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PREVAILING WAGE LAWS AND CONSTRUCTION LABOR MARKETS

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ABSTRACT

Prevailing wage laws, which require that construction workers employed by private contractors on public projects be paid at least the wages and benefits that are "prevailing" for similar work in or near the locality in which the project is located, have been the focus of an extensive policy debate. We find that the relative wages of construction workers decline slightly after the repeal of a state prevailing wage law. However, the small overall impact of law repeal masks substantial differences in outcomes for different groups of construction employees. Repeal is associated with a sizeable reduction in the union wage premium and a significant narrowing of the black/nonblack wage differential for construction workers.

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Introduction

Prevailing wage laws, which require that construction workers employed by private contractors on public projects be paid at least the wages and benefits that are "prevailing" for similar work in or near the locality in which the project is located, have been the focus of an extensive policy debate. Early empirical research on this topic sought to estimate the direct financial costs of prevailing wage laws to governments. To the extent that the prevailing wage is above the market wage, the laws may impose financial costs both through increased wage bills for construction projects (e.g., Gujarati 1967; GAO 1979; Bourdon and Levitt 1980; Goldfarb and Morrall 1981; Fraundorf et al. 1982, Thieblot 1996), and through an inefficient mix of capital and labor and of different types of workers (e.g., CBO 1982).

However, because public construction accounts for between one-fifth and one-quarter of all construction, and because prevailing wage laws cover a substantial number of private projects undertaken with public financing or assistance (e.g., CBO 1982, U.S. Department of Commerce 1996), prevailing wage laws may also affect construction labor markets more broadly. These broad effects of the laws may have public policy implications beyond those implied by the direct financial costs of the laws to government. If governments are the marginal purchasers of construction services, then laws that require governments to pay a supramarket wage on all projects may aid unions by reducing the potential cost advantage of nonunion labor. Furthermore, if worker training or safety is undersupplied from a social perspective by competitive construction markets, then prevailing wage laws may provide incentives for optimal training and safety (Philips et al. 1995). On the other hand, prevailing wage laws may aggravate discrimination against blacks in the construction industry by reducing nonunion-worker competition with members of historically-discriminatory trade unions. Bernstein (1993, 1994) has argued that the federal prevailing wage law, the Davis-Bacon Act, was passed with discriminatory intent (but see Belman and Philips (1996) for an opposing view), and Keyes (1982) finds that Davis-Bacon contributes to the low percentage of skilled black construction workers.

Despite this policy importance, there is little evidence on the extent of the effects of prevailing wage laws on construction labor markets. Identifying the impact of the Davis-Bacon Act on construction costs is difficult because its national scope and long history mean that there are no suitable construction labor markets to serve as a "control" group not subject to the Act. Thus, this paper investigates the impact of the repeal of *state* prevailing wage laws -- which specify that state governments must pay prevailing wages on state- and locally-financed construction -- from 1970-1993 on labor markets for construction workers. It employs a modified difference-in-difference-in-difference approach to estimating the impact of the laws, estimating the effect of law repeal as the change over time in labor market outcomes for blue-collar construction versus nonconstruction workers from states that repealed their laws to the change over time in outcomes for workers from states that did not. We also assess the extent to which law repeal has differential effects across groups of construction workers: on union versus nonunion, and black versus nonblack workers.

The paper proceeds in four sections. Section I provides background on prevailing wage legislation and discusses the existing empirical literature. Section II describes the state law repeals that we evaluate and presents our data and econometric models. Section III presents our results, and Section IV concludes.

I. Background on Prevailing Wage Laws

Prevailing wage laws exist at the federal, state, and local level. The federal prevailing wage law, the Davis-Bacon Act, was passed in 1931.[1] The current Act, modified by amendments in 1935 and 1964, requires private contractors to pay workers the prevailing wage/benefit package on all contracts of more than $2,000 for construction, alteration, or repair of federal public buildings or public works. The "prevailing wage" referenced in the Act is defined by the Secretary of Labor as the package of wages and benefits paid to the majority of workers in a given occupation grouping in the geographic area of the project; if the majority of workers do not earn the same wage/benefit package, then the prevailing wage is equal to the average wage/benefit package paid to these workers.[2] The "public projects" covered by the Act include all construction purchased directly by the federal government, plus most private but federally financed or assisted construction: the terms of the Act cover construction undertaken through more than 58 other laws (CBO 1982).

State prevailing wage laws set a minimum wage for construction workers on state (and generally municipal) works projects. Their terms differ across states in multiple dimensions, and are frequently defined customarily rather than by reference to written statutes or regulations (Thieblot 1986, 1995). Some state prevailing wage laws are almost nonbinding; others set wages for virtually all contracts at the collectively-bargained wage level. In addition, different states' laws treat jointly financed projects (e.g., state/federal, local/federal, private/public) differently. Some states defer to the federal Act; others preempt the federal Act; others set the state prevailing wage at the higher of the state or federal prevailing wage. The scope of projects and workers covered under state laws also varies. States explicitly include or exclude specific types of projects (such as road construction) and/or workers, and/or projects above or below a given value.

Even if state prevailing wage laws covered purely state and local public construction only, they would have a potentially significant influence on construction labor markets. State and local public construction accounts for 16 to 22 percent of all construction over the 1970-1993 period, as compared to 2 to 3 percent for federal public construction (US Department of Commerce 1996, Current Construction Reports C30, Tables 1-2). However, the federal prevailing wage law is likely to affect labor markets disproportionately, because state and local construction projects are often partially federally funded. (Indeed, even though federal public construction is only 2-3 percent of total construction, CBO estimates suggest that 20 to 25 percent of all construction is covered by the Davis-Bacon Act (CBO 1982).)

