

Are State Workers Overpaid? Survey Evidence from Liquor Privatization in Washington State

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Abstract Industry privatizations that result in exogenous job displacement of public employees can be exploited to estimate public sector wage rents. I report the findings of an original survey I administered to examine how wages of displaced government workers were affected by a 2012 privatization of liquor retailing in Washington State. Based on a panel difference-in-differences estimator I find that privatization reduced wages by \$2.51 per hour or 17 percent compared to a counterfactual group of nearly identical non-displaced workers, with larger effects for women. I decompose wage losses into three rents identified in the literature: public sector rents, union premiums, and industry-specific human capital. Public sector wage premiums separately account for 85 to 90 percent of overall wage losses, while union premiums and industry-specific human capital account for just 10 to 15 percent. The results are consistent with a roughly 16 percent public sector wage premium.

Keywords Public-sector labor markets · Union rents · Displaced workers · Privatization

JEL Classifications J45 · J51 · L33 · J63 · J68

Introduction

Are state government workers overpaid? The question has received renewed attention in recent years as state budget shortfalls and deteriorating public employee pension

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systems have prompted increased scrutiny of public sector pay. Labor economists have long recognized the potential for “rent extraction” by public employees in the form of wages and benefits in excess of workers’ outside alternatives. Government wages are not set in competitive markets (Brueckner and Neumark 2014), workers are more likely to be union represented (at least in those states that allow public-sector collective bargaining, see Visser 2006), and public employees are politically active (O’Brien 1992). Public-sector rent extraction has taken a variety of non-wage forms as well, including increased health and pension benefits (Clemens and Cutler 2014) and deflection of budget cuts away from programs associated with strong public sector unions (Clemens 2012). For these reasons, the stylized fact of public employee wage premiums has been incorporated into a variety of formal models of public sector labor markets (Brueckner and Neumark 2014; Glaeser and Ponzetto 2013; Holmlund 1993; Borjas 1980; Reder 1975; Fogel and Lewin 1974).

While economic theory makes clear predictions about public employee wage premiums, the large empirical literature on the subject is mixed.¹ Most studies find evidence of wage premiums among federal government employees on the order of 10 to 20 percent relative to private sector counterparts. However, premiums are typically found only at the highest levels of government: the federal government in the U.S., and comparable central governments internationally. Wage premiums for state and local employees are zero or slightly negative in most estimates. This suggests the rent seeking central to most theoretical models of public sector labor markets may be a poor description of local public employee labor markets.

In recent years, a vigorous political debate surrounding state budget crises has prompted a reexamination of public compensation. At one extreme Keefe (2012) finds no evidence of public sector wage premiums, reporting a 7.6 percent earnings penalty among state workers based on cross-sectional microdata from IPUMS-CPS and the Employer Costs for Employee Compensation survey. At the other extreme Gittleman and Pierce (2012) find a significant 3 to 10 percent earnings premium for state employees relative to comparable private sector workers through the use of an alternative treatment of controls for occupation, employer size, and union status (reflecting earlier methodological points about the appropriateness of these controls as a proxy for worker skills made by Linneman et al. 1990 and Hirsch et al. 1999). Both studies were covered extensively in the media, fueling the longstanding political controversy over the degree to which state employees are over (or under) compensated and thus a contributing (or non-contributing) factor to state budget woes.²

This study presents evidence on public sector wage premiums from a different source of identification. Rather than comparing private and public sector wages in the

¹Extensive reviews of the literature on public sector wage premiums are available in Gregory and Borland (1999) and Ehrenberg and Schwartz (1986).

²See for example, Ezra Klein, “Public Employees Don’t Make More than Private Employees,” *Washington Post*, September 16, 2010 (<http://voices.washingtonpost.com/ezra-klein/2010/09/public-employees-dont-make-mor.html>); and Sita Slavov, “How Politicians Buy Votes By Doling Out Public Worker Benefits,” *U.S. News and World Report*, May 2, 2013 (<http://www.usnews.com/opinion/blogs/economic-intelligence/2013/05/02/public-sector-employees-receive-generous-benefits-due-to-politics>).

cross section, I make use of a panel of exogenously displaced government workers laid off by a 2012 privatization of liquor retailing in Washington State. Enacted by voter ballot initiative, the privatization closed 167 state-run retail liquor stores and laid off an entire occupational category of public employees. As a comparison group, I use government workers in similar retail occupations who were unaffected by the policy. The decision by displaced workers to accept employment following layoffs serves as a revelation mechanism for the second-best wage offer facing displaced public sector workers. Due to the exogenous timing and non-selective nature of the displacements, these quasi-experimental “treatments” can be used to identify public sector wage premiums.³

The treatment group consists of exogenously displaced government workers for whom wages are observed before and after privatization. I collected information on post-policy wages and other demographic characteristics of the displaced workers via an original mail and online survey questionnaire. These responses were then matched to pre-policy wage information from administrative records, resulting in an individual-level panel of wages and other demographic details over the nine year period from 2005 to 2013. The control group consists of a panel of remarkably similar employees working in comparable retail occupations in state government: customer service specialists in the state’s motor vehicle licensing offices. These individuals were employed by the same state government, worked in retail occupations with similar job skills, were similarly unionized, and worked in comparable urban and suburban retail outlets. Average wages for both groups of workers followed nearly identical trends throughout the pre-policy period. I use these data to identify the causal effect of privatization-related job displacements on public sector wages using a panel difference-in-differences estimator with individual and time fixed effects.

Based on a panel of 262 workers from 2010 to 2013, I find that privatization reduced wages of displaced government workers by $-\$2.51$ per hour, or roughly 17.2 percent compared to the control group of unaffected workers. Consistent with past literature I find somewhat larger effects among female workers, with wages falling by $-\$2.87$ per hour or 19.7 percent for females compared with $-\$2.20$ per hour or 15.1 percent for males, although the two estimates are not statistically different. Quantile regression results reveal considerable heterogeneity of treatment effects throughout the conditional wage distribution, ranging from $-\$4.62$ per hour at the 5th percentile of wages to a statistical zero effect at the 95th percentile. These basic findings are unaffected by (1) the inclusion of individual-level controls for gender and length of job tenure; by (2) excluding from the sample individuals who reported more valuable non-wage fringe benefits in their post-policy employment; and by (3) estimation via

³An ideal comparison group would be a collection of similar private sector workers subject to a parallel exogenous mass layoff. However, this is an infeasible identification strategy. Private sector job displacements are rarely exogenous and are typically the result of adverse demand conditions affecting both layoffs and wages. However, unlike most literature on mass layoffs, a unique feature of my setting is that demand conditions were stable throughout the period. I exploit this feature to directly estimate public wage rents.

a kernel matching difference-in-differences estimator in which treated and control workers are propensity-score matched based on observables.⁴

By examining treatment effects in various subsamples, it is possible to provide a rough decomposition of overall wage losses into three types of wage rents identified by previous literature: public sector wage premiums; union wage premiums; and wage premiums from industry-specific human capital lost following job displacement. I decompose overall wage losses into lost union, public-sector and industry-specific wage rents by examining linear combinations of treatment effects from various subsamples of workers who sorted into (1) private-sector retail liquor jobs, (2) government jobs, (3) union jobs, and (4) non-union jobs following privatization.⁵ I find that public sector wage rents separately account for roughly 86 to 90 percent of the overall decline in wages following privatization. By contrast, lost industry-specific human capital accounts for 11 to 13 percent of wage declines, while lost union wage premiums account for roughly 1 percent or less. These results suggest that displaced state government workers earned a roughly 16 percent public sector wage premium prior to displacement. This estimate lies somewhat above the 3 to 10 percent earnings premium reported by Gittleman and Pierce (2012), and is substantially larger than estimates from comparable panel studies such as Krueger (1988) and Lee (2004) which are not based on exogenous job separations. However, the effects are considerably smaller than those found in Galiani and Sturzenegger (2008), and are consistent with those found in similar international studies of the effect of privatizations on displaced workers such as Firpo and Gonzaga (2010).

This study is not the first to use the privatization of state-owned firms as an exogenous shock to identify public sector wage rents. Studies in Brazil (Firpo and Gonzaga 2010), Portugal (Monteiro 2008), Argentina (Galiani and Sturzenegger 2008), and the U.K. (Disney and Gosling 2003) have used a similar approach in recent years, yielding a wide variety of estimates from zero to roughly 40 percent wage premia. This study contributes to this growing literature by applying the approach to public employees in the United States for the first time. This extension is relevant as U.S. labor markets are generally more flexible and labor unions weaker than in previous countries examined, and thus conclusions from overseas studies may not apply to public sector workers in the U.S. While the source of identification is novel, it is not without limitations. I examine a small sample of government workers in an unusual occupational category, thus, the results may not easily generalize to broader classes of public employees. However, by making use of a clean source of identifying variation the approach overcomes many of the sorting, selection and endogenous job separation problems that have plagued past research. With several states considering

⁴These alternative estimates are presented in the [Appendix](#).

⁵In Section “[Assessing Bias in Wage Rent Decompositions Due to Selection into Post-Policy Occupation](#)” I examine whether these results are driven by non-random selection of displaced workers into post-policy industries. I find no evidence that selection on observables such as education, work experience and gender explain the results.

similar liquor privatization initiatives, this study provides the first estimates of the likely effect of those policies on displaced public employees.⁶

The remainder of the paper is organized as follows. Section “[Related Literature](#)” reviews the related literature. Section “[Policy Background](#)” gives background on the privatization of liquor retailing in Washington State. Section “[Data](#)” describes the data and survey design. Section “[Identification Strategy](#)” explains the difference-in-differences identification strategy. Section “[Results](#)” presents my results, and Section “[Conclusion](#)” concludes. The [Appendix](#) presents tables of alternative results and a copy of the original survey questionnaire.

Related Literature

Public Sector Wage Premiums

The empirical literature on public sector wage premiums is large, both in the U.S. and internationally. Gregory and Borland (1999) and Ehrenberg and Schwartz (1986) review the early literature. Most research has focused on federal employees, with a smaller number of studies examining state and local workers. Among those examining state employees, most research has been cross sectional based on large, publicly available data sets. Following Smith (1976), early studies employed Oaxaca (1973)-style decompositions or simple OLS with controls to identify public sector wage premiums. Nearly all studies report a positive wage premium for federal workers on the order of 10 to 20 percent, with higher premiums for women and those in high-amenity urban locations, but zero or slightly negative wage premiums for state and local government workers (Gregory and Borland 1999).⁷

The basic identification problem faced by early literature is the inability to address non-random assignment of workers into sector of employment in the cross section. Workers choose jobs partly on the basis of unobserved characteristics, including risk aversion and attitudes toward public service, which are likely correlated with productivity. Recognizing this problem, a second wave of studies beginning with Robinson and Tomes (1984) and Gyourko and Tracy (1988) employed Heckman-style correction methods to cross-sectional data, resulting in somewhat smaller estimates of federal wage premiums but still zero or slightly negative premiums for state and local workers. The modern literature has made little progress beyond these methods. For example, the two recent studies of Keefe (2012) and Gittleman and Pierce (2012)

⁶For example, Pennsylvania and Oregon are currently engaged in active political debates regarding the privatization of their state-run liquor retailing systems. See Kate Giammarise, “Pennsylvania liquor overhaul brews big spending,” *Pittsburgh Post-Gazette* (May 26, 2014), available at <http://www.post-gazette.com/news/politics-state/2014/05/26/Pa-liquor-overhaul-brews-big-spending/stories/201405260074>; and Harry Esteve, “Liquor privatization initiative moves forward,” *The Oregonian* (May 17, 2014), available at http://www.oregonlive.com/politics/index.ssf/2014/05/liquor-privatization_initiativ.html.

