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The State and Local Pay Penalty: the Effect of Institutions, Skill, and College Major

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Abstract

Employment in state and local government is almost always governed by civil service statutes, often overlayed by collective bargaining agreements. These rules encourage seniority pay and limit performance pay, compress wages, regulate promotion, and provide significant job security. Such constraints in compensation should cause significant selection between the public and private sectors. Nonetheless, studies of public sector compensation employ OLS wage regressions controlling for observable worker characteristics and usually conclude that there is a large pay penalty for public sector work. This paper explores selection issues in the estimation of public sector pay differentials by employing two measures of ability not previously considered: AFQT scores and undergraduate major. The results using AFQT scores show that for those with college degrees the public sector (1) employs less skilled workers and (2) does not compensate for skill. These two results explain the entire public-private pay differential for those with college degrees. For those with high school degrees, public sector workers are compensated for skill similarly to private sector workers, and there is no negative selection into public sector employment. I argue that the disparate results for high school and college educated employees are consistent with the use of credentials to promote and hire college educated public sector workers, and the use of tests to hire and promote high school educated public sector workers. The AFQT results imply that public employees with high skills are significantly undercompensated. However, the AFQT is a measure of skill taken during high school. Skills acquired in college also explain a great deal of the differences in pay between the two sectors. Between one-half to two-thirds of the public sector pay penalty can be explained by crude controls for undergraduate major, which is a strong proxy for both skill and outside options available to employees. I conclude that OLS estimates of the public sector pay penalty are greatly overstated.

Employment in state and local government is almost always governed by civil service statutes, often overlayed by collective bargaining agreements. These rules encourage seniority pay, limit performance pay, compress wages, regulate promotion, and provide significant job security. Such constraints in compensation should cause significant selection between the public and private sectors. If government jobs are more secure and compensate based largely on seniority and credentials, then less able workers may seek public sector employment, while more skilled workers will seek employment in the private sector. Nonetheless, studies of public sector compensation employ OLS wage regressions controlling for observable worker characteristics such as such as age and education, and then interpret the dummy variable for employment by state and local governments as the state and local pay penalty. This approach has (with few exceptions) yielded a consensus view that state and local workers face a pay penalty between 6 and 12 percent. Additional back-of-the-envelope calculations generally suggest that the benefits premium in the public sector approximately offsets the state and local pay penalty (see, e.g., Munnell 2012). However, OLS estimates of the public sector pay penalty rely on the assumption of what Heckman and Robb (1985) call "selection on observables" to identify the wage differential between the two sectors. Put plainly, the approach requires that unobservable factors that would otherwise affect wages are not driving selection into public employment.

This paper tests for selection issues in the estimation of public sector pay differentials by employing two measures of ability not previously considered: Armed Forces Qualifications Test (AFQT) scores from the National Longitudinal Survey of Youth (NLSY) and undergraduate major from the American Community Survey (ACS). Consistent with prior work, regressions in both datasets suggest that state and local workers with only high school degrees enjoy a pay premium, while state and local workers with college degrees or more earn substantially less than their observationally equivalent private sector counterparts. However, for those with college degrees or more, the public sector pay penalty dramatically attenuates when proxies for skill are included in the regressions.

The results using AFQT scores in the NLSY show that for those with college degrees or more the public sector (1) employs less skilled workers and (2) does not compensate for skill. By contrast, the AFQT score is an important predictor of private sector compensation. AFQT score by itself does not explain a large portion of the public sector penalty. However, AFQT and differential return to AFQT together explain the entire public-private pay differential for those with college degrees or more. By contrast, for those with high school degrees, public sector workers are compensated for skill similarly to private sector workers, and there is no negative selection into public sector employment. I argue that these disparate results for high school and college eduated employees are consistent with the use of credentials to promote and hire college educated public sector workers, and the use of tests to hire and promote high school educated public sector workers.

The AFQT results imply that instead of under or overcompensating, the public sector may "miscompensate" due to its rigid pay structure. Highly skilled public employees are significantly undercompensated, while low-skilled public employees are significantly overcompensated relative to the private sector. To explore this possibility, I also consider skills acquired in college as measured by undergraduate major. Since 2009, the ACS has asked respondents with a college education or more about their undergraduate major. Pay varies greatly by undergraduate major, in part because of skill-based selection into different majors and in part because the market demand for different skills acquired in college—for example, quantitative skills versus verbal skills, or specific technical or scientific knowledge. Depending on the specification, simple controls for thirty-eight college major categories explain between one-half and two-thirds of the public-private wage differential for those with bachelor and master's degrees. I conclude that the public sector pay penalty is greatly overstated in OLS estimates.[1]

The large ACS sample allows some additional analyses. First, the non-profit sector provides another basis of comparing pay differences. If selection into public sector and non-profit employment are driven by similar factors, then a comparison between public sector and non-profit workers may be a more accurate estimate of the public sector pay differential. I find that public sector workers earn a premium relative to those in the non-profit sector. Second, the large ACS sample size is exploited to study regional variation in the public-private wage

[1] This paper does not employ measures of total compensation. A lingering problem in any comparability study is how to deal with benefits and other forms of in-kind compensation in the public sector. Among these are greater job security, defined-benefit pensions, early retirement, greater sick leave, and more holidays than private sector work. Such comparisons are beyond the scope of this paper and not easily monetized, but assessing the wage disparity remains an important step in understanding the private and public sector total compensation differentials. Apart from Gittelman and Pierce (2012), discussed below, most papers employ back-of-the-envelope calculations using approximations based on the Employer Cost of Employee Compensation survey.

differential. The results demonstrate that (1) in states with strong public-sector unions there is no public sector pay penalty for those with bachelor or master's degrees and (2) in states that prohibit state and local governments from collective bargaining, the public sector pay penalty is large. This dramatic regional variation suggests different institutional arrangements are important determinants of public sector pay.

This paper proceeds as follows. Part 1 discusses public sector compensation and the literature on public sector pay penalty; Part 2 discusses the pay penalty as estimated in the NLSY and its sensitivity to AFQT scores; Part 3 discusses the pay penalty in the ACS and its sensitivity to college major and different comparison groups; and Part 4 concludes.

1. Wage Differentials and Public Sector Compensation

Those making employment decisions in the public sector face significant constraints. The public sector is characterized by rigid compensation and promotion structures, largely determined by civil-service statutes and regulations combined (where available) with collective bargaining agreements. By contrast, private-sector firms are free to set their employment policies subject only to the regulations of anti-discrimination laws, the Fair Labor Standards Act, and other employment statutes. One-half of local employees and 35 percent of state employees in the United States are covered by collective bargaining agreements, compared to 7 percent of workers in the private sector. Civil service protections are the norm in state and local employment. As of the 1980s, roughly 80% of state and local employees were covered by a civil service statute or merit system and almost 90% of jurisdictions had a civil service system in place (Kearney 1984). A recent survey concluded that thirty-four states have comprehensive merit systems that cover almost all employees (Ferguson 2006). Moreover, federal law has long required that state and local employees paid through government grants be covered by a merit-based civil service system (42 U.S.C. §4701).

Civil service systems and collective bargaining agreements typically limit adverse employment actions without significant administrative process, govern promotion decisions and limit hiring discretion, and also regulate wages. Critics argue that the system has evolved in most places into a complex seniority promotion and pay system (for reviews see Kearney 2009 and Ingraham 1996). However, some rigidness in public sector employment policies is inevitable. Holmström and Milgrom (1991) suggest that in fields where performance of any of the activities of workers is difficult to measure, fixed wages and salaries may themselves be the optimal incentive structure, and indeed such policies are often observed in the private sector.

