A Report on the Direct and Indirect Effects of Prevailing Wage Legislations on Society[1]

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EXECUTIVE SUMMARY

Prevailing wage laws mandate that contractors of public works projects pay wages and benefits prevailing in their local labor market. Nevada first passed its own prevailing wage law in 1937, and this law was last amended in 2005. For projects worth $100,000 or more, Nevada's Labor Commissioner currently determines which categories of workers will be covered by prevailing wage legislation, and the prevailing wage rate is then determined through a survey.

In this study, we surveyed the existing research on this subject and also performed original analysis to document the complex relationship between prevailing wage laws, actual wages, and construction costs. Our findings are as follows:

- A number of academic studies have found that prevailing wage laws leads to modest increase in public construction costs, but other more recent studies have found little influence on total construction costs. These later studies found that the earlier results showing increases in total construction costs omitted key explanatory control variables in their analyses.

- Other studies in the academic literature have explored whether prevailing wage laws lead to higher wages and benefits, but the estimates vary. For example, one estimate found that wages and benefits rose by 23%, while another study found an increase of only 2%. There is research concluding that when specific employee benefits are not required, employers reduce those benefits to offset higher wages.

- There is a great deal of variation in study results, because different studies measure different things in different ways. Some studies compare the average wages earned by construction workers in prevailing-wage projects with some form of market wage, while other studies use econometric techniques to estimate what happens to construction wages when prevailing wage legislation is repealed. We find the econometric studies tend to

estimate smaller impacts than the raw comparisons, because these methods are better able to control for other factors.

- It is easier to connect changes in prevailing wage laws with actual wage changes, but more difficult to demonstrate how this affects overall construction cost. Higher wages may reduce the profit margins of contractors, for example, rather than the price charged for public projects. Higher wages also tend to attract better workers, so that wage increases are compensated for by higher productivity. The effect on overall construction costs is thus not cut and dried.

- Opponents of the Davis-Bacon Act argue that it suppresses minority employment. Proponents, however, argue that the observed correlation between prevailing wage and black under-representation in the construction sector is largely spurious, and that this correlation does not imply that the former causes the latter. Furthermore, the literature has found that while there is evidence that the black-white wage differential tends to shrink following repeal of prevailing wage laws, it is not because of any improvement in the average wages of black workers. Instead, wages fall for both black and white workers, but the wage differential shrinks because the higher average wages of white workers falls proportionately more.

- The effects of prevailing wage laws on public budget are inconclusive as well. One Study have projected that federal repeal of the Davis-Bacon Act would reduce federal income tax collection by at least $1 billion (in 1994 dollars), and that any savings from repeal would not be enough for the government to recover the lost tax revenue. However, the methodology of that study was later criticized by another study which claimed that repealing the Davis-Bacon Act could save as much as $1.8 billion. A number of other studies have found that repealing prevailing wage laws may have a negative effect on public budget. One study found that even though repealing prevailing wage laws would create more construction jobs, but these jobs create few additional jobs. They ultimately concluded prevailing wage laws raises income and taxes.

- There is evidence that higher-paying jobs for relatively less educated men lead to lower expenditures on welfare, in part because of higher marriage rates for males with higher

paying jobs. There is also evidence suggesting that workers in low-skilled occupations significantly benefit from prevailing wage laws. Together, these two observations suggest that repealing prevailing wage laws may lead to higher welfare participation rates.

- In this study, we include new original analysis of data for the mountain states (Arizona, Colorado, Idaho, Nevada, New Mexico, and Utah, Wyoming) using procedures similar to those in other recent studies. The mountain states have different labor market regulations and economic characteristics than the nation as a whole. Four of the mountain states (Arizona, Colorado, Idaho, and Utah) repealed their prevailing wage law, while four states (Nevada, New Mexico, Wyoming, and Montana) did not. Comparing the experience of repeal states to non-repeal states, our estimates show that wages in repeal states decreased by only 1.7% over a short-run period of up to ten years, a result that was not statistically significant. In the long run, we found a more significant decline in wages, with repeal decreasing wages by approximately 4.4%.

Introduction

Prevailing wage laws mandate that contractors of public works projects pay wages and benefits prevailing in their local labor market. The origin of prevailing wage laws in the United States can be traced back to Kansas, where the first prevailing wage law was passed in 1891. Many other states and localities followed, and, by 1923, seven states had adopted prevailing wage laws. The first federal prevailing law was the Davis-Bacon Act of 1931, which mandated paying prevailing wage on all federally funded or assisted construction projects with total cost of $2,000 or above.

Nevada passed its own prevailing wage law in 1937[2], which was last amended in 2005. Currently, under the Nevada law (for projects worth $100,000 or more), the Labor Commissioner determines which categories of workers will be covered by the prevailing wage legislation and the wage rate is determined through a survey. Wage, as defined in the prevailing wage legislation, includes both the hourly wage and benefits paid to the worker. To determine the prevailing wage in an area, the Office of the Labor Commissioner conducts annual wage surveys. The prevailing wage is the modal wage (i.e. the most common wage) for that occupational category, as long as the modal wage exceeds 40% of all work hours in that occupation. If the modal wage was received in less than 40% of all work hours in a given profession, the prevailing wage is set at the average wage for that occupational category. It has been argued by some critics (Dunn et. al. 2005, Theiblot 2005) that the process of using the modal wage effectively ensures the selection of a wage rate that is the same as those negotiated by a union contract, since market wages are more dispersed. Supporters of this method, on the other hand, argue that using modal wage makes the prevailing wage less susceptible to extreme observations. For example a very high or very low wage observation may significantly affect the mean wage, but it will not change the modal (i.e. the most commonly observed) wage. Glassman et al. (2008) focused on the issue of prevailing wage determination and found that the prevailing wage rate is 22% higher than the

[2] A study by Theiblot (1995) ranked the 31 states that have prevailing wage laws based on the "severity" of the law. In that study Nevada's rank was 17 out of 31. Another ranking by the same author categorizes Nevada's law as "average", in a scale of "weak", "average", and "strong".

market wage rate (defined as the BLS average wage), which they claim inflates construction cost by 9.91%. They argue that surveys conducted by the Wage and Hour Division (WHD) at the Department of Labor (DOL) are not representative, because modal wage is dominated by unions and the modal wage determination method allows wages to be determined by as few as 12.5% of the respondents. One problem with the Glassman et al. (2008) study is that it compares job categories without controlling for skill means, which is not an apples-to-apples comparison (i.e., underlying workers may not be exactly comparable in terms of skill) – an issue which we will address later in this report.

In this report we will attempt to summarize our current understanding of the direct and indirect effects of prevailing wage laws as they pertain to a variety of outcomes. In particular, we survey the current literature to understand the effect of prevailing wage laws on public sector construction costs, construction sector wages, racial inequality, and the public budget, among others. If the prevailing wage laws affect wages of construction workers, then it may have an indirect effect on society through changing marriage rates and/or welfare participation rates. We survey the literature for these possible indirect effects as well. Finally, we perform a new analysis to estimate the effects of repealing prevailing wage laws on construction wages in the Mountain States. For this part we follow the methods developed by Kesslar and Katz (2001) who performed a similar study at the national level.

Construction Costs

We begin our discussion with the effect of prevailing wage laws on construction costs. While there is no clear consensus about the effects of prevailing wage on construction costs, there is evidence on both sides. Furthermore, it seems that the understanding of this issue has evolved over the last few decades. While a majority of the older studies (using a wage differential approach[3]) found that prevailing wage legislation leads to higher construction costs, more recent studies explore whether there exists a causal relationship between prevailing wage and construction cost (using appropriate econometric techniques). These recent studies find that

[3] Wage differential approach refers to a method where authors attempt to estimate the magnitude of wage increases due to prevailing wage legislation, and then assume that increased wages will lead to increased construction costs.

there is little to no relationship between construction costs and prevailing wage legislation. Further complicating the picture, the studies that show that prevailing wage laws lead to increased construction costs do not agree on the size of the increase. In our discussion below we will cover a number of studies that have produced conflicting results.

Thiebolt (1975) used a three month suspension of Davis-Bacon Act in 1971 to estimate the effect of prevailing wage on construction costs. Using the projects that were awarded in the three month window as control group (i.e. ones that did not receive the treatment of prevailing wage) he estimated that the Davis-Bacon Act increased construction costs by approximately 1.5%. However, Gould and Bittlingmayer (1980) argued that Theiblot (1975) did not account for all relevant factors, and suggested that, once these factors are accounted for, the increase in construction costs due to prevailing wage legislation was about 4% to 7%.