⁷It is worth noting that most federal studies do not present separate estimates wage premiums for postal and non-postal workers, despite the fact that pay among postal employees is based on collective bargaining and is determined separately from other federal employees. Separate analyses of postal workers tend to find larger wage premiums than among other federal workers (Hirsch et al. 1999).

both estimate wage premiums for state government workers using OLS with controls in large, cross-sectional data sets.⁸

Three studies have used longitudinal data to address the problem of unobserved worker heterogeneity: Krueger (1988), Hirsch et al. (1999) and Lee (2004). All three identify government wage premiums via fixed effects estimators identified off workers who voluntarily shift between private and government jobs. Krueger (1988) uses matched files from the Current Population Survey and the supplemental Displaced Worker Survey to identify “switchers” who moved between sectors in subsequent years in the two panels. He finds somewhat smaller federal wage premiums of 5 to 10 percent for federal workers, but again a statistically zero wage premium for state workers. Subsequently, Krueger (1988) has been criticized for the small number of “switchers” used for identification in the two panels—for example, the Displaced Worker Survey used in the study contains information on just 91 workers who switched between sectors—and thus the representativeness of the results (Moulton 1990; Lee 2004). By contrast, Hirsch et al. (1999) restrict attention to U.S. Postal Service employees, analyzing earnings for workers switching between private sector and federal postal jobs based on longitudinal data from the Postal New Hire Survey, matched Current Population Survey panels, and the Displaced Worker Survey. They find large public sector wage premiums on the order of 30–40 percent among new postal employees.

Lee (2004) presents longitudinal estimates of public sector wage premiums using the National Longitudinal Survey of Youth (NLSY) for the first time. For state government employees, the survey contains information on 214 male and 309 female “switchers,” roughly five times the number used in Krueger (1988). He finds a 5 percent wage premium for federal workers that is well below most cross sectional estimates. For state government employees he reports a 4 percent wage premium for women and a -1 percent penalty for men, both of which are substantially larger (i.e., more positive) than typical cross-sectional estimates. An important criticism of both of Krueger (1988) and Lee (2004) is that neither makes use of exogenous job separations to identify wage premiums. Workers’ decisions to become “switchers” between sectors are likely endogenous with respect to productivity and wages. For example, if workers of low (or high) ability are disproportionately observed shifting from private sector jobs into the public sector over time, panel estimates of wage premiums will be biased and inconsistent. Without exogenous job separations the causal effect of sectoral shifts on wages is not identified, even in panel data.

In recent years, a smaller literature has emerged using quasi-experimental job displacements among government workers to identify public sector wage premiums. The transition of state-owned firms in banking, petroleum, and liquor retailing into private ownership typically results in layoffs for large numbers of government employees. To the extent that the policy decision to privatize is exogenous with respect to wages of the affected employees, and if layoffs are non-selective among

⁸One area modern literature has made progress on is the inclusion of non-wage “fringe” benefits in the estimation of public sector wage premiums, which was largely neglected in early research due to data limitations.

workers, these job displacements can be used as “treatments” to investigate the loss of wage rents among the affected workers. Recent examples of this approach include Firpo and Gonzaga (2010) in Brazil, Monteiro (2008) in Portugal, Galiani and Sturzenegger (2008) in Argentina, and Disney and Gosling (2003) in the U.K. Estimates of public sector wage premiums from this newer literature range from 40 percent among former petroleum workers in Argentina (Galiani and Sturzenegger 2008) to 11 percent among a variety of workers in Brazilian state-run firms (Firpo and Gonzaga 2010). Only Disney and Gosling (2003) find no evidence of wage premiums based on privatization from the 1990s in the U.K. This study contributes to the growing literature by applying this quasi-experimental approach to displaced public sector workers in the U.S. for the first time.

Displaced Worker Studies

Because this study examines public employees displaced by a mass layoff event, it is also related to the large empirical literature on “displaced workers.” Extensive reviews are provided by Kletzer (1998), Fallick (1996), and Hamermesh (1989). Among workers displaced by plant closings and other mass layoffs, studies typically find significant wage declines that persists for years after displacement. Summarizing the literature, Kletzer (1998) offers five broad explanations for these observed wage losses: (1) Loss of industry specific human capital; (2) loss of firm-specific human capital; (3) loss of high-quality matches with employers; (4) loss of industry specific rents (including public sector wage rents); and (5) loss of union premiums. A key challenge faced in this study is separately identifying which of these effects played a role in the observed changes in wages among displaced state workers.

A common finding throughout the displaced worker literature is that wage losses are highly concentrated among workers who change industries following layoff (Kletzer 1998). Workers who remain in the same industry typically suffer small or zero permanent wage losses. This suggests industry-specific capital plays a central role in explaining observed wage losses among displaced workers. Several high-quality studies have confirmed this pattern, finding most worker skills appear to be transferrable within the same industry following layoffs, and that workers who find post-displacement jobs in the same industry experience few wage effects (Neal 1995; Carrington 1993; Ong and Mar 1992; Addison and Portugal 1989).⁹

Among the displaced WSLCB workers I examine, the effect of any lost industry-specific human capital should be concentrated among those who shift out of the liquor retailing industry following privatization. By contrast, those who remain in the industry following privatization should not experience this effect, as industry-specific skills have been retained. Similarly, the effect of lost union premiums and public sector rents should be concentrated among those who shift into non-union and private sector jobs, respectively, while those remaining in union and government jobs

⁹An important caveat is that industry-specific human capital cannot be easily differentiated from occupation-specific human capital, as many occupations are highly clustered within specific industries. See Neal (1995), pp. 669–70, and citations therein for a discussion of this point.

should not experience these effects.¹⁰ It is possible that this decomposition based on post-policy employment decisions by workers introduces biases due to endogenous sorting across occupations; however, I show below that there is no evidence of this on observable worker dimensions. For simplicity, I group the effects of job displacement on wages into three categories: (1) lost public sector wage rents; (2) lost union premiums; and (3) wage losses due to lost industry specific capital. Using this approach, in Section “[Results](#)” I decompose overall wage losses suffered by displaced public workers into three distinct wage rents identified in past literature.

Policy Background

In November 2011, Washington State voters approved ballot initiative I-1183, privatizing the state’s liquor retailing and distribution system.¹¹ Previously the state was one of 19 “control” states that maintain some form of public monopoly over liquor retailing.¹² For seven decades, liquor retailing was state owned and operated under the supervision of the Washington State Liquor Control Board (WSLCB). The passage of I-1183 abruptly ended the system, closing 167 state-owned liquor retailers and liquidating the assets at auction.¹³ As a consequence, approximately 900 public sector workers employed in liquor retail establishments were laid off on June 1, 2012.

A key feature of the policy is that the resulting layoffs were strongly exogenous with respect to any wage premiums enjoyed by the affected workers. The primary impetus for the ballot initiative was business lobbying by local retailers, and was not explicitly designed to reduce public sector employment or wages.¹⁴ This is a unique feature of the policy relative to most existing literature on mass layoffs, as job displacements are typically accompanied by confounding shifts in industry demand conditions or endogenous state budget pressures, neither of which were present in this setting.

The displaced public employees were union represented by the United Food and Commercial Workers Local 21 (UFCW 21). Wages were set by a collective

¹⁰For workers who remain in unionized jobs following privatization, I assume union wage premiums are preserved. However, it is possible that a loss of tenure when transferring between unions could also affect wages. Because length of job tenure at the time of displacement is perfectly collinear with individual fixed effects, I am unable to fully resolve this issue in the data.

¹¹By “liquor” I refer only to distilled spirits. Beer and wine have long been privately retailed in the state and were unaffected by the privatization initiative.

¹²Following Washington’s privatization 18 states maintain public monopolies over liquor retailing and distribution. The remaining “control” states are: Alabama, Idaho, Iowa, Maryland (Montgomery and Worcester counties only), Maine, Michigan, Mississippi, Montana, New Hampshire, North Carolina, Ohio, Oregon, Pennsylvania, Utah, Vermont, Virginia, West Virginia, and Wyoming. Source: National Alcohol Beverage Control Association (<http://www.nabca.org>).

¹³The state also maintained 162 privately owned “contract” liquor stores primarily located in rural areas of the state. Contract stores remained in operation following privatization, but were required to purchase all remaining inventory from the state.

¹⁴See Austin Jenkins, “Costco Breaks Records With \$22M To Privatize Liquor,” *NPR*, October 19, 2011 (<http://www.npr.org/templates/story/story.php?storyId=141531406>).

bargaining agreement with the state. Nearly all of the affected workers held the job title “Liquor Store Clerk” or “Retail Manager.” Job responsibilities were typical of retail occupations: ringing up purchases at cash registers, maintaining merchandise displays, restocking shelves, and answering customer questions. The formal requirement for these positions was for a high school diploma, and the jobs required little specialized technical skills or knowledge. The displaced workers were employed in liquor retailers located primarily in urban and suburban areas throughout the state. Stores averaged 5,200 square feet, maintained staffs of 3 to 12 employees, and had average annual retail sales of \$4 million per store. Overall, the industry was likely characterized by a similar production function to small, urban grocery and convenience stores that operate in the private sector.

Following privatization, the displaced workers were eligible for ordinary unemployment insurance benefits, but no special benefit provisions were made. Some media outlets reported local firms offering open-door job interviews to displaced workers,¹⁵ but there was no formal process to retrain or place workers into alternative employment. Fearing wage losses and reduced employment, WSLCB employees and their union representatives were among the most vocal opponents of the privatization initiative.¹⁶ Both before and after the policy change there was widespread speculation about the fate of the roughly 900 displaced public sector workers. This study is the first to examine how earnings and employment of the displaced retail liquor workers were affected by the 2012 privatization.

Data

Survey Design

I implemented an original survey of the roughly 900 state workers whose occupations were eliminated by the 2012 liquor privatization, based on individual contact information provided by Local 21 of the United Food and Commercial Workers union (UFCW 21). The survey collected detailed information on earnings, employment and other demographic characteristics from respondents.¹⁷ Names and home addresses were obtained for 911 displaced workers, 284 of whom listed email addresses. The survey followed a multi-mode approach consisting of (1) a recruitment letter; (2) an online survey; (3) a traditional mail survey; and (4) a follow-up reminder letter. The initial wave consisted of 284 emails to individuals for whom addresses were available inviting them to complete an online survey between July 12 and August 4,

¹⁵See Melissa Allison, “Costco offers job interviews to displaced state liquor-store workers,” *Seattle Times* (November 10, 2011), available at http://seattletimes.com/html/localnews/2016734642_costco11.html.

¹⁶See Melissa Allison, “Unions sue to block liquor initiative from taking effect” (December 6, 2011), available at http://seattletimes.com/html/localnews/2016947384_liquorsuit07.html.

¹⁷The survey was granted institutional review board approval by the “Human Research Protections Program” at the University of California, San Diego on March 12, 2013. Information about the review process is available at <http://irb.ucsd.edu/about.shtml>. For reference, a complete copy of the survey recruitment letter and questionnaire is provided in the [Appendix](#).

2013, roughly one year after displacement. This was followed by a mail survey to the remaining individuals between August 5 and September 10, 2013. Mail surveys also included an option for completing an online survey using a unique 4-digit code, preventing duplicate responses. In total, the survey collected in $N = 404$ responses for a response rate of 44.3 percent. 199 online questionnaires were submitted and 205 were received via mail.

For the full population of 900 affected workers, I obtained information on pre-policy wages, hours, and gender from administrative data from the Washington State Office of Financial Management (WA OFM). These data are drawn directly from state accounting records and contain the name, gender, job title, hourly wage, and average weekly hours for 2010, 2011 and 2012 for all workers affected by the policy. These population characteristics enable me below to examine whether there is any evidence of systematic differences between this subset of survey respondents and the overall population. For the $N = 404$ survey respondents, the WA OFM data were matched to individual survey responses on the basis of employee names, providing a longitudinal file of earnings and other demographic information for both pre- and post-policy periods.

Tables 1 and 2 present basic descriptive statistics for the full sample of 404 respondents collected by the survey. The gender balance among survey respondents closely matches the overall population, with 42.1 percent male and 57.9 female compared to 43.8 and 56.2 percent in the overall population of displaced workers, respectively. The average age is 45 to 49 years. 84 percent report their ethnicity as white or Caucasian, with 4.2 percent African American, 3.7 percent Asian or Pacific Islander, and 3.5 percent Hispanic. Nearly half live in married households. The overwhelming majority of workers (84 percent) have less than a 4-year college degree.

