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Should Prevailing Wages Prevail? Reexamining the Effect of Prevailing Wage Laws on Affordable Housing Construction Costs

Matt Hinkel, Michigan State University & Dale Belman, PhD, Michigan State University

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Abstract

A current policy debate surrounding state prevailing wage laws is centered on whether they are costly to state governments and taxpayers. The authors contribute to this debate by extending and utilizing data from a 2017 prevailing wage study to replicate and investigate a controversial paper often cited in this debate. The authors examine the relationship between California's prevailing wage law and the costs of building affordable housing, and they do so by going beyond the 2017 study's estimates and estimating a carefully-constructed two-stage model. The authors find no causal effect of prevailing wages on affordable housing construction costs, contradicting previous literature. Further, in a supplemental analysis, the authors examine and highlight key methodological deficiencies present in a prior study. This timely research is designed to contribute to the current prevailing wage debate, which has important implications for researchers, practitioners, and legislators.

Introduction

The availability of affordable housing for U.S. low-income families remains in a state of crisis. Low-income families, those near or below the poverty line face a burdensome shortage of affordable housing. The National Low-Income Housing Coalition (March 2018) found that only 37 available affordable housing units are available per 100 U.S. low-income households. The shortage is especially pronounced in the Western U.S.: in Washington, Oregon, California, Nevada, and Arizona, only 25 affordable housing units are available for every 100 low-income households (Aurand, Emmanuel, Yentel, Errico, & Pang 2018). Part of making additional affordable housing available is keeping the cost of new affordable housing projects low while maintaining quality.

The effect of prevailing wage laws (PW hereafter) on the cost of government construction has been debated since 1891, when the first PW law was passed in Kansas. Passage of the federal Davis-Bacon Act in 1931, the first national PW law, was designed to preserve local construction markets by blocking contractors from using low-wage, out-of-area labor to underbid area contractors on federally funded construction. It was also designed to preserve the wages of construction workers on public projects, which could otherwise be depressed by the distortions induced by public sector bidding requirements, such as the lowest bidder being awarded the contract (Belman & Philips 2005; Duncan & Ormiston 2018).

Following Davis-Bacon, numerous states and cities implemented similar PW laws, known as “little” Davis-Bacon Acts, covering state and local projects. State PW laws have recently been the subject of a spirited policy debate centered on whether they increase public construction costs. For example, after considerable political maneuvering to overcome a gubernatorial veto, Michigan repealed its PW law in June 2018. This was borne out of the argument that repeal will "save hundreds of millions of dollars" because the law was an "antiquated price-fixing scheme" (Gray 2018). Given the current political environment, more repeals of these laws might occur soon. Additional PW research is then timely, because these laws have direct implications for construction workers.

Spurred by recent repeal efforts, there has been substantial growth in scholarly PW literature; policy debates have been informed by this work. Relationships between PW laws and the costs of school construction and highway maintenance projects have been extensively analyzed. The consensus finds that prevailing wage laws do not have significant effects on costs of these projects (for example, see Azari-Rad, Philips, & Prus 2002, 2003; Duncan, Philips, & Prus 2014; Duncan 2015a, 2015b).

The relationship between prevailing wages and the costs of affordable housing projects, however, has received less research attention, consisting of only three prior peer-reviewed, academic studies. More research attention is needed, for two reasons. First, the residential construction industry has growing rapidly since the Great Recession. Residential construction employed about 725,000 workers in the United States as of 2016, 150,000 more than in 2011 (Quarterly Census of Employment and Wages). Therefore, policies that directly affect this industry, such as prevailing wages, now affect a larger number of workers than previously.

Second, although this research is not the focus of this paper, several non-peer-reviewed studies have found that PW requirements raise the cost of affordable housing construction costs. For example, a recent study from New York's Empire Center for Public Policy estimated that PW inflates costs by as much as 25% (McMahon & Gardner 2017). These studies consistently suffer from egregious methodological flaws (see Duncan & Ormiston 2018 and Ormiston, Belman, & Hinkel 2018). Despite such flaws, these studies appear to influence public discourse on PW laws. Given the persistent shortage of affordable housing throughout the country, it is useful to thoroughly investigate the relationship between PW laws and affordable housing construction costs. If PW laws indeed inflate affordable housing project costs, as these studies claim, then these laws could be a contributor to the shortage. The relative dearth of peer-reviewed research on this relationship is therefore a sizable and problematic gap.

This study address this by extending Littlehale (2017), which utilized data from a 2014 State of California-sponsored affordable housing cost study. We examine the relationship between PW laws and affordable housing project costs in California, one of the states that has been hit hardest by the affordable housing shortage. We do so by going beyond the estimates Littlehale (2017) obtained and using a two-stage model that better accounts for endogeneity of prevailing wages. This is designed to ensure that any significant effects on project costs that are found are indeed attributable to prevailing wages. Further, we conduct a supplemental analysis that focuses on reconciling the findings of this paper with the findings of a previous study, which has been widely cited in the public policy debate surrounding prevailing wages.

Background and Literature Review

Alongside the state laws that preceded it, the federal Davis-Bacon Act of 1931 established the concept of prevailing wages in the United States. The act requires contractors and subcontractors working on federally funded or assisted projects exceeding $2,000 to pay their laborers no less than the prevailing wage rates and fringe benefits for corresponding laborers and mechanics performing on similar projects in the same geographic area (U.S. Department of Labor n.d.). The Wage and Hour Division of the U.S. Department of Labor determines the wage rates that are to be paid for federally funded or assisted projects in each area. The PW functions as a locally determined minimum wage for construction workers on public projects. PW rates for specific trades in a given area are based on what the local market wage rates are for similar workers on private projects. Thus, prevailing wage laws are designed to equalize wages paid on public projects with those paid on private projects for similar work. This provides a wage floor for construction workers that is largely consistent between public and private projects in each locality (U.S. Department of Labor 2015).

In addition to Davis-Bacon, which only covers federal projects, numerous states and cities have adopted PW laws covering state and local projects. Currently, 29 states and numerous cities around the country feature such ordinances. The goals of these regional laws differ, ranging from the preservation of local labor standards to ensuring a workforce that is safer and more highly skilled, via the proliferation of apprenticeship training (Duncan & Ormiston 2018). Despite the existence of these regional regulations in most states, legislators in a number of states have recently been engaged in a heated policy debate concerning these regulations. State PW laws have recently been repealed in Indiana, West Virginia, Kentucky and Michigan, and there have been notable challenges to them in states such as Wisconsin and Nevada (Duncan & Ormiston 2018). For example, in 2018, a PW repeal petition in Michigan cleared a major hurdle by receiving enough signatures to be able to be considered by the state legislature. Soon after, the Republican-controlled state legislature repealed Michigan's PW law over the governor's veto threat (Gray 2018).

The construction industry has long served as a pathway to a middle-class life for workers who lack a formal college education and is effective in increasing craft workers' wages and earnings. Kessler and Katz (2001), a definitive study, report that repealing a state's PW law decreases average hourly wages for blue-collar union and nonunion construction workers on both public and private projects by 2 to 4 percent.