The General Accounting Office did a study on prevailing wage in 1979, which estimated that the Davis-Bacon Act leads to 3.4% increase in total construction costs on federal projects. Another influential study by Fraundorf et al. (1984), which, in order to estimate the effect of the Federal Davis-Bacon Act, collected data from 215 contractors that built non-residential buildings. Of these projects, 113 were federally funded and, therefore, were covered by the Davis-Bacon Act. They found that public construction projects were 26.1% more expensive than the private projects. Further, they explained that the striking 26.1% differential (which was well above the differential reported by other studies at that time) may have been due to their approach, which internalized aspects of construction that other studies ignored. However, even then, they concluded that their set of control variables were not extensive and future studies should try to improve upon their framework. Prus (1996) re-estimated a model that was very similar to Fraundorf et al. (1984) with more elaborate data, which allowed more control variables. He estimated the cost differential between public and private construction projects, using data only from states that had prevailing wage, finding that public projects were 31.7% more expensive than comparable private projects. He also found that once he controlled for construction type (i.e. public or private) the prevailing wage state premium vanished. This result led him to conclude that the Fraundorf et al. (1984) result was driven by specification and design differences in public and private construction and not by prevailing wages.

Another report, prepared by the Maryland Department of Fiscal Services (DFS), found that Maryland prevailing wage laws increased public building construction costs by approximately 5% to 15%. However, a later paper, by Prus (1999), argued that the estimates suffered from missing variable bias. In particular, Prus argued that the DFS study did not control for whether a project was a new or a renovation project. He used the exogenous variation produced by the changes in the Maryland prevailing wage law to estimate the effect of prevailing wage on public school construction cost. His study concluded that, once appropriate control variable are used, the prevailing wage law has no significant impact of public school construction cost. Even though public school construction costs were 40% more than private school construction costs, the reason for these increases was not attributable to prevailing wage.

Bilginsoy and Phillips (2000) used the introduction of prevailing wage law (the Skill Development and Fair Wage Policy (SDFWP)), in British Columbia, Canada in 1992, to explore the effects of prevailing wage on school construction costs. First they looked at simple differences in construction costs before and after the law. This simple before-after difference suggested that the prevailing wage law increased school construction costs by a statistically significant 16%. However, once they control for a variety of confounding factors, such as the construction business cycle, number of competitors, type of school, type of school district, and time trend, they found that the prices increased by 9.4% but the increase was not statistically significant. Furthermore, they found that there was a downward trend in prices in the period following the passage of the law. This downward trend would imply that the original (statistically insignificant) 9.4% jump was negated within 20 months. They suggest that issues normally overlooked were factors such as number of bidders and price trends, which should be more carefully incorporated in the analysis. Research not incorporating these factors may lead to misleading conclusion.

Phillips (2001) used variation produced by changes in the prevailing wage laws in Ohio, Kentucky, and Michigan to estimate the effect of prevailing wage legislation on construction costs. His sample consisted of 391 schools constructed in these three states during 1990s. While he did not find any association between prevailing wage laws and construction costs, he did find dispersion in the cost of construction among the states, with Michigan being the most expensive. He also found that school districts could save substantial amount of money by starting projects in spring.

In another study Azari-Rad, Phillips, and Prus (2002) found that prevailing wage laws do not lead to higher school construction costs in the US. This paper also suggested that the single most important strategy a school district can follow to lower construction cost is to plan the timing of construction. Authors found that building large number of schools at one time drives up the demand for construction services and leads to higher costs. They suggest the optimal strategy was to follow a counter-cyclical school construction plan. They estimate that doubling the state unemployment rate reduces school construction costs by approximately 21 percent.

In another follow-up study, Azari-Rad, Phillips, and Prus (2003) use variation in the state prevailing wage laws to identify the effect of prevailing wage on school construction costs. They analyzed F.W. Dodge data for school construction projects for all 50 states in the US for the years 1991 to 1999, finding that doubling the square footage of a school increases cost by only 93%, suggesting there are economies of scale in school construction, consistent with Fraundorf, Farrell, and Mason (1984). They also found that a high school costs about 5% to 8.1% more than a comparable elementary school, and that public schools cost about 15.6% more than the comparable private schools, a result that holds even in non-prevailing wage states. This suggests that additional factors other than prevailing wage laws are responsible for higher construction costs of public schools. Most importantly, when all appropriate controls are used, they found that prevailing wage laws do not have a statistically significant effect on school construction costs.

While most of the papers focus on the effect of prevailing wage legislation on public school construction costs, one relatively recent paper by Dunn, Quigley, Rosenthal (2005) analyzed the effects of prevailing wage on low-income housing construction costs. In 2001 California expanded the set of construction projects that were covered by prevailing wage law. This paper used the variation brought on by the change in the California law to test the effects of prevailing wage on low-income housing construction costs using California data. Their sample consisted of 205 low-income housing projects (175 of them were covered by prevailing wage law and 30 were not) undertaken during the 1997-2002 period. They used both Ordinary Least Square (OLS) and Instrumental Variable (IV) regression methods[4]. This was the only paper that

[4] Instrumental Variable (IV) regression under certain circumstances produces an unbiased regression estimate even when Ordinary Least Square (OLS) estimates are biased. For example, if there are factors (variables) that are both correlated with the prevailing wage law and construction, but those factors are not controlled for in the regression

used IV regression method to estimate the effects of prevailing wage on construction costs. The results showed that, in California, the prevailing wage regulations increased the low income housing construction costs by approximately 10% (OLS estimates) to 35% (IV estimates). They conjectured that their estimates were relatively high (compared to other studies) because they looked only at California, which is supposed to have one of the most restrictive prevailing wage laws in the nation[5]. However, their IV estimates were suspect because their instruments were not very strongly correlated with prevailing wage law[6], a critical pre-requisite for using IV regression methods.

May and Waddoups (2010) explored the effects of Responsible Contractor Practices (RCP) in Ohio. These RCPs, adopted by individual municipalities, school districts, or other local governments, are similar to prevailing wage laws in structure and impact. This particular study analyzed construction cost for 321 elementary schools from 1997 to 2008, 19% of which were covered by RCPs. Their results show that, once geographic and other controls are introduced, RCPs do not have any impact on construction costs.

Summary

- There is no consensus on the effect of prevailing wage laws on total public construction costs. Glassman et al. (2008), Dunn et al. (2005), Theiblot (1975), Gould and Bittlingmayer (1980) The General Accounting Office (1979), Fraundorf et al. (1984), and Maryland Department of Fiscal Services found prevailing wage laws modestly increased construction wage and total construction costs.
- Prus (1996), Phillips (2001), Azari-Rad, Phillips, and Prus (2002), May and Waddoupd (2010) found little influence of prevailing wage laws on total

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(possibly because data on these variables are not available), then OLS estimate will be biased. In such cases if we can find another variable (called an Instrument in the econometrics literature) that is (a) strongly correlated with prevailing wage but (b) is not correlated with construction cost (other than through prevailing wage) then using the instrument may produce an unbiased estimate. However, one has to be careful because if either condition (a) is not satisfied (known as weak instrument problem) or condition (b) is not satisfied then IV regressions can produce highly unreliable (in some senses worse than OLS) estimates.

[5] A study by Theiblot (1995) ranked the 31 states that have prevailing wage laws based on the "severity" of the law. In that study California was ranked 28 out of 31, with 31 being the strictest.

[6] The joint first stage F-stat (even without any other covariate) is 2.98 which is very low. A rule of thumb suggests that the first stage F-stat should be at least 10.

construction costs. These studies show the results of other studies, showing substantial increases in total construction costs under prevailing wage, were likely due to omitted variables and other controls that were not incorporated in the analysis.

Wages and Benefits

Another strand of this literature explores whether prevailing wage laws lead to higher wages. One benefit of this approach is that it is easier to connect the law to wage changes, but the drawback is that increased wages may not tell us how it affects construction costs. For example a contractor might reduce his/her profit margins in response to higher wages. Another issue might be productivity. If, for example, higher wages attract a better quality of workers then some of the wage increases may be compensated by higher productivity. Therefore, it is not obvious (although it is certainly plausible) that higher wages will lead to higher construction costs. If our ultimate objective is to infer about construction costs, then resulting wage changes have different implications depending on whether the studies find an increase or decrease (or no change) in wages. If prevailing wage laws do not increase wages in the construction industry then it is highly unlikely that it will increase construction costs. In other words, if we find that prevailing wage laws increase wages in the construction industry, then that may or may not lead to higher construction costs, but, if the prevailing wage laws do not increase wage in the construction industry, then it will not lead to higher construction costs. With this discussion in mind let us look at the results from the relevant literature.

