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The Effect of Prevailing Wage Law Repeals and Enactments on Injuries and Disabilities in the Construction Industry

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Abstract

State prevailing wage law repeals have been shown to lower wages and benefits—including benefits providing safety training and associated with worker retention in construction. This study tests whether prevailing wage repeals affect construction injury rates and/or the prevalence of disabilities among construction workers. Controlling for time trends in injuries and disabilities, differences between construction industry subsectors, the business cycle, and time-invariant differences between states, we find that repealing state prevailing wage laws increase construction injury rates across various types of injuries from 11.6% to 13.1% as the seriousness of injuries increases. Disabilities increase by 7.5% to 8.2% depending on the model specification. Conjoining an analysis of the effects of prevailing wage law repeals on injury rates with disability rates in construction provides alternative measures of the effects of prevailing wage laws on construction workplace safety, which addresses a well-known problem of underreporting construction injuries.

Keywords

prevailing wage, repeal, injuries, disabilities, construction

Introduction

Prevailing wage laws regulate the payment of wages and benefits on public works. Prevailing wage regulations set wages and, in most cases, benefits by blue-collar

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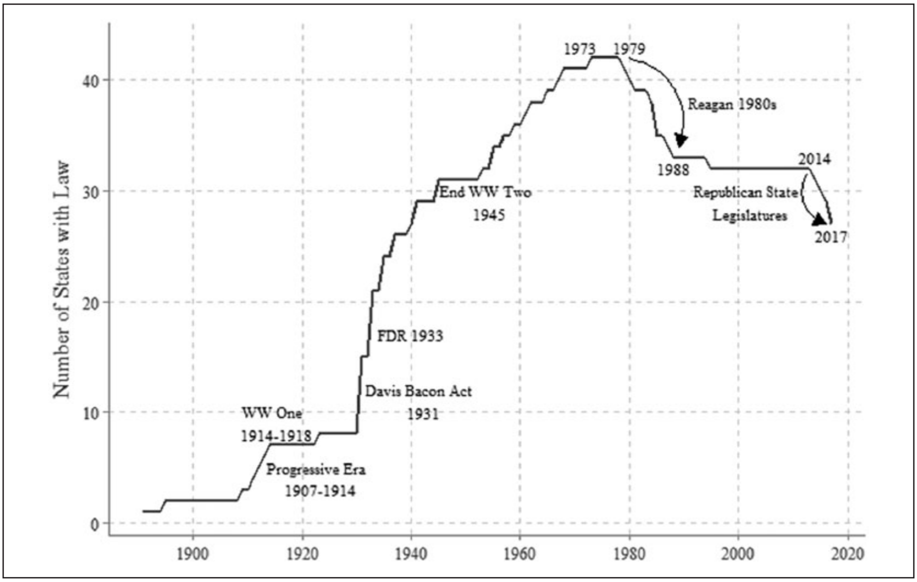

Figure 1. Number of states with prevailing wage laws, 1891 to 2017.
Note. WW = World War; FDR = Franklin D. Roosevelt.

construction craft for journey workers and apprentices. Federal projects are governed by the Davis Bacon Act (1931), while currently 27 states plus the District of Columbia have prevailing wage laws regulating state and local government projects (U.S. Department of Labor, 2018). The first state law was enacted in Kansas in 1891; eight states had adopted prevailing wage laws prior to the Great Depression. By 1945, 30 states and the District of Columbia had prevailing wage laws, and at the peak in 1973, all but nine states primarily in the South and upper plains states had prevailing wage laws (Figure 1). Starting in 1979 and through the Reagan presidency, nine states repealed their prevailing wage laws. Oklahoma's Supreme Court invalidated that state's law in 1995 while Vermont enacted a prevailing wage law in 1998 (Philips, Mangum, Waitzman, & Yeagle, 1995). Since the increased Republican control of various state legislatures after 2010, five states have repealed their prevailing wage laws. These legal changes have kept prevailing wage laws at the forefront of public works construction policy debates for several decades.[1]

Research has shown that the repeal of state prevailing wage laws leads to lower construction wages in those repeal states (Belman & Voos, 1995; Clark, 2005; Harris, Mukhopadhyay, & Wiseman, 2017; Kelsay, 2015; Kessler & Katz, 2001; Manzo, Bruno, & Littlehale, 2014; Petersen, 2000; Philips, 1998; Philips et al., 1995; Price, 2005) and reduces voluntary benefits (Fenn, Li, Pleites, Zorigtbaatar, & Philips, 2018). Voluntary benefits comprise employer-provided health insurance, employer contributions to pensions, holiday/vacation pay, and other benefits including collectively bargained contributions to apprenticeship training (Bilginsoy, 2003), OSHA10, OSHA30, and other training programs.[2]

But, the loss of voluntary benefits can also lead to a loss of industry-specific experience in construction. Previous research (Andrietti & Hildebrand, 2016; Dorsey, 1995; Gustman & Steinmeier, 1993; Kim & Philips, 2010) has shown that decreases in employer-provided health insurance, and a fortiori, portable, union/multi-employer -provided portable health insurance, decreases worker attachment to the construction industry with a consequent loss of construction industry–specific worker experience. Apprenticeship training, safety training, and the accumulation of industry-specific human capital through work experience are channels through which construction worksites become safer (X. Dong, Entzel, Men, Chowdhury, & Schneider, 2004; Sokas, Jorgensen, Nickels, Gao, & Gittleman, 2009).