Given the uniqueness of the incentives facing the public sector and the constraints in public sector pay, it is reasonable to ask whether public sector workers are compensated appropriately. As a theoretical matter, state and local workers could be compensated more than necessary to retain or attract the current quality of public sector employee. Overcompensation could result from a combination of interest group action and poorly incentivized officials acting as agents for taxpayers. Overcompensation could result also from the regulated nature of public sector wage and benefits structures.

Undercompensation, a frequent finding in the literature, is actually a bit more challenging to explain from a theoretical perspective. For any wage-benefits-amenities offer, the public sector will get a set of applicants given the outside options available to the potential labor pool, and public sector decision-makers will choose from among them. If by "undercompensation," the literature means "below market wages" or that that public sector work offers a career trajectory and implicit contract that is not competitive with the private sector, then we should observe vacancies that the public sector cannot fill. However, the available evidence, though it is not recent, is that there tends to be queuing for public sector jobs at the federal, state, and local level.[2]

Monopsony power on the part of the state and local government as employers could result in below market wages. Because the public sector is a large employer and the primary employer of certain occupation groups, the potential for monopsony power cannot be dismissed out of hand. Moreover, recent evidence suggests that monopsony power may be more present in labor markets than has previously been assumed, in part because asymmetric information and employer differentiation creates some pricing power (see Ashenfelter et al. 2010 for a survey).

As a large employer with only a few similarly situated competitors nearby, state governments may have some monopsony power with respect to employment, even if the public sector must compete with the private sector for workers. It is harder to believe the monopsony story with respect to local governments, or at least that local governments possess greater

[2] Kruger (1988) and Venti (1987) examine queuing for federal jobs and conclude that they receive significantly more applications than private sector counterparts. Heywood and Mohanty (1993) apply a sector choice model to state and local government workers and find queuing as well, though for state governments only when the union pressure is evident.

monopsony power than private sector firms. Even in the case where one's training or career path makes the public sector the most likely employer, there are thousands of local jurisdictions. There are more than 13,000 school districts, 2,600 counties, and over 600 municipalities with populations over 50,000 in the United States. There is some evidence of competition between local governments for workers. For example, at the school district level, between three and four percent of teachers change districts each year (U.S. Department of Education, 2000).

The issue of public sector compensation includes not just the wage differential, but the ability to select, promote, and retain good workers within the constraining institutional contexts the public sector faces. Miscompensation may be a more significant concern than poorly defined terms such as over or undercompensation. For example, the public sector, faced with far weaker price signals for the value of its goods than that which the private sector receives, may offer a wage and benefits package that draws too low a quality worker than would be socially optimal. Moreover, collective bargaining and civil service protections create a relatively uniform compensation structure, which should lead to significant selection between the two sectors.

Previous Estimates of Public-Private Wage and Benefit Differentials

Most recent studies of state and local compensation conclude that the wage penalty for state and local workers is between -6 and -12 percent, with the penalty being substantially larger for state workers than for local workers and for the highly educated relative to those with less education. Most of these approaches take as their starting point OLS wage regressions based on the March Current Population Survey, and typically control for years of education, age, race, sex, and marital status, and sometimes employer size and occupation (Lewin et al. 2012; Munnell 2012; Allegretto and Keefe 2010; Bender and Heywood 2010; Keefe 2010; Center for Retirement Research at Boston College 2011a; Richwine and Biggs 2011).

Some of the literature tries to address the value of benefits as well, which is thought to favor public sector employees. Allegretto and Keefe (2010) and Keefe (2010) use the Employer Cost of Employee Compensation (ECEC) survey to adjust for benefits and conclude that benefits do not fully offset the pay penalty. Munnell (2012) and Richwine and Biggs (2011) critique this approach for not adequately valuing pensions or retiree health insurance. Because public sector pensions are defined benefit, usually at least 50 percent of wages and indexed-linked, comparability to 401(k) plans requires significant adjustments. Moreover, employers do not prepay for retiree health care, and consequently it is missing from the ECEC. Underfunding of pension obligations would also reduce ECEC benefit estimates because the cost to the employer is significantly understated. Both the Munnell and Richwine and Biggs analyses conclude that health and retirement benefits roughly cancel out the wage differential.[3]

Gittleman and Pierce (2012) employ the ECEC data on wages and benefits, which is gathered from employer administrative records, to estimate the public-private wage differential and total compensation differential based on standard controls for age, sex, race, and education. In a notable departure from the literature, they estimate a -8 percent pay penalty for state workers (smaller than in prior work), and a 2.5 percent premium in favor of local employees. When the authors consider total compensation based on the value of fringe benefits provided in the administrative data, they estimate a total compensation premium of 3 to 8 percent for state workers and 10 to 19 percent for local workers. The authors attribute the difference between their findings and that in the broader literature to the nature of the administrative data, noting that occupational pay differentials likewise exhibit differences across data sources.

Only a few papers address selection issues in public sector employment. In the teaching context, Hoxby and Leigh (2004) document that (1) teacher wages became more compressed since the 1960s, which they attribute to unionization, and that (2) the aptitude tests of education majors declined similarly since then. Koedel et al. (2013) study teacher pensions and conclude that pensions create incentives for teachers to stay too long or leave too early, but find no consistent evidence of an effect of teacher pensions on teacher quality.

Borjas (2002) finds that public sector wages became more compressed relative to those of the private sector since the 1960s, leading to an increase in the private sector wage premium, and further that the tendency of high-quality public sector workers to leave to the private sector increased over time. His analysis, however, did not account for the tendency of public sector workers to take early retirement and transition to the private sector. Earlier work by Gyourko and Tracy (1988) used Heckman-type selection models to correct for selection into the public

[3] In the literature, the value of job security and early retirement as benefits are rarely accounted for and can only be addressed by making strong and contestable assumptions. Moreover, these in-kind benefits also interact with retiree health and pension benefits. A study using the Health and Retirement Survey (Center for Retirement Research at Boston College 2011b) found that couples who spent most of their careers in the public sector had 22 percent greater wealth at age 65 than private sector couples with similar observable characteristics. A portion of this significant differential is likely due to defined-benefit pension plans, both because they are generous and less risky than 401(k) plans, and also because a majority of public sector workers continue to work after taking retirement from their public sector jobs (Id.).

and private sector, but their identifying assumption was that educational attainment was exogenous.

Controls and Unobservables

The key problem with estimating pay differentials is in establishing the counterfactual. What would a public sector employee make in the private sector? The attempt to create this counterfactual has largely been through controlling for observables, under the assumption that the observables are proxying to some extent for ability. Two controversial characteristics significantly affect the estimate of the public sector pay penalty: (1) establishment size and (2) occupational controls. Controlling for education, race, and sex, has been relatively uncontroversial, but I will argue that these controls are also problematic.

Establishment size (which the ACS lacks and the NLSY codes crudely) is well-known to be strongly correlated with worker wages. Why exactly this is true remains unclear, but the two ideas usually advanced are (1) positive selection into large firms and (2) rent sharing created by higher productivity in large firms. There is likely truth in both explanations. For example, it is well established that workers in large firms are more productive and positively selected on observables, and that controlling for observables still leaves a significant unexplained establishment-size wage premium (Idson and Oi 1999; Troske 1999). There is also some evidence consistent with positive selection on unobservables, combined with better managerial skill, in larger firms (Fox 2009).

However, neither the rent-sharing or positive selection stories justify an establishment size control in studies of public sector pay. Under the selection story, firm size should be controlled for only if the selection on worker ability in the private sector is the same as in the public sector. The results below on the AFQT and public employment will show that there is negative selection on ability in public sector employment. The rent sharing story also does not justify establishment size controls. If the story is primarily one of rent sharing, then underlying worker characteristics are not relevant.