In terms of employment, 56.2 percent of respondents reported being employed one year after displacement, with 25.2 percent full time, 29 percent part time, and 2 percent self-employed. 30 percent of respondents were unemployed by the usual definition. The remaining 13.8 percent exited the labor force for a variety of reasons, including retirement (8.4 percent), to become a student or homemaker (4 percent), or because they simply stopped looking for work (1.5 percent). The implied unemployment rate among displaced workers was 34.5 percent one year after the policy. By comparison, the state's overall unemployment rate was roughly 7 percent during the same period.

Because we do not observe post-policy wages for the subset of displaced workers who remain unemployed, our estimates of the effect of displacement on wages may suffer bias from this exclusion. To the extent that unemployed workers would have accepted wages that are above (or below) those observed among employed workers, the estimated treatment effects will over- (or under-) state the impact of the policy. To assess the importance of this concern, I examine whether employed and unemployed workers significantly differ on observable individual characteristics. Letting U_{it} be a binary indicator equal to 1 if survey respondent i was unemployed during the post-policy period and 0 otherwise, I estimate the following linear probability model,

$$\text{Prob}(U_i = 1 | X) = \beta_0 + \beta_1 \text{Wage}_i + \beta_2 \text{Hours}_i + \beta_3 \text{Tenure}_i + \beta_4 \text{Female}_i + \beta_5 \text{Age} + \beta_6 \text{Moved} + \beta_7 \text{Race} + \beta_8 \text{Education} + \beta_9 \text{HHSize} + \epsilon_i \quad (1)$$

Table 1 Descriptive statistics for all survey respondents (1 of 2)

	Number	Freq.		Number	Freq.			
Population	912		Education					
Responses	404		Less than high school	1	0.2			
Response Rate		44.3	Some high school	3	0.7			
Response Type			High school diploma	111	27.5			
			Some college	161	39.9			
Online	199	49.3	Associate’s degree (A.A.)	64	15.8			
Mail	205	50.7	College degree (B.A.)	47	11.6			
Gender			Some grad school	9	2.2			
			Graduate degree	7	1.7			
Male	170	42.1	n.a.	1	0.2			
Female	234	57.9	Adults in HH					
Age			1	91	22.5			
			20-24 years	5	1.2	2	240	59.4
			25-29 years	15	3.7	3	43	10.6
			30-34 years	22	5.4	4	25	6.2
			35-39 years	30	7.4	5+	5	1.2
			40-44 years	42	10.4	Children in HH		
45-49 years	40	9.9	0	280	69.3			
50-54 years	66	16.3	1	75	18.6			
55-59 years	73	18.1	2	33	8.2			
60-64 years	69	17.1	3	14	3.5			
65+ years	42	10.4	4	1	0.2			
Race			5+	1	0.2			
			White	337	83.4	Employment Status		
Black	17	4.2	Employed full time	102	25.2			
Asian	15	3.7	Employed part time	117	29.0			
Mixed race	15	3.7	Self-employed	8	2.0			
Hispanic	14	3.5	Not employed, looking	121	30.0			
Native Amer.	3	0.7	Not employed, not looking	6	1.5			
Other race	3	0.7	Homemaker	4	1.0			
Marital Status			Student	12	3.0			
			Single, never married	63	15.6	Retired	34	8.4
			Living with a partner, single	44	10.9	Job Tenure		
Married	199	49.3	Less than 1 year	60	14.9			
Separated	5	1.2	1-2 years	59	14.6			
Divorced	83	20.5						

Table 1 (continued)

	Number	Freq.		Number	Freq.
Widowed	10	2.5	3-4 years	83	20.5
			5-6 years	51	12.6
Separation Reason			7-10 years	49	12.1
Quit Voluntarily	37	9.2	10+ years	102	25.2
Fired from job	367	90.8			

Survey of the displaced WSLCB workers was conducted online and via mail between July 12 and September 10, 2013. Based on $N = 404$ survey respondents. A complete copy of the survey recruitment letter and questionnaire is provided in the [Appendix](#)

Pre-policy wages and hours are from administrative data from WA OFM. Mover status is determined by a comparison of individuals' pre-policy addresses with post-policy address from roughly one year after the policy based on a USPS-validated mailing list. Data for all other characteristics are from the administered survey. The estimation uses the full sample of $N = 404$ survey respondents. Table 3 presents the results. Pre-policy wages, hours, and all other individual characteristics have zero predictive power on post-policy employment status. Put differently, unemployed and employed workers do not differ significantly in terms of pre-policy observables. None of the coefficients on worker characteristics are significant, and together they explain less than 4 percent of the variation in employment status. It is possible that unemployed workers may differ on unobservable characteristics from employed workers, but there is no evidence of systematic selection into unemployment on observables.

Average pre-policy wages among respondents were \$14.35 per hour, with a standard deviation of \$2.57. This amounts to annual earnings of roughly \$29,850 for full-time workers, well below the average annual \$55,000 earnings for Washington state employees overall in 2012.¹⁸ Post-policy wages were a significantly lower \$13.33 per hour, with a larger standard deviation of \$4.93.¹⁹ Most respondents who were employed found jobs in the same or similar fields, the most common being liquor retail (26.9 percent), with 13.7 percent finding jobs in the same store they previously worked in. The second most common was general non-liquor-related retail jobs (22 percent), followed by government jobs (8.8 percent), administrative jobs (8.4 percent), education (5.7 percent), and restaurant or hotel services (4.8 percent).

Among the employed, just under half reported receiving zero non-cash "fringe" benefits post-policy, with 44 percent reporting health insurance, 40.5 percent dental insurance, 44 percent paid vacation, 34.4 percent retirement benefits, and less than 10 percent childcare or transportation benefits. To assess whether wage declines

¹⁸See the U.S. Census Bureau's "Quarterly Census of Employment and Wages," Series ID ENU5300 050292.

¹⁹I address the issue of possible misreporting of wages by survey respondents in the following section.

Table 2 Descriptive statistics for survey respondents (2 of 2)

	Number	Freq.		Number	Freq.
Industry (Employed)			Earnings (Employed)		
Administrative	19	8.4	Pre-Policy		
Agriculture and fishing	2	0.9	Mean	\$14.35	
Auto Repair	1	0.4	St. Dev.	2.57	
Construction	5	2.2			
Custodial	1	0.4	Post-Policy		
Education	13	5.7	Mean	\$13.33	
Entertainment	1	0.4	St. Dev.	4.93	
Finance or real estate	8	3.5			
General labor - car wash	1	0.4	Fringe Benefits		
Government	20	8.8	Health Insurance	100	44.1
Health	4	1.8	Dental Insurance	92	40.5
Manufacturing	3	1.3	Paid Vacation	100	44.1
Media	1	0.4	Retirement (401(k))	78	34.4
Personal services - daycare	1	0.4	Childcare	3	1.3
Restaurant or hotel	11	4.8	Transportation	19	8.4
Retail liquor, new location	30	13.2	None	108	47.6
Retail liquor, same location	31	13.7			
Retail, non-liquor	50	22.0	Value of Fringe Benefits		
Sales and marketing	5	2.2	Less than Pre-Policy	168	74.0
Security services	3	1.3	Same as Pre-Policy	39	17.2
Social services	1	0.4	Greater than Pre-Policy	19	8.4
Technology or software	1	0.4	n.a.	1	0.4
Transportation or warehouse	10	4.4			
Wholesale	4	1.8	Union Representation		
n.a.	1	0.4	Union Member	43	18.9
			Non-Union	184	81.1
			Moved to New Location		
			Same Address	352	87.1
			New Address	52	12.9

Survey of the displaced WSLCB workers was conducted online and via mail between July 12 and September 10, 2013. Based on $N = 404$ survey respondents. Figures reported for pre- and post-policy earnings are mean hourly wages for the subset of $n = 227$ displaced workers who reported being employed in 2013. A complete copy of the survey recruitment letter and questionnaire is provided in the [Appendix](#)

were partly (or completely) offset by increases in non-wage benefits, respondents were asked to compare the dollar value of their current benefits to those provided by their previous government employer. 91.2 percent reported post-policy fringe benefits were less valuable or of roughly the same value as pre-policy

Table 3 Assessing selection into unemployment: regression of probability of post-policy unemployment on observed worker characteristics

Variable	(1)	(2)	(3)	(4)	(5)
Pre-Policy Wage (2012)	0.009 (0.008)	0.007 (0.008)	0.002 (0.009)	0.001 (0.010)	0.005 (0.010)
Pre-Policy Hours (2012)		0.004 (0.003)	0.003 (0.003)	0.003 (0.003)	0.004 (0.004)
Job Tenure			0.016 (0.015)	0.015 (0.015)	0.009 (0.016)
Female				0.061 (0.046)	0.068 (0.050)
Age					0.012 (0.012)
Moved to New Address					0.031 (0.073)
Black / African American					-0.020 (0.115)
Hispanic					0.201 (0.136)
Asian / Pacific Islander					-0.085 (0.115)
Education					0.061 (0.254)
Married					-0.061 (0.111)
Adults in Household					-0.013 (0.020)
Children in Household					-0.088 (0.084)
n					-0.110
R-Squared					(0.082)

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of a binary indicator for unemployment during the post-policy period on observed pre-policy worker characteristics in the full sample of $N = 404$ survey respondents. Heteroskedasticity-robust standard errors are reported in parentheses

benefits.²⁰ In terms of union membership, 18.9 percent of the employed reported working in a union job. 13 percent of respondents moved to a new home address

²⁰ Respondents were asked, “If you are employed, think about the dollar value of your current [fringe] benefits. Are the worth less, more, or about the same as the benefits you received at your Washington State liquor retail job?”

during the year after job displacement, while 87 percent remained in the same location.

Construction of Treated Group

For the treatment group, I restrict the sample to a balanced panel of state workers who were involuntarily laid off by the 2012 privatization and for whom wages are observed in all pre- and post-policy years. From the sample of $N = 404$ survey respondents, I omit 37 individuals who self-selected out of treatment by voluntarily quitting before the policy went into effect in June 2012. This is done to isolate only those individuals for whom job displacement was involuntary and strongly exogenous with respect to wages.²¹ To avoid confounding effects of attrition from individuals moving in and out of public sector employment over time, I further restrict the sample to individuals for whom I observe public-sector wages in all years, creating a balanced panel of treated workers.²² In choosing panel size, I face a trade-off between panel length T and total observations NT as the number of individuals observed in all years falls sharply as the panel length is extended backward into the pre-policy period. I select the four-year panel from 2010 to 2013 that maximizes NT , consisting of three pre-policy periods and one post-policy period. This results in a balanced panel of $N = 143$ workers over $T = 4$ periods ($NT = 572$).²³ This panel serves as the “treated” group in the difference-in-differences estimates below. Table 4 presents summary statistics for this treated group.

Non-Response Bias in Treated Group

Because survey participation was voluntary, it is possible that the sample of treated workers differs systematically from the population due to survey non-response. To examine the representativeness of the treated group I regress an indicator of survey response on various pre-policy characteristics of workers for whom wages are observed in all years. For a random sample, pre-policy observables should have little explanatory power when regressed on the probability of response. Letting Y_{it} be a binary indicator equal to 1 for survey respondents and 0 for non-respondents, I estimate the following linear probability model,

$$\text{Prob}(Y_i = 1 | X) = \beta_0 + \beta_1 \text{Wage}_i + \beta_2 \text{Hours}_i + \beta_3 \text{Female}_i + \beta_4 \text{Moved}_i + \epsilon_i \quad (2)$$

As noted above, pre-policy wages, hours and gender are drawn from administrative data from WA OFM, and mover status is determined by a comparison of

²¹Workers who resigned early may have done so due to ordinary job shifting that was unrelated to the policy, such as the acceptance of a superior outside offer. In the [Appendix](#) show estimates including these 37 individuals, and doing so has no effect on the main results.

²²Because post-policy wages are unobserved for unemployed individuals, they are excluded from the sample. If instead unemployed workers are included with their post-policy wage w_{post}^* set equal to zero, estimated treatment effects are roughly three times larger.