The potential loss of safety training associated with the repeal of prevailing wage laws has led others to propose a link between prevailing wage repeals and construction injury rates (Belman & Voos, 1995; Kelsay, 2015; Philips, 1998; Philips et al., 1995). Azari-Rad (2005) was the first study to systematically estimate this potential effect finding that the presence of prevailing wage laws reduces total construction injuries by 8.25%. We extend Azari-Rad's 1976 to 1999 data by 17 years finding comparable effects on injuries from repealing prevailing wage laws. In addition, this study is the first to estimate a link between prevailing wage law repeals and the prevalence of disabilities in construction.

Data

Injuries

The Bureau of Labor Statistics (BLS) provides annual injury rate data for construction by industry subsector, state, and year over the s 1976 to 1995 in mimeo and 1996 to 2016 digitally (U.S. BLS, 2018). We have selected seven construction subindustries that have remained definitionally similar when the BLS switched from Standard Industry Codes (SIC) to the North American Industry Code System (NAICS) in 2003. Our subindustries include residential building construction (NAICS 2361), masonry contractors (NAICS 23814), roofing contractors (NAICS 23816), electrical contractors (NAICS 23821), plumbing and HVAC contractors (NAICS 23822), painting and wall covering contractors (NAICS 23832), and carpentry contractors (NAICS 23835).

For each of these industries, the BLS reports a total injury rate and component rates based on increasing injury severity. We analyze (a) total injury rates; (b) injuries that resulted in no lost work; (c) days away, restricted or job transfer (DART); and (d) injuries resulting in lost work-days when an injury compelled the worker to be absent from work for one or more days (a subset of DART injuries).

Injury rates in construction tend to be counter cyclical (X. S. Dong, Wang, & Herleikson, 2010). As the economy turns down and contractors shed workers, contractors tend to retain their more valued employees who, overall, are more experienced, better trained, and less likely to become injured. Furthermore, as work becomes scarce and the press to finish projects relaxes, the pace of work slows making construction worksites safer. In addition, during periods of high unemployment, workers

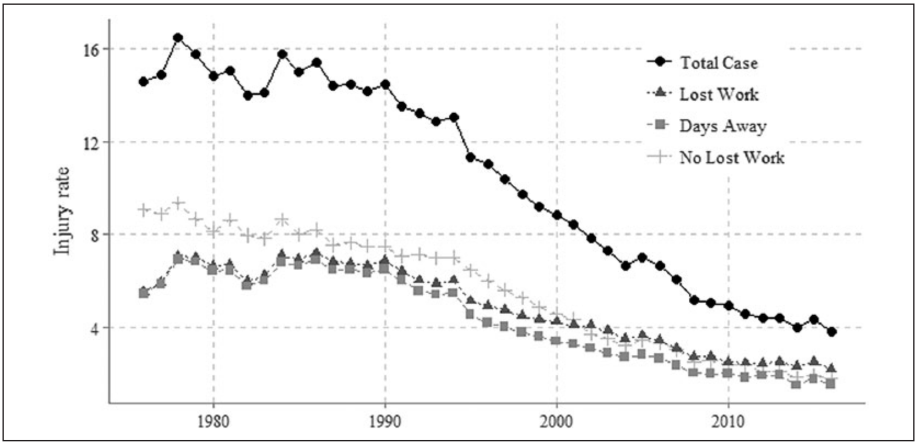

Figure 2. Trends in injury rates, 1976 to 2016.

are sometimes reluctant to report injuries that might risk dismissal. To control for these effects in our regression models, we use the state unemployment rate to capture the construction business cycle.

It is well documented that overall, in the United States, workplace injuries have declined over time (U.S. BLS, 2017) subsequent to the enactment of OSHA regulations and coincident advances in knowledge regarding workplace safety technologies and management (Conway & Svenson, 1998; Friedman & Forst, 2007). This is also true in construction (National Research Council, 2009). Figure 2 shows the long-term decline in construction injuries by injury type. Much of this decline in reported injuries is real, associated with the establishment of OSHA in 1970 and the rise in medical costs since the early 1970 driving up worker compensation premiums and incentivizing safety (Lengagne, 2016). However, some of this decline may be due to injury costs incentivizing the underreporting of workplace injuries.