Whether and how to control for occupational categories is another significant challenge. There are large observed pay differentials between different occupations, likely having to do with skill and demand side factors. However, some occupational controls may not be reasonable because there is little common support between the public and private sectors. For example, law enforcement workers are almost exclusively public employees. Controlling for a narrow category would be like saying public employees are not paid differently relative to public employees. If one broadens the occupational category of public safety to "safety worker" so as to include private-sector employees, the researcher is imposing a condition in which a mall security guard or front desk worker at an office building is similar to that of a highway patrol officer or beat cop. Of course, these are jobs with different amenities requiring different levels of skill and training. In some specifications, I will include the broad, 2-digit Census occupation controls (twenty-five categories in all) so the reader can assess the sensitivity of the results to occupational controls.

Race and sex are often included as controls, but it is not clear they are valid. For example, if the public sector discriminates less than the private sector, then controls for race and sex are valid only if one wishes to measure the public sector pay penalty against a backdrop of private sector discrimination. However, it is possible that race and sex may be accounting for preferences for job security or benefit packages, which might suggest they are valid controls, since they may proxy for in-kind compensation. To make my results comparable to the previous literature, I include controls for race and sex. Race controls do not greatly affect the results, but controls for sex tend to reduce the public sector pay penalty. Some of the analysis below will separate the sample by sex to test for differential selection effects for men and women.

Education is a relatively uncontroversial control, but it is actually quite problematic. Previous work on public sector pay implicitly assumed that a college degree is a college degree—that either skill acquisition is equivalent across college majors or that less remunerative majors or less skilled workers are not disproportionately in government work. However, post-secondary education is a highly differentiated product. In the ACS data 2009-2012 employed here, the highest earning majors (engineering, math, biology, and the physical sciences) earn almost twice as much as the bottom four categories (education, fine arts, family and consumer studies, and theology (see Table 5)). State and local workers are heavily clustered in the lowest-earning majors, while private sector workers are disproportionately in high-earning majors. Equally troubling is that most studies report results pooling public sector workers of different education levels in the same regressions, despite the different arrangements for promotion, hiring, and pay among white collar and blue collar public sector workers.

There is general consensus that there are large differences in returns to college majors, and that these differences evidence both selection and skills acquisition.[4] James et al. (1989), considered the tradeoff between college selectivity and choice of major, and concluded that "... while sending your child to Harvard appears to be a good investment, sending him to your local state university to major in Engineering, to take lots of math, and preferably to attain a high GPA, is an even better private investment (page 252)." Daymont and Andrisani (1984), Grogger and Eide (1995), James et al. (1989), Loury (1997), and Loury and Garman (1995) all confirm in different contexts that there are large differences in earnings across college majors. Turner and Brown (1999) find that ability, as measured by verbal and math entrance exam scores, is a key determinant in choice of college major. Arcidiacano (2004) and Arcidiacono et al. (2012) demonstrate ability sorting as well, but argue that student preferences for career and major as well as expectations about success are key components of major choice because differences across majors remain large even conditional on various ability measures.

Teacher Compensation

Teachers make up about 40% of the state and local workforce with a college degree, but there are unique challenges in considering teacher compensation. First, teachers work fewer weeks a year, so an examination of teacher earnings should in principal be adjusted to reflect this. Interestingly enough, almost two-thirds of teachers in the American Community Survey report working 50 to 52 weeks a year, even though (1) the median number of days teachers are contracted to be in school is 186 (Education Week, April 13, 2005, p. 14) and (2) the School and Staffing Surveys of teachers regarding summer work finds that only one-third of teachers take summer employment (Podgursky 2005).

Using wages as dependent variable will not necessarily eliminate this problem, however. Many teachers are paid nine-month contracts over a twelve-month period, and therefore reporting monthly pay and hours worked in a reference month will greatly reduce the estimated wage. Using contract hours from the National Compensation Survey as a proxy for actual hours dramatically reduces differences in earnings between teachers and other professionals (Podgursky and Tongrut, 2006; Bureau of Labor Statistics, 2012). On the other hand, Allegretto et al. (2004, pg. 35) conclude that teacher contract hours as provided in the

[4] There is a substantial literature on the effects of school quality and undergraduate major on later earnings, but I lack controls for school quality.

National Compensation Survey (which relies on administrative records) understates teacher workweek by about 1.5 hours. Using time diary data to get a better measure of actual hours, West (2013) finds that accounting for teacher hours of work throughout the year actually suggests a wage premium associated with public school teachers, though high school teachers still suffered a pay penalty.

Because of the problems inherent in calculating reliably teacher wage and annual salaries, the results below will both include and exclude teachers. One of the important findings below is that the teacher-private sector wage differentials are responsible for a large portion of the OLS estimate of the public-private wage differential. Most studies of public sector compensation simply include teachers in the estimate, despite the well-known problems in estimating comparability in teacher pay.[5]

2. Estimation of Public-Private Wage Differentials and AFQT

The National Longitudinal Survey of Youth (1979) follows a nationally representative sample of 14 to 22 year olds taken in 1979. There was also an additional supplemental sample of minority and low socio-economic status whites. In 1980, the Armed Services Vocational Aptitude Battery ("ASVAB"), a group of ten tests, was administered to almost the entire NLSY sample. A subset of four of these tests constitute the Armed Forces Qualifications Test, or AFQT. The AFQT is perhaps among the most validated skills test ever devised, and has been used by the U.S. military since 1968. Extensive study has been made of its predictive power with respect to a variety of military jobs, and it has strong correlations with measured job performance. Moreover, the government commissioned a large-scale study through the National Academy of Science (Wigmore and Green 1991) of whether the test is culturally or otherwise biased based on how it predicted performance in objectively graded tasks. The study found no evidence of bias in the AFQT and generally found strong correlations (.13 to .49) for the objective performance measures of the tasks studied.

There is some debate about what precisely the AFQT measures, whether it is innate skill or acquired skill.[6] For the purposes of this paper, it is not necessary to decide precisely what

[5] Gittelman and Pierce (2012) are an exception and make careful note of the problem of teacher compensation.

[6] Herrnstein and Murray (1994) generated a significant amount of controversy by claiming that AFQT provided a measure of inherent ability, linking race and AFQT score. Most scholars reject the notion that AFQT measures innate ability only or primarily. Neal and Johnson (1996) argue that AFQT is a measure of skill that is affected by schooling and family inputs. Other work finds that AFQT appears to be mutable and school quality also explains some of the

AFQT measures. Whether it is innate ability primarily or some combination of innate ability and skill acquired by time of test taking, it is a measure of skill taken prior to entry into the labor market. Therefore it will not be affected by later education, career path, or experience. Moreover, in the NLSY, the AFQT score by itself is as strong a predictor of wages as actual years of schooling. The R-square of a wage regression in 2008 with only AFQT score is .12, compared to .11 for a regression with dummy variables for years schooling completed. Moreover, the AFQT score is strongly correlated with wages even conditional on education. In short, even though the AFQT is a measure of skill taken at ages 14 through 22, it has significant explanatory power on the wages of middle-aged NLSY subjects many years later.

The question for private-public wage differentials then becomes, (1) what is the effect of controlling for AFQT on the private-public wage differential; (2) does the government sector reward skill in the same manner as the private sector; and (3) are government workers differently skilled than private sector workers? To answer these questions, I begin by estimating the following equation:

$$LnWage = a + \beta Government + \mu AFQT + \delta AFQT \times Government + \gamma X + \varepsilon$$

Government equals one if the respondent is in the state or local sector and zero othwerwise; federal employees are controlled for by a separate dummy. Because of the small NLSY sample size, I do not separate out state and local employment. It bears mentioning that as a cohort, the NLSY respondents are between ages 43 and 51 in 2008 and are probably at the peak of their earnings ability. I also will report some results from 2002 and 2000 as a robustness check and to test for time patterns in government employment and wages.[7] *AFQT* is the standardize AFQT score adjusted for age at test taking.[8] The coefficient δ on the interaction between *Government* and *AFQT* reflects the different rate or return to skill, as measured by AFQT, in the government sector.