Phillips (1998) investigated the effects of the repeal of the Kansas prevailing wage law in 1987. He compared Kansas with neighboring states with prevailing wage laws and analyzed a number of outcomes, concluding that repeal led to a decline in the wages and pension benefits of Kansas construction workers. He also found a decline in apprenticeship programs and a rise in work related injuries in Kansas after the repeal of the prevailing wage law.

Keller and Hartman (2001) compared the wages of workers in public and private construction projects in Pennsylvania during the years 1992 to 1997. They found that the Pennsylvania prevailing wage law increased public sector construction wage by 17% and benefits by 21.5% compared to private sector construction wage. However, when extrapolated, they suggested that prevailing wage inflated total public school construction costs by only 2.25%.

Clark (2005) compared the wages of construction workers on a project covered by the prevailing wage law to the wages earned by the same workers on a project not covered by the prevailing wage law to estimate its effect on construction wages. A significant proportion of non-union contractors pay their workers at different rate depending on whether the project is covered by the prevailing wage law or not. This allows Clark (2005) to compare the wages of the same workers with and without the prevailing wage law, using a sample of 267 construction workers in Kentucky during 1999-2000. The biggest advantage of this data (and therefore the study) is that it automatically control for the workers skill level and therefore less likely to suffer from unobserved variable bias. He found that, in 60% of the cases, workers were paid more in prevailing wage projects compared to projects that were not covered under the prevailing wage law. He also found that, in 12% of the cases, workers were paid less in prevailing wage projects as compared to projects not covered under the prevailing wage law. In the remaining 28% of the cases there were no differences. He also found that mean wage received by the workers in prevailing wage projects was $3.68, or about 23%, higher than the mean wage received by the same workers when they worked on projects that were not covered under the prevailing wage law.

Peterson (2000) analyzed the effect of prevailing wage laws on benefits, especially health and pension benefits of constructions workers. He compared the wages and benefits received by construction workers in states that never had prevailing wage law, states that repealed their prevailing wage laws, and states that kept their prevailing wage laws between 1982 and 1992. He found that, during the 1980s, while average total compensation remained unchanged (around $35,000 per year) in the non-repeal states (28 states), it decreased by 16.6 % (from about $35,000 to $29,300) in the repeal states (6 states), and it actually increased by (a statistically insignificant) 10.5% (from $27,500 to $30,400) in states that never enacted prevailing wage laws (8 states). However, most of the changes in total compensation came from changes in benefits (pension and healthcare), with repeal states having the most dramatic decline. During this timeframe, while average benefits in the non-repeal states increased by 3.2%, they decreased by 53.4% in the repeal states. They also noted that the percentage of compensation that comes as wages (including benefits) declined from 94.1% to 92.2% in the non-repeal states, but increased from 96.4% to 98.0% in the repeal states. It stayed flat at around 98.5% in states that never enacted a prevailing wage law. Then he used regression method to control for other confounding

variables, such as unionization rate, state and year fixed effects, among others. Regression results showed that total compensation was approximately 12% higher in the prevailing wage states compared to the repeal states, but the total benefits are about 61% higher. The most dramatic difference comes from pension benefits, with construction workers in the prevailing wage states having 104% higher pension benefits than comparable workers in the repeal states. His analysis further shows that the decline in wage does not happen immediately after repeal, rather, wages decline gradually over the first five years after repeal. Decline in pension benefits is even more gradual, with no decline in the first three years after repeal, but a steady decline starting from the fourth year. Therefore the results show that, when the employers are not required to pay certain benefits as part of total compensation, they reduce benefit more than the wages. Peterson concludes that the larger decline in pension benefits compared to wages is most likely due to employee preference for wages over benefits.

Price (2005) estimated the effect of repeal on pension and health insurance coverage. He found that the odds of having pension coverage declined by 11 to 16 percent, and the odds of having health coverage declined by 11 to 14 percent after repeal. He also explored whether these declines are different for workers with different skill levels, but found that these estimates do not differ by skill level. However, his analysis suggests that most of the decline in pension and health coverage did not come from unions eliminating benefit plans but as a result of the reduction of union density itself.

Perhaps one the most thorough studies on this subject is a paper by Kesslar and Katz (2001), which is also one of the more influential studies. Almost all later studies compare their results with Kesslar and Katz (2001). In our own analysis (later in this report) we follow their methodology as well. Therefore, we will explain the methodology and the results of this paper in detail. What sets this paper apart from other studies is that these authors are very mindful of the pitfalls of finding a causal relationship between prevailing wage law and construction wage, namely the many other confounding factors that can create a spurious correlation[7] between these two variables. They use Current Population Survey (CPS) and Census data and follow what is

[7] When there exists a statistical correlation between two variable but they are not causally related then it is called spurious correlation. For example if we were to compute the correlation coefficient between banana production in Costa-Rica and number of car accidents in the New York City for the last 100 years then it will show a positive correlation coefficient. However, clearly they are not causally related in the sense that one is not causing another.

known as difference-in-difference-in-difference (DDD) strategy. They exploit the fact that a number of states repealed prevailing wage laws to compare wages of blue-collar construction workers to blue collar non-construction workers in repeal states and non-repeal states before and after the repeal. In particular, they take the difference in the average wages of blue collar construction workers before and after the repeal in the states that repealed the law. This is known as before-after estimator. Since the wage change in these states could be attributable to something other than the repeal of the prevailing wage laws, any before-after difference should not be attributed to the change in the law alone. One way to circumvent this problem is the look at the before-after differences in the non-repeal states. Since these states did not repeal their prevailing wage laws, any change in these states cannot be attributed to prevailing wage laws. If we observe the mean wages in the non-repeal states have changed then that must be due to other factors (such as change in overall economic condition). To the extent that these "other factors" also affect the repeal states, any before-after estimate obtained for these states includes the effects of these changes. If we subtract the difference in wage in repeal states from the difference in wage in non-repeal states then that would be a better estimator of the effect of prevailing wage law. This estimator is known as difference-in-difference (DD) estimator. However, Kesslar and Katz (2001) argue that unobserved time varying factors that affect all blue-collar workers (not just those in constructions) may be correlated with changes in legal structure. To rule out the possibility of such an event corrupting the estimate, the difference-in-difference estimate for blue-collar workers in construction is subtracted from the difference-in-difference estimate for blue-collar-non-construction workers. This is what we referred as the DDD estimate.

The following discussion is based on Table 2 of Kesslar and Katz (2001). They show that the average wage of blue-collar construction workers in the repeal states in 1979 (before repeal) was $8.717; however, the wage decreased to $6.828 in 1993 (after repeal), a decline of 21.7% (this is what we called the before-after estimate in our discussion above). However, average wage of blue-collar construction workers in the non-repeal states also decreased from $9.995 to $7.997, a decline of 19.7%, which clearly occurred due to reasons other than the repeal of prevailing wage law. Therefore the 21.7% decline in the repeal states cannot be due to change in prevailing wage law alone. If we subtract 19.7% from 21.7%, we get an estimate of 2.0%. In other words repeal of prevailing wage law lead to a 2.0% decline in the wages of blue-collar construction workers. This is difference-in-difference estimate as described above. Then they

computed the corresponding difference-in-difference estimate for the blue-collar-non-construction workers, which show a 0.7% decline. Again, this 0.7% decline cannot possibly be due to prevailing wage. Therefore, the DDD estimate suggests that repeal of prevailing wage law lead to a 1.3% (2.0-0.7) decline in the wages of blue-collar construction workers. A similar calculation shows that repeal of prevailing wage laws lead to a 1.9% decline in the wages of all construction workers. A regression based approach that controls for demographics arrives at a similar estimate.

Kesslar and Katz (2001) also explored whether the effect of repeal of prevailing wage laws was heterogeneous, or different, across groups. They were primarily concerned with differences across unionization status and race. Here we will discuss only the differences between union and non-union workers, as race issues are discussed later in this report. They show that repealing prevailing wage laws do not alter the unionization rate. However, after controlling for a plethora of demographic variables, state, and time fixed effects, they found that repealing the prevailing laws reduced union premium (the difference between union and non-union wages) by 5.9 percentage points. They also found that this decline in union premium grew over time. According to their estimates, the union premium declined by 9.8 percentage points three years after repeal, and 11.2 percentage points five years after repeal. They note that the average union premium in their sample period (1983-93) was about 20%. Thus, repealing the prevailing law decreases union premium by roughly half.