Research shows that injuries are substantially underreported in construction (X. S. Dong et al., 2011). Workers may fail to report their injuries because they perceive the injury to be minor, an expected outcome of construction work, or the worker may fear of the consequences that may ensue from reporting an injury (Taylor Moore, Cigularov, Sampson, Rosecrance, & Chen, 2013). Reasons employers fail to report a workplace injury include a range of record keeping problems and failures, the view that the injury was not work related, and/or a willful failure to report (Rappin, Wuellner, & Bonauto, 2016). A study of union carpenter apprentices found that safety programs that incentivize safety and/or penalize injury incidences result in workers underreporting their injuries (Lipscomb, Nolan, Patterson, Sticca, & Myers, 2013). State variation in the generosity of worker compensation insurance programs may affect the behavior of both workers and employers. Longer waiting periods before receiving worker compensation benefits may discourage, and better benefits may encourage, workers in reporting injuries. Higher worker compensation premiums may incentivize employers to suppress injury reporting, but higher premiums may also encourage safety innovations at the jobsite (Mendeloff & Burns, 2013). Thus, there is both noise and a downward bias in the measurement of workplace injuries.

However, for our purposes, once controlling for the overall downward trend in reported injuries, these reporting biases may not be problematic if the underreporting of injuries is not correlated with the passage or repeal of prevailing wage laws. In this case, underreporting simply adds noise to the data that may deter finding underlying statistically significant relationships.

It is possible that injury underreporting is correlated with an enactment or repeal of prevailing wage laws. It may be that repealing prevailing wage laws encourages injury underreporting by reducing the prevalence of collective bargaining and worker contractual protections. Or it may be that repealing prevailing wage laws by lowering wages reduces the costs of worker compensation and reduces contractor incentives to underreport. One of the advantages of examining the relationship between self-reported disabilities and prevailing wage repeals is that this provides an alternative test of the effects of prevailing wages on construction worksite safety relatively independent from the BLS injury-underreporting puzzle.

Disabilities

The U.S. Census provides decennial self-reported responses to questions regarding disability from 1970 to 2000 and annual disability data from the American Communities Survey thereafter (Ruggles, Genadek, Goeken, Grover, & Sobek, 2017). The disability question changes over time with the primary survey change occurring in 2000. This basic change was an increase in the types of disabilities the survey suggested that the individual might have. This raises the possibility that more disabilities might be reported due to more possibilities being identified in the survey question. Therefore, in our regression analysis, we enter a dummy variable equaling one for all years from 2000 onward. While our injury data are annual, our disability data are decennial until 2000. So, for the sake of comparability, we limit our post-2000 disability data to the years 2010 and 2016.

Workers in the construction industry that report a disability need not have acquired that disability while working construction. However, because construction is a physically demanding industry, it is unlikely that workers who have become disabled elsewhere are subsequently attracted to construction work. Presumably, most disabled construction workers became disabled while participating in the construction industry, but their disability might not have come from the construction work itself.

We calculate a disability rate by counting within each state for each year the number of construction workers reporting any kind of disability and dividing by the number of persons in the construction industry (in units of 100 workers) for that state and year. This calculation differs from the BLS injury rate which is injuries per 100 full-time equivalent workers—that is, per 100 units of 2,080 hr worked. So our injury rate is benchmarked against hours worked while our disability rate is benchmarked against a headcount.

The same channels that link injuries to the business cycle may also link unemployment to reported disabilities (Autor & Duggan, 2006). Fewer injuries in a year of high unemployment may mean fewer disabilities occurring during that year. However, in contrast to injuries which are an occurrence within a year, disabilities in our data are a health status that may last for multiple years and not first occur in the census year in which the disability is reported. Furthermore, high rates of unemployment may incentivize workers to claim disability benefits. So, the relationship between unemployment and reported disabilities may be either positive or negative. And the connection between past disabilities and the contemporary unemployment rate may be attenuated leading to no statistically significant relationship. Consequently, we provide two disability models—one including the state unemployment rate (U.S. BLS) and one excluding unemployment.

Self-reported disabilities to the U.S. Census Bureau are not caught up in the web of incentives associated with worker compensation premia and worksite safety programs. Thus, some of the puzzles associated with interpreting BLS injury rate data are less present in the interpretation of disability data. It is possible that more generous worker compensation programs may encourage more worker disability claims (Meyer, Viscusi, & Durbin, 1995). So there remains the possibility that the repeals of prevailing wage laws are positively correlated with increasing the generosity of worker compensation benefits leading to an artificial increase in disabilities. However, the politics of prevailing wage repeals are not typically associated with a movement to improve worker compensation benefits making this potential measurement problem less likely. For instance, Indiana, which repealed its prevailing wage law in 2015, implemented worker compensation cost containment measures in 2014 (Jones, 2014). According to ProPublica, of the seven states that repealed their prevailing wage laws between 2013 and 2018 (Arkansas, Indiana, Kentucky, Michigan, Tennessee—building construction but not roads, West Virginia, Wisconsin), five had lower worker compensation benefits in 2014 relative to the previous decade, one remained the same and one raised their benefits. There is little evidence that prevailing wage repeals go hand in hand with improved worker compensation benefits. Thus, a test of the relationship between prevailing wage repeals and the prevalence of disabilities in construction probably provides results that are independent of concerns regarding reporting error (Qui & Grabell, 2017; U.S. Department of Labor, 2018).