AFQT gap, and the black-white AFQT gap has declined over time. For a general discussion, see Jencks and Phillips, (eds. 1998).

[7] The public-private sector classification in years 2004 and 2006 are incorrect in the public use NLSY files, and hence those years are unusable. Confirmed by correspondence with Bureau of Labor Statistics.

[8] Following Neal and Johnson's (1996) approach, AFQT is age adjusted because of the different ages at which NLSY respondents took the exam.

The matrix *X* is a set of control variables including age, age-squared, urban-rural, U.S. region (5 regions), employment with the Federal Government, race, sex, marital status, and years of education dummies. Establishment size is not well measured in the NLSY (simply an indicator for large establishment status), and it is far from clear that one should control for it or any of the other factors. To keep my approach comparable to previous work, I include controls for sex, race, and a marital status. Using these variables as controls tends to reduce the estimated public sector pay penalty, but does not qualitatively affect the estimated effect of controlling for AFQT or the interaction between AFQT and government employment.

The top-half of Table 1 presents summary statistics for the NLSY sample by sector and educational status. Figures 1 and 2 are kernel density estimates of the distribution of log wages for high school graduates and those with college degrees or more in both the private and public sectors. The first thing to note is that the wage distributions for public sector and private sector workers who are high school graduates look roughly similar, with the public sector distribution slightly skewed to the right. This is in stark contrast to the wage distributions for those with college degrees or more. For those with college degrees or more, the wage distribution for the public sector is to the left of that of the private sector and relatively compressed. At the means, state and local workers with a high school degree earn on average \$3.34 more per hour more than private sector workers with a high school degree; public sector workers with a college degree or more earn \$5.13 per hour less than private sector workers with a college degree.

Figures 3 and 4 are kernel-density estimates of the distributions for AFQT for high school and college or higher graduates. As with the wage distribution, there is little difference between the public and private sector AFQT distributions for high-school graduates. However, for college grads, the distribution of AFQT for public sector workers is markedly to the left of that for private sector workers. At the means, the AFQT score for public sector workers with college degrees or higher is .25 standard deviations below that of private sector workers. Because of the stark differences between the compensation and skill distributions in public sector by levels of education, separate regressions are run for each level of education. The distributions for those who attended some college are not reported, but those distributions were quite similar to those with high school degrees.

Figures 5 through 8 further describe AFQT and wages nonparametrically by estimating their relationship via Lowess curves (bandwith=.8). For those with high school degrees only, the AFQT Score-wage relationship among the public and private sector are approximately identical and suggest a significant return to AFQT. For those with college degrees, by contrast, the wage curve slopes upward in AFQT for those in the private sector but is remarkably and consistently flat for those in the public sector.

Public Sector Pay and Returns to Skill

The results from the basic specification are presented in Table 2 using data from the NLSY 2008, the most recent year available. The top half of Table 2 reports OLS results, and the bottom half reports results using the same control variables but employing median regressions to account for the potential of outliers. The sample is divided into three levels of education: (1) high school graduates; (2) those with some college, and (3) those with college degrees or more. A specification excluding teachers is also reported. Within each educational group, coefficients from three separate regressions are reported: (1) the public sector pay differential conditional on basic demographic and geographic controls; (2) the public sector pay differential conditional on the same basic controls and on AFQT; and (3) then the addition of AFQT score and its interaction with government work, which allows for different returns to skill in each sector.

The pattern evident in the kernel-density estimates is repeated in the parametric results. First, the public-private wage differential varies greatly depending on completed levels of education. For those who only completed high school, there is a public sector pay *premium* of 8.61%, though it is fairly imprecisely estimated. For those with college degrees or higher, the public sector pay *penalty* is almost 16%, but when teachers are excluded from the sample, this differential declines to 9.8% and is no longer statistically significant. For those with some college, the estimates are imprecise but are similar to those with high school degrees.

The second column within each education group includes controls for standardized, age-adjusted AFQT score. Thus, the coefficient on *AFQT* should be interpreted as the percent increase in log wages that results from a one-standard deviation increase in age-adjusted AFQT score. The third column reports results from a regression that interacts *AFQT* with *Government*. The coefficient on this interaction is the difference in return to AFQT score earned by government workers.

In general, there is a large return to AFQT score across education groups, though it is nearly twice as high for those without college degrees.[9] The addition of AFQT as a control slightly reduces the public sector pay penalty in the OLS regressions, by about 1.3 percentage points for those with high school diplomas and by about 2.1 percentage points for those with college degrees. These differences were not statistically significant.

When public sector employment is interacted with AFQT score, however, a different pattern emerges across the education groups. For those with less than college degrees, the coefficient on *Government x AFQT* is close to zero and the coefficient on *AFQT* is little changed. Though not precisely estimated, the results imply that there is no measureable difference in returns to skill associated with government employment for those without college degrees. Not only is there no measureable difference in the return to skill, but we can fairly precisely estimate a return to AFQT score that is nearly the same in both the private and public sectors. For example, the estimated return to AFQT score for those with high school degrees is 17.9% in the public sector and 19.3% in the private sector, and both coefficients have p-values less than .01.

By contrast, for those with college degrees or more, the coefficient on *AFQT* is more than fully offset by the coefficient on *Government x AFQT*. In other words, the point estimate is that there is no return, or even a slightly negative return, to AFQT score in government work. The p-value of the coefficient on *Government x AFQT* is less than 0.01, providing strong evidence that the return to AFQT score in the public sector is substantially less than that in the private sector. It bears mentioning that the standard errors allow for the possibility of a positive return to skill in government work: the 95% confidence interval for return to AFQT score among government workers is [-.148, .0692].

To test the sensitivity of the results to outliers and other effects not evident in OLS results because of skewed distributions, the results of median regressions are reported as well. Very little is changed between the OLS and median outcomes. Other quantiles were explored in unreported regressions, which suggested that the public sector wage penalty is larger at higher deciles of the distribution. However, it was clear that most of the relevant quantile affects are largely accounted for simply by dividing the sample into high school and college graduates.

[9] Arcidiacano et al. (2010) find that returns to AFQT increase for high school graduates as they gain more experience, while the same is not true for college graduates. At this point in the NLSY, high school graduates have significant experience. Because they do not report AFQT results without experience interactions, I cannot directly compare my estimates to theirs.

The NLSY raises some unique concerns that are addressed in Table 3. First, the NLSY is a cohort, so it is important to consider whether the results here are similar at earlier points when the sample was younger. Second, the NLSY 1979 panel, though representative at the time it was initiated, has had significant attrition. By 2002, attrition in the NLSY stood at about 20% (NLSY Handbook 2005), and by 2008 it was roughly 24% (author's calculation). Generally, attrition has been higher for those with lower levels of education and those who are less attached to the labor force (for a discussion, see MacCurdy et al. 1998), which means that attrition is less a concern with those present labor force sample. Third, the NLSY 1979 had a supplemental sample associated with it that oversampled low-socioeconomic status individuals and minorities. Although many researchers using the NLSY for a nationally representative study leave out the supplemental sample, it could also be used to gain precision by employing the sampling weights to ensure a representative estimate. The NLSY produces sampling weights that take account of both attrition and the oversampling of certain groups in the supplemental sample.