In addition to these wage effects, PW laws are associated with increased apprenticeship training. In the construction industry, apprenticeship training is crucial for workers entering the industry to gain the requisite skills and experience to succeed in their careers. Bilginsoy (2005) examined the impact of state PW laws on apprenticeship training between 1989 and 1995 for 36 states, and the author found that PW laws were associated with 6 to 8 percent higher apprenticeship enrollment. Further, the results showed that apprenticeship enrollment was higher as the strength of PW laws increased.

PW laws are also associated with improved workplace safety. Azari-Rad (2005), in an analysis of the construction industry between 1976 and 1999, found that non-fatal injury rates were 7 to 10 percent lower in states that had PW laws. Philips, Mangum, Waitzman, and Yeagle (1995) examined injury rates for plumbers and pipefitters in the United States between 1978 and 1991. The results showed that states with PW laws had lower rates of total workplace injuries compared to states which lacked them. The above results suggest a possible offset on higher labor costs that would otherwise be incurred in the presence of prevailing wages.

The body of peer reviewed literature generally supports the view that PW laws do not raise the cost of public construction. Over the last 20 years, researchers have examined this impact on costs of such construction projects as school construction (e.g., Prus 1999; Azari-Rad, Phillips, & Prus 2002, 2003; Vincent & Monkkonen 2010; Duncan, Phillips & Prus 2014; Kelsay 2016) and highway maintenance (e.g., Vitaliano 2002; Duncan 2015a, 2015b). Of the studies listed above, only Vitaliano (2002)[1] and Vincent & Monkkonen (2010)[2] found significant

[1] However, Duncan (2015a, 2015b) criticized Vitaliano (2002), because Vitaliano's models did not control for such salient variables as the amount of new highway construction ordered and the level of law enforcement staffing. In his two follow-up studies, Duncan (2015a, 2015b) included these variables in his regression models that examined the effect of prevailing wage laws on highway maintenance in Colorado between 2000 and 2011. In doing so, the author found no significant effect.

[2] As Duncan and Ormiston (2018) point out, this finding contrasts with those of Azari-Rad, Philips and Prus (2002, 2003). This is likely due to a key difference in model specification: Vincent and Monkkonen omit a business cycle

positive effects of prevailing wages on school construction and highway maintenance project costs, respectfully. With these exceptions, other peer-reviewed studies in the PW literature on school construction and highway maintenance concludes that PW laws have no significant effects on costs of school construction and highway maintenance projects.

Affordable Housing: A Break from the Consensus

The effect of PW laws on the costs of affordable housing construction has also received attention in scholarly literature, but the attention is comparatively sparse. This could be because California's PW law is the only state PW law that covers both public and private affordable housing projects (Dunn, Quigley, & Rosenthal 2005; DQR hereafter).[3] More attention is needed here, because residential construction as a whole is a rapidly growing industry, employing 125,000 more workers in the United States in 2016 than it did in 2011. Further, residential construction employment increased every year between 2011 and 2016, after sinking to very low levels during the Great Recession.

The workforce in affordable housing also differs from school construction and highway maintenance workforces in that affordable housing projects (and residential projects generally) predominantly utilize nonunion labor (Duncan & Ormiston 2018). As a result, workers on affordable housing projects are, on average, less skilled, less trained, and have reduced access to more advanced equipment and technology (Blankenau & Cassou 2011). It cannot be safely

measure, which Azari-Rad, Philips and Prus include. This is key, because if states with prevailing wage laws also have low unemployment rates, then Vincent and Monkkonen's estimates are biased upward, due to omitted variable bias (Duncan & Ormiston 2018).

[3] As a result of a 2001 amendment to California's prevailing wage law, the law covers affordable housing projects that are funded by both public and private sources, whereas before 2001, California's law only covered publicly funded affordable housing projects (DQR 2005). Private sources are primarily incentivized by the federal Low-Income Housing Tax Credit (LIHTC), which incentivizes private owners to create and maintain low-income housing (National Housing Law Project 2017).

assumed that results for school construction or highway maintenance will generalize to affordable housing construction.

To date, only three peer-reviewed studies have examined the relationship between prevailing wages and affordable housing construction costs. They all utilize California datasets, since California's PW law is currently the only state law in the United States that covers public and private affordable housing projects. The first, a study by DQR (2005), analyzed the construction of 205 affordable housing projects in California between 1997 and 2002. Two different sets of regression estimates were obtained in this analysis. The first set, ordinary least squares (OLS) estimates, showed an estimated effect of PW laws on affordable housing project costs of 9 to 11 percent, a statistically significant effect. DQR (2005) utilized extensive OLS models, one of which featured the natural log of total project costs as the dependent variable; controls included the number of units in a project, the fraction of units meeting affordability guidelines, the populations targeted by each project, the developer in charge of a project (e.g., for-profit and non-profit), and the structure type (e.g., two-story, townhome, single-family). The authors report an effect of 9.7 percent.[4]

The second set of estimates, two-stage least squares (2SLS) estimates, estimated a statistically significant cost effect of 19 to 37 percent (DQR 2005). In the first stage, two models were run: the first model included all project characteristic control variables from the OLS models, with the exception of the number of units in each project. The second model added in

[4] It is well established that the residential sector is characterized by extensive misclassification, off-the-books employment and other forms of wage theft as well as violations of other employment laws (see Ormistion, Belman, Brockman & Hinkel forthcoming). PW laws carry the requirement of weekly filing of certified payrolls with an oversight body. This acts to limit contractors' freedom to engage in the employment practices that characterize much of residential construction. The enforcement of labor and employment law carries costs beyond directly increasing wages, including paying unemployment insurance, workers' compensation and the employer share of Social Security. Several industry experts, including building trades union officials, have expressed surprise to the authors that the estimated effect of the residential PW law raised construction expenses by *only 9 to 11*%.

the number of units as an additional control. First-stage instruments used in both models included the fraction of yes votes on various California propositions in the 1990s, the fraction of voters registered as Democrats, the percentage of the population over 40 years old, the percentage of affordable housing project workers in highly unionized industries and occupations, and the fraction of workers who were unionized (DQR 2005).

In the second peer-reviewed study examining this relationship, Littlehale (2017) examined 321 affordable housing projects in California between 2001 and 2011. Littlehale applied a more extensive OLS regression model than that of DQR (2005), and the author found a lower, but still statistically significant, estimated effect of PW laws on affordable housing construction costs of 5 to 7 percent. Third, Palm and Niemeier (2018) examined housing projects built between 2008 and 2016 in California's four largest metropolitan areas, and the authors find significantly positive cost effects of 15 to 16 percent. Based on these three studies, affordable housing construction seems to be the only type of construction researched thus far for which PW laws significantly increase costs.

This seems plausible, for two reasons. First, each of these studies took place in the state of California, a state with one of the strictest PW laws (Duncan & Ormiston 2018; Belman, Ormiston, Petty, & Hinkel 2019). If PW laws affects housing costs, it should be most apparent in California. Second, the largely nonunion workforce in residential construction is, on average, less trained and less skilled than its union counterpart (Blankenau & Cassou 2011). The effect of the wage floor established by prevailing wages is less likely to be offset by higher worker productivity, quality of work, improved safety outcomes or faster project completion. Absent these offsets, which may characterize school and highway construction, PW requirements are more likely to increase labor and total project costs.[5]

On the other hand, unresolved methodological issues present in previous literature warrant further exploration. As Littlehale (2017) pointed out, the 2SLS models employed by DQR (2005) potentially suffer from serious methodological issues. The instruments were not strong; of the 17 instruments, only 2 of them were significantly related to whether projects required payment of prevailing wages: the fraction of yes votes on California Propositions 160 and 167 in 1992. These variables were barely significant in a 10% test. The relevance requirement for instrumental variables estimation (i.e., that each instrument is statistically significant, *and* the instruments are sufficiently jointly significant) was not met, because the joint F statistic for instruments was less than 3, well below the value of 11.5 required with more than 15 first-stage instruments (Stock, Wright, & Yogo 2002).