²³By comparison, the longest possible panel length consists of $N = 43$ over $T = 7$ periods ($NT = 301$), a significantly smaller sample size.

Table 4 Summary statistics for the treated group ($N = 143$; $T = 4$)

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Year	143	n.a.	n.a.	2010	2013
2010 Hourly Wage	143	15.12	2.63	11.35	40.36
2011 Hourly Wage	143	14.48	2.70	10.83	39.15
2012 Hourly Wage	143	15.30	1.57	11.50	20.00
2013 Hourly Wage	143	13.44	4.10	6.54	38.46
Job Tenure	143	4.11	1.37	2	6
Female	143	0.56	0.5	0	1

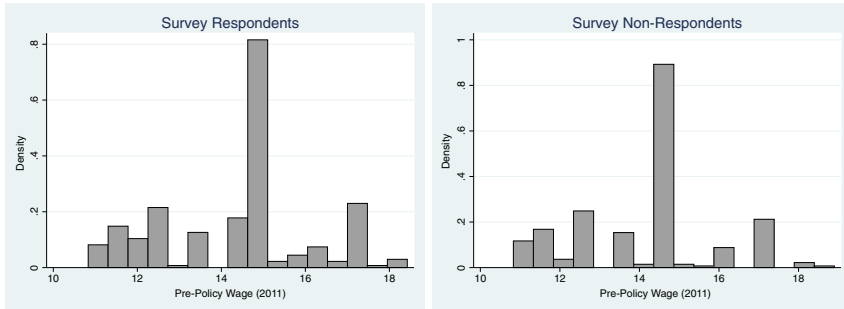
Treatment group consist of the $N = 143$ involuntarily displaced WSLCB workers for whom wages are observed in all years 2010 to 2013. Wages for 2010 to 2012 are from administrative records provided by the WA OFM. Wages for 2013, gender, and job tenure are from an original survey administered by the author. Job tenure codes correspond to the six chronological categories reported bottom-right section of Table 1

Table 5 Assessing bias from survey non-response: regression of probability of survey response on observed population characteristics

	(1)	(2)	(3)	(4)	(5)
Pre-Policy Wage (2011)	0.015*	0.009	0.021	0.023	0.022
	(0.008)	(0.011)	(0.014)	(0.014)	(0.014)
Pre-Policy Wage (2010)		0.013	0.002	0.001	0.000
		(0.012)	(0.015)	(0.015)	(0.015)
Pre-Policy Hours (2011)			-0.005	-0.006	-0.006
			(0.004)	(0.005)	(0.005)
Pre-Policy Hours (2010)			0.005	0.006	0.005
			(0.005)	(0.005)	(0.005)
Female				0.037	0.041
				(0.043)	(0.043)
Mover (New Post-Policy Address)					-0.173***
					(0.064)
n	557	557	557	557	557
Adjusted R-squared	0.003	0.003	0.001	0.001	0.012
F-statistic	3.337	2.717	2.159	1.843	2.717

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of a binary indicator of survey response on observed pre-policy characteristics of individuals in the population of displaced state workers. Pre-policy wage and hours information is from administrative records provided by the WA OFM. Mover status is determined by comparing pre-policy mailing addresses to a post-policy USPS-validated address list. The sample consists of 286 survey respondents and 271 non-respondents for whom wages are observed in all pre-policy years from 2010 through 2012. Heteroskedasticity-robust standard errors are reported in parentheses



Note: Histograms are based on pre-policy 2011 wages for 286 survey respondents (left panel) and 271 non-respondents (right panel) for whom wages are observed in all years from 2010 to 2012. Pre-policy wages are from administrative records provided by the WA OFM.

Fig. 1 Distributions of Pre-Policy Wages for Survey Respondents (*left panel*) and Non-Respondents (*right panel*)

individuals’ pre- and post-policy addresses. The sample consists of 557 individuals for whom wages were observed on all pre-policy years, of whom 286 are survey respondents and 271 are non-respondents. Table 5 presents the estimation results. Taken together, worker characteristics explain roughly 1 percent of the variation in response probabilities. In Column (1), the univariate regression of 2011 wages on response probability is statistically significant but small, suggesting each \$1 higher wages corresponds to a 1.5 percent increased likelihood of survey response. However, this effect disappears when additional controls are included.²⁴ In Columns (2) - (5) all of the estimated coefficients on wages, hours and gender are insignificant, suggesting an absence of systematic selection into survey participation on observables. The only factor that significantly predicts survey nonresponse when all controls are included in Column (5) is having moved to a new address during the post-policy period. Movers were roughly 17 percent less likely to have responded to the survey. To the extent that movers and non-movers experienced different post-policy outcomes, this will not be fully reflected in the sample. However, for those whom I have data on post-policy outcomes, movers and non-movers report post-policy wages that are not statistically different from one another, limiting the practical importance of this potential underrepresentation of migrating households.

Figure 1 shows the unconditional distributions of pre-policy wages among survey respondents and non-respondents. The left panel shows the distribution of 2011 wages among the 286 survey respondents for whom wages are observed in all years, while the right panel shows the distribution of pre-policy wages for the 271 non-respondents. The two distributions are nearly identical, with a modal wage just below

²⁴To further examine differences in wages between respondents and nonrespondents, I estimated the “reverse” of Eq. 2 by regressing wages on a dummy indicator of survey response and all other controls. In all specifications I find the coefficient on survey response is not statistically significant, suggesting no important differences in wages between respondents and nonrespondents. I am grateful to an anonymous reviewer for suggesting this robustness check.

\$15 per hour and a range of roughly \$11 to \$19 per hour. Overall, systematic survey non-response does not appear to pose an important threat to identification.²⁵

Construction of Control Group

As a control group, I identified a collection of similar public employees who were unaffected by the privatization, but whose path of earnings provides a reasonable counterfactual for what the treated workers would have experienced in the absence of displacement. Ideally these individuals would be employed within the same state government, work in similar occupations with comparable skills, and in similar geographic areas as the displaced workers. A category of public employees who closely fit this description are “Customer Service Specialists” in the Washington State Department of Licensing (WA DOL). These employees provide over-the-counter driver’s licensing application and renewal services to the public. As one of the few other retail occupations in state government, these employees have similar job requirements, skill profiles, and urban and suburban locations as the displaced workers. The workers are similarly unionized, and are represented by the American Federation of State, County, and Municipal Employees union (AFSCME 28) Council 28. Wages are established by collective bargaining agreement with the state in a similar way. The path of earnings for these employees would almost certainly have mirrored the path of wages among the displaced WSLCB workers had they been unaffected by the policy, making them an ideal counterfactual group.²⁶

Data on wages, hours, and gender for the control group of workers was provided from WA OFM for the years 2010 to 2013. Unlike treated individuals who responded to the survey, little demographic information is available for the control workers. The information provided by WA OFM was matched at the individual level to public wage data for 2005, 2007 and 2009. Doing so allowed me to construct one additional covariate for the control group: estimated job tenure at WA DOL. The resulting individual-level panel consists of $N = 281$ workers between 2005 and 2013. As with the treated group, the sample was restricted to workers for whom wages were observed in all years from 2010 to 2013. This resulted in a balanced panel of $N =$

²⁵ A second concern is possible misreporting of wages by survey respondents. Displaced workers who were politically opposed to privatization may have incentives to strategically misreport earnings to maximize apparent harm suffered from displacement. It is possible to verify reported pre-policy wages based on administrative records. Survey respondents were asked to report both wages just prior to displacement in June 2012 to allow for such a verification. The average self-reported pre-policy wage was \$14.44 per hour. From administrative records, the actual average pre-policy wage for these same individuals was \$14.18 per hour, a small difference of 26 cents. The remaining gap is likely due to timing differences between self-recall wages and official records, as administrative records are based on a snapshot of wages in early January while self-reported wages are based on self-recall from the pay period immediately preceding displacement in June. For post-policy wages, there is unfortunately no way to independently verify their accuracy and is an inherent limitation of the survey data.

²⁶ Human resources representatives from the WA DOL reported several cases of retail liquor clerks moving into licensing customer service jobs following privatization, further confirming the broad similarity of the two occupational categories (obtained via telephone on April 2, 2014).

119 individuals over $T = 4$ years, for a panel of size $NT = 476$. Table 6 presents summary statistics for this control group. Combining treated and control groups, the overall panel used for my estimation contains $N = 262$ individuals over $T = 4$ years, for a panel of size $NT = 1,048$.

Identification Strategy

Method for Estimating Wage Losses

I identify the causal effect of liquor privatization on wages via a standard difference-in-differences (DD) estimation strategy. Conceptually, I estimate a pooled DD estimator in which panel observations of individuals from the treated and control groups are pooled into two groups in two pre- and post-policy time periods:

$$w_i = \beta_0 + \beta_1 \text{Post}_i + \beta_2 \text{Treated}_i + \beta_3 \text{Post}_i \text{Treated}_i + X_i' \Gamma + \epsilon_i, \quad i = 1, \dots, N, \quad (3)$$

where w_i is the hourly wage of observation i , Post_i and Treated_i are binary indicators equal to one during the post-policy period and for members of the treated group, respectively. X_i is a matrix of individual-level controls consisting of gender and length of job tenure, and ϵ_i is a mean-zero error term. The coefficient of interest is β_3 , the usual DD estimator of the treatment effect of the policy on wages. Because my estimation makes use of a balanced panel of individual-level data, estimating treatment effects via (3) is equivalent to a fixed-effects panel model of the form,

$$w_{it} = \alpha_i + \gamma_t + \beta_3 P_{it} + X_{it}' \Gamma + \epsilon_{it}, \quad i = 1, \dots, N \quad t = 1, \dots, T \quad (4)$$

where α_i and γ_t are fixed effects for individual i and time period t , and P_{it} is a dummy equal to 1 if individual i is treated in period t , and zero otherwise. In both (3) and (4) the estimated $\hat{\beta}_3$ has the same expectation and standard error, and provides

Table 6 Summary statistics for the control group ($N = 119$; $T = 4$)

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Year	119	n.a.	n.a.	2010	2013
2010 Hourly Wage	119	17.45	1.11	14.00	21.50
2011 Hourly Wage	119	17.14	0.93	14.23	20.85
2012 Hourly Wage	119	17.41	1.03	14.54	20.85
2013 Hourly Wage	119	18.32	1.13	15.72	21.50
Job Tenure	119	4.50	0.70	3	5
Female	119	0.71	0.46	0	1

Control group consist of the $N = 119$ Washington State Department of Licensing (WA DOL) “Customer Service Specialist” workers for whom wages are observed in all years 2010 to 2013. Wages, gender, and job tenure are from administrative records provided by the WA OFM. Job tenure codes correspond to the six chronological categories reported bottom-right section of Table 1

a consistent estimate of the causal effect of the policy on wages. Equation (4) is my basic estimating equation.²⁷

To examine whether treatment effects vary throughout the conditional distribution of wages, I also present quantile regression estimates of the effect of job displacement on earnings.²⁸ Finally, as a robustness check I present estimates of propensity-score kernel matching difference-in-difference estimates in the [Appendix](#). For these estimates, I use individual characteristics on gender and length of job tenure to estimate likelihoods of receiving treatment conditional on observables. Individuals in the treated group are then kernel-matched to a composite group of individuals from the control group with similar propensity scores.²⁹