Price (2005) extended Kesslar and Katz (2001) study in several dimensions. Along with the question explored by Kesslar and Katz (2001), he also estimated whether the changes in unionization, wages, and benefits differ by the skill level of the worker. He found that repealing prevailing wage laws reduces the unionization rate by approximately 20% five or more years after repeal. He found that the change was largest for laborers. In particular, five or more years after repeal, the odds of a laborer being in a union declined by 46% compared to a semi-skilled worker, by 62% compared to a medium skilled worker, and by 79% compared to a high skilled worker.

Price (2005) found that the overall decline in hourly wage rate was 1% to 2%, which is consistent with Kesslar and Katz (2001). However, he also reported that the decline in the union premium was about a quarter of total union premium. This loss in union premium is lower than that found by Kesslar and Katz (2001), who reported that repealing prevailing wage laws cuts the

union premium in half. Price (2005) also found that low-skilled workers bore the brunt of the repeals as they experienced largest decline in the wages.

Bilginsoy and Philips (2000) concluded that prevailing wage has no effect on construction costs, but found that the number of bidders as an important factor in effecting construction costs. Their argument is that more bidders mean more competition, which should result in lower prices (in this case construction costs). However, only one paper has addressed the competition issue. A very recent paper by Kim et al. (2012) analyzed data from California to explore whether prevailing wage laws affect the number of bidders in a public project. This paper uses a variation of the California law that allows municipalities to opt-out from the state prevailing wage law requirements. If a municipality choses to opt-out then the construction financed by local funds will be exempt from state prevailing wage law requirements. This paper analyzes data from 565 bids on 140 projects, some of which were covered by prevailing wage law requirements, and some of which were not. They found that prevailing wage law requirements do not affect the number of bidders on a project. They also analyzed whether the bids submitted by the contractors were affected by prevailing wage law restrictions. To do so, they compare the final bid price with engineers' estimates. Again, they do not find any evidence that contractors changed their bids based on prevailing wage law requirements.

Summary:

- This section surveyed the current literature to document whether prevailing wage laws lead to higher wages. One advantage of this approach is that it is easier to connect the law to wage changes, but the drawback is that increased wages may not tell us how the laws affect construction costs.
- A contractor might reduce his/her profit margins in response to higher wages. Another issue might be productivity. If, for example, higher wages attract a better quality of workers then some of the wage increases may be compensated by higher productivity. Therefore, it is not obvious (although it is certainly plausible) that higher wages will lead to higher construction costs. It is also possible that, without prevailing wage laws, less productive labor may lead to higher total construction costs.Among the studies that explore the relation between prevailing wage legislations and construction wages, the estimates vary, ranging from a high of 23% (Clark, 2005) to approximately 2% (Kesslar and Katz, 2001).

- One reason behind such variation in results of these studies is that the different studies do not always estimate the same thing in the same way. Some of the studies compare the wages earned by construction workers in projects that are covered by prevailing wage legislation to some form of "market wage", while other studies use econometric techniques to estimate what happens to construction wages when prevailing wage legislation is repealed. The first group of studies usually finds larger impacts than the second group.
- Peterson (2000) analyzed the effect of prevailing wage laws on benefits, especially health and pension benefits of constructions workers. His results show that, when the employers are not required to pay certain benefits as part of total compensation, they reduce benefit more than the wages.

Minority Outcomes

Like many aspects of the prevailing wage laws, their effect of racial equality (or lack thereof) in the construction industry has also been debated. It has been argued by the opponents of the Davis-Bacon Act that such a law suppresses minority employment. For example, Theiblot (1999) reported that prevailing wage laws reduce black employment in construction. Using 1990 census data he found that black workers were about 5.8% of all construction workers in states with prevailing wage laws, but in states without prevailing wage laws black workers were about 13.0% of all construction workers, even though black workers constituted 9.1%, and 14.0% of all workers in these states respectively. Therefore the "sector-ratio" in prevailing wage states is 64% as compared to 91% in the states without a prevailing wage law. He further explores whether within prevailing wage law states, the strength of law matters. By ranking the strength of the state's prevailing wage laws, he found that in states with "strong" prevailing wage laws, black workers were about 5.7% of all construction workers. However, states with "average" prevailing wage laws, black workers were approximately 4.0% of all construction workers, while states with "weak" prevailing wage laws, black workers were about 8.0% of all construction workers, even though black workers constituted 9.3%, 6.1%, and 11.6% of all workers in these respective states. Therefore the "sector-ratios" for strong, average, weak law states were 62%, 65%, and

69% respectively. Given these results, Theiblot concluded that prevailing wage laws restricted black employment in construction, and predicted that repeal of prevailing wage laws would lead to more jobs for black workers in the construction sector.

These conclusions were later questioned by Azari-Rad and Philips (2003). They reproduced the Theiblot (1999) results using 1990 census data, but argued that the observed correlation between prevailing wage laws and black under-representation in the construction sector is a spurious one and these two variables are not causally related. By analyzing the 1970 census data they showed that the "sector-ratio" in prevailing wage states was 79%, while it was 100% in the states without a prevailing wage law (as of 1990). Furthermore, they separated the states without a prevailing wage law (as of 1990) into two groups. One group, which repealed the law between 1970 and 1990, and the other group – which never enacted a prevailing wage law. Data showed that the sector ratio in the states that repealed the law between 1970 and 1990 was 102% in 1970. Also in 1970, all these states had prevailing wage laws and still had a high sector ratio. Therefore Azari-Rad and Philips (2003) concluded that the reason behind high sector ratio in states without a prevailing wage law was not the law itself, but something else, which they conjecture is an artifact of idiosyncrasies in the Southern labor market. The debate continued when Theiblot (2003) argued that Azari-Rad and Philips (2003) criticism of his earlier paper was wrong and that his original result about a negative relationship between prevailing wage laws and black employment in construction was indeed causal. To prove his point, Theiblot (2003) added the 1980 census to the 1970 and 1990 census data. By using three consecutive censuses, he tried to show that black employment in the construction sector suffered the least in states that never enacted the prevailing wage law, followed by states that repealed the law, with states with a prevailing wage law (as of 1990) having the highest negative effect. His analysis showed that sector ratio declined from 100% to 96% to 89% in the states that never enacted the law, 102% to 96% to 94% in the states that repealed their law, and 79% to 70% to 64% in the states with a prevailing wage law. Bloch (2003) also explored whether prevailing wage laws suppress minority employment. He analyzed data for 162 metropolitan areas, and estimated that repealing the Davis-Bacon Act would create 37,000 construction jobs in these areas, including about 27,000 for minorities.

Most of these papers use descriptive statistics to substantiate their results. Kesslar and Katz (2001), again, provide the most comprehensive analysis of this issue. They found, in the

1990 census data, that 11.7% of blue-collar workers were black, but only 7.4% of blue-collar construction workers were black. However, this 4.3 percentage point employment differential does not necessarily mean that prevailing wage is the reason behind this difference. If the prevailing wage law was indeed suppressing black employment in construction, we would expect this differential to shrink after the repeal of a prevailing wage law. To this effect, they explored whether black employment in construction increased after repeal of the prevailing wage laws. They found that the differential shrank by 1%, but once they account for the fact that blacks in different states may be heterogeneous (by introducing black*state interaction dummies) then the coefficient is no longer statistically significant. They then focused on how the repeals changed the relative wage of blacks/whites in the construction sector. They found that repeal leads to an increase in the black premium of about 4%. They also found that in their sample (1990 census) blacks earned about 6.5% less than whites. Therefore, the 4% increase in black premium eliminates about 60% of the difference. However, they also found that even though relative incomes of blacks increased, their absolute income did not. For some of their specifications, they remain the same, and for other specifications the absolute income of blacks actually declined. In other words, repealing the prevailing wage law reduces wages of all workers, but it reduces the wages of white workers more than the black workers, thereby reducing the relative difference.

Price (2005), on the other hand, reported that repealing prevailing wage laws does not change the wage of black workers compared to non-blacks. Price (2005) also explored whether change in union density was different by race and found that the decline among the black construction workers were dramatic (about 58% five or more years after repeal).