The other requirement for instrumental variables, the exclusion restriction, was not adequately addressed in DQR (2005). By definition, in this case, the exclusion restriction says that all first-stage instruments used are required to not have any direct causal effect on project costs. Rather, the instruments can only impact project costs via the explanatory variable of interest, prevailing wages. Although the exclusion restriction cannot be proven empirically, justification must be provided for why first-stage instruments are likely to satisfy the exclusion restriction (Wooldridge 2013). The authors state that their instruments have no direct effect on construction costs (DQR 2005, p. 150), but do not explain this assertion. Further, the authors do not test for prevailing wage endogeneity; thus, they provide no evidence that their 2SLS estimates are improvements over OLS. It is then the case that the instruments utilized by DQR

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[5] Lower costs might also be an outcome of violations of employment and labor laws. See Ormiston, Belman, Brockman and Hinkel (forthcoming) on the extent of these issues in residential construction.

were weak, and weak instruments can lead to large inconsistencies and bias in instrumental variables estimates (Wooldridge 2013). Thus, greater confidence should be place in the DQR OLS estimates of a 9 to 11 percent effect.

These methodological issues, such as the weak first-stage instruments used in DQR (2005) and potential prevailing wage endogeneity, could be contributors to the significant effects that have been found and could be sources of bias. The issue of endogeneity will be discussed in greater detail in the next section. Given these issues, an explicit hypothesis is not sensible for this paper. Rather, we propose the following research question:

Research Question*:* What is the causal effect of California's prevailing wage law on affordable housing project costs?

Methods

Data and Sample

Littlehale (2017)'s dataset was originally developed in 2014 for an affordable housing cost study commissioned by the state of California. It consists of 356 affordable housing projects that were built between 2001 and 2011. Of these projects, 233 were subject to PW requirements, 133 of them were not. The projects were located in 153 cities, suggesting that this sample captures variation between construction in different areas of the state.[6]

The justification for extending Littlehale's study and utilizing 2SLS estimation lies in a DQR (2005) recognition that:

[6] In order to obtain access to this dataset, we signed a limited non-disclosure agreement with Blue Sky Consulting Group, the group that originally compiled the dataset. Once this signed non-disclosure agreement was received by Blue Sky, Scott Littlehale graciously provided the data.

"If projects located in higher-cost areas (for example, in highly urbanized areas) were more likely to be required to pay prevailing wages…then simple ordinary least squares regression models would falsely attribute these higher costs to the payment of prevailing wages" (DQR 2005, p. 149; Littlehale 2017, p. 124).

This scenario is a classic example of endogeneity (i.e., when a variable is significantly correlated with the error term in the regression). Intuitively, a variable is endogenous when it is significantly related to variables (either observable or non-observable) that are not included in the model. This endogeneity problem leads to significant effects on a dependent variable being incorrectly attributed to that endogenous variable. In this case, if prevailing wages are endogenous, we are faced with a causality problem: even if a statistically significant OLS correlation between PW and project costs is found, there may not be a *causal* effect of PW on project costs, due to endogeneity. This implies an upward bias in the OLS estimate (Wooldridge 2013). Thus, in the presence of possible PW endogeneity, care should be taken by researchers to ensure that any effects of prevailing wages on project costs can be correctly attributed to prevailing wages. That is, exploring potential endogeneity allows us to isolate any true causal effects of prevailing wages.

Littlehale (2017) only uses OLS and does not use methods, such as 2SLS or IV, to address endogeneity. Littlehale's OLS estimates may then be biased upward or downward by endogeneity. To his credit, Littlehale recognized this gap and called on researchers to use other estimation procedures to explore endogeneity.

Measures

Table 1 provides a complete list of the variables that will be used in the analysis, along with the definitions of each variable. Following Littlehale (2017), our dependent variable is *log Project Cost,* the natural log of total affordable housing project development costs exclusive of land costs. The log form allows the coefficient to be interpreted as a percentage change for a one-unit change in an explanatory variable. Following Littlehale (2017) and DQR (2005), the explanatory variable of central interest is *Prevailing Wage.* This has a value of one if a project was subject to state PW requirements and zero if a project was not subject to these requirements.

[[Table 1 near here]]

The controls fall into three broad groups: variable drivers of affordable housing construction costs, fixed drivers of costs, and other controls. The first set involves drivers of affordable housing construction costs that are variable in nature. These include developer variables, *Developer Type* and *log Developer Employees,* which capture whether the project developer is the government, nonprofit, or another type and the number of workers employed by the developer, the party ultimately responsible for completion. *Community Meetings, Permit and Impact Fees, and log Permit and Impact Fees per Square Foot (SF)* control for effect of additional state regulations. These capture costs of obtaining building permits and dealing with regulations inherent in that process; these factors are expected to be positively related to total costs. Site and structure variables capture the cost effects of affordable housing structures and the sites on which they are located. These site and structure variables are *log Building Area, Parking-to-Rentable Building, SF Non-Residential and Non-Parking, Number of Stories,* and *Residential SF Per Unit.*

The second general set of controls involves drivers of affordable housing construction costs that are fixed. These variables include *Average Specialty Contractor Pay, Project Snow,* *Project Region, and Project Year.* The region in which a project is built and year when it began influences costs, because there may be certain characteristics of specific regions and years that distinguish them from others. For instance, costs are affected by price inflation, the business cycle and the regional demand for construction labor. Finally, snow depths are likely to influence costs, because they influence a project's design as well as shorten the window of time for completion of various stages of construction (Littlehale 2017).

Additional controls include: *log Lenders*, because a project's costs are affected by the number of loans that finance it; *Project Duration,* because longer duration is associated with greater costs; and *log Architect and Engineer (A&E) Cost per SF,* which is a benchmark A&E estimate of project cost per square foot. This variable is included because prevailing wage requirements could push costs above this benchmark. The OLS model we estimate is:

$$\text{(1)} \quad \log y_i = \beta_0 + \beta_1 Prevailing\ Wage_i + \boldsymbol{\beta_2 V_i} + \boldsymbol{\beta_3 F_i} + \boldsymbol{\beta_4 X_i} + \mu_{i,}$$

where y_i = project cost, V_i is a vector of variable project cost drivers, F_i is a vector of fixed project cost drivers, and X_i is a vector of other project characteristics, with each variable indexed by construction project.

The 2SLS estimation utilizes two variables, *Community Development Block Grant (CDBG) Funding* and *Density Bonus,* as instruments for *Prevailing Wage.* The CDBG program is a state of California grant program that, among other activities, is designed to at least partially fund the construction of affordable housing units. Any non-entitlement jurisdiction[7] that does *not* also participate in the equivalent federal CDBG program administered by the Department of Housing and Urban Development (HUD) is eligible for state of California CDBG funding.