Assessing Pre-Policy Wage Trends

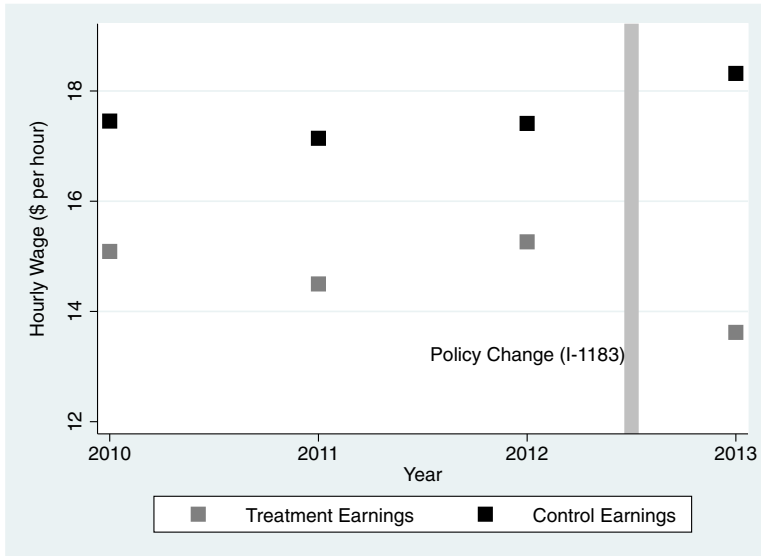
The key identifying assumption for my difference-in-differences estimator is that pre-policy wages for treatment and control workers followed parallel trends. [Figure 2](#) shows hourly wages for the balanced panel of workers for whom wages are observed in all years from 2010 to 2013. The panel consists of 143 treated individuals and 119 control individuals observed over $T = 4$ periods. During the decade before privatization, average wages followed nearly identical trends for the two groups. This is practically by construction as both groups were union represented, had wages established by similar collective bargaining agreements, and were employed by the same state government in similar retail occupations. Average wages grew steadily for both groups from 2005 to 2010, dipping slightly during the post-recession state budget crisis in 2011,³⁰ and stabilizing near pre-recession levels in 2012. Liquor privatization went into effect in June 2012, resulting in job displacement for the treated group. In the post-policy period of 2013, average wages diverge sharply for the two groups, with wages for the control individuals continuing their upward trend while wages fell sharply for treated workers. This parallel evolution in pre-policy wages provides an

²⁷In contrast to much of the previous literature on public-private pay differentials, I estimate effects on direct dollar wages rather than the log of wages. As noted in [Blackburn \(2007\)](#) and [Blackburn \(2008\)](#) the use of log transformations can provide misleading results for public-private pay differentials when wage distributions are less dispersed in the public compared to the private sector, which is clearly the case in this study's data. This point has also been emphasized in [Gittleman and Pierce \(2012\)](#) and [Falk \(2015\)](#).

²⁸Rewriting the linear model from [Eq. 3](#) as $w_i = X_i' \beta + \epsilon_i$, the Koenker and Bassett (1978) quantile estimator $\hat{\beta}_q$ for the average treatment effect at quantile q is given as the solution to $\hat{\beta}_q = \arg \min_{\beta \in R^k} \sum_{i=1}^N \rho_q(w_i - X_i' \beta)$, where $\rho_q = (q - \mathbb{1}\{w_i - X_i' \beta < 0\})(w_i - X_i' \beta)$ is the usual “check function” that penalizes positive regression residuals by q and negative residuals by $1 - q$.

²⁹As detailed in [Heckman et al. \(1998\)](#) and [Todd \(2008\)](#), the resulting kernel-matching difference-in-difference estimator $\hat{\beta}_M$ is given by $\hat{\beta}_M = \frac{1}{n_1} \sum_{i \in I_1} \left\{ (w_{1ti} - w_{0t'i}) - \sum_{j \in I_0} W(i, j)(w_{0tj} - w_{0t'j}) \right\}$, where I_1 is the set of treated workers, I_0 is the set of control workers, t' and t are the pre- and post-policy periods, w_1 and w_0 are earnings for the treated and control groups, and $W(i, j)$ is a weighting function based on the epanechnikov kernel with the default bandwidth of 0.06.

³⁰See Andrew Garber, “Gregoire and Unions Reach Agreement on Pay, Benefit Cuts,” *Seattle Times* (December 15, 2010), available at http://seattletimes.com/html/localnews/2013680687_paycuts15m.html.



Note: Figure shows mean hourly wages for the balanced panel of workers observed in all years from 2010–2013. Sample consists of 143 treated and 119 control individuals, for panel of size $N = 262, T = 4$. Pre-policy wages are observed in January of each year, and are based on administrative records from the WA OFM. The policy went into effect June 1, 2012.

Fig. 2 Evolution of pre-policy wages for the treatment and control groups, 2010–2013

ideal setting for the identification of the causal effect of privatization on earnings via a standard difference-in-differences estimator.³¹

Method for Decomposition of Wage Rents

Treatment effects for displaced WSLCB workers should reflect the loss of three distinct wage rents: union premiums; public sector wage premiums; and lost industry-specific human capital rents. If workers are choosing jobs to maximize earnings, the pre-policy wage is the maximum attainable wage w_{pre}^* for public employees. Following displacement, workers engage in job search, selecting the next highest alternative wage, w_{post}^* . The gap between pre- and post-policy wages $w_{pre}^* - w_{post}^*$ provides an estimate of the extent to which public employee wages exceeded workers’ opportunity cost of employment in WSLCB jobs. The decision by workers to accept employment following job displacement serves as a mechanism for revealing second-best wage offers facing public employees.

By examining linear combinations of treatment effects from various subsamples of workers who sorted into (1) private-sector retail liquor jobs, (2) government jobs, (3) union jobs, and (4) non-union jobs following privatization, overall wage losses can

³¹In the [Appendix](#), I include a figure illustrating parallel pre-policy wage trends for the longer (but smaller NT) panel from 2005 to 2013 as well.

be decomposed into separately identifiable wage rents. Let D be a binary indicator of treatment, T be a binary indicator equal to one in the post-policy period, and I be the set of m post-policy industries into which displaced workers select for employment. Conceptually, estimated treatment effects can be expressed as a linear combination of the three lost wage rents,

$$E(\beta_3 | D, T, I) = \begin{bmatrix} \mathbb{1}_1(\text{union}) & \mathbb{1}_1(\text{govt.}) & \mathbb{1}_1(\text{retail liquor}) \\ \vdots & \ddots & \vdots \\ \mathbb{1}_m(\text{union}) & \mathbb{1}_m(\text{govt.}) & \mathbb{1}_m(\text{retail liquor}) \end{bmatrix} \begin{bmatrix} w_U \\ w_G \\ w_F \end{bmatrix} = \begin{bmatrix} \hat{\beta}_3^1 \\ \vdots \\ \hat{\beta}_3^m \end{bmatrix} \quad (5)$$

where w_U is lost union wage rents, w_G is lost public sector wage rents, w_F is lost rents from industry-specific human capital, and the lefthand matrix is an $m \times 3$ array of zeros and ones reflecting which rents are present among wages in each of the m post-policy industries. By conditioning on the post-policy industry into which workers sort, I can use the m conditional estimates of $\hat{\beta}_3$ to separately identify wage rents. For example, the subsample of workers who remained employed in retail liquor following displacement retained industry-specific human capital rents w_F , but lost union and public sector wage rents w_U and w_G . Similarly, workers who moved into union-represented government jobs elsewhere retained union and public sector rents w_U and w_G , but lost rents due to industry-specific human capital w_F . Similarly, workers in non-union government jobs and private-sector union jobs can be used to separately identify union premiums w_U and public sector premiums w_G .³²

Assessing Bias in Wage Rent Decompositions Due to Selection into Post-Policy Occupation

The above method for decomposing wage rents assumes that displaced workers are of homogeneous ability and their distribution among post-policy occupations is as good as random. Table 7 examines whether systematic self-selection of displaced workers into post-policy occupations poses a threat to the above decomposition approach. For example, before privatization it is possible that wages of public employees masked heterogeneity in ability among workers, as the wage structure was determined by collective bargaining agreement rather than individual negotiations. This heterogeneity could result in non-random selection of displaced workers into post-policy occupations. If high (or low) ability workers systematically sort into high (or low) wage occupations post-policy, the above decomposition method based on subsamples of workers in various occupations may be downward (or upward) biased.

Table 7 shows estimates from a multinomial logistic regression of indicators for each of the four occupations used for the rent decomposition (along with a fifth excluded category for all other occupations) on observed education, experi-

³²This approach is equivalent to specifying dummy indicators for post-policy occupation, and including interaction terms in my basic estimating Eq. 3 for *Post* \times *Treatment* \times *Occupation*. Doing so yields identical results.

Table 7 Testing for post-policy occupational selection: multinomial logitistic regression of post-policy industry on observable characteristics of treated workers

Variable	(2)		(7)		(8)		(10)		Likelihood Ratio Test (H ₀ : All Coefficients = 0)		
	Retail	Liquor	Government	Union	Government	Non-Union	Government	Union Non-Government	Chi-Squared	d.f.	p-value
Education	-0.212 (0.155)	-0.212 (0.155)	0.251 (0.249)	0.251 (0.249)	0.409 (0.385)	0.409 (0.385)	0.116 (0.180)	0.116 (0.180)	5.459	4	0.243
Experience	-0.446 (0.479)	-0.446 (0.479)	1.215 (0.964)	1.215 (0.964)	-1.186 (1.116)	-1.186 (1.116)	-0.145 (0.639)	-0.145 (0.639)	4.244	4	0.374
Experience Squared	0.051 (0.033)	0.051 (0.033)	-0.112 (0.075)	-0.112 (0.075)	0.076 (0.085)	0.076 (0.085)	0.018 (0.044)	0.018 (0.044)	6.51	4	0.164
Female	0.159 (0.363)	0.159 (0.363)	0.142 (0.612)	0.142 (0.612)	1.594 (1.262)	1.594 (1.262)	-0.525 (0.476)	-0.525 (0.476)	3.895	4	0.420
Black / African American	0.089 (0.737)	0.089 (0.737)	-0.01 (1.139)	-0.01 (1.139)	-13.449 (971.405)	-13.449 (971.405)	-13.637 (661.441)	-13.637 (661.441)	4.434	4	0.350
Hispanic	0.7 (0.977)	0.7 (0.977)	-13.51 (1201.510)	-13.51 (1201.510)	-12.931 (1384.244)	-12.931 (1384.244)	0.573 (1.206)	0.573 (1.206)	2.167	4	0.705
Asian / Pacific Islander	0.055 (0.874)	0.055 (0.874)	1.047 (0.910)	1.047 (0.910)	-13.014 (1536.707)	-13.014 (1536.707)	0.463 (0.888)	0.463 (0.888)	1.764	4	0.779
n	222										
Pseudo R-squared	0.077										
Likelihood Ratio Test (Chi-squared)	41.651										
p-value (LR Test)	0.047										

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Multinomial logitistic regression of dummy indicators for post-policy occupation on observable worker characteristics. Columns (2), (7), (8) and (10) correspond to industries from Table 13 used for the wage rent decomposition. The excluded category is a dummy indicator for all other industries. Homoskedastic standard errors are reported in parentheses

ence, experience squared, gender and race for the displaced workers who were employed post-policy. Following the usual practice for Mincerian wage equations, I use reported age as a proxy for labor market experience. The first four columns correspond to the occupations used in the above rent decompositions. All of the estimated coefficients are statistical zeros, and the model explains less than 8 percent of post-policy selection into occupations. The three right-hand columns show results for likelihood ratio tests of joint significance for all of the coefficients for each observable. In all cases, the tests fail to reject the null of zero coefficients with p-values ranging from 0.164 to 0.779. Although this test does not preclude the presence of occupational selection based on unobservable characteristics, there is no evidence in the data that selection on observables is an important concern.