Assuming a linkage between workplace injuries and disabilities, and assuming that the decline in reported construction injuries is real and not simply an artifact of incentives to underreport injuries, then we would expect a consequent decline in self-reported disabilities among construction workers. We therefore enter a time trend into both our injury and disability models hypothesizing that, all other things being equal, if actual injury rates trend downward in construction, then disabilities will follow suit.

State Prevailing Wage Regulations

Between 1970 and 2016, there have been 13 state prevailing wage law repeals, nine between 1979 and 1988—primarily in Southern, Plains, and Mountain states—and four from 1995 to 2016 primarily in the upper South and Midwest. There have been

Table 1. Descriptive Statistics.

Injury type/disability	Total injury	No lost work	Lost work	Days away	Disability
Cases/100 people	10.79 (5.86)	5.94 (3.59)	5.05 (2.92)	4.58 (2.93)	7.29 (2.28)
Repeal	0.14	0.14	0.14	0.14	0.15
Enact	0.004	0.004	0.004	0.004	0.026
Unemployment rate	6.11 (2.03)	6.09 (2.03)	6.11 (2.03)	6.1 (2.02)	5.92 (2.22)
n	5,920	5,683	5,914	5,784	306

Note. No state-level unemployment rate for 1970 reduces the disability sample when unemployment is included.

two enactments: Minnesota in 1973 and Vermont in 1998 (Philips et al., 1995; U.S. Department of Labor, 2018). Two states repealed their laws in 2017 (Wisconsin and Kentucky) and 2018 (Michigan), outside our period of analysis.

Descriptive Statistics

Table 1 shows injuries are lower as the measure of injury severity increases. The disability rate is not directly comparable to the injury rates because the measure of exposure to possible injuries is employment and not hours worked. While 14% of the injury observations entail repeals, only 0.4% involve enactments; in the case of disabilities that are based on decennial data, 15% of observations entail repeals and 2.6% enactments. The average unemployment rate is about 6.1% but varies slightly with the varying sample sizes for different injury types. The injury sample sizes vary slightly with different measures of severity due to some states and industry subsectors reporting only aggregate injuries. The sample for disabilities is about 10% of the size of the injury samples because the disability data are decennial and for the entire construction industry while the annual injury data include construction subsectors.

Figure 3 illustrates our test strategy by showing the average total injury rates for electrical and plumbing contractors in Kansas from 1976 to 1986 prior to the repeal of Kansas's prevailing wage law in 1987 comparing it to the average total injury rates from 1987 to 2016 after Kansas repealed its law. Figure 3 benchmarks this change by showing the average total injury rates for the adjacent states of Iowa (which never had a prevailing wage law) and Missouri (which during our time period always had a prevailing wage law). While these two states did not change their policies over our entire period of analysis, we nonetheless break their average injury rates into the time before and after Kansas repealed its law. In all three states for both electricians and plumbers, total injury rates fell, but the fall in Kansas was smaller than in either of the two states that maintained their prevailing wage policy during this period. This suggests that repealing Kansas's prevailing wage law slowed the decline in their total injury rate compared with adjoining states. Our injury models test whether this pattern for three states is generalizable across all states controlling

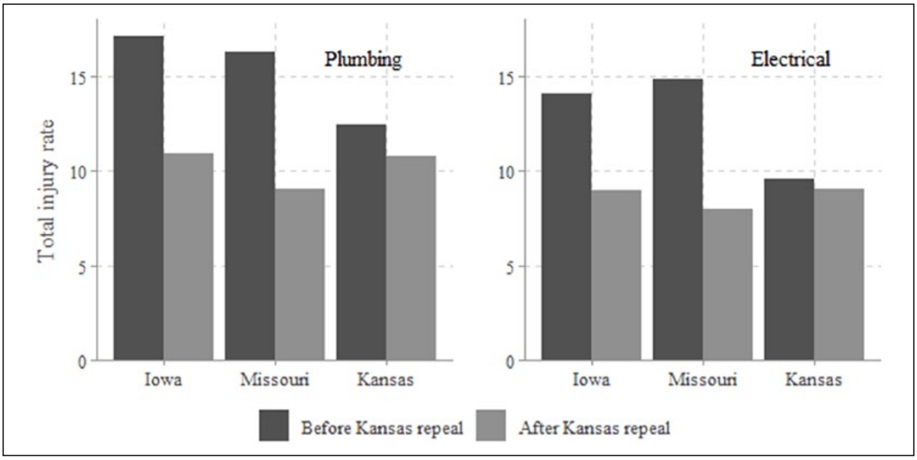

Figure 3. Average total injury rate for electricians and plumbers before and after Kansas repealed its prevailing wage law in 1987.