[7] Non-entitlement jurisdictions are defined as either (1) cities with populations under 50,000 that are not principal cities of Metropolitan Statistical Areas or (2) counties with populations under 200,000 (California Department of Housing and Urban Development 2018).

Funding is therefore geared toward smaller and more rural communities that otherwise may lack access to other resources, and the likelihood of receiving funding is independent of the expected costs of a particular project.[8] Thus, it does not seem to be the case that CDBG grants are more likely to be awarded to large projects with larger costs, which provides evidence of the likely satisfaction of the exclusion restriction.

Further, construction projects making use of state CDBG funds are subject to prevailing wage requirements, aside from the usual exceptions (California Department of Housing and Community Development 2018). It is then likely the case that state CDBG funds will only impact costs via the explanatory variable of interest, *Prevailing Wage.* Also, this means the relevance requirement (i.e., that this instrument, *CDBG Funding.* be partially correlated with whether a project pays prevailing wages), is likely met. Thus, *CDBG Funding* should be a valid instrument for *Prevailing Wage.*

California's density bonus law, like the CDBG program, is designed to spur the development of a range of projects, including affordable housing. It allows housing developers to obtain more favorable development regulations in exchange for offering to build more housing on a site or donating land for that purpose, and it allows for up to a 35% increase in housing unit densities on a given site. Of note here is that the relaxation of prevailing wage requirements is *not* among the more favorable regulations that developers can negotiate for (Goetz & Sakai 2017). It is then likely that the relevance requirement is met.

The exclusion restriction should also be met: eligibility is independent of the project size and, hence, its costs. Eligibility is based on how housing units are utilized and the specific

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[8] This is evidenced by the wide variance of grant awards for housing projects that took place in the 2017-2018 fiscal year. In 2017-2018, the city of Pacific Grove, CA was awarded $390,854 in state CDBG funding for a housing rehabilitation project, whereas the city of Arcata, CA was awarded $1,767,442 for a similar, much larger project (California Department of Housing and Community Development 2018).

vulnerable populations they are geared toward. Further, density bonuses are inherently exogenous; as Goetz and Sakai (2017) point out, developers who meet the requirements of the density bonus law are "entitled to receive the density bonus and other benefits as a matter of right" (Goetz & Sakai 2017, p. 2). Therefore, provided that the requirements are met, the density bonus approval process is automatic and independent of other factors. California cities and counties are *required* by law to grant density bonuses to projects which meet at least one out of a specified set of guidelines, which include the following (Goetz & Sakai 2017):

- At least 5% of units are reserved for very low-income residents.
- At least 10% of units are reserved for low-income residents.
- At least 10% of units are geared toward foster youth, disabled veterans, or homeless persons, with subsidized monthly rents that reflect very low-income levels.

Further, it is unlikely that California's density bonus law has any direct effects on project costs. Any costs that may otherwise be incurred from increased unit densities on a given site should be canceled out by the relaxation of regulations such as minimum square footage requirements and parking requirements. As a result, cost impacts of density bonuses are likely to only occur via the payment of prevailing wages; thus, the exclusion restriction should be satisfied. Since the relevance requirement and exclusion restriction should be met, *Density Bonus* should be a second valid instrument for *Prevailing Wage.*

The two-stage least squares model is then:

(2) $\log y_i = \beta_0 + \beta_1 \mathit{Prevailing\ Wage}_i + \boldsymbol{\beta_2 V_i} + \boldsymbol{\beta_3 F_i} + \beta_4 X_i + \mu_i,$

(3) $\mathit{Prevailing\ Wage}_i = \beta_0 + \beta_1 \mathit{CDBG\ Funding}_i + \beta_2 \mathit{Density\ Bonus}_i + \boldsymbol{\beta_3 V_i} + \boldsymbol{\beta_4 F_i} + \boldsymbol{\beta_5 X_i} + \mu_i,$

where equation (2) is the reduced-form, second-stage equation, and equation (3) is the first stage. As before, y_i = project cost, V_i is a vector of variable project cost drivers, F_i is a vector of fixed project cost drivers, and X_i is a vector of other project characteristics, with each variable indexed by construction project.

We initially replicate the two OLS models utilized in Littlehale (2017), and then estimate a more parsimonious model where we eliminate control variables that are highly collinear. We then estimate the OLS model given by equation (1), where we include variables that are more continuous in nature. Third, as is customary with 2SLS, we conduct a first-stage regression of *Prevailing Wage* on the proposed instruments, *CDBG Funding* and *Density Bonus,* including all controls. This is given by equation (3). If the relevance requirement is met (i.e., $p < .05$ for *CDBG Funding* and *Density Bonus*), we then conduct a joint significance test.

If the two instruments are jointly significant ($F > 10$, Stock and Yogo 2005; Wooldridge 2013), we then estimate the second stage regression, equation (2). We regress the dependent variable, *log Project Cost,* on all proposed controls, and use *CDBG Funding* and *Density Bonus* as instruments for the independent variable, *Prevailing Wage*. Lastly, following this 2SLS estimation, we conduct statistical tests for endogeneity. Statistically significant test values help to ensure that 2SLS estimation is both consistent and asymptotically more efficient than OLS.

Results

Models 1-4 of Table 2 presents four different variations of ordinary least squares regression models that build on Littlehale (2017). Across all four specifications, the results are consistent with Littlehale (2017): the presence of prevailing wages is associated with a 5 to 7 percent increase in affordable housing project costs. Models 1 and 2 are replications of Littlehale (2017). Following Littlehale (2017), region and year dummies are included in Model 1, whereas they are not included in Model 2. As expected, these replications produce equivalent OLS estimates to those of Littlehale (2017). In Model 3, we eliminate control variables from the analysis that were highly collinear. We include region and year dummies in this regression, making Model 3 a more parsimonious version of Model 1. We find that, all else equal, the presence of prevailing wages is associated with an estimated 5.8% increase in affordable housing project costs, a statistically significant effect.

[[Table 2 near here]]

Model 4 is similar to Model 3, except that it substitutes continuous variables for some of the indicators in Model 3. Specifically, *Meetings 4 Plus* is replaced with *Community Meetings, Stories 4 Plus* with *Number of Stories,* and *Duration 24 Plus* with *Project Duration.* This substitution better guards against omission of important sources of variance. For example, *Stories 4 Plus,* by definition, assumes that affordable housing units with three stories are just as costly to complete as units with only one story. Likewise, *Duration 24 Plus* assumes that a project requiring 6 months to complete is as expensive as a project requiring 18 months to complete. Therefore, Model 4 is our preferred OLS model. Here, we find that the presence of prevailing wage requirements is associated with an estimated 6.7% increase in affordable housing project costs.

The left-hand column of Table 3 presents the first-stage regression given by equation (3), featuring *CDBG Funding* and *Density Bonus* as potential instruments for *Prevailing Wage.* This model includes all controls from the preferred OLS model, Model 4 of Table 2. We find that *CDBG Funding* and *Density Bonus* are both statistically significant at the 1% level. Further, the joint F statistic for *CDBG Funding* and *Density Bonus* was 15.61, above the required threshold (Stock & Yogo 2005). The relevance requirement is met.

[[Table 3 near here]]

The right-hand column of Table 3 presents the results of the second-stage regression, using *CDBG Funding and Density Bonus* as instruments for *Prevailing Wage.* This also incorporates the Model 4 (Table 2) controls. The key finding is that the PW variable has no statistically significant causal effect on affordable housing project costs. The 2SLS estimate of PW (β = -.066) has the opposite sign of the OLS estimates and is no longer statistically significant.