Results

Effect on Earnings

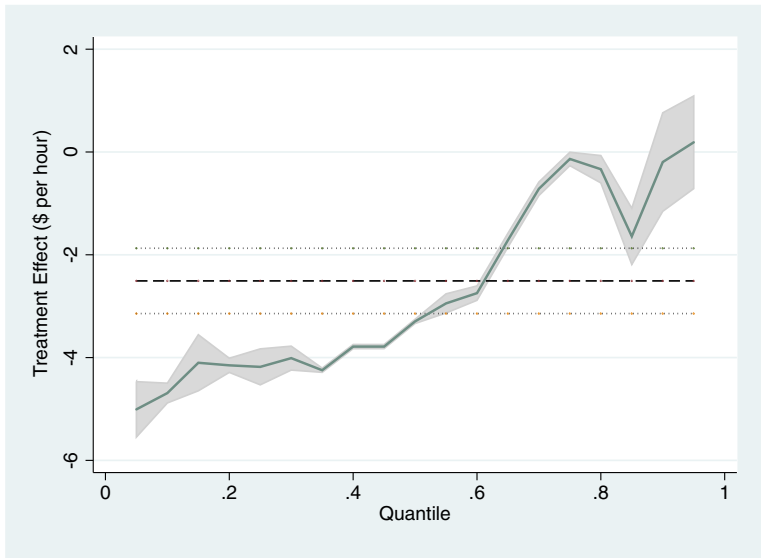
Table 8 shows panel difference-in-differences estimates of the effect of privatization on wages for all workers from 2010 to 2013. The coefficient of interest is β_3 , the standard difference-in-differences estimator. Controls for gender and length of job

Table 8 Panel difference-in-differences estimates of the treatment effect of privatization on wages, all workers, 2010-2013

Observations	Control	Treatment	Total
Pre-Policy	357	429	786
Post-Policy	119	143	262
Total	476	572	1048
R-squared	0.334		
Mean Hourly Wages	Control	Treatment	Difference
Pre-Policy	17.34 (0.088)	14.96 (0.158)	-2.371*** (0.181)
Post-Policy	18.32 (0.104)	13.44 (0.343)	-4.879*** (0.358)
		Difference-in-Differences ($\hat{\beta}_3$)	-2.508***
		Standard Error	(0.396)
		Effect / Treatment Mean	-17.2 %

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level



Note: Quantile regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction for all workers. Effects are estimated at the 5th through 95th quantiles at 5 percent increments. Grey band illustrates 95 percent confidence intervals; dashed line is the OLS mean effect.

Fig. 3 Quantile treatment effects for all displaced workers, 2010-2013

tenure ($X_i' \Gamma$ from Eq. 3) are omitted as they have essentially no effect on the results, and estimates that include them are reserved for the Appendix. Standard errors are reported in parentheses, which are clustered at the individual level. The difference-in-differences estimate of the treatment effect is $\hat{\beta}_3 = -2.508$ per hour. The estimate is highly statistically significant, and represents a 17.2 percent loss in average wages for the treated workers.³³ As expected, liquor privatization resulted in significant earnings losses among displaced public employees and resulted in sharply lower wages during the post-policy period.

Figure 3 shows quantile regression results for treatment effects throughout the conditional wage distribution. The colored line plots estimates of β_3 for quantiles ranging from the 5th to the 95th percentile, in 5-percent increments. The grey band plots the 95-percent confidence interval around these estimates. For comparison, the mean OLS treatment effect and confidence interval from Table 8 is shown as a horizontal dashed line in the figure. The impact of privatization varied widely throughout the wage distribution, with the most severe wage losses occurring in the lowest quintiles. All quintiles below the 60th percentile suffered larger wage losses than the mean, with losses of $-\$4.62$ per hour or 32 percent for the most heavily affected

³³For percentage wage gaps, I use the mean wage of treated workers as the denominator. This can be interpreted as an estimate of the percentage that public-sector wages were reduced by the policy. If mean private-sector wages—which are significantly lower—are used as a denominator instead, percentage effects are roughly 1.5 percentage points larger (e.g., an -18.7 percent treatment effect compared with the -17.2 percent effect in Table 8).

workers at the 5th percentile. By contrast, wages at the 80th, 90th and 95th percentiles were essentially unaffected by the privatization. This is suggestive that low-wage workers were disproportionately adversely affected by the privatization, and that mean effects mask considerable heterogeneity among workers. This evidence is consistent with previous literature on the effect of wage compression in public-sector labor markets. Studies generally find employees at the bottom of the public-sector pay scale enjoy a large earnings advantage relative to private-sector workers at similar points on the earnings distribution, while public employees at the top of the earnings distribution enjoy little or no earnings advantage (see Gregory and Borland 1999 for a discussion of this literature).

Effects in Subsamples

Effects by Gender

Tables 9 and 10 show results separately for male and female employees. Previous research has found female public employees tend to exhibit somewhat larger public sector wage rents than men (Gregory and Borland 1999). Table 9 presents results for men only, consisting of 63 treated individuals and 35 control individuals observed

Table 9 Male workers only: panel difference-in-differences estimates of the treatment effect of privatization on wages, 2010–2013

Observations	Control	Treatment	Total
Pre-Policy	105	189	294
Post-Policy	35	63	98
Total	140	252	392
R-squared	0.244		
Mean Hourly Wages	Control	Treatment	Difference
Pre-Policy	17.41 (0.188)	14.82 (0.309)	–2.590*** (0.362)
Post-Policy	18.72 (0.250)	13.93 (0.520)	–4.789*** (0.577)
		Difference-in-Differences ($\hat{\beta}_3$)	–2.200***
		Standard Error	(0.643)
		Effect / Treatment Mean	–15.1 %

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of involuntarily displaced male workers observed between 2010 and 2013. Standard errors are in parentheses, which are clustered at the individual level

Table 10 Female workers only: panel difference-in-differences estimates of the treatment effect of privatization on wages, 2010–2013

Observations	Control	Treatment	Total
Pre-Policy	252	240	492
Post-Policy	84	80	164
Total	336	320	656
R-squared	0.428		
Mean Hourly Wages	Control	Treatment	Difference
Pre-Policy	17.30 (0.098)	15.07 (0.144)	−2.229*** (0.174)
Post-Policy	18.15 (0.099)	13.05 (0.454)	−5.100*** (0.465)
		Difference-in-Differences ($\hat{\beta}_3$)	−2.871***
		Standard Error	(0.496)
		Effect / Treatment Mean	−19.7 %

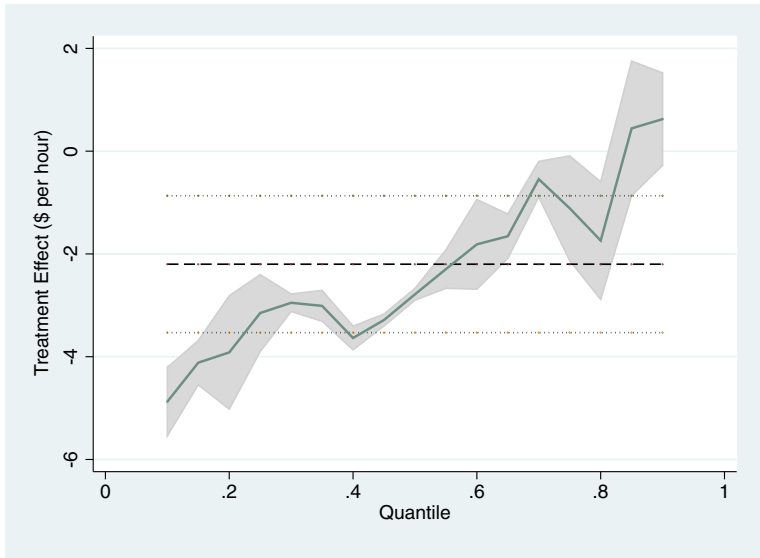
*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of involuntarily displaced female workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level

over 4 periods, for panel of size $NT = 392$. The estimated treatment effect for men is $\hat{\beta}_3 = -2.200$ per hour, a 15.1 percent reduction in average hourly wages. The estimate is roughly 31 cents per hour smaller than for all workers, although the two figures are not statistically different.³⁴ Figure 4 presents quantile regression results for male workers. The size of treatment effects shows considerably more heterogeneity than in the full sample, with sharply different outcomes for workers at the tails of the distribution. The largest negative effects were concentrated in the lowest quantiles, with males at the 5th percentile experiencing treatment effects of $-\$4.225$ per hour or a 29 percent drop in wages. However, male workers above the 65th percentile had a statistically zero treatment effect on wages from displacement.

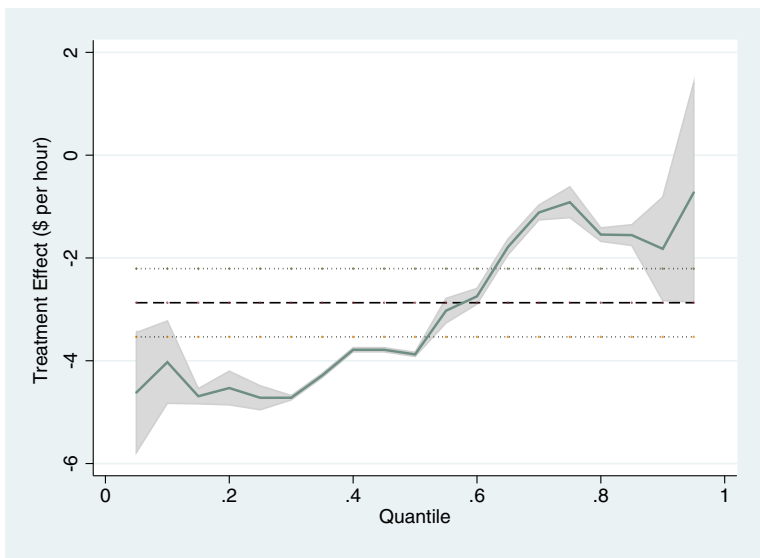
Table 10 shows results for female workers only. The sample consists of 80 treated individuals and 84 control workers observed over 4 years, for a panel of size of $NT = 656$ observations. The mean treatment effect for female workers is $\hat{\beta}_3 = -2.871$ per hour, a 19.7 percent reduction in average wages. The effect of displacement on females is 36 cents larger than all workers, and 67 cents larger than for male workers, suggesting women’s earnings were disproportionately affected by job displacement.

³⁴The pairwise test statistic comparing treatment effects for all workers to male workers ($\hat{\beta}_3^A = \hat{\beta}_3^M$) is $z_{A,M} = -0.75$. Comparing all workers to female workers ($\hat{\beta}_3^A = \hat{\beta}_3^F$), $z_{A,F} = 1.09$. And comparing male workers to female workers ($\hat{\beta}_3^M = \hat{\beta}_3^F$), $z_{M,F} = 0.91$.



Note: Quantile regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction for male workers only. Effects are estimated at the 5th through 95th quantiles at 5 percent increments. Grey band illustrates 95 percent confidence intervals; dashed line is the OLS mean effect.

Fig. 4 Quantile treatment effects for male workers only, 2010-2013



Note: Quantile regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction for all workers. Effects are estimated at the 5th through 95th quantiles at 5 percent increments. Grey band illustrates 95 percent confidence intervals; dashed line is the OLS mean effect.

Fig. 5 Quantile treatment effects for female workers only, 2010-2013

This is consistent with the presence of somewhat larger public sector wage premiums among female employees reported in past literature, although the difference between male and female treatment effects is not statistically significant. Figure 5 shows quantile regression results for female workers. Unlike males, treatment effects are more homogeneous and uniformly negative throughout the conditional wage distribution. The lowest quantiles suffered larger wage losses than the upper quantiles, but treatment effects were negative and significant for all quantiles examined. Women in the

Table 11 Treatment effect of job displacement on wages in various subsamples, 2010–2013

	(1)	(2)	(3)	(4)	(5)
	Full Sample	Retail Liquor (Any)	Retail Liquor, Same Store	Retail Liquor, Different Store	Retail Non-Liquor
Treatment Effect ($\hat{\beta}_3$)	-2.508*** (0.396)	-2.054*** (0.562)	-2.019** (0.979)	-2.090*** (0.518)	-4.433*** (0.565)
n	1048	664	572	568	616
Adjusted R-squared	0.102	0.105	0.099	0.303	0.470
	(6)	(7)	(8)	(9)	(10)
	Any Government	Government Union	Government Non-Union	Any Union	Union Non-Govt.
Treatment Effect ($\hat{\beta}_3$)	-0.279 (1.037)	-0.300 (1.148)	-0.223 (2.256)	-1.698** (0.794)	-2.319** (0.992)
n	520	508	488	580	548
Adjusted R-squared	0.320	0.357	0.444	0.183	0.248
	(11)	(12)	(13)	(14)	(15)
	Young	Middle Age	Older	White	Non-White
Treatment Effect ($\hat{\beta}_3$)	-3.576*** (0.890)	-2.387*** (0.519)	-2.524*** (0.578)	-2.568*** (0.455)	-2.211*** (0.670)
n	520	880	600	952	572
Adjusted R-squared	0.455	0.082	0.278	0.100	0.263
	(16)	(17)	(18)	(19)	(20)
	High School	Some College	AA	BA	Grad School
Treatment Effect ($\hat{\beta}_3$)	-3.894*** (0.666)	-2.204*** (0.684)	-1.547* (0.858)	-1.400 (1.272)	-1.098* (0.650)
n	652	680	568	548	488
Adjusted R-squared	0.226	0.125	0.195	0.153	0.474

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for various subsamples of the balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level

5th percentile experienced wage declines of $-\$5.007$ per hour, a 34 percent average decline, while those in the 95th percentile experienced losses of $-\$1.586$ per hour, an 11 percent decline.