To test whether these issues make a significant difference to the interpretation of our main result of interest, Table 3 reports results from the 2000, 2002, and 2008 NLSY surveys for the regression specification that interacts *AFQT* and *Government*. For each year, the first column reports unweighted results, the second column reports results using cross-sectional weights that account for attrition, and the third column reports results that include the supplemental sample using sample weights. The years 2004 and 2006 are not used because of anomalies in the coding of the government employment variables in those years.[10] The results of Table 3 are quite similar to those of Table 2, and change little year to year.

Table 4 reports linear probability models of likelihood of public sector employment for 2000, 2002, and 2008. For those with college degrees, a higher AFQT score significantly reduces the probability of public sector employment. A one standard deviation increase in AFQT score reduces the probability of government employment by 5 to 7 percentage points depending on the year, and these estimates are fairly precise. Given that government workers constitute about 25 percent of those with college degrees or more, the point estimates suggest a large reduction in employment probabilities as one moves up the AFQT distribution. The results shrink a bit when educators are removed from the sample but remain statistically significant, with coefficients for this subsample implying that a one-standard deviation increase in AFQT score

[10] Confirmed in correspondence with the Bureau of Labor Statistics.

reduces the probability of public sector work by 4 to 5 percentage points. The last two columns split the sample by sex, and the results for both men and women are largely the same. By contrast, for those with high school diplomas or some college, there is no evidence of negative selection into the public sector. For these groups, the coefficient on AFQT is positive and the standard errors fairly small.

Discussion

The results as a whole tell a consistent story based on sorting by skill. For those with college degrees, there is no measurable return to AFQT score in public employment, and, not surprisingly, college graduates with higher AFQT scores are significantly less likely to be in public employment. For those with less than college degrees, the returns to AFQT for public and private sector workers are similar and, not surprisingly, AFQT and public sector employment are uncorrelated. But this raises the question of why are there returns to AFQT for public-sector high school graduates and those with some college, but substantially lower (and possibly no) returns for college graduates in the public sector?

The answer I suspect is involves institutional arrangements within the civil service. In public-sector jobs that often require only a high school education, skills tests are key determinants of entry and promotion--this is particularly the case for public safety workers. The rules vary by jurisdiction, but when testing is used to determine employment and promotion, managers typically must select from among the top several scorers, and in some cases they may even have to offer the job or promotion to the top-scorer (Kearney 2009). Sometimes these tests involve a heavy written component comparable to the AFQT, and in other cases they are combined with softer assessments of leadership skills and oral exams (for a survey see Lowry 1996). Some jobs taken by high school graduates require independent credentialing and testing in both the public and private sectors; for example, bus drivers, electricians, emergency medical technicians, and the like. AFQT score should be predictive of performance on such tests since the AFQT is a test of general skills. This is not confirmation that the public sector effectively employs promotion or entry exams, the use of which has been the subject of some controversy.

By contrast, college educated public sector workers are less likely to face skills-based promotion tests, and hiring and promotion decisions are more likely to be based on seniority and credentialing such as master's degrees. Teacher promotion and salary determinations are perhaps emblematic of this phenomenon. While teachers must pass licensing examinations, there is no requirement that those with the highest scores be hired, and wages are linked overwhelmingly to seniority and attaining master's degrees and additional training (Ballou and Podgursky, 1995; Eide et al. 2004). Moreover, there is little evidence that public school administrators choose to hire on the basis of academic performance (Ballou 1996).

3. Public-Private Wage Differentials in the ACS

The American Community Survey, which replaced the U.S. Census long form, is a survey administered each month to a sample of U.S. residents. As in the Current Population Survey, smaller states tend to be oversampled, and consequently sampling weights are used to create a nationally representative sample. Earnings, labor force participation, age, race, sex, occupational category, and education level are collected from almost three million individual respondents each year. In addition to collecting information on sector of employment (private, self-employed, state, local, or federal) the survey asks respondents in the private sector whether they work for a for-profit or not-for-profit business. Beginning in 2009, the ACS also began asking those who had completed a college education or more what their primary undergraduate major was according to thirty-eight categories.

The sample size and unique characteristics of the ACS allow us to explore the public pay penalty in a number of new ways. First, by controlling for choice of college major, we introduce some controls that capture both unobserved skill of workers as well as the outside options of public sector workers. Unlike controls for occupational categories, which are subject to criticism of public-sector dominance in fields such as education or law enforcement, there is substantial common support across the public and private sector for college majors. Even college majors for which public sector work is the main career option, such as education and criminal justice, have a large number of workers engaged in the private sector. The large ACS sample size allows us to separately consider each level of educational attainment by itself: high school graduate, some college, college graduate, master's degree, professional degree, and doctoral degree. As before, stark differences are observed across education levels, and the large sample size permits us to report precise results for each educational level degree. In addition, the sample size allows us to examine men and women separately.

Second, the large sample size allows comparisons between public sector employees and those in the non-profit sector. If selection into non-profit and government work are driven by similar factors, then the wage differential between non-profit and government work will be more informative.

Third, the ACS sample size allows us to compare differences in the public-private differential across states. Some states prohibit state and local governments from collectively bargaining with public employee unions, while others have high union contract coverage rates. I examine whether patterns of public sector wages map onto state receptiveness to public sector unions.

There are some drawbacks to the ACS. First, wages cannot be reliably computed because the reference period for earned income is one year and only "usual hours worked in a week" is asked. Each regression therefore takes log earnings (wage and salary income) in the past twelve months as the dependent variable, employing usual hours worked as a control. Because the ACS topcodes earnings at the 99.5 percentile of a state's earnings distributions, I also run median regressions to see if topcoding or outliers are affecting the estimation. Second, weeks of work are reported only as six categorical variables, but since earnings is the dependent variable, it is important to limit the sample to those who worked full time during the one-year reference period. Thus, to ensure that we are capturing regularly employed workers, I only include those who report working 50 to 52 weeks in the past 12 months, which is 80% of the sample in the labor force. One-third of those in the teaching profession reported working around 40 weeks per year, but the inclusion or exclusion of this group did not appreciably affect results. Almost all the remaining teachers report working 50 to 52 weeks a year. As before, most of the results will be reported with and without teachers. To account for hours worked in a week, the regressions include a cubic for hours "usually" worked in week.

The key additional control available in the ACS is undergraduate college major. As discussed above, college major is an important determinant of earnings both because of heterogeneous returns to college major and because of selection on ability. For the purposes of testing the sensitivity of the public-private sector wage differential, what portion of the return to college major reflects selection versus the value of the skills acquired is not important. The key is that different majors are paid differently, reflecting some combination of innate ability, acquired skill, and labor demand conditions.

The basic regression is as follows:

$$LnEarnings = \alpha + \beta State + \mu Local + \lambda CollegeMajor + \gamma X + \varepsilon$$

Where *LnEarnings* are total labor income in the past year, *State* equals one if the worker reports employment in the state sector, and *Local* equals one if the worker reports employment in the local sector. The matrix *CollegeMajor* contains 38 dummy variables for college major.[11] Results will be reported by degree category (high school, college, master's, professional, and doctoral). The matrix *X* contains age, age-squared, race, sex, marital status, state dummies, urban-rural dummies, and a cubic in usual hours worked in a week. Some specifications will also report results controlling for two-digit occupation codes.

Public-Private Differentials and College Major

Table 5 details the share of college majors within each sector and the average earnings of those college majors in the data. As can be seen, popular top-earning majors such as engineering and business represent 2.5 to 3 times greater shares in the private sector than the public sector. By contrast, popular but low-earning majors such as education and criminal justice have much greater shares of employment in the public sector. However, all majors have substantial common support in both sectors.

Table 6 reports earnings regressions for those with high school degrees and some college, with and without occupational controls (entered as the twenty-five two-digit census occupation codes). The dependent variable is log earnings. As in the NLSY, for those with terminal high school degrees there is a premium in favor of public sector workers of between 4 and 7 percent, depending on whether we control for occupational categories. For those with some college, there is no difference between the public and private sectors unless we include occupational controls, in which case there is public sector premium of 4.5%. The rest of the analysis will focus on those with college degrees or higher.