Summary:

- It has been argued by the opponents of the Davis-Bacon Act that such a law suppresses minority employment. For example, Theiblot (1999, 2003) reported prevailing wage laws reduce black employment in construction.
- Azari-Rad and Philips (2003) argued that Theiblot's (1999) observed correlation between prevailing wage and black under-representation in the construction sector is spurious and that the two variables are not causally related.
- Bloch (2003) explored whether prevailing wage suppresses minority employment. He estimated that repealing the Davis-Bacon Act would create 37,000 construction jobs, including about 27,000 minority jobs.

Kessler and Katz (2001) provide the most comprehensive analysis of this issue. They show that black-white wage differential shrinks following repeal of prevailing wage law. However, they also found that even though relative incomes of blacks increased, the absolute income does not. In other words, repealing the prevailing wage law reduces wages of all workers, but it reduces the wages of white workers more than the black workers, thereby reducing the relative difference.

Budget

Some studies go beyond the narrower outcomes such as wages, construction costs, etc... and try to estimate the overall economic impact of the repeal of state prevailing wage laws. One of the earliest such studies was Philips et al (1995). They analyzed the effects of the repeal of prevailing wage laws in nine states, with a special focus on Utah. They conclude that construction earnings in Utah dropped from 125% of average non-agricultural earnings in 1970s to 103% in 1993, attributing the reduction to both lower wages and a shift to less-skilled construction workers. They also concluded that the repeal of the prevailing wage law tripled cost overruns on state road construction projects in Utah. They attribute these overruns to a shift to less-skilled construction workers and suggest that such frequent cost overruns make any savings in public construction cost unlikely. Further, they conclude that repeals reduced average construction earnings by about $1,477 per year (in 1994 dollars), reduced construction training by about 40%, and increased occupational injuries by 15% in the nine states that repealed prevailing wage laws. Based on these results, they project that repeal of the Federal Davis-Bacon Act would reduce federal income tax collection by at least $1 billion (in 1994 dollars) and the savings from repeal would not be enough for the government to recover the lost tax revenue. They also project that repeal would lead to 76,000 workplace injuries leading to a loss of 675,000 work days. However, Theiblot (1996) argued that the methodology of Philips et al (1995) was flawed. He argued that (p 312) "*repeal lowers artificially inflated wage rates but only towards the market level, not below it; that the lower construction worker earnings and government tax losses portrayed in the study exist only when measured in theoretical constant dollars but do not exist or are really increases when measured in actual dollars*". He concluded

that repeal of the Federal Davis-Bacon Act would save federal government at least $1.8 billion (in 1994 dollars).

A number of other studies have tried to estimate the impacts on state budgets. The central argument in these studies is that repealing prevailing wage laws would reduce the income of construction workers, which would lead to lower income and sales tax revenue for the state. Belman and Voos (1995) estimated the economic impact of repealing prevailing wage laws in Wisconsin. They conclude that repealing the Federal Davis-Bacon Act would reduce the total wage of construction workers by at least $123 million (in 1995 dollars). They further argue that if the indirect effects are taken into account then the total loss could reach $250 million. This, they argue, would reduce tax revenue by $11.6 million. If the Wisconsin state prevailing wage law was repealed along with the Davis-Bacon Act, the total loss in state tax revenue would be $23 million (in 1995 dollars). They further estimate that repealing Davis-Bacon Act would affect the state budget negatively. Their calculations show that loss of tax revenue ($11.6 million) would exceed savings in public construction ($4.8 million), causing a net decline of $6.8 million in state income. If both the state and the federal prevailing wage laws are repealed then that loss would be doubled.

Using an input-output model, Kelsay et al. (2004) estimate that repealing the prevailing wage law in Missouri would reduce total construction wage by at least $294 million, sales tax by at least $5.7 million, and income tax by $17.7 million, leading to a total economic loss of at least $318 million. They also observed that repealing prevailing wage laws leads to reduced training, more injuries, and entry of smaller, inexperienced construction firms with poorer safety records. Kelsay et al. (2004) also found that 91.7% of all construction work was done by instate firms in prevailing wage states compared to 84.5% in non-prevailing wage states. They take that as evidence that even the in-state construction firms would not benefit from repealing the prevailing wage law.

Another study by Jordan et al (2006) concluded that repealing the prevailing wage law in Minnesota would lead to a decline in state tax revenue by at least $38 million. The same study also concludes that repealing the prevailing wage law *"would result in weakening of apprenticeship training programs, an increase in injury rates, a weakening of the position of women and people of color in the construction industry, an increase in project cost overruns, and a reduction in construction employee wages."*

Greenberg et al. (2005) used an input-output model to estimate the overall economic impact of the prevailing wage law in New Jersey. They first simulated the overall effect of a $10 billion new, state funded, school construction program with prevailing wage and without prevailing wage. They found that under the prevailing wage scenario the project would generate $6 billion in personal income, approximately $11 billion in state gross domestic product, and about $1.3 billion in state and local tax revenues. With non-compliance (or partial compliance) of prevailing wage law restrictions, the resulting impacts would be substantially negative for personal income, taxes, and employment creation. The effect depends on the proportion of workers that pay taxes and spend their earnings inside the state. They concluded that *"prevailing-wage compliance has positive benefits for income and taxes. Non-prevailing wage construction should create more construction jobs, but these jobs create fewer additional jobs in the economy."*

Summary

- Some studies attempt to estimate the overall economic impact of repeal of state prevailing wage laws. Phillips et al. (1995) estimated the effects of repeal of the prevailing wage law in nine states with an emphasis on Utah. They project that repeal of the Federal Davis-Bacon Act would reduce federal income tax collection by at least $1 billion (in 1994 dollars) and the savings from repeal would not be enough for the government to recover the lost tax revenue. However, Theiblot (1996) argued that the methodology of Philips et al (1995) was flawed.
- Studies by Belman and Voos (1995), Kelsay et al. (2004), Jordon et al. (2006) and Greenberg et al. (2005) also find that the repeal of prevailing wage laws lowers state revenues and state economic activity because of lower personal income expenditures by construction laborers.

Externalities

This section considers possible externalities, other than the indirect effect on taxes discussed earlier, of prevailing wage. Earlier we discussed the effects of prevailing wage on a number of outcomes including wages in the construction sector. Our discussion of the related

literature suggests that prevailing wage legislation increased the wages in the construction sector. Furthermore, results from Price (2005) suggest that the biggest beneficiary are the low skilled workers such as laborer, painters etc. In other words it generates relatively high paying jobs for relatively less educated individuals (primarily males). A higher wage for relatively less educated men may lead to higher marriage rates which may in turn lead to lower welfare participation rates. This issue has not been studied in the context of prevailing wages, but the following discussion reveals that such a connection may exist.

Before going into the details we should note one caveat. Higher wage rates may (because of prevailing wage law) reduce the demand for labor. This is the primary argument behind the minimum wage debate. This argument is as follows: in a competitive labor market model, an artificial floor on wage rates (such as one created by minimum wage) reduces the quantity demanded for labor and increases the quantity supplied. The net result is that some of the individuals who are willing to work at that wage will not be able to find jobs, rendering them unemployed. While prevailing wage requirements are different, a somewhat similar argument can be made in the context of prevailing wage as well. In our review of the prevailing wage literature it did not seem that this was the case[8], nonetheless, this caveat should not be forgotten or dismissed.

At the same time it has been shown by Card and Krueger (1994), in the context of minimum wage, that, while an increase in unemployment is a possibility, it is not the only possibility. They found that a "reasonable" increase in the minimum wage can actually lead to more employment and not less. This may happen if the labor market was not competitive but monopsonistic, where employers have significant market power. An increase is also possible when workers are freely mobile across sectors perhaps because of training costs (see Borjas for a detailed analysis). The Card and Krueger (1994) study was later questioned by Neumark and Wascher (1995) and the debate on this issue currently continues. In what follows we assume that prevailing wage policy can increase the wage of relatively less educated individuals without

[8] Greenberg (2005) can be counted as a qualified exception. They state that "*Non-prevailing wage construction should create more construction jobs, but these jobs create fewer additional jobs in the economy*", which suggests that the overall effect of prevailing wage on job creation is somewhat ambiguous.

creating any significant reduction in the quantity of labor demanded. It is important to note that, as we have argued above, this is not a certainty (but a plausible scenario).

Black, McKinnish and Sanders (2003) [hereafter BMS] discussed the competing explanations put forth by Murray (1984) and Wilson (1987) behind increased welfare dependence in the U.S. The "culture of poverty" argument put forward by Murray suggests the causality runs from welfare to marriage. In other words availability of increased welfare benefits reduces the incentive for marriage, especially for women. This leads to lower marriage rates, lower labor force participation rates, and higher out-of-wedlock childbirth rates. Wilson (1987), on the contrary, points to systematic destruction of "middle class jobs," in other words jobs that have a relatively high pay but do not require too much skill, as the root cause. In his view reduced or non-availability of such jobs makes men less attractive to women, thereby reducing marriage rates.