Effects by Occupation, Age, Race and Education

Table 11 shows treatment effects for a variety of worker subsamples. Column (1) repeats the overall treatment effect for all workers as a comparison. Columns (2) to (10) show treatment effects for workers who found employment in a variety of industries during the post-policy period: those who remained in liquor retailing, those who were employed in other government agencies, those who worked in union-represented jobs, and so on. These subsample treatment effects serve as the basis for the decomposition of wage rents in the following section. Treatment effects varied widely by post-policy industry, from an insignificant $-\$0.223$ per hour for those in non-union government jobs to a highly significant $-\$4.433$ per hour for those in non-liquor-related retail jobs.

Columns (11) to (15) show treatment effects for young, middle-age and older workers, as well as for white and non-white employees. The point estimate for younger workers aged 18 to 34 years of $-\$3.576$ per hour is more than one dollar per hour larger than for middle aged or older workers, although it is not statistically different. White and non-white workers suffered similar wage losses from the policy, with slightly larger losses of $-\$2.568$ per hour for white employees compared with $-\$2.211$ per hour for non-white workers. Columns (16) to (20) show treatment effects by level of education. Wage losses from the policy were monotonically decreasing in years of education, ranging from $-\$3.894$ per hour for those with a high-school diploma or less, to $-\$1.098$ per hour for those with some

Table 12 Subsample treatment effects used to decompose lost wage rents

	(2)	(7)	(8)	(10)
	Retail	Government	Government	Union
	Liquor	Union	Non-Union	Non-Government
	($I = r_l$)	($I = g_u$)	($I = g_n$)	($I = u_n$)
Treatment Effect ($\hat{\beta}_3$)	-2.054*** (0.562)	-0.300 (1.148)	-0.223 (2.256)	-2.319** (0.992)
Lost Wage Rents:				
Union (w_U)	X		X	
Public Sector (w_G)	X			X
Industry-Specific Capital (w_F)		X	X	X
n	664	508	488	548

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Subsample treatment effects are repeated from columns of Table 11. Standard errors are presented in parenthesis and clustered at the individual level

graduate school or above. As with many labor market outcomes, the severity of the effect of job displacement on wages is strongly correlated with workers' prior educational attainment.

Results for Wage Rent Decomposition

Table 12 presents the four subsamples from Table 11 used for the wage rent decomposition. Column (2) contains workers who remained in liquor retailing jobs (industry r_l); Column (7) contains workers who moved into union-represented government jobs (industry g_u); Column (8) contains workers who moved into non-union-represented government jobs (industry g_n); and Column (10) contains workers who moved into union-represented, non-government jobs (industry u_n). For each subsample, the table shows treatment effects and the type of wage rent(s) identified. Using these four subsamples there are ${}_4C_3 = (4!)/(3!(4 - 3)!) = 4$ ways to decompose wage rents as the solution to a system of three equations in three unknowns. In order of reliability from largest to smallest sample size, Method 1 uses Columns (2), (7) and (10) to decompose wage rents based on labor market information from 73 treated individuals as follows:

$$\begin{aligned} \text{Column (2): } & E(\beta_3 | D, T, I = r_l) = -2.054 = E(w_U + w_G \quad) \\ \text{Column (7): } & E(\beta_3 | D, T, I = g_u) = -0.300 = E(\quad w_F) \quad (8) \\ \text{Column (10): } & E(\beta_3 | D, T, I = u_n) = -2.319 = E(\quad w_G + w_F) \end{aligned}$$

Method 2 uses treatment effects from Columns (2), (8) and (10) for 68 treated individuals:

$$\begin{aligned} \text{Column (2): } & E(\beta_3 | D, T, I = r_l) = -2.054 = E(w_U + w_G \quad) \\ \text{Column (8): } & E(\beta_3 | D, T, I = g_n) = -0.233 = E(w_U \quad + w_F) \quad (7) \\ \text{Column (10): } & E(\beta_3 | D, T, I = u_n) = -2.319 = E(\quad w_G + w_F) \end{aligned}$$

Method 3 uses estimates from Columns (2), (7) and (8) for 58 treated individuals:

$$\begin{aligned} \text{Column (2): } & E(\beta_3 | D, T, I = r_l) = -2.054 = E(w_U + w_G \quad) \\ \text{Column (7): } & E(\beta_3 | D, T, I = g_u) = -0.300 = E(\quad w_F) \quad (8) \\ \text{Column (8): } & E(\beta_3 | D, T, I = g_n) = -0.223 = E(w_U \quad + w_F) \end{aligned}$$

Finally, Method 4 uses treatment effects from Columns (7), (8) and (10) for 29 treated individuals:

$$\begin{aligned} \text{Column (7): } & E(\beta_3 | D, T, I = g_u) = -0.300 = E(\quad w_F) \\ \text{Column (8): } & E(\beta_3 | D, T, I = g_n) = -0.233 = E(w_U \quad + w_F) \quad (9) \\ \text{Column (10): } & E(\beta_3 | D, T, I = u_n) = -2.319 = E(\quad w_G + w_F) \end{aligned}$$

Table 13 shows the results of the decomposition. The rows corresponds to the four decomposition approaches described above, presenting separate estimates for union wage premiums w_U , public sector wage premiums w_G , and a residual wage premium attributable to lost industry-specific human capital w_F . Under all four approaches, the sum of lost wage rents is consistent with the overall treatment effect above, ranging from $-\$2.24$ per hour to $-\$2.35$ per hour, compared with the overall treatment effect of $-\$2.508$ per hour. Wage losses attributable to public sector rents are by far the

Table 13 Estimated decomposition of wage rents earned by former WSLCB employees

	Lost Wage Rents				
	Treated Group Size	Union (w_U)	Public Sector (w_G)	Industry-Specific Capital (w_F)	Total
Method 1: (2), (7) and (10)	73	-0.03 (1.577)	-2.02 (1.507)	-0.30 (1.142)	-2.35* (1.274)
Method 2: (2), (8) and (10)	68	0.02 (1.246)	-2.08 (1.266)	-0.24 (1.246)	-2.30* (1.269)
Method 3: (2), (7) and (8)	58	0.08 (2.515)	-2.13 (2.577)	-0.30 (1.142)	-2.35* (1.274)
Method 4: (7), (8) and (10)	29	0.08 (2.515)	-2.02 (1.507)	-0.30 (1.142)	-2.24 (2.702)

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Decomposition of wage rents based on linear combinations of estimates from the noted subsample models from Table 11. Standard errors reported in parentheses are clustered at the individual level, and calculated as $\text{Var}[\sum_{i=1}^n \beta_i] = \sum_{i=1}^n \sum_{j=1}^n \text{Cov}[\beta_i, \beta_j]$ for the linear combination of n subsample coefficients

largest, accounting for $-\$2.02$ to $-\$2.13$ per hour or roughly 86 to 91 percent of total wage losses following privatization. Industry-specific human capital accounts for the second largest component, ranging from $-\$0.24$ to $-\$0.30$ per hour or roughly 11 to 13 percent of the total. Union premiums were negligible in all four decompositions, accounting for just $-\$0.03$ per hour of wage losses using the first approach and a slightly negative union premium of between 2 and 8 cents in the remaining three approaches. The results are consistent with the presence of a roughly 16 percent public sector wage premium among the displaced WSLCB workers.³⁵

Conclusion

The issue of public employee compensation has long been controversial. Despite a well-established literature finding public sector wage premiums among federal workers, the evidence for the roughly 5.3 million state government employees currently employed in the United States remains mixed. This study contributes to the literature by providing new estimates of public sector wage rents for state employees based on quasi-experimental evidence from a 2012 privatization of liquor retailing in Washington State.

Based on a panel difference-in-difference estimator, I find that wages of state employees displaced by privatization fell roughly $-\$2.508$ per hour or 17.2 percent

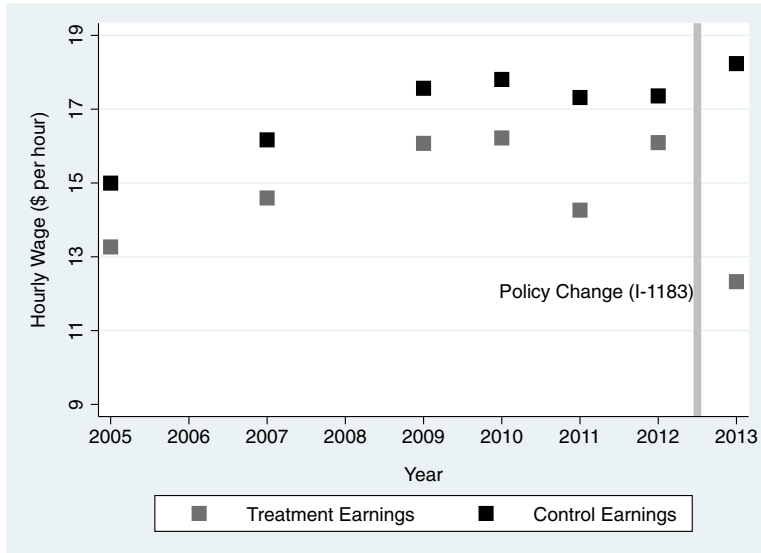
³⁵It is important to note that the finding of a roughly zero union wage premium is the result of there being no detectible private-sector union wage differential in my data. It is likely the results would find a larger union premium if my survey revealed a private-sector union pay differential similar to those found in national studies based on Current Population Survey data.

relative to a similar group of public employees unaffected by the policy, with somewhat larger effects for female workers. By decomposing this overall effect into public sector wage rents, union premiums and losses due to industry-specific capital I find evidence of a roughly 16 percent public sector wage premium. The results are unaffected by the inclusion of controls for gender and length of job tenure; by excluding workers who reported a higher value of non-wage “fringe” benefits in post-policy jobs; and when estimated via a propensity score kernel matching estimator.

The finding of a roughly 16 percent public sector wage premium is considerably larger than estimates reported by previous longitudinal studies that do not rely on exogenous job separations, suggesting endogenous job switching may be a source of significant downward bias these estimates. However, the findings are broadly consistent with other studies that have examined earnings of government workers following privatization-related displacements in Brazil, Argentina and elsewhere. Although the estimated wage premium for liquor retail workers may not easily generalize to broader categories of state workers, it may be informative to other U.S. states considering privatization of liquor retailing.

Appendix

Pre-Policy Wage Trends 2005 to 2013



Note: Figure shows mean hourly wages for the longer (but smaller *NT*) balanced panel of all involuntarily displaced workers for whom wages are observed in all years from 2005–2013. Sample consists of 43 treated and 73 control individuals, for panel of size $N = 116$, $T = 7$. Pre-policy wages are observed in January of each year, and are from administrative records provided by the WA OFM. The policy went into effect June 1, 2012.

Fig. 6 Evolution of pre-policy wages for treated and control workers, 2005–2013

Treatment Effects Including Worker Covariates

Table 14 shows treatment effects of displacement on wages including individual-level covariates for gender and job tenure, which are omitted from the baseline estimates presented in the paper. The figures correspond directly to my estimating Eq. 4 and are presented separately for all workers, males, and females. The additional covariates are shown in the first two rows. In the regression for all workers in Column (1), gender and length of job tenure are statistically insignificant. Job tenure is statistically significant only in the model restricted to female workers, in which case it has a small effect of \$0.17 per hour. Overall, the point estimates for treatment effects $\hat{\beta}_3$ are identical to those that omit individual covariates presented in the paper.