Table 7a reports results by terminal degree: college degree, master's degree, professional degree, and doctoral degree, and Table 7b repeats the same specifications using median regressions. For those with college and master's degrees, results excluding teachers are also

[11] I use this variable instead of the 171-category detailed college degree major because of concerns about reducing common support across sectors. Using the detailed codes tended to further shrink the public sector pay penalty, but not by a large degree.

reported. The estimated public sector pay penalty for those with college degrees is -10.7% for state workers and -12.4% for local workers. This is substantially smaller than the NLSY results. However, when the regressions were rerun to mimic the NLSY regressions (by collapsing the state and local categories, conditioning on region instead of state, and limiting the age of subjects to be between 43 and 51), the public sector pay penalty was -17.1% in the ACS versus -15.9% in the NLSY. In short, the earnings measure in the ACS and the wage measure in the NLSY produce very similar pay penalties.

Including controls for thirty-eight college major categories reduces both state and local pay penalties by a significant degree: by 40% for state workers (from -10.7% to -6.28%) and by almost 60% for local workers (from -12.4% to -5.04%). Including occupational controls further reduces the differential for state workers and completely eliminates it for local workers, for whom there is a fairly precisely estimated zero pay differential. For those with advanced degrees, the same results for local workers hold as for those with college degrees. Including controls for college major reduces the public sector pay penalty by over half in the case of master's degrees and doctoral degrees, and by nearly one-third in the case of professional degrees. By contrast, the pay differential for state workers with advanced degrees is not greatly affected by controls for college degree, although it is greatly reduced by the inclusion of occupational controls.

When teachers are excluded in the bottom half of the table, the estimated public sector pay penalty is one-third to one-half smaller, but the effect of college degree has roughly the same proportionate effect on the pay penalty as when teachers were included. These basic results are little changed when median regressions are used in Table 7b.

Table 8 reports separate regressions for men and women. As before, results including and excluding those in the teaching profession are reported. The prior results hold generally for the divided sample, though the effect of controlling for college major is greater for females. Depending on the specification, for local workers the inclusion of college major controls explains roughly 40% of the public sector pay penalty for male employees and between one-half and entirety of the pay penalty for females. As before, college major explains less for state workers, but still reduces the pay penalty.

Public-Non Profit Wage Differentials

Table 9 compares public sector and non-profit private sector workers by degree completion, breaking out those who are in the teaching profession separately. All other workers are excluded from the sample. In general, the public sector enjoys a premium in these comparisons, especially those with high school diplomas. For local employees with college degrees, there is a *premium* associated with public sector work relative to non-profit work when we control for degree. For local employees holding master's degrees, the premium is fairly constant and large (7% or more) regardless of controls. State workers are little different from non-profit workers, with a fairly precisely estimated null effect when we control for degree. Those with professional degrees enjoy a public sector premium, but not once one controls for occupational category. In stark contrast with the prior results, including controls for college major does not dramatically change the estimated pay differential, though in some cases it increases the public sector pay premium.

For teachers, the results are pretty dramatic: the differential is between 14% and 16% in favor of public sector teachers whether state or local employees or college graduates or master's degree holders. This public-private teaching differential is not a new result (for recent discussions see Podgursky 2011 and West 2013), so I do not dwell on it.

Wage Differentials by State and Region

Table 10 breaks out the results by groups of states: those with strong public sector unions, unions of intermediate strength, and those that prohibit state and local governments from collectively bargaining with public employee's unions. There is admittedly some discretion in choosing which states are "strong" and "intermediate" public sector unions. I define states where more than 50% of public sector workers are unionized, that are not right-to-work states, and that mandate collective bargaining for teachers and public safety officers as "strong union" states. The major states in this category are California, Illinois, Michigan, New York, Ohio, and Pennsylvania. Smaller states are: Connecticut, Maryland, Massachusetts, Minnesota, New Jersey, and Rhode Island. States that prohibit collective bargaining are North Carolina, Georgia, South Carolina, Texas, and Virginia. Other states either have permissive rules for localities or low union concentration.

For those with high school diplomas, there is a premium for public sector work in strong union states, no difference in the intermediate states, and there is a pay penalty in the no bargaining states. For those with college degrees and master's degrees, the public sector pay penalty is largest in states that prohibit bargaining and smallest in states that mandate bargaining. As before, when controls for college major are added, the magnitude of the pay penalty declines significantly and is eliminated in the strong union states.

Figures 5 and 6 provide point estimates and confidence intervals for local and state employees with college and master's degrees from regressions on each individual state. The regressions include the usual controls as well as college major controls. The states that prohibit collective bargaining at the local level are clustered on the bottom, and the states at the top are generally strong union states. There are some notable exceptions. For example, Connecticut and Massachusetts, despite strong public sector unions by coverage rates, are roughly in the middle of the pack.

In sum, there are significant differences across states in the pay differentials between the public and private sector, and there are significant differences between states based on the strength of the public sector unions. The approach is not casual as we do not measure changes to union strength caused by exogenous shocks. Nonetheless, the results establish that the public sector pay penalty varies a great deal by state, and consequently state level factors are important elements in the public sector pay penalty.

4. Conclusion

Using both the National Longitudinal Survey of Youth (NLSY) and the American Community Survey (ACS), this paper explored the senstivity of the estimated public-private sector pay penalty by focusing on two data sets that permitted the inclusion of relevant controls for skill that have not previously been employed.

The evidence from the NLSY relies on the AFQT score, a test widely validated as a good measure of skill. The NLSY results strongly suggested that, conditional on educational attainment, college-educated public sector workers are significantly less skilled and are also less compensated for skill than their private sector counterparts. The AFQT score by itself does not explain much of the public sector pay penalty; lower returns to skill in the public sector appear to be much more important. Given lower returns to skill, it is not surprising that less-skilled workers select into government employment. For those with only high school diplomas, there were no measured differences between public sector and private sector returns to skill. This outcome is consistent with civil service regulations that rely heavily on test taking and independent credentialing in the hiring and promotion of blue-collar government workers.

The NLSY results raise important questions of public policy. If there is social value in attracting more skilled college-educated workers to the public sector, then attention must be focused on creating returns to skill for public sector workers. To do so, decision makers in the public sector need to have the incentives and ability to select and retain better workers. This will be particularily challenging in an institutional context that compresses public sector wages—a result of both civil-service statutes regulating wages and promotion and high rates of union coverage that further compress wages.

The AFQT results imply that college-educated public employees with high skills are significantly undercompensated relative to their probable earnings in the private sector. However, the public-private pay differential is also sensitive to controls for college degree, which is a rough measure of skill acquired after high school. For local workers, up to two-thirds of the pay differential is explained by simple controls for college major. Similar dramatic reductions in the pay penalty are observed even for those with advanced degrees, for whom one may have thought college major would provide less information. The differential for state workers is somewhat less sensitive to college major controls, but still generally declines when controls for college major are included.

The non-profit results are likewise revealing. If one thinks that the non-profit and government sectors have much in common in terms of selection or amenities, then the non-profit sector is a better comparison group than the private sector writ large. This view is partially supported by the fact that college major controls make very little difference (or slightly enhance) the premium in favor of public sector workers relative to non-profit workers.

Moreover, the ACS data reveals dramatic regional variation in the public-private sector differential, suggesting that institutional arrangements are key in public sector wage determinations. While the evidence is not quasi-experimental, the results suggest institutional arrangements (either union power or something else correlated with union power) are factors in the public-private sector wage differential.