Note. This graph compares average total injury rate for plumbing and electrical contractors in Iowa, Missouri, and Kansas before and after Kansas repealed the law. Kansas repealed the law in the year 1987.

for the unemployment rate, differences between construction industry subsectors, and the general decline in reported injuries. In our models, we select three samples: all states, repeal and always-had-law states, and repeal and never-had-law states. This is analogous in Table 3 to comparing Kansas to both states, then Kansas to Missouri, then Kansas to Iowa.

In a similar manner, Figure 4 looks at total disability rates before and after repeal. Because the disability data are decennial, our example test comprises the six states that repealed their prevailing wage laws after 1980 and before 1990. For these states (Arizona, Colorado, Idaho, Kansas, New Hampshire, and Utah), we report average disability rates before repeals (1970 and 1980) and after repeals (decennially 1990 to 2010 plus 2016). For comparison, we present the average disability rates for those states that never had a prevailing wage law throughout our period and states that always had a prevailing wage law over our period. In Figure 4, we exclude states that repealed their law prior to 1981 or after 1989. While disability rates fell for states with unchanging legal regimes, disability rates for construction workers in states that repealed their prevailing wage laws rose. Our disability model tests whether this pattern holds once all states are included and time trends and state unemployment rates are controlled for.

Model and Results

Our data are a long panel data set. Following Fenn et al. (2018), we use Feasible Generalized Least Squares (FGLS) estimation which allows the use of first-order autoregressive processes and permits the error terms in the model to be heteroskedastic

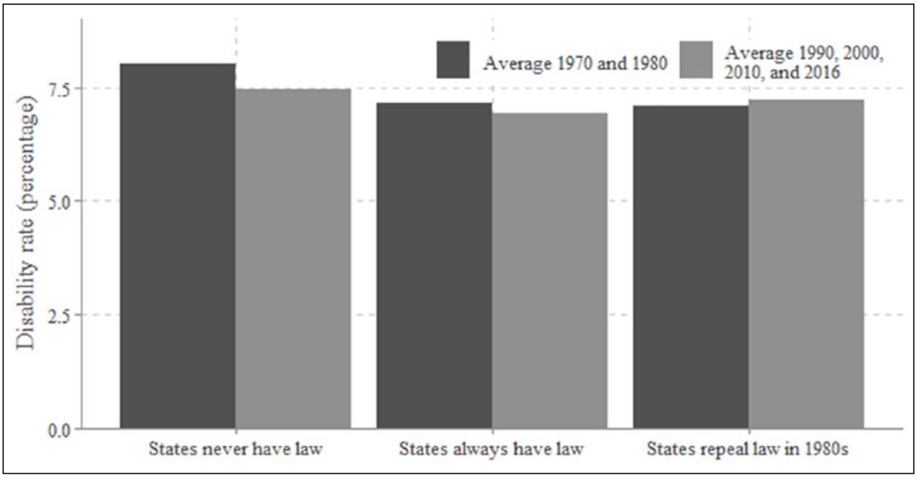

Figure 4. Average disability rates for states that always had, never had, and repealed their prevailing wage laws before and after the repeals.
Note. This graph compares the average disability rate of three groups of states. The three groups include states who never have the law during the whole period, states who always have the law during the whole period, and the states who repeal the law in the 1980s, which include Colorado, Idaho, Kansas, Louisiana, New Hampshire, and Utah.

(Cameron & Trivedi, 2010). More specifically, this method produces panel and cross-section corrected standard errors specifying the error terms to be independent with a variance of $E\left(u_{it}^2\right) = \sigma_i^2$ that can be different for each state over time. As error terms are possibly correlated and observations could very well depend on previous periods in a longitudinal data, the use of FGLS estimations with heteroskedastic disturbances will address these distributional issues of cross-sectional correlations and variances as well as time series autocorrelations.

We estimate the effect of prevailing wage law repeals on injury rates by state and year for seven construction industry subsectors, the prevalence of disabilities among all construction employees in the construction industry by state and year controlling for long-term trends in these health measures, differences among states using state dummy variables, long-term trends in injuries and disabilities using a year trend, an indicator variable marking when the Census expanded its list of disability questions, and a dummy variable indicating if and when a state repealed its prevailing wage law.

The equation for the FGLS is as follows:

$$Y_{it} = \alpha + B_1 X_{it}^1 + \ldots + B_k X_{it}^k + u_{it},$$

where Y_{it} is the injury or disability rate in a construction subindustry in the case of injuries and overall construction in the case of disabilities in a state i and year t; $X_{it}^1 =$ 1 in a state i and year t if that state i had currently or previously repealed its prevailing wage law, otherwise zero; $X_{it}^2 = 1$ in a state and year if state i had currently or previously enacted a prevailing wage law, otherwise zero; X_{it}^3 = state i overall unemployment rate in year t; $X_{it}^4 = 1$ in the disability model for all states and years after 1999 when the Census expanded its list of disability questions; X_{it}^5 = indicator of year t; and u_{it} is an error term for state i in time t.