The early empirical literature was concerned with assessing the impact of the Davis-Bacon Act on the federal government's construction costs (e.g., Gujarati 1967; GAO 1979; Bourdon and Levitt 1980; Goldfarb and Morrall 1981; Fraundorf et al. 1982). These studies estimate the impact of Davis-Bacon as the difference between the Department of Labor's posted Davis-Bacon prevailing wage and the average wage for construction workers of a given occupation in a given geographic area. Although the studies agree that Davis-Bacon increases the government's labor costs for construction, they report a wide range of point estimates (from 4 to 38 percent).

However, identifying the impact of the Davis-Bacon Act on construction costs is difficult because its national scope and long history mean that there are no suitable construction labor markets to serve as a “control” group not subject to the Act. In particular, because the average wage in an area is a function of the Davis-Bacon prevailing wage, the difference between the Davis-Bacon wage and the average wage is not an unbiased estimate of the additional labor costs borne by the government, over what they would have paid in the absence of Davis-Bacon (but see Allen (1983) for a novel way to correct for this bias). Most seriously, though, this identification problem limits the generalizability of the findings from the early literature to construction labor markets more broadly, and thereby limits the extent to which the early literature can illuminate the policy questions of interest.

In recognition of these gaps, and of the size of the share of state and local public construction, more recent work investigates the impact of *state* prevailing wage laws, by comparing labor market outcomes across states with differing laws. Building on prior research (Thieblot 1986, 1996; Philips et al. 1995), Table 1 presents the effective dates for the adoption and repeal of state prevailing wage laws. As of 1969, forty states had prevailing wage laws that covered construction financed by state and local governments. Between 1969 and 1993, nine states repealed their prevailing wage laws, and Minnesota enacted a prevailing wage law. As discussed above, because of the multidimensional variation across states in the prevailing wage statutes and regulatory policies, there is no way a priori to categorize the laws more finely.

Conclusions from the state prevailing wage law studies are not as uniform as results from analyses of the effects of Davis-Bacon. Allen and Reich (1980) report that state prevailing wage laws have no significant effect on school construction costs, holding constant price levels, urbanization, and climate variables, although Allen (1987) suggests that prevailing wage laws enable union contractors to receive a higher price for similar school and hospital projects than can nonunion contractors. Philips et al. (1995) find that the average construction wage declined more in states that repealed their prevailing wage laws than in states that did not, based on 4-digit SIC average wages. They also find that repeal of state prevailing wage laws is associated with an increase in injury rates for plumbers and pipe fitters, although Thieblot (1996) shows that average construction injury rates in repeal states declined by more than average rates in nonrepeal states.

However, concerns about unobserved differences across states and over time complicate interpretation of the estimated effect of interest from all of the state law studies. The studies fail to control for fixed differences across states in laws, regulatory policies, and other characteristics; for state/time-varying macroeconomic factors affecting all blue-collar labor markets; and for state/time-varying microeconomic factors such as the occupational and skill mix of construction workers. For example, if states that repeal their prevailing wage laws also have unobserved fixed differences in policies that lead to lower construction wages, then the estimated effect of the laws might represent in part unobserved fixed differences across states, leading to the overstatement of the impact of prevailing wage law repeal. Along these lines, if states repeal prevailing wage laws in response to weak blue-collar labor markets (to improve competitiveness), then the estimated absolute effect of the laws on construction workers might represent in part the effect of unmeasured labor market conditions for all blue-collar workers. Either of these biases would be magnified if contractors respond to repeals by employing less-skilled workers, because the estimated effects might represent in part unobserved changes in workforce composition. And, in any event, the existing work does not address other policy questions of interest, such as the differential impact of prevailing wage laws on black and union workers.

II. Models and Data

We investigate the impact of state prevailing wage law repeal with methods that directly address these concerns. Our basic specification compares time trends in blue-collar construction and nonconstruction labor market outcomes across repeal and nonrepeal states during a 24 year period. We model wages and unionization rates as nonparametric functions of worker characteristics, state-fixed-effects, time-fixed-effects, and prevailing wage laws; thus, we control for fixed differences across states and over time. We use individual data on workers from the census and Current Population Survey (CPS), which enable us to control for changes in workforce composition. Finally, we estimate the impact of prevailing wage law repeal as the difference between the change over time in the *relative* blue-collar construction/nonconstruction wage in repeal states and nonrepeal states, 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.

While this is fundamentally a difference-in-difference-in-difference (DDD) approach to estimating the impact of the laws, we modify conventional DDD strategies in several ways. First, our models include few restrictive parametric or distributional assumptions about the relationship between laws, individual characteristics, and labor market outcomes. Second, we do not only model the impact of law repeal as having a one-time effect on the levels of outcomes. For example, to the extent that multi-year contracts influence construction wages, changes in prevailing wage laws may not have instantaneous effects on labor markets. In addition, to the extent that prevailing wage laws affect the bargaining positions of workers and employers, the laws may affect future wage growth as well as current wage levels. Third, in order to investigate the impact of repeal on black and on union workers, we use a difference-in-difference-in-difference-in-difference (DDDD) approach to estimate the relative growth in the blue-collar black/nonblack (union/nonunion) construction/nonconstruction wage differential in repeal versus nonrepeal states.

To illustrate DDD estimation of the impact of law repeal, Table 2 presents descriptive statistics from both individual- and establishment-level data sets on wage trends during the 1980s for construction and nonconstruction workers, for states repealing and not repealing prevailing wage laws.[3] In addition to analysis of the census and CPS, Table 2 reports for comparison purposes descriptive statistics from the Department of Labor's ES-202 data gathered by establishment on employees covered by various unemployment insurance programs. The "before law repeal" columns (1) and (4) present average wages for 1979 (1979 CPS and ES-202, 1980 census); the "after law repeal" columns (2) and (5) present average wages for 1993 (CPS and ES-202) and 1989 (1990 census). The "time difference for location" columns (5) and (6) present the proportional change in wages before versus after law repeal in a given type of state.