The results for the most part still find a public sector pay penalty for those with college degrees or more. A word of caution, however, is merited in interpreting the remaining disparity as a true and accurate measure of the difference in compensation between public sector and private sector work. First, there is a large benefits premium for public sector work, which has been estimated to be as much as 10 percent of total compensation. Second, there are additional non-wage premia for public sector employment in hard to quantify amenities such as job security, early retirement, and health care for retirees, which is not factored into the estimates of the benefits premium. Third, controls for college major or test scores during high school are fairly crude proxies for skill. More accurate measures of skill and aptitudes, such as schooling quality or type of advanced degree, would likely further reduce the public sector pay penalty.

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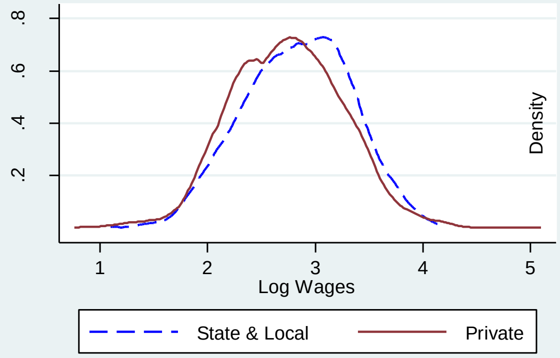


Figure 1: Log Wages, High School Graduate/Some College

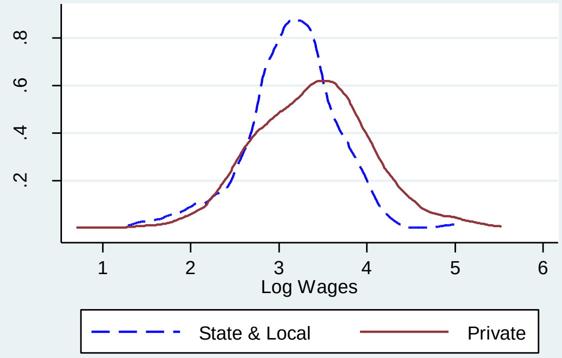


Figure 2: Log Wages, 4 Years of College or More

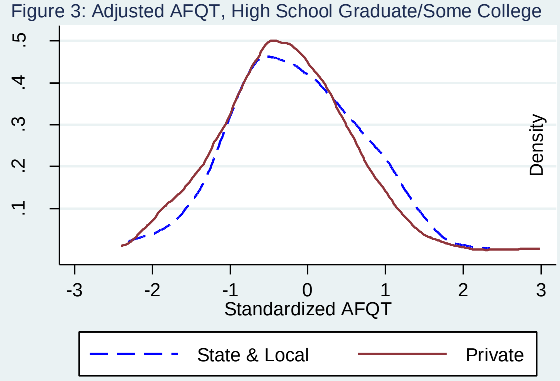


Figure 3: Adjusted AFQT, High School Graduate/Some College

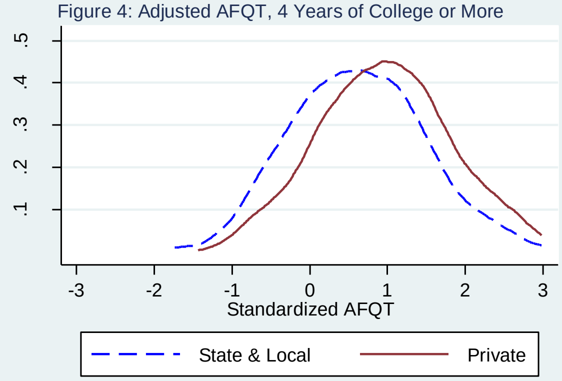


Figure 4: Adjusted AFQT, 4 Years of College or More

Figure 5: Private Sector, Lesst than College

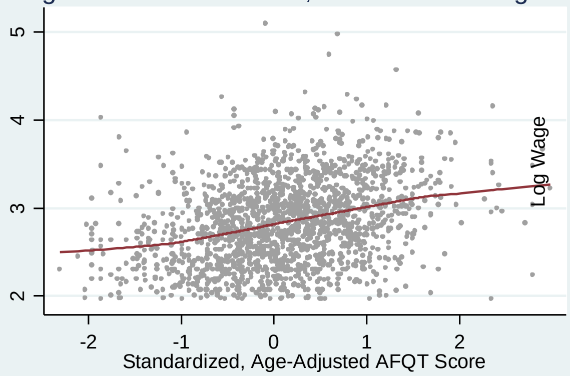


Figure 6: State & Local, Less than College

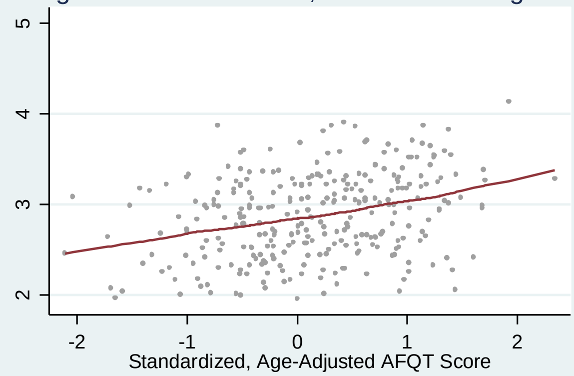


Figure 7: Private Sector, College or More

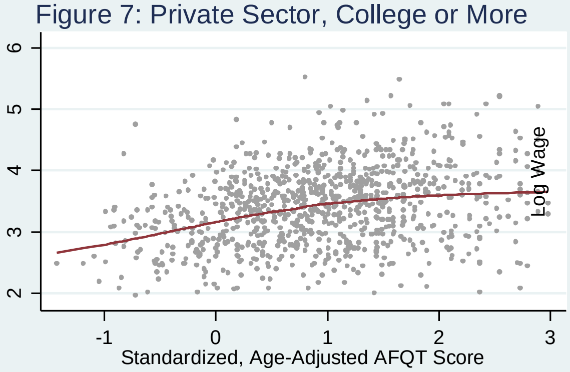
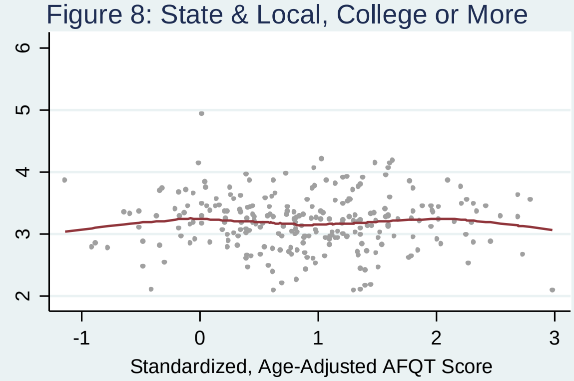