Following the literature on the effect of prevailing wage repeals on blue-collar construction worker income, we include in all our models, but do not report, time-invariant state dummy variables to capture relevant differences in state construction industries that are unchanging across the 1972 to 2016 period of analysis.

Results

Repeals. Table 2 shows the results for our models predicting injuries and disabilities tested against our full sample of years and states. Table 3 shows the same models but limits the sample to states that kept their prevailing wage laws and states that repealed their laws. Table 4 shows the injury models tested against a sample of states that never had the law and repeal states. Table 4 omits the disability models because in this subsample, the disability observations are too few.

All the injury models include a time trend, dummy variables for industry subsectors, unreported state dummy variables and the state unemployment rates. The disability models are for construction as a whole with no subindustries, have unreported state dummy variables, include a time trend, and, in one model, include and, in one model, exclude state unemployment rates. When included, across all models, the state unemployment rates are always negatively related to injuries and disabilities. These results are statistically significant and substantial. A doubling of the unemployment rate leads to a roughly 25% decline in injury and disability rates. Both injury and disability rates decline over time at rates of 2% to 5% per year.

The time trends in the injury models are all negative and statistically significant, as one would expect from examining Figure 2. However, if all the decline in reported injuries in Figure 2 were artifacts of increased incentives overtime to underreport injuries (say due to rising worker compensation costs), then there would be no expectation that construction worker disabilities would trend downward over time. In both the disability models, we, in fact, find statistically significant negative time trends in reported disabilities. These downward trends of about 2% per year are roughly similar to the 4% per year decline in reported injury rates. This suggests that despite well-known underreporting issues and moral hazard incentives in injury rates, the downward trend in construction injuries over decades is real and not an artifact of reporting problems.

Table 5 provides a summary of results for injury rates while comparing our results to Azari-Rad[3] (Azari-Rad, 2005). In all of our samples, the increase of injury rates associated with prevailing wage repeals rises with the severity of the injury measure. Azari-Rad finds this as well suggesting that repeals increase the overall danger of construction work as well as increasing injury rates.

Table 2. FGLS Regression Models Predicting Injury and Disability Rates (Full Sample).

	(1)	(2)	(3)	(4)	(5)	(6)
	Total injury	No lost work related injury	Lost work related injury	Injury results in absence from work	Disability	Disability
Repeal	0.102***	0.110***	0.123***	0.123***	0.0722***	0.0789***
	(3.04)	(2.80)	(3.31)	(3.06)	(2.82)	(4.18)
Enact	−0.163	−0.0983	−0.113	−0.303**	0.0841	−0.0267
	(−1.48)	(−0.70)	(−0.95)	(−2.18)	(0.95)	(−0.38)
Year	−0.0413***	−0.0486***	−0.0339***	−0.0442***	−0.0259***	−0.0226***
	(−71.64)	(−69.58)	(−55.17)	(−63.10)	(−27.80)	(−27.00)
Unemployment	−0.250***	−0.266***	−0.246***	−0.241***		−0.229***
	(−13.33)	(−11.58)	(−11.72)	(−10.42)		(−13.69)
Masonry	0.279***	0.260***	0.347***	0.353***		
	(12.26)	(9.88)	(14.37)	(13.42)		
Roofing	0.521***	0.442***	0.600***	0.606***		
	(20.81)	(14.53)	(22.71)	(21.60)		
Electrical	0.106***	0.299***	−0.117***	−0.164***		
	(5.13)	(12.44)	(−5.20)	(−6.50)		
Plumbing	0.338***	0.526***	0.151***	0.116***		
	(17.40)	(22.94)	(7.31)	(5.10)		
Painting and wall	−0.158***	−0.174***	−0.135***	−0.0624		
	(−4.55)	(−4.32)	(−3.63)	(−1.62)		
Finish carpentry	0.309***	0.333***	0.314***	0.355***		
	(9.75)	(9.43)	(9.71)	(10.04)		
Data change					0.897***	0.823***
					(30.96)	(38.05)
State dummies	Yes	Yes	Yes	Yes	Yes	Yes
Constant	84.73***	98.49***	69.29***	89.61***	53.18***	47.14***
	(73.56)	(70.71)	(56.41)	(64.02)	(28.94)	(28.54)
n	5,901	5,663	5,895	5,763	306	255
Wald χ^2	8,391	7,399	5,564	6,528	1,622	3,089
$p > \chi^2$	0	0	0	0	0	0

Note. t statistics in parentheses. In this set of regression, we include all states. All the dependent variables and unemployment rate are the logged value. Injury data are annual from 1976 to 2016. Disability data are 1970, 1980, 1990, 2000, 2010, and 2016, but in regression (6), because of the absence of state-level unemployment rates, 1970 is omitted. Minnesota enacted the law in 1973, so in regression (6), this state does not have an enact effect. Florida repealed the law in 1979, so in regression (6), this state does not have a repeal effect. Disability measures the number of persons who report having any kind of disability among 100 persons in the construction industry. Data change refers to 2000 change in the Census American Community Survey disability questions. The omitted occupation is workers employed by residential building contractors. FGLS = Feasible Generalized Least Squares.
*$p < .1$. **$p < .05$. ***$p < .01$.