The top panel of Table 2 compares the change in average wages for construction workers in the states that repealed their laws ("expermental" states) to the change in average wages for construction workers in the states that did not ("nonexperimental" states). In the census, for example, there was a 17.5 percent fall in construction workers' wages over the 1980s in states that repealed their laws, compared to a 12.9 percent fall in wages in states that did not. Thus, there was a 4.7 percent relative fall in construction workers' wages in states that repealed their prevailing wage laws; this is the differences-in-differences (DD) estimate of the impact of law repeal.

However, if there were unobserved time-varying factors that affected all blue-collar labor markets and were correlated with the status of labor market regulation, this DD estimate would not identify the impact of the law: the DD estimate would be a combination of the impact of the law and of the impact of the unobserved factors affecting all blue-collar labor markets. The bottom panel of Table 2 investigates this possibility by examining the change in average wages for nonconstruction workers in experimental relative to nonexperimental states. In fact, in the census, there is a slight fall in relative wages in experimental states of 2.2 percent, which suggests that controlling for factors correlated with law repeal that influence all blue-collar workers may be important.

Taking the difference between the first and seventh rows of column (7) of Table 2 shows that there is a 2.5 percent fall in the *relative* wages of construction workers in states that repealed their prevailing wage laws, compared to the change in relative wages in the nonexperimental states. This DDD estimate suggests that prevailing wage law repeal has a causal impact on construction labor markets. This conclusion is supported by replication of this result with other data sets, collected for different types of workers. Simple DDD estimates from the CPS and ES-202 are similar, at -1.3 percent for blue-collar workers in the CPS (= -0.020 + 0.007), -1.9 percent for all workers in the CPS (= -0.016 - 0.003), and -1.8 percent for all workers in the ES-202 (= -0.041 + 0.023).

Our formal models analyze repeated cross sections of blue-collar workers from the 1970, 1980, and 1990 census and the 1977-1993 CPS. In state s = 1, ..., S during year t = 1 , ..., T, one observation in our model is an individual worker i = 1, ..., N_{st}. Each worker has occupational and demographic characteristics X_{ist}, which we describe as a set of binary variables. We describe worker i's industry of employment with C_{ist}, where C_{ist} = 1 if worker i was employed in construction in state s and year t and 0 otherwise. Depending on the year of the survey and the worker's state of residence, the worker's labor market outcome may be affected by a prevailing wage law. We define L_s = 1 if state s repealed its prevailing wage law over the 1970-1990 period (L_s = 0 otherwise), and A_{st} = 1 if the repeal occurred before year t (A_{st} = 0 otherwise).[4] We study two labor market outcomes: real hourly wages W_{ist} and union status U_{ist}, where U_{ist} = 1 if worker i was a union member and 0 otherwise.

In regression terms, the basic DDD models of wages and unionization rates are of the form

$$\ln(W_{ist}) = \theta_t + \theta_t^c \cdot C_{ist} + \alpha_s + \alpha_s^c \cdot C_{ist} + L_s \cdot A_{st}\gamma_1 + L_s \cdot A_{st} \cdot C_{ist}\gamma_2 + X_{ist}\beta + \varepsilon_{ist}, \qquad \textbf{(1)}$$

where $è_t$ is a time-fixed-effect, $á_s$ is a state-fixed-effect, $å_{ist}$ is an error term,[5] and $ã_2$ is the effect of interest -- the change over time in the construction wage premium in states that repealed their prevailing wage laws relative to the change over time in states that did not.

We use the CPS to investigate the dynamic impacts of prevailing wage law repeal by estimating separately the change over time soon after repeal and long after repeal in the construction wage premium in states that repealed their prevailing wage laws relative to the change over time in states that did not, where

$$\ln(W_{ist}) \ ' \ è_t \% è_t^c (C_{ist} \% á_s \% á_s^c (C_{ist} \% L_s (SA_{st} ã_{1s} \% L_s (SA_{st} (C_{ist} ã_{2s} \% \\ L_s (LA_{st} ã_{1l} \% L_s (LA_{st} (C_{ist} ã_{2l} \% X_{ist} â \% å_{ist}. \tag{1a}$$

In one time-since-adoption specification, we defined $SA_{st} = 1$ if the repeal occurred 1-2 years before year t ($SA_{st} = 0$ otherwise), and $LA_{st} = 1$ if the repeal occurred 3 or more years before year t ($LA_{st} = 0$ otherwise). In an alternative time-since-adoption specification, we defined $SA_{st} = 1$ if the repeal occurred 1-4 years before year t ($SA_{st} = 0$ otherwise), and $LA_{st} = 1$ if the repeal occurred 5 or more years before year t ($LA_{st} = 0$ otherwise).

Finally, we estimate DDDD models to assess the impact of prevailing wage law repeal on the change over time in differences by race and union status in the construction wage premium ($ã_4$):

$$\ln(W_{ist}) \ ' \ è_t \% è_t^b (B_{ist} \% è_t^c (C_{ist} \% è_t^{b(c} (B_{ist} (C_{ist} \% á_s \% á_s^b (B_{ist} \% á_s^c (C_{ist} \% á_s^{b(c} (B_{ist} (C_{ist} \\ \% L_s (A_{st} ã_1 \% L_s (A_{st} (B_{ist} ã_2 \% L_s (A_{st} (C_{ist} ã_3 \% L_s (A_{st} (B_{ist} (C_{ist} ã_4 \% X_{ist} â \% å_{ist}. \tag{2}$$

We estimate the effect of law repeal on trends in differences in the construction wage premium soon after versus long after repeal with versions of equation (2) analogous to equation (1a). Analogues to equation (2) that substitute U_{ist} for B_{ist} assess the impact of law repeal on the change over time in differences by union status in the construction wage premium.