Further, tests of the two equation specification support the validity of the current specification. Specifically, these tests determine whether the 2SLS estimate is consistent and whether it is more efficient than OLS. In more intuitive terms, these tests examine whether the presence of prevailing wages on a project is at least partially determined by other variables not included in equation (1) (i.e., whether prevailing wages are endogenous). The values for the robust score χ^2 and robust regression F statistics were 3.322 (p = 0.068) and 2.966 (p = 0.086), respectively (Table 4). The statistical significance of these two tests, albeit only at the 10% level, indicates that prevailing wages are endogenous. Thus, 2SLS is the consistent and asymptotically more efficient estimator. The non-statistically significant 2SLS estimate in Table 4 is preferred; the key result is that California's prevailing wage law has no significant causal effect on affordable housing project costs.

A supplementary test of overidentifying restrictions was also conducted following 2SLS estimation, because the two-stage model used in this paper is overidentified. By definition, an overidentified model is a model that features more instruments than endogenous explanatory variables; in this 2SLS model, there are two instruments (*CDBG Funding* and *Density Bonus),* with only one endogenous explanatory variable (*Prevailing Wage*). Because the model is overidentified, we must test whether having a second (i.e., overidentifying) instrument for *Prevailing Wage* is empirically valid. A statistically significant test value provides evidence that using a second instrument for *Prevailing Wage* is likely invalid. The value of the χ^2 test of overidentifying restrictions was 0.644 (p = 0.422). Since this is far from statistically significant, this test gives no indication that the overidentified model was problematic, and it allows readers to be more confident in the effectiveness of the two-stage model and its result. Thus, the answer to the research question, in line with the literature on schools and highways, is that California's prevailing wage law does not have a significant causal effect on affordable housing project costs.[9]

Reconciling Current and Prior Research

What remains is identifying the sources of the differences between DQR's estimates and ours, whether these are due to differences in data, specification, sample or estimation techniques. As DQR's data has been lost, we update our current data with variables similar to those used by DQR and systematically move from their preferred specification to ours. An obvious difference between DQR's work and ours is their usage of first-stage instruments measuring the fractions of yes votes on eight California ballot propositions; these are used to capture monetary and local political effects. We replicate these instruments with election data for similar propositions from 2002-2010 ballots.[10] These data was merged with the existing Littlehale (2017) dataset by FIPS

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[9] As a robustness check, we also conducted a supplemental 2SLS analysis using the control variables in Model 1 of Table 2. This is done to ensure that the key result of this paper is not dependent on model specification. The result does not change. we still find no statistically significant causal effect of prevailing wages on affordable housing project costs, with a similar coefficient (β = -.08). I also find statistical evidence of endogeneity. The joint F statistic of *CDBG Funding* and *Density Bonus* in the supplemental first-stage regression was 12.24, which is lower than in Table 3. However, this is likely due to higher multicollinearity: the mean Variance Inflation Factor (VIF) for the supplemental first-stage regression was 3.67, whereas the mean VIF for the first-stage regression in Table 3 was only 2.49, indicating lower multicollinearity. Thus, there is no evidence of this paper's result being dependent on specification. Full results of this supplemental analysis are available from the author upon request.

[10] This was obtained from the Office of the California Secretary of State.

county code. The regional variables were modified to parallel DQR's work. Although not all of DQR's controls could be reproduced, our version of DQR's specification is reasonably similar.

Before moving to two-stage models (Table 4), we estimate DQR's OLS specification to determine if our estimates are similar to those of DQR (Model 5, Table 2). This model features a majority of the DQR controls with variables for housing type (*Non-Targeted, Senior, SRO, Special Needs*), building characteristics (*Townhouse, Single Family, Three Stories, Number of Bedrooms*), and their preferred regional controls; some measures of site, building, construction and finance characteristics were unavailable in our data.[11] The estimated prevailing wage effect is 17.8%, nearly twice the 9.7% OLS effect reported by DQR (2005), and three times the estimated effects reported in Models 1 – 4 in Table 2. The replication is then similar in sign and significance but with a considerably larger PW effect.

We then reproduce DQR's two-stage specification with our enhanced data and explore the sources of differences between DQR's estimates and ours. Table 4 summarizes the essential estimates from these replications (PW estimates and standard error, estimates of instruments and standard errors, F-tests of the significance of the instruments and of PW endogeneity).[12] Model 1 replicates DQR by using their preferred instruments and regional controls; Model 2 is similar to Model 1, except that it shifts to our preferred geographic controls; Model 3 adds the year indicators; Model 4 adds our preferred instruments to those used by DQR; Model 5 drops DQR's instruments.[13]

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[11] We use the full set of regional controls but do not have measures of affordability, the occupancy date, the fraction of financing provided from public sources, and whether the project was built on an inner city infill site. Only one project in the Littlehale dataset was built on an island, so that variable was intentionally left out of the analysis.

[12] The complete estimates are available from the corresponding author.

[13] It was not possible to include DQR's measures of union density or the proportion of employment in highly unionized industries and occupations by CMSA/MSA included in their first stage model (see DQR footnote 14 and Table 4). We recreated these variables using current data from Hirsch and MacPherson (2003) but determined that they were highly collinear with other variables in our current sample. It was likewise not possible to estimate the exact replication of DQR (Model 1) with the union density measure. It was possible to obtain estimates for union

[[Table 4 near here]]

In Model 1, the approximate replication of DQR, the PW estimate in the second-stage model is larger in magnitude than DQR (β = 0.463) and is statistically significant. Only two of the first-stage instruments are statistically significant; thus, their joint F-statistic is far from significant. The endogeneity of PW in the second stage is also rejected.[14] The results are therefore consistent with DQR's work, albeit with a higher PW estimate.[15]

Model 2 is similar to Model 1 in using the DQR instruments, but it eliminates DQR's regional variables and replaces them with ours. The estimate of the PW effect is larger (β = 0.564) and statistically significant. However, only three of the eight first stage instruments are significant and a group F-test on the instruments does not reject the null. As the relevance criteria are not met, the second-stage estimates may be biased upward. However, evidence of PW endogeneity is found, suggesting our regional variables fit the data better than those of DQR.

Model 3 adds year effects to Model 2. The PW coefficient is smaller (β = 0.379) but remains highly significant. As was also the case with Model 2, only three of the instruments are statistically significant, the instruments fail the F-test of group significance, but strong evidence of endogeneity is found. Model 4 adds CDBG Funding and Density Bonus, the instruments in our preferred model. Both of these instruments are highly statistically significant, and as a result, a test of group significance for the instruments rejects the null. The PW coefficient remains

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density with our preferred model (Model 5), although inclusion resulted in five other variables and 43 observations being dropped.
Union density had a large negative effect on the presence of prevailing wages in the first stage; in the second stage, the estimated prevailing wage effect was -0.042 but not significant. The null that the instrument coefficients were zero was rejected, and the null that prevailing wages were not endogenous was not rejected. It appears that changes in the samples between DQR and our work resulted in much greater collinearity in our work. Further, it is not apparent that there were any gains from inclusion of the variables where possible.

[14] These measures were obtained from the supplemental California Secretary of State data.

