probability of being employed in construction as a result of prevailing wage repeal for construction workers in general, but little evidence to suggest differential effects among groups of construction workers by race or skill-level. This could be partially due to the failure of variables like high-school dropout and laborer in accurately estimating skill-level by proxy. The exceptions are a short-term positive eleven point employment effect for construction laborers which disappears three or more years after repeal and a long-term negative five point employment effect for Hispanic construction workers. The mixed differential group effects in all three models corroborate the result of a strong positive employment effect for construction workers in general, without seeing

differential effects within construction. Evidence from state-level employment variables in Models 1 and 2 generally do not pick up these effects.

Table 3: Effect of Prevailing Wage Law Repeal on Proportion Employed in Construction

	(1)	(2)	(3)	(4)
	% Construction = (Number Employed in Construction/All Employed) *100		% Construction = (Number Employed in Construction/Employed in Blue-Collar Occupations Only) *100	
Repeal	0.31	--	0.36	--
	(0.26)		(0.34)	
1 – 2 Years After Repeal	--	0.52	--	0.51
		(0.33)		(0.39)
3+ Years After Repeal	--	0.07	--	0.19
		(0.33)		(0.43)
Presence of Minimum Wage Law	-0.71***	-0.71***	-0.86***	-0.85***
	(0.18)	(0.17)	(0.22)	(0.22)
Employment Rate (0 to 100)	12.57***	12.65***	17.86***	17.92***
	(2.01)	(1.98)	(2.57)	(2.56)
Income Per Capita (thousands of dollars)	0.00***	0.00**	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
Age and Racial Composition Controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trend	Yes	Yes	Yes	Yes
Observations	1,757,687	1,757,687	1,757,687	1,757,687
R-squared	0.78	0.78	0.76	0.76

Standard errors in parentheses clustered at the states level ***p<0.01 **p<0.05 *p<0.1. An alternative lag specification using years since repeal date and years since repeal date squared revealed weak evidence of a positive employment effect beginning to appear over time.

Table 4: Effect of Prevailing Wage Law Repeal on Employment in Construction

	(1)	(2)	(3)	(4)
	%High-School Dropouts = (Number of Dropouts Employed in Construction/ All Blue-Collar) *100	% Hispanic = (Number of Hispanics Employed in Construction/ All Blue-Collar) *100	%Black = (Number of Blacks Employed in Construction/ All Blue-Collar) *100	%Laborers = (Number of Laborers Employed in Construction/ All Blue-Collar) *100
Repeal	5.50	-0.68	-2.30**	-0.94
	(4.75)	(1.58)	(1.06)	(1.60)
Presence of Minimum Wage Law	-48.11***	-1.48*	-0.83**	-0.11
	(1.73)	(0.77)	(0.41)	(1.08)
Employment Rate	111.66***	1.82	3.60	11.09
	(26.93)	(6.59)	(5.41)	(12.73)
Income Per Capita (1000's of dollars)	0.00***	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age and Racial Controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trend	Yes	Yes	Yes	Yes
Observations	1,759,179	1,759,179	1,759,179	1,759,179
R-squared	0.78	0.96	0.89	0.37

Standard errors in parentheses clustered at the states level ***p<0.01 **p<0.05 *p<0.1.

Table 5: Lagged Effect of Prevailing Wage Law Repeal on Employment in Construction

	(1)	(2)	(3)	(4)
	%High-School Dropouts = (Number of Dropouts Employed in Construction/All Blue-Collar) *100	% Hispanic = (Number of Hispanics Employed in Construction/ All Blue-Collar) *100	%Black = (Number of Blacks Employed in Construction/ All Blue-Collar) *100	%Laborers = (Number of Laborers Employed in Construction /All Blue-Collar) *100
1 – 2 Years After Repeal	0.84 (1.89)	0.10 (1.89)	-1.98** (0.81)	0.22 (2.11)
3+ Years After Repeal	0.55 (2.87)	-1.58 (1.93)	-2.72 (1.54)	-1.53 (2.51)
Presence of Minimum Wage Law	2.72*** (0.86)	-1.34* (0.75)	-0.84** (0.40)	0.41 (1.09)
Employment Rate	18.36*** (15.42)	1.91 (6.70)	5.71 (5.50)	4.25 (12.87)
Income Per Capita (1000's of dollars)	0.00*** (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Age and Racial Controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trend	Yes	Yes	Yes	Yes
Observations	1,757,687	1,757,687	1,757,687	1,757,687
R-squared	0.75	0.96	0.89	0.37

Standard errors in parentheses clustered at the states level ***p<0.01 **p<0.05 *p<0.1. An alternative lag specification using years since repeal date and years since repeal date squared found no differing or additional evidence.