We estimate the parameters of models (1) and (2) with nonfarm private-sector blue-collar wage and salary workers aged 16 to 64 using the Current Population Survey (CPS) and the census. Our principal CPS results are based on the currently employed workers in the Merged Outgoing Rotation Groups (MORGs) for 1979-1993 plus such workers in the May CPS survey groups from 1977-1978; we appended the earlier May CPS samples to the CPS MORGs to expand the time period covered by our analysis in the years before law changes occurred.[6] For the same reason, our CPS results estimating the impact of repeal on the relative union wage premium for construction workers (equation (2)) are based on the 1983-1993 MORGs matched with May CPS survey groups from 1977-1981 (because the CPS did not begin asking all outgoing rotation groups about their union status until 1983, and the CPS did not ask any workers about their union status in 1982).[7] The vector of control variables X in the CPS includes indicators for age (16-20, 21-25, 26-30, 31-35, 36-40, 41-45, 46-50, 51-55, 56-60, 60-64), educational attainment (less than high school, high school, some college, college graduate or more), marital status (married or unmarried), gender, black or nonblack race, and one-digit occupational classification (craftmen and kindred workers, operatives except transport, transport equipment operatives, nonfarm laborers, service workers), plus an interaction effect between each element of X and an variable indicating whether the observation was from the 1986 CPS or later.

Our census results are based on workers employed at any time during the previous year from the 1970, 1980, and 1990 public use census files. Thus, the wage information in the three censuses pertains to the years 1969, 1979, and 1989. To calculate the hourly wage for workers in 1970 with missing hours, we imputed average hours per week based on reported hours per week for workers in 1980 with the same age, sex, race, ethnicity, marital status, educational attainment, industry, and occupation. For all years, we analyze a 1 percent sample of blue-collar nonconstruction workers. For 1980 and 1990, we analyze a 5 percent sample of blue-collar construction workers; for 1970, a 5 percent sample with state information is not available, so we analyze a 2 percent sample of blue-collar construction workers (U.S. Department of Commerce 1972). Because Minnesota enacted a prevailing wage law in 1973, we omit workers from Minnesota from our analysis of the census data. The vector of control variables X in the census includes all of the variables from the CPS analysis plus indicators for hispanic/nonhispanic ethnicity and foreign/US country of birth, plus interactions between year indicators and each element of X. Analyses of the census data weight each observation by the inverse of its sampling probability.

III. Results

Basic Difference-in-Difference-in-Difference Results

Table 3 presents estimates of $\tilde{a}_2$, the effect of prevailing wage law repeal on construction wage differentials. In contrast to the simple DDD estimates from Table 2, our regression estimates of the impact of law repeal control for changes in workforce demographic and occupational composition across states and over time (except column (1)), for time- and state-fixed-effects, and for construction*time- and construction*state-fixed-effects (except columns (2) and (4), which control for construction*time and construction*L_s). In addition, our regression estimates allow returns-to-characteristics to differ in 1970, 1980, and 1990 (census) and to differ before and on/after 1986 (CPS). Columns (1)-(3) provide estimates based on the 1970, 1980, and 1990 census (column (1) reports DDD estimates on the effect of repeal on the change over time in wage levels from equation (1) without controls for demographic and occupational characteristics X_{ist}). Columns (4)-(7) provide estimates based on the 1977-1993 CPS: columns (4) and (5) report estimates from equation (1), and columns (6) and (7) report estimates of the time-since-adoption specification given by equation (1a).

The regression results reported in Table 3 echo the simple DDD estimates reported in Table 2: repeal of prevailing wage laws leads to slight decreases in construction wage differentials. In the census, the DDD impact of repeal on the change over time in wage levels ranges from 2 to 4 percent, depending on the controls included in the regression.[8] Average effect estimates of the impact of law repeal from the CPS are of the same or smaller magnitude as those from the census. The estimated impact of repeal from equation (1) calculated controlling for construction*L_s (column (4)) is -3.9 percent. Controlling for a full set of construction*state-fixed-effects reduces this estimate substantially in both the basic and the time-since-adoption specification (columns (5) - (7)), suggesting that state heterogeneity in construction labor markets is an important determinant of wage schedules.

Differential Impacts of Prevailing Wage Law Repeal by Race

However, estimates of the average effect of law repeal fail to identify the differential effects of repeal across groups of construction workers, which also may be important from a policy perspective. We began by analyzing the effects of law repeal on racial differences in construction employment. In the 1990 census, blacks comprised 11.7 percent of blue-collar nonconstruction workers and 7.4 percent of blue-collar construction workers, leading to a black construction employment differential of -4.3 percentage points (= 0.074 - 0.117). In the census, we found that the black construction employment differential shrunk by 1 percentage point in repeal versus nonrepeal states over our sample period, controlling for demographic and occupational characteristics (allowing returns-to-characteristics to vary over time), and for state-fixed-effects, time-fixed-effects, time-fixed-effects*black, and L_s*black. However, the statistical significance of this result was not robust to the inclusion of a full set of state*black interactions, and we did not find any significant trends in blacks' construction employment differentials in the CPS.

For this reason, we focus on the relative wage impacts of repeal. Table 4 reports DDDD estimates of $\tilde{a}_4$, the impact of repeal on racial differences in the construction wage premium from equations (2) and (2a) using the census and the CPS. Except for column (1), the models in the Table control for demographic and occupational characteristics (allowing returns-to-characteristics to vary over time), and for state-fixed-effects, time-fixed-effects, state-fixed-effects*construction, time-fixed-effects*construction, time-fixed-effects*black, state-fixed-effects*black, time-fixed-effects*black*construction, and state-fixed-effects*black*construction.