[15] The larger magnitude of the PW effect in these intermediate estimates may be due to the omission of the union density and highly unionized industry and occupation variables discussed in footnote 11.

large, positive and statistically significant (β = 0.261). Further, as in the previous two models, evidence of PW endogeneity is found.

Finally, Model 5 summarizes the results from Table 3 for our two-stage model, the model with only our preferred instruments. This model meets each of the criteria for assessing a two-stage model: both instruments are individually significant and soundly reject a joint test of the null, and PW is indicated to be endogenous. The PW variable is not statistically significant in the second-stage equation (β = -0.066). It is then reasonable to conclude that DQR's finding of a large positive effect of prevailing wages on affordable housing construction costs was the result of inflation of the PW estimate associated with the usage of weak instruments.

This section began with the question of why the results of this paper differ from DQR (2005) and whether DQR's estimated effects of prevailing wages might be biased upward.[16] First, as Model 3 shows, DQR's omission of project year fixed effects leads to inflated estimates because the business cycle and differences between years in the tightness of construction markets explain a substantial amount of variance in project costs. Second as Model 4 shows, the mix of instruments DQR (2005) used was weak. The addition of our two instruments in Model 4 of Table 4 raises the F statistic of the instruments in the first stage and leads to a smaller estimated effect of prevailing wages on project costs than in Model 3. Third, the mix of control variables used in DQR (2005) were not optimal, as they also likely contributed to the upward bias in the prevailing wage estimates. Alongside omitting DQR's prevailing wage instruments, substituting DQR's controls for the Littlehale (2017) controls used in this paper results in a much lower estimated effect of prevailing wages on project costs in Model 5 of Table 4. In this section, we

[16] Although three of the first four models in Table 4 feature evidence of prevailing wage endogeneity, DQR (2005) neglect to investigate endogeneity in their paper.

have shown that the specification utilized in this paper is an improvement over the specifications used in DQR (2005).

Discussion

The key finding of this paper is that California's prevailing wage law has no causal effect on affordable housing project costs. That makes this paper the first in the prevailing wage literature to find no such causal effect. Affordable housing construction is the only subindustry of construction for which there has been substantial peer reviewed empirical evidence of significant cost effects. This seemed plausible on its face, as residential construction is mostly nonunion, with a workforce that has lesser skills and training than those of school construction and highway maintenance (Duncan & Ormiston 2018). Imposing a wage floor on businesses who typically do not face wage constraints and typically provide low wages and bad working conditions makes affordable housing a leading candidate for significant cost effects of prevailing wages (Ormiston, Belman, Brockman, & Hinkel 2019). However, the weak first-stage instruments featured in DQR (2005)'s 2SLS models and the potential for endogeneity-induced bias present in Littlehale (2017)'s OLS models warranted further empirical examination. The finding of no causal effect of prevailing wages on affordable housing project costs brings this paper in line with the majority of peer-reviewed prevailing wage literature on schools and highway maintenance that find no effect of prevailing wages on costs.

This paper provides a two-pronged empirical response. First, it responds to the claim by DQR (2005) that prevailing wages inflate affordable housing construction costs by as much as 37%. DQR used weak first-stage instruments; of 17 first-stage instruments that were used for *Prevailing Wage*, only 2 were significantly partially correlated with *Prevailing Wage,* making it likely that their estimates suffer from an upward bias. Further evidence is found in a joint F statistic of the instruments of under 3, well below any conventional standard for inclusion. In contrast, this paper makes use of two instruments for *Prevailing Wage* that were (a) significantly partially correlated with *Prevailing Wage* and (b) had a joint F statistic that was above conventional thresholds. Further, DQR (2005) provide insufficient discussion of whether instruments satisfy the exclusion restriction; this paper provides explicit justification for why *CDBG Funding* and *Density Bonus* should satisfy the exclusion restriction. The usage of empirically stronger instruments in this paper results in a finding that is in sharp contrast to that of DQR (2005).

Second, this paper responds to Littlehale's call to explore estimation procedures other than OLS and the possibility that prevailing wages are endogenous. In this analysis, we find that prevailing wages are endogenous in some specifications, lending credence to the notion of OLS bias and providing rationale for 2SLS estimation. Thus, this paper furthers Littlehale's work by reducing bias in the estimated effect of prevailing wages on affordable housing project costs and ensuring that any significant effect on costs that are found can confidently be attributable to prevailing wages. In doing so, no significant causal effect is found.

This paper's findings are significant for construction researchers, practitioners and legislators. For researchers, they highlight the importance of using thoughtful approaches to empirical methodology. Non-peer reviewed studies on affordable housing routinely assert that prevailing wages inflate project costs by as much as 25%. Their methodological flaws have been discussed by Duncan & Ormiston (2018) as well as Ormiston, Belman, & Hinkel (2018). The very large cost effects reported by DQR (2005), with prevailing wages inflating costs by as much as 37%, seem to be a product of weak instruments and a weak specification. Advances in understanding and tests for endogeneity in two stage models have allowed us to both formulate a stronger statistical model and examine the weaknesses of prior work. In this paper, we utilize an extensive two-stage model explaining nearly 95% of the variance in project costs that uses two instruments for *Prevailing Wage*, which meet thresholds for relevance as instruments and are justified as likely satisfying the exclusion restriction.

For construction practitioners and legislators, this finding builds on a key idea pervasive in the majority of peer-reviewed prevailing wage studies: prevailing wage laws are not as harmful and costly to the construction industry as is claimed in some non-peer reviewed studies. The findings of much of the peer-reviewed literature support the idea that any increase in direct labor costs due to prevailing wages is offset by factors such as higher worker skill, increased apprenticeship training, higher workplace safety, reduced labor turnover, and other workplace factors, which prior research has shown to be correlated with the presence of prevailing wages (e.g., Philips et al. 1995; Azari-Rad 2005; Bilginsoy 2005). These factors can reduce, or even eliminate, causal effects of prevailing wages on project costs. We also suggest, based on the research of ourselves and others on working conditions in residential housing, that the OLS estimates of the prevailing wage variable is as likely to proxy for the enforcement of labor and employment law and social insurance law on PW projects as the direct cost of higher wages.

In this paper, we make a strictly empirical contribution to knowledge of the cost impacts of prevailing wages. Responding to a peer-reviewed article by DQR (2005), which found that PW laws had large cost effects on affordable housing, we find direct empirical evidence of no effect. We do so by building on the work of Littlehale (2017) and using a much stronger two-stage model than DQR (2005); this model eliminates any significant causal effects. The findings of this paper provide evidence that PW laws accomplish their goal of providing consistent wage floors for construction workers without imposing burdensome economic impacts. Further, this paper provides no evidence these laws are contributing to the affordable housing shortage by raising housing costs to unsustainable levels. This lends credence to the notion that prevailing wage laws can, and should, prevail.

Conclusion

This study has limitations. First, as is the case with any paper that uses two-stage models, its credibility depends on whether the exclusion restriction is indeed satisfied for each of the *Prevailing Wage* instruments. The exclusion restriction can never be proven empirically; rather, explicit plausible justification must be provided for why the exclusion restriction is reasonably satisfied. This justification for each of the two instruments has been laid out clearly and, hopefully, convincingly.