Results for our full sample and the sample that excludes never-had states are similar to each other and modestly higher than Azari-Rad's estimated effects. However, our sample that excludes always-had states using never-had states as the lone benchmark yields substantially higher repeal effects on injuries.

This is so for two related reasons. In comparison with the sample with always-had-law states as the benchmark (Table 3), the results for all injury rate models using the sample with never-had states as the benchmark (Table 4) systematically have larger constants and more steeply declining injury rate time trends. This means that the never-had states begin

Table 3. FGLS Regression Models Predicting Injury and Disability Rates (Sample Excludes States That Never Had the Law).

	(1) Total injury	(2) No lost work related injury	(3) Lost work related injury	(4) Injury results in absence from work	(5) Disability	(6) Disability
Repeal	0.0926***	0.0939**	0.120***	0.118***	0.0659**	0.0747***
	(2.76)	(2.37)	(3.21)	(2.92)	(2.50)	(3.91)
Year	−0.0406***	−0.0472***	−0.0339***	−0.0440***	−0.0251***	−0.0221***
	(−65.51)	(−62.39)	(−50.93)	(−57.51)	(−23.78)	(−23.81)
Unemployment	−0.258***	−0.275***	−0.266***	−0.253***		−0.232***
	(−12.75)	(−11.03)	(−11.65)	(−9.99)		(−11.85)
Masonry	0.278***	0.269***	0.352***	0.364***		
	(11.43)	(9.60)	(13.57)	(12.90)		
Roofing	0.523***	0.429***	0.618***	0.615***		
	(19.37)	(12.87)	(21.56)	(19.91)		
Electrical	0.0706***	0.275***	−0.150***	−0.195***		
	(3.19)	(10.67)	(−6.16)	(−7.15)		
Plumbing	0.308***	0.495***	0.131***	0.101***		
	(14.81)	(20.07)	(5.84)	(4.05)		
Painting and wall	−0.163***	−0.173***	−0.140***	−0.0592		
	(−4.43)	(−4.11)	(−3.48)	(−1.44)		
Finish carpentry	0.315***	0.341***	0.330***	0.382***		
	(9.19)	(8.76)	(9.20)	(9.59)		
Data change					0.878***	0.813***
					(26.95)	(34.44)
State dummies	Yes	Yes	Yes	Yes	Yes	Yes
Constant	83.24***	95.85***	69.30***	89.18***	51.71***	46.20***
	(67.31)	(63.43)	(52.08)	(58.35)	(24.79)	(25.19)
n	4,945	4,736	4,944	4,828	246	205
Wald χ^2	7,957	6,266	4,856	5,512	1,282	2,505
$p > \chi^2$	0	0	0	0	0	0

Note. t statistics in parentheses. In this set of models, the states which repealed their law during the regression period and the states that always have the law are included. For model (6), there is no repeal effect for Florida because it repealed its law in 1979 (see also notes in Table 2). FGLS = Feasible Generalized Least Squares.
$^{*}p < .1$. $^{**}p < .05$. $^{***}p < .01$.

with higher injury rates but these are declining faster than the always-had states which begin with lower injury rates, but these rates decline more slowly. Thus, the repeal effect on injuries appears sharper when measured against the set of never-had states experiencing faster declines from higher initial rate levels.

Underreporting could confound our injury results if the repeal of prevailing wage laws discourages under-reporting. However, to the extent that repealing prevailing wage laws reduces the practice of collective bargaining, repeals are likely to lead to greater underreporting of injuries. The loss of union protections is likely to discourage workers from reporting injuries while encouraging contractors to ignore or informally

Table 4. FGLS Regression Models Predicting Injury and Disability Rates (Sample Excludes States That Always Had the Law).

	(1) Total injury	(2) No lost work related injury	(3) Lost work related injury	(4) Injury results in absence from work
Repeal	0.158***	0.194***	0.169***	0.194***
	(4.49)	(4.70)	(4.37)	(4.63)
Year	−0.0449***	−0.0543***	−0.0362***	−0.0486***
	(−46.49)	(−47.65)	(−35.37)	(−41.29)
Unemployment	−0.174***	−0.184***	−0.156***	−0.144***
	(−6.02)	(−5.30)	(−4.81)	(−4.04)
Masonry	0.317***	0.292***	0.384***	0.364***
	(8.98)	(7.19)	(10.42)	(8.72)
Roofing	0.548***	0.505***	0.647***	0.664***
	(14.96)	(11.43)	(17.06)	(16.47)
Electrical	0.232***	0.410***	0.0207	−0.0781*
	(7.09)	(10.86)	(0.59)	(−1.95)
Plumbing	0.419***	0.607***	0.250***	0.196***
	(13.34)	(16.63)	(7.91)	(5.49)
Painting and wall	−0.0816	−0.0903	−0.0338	0.0228
	(−1.62)	(−1.61)	(−0.60)	(0.38)
Finish carpentry	0.384***	0.349***	0.426***	0.447***
	(7.64)	(6.35)	(8.58)	(8.29)
State dummies	Yes	Yes	Yes	Yes
Constant	91.56***	109.6***	73.40***	98.10***
	(47.62)	(48.34)	(36.05)	(41.81)
n	2,358	2,285	2,357	2,303
Wald χ^2	3,548	3,493	2,316	2,838
$p > \chi^2$	0	0	0	0