Table 6: Effect of Prevailing Wage Law Repeal on Probability of Being Employed

	(1)	(2)	(3)	(4)	(5)	(6)
	All Workers		Sampling "Blue-Collar" Occupations Only		Sampling Laborers Only	
	DD	DDD	DD	DDD	DD	DDD
Repeal	0.01	0.00	0.01	0.00	-0.05**	-0.09***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Construction	--	0.04***	--	0.05***	--	-0.17***
	--	(0.01)	--	(0.01)	--	(0.02)
Repeal*	--	0.05***	--	0.06**	--	0.09***
Construction	--	(0.02)	--	(0.02)	--	(0.03)
Minimum Wage	-0.03***	-0.03***	-0.02***	-0.02***	0.08***	-0.06***
(Binary)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographic, Education, Marital Status controls	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,767,021	1,767,021	1,366,611	1,366,611	47,668	47,668
R-squared	0.12	0.12	0.11	0.11	0.04	0.06

Standard errors in parentheses clustered at the state level ***p<0.01, **p<0.05, *p<0.1

Table 7: Lagged Effect of Prevailing Wage Law Repeal on Probability of Being Employed

	(1)	(2)	(3)	(4)	(5)	(6)
	All Workers		Sampling "Blue-Collar" Occupations Only		Sampling Laborers Only	
	DD	DDD	DD	DDD	DD	DDD
1-2 Years After Repeal	-0.01	-0.01	-0.00	-0.00	-0.04	-0.10**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)
3+ Years After Repeal	0.00	0.01	0.01	0.01)	-0.07***	-0.10***
	(0.01)	(0.01)	(0.01)	(.0.01)	(0.03)	(0.02)
Construction	--	0.01	--	0.05***	--	-0.17***
	--	(0.01)	--	(0.01)	--	(0.02)
1-2 Years After Repeal* Construction	--	0.04	--	0.04	--	0.15***
	--	(0.03)	--	(0.03)	--	(0.05)
3+ Years After Repeal* Construction	--	0.05***	--	0.06***	--	0.08***
	--	(0.02)	--	(0.02)	--	(0.03)
Minimum Wage (Binary)	0.01***	0.00***	0.00***	0.00***	-0.07***	-0.06***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Demographic, Education, Marital Status controls	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,767,021	1,767,021	1,366,611	1,366,611	47,668	47,668
R-squared	0.12	0.12	0.11	0.11	0.04	0.06

Standard errors in parentheses clustered at the states level ***p<0.01 **p<0.05 *p<0.1. An alternative lag specification using years since repeal date and years since repeal date squared found no differing or additional evidence.

Table 8: Effect of Prevailing Wage Law Repeal on Probability of Being Employed (By Group)

	(1)	(2)	(3)	(4)
	High-School Dropouts	Hispanics	Blacks	Laborers
	DDDD	DDDD	DDDD	DDDD
Repeal	0.02*	0	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Construction	0.03***	0.04***	0.05***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)
Group	0.03***	0.03	0.01	0.25***
	(0.01)	(0.02)	(0.02)	(0.01)
Repeal*Construction	0.06***	0.06***	0.06**	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)
Repeal*Group	-0.02*	0.03	0.04**	0.00
	(0.01)	(0.02)	(0.01)	(0.01)
Construction*Group	0.04***	0.08***	-0.03**	-0.35***
	(0.01)	(0.01)	(0.01)	(0.02)
Repeal*Construction	0.00	-0.05**	-0.02	0.03
*Group	(0.01)	(0.02)	(0.03)	(0.02)
Minimum Wage	-0.01***	-0.02***	-0.02***	-0.02***
(Binary)	(0.00)	(0.00)	(0.00)	(0.00)
Demographic, Education, Marital Status controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes
Observations	1,366,611	1,366,611	1,366,611	1,366,611
R-squared	0.11	0.11	0.11	0.11

This table only includes blue-collar workers. Standard errors in parentheses clustered at the state level.
*** p<0.01 ** p<0.05 *p<0.1

Table 9: Lagged Effect of Prevailing Wage Law Repeal on Probability of Being Employed (High-School Dropouts, Hispanics, Blacks, Laborers)