A second limitation is that our sample comes only from California. It would be desirable to conduct this research in a state other than California and bring a new research setting to this literature. California is, however, the only state with a PW law that covers both public and private affordable housing projects. It is then the only state where these studies can be conducted. If additional states extend their PW laws in a fashion similar to California, it would be possible to determine whether our findings are specific to California or can be generalized.

The affordable housing crisis makes further work on this topic timely. Recent research on residential construction indicates it provides poor wages, benefits and conditions of work, well below those established by U.S. labor and employment law. Prevailing wage laws are tools that provide better working conditions; finding that there is no straightforward trade-off between better labor standards and project costs should encourage a greater ease of using this tool.

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Table 1: Variable Definitions

Variables	Definitions
Log Project Cost	Natural log of total project cost (excluding land)
Prevailing Wage	=1 if project required payment of prevailing wages, =0 if not
CDBG Funding	=1 if a project received funding from the Community Development Block Grant program, =0 if not
Density Bonus	=1 if a project was incentivized by California's density bonus law, =0 if not
Developer Type	Nonprofit, Government, Other (all dummies)
Developer General Contractor	=1 if the developer was an in-house general contractor, =0 if not
Log Developer Employees	Natural log of each developer's number of employees
Meetings 4 Plus	=1 if more than 4 community meetings occurred during a project, =0 if not
Community Meetings	=1 if at least 1 community meeting occurred during a project, =0 if not
Permit and Impact Fees	Sum of permit fees and impact fees
Log Permit and Impact Fees/SF	Natural log of the sum of permit and impact fees, per square foot
Funding-Redevelopment	=1 if a project was a redevelopment project, =0 otherwise
9% Tax Credit	=1 if a project's developer received a 9% tax credit, =0 if not
Log Lenders	Natural log of the total number of loans that finances a project
Log Building Area	Natural log of gross building area in square feet, excluding parking
Structure Includes Parking	=1 if an affordable housing structure included parking, =0 if not
Parking-to-Rentable Building	Ratio of area of structured parking to gross area of building, minus parking
Sq. Ft. Non-Residential: Non-Parking	Percentage of total square feet that is non-residential, minus parking
Stories 4 Plus	=1 if more than 4 stories, =0 otherwise
Number of Stories	Number of stories in a given unit
Residential SF per Unit (100s)	Gross area of parking, community, common, and commercial divided by total units (100s of SF)
Log Site in Acres	Natural log of the size of an affordable housing project site, in acres

Includes Non-Residential Area	=1 if an affordable housing site included a non-residential area, =0 if not
Log A&E Cost per SF	Natural log of the sum of architect and engineer fees divided by building area, excluding parking
Duration 24 Plus	=1 if a project took at least 24 months (2 years) to complete, =0 otherwise
Project Duration	Length of time required to complete a project, in months
Average Specialty Contractor Pay	Average pay for specialty trade contractors, by county, as a percentage of state average in project year
Log Fair Market Rent (2-Bedroom)	Natural log of the HUD-established going rental rate for a 2-bedroom affordable housing unit
Project Snow	City's average January snow depth in 2017
Project Region	Regions: Central, Rural, Capital/Northern, North & East Bay, South & West Bay, San Francisco, Central Coast, Inland Empire, Los Angeles, San Diego
Project Year	Years: 2002-2010

Table 2: Ordinary Least Squares Estimates (Dependent Variable: Log Project Cost)

Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Prevailing Wage	**0.050****	**0.065*****	**0.058****	**0.067*****	**0.178*****
	(0.024)	(0.021)	(0.024)	(0.025)	(0.048)
Log Units					**0.775*****
					(0.061)
Nonprofit Developer	**0.032**	**0.040***	**0.046****	**0.045***	**0.019**
	(0.022)	(0.021)	(0.022)	(0.023)	(0.037)
Government Developer	**0.130***	**0.142*****	**0.155*****	**0.151*****	**0.066**
	(0.052)	(0.050)	(0.048)	(0.049)	(0.088)
Other Developer	**-0.003**	**-0.047**	**-0.008**	**0.021**	
	(0.041)	(0.035)	(0.041)	(0.046)	
Log Developer Employees	**-0.022*****	**-0.020*****	**-0.025*****	**-0.025*****	
	(0.006)	(0.007)	(0.006)	(0.006)	
Developer General Contractor	**(-0.038)***	**-0.031***			
	(0.021)	(0.019)			
Meetings 4 Plus	**0.064*****	**0.067*****	**0.071*****		
	(0.019)	(0.019)	(0.019)		
Community Meetings				**0.017**	
				(0.022)	
Permit & Impact Fees	**-0.154*****	**-0.125****	**-0.174*****	**-0.17*****	
	(0.050)	(0.053)	(0.049)	(0.048)	
Log Permit & Impact Fees per SF	**0.042*****	**0.040*****	**0.048*****	**0.046*****	
	(0.014)	(0.013)	(0.014)	(0.014)	
Funding-Redevelopment	**0.041****	**0.043****			
	(0.019)	(0.018)			
9% Tax Credit	**-0.030**	**-0.018**			
	(0.021)	(0.018)			
Log Building Area	**0.877*****	**0.887*****	**0.919*****	**0.920*****	
	(0.020)	(0.024)	(0.017)	(0.018)	
Structure Includes Parking	**-0.014**	**-0.006**			
	(0.027)	(0.028)			
Parking-to-Rentable Building	**0.371*****	**0.247*****	**0.372*****	**0.367*****	
	(0.062)	(0.056)	(0.050)	(0.050)	
SF Non-Residential: Non-Parking	**-0.533*****	**-0.585*****	**-0.597*****	**-0.643*****	
	(0.135)	(0.115)	(0.128)	(0.128)	
Stories 4 Plus	**0.054**	**0.083*****	**0.023**		
	(0.034)	(0.031)	(0.031)		
Number of Stories				**0.014*****	
				(0.004)	
Residential SF per Unit	**-0.029*****	**-0.028*****	**-0.032*****	**-0.030*****	
	(0.004)	(0.004)	(0.004)	(0.004)	
Log Site in Acres	**0.026**	**0.015**			
	(0.021)	(0.017)			
Includes Non-Residential Area	**0.024**	**0.009**			

	(0.039)	(0.041)			
Average Specialty Contractor Pay	**0.245***	**0.225****	**0.421*****	**0.480*****	
	(0.130)	(0.084)	(0.111)	(0.107)	
Log Fair Market Rent (2-Bedroom)	**0.212*****	**0.232*****			
	(0.070)	(0.057)			
Project Snow	**0.190*****	**0.206*****	**0.197*****	**0.168*****	
	(0.055)	(0.054)	(0.060)	(0.057)	
Log Lenders	**0.026**	**0.022**	**0.041****	**0.044****	**0.027*****
	(0.020)	(0.020)	(0.019)	(0.020)	(0.006)
Duration 24 Plus	**0.033***	**0.051*****	**0.028**		
	(0.018)	(0.018)	(0.018)		
Project Duration				**0.002**	
				(0.002)	
Log A&E Cost per SF	**0.148*****	**0.154*****	**0.162*****	**0.166*****	
	(0.017)	(0.020)	(0.018)	(0.019)	
New Structures Price Index		**0.934*****			
		(0.064)			
Interest 10-year Rate		**0.063*****			
		(0.018)			
Housing Type: Non-Targeted					**-0.047**
					(0.108)
Housing Type: Senior					**-0.280*****
					(0.056)
Housing Type: SRO					**-0.594*****
					(0.085)
Housing Type: Special Needs					**-0.234****
					(0.088)
Number of Bedrooms					**0.000**
					(0.000)
Mitigation					**0.138*****
					(0.048)
Structure: Townhouse					**0.085**
					(0.064)
Structure: Single Family					**0.157**
					(0.114)
Structure: Three Stories					**0.101*****
					(0.040)
Constant	**4.72*****	**3.29*****	**5.47*****	**5.34*****	**12.90*****
	(0.462)	(0.386)	(0.218)	(0.217)	(0.196)
Project Year Fixed Effects	**Yes**	**No**	**Yes**	**Yes**	**Yes**
Project Region Fixed Effects	**Yes**	**No**	**Yes**	**Yes**	**Yes**
N	321	321	321	319	279
R^2	0.958	0.952	0.954	0.953	0.836
F	246	279	254	262	54.50

Notes: Estimated coefficients are in bold. Robust standard errors are in parentheses.

*Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

Table 3: Two Stage Least Squares Model
Prevailing Wage and Instruments in Italics

Variables	First-Stage Coefficients	Reduced-Form Coefficients
Prevailing Wage		***-0.066***
		(0.083)
CDBG Funding	***0.379**** ***	
	(0.071)	
Density Bonus	***0.150**** ***	
	(0.056)	
Nonprofit Developer	**0.085**	**0.075*** **
	(0.065)	(0.026)
Government Developer	**0.443*** **	**0.215*** **
	(0.090)	(0.058)
Other Developer	**0.405*** **	**0.092**
	(0.136)	(0.064)
Log Developer Employees	**-0.051*** **	**-0.031*** **
	(0.020)	(0.008)
Community Meetings	**-0.034**	**0.017**
	(0.072)	(0.023)
Permit & Impact Fees	**0.173**	**-0.124** **
	(0.146)	(0.056)
Log Permit & Impact Fees per SF	**-0.060* **	**0.036** **
	(0.037)	(0.017)
Log Building Area	**-0.075* **	**0.910*** **
	(0.049)	(0.023)
Parking-to-Rentable Building	**0.277* **	**0.412*** **
	(0.148)	(0.061)
SF Non-Residential: Non-Parking	**-0.468**	**-0.766*** **
	(0.297)	(0.141)
Number of Stories	**0.022** **	**0.017*** **
	(0.011)	(0.004)
Residential SF per Unit	**0.020* **	**-0.028*** **
	(0.012)	(0.005)
Average Specialty Contractor Pay	**-0.007**	**0.367*** **
	(0.442)	(0.128)
Project Snow	**0.371** **	**0.214*** **
	(0.173)	(0.064)
Log Lenders	**0.164*** **	**0.065*** **
	(0.058)	(0.025)
Project Duration	**-0.003**	**0.001**
	(0.005)	(0.002)
Log A&E Cost per SF	**0.072**	**0.194*** **
	(0.058)	(0.019)
Constant	**0.839**	**5.52*** **
	(0.689)	(0.274)
Project Year Fixed Effects	**Yes**	**Yes**

Project Region Fixed Effects	**Yes**	**Yes**
N	280	280
R^2	0.493	0.942
F	13.14	6589.79

Notes: Estimated coefficients are in bold. Robust standard errors are in parentheses. The joint F statistic for *CDBG Funding* and *Density Bonus* was 15.61, above the required threshold for this number of instruments (Stock & Yogo, 2005).
The values for the robust score χ^2 and robust regression F statistics were 3.322 (p = 0.068) and 2.966 (p = 0.086), respectively; the statistical significance of these two tests provides clear evidence of endogeneity of prevailing wages.
The value of the χ^2 test of overidentifying restrictions was 0.644 (p = 0.422). A statistically significant value would have indicated that using a second instrument for *Prevailing Wage* was likely invalid.
*Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

Table 4: Supplemental Analysis

Variables[17]	Model 1	Model 2	Model 3	Model 4	Model 5
Prevailing Wage	**0.463****	**0.564*****	**0.379*****	**0.261*****	**-0.066**
	(0.191)	(0.137)	(0.114)	(0.071)	(0.083)
First-Stage Joint F Statistic	**1.220**	**1.750**	**1.510**	**4.440*****	**15.610*****
% Yes Vote on Prop. 40	**3.560**	**1.570**	**2.210**	**1.530**	
Water, Air and Parks, 2002	(2.190)	(1.770)	(1.850)	(1.780)	
% Yes Vote on Prop. 45	**-6.410****	**-1.130**	**-1.270**	**-0.809**	
Term Limits, 2002	(3.230)	(2.820)	(2.740)	(2.730)	
% Yes Vote on Prop. 46	**5.370**	**-4.400**	**-3.660**	**-1.750**	
Housing/Emergency Shelter, 2002	(4.450)	(3.130)	(3.110)	(3.030)	
% Yes Vote on Prop. 77	**-1.510**	**0.401**	**-0.175**	**-0.299**	
Redistricting, 2005	(2.070)	(1.430)	(1.420)	(1.420)	
% Yes Vote on Prop. 1B	**-1.940**	**-3.910****	**-3.930****	**-3.830****	
Highways and Air Quality, 2006	(2.350)	(1.750)	(1.740)	(1.620)	
% Yes Vote on Prop. 1C	**-5.310**	**7.420***	**6.440**	**4.760**	
Housing/Emer. Shelter, 2006	(3.990)	(3.940)	(3.930)	(3.660)	
%Yes Vote on Prop. 99	**0.105**	**-1.610**	**-1.700**	**-1.110**	
Eminent Domain, 2008	(2.150)	(1.530)	(1.480)	(1.470)	
% Yes Vote on Prop. 1A	**0.823**	**3.540***	**3.670****	**3.260***	
State Taxes, 2009	(2.220)	(1.850)	(1.770)	(1.750)	
Percent of Voters Registered	**-0.564**	**-4.420****	**-4.760****	**-4.480****	
as Democrats	(2.150)	(2.220)	(2.230)	(2.240)	
Percent of Population over	**-4.160****	**0.067**	**-0.202**	**-0.719**	
40 Years Old	(1.960)	(2.240)	(2.170)	(2.030)	
Percent of Housing Units	**1.110**	**-2.190**	**-1.560**	**-1.110**	
Owner-Occupied	(1.440)	(1.740)	(1.720)	(1.630)	
CDBG Funding				**0.326*****	**0.379*****
				(0.079)	(0.071)
Density Bonus				**0.235*****	**0.150*****
				(0.059)	(0.056)
Endogeneity F Statistic	**2.390**	**8.839*****	**6.110****	**3.844***	**2.966***
N	279	279	279	279	280
First-Stage R^2	0.508	0.461	0.482	0.523	0.493
Second-Stage R^2	0.808	0.781	0.901	0.918	0.942

Notes: Estimated coefficients are in bold. Robust standard errors are in parentheses.
*Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

[17] Because of missing data, inclusion of the CBDG Funding and Density Bonus variables in Models 4 and 5 reduced the number of observations from 315 to 279. As a robustness check, Models 1 – 3 were run with 315 observations. The reduction in the number of observations had little effect on the signs, magnitudes or significance of the PW results. These results are available from the corresponding author upon request.