Wilson's hypothesis suggests that the reason behind the declining marriage rate (and associated effects such as increased out-of-wedlock childbirths and welfare dependency) is reduced job opportunities for relatively less educated men. Therefore, one can use the data to test whether entry into marriage is affected by wage. A number of empirical studies have tried to test this hypothesis. One complicating issue in this context is what is known as "endogeneity". This refers to a situation where two variables are correlated but are not necessarily causally related. In other words, one variable is not causing the change in the other variable. For example, if there exist characteristics (such as smartness and/or a personable character) that lead some men to earn more, and also makes them more "marriageable," then we would observe an association between high earners and marriage rates, even though they are not causally related. While a number of studies in this area have taken a different approach to account for this problem, not all are equally rigorous. A number of different data sets have been used in these studies. Some of these data sets are cross-sectional – where there is one observation for each individual, and some are panel – where individuals were repeatedly interviewed over a number of years. Most recent studies use panel data, which allows the author to data to control for time-invariant, unobserved,

heterogeneity[9]. We provide a summary from a number of studies in this area below. First we will discuss the studies that establish an association between earnings and marriage rates for men.

Using Census data, Schultz (1994) showed that marriage rates are associated with male wages. For men, an increase in wage increases the probability of being married. Clarkberg et al. (1995) found that high earning men with stable jobs are more likely to be in a marital relationship (as opposed to a cohabiting relationship). Smock and Manning (1997) found that the probability that a relationship will result in marriage increases with the male partners' income. Some of the other studies that have arrived at similar conclusion include Clarkberg (1997), Sweeny (2002), Oppenheimer (2003) and Burgess et al. (2004). Xie et al. (2003) found that earning potential (as opposed to actual earnings, as studied in the papers cited above) is positively related to probability of marriage for men.

On the other hand, in an influential article, Wood (1995) found that the decline in "marriageable" men is not the most important reason behind the decline in marriage rates among blacks. Wood (1995) explicitly addressed the endogeneity problem discussed above by using the standard metropolitan statistical area (SMSA) industrial mix as an instrument.

While these studies are informative, BMS argues that the spirit of Wilson's hypothesis requires testing using exogenous variation in income for relatively less educated men. Therefore BMS used exogenous variation in income from coal "boom and bust" periods in Appalachian states. The primary logic behind their study is that exogenous changes in the price of coal during the 1970s and 1980s changed the economic environment faced by the relatively coal rich counties in the Appalachian region. They document that the price of coal more than doubled between 1970 and 1975 (primarily due to OPEC oil embargo) but then came back down to pre-1970 level between 1975 and 1980. The coal "boom" in the first half of 1970s created a significant number of high paying jobs for relatively less educated men, but, with the resulting coal "bust", destruction of many of those jobs occurred. This quasi-natural experiment allows BMS to study the effects of the availability of high paying jobs for relatively less educated men on marriage rates and welfare participation rates. Their results show that increased availability of

[9] This term refers to individual level characteristics that may be important to the analysis but are not observable to the analyst. One example may be personality. Obviously this may affect chances of marriage but is cannot be observed by the analyst in the data.

high paying jobs for relatively less educated men results in lower expenditure on welfare programs.

BMS then investigated why the availability of high paying jobs for men leads to lower expenditure on welfare – a program primarily used by women. They concluded that the primary reason lies in higher marriage rates. While BMS tied the issue of the availability of "marriageable" men and welfare participation, it was not the only study to investigate the linkages between the marriage market and what has been termed as welfare dependency (welfare participation rates and welfare duration). Other studies in this area included Ellwood (1986), Fitzgerald (1991), and Moffitt (1992). Fitzgerald (1991) found very little evidence that the availability of marriageable men was important in explaining the exit rates of Black women from Aid to Families with Dependent Children (AFDC) program. This result was consistent, at least in spirit, with Wood (1995). Moffitt (1992) on the other hand found that the decline in male wages in the 1980s was the primary factor behind the rise of female headed households, though his results are more robust for whites than for blacks. The association between marriage and welfare participation was acknowledged in the 1996 welfare reform that led to the Personal Responsibility and Work Opportunity Reconciliation Act. Even though there was a lack of a consensus in the literature on this issue, BMS was probably closest to the question which we have at hand – which is the extent to which relatively high paying jobs for relatively low educated men affect welfare participation rates. BMS continued to document that availability of high paying jobs increases the "marriageability" of these men, and, thus, leads to a higher marriage rate. As more women became and remained married, the welfare participation fell. The other mechanism through which welfare participation rates fell was through a higher labor force participation rate for women. This, caused by more income generated by men, created secondary demand for products and services. In this case they showed that in this period, while the labor force participation rates for single women did increase, that of married women declined, most likely due to higher incomes earned by their husbands.

Above we discussed policies that increased the availability of high paying jobs for relatively less educated men, which are likely to increase marriage rates and reduce welfare participation rates. There are other ways prevailing wage may affect society at large. One important area is children's education. While there is no study that specifically investigated

relationship between prevailing wage and children's educational performance, there is a literature that addresses the broader questions such as socioeconomic status of parents and educational outcomes. There is a large literature on this topic, but the following paragraph from Duncan and Brooks-Gunn (2000) provides a nice summary of the salient results: "*In terms of achievement, the risk for poor relative to non-poor children is 2.0 times as high for grade repetition and dropping out of high school, and 1.4 times as high for having a learning disability. For other conditions and outcomes, these risk ratios are: 1.3 times as high for parent-reported emotional or behavior problems, 3.1 times as high for a teenage out-of-wedlock birth, 6.8 times as high for reported cases of child abuse and neglect, and 2.2 times as high for experiencing violent crime*". Therefore any policy that may increase the wage of relatively less educated men may improve the outcomes of children by providing a stable environment to live. Another study by Ananat, Gassman-Pines and Gibson-Davis (2011) found that job losses from local plant closings have detrimental effects on child educational outcomes.

Summary

- This section addressed other outcomes besides increased tax revenues and higher construction sector wages.
- Black, McKinnish, and Sanders (2003) investigated why availability of high paying jobs lead to lower expenditures on welfare. They concluded that the primary reason lies in higher marriage rates, due to males with higher paying jobs. Price (2005) suggests that the biggest beneficiary of prevailing wage is low skilled occupations. These two observations together suggest that repealing prevailing wage laws may lead to higher welfare participation rates.

New Analysis for the Mountain states

In this part we follow procedures outlined by Kesslar and Katz (2001) but with a focus on the Mountain states, as defined by the 1970 census. The Kesslar and Katz (2001) study was national in scope, using data from all 50 states and D.C. in their analysis. The Mountain region includes the states of Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Wyoming, and Utah. We focus on this region because different regions of the U.S have very different labor market structures, so it is conceivable that labor market regulation would have different impacts

in different regions. We restrict our focus on this region because it is relatively homogeneous and includes Nevada. Four states (Arizona, Colorado, Idaho and Utah) in this region have repealed the prevailing wage law and four (Nevada, New Mexico, Montana, and Wyoming) did not. In other words four states received the "treatment" of repeal while four did not. These four non-treatment states are classified as the control group for the four states that received treatment. This means we can meaningfully compare the experiences of repeal states to the experiences of non-repeal states within the region. Arizona repealed its prevailing wage law in 1979, Utah in 1981; both Idaho and Colorado in 1985.

Current Population Survey (CPS) Outgoing Rotation Group (ORG) data from 1979 to 2002 was used for our analysis[10] (Kesslar and Katz (2001) used the same data but from 1979 to 1993). One issue that has been raised by many papers in this literature is how long it takes after a change in law before the full impact of the change is realized. Kesslar and Katz (2001) estimated the impact for three or more years after repeal. Since they considered data only until 1993, it may be possible that the full effect of law change was not realized by the end of their study period. By extending their analysis we can find whether the estimate changes with a longer horizon. First we present the analysis with 1993 as the end year (same as Kesslar and Katz, 2001) and then we extend it to 2002.