The results in Table 4 show that repeals affect black and nonblack construction workers differently. Columns (1) and (2) show that in the census, the construction wage premium earned by black workers rose by approximately 4 percentage points relative to that earned by nonblacks as a result of prevailing wage law repeal; this difference is statistically significant at the 5 percent level. According to the CPS, law repeal led to statistically significant increases (at the 10 percent level) in the long-run relative construction wage premium for blacks of 5.5 to 6.8 percentage points, depending on specification (columns (3) - (5)). In both the census and the CPS, repeal has a negative effect on nonblacks' construction wage premium, although this effect is only statistically significant in the census.

The differential impact of law repeal on blacks' construction wage premium has two important implications. First, because the raw difference between blacks' and nonblacks' construction wage premium is 6.5 percentage points,[9] law repeal eliminates at least two-thirds of this difference (. 4 / 6.5). Second, in no specification does law repeal significantly reduce the absolute level of black workers' construction wage differential, and in some specifications positively *increases* black workers' construction wage differential, even as it reduces the construction differential for nonblack workers. In terms of equation (2), the impact of law repeal on the absolute level of black workers' construction wage differential is equal to $\tilde{a}_3 + \tilde{a}_4$. In the census, $\tilde{a}_3 + \tilde{a}_4$ is small in magnitude, but in the CPS, the long-run estimate of $\tilde{a}_3 + \tilde{a}_4$ from column (5) is approximately equal to 4.8 percent.

Differential Impact of Prevailing Wage Law Repeal by Union Status

Based on the CPS, Table 5 presents estimates of the effect of law repeal on the construction unionization differential (equations (1) and (1a), columns (1) - (3)) and the relative union wage premium for construction workers (equations (2) and (2a), columns (4)- (6)). Regression results suggest that repeal in the long run reduces the relative construction unionization differential slightly, although this result is not statistically significant, and is extremely small in magnitude (and actually positive in sign in the short run). However, prevailing wage law repeal does significantly reduce the relative union wage premium for construction workers, controlling for worker demographic and occupational characteristics (allowing returns-to-characteristics to vary over time), and for state-fixed-effects, time-fixed-effects, state-fixed-effects*construction, time-fixed-effects*construction, time-fixed-effects*union, state-fixed-effects*union, time-fixed-effects*union*construction, and state-fixed-effects*union*construction. The first row of column (4) suggests that prevailing wage law repeal reduces the relative union wage premium paid to construction workers by approximately 5.9 percentage points. Columns (5) and (6) show that this effect grows in magnitude over time: three years after law repeal, the relative union wage premium paid to construction workers falls by approximately 9.9 percentage points, growing to 11.2 percentage points after 5 or more years.[10] These effects are both statistically and economically significant. Estimates from the 1983-1993 CPS MORGs indicate that the additional average union wage differential earned by construction workers over that earned by nonconstruction workers is approximately 20 percent. Thus, law repeal reduces the relative union wage differential for construction workers by approximately half. However, law repeal in the long run is not statistically significantly negatively correlated with nonconstruction union wage differentials. Indeed, states repealing their prevailing wage laws show statistically significant *increases* in nonconstruction unionization. Thus, there is no evidence that prevailing wage law repeal is correlated with other state-level policies that have adverse effects on unions.

Understanding Differential Impacts by Race and Union Status

We conducted additional analyses to investigate the mechanism by which law repeal affects racial differences in construction wage differentials, and the construction wage premium generally. First, law repeal does not have its most important impact on racial differentials through its impact on the unionization rate. Following Ashenfelter (1972), we calculated the total contribution of unionization to the black/nonblack wage differential as

$u_{bj}\tilde{a}_j - u_{wj}\tilde{a}_{j'}$, where u_{bj} (u_{wj}) is the share of black (nonblack) workers who are union members in industry j and $\tilde{a}_j$ ($\tilde{a}_{j'}$) is the union/nonunion wage differential for blacks (nonblacks) in industry j. In other words, we estimate what the change in the racial wage differential would be if the unionization rate were zero, relative to the current racial differential. Since the estimated total contribution of unionization to racial differences in the construction wage premium is approximately 3 percentage points,[11] even a 6 percent decrease in the unionization rate (= 1.5

percentage points/25 points total unionization rate, where 1.5 percentage points is approximately the maximum impact of the law repeals) would reduce the black/nonblack construction wage differential by at most 0.18 percentage points (=0.06 * 3). Thus, law repeal, which has an estimated total effect an order of magnitude larger, must affect wage differentials primarily through means other than reduced unionization.

Second, repeal achieves its approximately 2-percent effect on the aggregate construction wage differential primarily by reducing the union wage premium paid to construction workers. If the average construction unionization rate is equal to u, then the average effect for all construction workers of law repeal in terms of the parameters from equation (2) (substituting U_{ist} for B_{ist}) is equal to $(1-u)*\tilde{a}_3 + u*(\tilde{a}_3 + \tilde{a}_4)$. Since u . 0.25 (based on the CPS), $\tilde{a}_3 = 0.007$ and $\tilde{a}_4 = -0.112$ (Table 5, column (6)), most of the effect on the aggregate wage differential comes through $\tilde{a}_4$ rather than through $\tilde{a}_3$.

Sensitivity Analyses

One important concern with any analysis of the effects of law changes is policy endogeneity – the correlation of law repeal with unobserved time-varying determinants of wages. We found no substantial evidence that prevailing wage law repeal was endogenous. First, according to Thieblot (1986, 1996), the ways in which repeal occurred are consistent with an assumption of exogenous timing. Some repeals (e.g., Arizona) were imposed judicially (rather than legislatively), and therefore likely to be less dependent on contemporaneous economic conditions. Similarly, most legislative repeals (e.g., Alabama, Colorado, Idaho, Kansas, New Hampshire, Utah) only occurred after previous failed attempts; repeal efforts often started years before a change in law took place. Second, as discussed above, we found no systematic correlations between repeal and wages or union wage differentials of nonconstruction blue collar workers, suggesting that repeal was not correlated with other state-level policies that have adverse effects on unions or unionized workers.