Treatment Effects with Kernel Matching Difference-in-Differences Estimator

Table 16 shows estimated treatment effects of displacement on wages using a propensity score kernel matching difference-in-difference estimator. Table 15 shows the

Table 14 Panel difference-in-differences estimate of treatment effect, including gender and job tenure covariates, 2010–2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Female	−0.172 (0.184)		
Job Tenure	0.157 (0.105)	0.126 (0.215)	0.173* (0.100)
Post Policy	0.983*** (0.062)	1.307*** (0.149)	0.848*** (0.055)
Treatment	−2.336*** (0.163)	−2.504*** (0.308)	−2.205*** (0.170)
Post Policy x Treatment ($\hat{\beta}_3$)	−2.508*** (0.396)	−2.200*** (0.644)	−2.871*** (0.496)
Constant	16.751*** (0.529)	16.851*** (0.991)	16.523*** (0.479)
n	1048	392	656
Adjusted R-squared	0.335	0.238	0.430

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly worker wages on binary dummies for treatment, post-policy period, their interaction, and individual-level controls for gender and length of job tenure. Estimation is for the balanced panel of all workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level

Table 15 First stage propensity score estimation: estimated probability of treatment conditional on observables, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Job Tenure	-0.173*** 0.042	-0.339*** (0.070)	-0.074 0.053
Gender	-0.335*** 0.096		
Constant	1.410*** 0.226	1.770*** (0.304)	0.300 0.242
n	1048	392	656
Pseudo R-squared	0.03	0.06	0.01

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression of the probability of treatment on individual characteristics for gender and length of job tenure. Sample is a balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors in parentheses are clustered at the individual level

Table 16 Second stage: kernel matching difference-in-differences estimates of the treatment effect, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Post Policy	1.265*** (0.160)	1.532*** (0.267)	0.903*** (0.068)
Treatment	-2.070*** (0.252)	-2.072*** (0.447)	-2.153*** (0.187)
Post Policy x Treatment ($\hat{\beta}_3$)	-2.790*** (0.422)	-2.425*** (0.680)	-2.926*** (0.498)
Constant	17.034*** (0.196)	16.897*** (0.323)	17.226*** (0.119)
n	1048	392	656
Adjusted R-squared	0.299	0.207	0.409

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression uses a propensity score matched kernel difference-in-differences estimator to estimate the effect of treatment on hourly worker wages. Treated workers are kernel matched to members of the control group via the epanechnikov kernel using a default bandwidth of 0.06. Estimation is for the balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level

results of the first stage of the procedure in which individual characteristics are used to estimate treatment likelihoods. In the second stage, the resulting propensity scores are used to kernel match treated individuals to a composite group of control group members based on the epanechnikov kernel with a default bandwidth of 0.06. Overall, the procedure results in somewhat larger estimated treatment effects of $-\$2.790$ per hour for all workers, and $-\$2.425$ and $-\$2.926$ per hour for males and females, respectively. However, none of the results are statistically different from the OLS difference-in-differences estimates presented in the paper.

Treatment Effects Excluding Workers with Higher Value of Post-Policy Fringe Benefits

Table 17 shows treatment effects of displacement on wages excluding from the sample the 8 individuals who reported receiving more valuable non-cash “fringe” benefits in their post-policy employment. The estimates address the concern that observed wage losses among displaced WSLCB workers may have been partially or completely offset by increases in post-policy non-wage benefits. On the contrary, estimated wage losses are somewhat larger when the sample is restricted to workers reporting the same or less valuable fringe benefits ($-\$2.809$ per hour compared with $-\$2.508$), suggesting substitution between cash and non-cash compensation does not explain the pattern of treatment effected reported in the paper.

Table 17 Treatment effect of job displacement on wages, excluding individuals who reported higher value of fringe benefits post-policy, 2010–2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Post Policy	0.983*** (0.062)	1.307*** (0.149)	0.848*** (0.055)
Treatment	-2.331 *** (0.187)	-2.616 *** (0.370)	-2.128 *** (0.175)
Post Policy x Treatment ($\hat{\beta}_3$)	-2.809 *** (0.357)	-2.271 *** (0.658)	-3.384 *** (0.354)
Constant	17.335*** (0.088)	17.415*** (0.188)	17.302*** (0.098)
n	1016	384	632
Adjusted R-squared	0.378	0.245	0.550

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of all involuntarily displaced workers observed between 2010 to 2013, excluding the 8 employed individuals who reported receiving more valuable non-cash “fringe” benefits in their post-policy employment. Standard errors are in parentheses, which are clustered at the individual level

Treatment Effects Including Voluntary Job Separators

The paper's baseline estimates exclude all individuals from the sample who voluntarily separated from their WSLCB job prior to the policy enactment date of June 1, 2012. Table 18 shows treatment effects including in the sample the 10 individuals for whom wages are observed in all pre-policy years and who voluntarily separated. The treatment effect of job displacement is a somewhat smaller $-\$2.342$ when these individuals are included, suggesting voluntary quitters fared better on average than involuntarily displaced workers. This is consistent with ordinary job shifting behavior that is unrelated to the policy change, as voluntary separators likely quit to accept higher wage offers elsewhere.

Survey Materials

A copy of the survey questionnaire of displaced WSLCB workers is provided below. Information on institutional review board approval by the University of California, San Diego's Human Research Protections Program is available at <http://irb.ucsd.edu/>.

Table 18 Treatment effect of job displacement on wages, including individuals who voluntarily quit prior to the policy date of June 1, 2012, 2010–2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Female	−0.112 (0.182)		
Job Tenure	0.184* (0.103)	0.141 (0.211)	0.204** (0.102)
Post Policy	0.983*** (0.062)	1.307*** (0.149)	0.848*** (0.055)
Treatment	−2.299*** (0.159)	−2.484*** (0.300)	−2.179*** (0.165)
Post Policy x Treatment ($\hat{\beta}_3$)	−2.342*** (0.385)	−2.258*** (0.618)	−2.516*** (0.492)
Constant	16.585*** (0.527)	16.788*** (0.973)	16.381*** (0.487)
n	1088	404	684
Adjusted R-squared	0.316	0.241	0.382

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively

Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of all workers observed between 2010 to 2013, including the 10 individuals who voluntarily quit WSLCB jobs prior to the policy date of June 1, 2012 and for whom wages are observed in all years 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level

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SANTA BARBARA • SANTA CRUZ

**PLEASE ANSWER THE FOLLOWING QUESTIONS AS HONESTLY AS POSSIBLE.
ALL REPOSSES WILL REMAIN COMPLETELY ANONYMOUS.**

1. When was your last day at your retail liquor job for the Washington State Liquor Control Board? (Mark the month and year).

Year:

Month:

 2011 Jan April July Oct 2012 Feb May Aug Nov Mar June Sept Dec

2. Were you laid off or did you quit voluntarily?

 Laid off Quit voluntarily

3. Before you left that job, how long did you work for the Wash. State Liquor Control Board?

 Less than one year 5-6 years 1-2 years 7-10 years 3-4 years 10+ years

4. What is your current employment status today? (Please check only one box.)

 Employed full time Not employed, not looking for work Employed part time Retired Self-employed Student Not employed, but looking for work Homemaker

5. If you are unemployed, how much are you receiving per week in unemployment benefits? (Your best estimate is fine.)

 My unemployment benefits are: \$ /week. I am not currently receiving any unemployment benefits.

6. If you are employed, what is the industry of your main job? (If you are not employed please skip ahead to **Question 12**.)

 Retail liquor store, in the same location I used to work at Retail liquor store, in a different location Retail, but not at a liquor store Education or healthcare Restaurant or hotel services Manufacturing Construction Transportation (e.g., bus driver) or warehouse job Technology or software Administrative job Finance, real estate, or insurance

Agriculture, forestry, or fishing
 Government (city, state or federal government)
 Other industry (please specify) _____

7. If you are employed, how many hours per week do you work at your main job? (If you are not employed please skip this question.)
 Full time: 40 hours per week
 Part time: 20 hours per week
 Other: Please specify hours per week _____.

8. If you are employed, are you union represented at your current job? (If you are not employed please skip this question.)
 Yes
 No

9. If you are employed, what is your hourly wage or monthly salary? (If you are not employed please skip this question.)
Hourly wage: \$ _____/hour OR Monthly salary: \$ _____/month

10. If you are employed, what benefits does your current employer provide? (If you are not employed please skip this question.)
 Health insurance 401(k) or other retirement plan No benefits
 Dental insurance Daycare or child care
 Paid vacation / sick leave time Transportation or bus pass subsidy

11. If you are employed, think about the dollar value of your current benefits. Are they worth less, more, or about the same as the benefits you received at your Washington State liquor retail job? (If you are not employed please skip this question.)
 My benefits are less valuable than my old ones
 My benefits are more valuable than my old ones
 My benefits are about the same value as my old ones

12. Thinking back to your old Washington State liquor retail job, how many hours per week did you work at that job?
 Full time: 40 hours per week
 Part time: 20 hours per week
 Other: Please specify hours per week: _____ hours/week

13. Thinking back to your old Washington State liquor retail job, what was your hourly wage or monthly salary at that job?
Hourly wage: \$ _____/hour OR Monthly salary: \$ _____/month

14. Thinking back to your old Washington State liquor retail job, do you remember the store number or address of the location where you worked?

Store Number: # Address: _____
 City: _____ Zip: _____

15. What is your age?

- | | | |
|--|--|--|
| <input type="checkbox"/> 16 – 19 years | <input type="checkbox"/> 35 – 39 years | <input type="checkbox"/> 55 – 59 years |
| <input type="checkbox"/> 20 – 24 years | <input type="checkbox"/> 40 – 44 years | <input type="checkbox"/> 60 – 64 years |
| <input type="checkbox"/> 25 – 29 years | <input type="checkbox"/> 45 – 49 years | <input type="checkbox"/> 65+ years |
| <input type="checkbox"/> 30 – 34 years | <input type="checkbox"/> 50 – 54 years | |

16. What is your race or ethnicity?

- | | |
|--|--|
| <input type="checkbox"/> White | <input type="checkbox"/> Native American or Alaskan native |
| <input type="checkbox"/> Black / African American | <input type="checkbox"/> Other race |
| <input type="checkbox"/> Hispanic | <input type="checkbox"/> Mixed racial background |
| <input type="checkbox"/> Asian or Pacific Islander | |

17. What is the highest grade you completed in school?

- | | |
|--|--|
| <input type="checkbox"/> Less than high school | <input type="checkbox"/> Associate's degree (e.g., A.A.) |
| <input type="checkbox"/> Some high school | <input type="checkbox"/> College degree (e.g., B.A., B.S.) |
| <input type="checkbox"/> High school diploma or GED | <input type="checkbox"/> Some graduate school, but no degree |
| <input type="checkbox"/> Some college, but no degree | <input type="checkbox"/> Graduate degree (e.g., M.A., Ph.D.) |

18. What is your marital status?

- | | |
|--|--|
| <input type="checkbox"/> Single, never married | <input type="checkbox"/> Separated |
| <input type="checkbox"/> Married | <input type="checkbox"/> Widowed |
| <input type="checkbox"/> Divorced | <input type="checkbox"/> Living with a partner, but single |

19. If you are married or living with a partner, is your spouse/partner currently employed?

- Yes
 No
 Not Applicable (single, divorced, widowed, etc.)

20. How many adults currently live in your household (including you)?

- | | | |
|----------------------------|----------------------------|-----------------------------|
| <input type="checkbox"/> 1 | <input type="checkbox"/> 3 | <input type="checkbox"/> 5+ |
| <input type="checkbox"/> 2 | <input type="checkbox"/> 4 | |

21. How many children currently live in your household?

- | | | |
|----------------------------|----------------------------|-----------------------------|
| <input type="checkbox"/> 0 | <input type="checkbox"/> 2 | <input type="checkbox"/> 4 |
| <input type="checkbox"/> 1 | <input type="checkbox"/> 3 | <input type="checkbox"/> 5+ |

22. What is your gender?

- Male
 Female

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