Table 1 shows the change in hourly wage of blue-collar[11] workers. Panel A shows the changes in the wages of blue collar construction workers. All wages are in 1982 dollars. Average wage of a blue collar construction worker in the repeal states declined from $9.55 in 1979 to $7.54 in 1993 or a decline of 21.0%. However, during the same time period, wages of blue collar construction workers declined by approximately 20.3% (from $10.33 in 1979 to $8.23 in 1993) in states that did not repeal the law. Therefore, the difference-in-difference estimate would suggest that repealing the prevailing wage laws reduced wages of blue-collar construction workers by 0.7%.

[10] This was appended with data from the 1977 and 1978 CPS May extracts when running regressions, in order to get a better picture of the conditions in Arizona prior to 1979. This is the same method that was used in Kesslar and Katz (2001).

[11] We used the definition followed by the 1970 Census to categorize blue-collar workers, With later occupational codes matched to these definitions through the census "crosswalks" available from IPUMS.

The data also shows (Panel B of Table 1) that wages of blue-collar non-construction workers declined by approximately 30.3% between 1979 and 1993 in the prevailing wage states. However, the wages of blue collar non-construction workers declined approximately by 27.3% between 1979 and 1993 in the repeal states, a difference-in-difference of 3%. While we do not know exactly what caused these changes, clearly these declines are not related to changes in prevailing wage laws. However, they could have affected the construction sector as well. This is precisely why Kesslar and Katz (2001) advocated use of difference-in-difference-difference (DDD). The difference-in-difference-difference (DDD) model found a 3.7% (-0.7%-3.0%) decline in the wages of blue-collar construction workers compared to other blue collar workers in the eight years following the last Mountain State repeal. This estimate is somewhat higher than what Kesslar and Katz (2001) found for the nation (1.3%).

Next, we extended our sample to include the years 1994 to 2002. Our starting date was 1979, and table 2 shows the results. Average wage of a blue collar construction worker declined from $9.55 in 1979 to $7.94 in 2002, or a decline of 17.2%, for the repeal states. Also, during the same period, wages of blue collar construction workers declined by approximately 20.8% (from $10.33 in 1979 to $8.18 in 1993) in the non-repeal states. Therefore the difference-in-difference estimate would suggest that repealing the prevailing wages laws actually increased the wages of blue color construction workers by 3.5%.

Again, data shows (Panel B of Table 2) that the wages of blue collar non-construction workers declined approximately by 25.0% between 1979 and 1993 in the prevailing wage states. The wages of blue collar construction workers declined by about 17.9% between 1979 and 1993 in the repeal states, a difference-in-difference of 7.1%. Clearly these declines are not related to changes in prevailing wage laws. Therefore, the difference-in-difference-difference (DDD) results for 2002, or 17 years after the last changes in the Mountain states, was estimated to be a 3.6% (+3.5%-7.1%) decline in the wages of blue-collar construction workers compared to other blue-collar workers. Notice that this estimate is very similar to the estimate derived for 1993 as the ending point. This suggests that most of the adjustments were already made by 1993.

Next, we use regression framework to control for confounding variables. The dependent variable in these regressions is the natural log of real hourly wage[12] (in 1982 dollars). The demographic controls include the age dummies of the workers (one for 21 to 25 year olds, one for 26-30 year olds, and so on), educational achievement of the workers (less than high school, high school graduate, some college, and college graduate or more), marital status, and race dummies. We also used state fixed effects to control for the fact that states are different from each other, and year fixed effects to capture any secular changes that might have affected the relative differential between construction and non-construction blue collar workers. We further use interaction between construction and state and year effects to capture other changes that were specific to the construction sector. Our regression framework is exactly same as procedures outlined by Kesslar and Katz (2001), except, as noted earlier, we restrict our sample to just the Mountain states and the data was extended to 2002. First, we estimate the following two equations, which are same as equation (1) in Kesslar and Katz (2001).

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

We use the same notation used by them. θ_t represents time fixed effects, α_s represents state fixed effects. C_{ist} is a dummy variable that is one if the individual *i* is working in construction industry in state *s* at time *t*. L_s is one for the repeal states and zero otherwise. A_{st} is another dummy which is one for all the years after repeal and zero before that. For the states that never repealed the law it is always zero. For example, Colorado repealed the prevailing wage law in 1985, therefore for Colorado $A_{st}$is zero from 1979 to 1985 and one for 1986 to 2002. Nevada, on the other hand, never repealed its law, and therefore it is zero for all the years for Nevada. X_{ist}includes all the demographic characteristics.

The coefficient γ_1 shows how the wages changed for all blue collar workers in the repeal states after the prevailing wage law was repealed. The coefficient we are most interested is γ_2 , which shows how the wages changed in construction sector in the repeal states after the prevailing wage law was repealed. Table 3 presents the regression results, with only the

[12] By using the natural log, coefficients obtained through the regression can be interpreted as the percentage change in hourly wage due to a one unit change in the independent variable.

coefficients that are of interest. The first four columns are results that reproduce columns 4-7 of table 3 (page 267) in Kesslar and Katz (2001). The first column presents the results after controlling for demographics and the full set of state*construction, state*year, and year*construction dummies (similar to column 5 of KK). Results show that after repeal blue-collar construction wages decreased by 2.4%, but this result is not statistically significant at any conventional level. Therefore, we conclude that repeal did not affect blue-collar construction wages.

Next, following Kesslar and Katz (2001) we distinguish between short run and long run effects of repeal. For this we estimate the following equation which is same as equation (1a) in Kesslar and Katz (2001).

$$\ln(W_{ist}) = \theta_t + \theta_t^c * C_{ist} + \alpha_s + \alpha_s^c * C_{ist} + L_s * SA_{st}\gamma_{1s} + L_s * SA_{st} * C_{ist}\gamma_{2s}$$

$$+L_s * LA_{st}\gamma_{1l} + L_s * LA_{st} * C_{ist}\gamma_{2l} + X_{ist}\beta + \varepsilon_{ist}$$

Where SA_{st} is one shortly after repeal, and LA_{st} one long after repeal. We use three different definitions of short and long run. The reason for distinguishing between short run and long is that all effects may not be realized immediately. Columns three, four, and five distinguishes between short run and long run effects with different definitions of long run. In column 2, long run is defined as three or more years after repeal (similar to column 6 of KK). The results show that in the short run (i.e. with first two years of repeal) wages in repeal states actually increased by 0.7% but the coefficient is not statistically significant. This is in contrast to in the long run, in which wages decreased by about 3.4%, suggesting that the adjustment process takes some time to materialize. This result is consistent with Kesslar and Katz (2001). In column 3, long run is defined as five or more years after repeal (similar to column 7 of KK). The results show that in the short run (i.e., the first four years following repeal) wages in repeal states decreased by 0.4%, but, again, the coefficient was not statistically significant. However, in the long run wages decreased by about 4.1%. This result shows that the dynamic effects get larger over time, which is also consistent with Kesslar and Katz (2001). Since ex-ante we do not know how long it takes for all the adjustments to take place we add yet another definition of long run. The last column defines long run as ten or more years after repeal (this was not done by KK due to their data restrictions). Our estimates show that, even when the short run is defined as less than ten years,

wages in repeal states decreased by only 1.7%, but, again, the coefficient was not statistically significant. But in the long run, wages decreased by about 4.4%. In other words, dynamic effects are even larger (but not by much) ten years after repeal.

Summary:

- Our study employs data and procedures similar to Kesslar and Katz (2001). The Kesslar and Katz (2001) study was national in focus, while our study only encompasses the Mountain states.
- The Mountain States are Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming. We focus on this region because different regions of the nation have different labor market regulations and economic characteristics.
- Among the Mountain States, four states (Arizona, Colorado, Idaho, and Utah) repealed the prevailing wage law and four states (Nevada, New Mexico, Montana, and Wyoming) did not.
- Given that four states repeal prevailing wage laws, we can treat those states as receiving the treatment, while the other three states are denoted as the control group. This means we can meaningfully compare the experience of repeal states to experience of non-repeal states within the Mountain States.
- Our estimates show that even when the short-run is defined as less than ten years, wages in repeal states decreased by only 1.7%, but the coefficient was not statistically significant. However in the long-run, wages decreased approximately by 4.4%. In other words the long-run effects are larger ten years after the repeal, but not by much.

Discussion

Our discussion shows that there is no consensus on the effect of prevailing wage laws on either public sector construction wages or public construction costs. However, a majority of studies that explore this relationship between the prevailing wage laws and construction wages find a positive correlation. Conversely, a majority of recent studies that explore the relationship between prevailing wage laws and public construction costs report very low to no correlation.