Third, to investigate directly the possibility that time-varying state macroeconomic factors correlated with but not caused by repeal were responsible for differential trends in wage differentials, we parameterized the differential impact of state macroeconomic factors by reestimating equations (1) and (1a) with controls for the state unemployment rate and the state unemployment rate*construction and by reestimating equations (2) and (2a) with controls for unemployment, unemployment*black, unemployment*construction, and unemployment*black*construction (for those versions of equations (2) and (2a) that model the impact of law repeal on the union/nonunion wage differential, we included controls for unemployment, unemployment*union, unemployment*construction, and unemployment*union*construction). In no case did inclusion of these macroeconomic control variables alter substantially the estimated effects of law repeal.

We further investigated the hypothesis that the estimated effects of law repeal were caused by underlying macroeconomic labor market conditions that differentially affected the construction industry and were correlated with state labor market rules by reestimating the basic model with altered control groups. We estimated equations (1) and (1a) based on the census dropping from the analysis alternately all individuals from states that always had a prevailing wage law in effect during the sample period, and all individuals from states that never had a prevailing wage law in effect during the sample period. Although the point estimates of the effect of law repeal were somewhat larger in those models that used only individuals from states who never had a prevailing wage law as the control group, the substantive conclusions did not change.

IV. Conclusion

We find that state prevailing wage laws have small but significant average effects on construction labor markets. Repeal of prevailing wage laws leads to slight decreases in the relative wage levels of construction workers. However, the effects of repeal differ substantially across groups of construction workers. The negative effects of repeal on wages are borne primarily by union and by white workers. Although relative construction unionization rates do not decline significantly in response to repeal, the long-run union wage premium earned by construction workers decreases by approximately 10 percentage points, or almost half of the total union wage premium in construction. Since union members account for approximately 25 percent of all construction workers, the 10-percentage-point decrease in the union wage premium accounts for essentially all of the (approximately 2 to 4 percent) decline in construction workers' wages.

Second, despite the negative overall effects of repeal on construction workers, repeal of prevailing wage laws does not harm (or actually benefits) black construction workers. Prevailing wage law repeal raises black workers' construction wage differential relative to nonblack workers' differential. Furthermore, in no specification does law repeal significantly reduce the absolute level of black workers' construction wage differential, and in some specifications *increases* the level of black workers' construction wage differential, even as it reduces the construction differential for nonblack workers.

The policy implications of these findings, and their applicability to other states considering repeal of their prevailing wage laws, depend crucially on the mechanism causing the differential impact of repeal across groups. On one hand, the differential impact of repeal may reflect a decrease in discrimination due to a weakening of construction unions, or due to a change in unions' behavior arising out of a declining union wage premium. On the other hand, if repeal affects workers in heavy construction (SIC 16, e.g., road and sewer construction) more than workers in light construction (SIC 15 and 17, e.g., general and special trade contractors)[12], and heavy construction workers are more likely to be white and unionized, then the differential impact of repeal by race and union status may simply reflect the differential composition of workers in the two segments of the construction industry. In this case, law repeal might simply be transferring resources from workers to purchasers of heavy construction projects. Furthermore, to the extent that the composition of the construction industry differs between repeal states and other states considering repeal, our findings may not even accurately forecast the average effects of repeal in states considering repeal.

Unfortunately, since neither the CPS nor the census includes detailed industry or union information on construction workers, we cannot distinguish definitively between these two hypotheses. Indeed, in our supplementary analyses, we found no clear support for either causal mechanism. We found no evidence that repeal affected racial wage differentials through the unionization rate. The counterfactual of reducing the unionization rate to zero in both construction and nonconstruction industries would only reduce the magnitude of the raw difference between blacks' and nonblacks' construction wage differential by half. Compared to the contribution of unionization to racial wage differentials in the 1960s, the minimal contribution of unionization in the 1980s and 1990s is striking. Because law repeal has a relatively small impact on the unionization rate, law repeal must affect race-based wage differentials primarily through other means.

On the other hand, we found no evidence that repeal had different effects on different segments of the construction industry. Differential trends in payroll per worker from the ES202 establishment data presented in Table 2 suggest that repeal affects workers in heavy construction and light construction equally: the simple DDD estimates of the effect of repeal in the heavy construction industry are only 0.1 percent greater than the estimates of the effect of repeal in the light construction industry (-1.9 percent compared to -1.8 percent). Of course, because the establishment data do not allow us to control for worker heterogeneity, this finding is not a definitive rejection of differential impacts across 2-digit SIC construction industries. We leave further investigation of the mechanism by which law repeal affects wage schedules to future research. One possible avenue for investigation might be the extent to which construction union behavior or decision-making differs in repeal and nonrepeal states.

Table 1: Chronology of State Prevailing Wage Laws Through 1993

	Year Effective			Year Effective	
State	Enactment	Repeal	State	Enactment	Repeal
Alabama	1941	1980	Montana	1931	
Alaska	1931		Nebraska	1923	
Arizona	1912	1979	Nevada	1937	
Arkansas	1955		New Hampshire	1941	1985
California	1931		New Jersey	1913	
Colorado	1933	1985	New Mexico	1937	
Connecticut	1935		New York	1894	
Delaware	1962		North Carolina		
D.C.	1931		North Dakota		
Florida	1933	1979	Ohio	1931	
Georgia			Oklahoma	1909	
Hawaii	1955		Oregon	1959	
Idaho	1911	1985	Pennsylvania	1961	
Illinois	1931		Rhode Island	1935	
Indiana	1935		South Carolina		
Iowa			South Dakota		
Kansas	1891	1987	Tennessee	1953	
Kentucky	1940		Texas	1933	
Louisiana	1968	1988	Utah	1933	1981
Maine	1933		Vermont		
Maryland	1945		Virginia		
Massachusetts	1914		Washington	1945	
Michigan	1965		West Virginia	1933	
Minnesota	1973		Wisconsin	1931	
Mississippi			Wyoming	1967	
Missouri	1957				