Note. t statistics in parentheses. In this set of regressions, only the states which repealed the law during the regression period and the states that never had the law are included. Disability regressions excluded due to limited sample size. (see also notes in Table 2). FGLS = Feasible Generalized Least Squares.
$^{*}p < .1$. $^{**}p < .05$. $^{***}p < .01$.

Table 5. Summary Injury Results and Comparison With Azari-Rad.

	Total	No lost work	Total lost work	Days away from work
Total sample	10.7	11.6	13.1	13.1
Excludes never-had states	9.7	9.8	12.7	12.5
Excludes always-had states	17.1	21.4	18.4	21.4
Azari-Rad (1976 to 1999 data)	8.3	7.1	9.8	10.2

treat injuries workers do report. Thus, if repeals reduce collective bargaining, and if reduced collective bargaining encourages the underreporting of injuries, then, absent any real effect of repeals on injuries, we would expect to find repeals associated with a decline in reported injuries. The fact that we find repeals associated with an increase in reported injuries suggests that either (a) underreporting is not associated with repeals or (b) our estimated effect of repeals on the increase in actual injuries is underestimated by the amount that underreporting has hypothetically increased after repeals.

In the case of disabilities, in our full sample, controlling for time trends and a change in how disability questions were asked, repeals raised self-reported disabilities by 7.5% when not controlling for the unemployment rate and 8.2% controlling for unemployment. When the sample is restricted to repeal and always-had states, repeals raised disabilities by 6.5% not controlling for unemployment, and 7.5% controlling for unemployment. In the case of disabilities, the sample that excludes always-had states is too small for model estimation.

Enactments. Our injury data spanning the years 1976 to 2016 include only one prevailing wage enactment (Vermont in 1998) while our disability data spanning 1970 to 2016 include a second enactment (Minnesota 1973). With these limited legal changes, we were unable to discover statistically significant relationships except for lost days away from work where enactment reduced this injury rate by a statistically significant and meaningful 26%.[4] These results held whether or not the unemployment rate was included in the model.

Conclusion

Prevailing wage policies are the current focus of an intense public policy debate. The scientific literature on prevailing wage policy has focused on the effects of repeals on wages, benefits, and the direct cost of public construction. Following Azari-Rad, this article expands the analysis of the effects of repealing prevailing wage laws to workplace safety.

Prevailing wage repeals have been shown to reduce construction worker wages and benefits. Because the financing of worker safety training is among these reduced benefits, and the loss of health care benefits have been shown to reduce worker retention within the industry, we are not surprised that our empirical results are consistent with the hypothesis that prevailing wage repeals also increase construction workplace dangers. Our finding that injury rates rise from 11% to 13% as the severity of injuries increases is consistent with Azari-Rad and suggests that prevailing wage repeals increase not only the prevalence of injuries but their severity. Our finding that subsequent to repeals disability rates rise from 7.5% to 8.2% depending on model specification is consistent with the finding that prevailing wage repeals increase actual injuries and that these results are not an artifact of underreporting driven by moral hazard incentives within the worker compensation system and worker safety programs. These workplace safety issues are an additional consideration when evaluating the advisability of enacting or repealing state prevailing wage policies.

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Notes

1. For a fuller review of the economic effects of prevailing wage laws, see Duncan and Ormiston (2018).
2. The 2012 Economic Census questionnaire for construction states: "Employer's cost for fringe benefits: Voluntarily provided fringe benefits (Include such items as payments for life insurance, medical insurance, pensions, welfare benefits, and union-negotiated benefits)" https://www2.census.gov/programs-surveys/economic-census/2012/questionnaires/forms/cc23601.pdf (Form CC-23601).
3. The percentages shown in Table 5 differ slightly from the coefficients shown in the regression tables due to the transformation of the estimated coefficients into percentages using the formula percent = $(\exp(b) - 1) \times 100$.
4. For consistency, we retain Feasible Generalized Least Squares (FGLS) in Models 5 and 6 despite having only six time periods separated by decades (five when we include unemployment). In unreported regressions, we replicate Models 5 and 6 using fixed-effect regressions with clustered robust errors. The results are comparable with FGLS estimating a 10% increase in disabilities with prevailing wage repeals. The estimate for the effect of enacting prevailing wage laws is again statistically insignificant.

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