Among the studies that explore the relation between prevailing wage legislations and construction wage the estimates vary, ranging from a high of 23% (Clark, 2005) to approximately 2% (Kesslar and Katz, 2001). One reason behind such variation in results of these studies is that the different studies do not always estimate the same thing in the same way. Some of the studies compare the wages earned by construction workers in projects that are covered by prevailing wage legislation to some form of "market wage," while other studies use econometric techniques to estimate what happens to construction wages when prevailing wage legislation is repealed. The first group of studies usually finds larger impacts than the second group. In our own analysis, which follows procedures by Kesslar and Katz (2001), we found that in the Mountain states repeal of prevailing wage laws leads to only a 4.4% decline in wages in the long run (defined as ten years or more after the repeal). However, we should note that this estimate includes the effect of repeal on hourly wage only, and there is evidence from Peterson (2005) that repeal has larger impacts on benefits compared to wages. Kesslar and Katz (2001), as well as our own analysis, do not address this issue since benefit data is not available in the CPS MORG Data set.

While theoretically possible this apparent dichotomy has not been fully explored. We conjectured that, for example, a contractor might reduce his/her profit margins in response to higher wages. Another issue might be productivity. If, for example, higher wages attract more qualified workers, then some of the wage increase may be compensated by higher productivity. Therefore, it is not obvious (although it is certainly plausible) that higher wages will lead to higher construction cost. Also, it is possible that lower wages may not lower overall construction costs.

Among the studies that explore the relation between prevailing wage legislations and construction cost, the estimates vary as well. A number of studies estimate that the prevailing wage laws increase construction cost by 1% to 3%, while others estimate construction cost is increased by as much as 26.1% (Fraundorf et al, 1984). However, some of the more recent studies suggest that prevailing wage laws have no effect on construction cost.

Phillips (2006) tried to explain this dichotomy. Using 2002 Census of Construction data, Phillips (2006) found that while construction workers in prevailing wage states earn more than construction workers in non-prevailing wage states, they are also more productive. In fact, he found that, nationally, construction workers in prevailing wage states earn about 15% more in

wages and about 25% more in benefits. These estimates are similar to results derived in Pennsylvania by Keller and Hartman (2001). However, Phillips (2006) also found that construction workers in prevailing wage states are 13% to 15% more productive than construction workers in non-prevailing wage states. He further found that *"the fatality rate from construction work-site accidents is 25% lower in states with prevailing wage laws and 35% lower in states with the strongest and best-enforced prevailing wage laws."*

This leads to another issue that was discussed, which is worker training and work related injury. Some studies found that prevailing wage laws facilitates training of construction workers, thereby increasing productivity and reducing work related injuries. Price (2005) found evidence that the general human capital level of the construction workers decline as well following repeal of prevailing wage laws. His estimates show that the odds of a high school dropout working construction relative to non-construction increased by 12% one or more years after repeal, 16% three or more years after repeal, and 21% five or more years after repeal. This suggests that the construction sector attracted relatively more high school drop outs compared to the non-construction sector after the repeal of the prevailing wage law. Price (2005) suggests the attraction of high school dropouts is due to lower wage and benefits that we have discussed earlier. He also found that the high school dropouts who join construction sector after repeal are mostly concentrated in low skilled work such as laborers. The lower amount of human capital may translate into lower productivity and may explain why a reduction in wages after repeal is not accompanied by a reduction in construction cost.

Another possible explanation (not yet fully explored in the literature) may come from the Efficiency Wage Theory. This theory suggests that firms may be able to increase worker productivity by paying workers above the market wage. According to this theory if workers are paid above market wage then

1) Workers are less likely to shirk at work because if they are caught they will be fired. They may not want to put their above market wage job in jeopardy.
2) Workers may work harder as a return gift to employer (gift-exchange model)
3) Above market wage means less quits (and therefore less turnover). Less turnover means less disruption and better trained employees.

Raff and Summers (1987) argue that such a strategy was used by Henry Ford in 1914. They note that in 1913 Ford had a turnover rate of around 370 percent. In addition the absenteeism rate was

about 10 percent. On January 5, 1914, Ford raised the wage of assembly workers from $2.34 to $5.00 (i.e. wages were more than doubled). In addition Ford reduced the workday from nine to eight hours. By 1915, the turnover rate at Ford declined from 370 percent to 15 percent, absenteeism from 10 percent to 2.5 percent. The productivity of workers increased between 40 and 70 percent. The huge increase in productivity meant that even though the wages were more than doubled the profit of the Ford Motor Company increased by about 20 percent.

Our discussion also includes a number of other related issues. One such issue is effect of prevailing wage on racial inequality. Some studies argued that prevailing wage laws puts an unfair burden on minority workers while others have challenged this supposition. Kesslar and Katz (2001) again provided one of the most comprehensive analyses on this issue. They found that repeal eliminates about 60% of the Black-White wage differential in the construction sector. However, they also note that even though relative incomes of blacks increase after repeal, their absolute income does not. In other words repealing the prevailing wage law reduces wages of all workers, but it reduces the wages of white workers more than the black workers, reducing the relative difference.

Some studies go beyond the narrower outcomes such as wage, construction cost etc. and attempt to estimate overall economic impact of repeal of state prevailing wage law. One aspect of these studies is that they try to estimate the loss of tax revenue due to reduced "economic activity" and compare it with any potential savings from repealing prevailing wage laws. A number of these studies conclude that the overall effect on tax revenue is negative, as loss of tax revenue, due to reduced "economic activity," is bigger than potential savings from repealing prevailing wage laws. Again, some studies dispute this conclusion. The following quote from Greenberg et al. (2005) is indicative of overall sense of this literature *"prevailing-wage compliance has positive benefits for income and taxes. Non-prevailing wage construction should create more construction jobs, but these jobs create fewer additional jobs in the economy."*

Table 1: The effect of repealing PWLs on construction wage in the Mountain States (1993 as ending year).

Panel A: Construction			
Construction	Before (1979)	After (1993)	Difference
Repeal sates	9.55	7.54	-0.210
Prevailing wage sates	10.33	8.23	-0.203
Difference-in-Difference			-0.007
Panel B: Non-Construction			
Repeal sates	7.82	5.68	-0.273
Prevailing wage sates	8.47	5.90	-0.303
Difference-in-Difference			+0.03

Table 2: The effect of repealing PWLs on construction wage in the Mountain States (2002 as the ending year).

Panel A: Construction			
Construction	Before (1979)	After (2002)	Difference
Repeal sates	9.55	7.94	-0.172
Prevailing wage sates	10.33	8.18	-0.208
Difference-in-Difference			+0.035
Panel B: Non-Construction			
Repeal sates	7.82	6.43	-0.179
Prevailing wage sates	8.47	6.45	-0.250
Difference-in-Difference			0.071

Table 3: Regression Results: The effect of repealing PWLs on construction wage in the Mountain States.

Repeal state * after repeal	0.0385***			
	(-0.0102)			
Repeal state * after repeal*	-0.024			
construction blue-collar	(-0.0161)			
Repeal state * shortly after repeal		0.00813	0.0107	0.0261***
		(-0.0111)	(-0.009)	(-0.00933)
Repeal state * shortly after repeal*		0.00766	-0.00396	-0.0168
construction blue-collar		(-0.0275)	(-0.0192)	(-0.0164)
Repeal state * long after repeal		0.0480***	0.0608***	0.0680***
		(-0.0108)	(-0.0111)	(-0.0117)
Repeal state * long after repeal*		-0.0338**	-0.0411**	-0.0444**
construction blue-collar		(-0.0161)	(-0.0172)	(-0.0187)
Observations	144,332	144,332	144,332	144,332
R-squared	0.388	0.388	0.388	0.388

Note: This regression controls for state and year fixed effects, construction*year and construction*state interaction terms (which account for changes in the construction sector in a given state or year that are not due to the prevailing wage law), age (in 5 year intervals from 16-64 years of age), educational attainment (less than high school, high school graduate, some college, or college graduate), whether the individual was married or not, black or non-black race, and occupational classification (craftsmen, transport, operatives, or laborers, as defined in the 1970 Census), as well as an interaction term for all demographic variables and a dummy variable indicating whether the observation was from the 1986 CPS or later. These are the same controls used in Kesslar and Katz (2001). The regression was estimated using robust standard errors (in parentheses) to control for heteroskedasticity, and error terms are clustered by state and year.

Significant results are indicated by asterisks (** indicates that the result is significant with $P \leq 0.05$, *** indicates the result is significant with $P \leq 0.01$).

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