Table 2: Wage Trends in Construction and Nonconstruction Industries, for States Repealing and Not Repealing Prevailing Wage Laws

	Experimental States: States that Repealed Prevailing Wage Laws			Nonexperimental States: States That Did Not Repeal Prevailing Wage Laws			difference
	Before Law Repeal (1)	After Law Repeal (2)	Time Diff. For Location (3)	Before Law Repeal (4)	After Law Repeal (5)	Time Diff. For Location (6)	in % change (7)
Treatment Group: Construction Workers							
Census:							
BC Hourly wage	9.654	7.960	-0.175	10.685	9.311	-0.129	-0.047
CPS:							
Hourly wage	8.932	7.302	-0.182	10.098	8.414	-0.167	-0.016
BC hourly wage	8.717	6.828	-0.217	9.955	7.997	-0.197	-0.020
ES202:							
payroll/employment							
All construction	19266	15981	-0.171	21546	18749	-0.130	-0.041
Light construction (SIC 15 and 17)	18318	15408	-0.159	20657	18229	-0.118	-0.041
Heavy construction (SIC 16)	22642	18632	-0.177	25468	22029	-0.135	-0.042
Control Group: Nonconstruction Workers							
Census:							
BC Hourly wage	7.875	6.838	-0.132	8.442	7.516	-0.110	-0.022
CPS:							
Hourly wage	7.439	7.099	-0.046	8.068	7.679	-0.048	0.003
BC hourly wage	6.690	5.588	-0.165	7.229	6.087	-0.158	-0.007
ES202:							
payroll/employment	15799	15432	-0.023	17297	17300	0.000	-0.023

Notes: ES-202 includes private nonagricultural firms; CPS and census include private-sector nonfarm wage and salary workers aged 16 to 64. For census, 1980 is “before” period; 1990 is “after” period. For CPS and ES202, 1979 is “before” period; 1993 is “after” period. Observations from MN omitted from census analysis because MN enacted a prevailing wage law in 1973. Observations from AK, DE, NY, and TX omitted from ES202 analysis because of suppression of 2-digit SIC construction industry data from those states. In census, nonconstruction workers are a 1% sample; 1970 construction workers are a 2% sample; 1990 construction workers are a 5% sample. CPS means calculated using CPS sampling weights. All dollar amounts in 1982 constant dollars.

Table 3: Effects of Prevailing Wage Law Repeal on Blue Collar Wages and the Construction Wage Premium

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Repeal state*after repeal*construction	-0.023 (0.013)	-0.039 (0.025)	-0.034 (0.013)	-0.039 (0.016)	-0.002 (0.013)		
Repeal state*after repeal	0.008 (0.017)	-0.001 (0.018)	-0.001 (0.017)	-0.012 (0.007)	-0.015 (0.007)		
Repeal state*shortly after repeal *construction						0.005 (0.015)	0.004 (0.014)
Repeal state*long after repeal *construction						-0.005 (0.014)	-0.008 (0.015)
Repeal state*shortly after repeal						-0.020 (0.009)	-0.021 (0.007)
Repeal state*long after repeal						-0.014 (0.007)	-0.010 (0.008)
Demographic controls?	No	Yes	Yes	Yes	Yes	Yes	Yes
Full set of state*construction interactions?	Yes	No	Yes	No	Yes	Yes	Yes
Data Set	census	census	census	CPS	CPS	CPS	CPS
N	1,468,033	1,468,033	1,468,033	1,017,875	1,017,875	1,017,875	1,017,875

Notes: Standard errors in parentheses are robust to heteroscedasticity and to within state/year group correlation. Census data from 1970, 1980, and 1990. CPS data from 1977-78 May surveys and 1979-93 MORGs. See text for full list of demographic and other controls. Relevant models allow returns to demographic characteristics to differ in 1970, 1980, and 1990 (census) and to differ before and on/after 1986 (CPS). Columns (2) and (5) control for repeal state*construction. In column (6), “shortly after repeal” is 1-2 years, “long after repeal” is 3 or more years; in column (7), “shortly after repeal” is 1-4 years, “long after repeal” is 5 or more years. Columns (1)-(3) using census data omit observations from MN because MN enacted a prevailing wage law in 1973. Observations weighted with sampling weights described in text.

Table 4: Effect of Prevailing Wage Law Repeal on the Construction Wage Premium by Race

Variable	(1)	(2)	(3)	(4)	(5)
Repeal state*after repeal*construction *black	0.043	0.041	0.055		
	(0.019)	(0.012)	(0.032)		
Repeal state*after repeal*construction	-0.040	-0.040	-0.013		
	(0.013)	(0.013)	(0.014)		
Repeal state*after repeal*black	0.003	-0.011	-0.036		
	(0.030)	(0.014)	(0.011)		
Repeal state*shortly after repeal *construction*black				0.051	0.037
				(0.034)	(0.031)
Repeal state*long after repeal *construction*black				0.056	0.068
				(0.034)	(0.036)
Repeal state*shortly after repeal *construction				-0.006	-0.006
				(0.016)	(0.015)
Repeal state*long after repeal *construction				-0.015	-0.020
				(0.015)	(0.016)
Repeal state*shortly after repeal *black				-0.040	-0.039
				(0.016)	(0.013)
Repeal state*long after repeal *black				-0.035	-0.034
				(0.012)	(0.012)
Full set of state*black interactions?	No	Yes	Yes	Yes	Yes
Data Set	census	census	CPS	CPS	CPS
N	1,468,033	1,468,033	1,017,875	1,017,875	1,017,875

Notes: Standard errors in parentheses are robust to heteroscedasticity and to within state/year group correlation. Census data from 1970, 1980, and 1990. CPS data from 1977-78 May surveys and 1979-93 MORGs. See text for full list of demographic and other controls. Relevant models allow returns to demographic characteristics to differ in 1970, 1980, and 1990 (census) and to differ before and on/after 1986 (CPS). Column (1) controls for repeal state*black and repeal state*black*construction; columns (2)-(5) include controls for state-fixed-effects*black and state-fixed-effects*black*construction. In column (4), “shortly after repeal” is 1-2 years, “long after repeal” is 3 or more years; in column (5), “shortly after repeal” is 1-4 years, “long after repeal” is 5 or more years. Columns (1) and (2) using census data omit observations from MN because MN enacted a prevailing wage law in 1973. Observations weighted with sampling weights described in text.