	(1)	(2)	(3)	(4)
	High-School Dropouts	Hispanics	Blacks	Laborers
	DDDD	DDDD	DDDD	
1 – 2 Years After Repeal	-0.01	-0.00	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
3 + Years After Repeal	-0.00	0.00	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Construction	-0.00	0.03***	0.05***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)
Group	-0.16***	0.02	0.01	0.24***
	(0.00)	(0.02)	(0.02)	(0.01)
1 – 2 Years After Repeal*Construction	0.04*	0.05*	0.04	0.02
	(0.02)	(0.03)	(0.03)	(0.03)
3+ Years After Repeal*Construction	0.05**	0.06***	0.06***	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)
1 – 2 Years After Repeal*Group	0.01	0.04**	0.04	-0.02
	(0.01)	(0.02)	(0.03)	(0.02)
3+ Years After Repeal*Group	-0.03**	0.03	0.04***	0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Construction*Group	0.11***	0.08***	-0.03**	-0.35***
	(0.01)	(0.01)	(0.01)	(0.02)
1 – 2 Years After Repeal*Construction* Group	-0.01	-0.08	0.00	0.11**
	(0.03)	(0.05)	(0.08)	(0.04)
3 + Years After Repeal*Construction* Group	0.04	-0.05**	-0.03	0.01
	(0.02)	(0.02)	(0.03)	(0.02)

Table 9: Continued

Minimum Wage (Binary)	0.01*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)
Demographic, Education, Marital Status controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes
Observations	902,332	1,366,611	1,366,611	1,366,611
R-squared	0.13	0.11	0.11	0.11

This table only includes blue-collar workers. Standard errors in parentheses clustered at the states level ***p<0.01 **p<0.05 *p<0.1. An alternative lag specification using years since repeal date and years since repeal date squared found no differing or additional evidence.

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APPENDICES

APPENDIX A

DATA

This appendix accompanies the data chapter. The CPS dataset includes 45 variables:

1. “year”
2. “serial”: household serial number
3. “hwtsupp”: household weight
4. “statefip”: state FIPS code
5. “asecflag”: flag for ASEC
6. “metro”: metropolitan central city status
7. “metarea”: metropolitan area
8. “county”: FIPS county code
9. “cpi99”: CPI-U adjustment factor to 1999 dollars
10. “month”
11. “pernum”: person number in sample unit
12. “wtsupp”: supplement weight
13. “earnwt”: earnings weight
14. “age”
15. “sex”
16. “race”
17. “marst”: marital status
18. “popstat”: adult civilian, armed forces, or child
19. “bpl”: birthplace
20. “citizen”: citizenship status
21. “hispan”: Hispanic origin

22. “educ”: educational attainment recode

23. “empstat”: employment status

24. “labforce”: labor force status

25. “occ1990”: occupation, 1990 basis

26. “ind1990”: industry, 1990 basis

27. “classwkr”: class of worker

28. “uhrswork”: usual hours worked per week last year

29. “fullpart”: worked full or part time last year

30. “hourwage”: hourly wage

31. “paidhour”: paid by the hour

32. “pension”: pension plan at work

33. “union”: union membership

34. “firmsize”: number of employees

35. “incwage”: wage and salary income

36. “incss”: Social Security income

37. “incwelfr”: welfare (public assistance) income

38. “incgov”: income from other government programs

39. “incwkcom”: income from worker’s compensation

40. “earnweek”: weekly earnings

41. “disabwrk”: work disability

42. “paidgh”: employer paid for group health plan

43. “emcontrb”: employer contribution for health insurance

44. “jobtrain”: attended job training program

45. “schlcoll”: school or college attendance

The non-blue collar occupations dropped include:

1. “managerial and professional specialty occupations” (occ1990 = 003-083),
2. “health occupations” (occ1990 = 084-106), other occupations (occ199=113-200)
3. “technical occupations” (occ1990 = 203-235)
4. “sales and administrative support” (occ1990= 243-391), and
5. “farming” (occ1990= 472-499).