Table 5: Effect of Prevailing Wage Law Repeal on Unionization Rates and the Construction Wage Premium by Union Status

Variable	Dependent Variable					
	Union member	Union member	Union member	ln(wage)	ln(wage)	ln(wage)
	(1)	(2)	(3)	(4)	(5)	(6)
Repeal state*after repeal*construction *union member				-0.059 (0.026)		
Repeal state*after repeal *construction	0.003 (0.016)			-0.001 (0.019)		
Repeal state*after repeal*union				-0.015 (0.012)		
Repeal state*after repeal	0.019 (0.005)			-0.007 (0.008)		
Repeal state*shortly after repeal *construction*union member					0.051 (0.035)	-0.006 (0.030)
Repeal state*long after repeal *construction*union member					-0.098 (0.026)	-0.112 (0.028)
Repeal state*shortly after repeal *construction		0.029 (0.028)	0.026 (0.019)		-0.026 (0.026)	-0.012 (0.019)
Repeal state*long after repeal *construction		-0.004 (0.017)	-0.015 (0.018)		0.006 (0.020)	0.007 (0.021)
Repeal state*shortly after repeal *union					-0.043 (0.026)	-0.020 (0.018)
Repeal state*long after repeal *union					-0.006 (0.013)	-0.011 (0.014)
Repeal state*shortly after repeal		0.015 (0.008)	0.013 (0.006)		-0.004 (0.011)	-0.009 (0.009)
Repeal state*long after repeal		0.020 (0.005)	0.024 (0.006)		-0.008 (0.009)	-0.006 (0.009)
N	754,609	754,609	754,609	754,609	754,609	754,609

Notes: Standard errors in parentheses are robust to heteroscedasticity and to within state/year group correlation. Data from 1977-81 May CPS surveys and 1983-93 CPS MORGs; data on unionization is unavailable in 1982. All columns control for state- and time-fixed-effects, state-fixed-effects*construction, time-fixed-effects*construction, and for demographic characteristics (see text) allowing returns-to-characteristics to differ before and on/after 1986. Columns (4)-(6) also control for state-fixed-effects*union member, time-fixed-effects*union, state-fixed-effects*union*construction, and time-fixed-effects*union*construction. In columns (2) and (5), “shortly after repeal” is 1-2 years, “long after repeal” is 3 or more years; in columns (3) and (6), “shortly after repeal” is 1-4 years, “long after repeal” is 5 or more years. Observations weighted with sampling weights described in text.

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Endnotes

1.40 U.S.C. Sec. 276(a) (1982).

2.Before 1985, the prevailing wage was equal to that paid to more than 30 percent of the workers in a given industry/occupation/area group, or the average if less than 30 percent of the workers earned the same amount.

3.In this and all of our subsequent analysis, we deleted individuals reporting wages of less than $1.65 or greater than $50 per hour in 1982 constant dollars.

4.We assume in all of the models that law repeals take effect beginning in the calendar year after the repeal is adopted.

5.We estimate all models allowing for heteroscedasticity and for within state/time-group correlation in $å_{ist}$, which may be important in models of state policy effects (Moulton 1990).

6.Prior to 1989, workers in the CPS earning more than $999 per week were topcoded at $999. We recoded these workers to earning $1,400 per week.

7.If the unadjusted CPS sampling weight for individual i in state s during year t is defined as $Ù_{ist}$ (e.g., the number of individuals represented by individual i), analyses of the May/MORG sample weight each observation by its adjusted sampling weight $ù_{ist}$, where

$$ù_{ist} \text{ ' } \frac{Ù_{ist}}{\mathbf{j}_{i,s} Ù_{ist}}.$$

8.The impact of repeal on the change over time in the census in wage *growth rates* (e.g., 1980-90 wage growth relative to 1970-80 wage growth in repealing versus nonrepealing states) is small and statistically insignificant, suggesting some caution in a strong causal interpretation of our findings; state repeals may be correlated with preexisting factors affect construction labor markets.

9.Calculations based on the 1990 census.

10.Interpretation of the long-run rather than the short-run effects as the equilibrium impact of law report depends on two key assumptions: 1) that the full effects of repeal on labor markets take more than two years to appear and 2) no other changes in the labor market or legal environment correlated with but not caused by repeal occurred three to five years after repeal. On one hand, to the extent that wage levels, and especially wages differences across types of workers, take several years to re-equilibrate in response to changes in state policy, the long-run effects would accurate represent the equilibrium impact of law reform. On the other hand, to the extent that union contracts in the construction industry are shorter than two years in duration, the short-run effects would accurately represent the equilibrium impact of repeal, with fewer potential unobserved confounding factors.

11. The minimal contribution of the unionization rate to racial wage differentials in construction in the 1980s and 1990s is particularly striking when compared either to its contribution in 1967, or to the total racial wage differential. According to Ashenfelter (1972), the estimated contribution of unionization to racial differences in construction wage premiums was 8.9 percentage points in 1967. According to calculations based on the 1990 census, the total racial wage differential in the construction industry was 17.6 percent.

12.This would be true if, for example, (prevailing-wage-law covered) state and local construction projects involve more heavy than light construction, and light construction workers are poor substitutes for heavy construction workers.