The fourteen “laborer” occupations chosen are for the “conlaborer” dummy variably are:

1. “painters, construction, and maintenance” (occ1990 = 579)
2. “roofers and slaters” (occ1990 = 595)
3. “construction laborers” (occ1990 = 869)
4. “drilling and boring machine operators” (occ1990 = 708)
5. “grinding, abrading, buffing, and polishing workers” (occ1990 = 709)
6. “fabricating machine operators” (occ1990 = 717)
7. “metal platers” (occ1990 = 723)
8. “textile sewing machine operators” (occ1990 = 744)
9. “miscellaneous textile machine operators” (occ1990 = 749)
10. “packers, fillers, and wrappers” (occ1990 = 754)
11. “solderers” (occ1990 = 784)
12. “bus drivers” (occ1990 = 808)

13. “parking lot attendants” (occ1990 = 813)

14. “helpers, construction” (occ1990 = 865)

APPENDIX B

FIGURES

Figure 8: Construction Workers by Age (1976 – 2000)

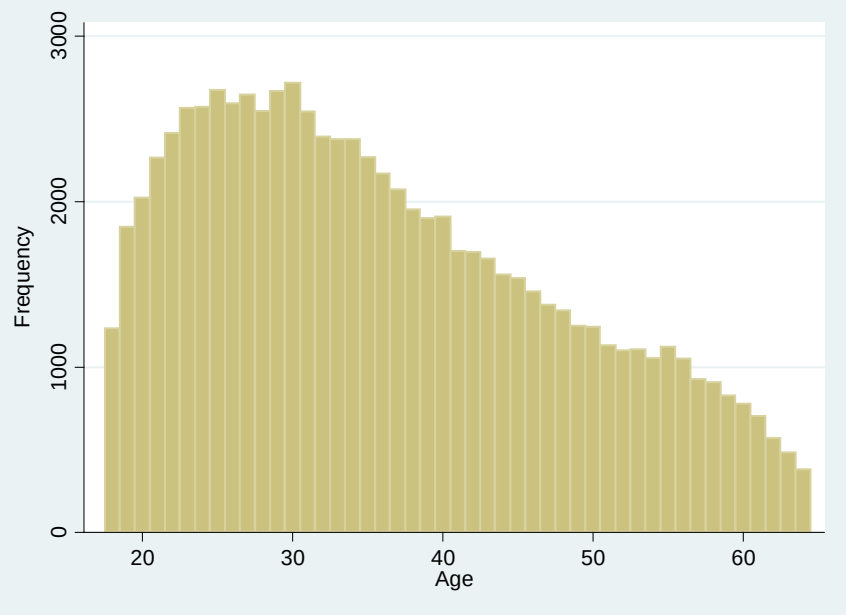

Figure 9: Construction Workers by Race (1976 – 2000)

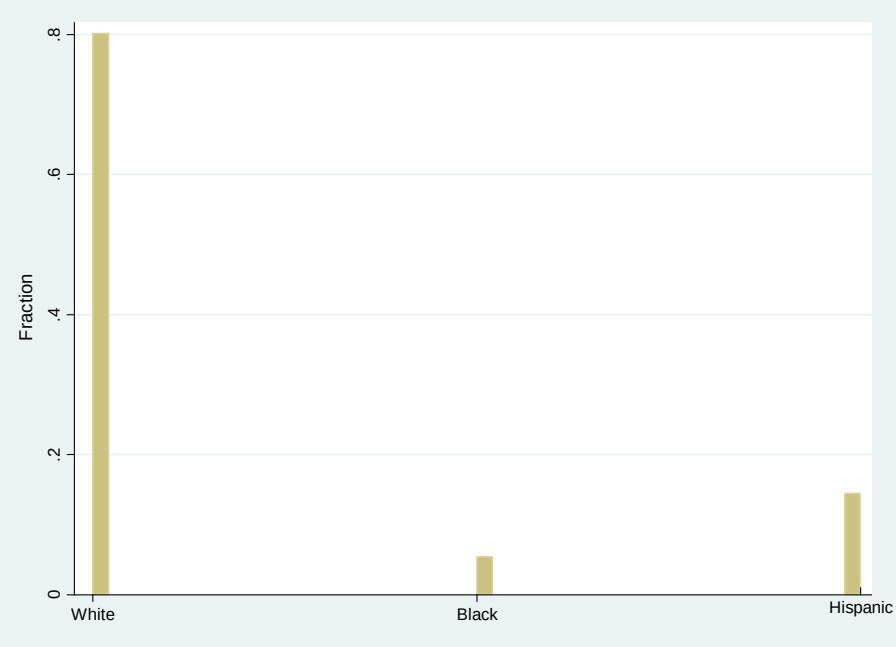

Figure 10: Construction Workers By Education Level (1976 – 2000)