Figure 8: State & Local, College or More



Lowess curves, those earning minimum wage or above only

bandwidth = .8

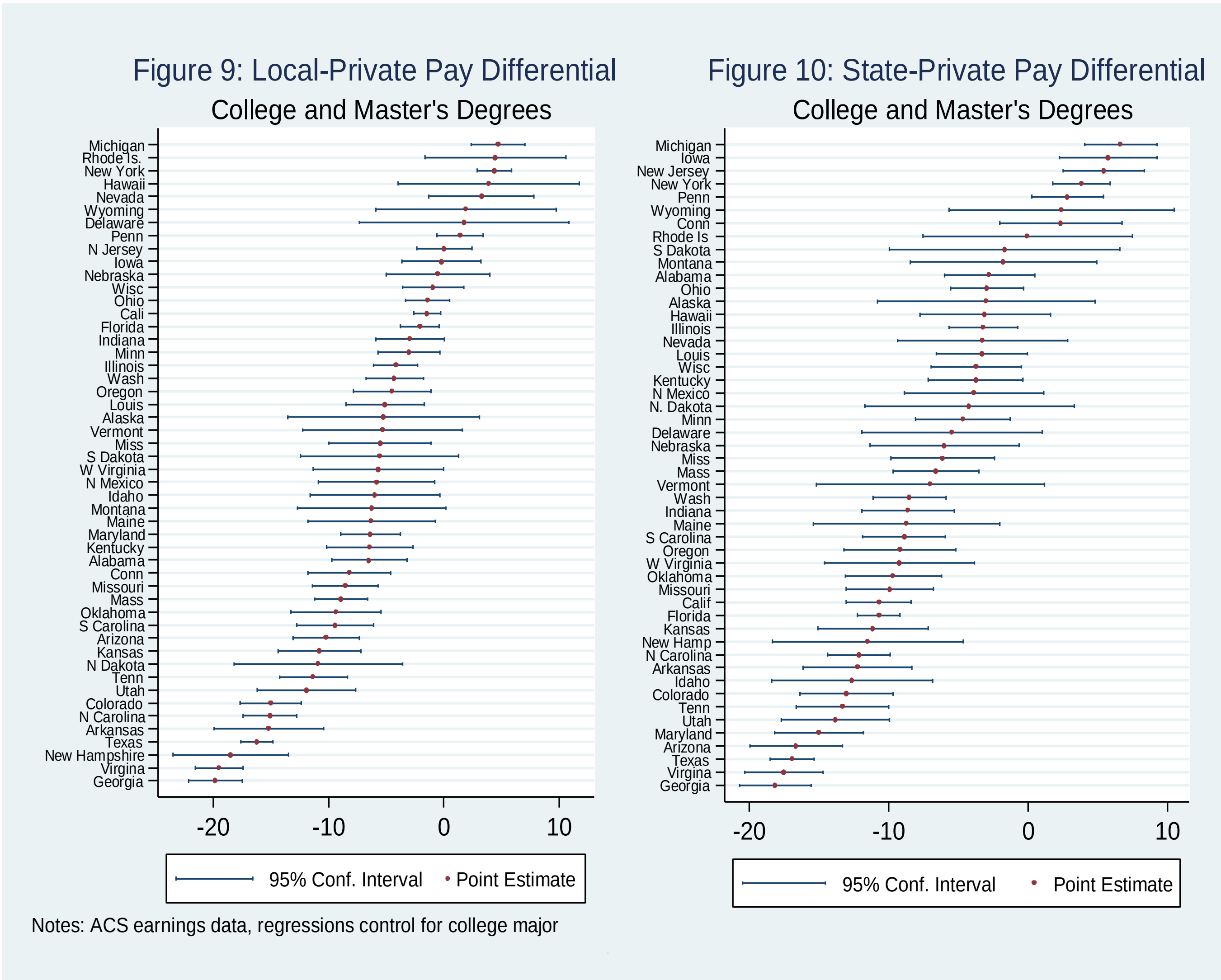


Figure 9: Local-Private Pay Differential
College and Master's Degrees

Figure 10: State-Private Pay Differential
College and Master's Degrees

Notes: ACS earnings data, regressions control for college major

Table 1: Summary Statistics

	High School Graduate		College Degree or More	
	Public Sector	Private Sector	Public Sector	Private Sector
NLSY				
Wage (hourly)	18.14	14.58	27.49	32.62
	(9.60)	(13.79)	(49.14)	(33.32)
AFQT (standardized, age adjusted)	-0.37	-0.31	0.63	0.88
	(0.79)	(0.79)	(0.77)	(0.76)
Age	46.5	46.6	46.5	46.7
	(2.22)	(2.19)	(2.23)	(2.23)
Urban	54.2%	61.2%	73.8%	70.1%
Union Coverage	50.3%	11.4%	57.2%	7.5%
Teaching Occupation	-	-	41%	4.1%
Sample size	179	1,375	267	815
ACS				
Earnings	36,009	36,960	58,133	81,328
	(27,200)	(22,205)	(34,543)	(72,917)
Age	47.5	45.3	45.0	43.2
	(9.8)	(10.8)	(11.0)	(10.9)
Urban	61.8%	67.7%	74.3%	85.0%
Teaching Occupation	-	-	37.8%	3.6%
Sample Size	95,239	740,428	203,730	743,510

"Public sector" is defined as state and local employees. *Urban* is defined as located in urban area, excluded are rural and unknown categories. *Union Coverage* is both membership and contract coverage. *Teaching Occupation* is defined as the following occupational categories: preschool and kindergarten teachers, elementary and middle school teachers, secondary school teachers, special education teachers, and "other" teachers—post-secondary teachers are not included.

Table 2: Public-Private Wage Differentials and AFQT

	High School Graduate			Some College			College Graduate or Higher			College Graduate or Higher (no Teachers)		
<u>OLS Regressions</u>												
Government Employee	0.0861*	0.0730+	0.0726+	-0.0517	-0.0575	-0.0430	-0.159*	-0.138*	0.0383	-0.0983	-0.0811	0.0579
	(0.0432)	(0.0420)	(0.0421)	(0.0661)	(0.0658)	(0.0732)	(0.0618)	(0.0624)	(0.102)	(0.0615)	(0.0610)	(0.0908)
Standardized AFQT		0.190**	0.191**		0.174**	0.178**		0.0975**	0.141**		0.0886*	0.114*
		(0.0211)	(0.0222)		(0.0365)	(0.0394)		(0.0377)	(0.0447)		(0.0387)	(0.0432)
Government Employee x Standardized AFQT			-0.0137			-0.0363			-0.178**			-0.136*
			(0.0505)			(0.0876)			(0.0685)			(0.0640)
Return to AFQT for Gov't Employees			*0.179***			*0.141+*			*-0.0395*			*-0.0221*
			(0.0481)			*(0.0803)*			*(0.0554)*			*(0.0556)*
<u>Median Regressions</u>												
Government Employee	0.0773	0.0792	0.0645	-0.00388	0.0399	0.0683	-0.166**	-0.161**	-0.0439	-0.0843	-0.112+	-0.0050
	(0.0489)	(0.0491)	(0.0493)	(0.0647)	(0.0637)	(0.0721)	(0.0535)	(0.0529)	(0.0854)	(0.0622)	(0.0630)	(0.0985)
Standardized AFQT		0.205**	0.205**		0.151**	0.161**		0.0751*	0.121**		0.0827*	0.108**
		(0.0228)	(0.0241)		(0.0336)	(0.0352)		(0.0308)	(0.0353)		(0.0327)	(0.0355)
Government Employee x Standardized AFQT			-0.0417			-0.0355			-0.115+			-0.127+
			(0.0617)			(0.0844)			(0.0670)			(0.0765)
Return to AFQT for Gov't Employees			*0.166***			*0.125*			*-0.0006*			*-0.0137*
			(0.0584)			*(0.0810)*			*(0.0596)*			*(0.0715)*
Sample size		1,550			923			1,066			921	

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Dependent variable is log wage of primary job. Sample is NLSY 2008. *Government Employee* is one when respondent works for the state or local government sector, zero otherwise; federal employees are controlled for as a separate category. Each regression controls for age, age-squared, urban-rural, U.S. region (5 regions), race, sex, and marital status and, for those with some college or more, years of education. Standard errors are clustered by family ID.

Table 3: Public-Private Wage Differentials and AFQT by Sample Year and Weighting Method

	2000			2002			2008		
	No Weights	Cross-Section Weights	Sample Weights	No Weights	Cross-Section Weights	Sample Weights	No Weights	Cross-Section Weights	Sample Weights
College Graduates or More									
Government Employee	-0.0096	0.00957	-0.00266	-0.0154	0.0137	-0.0129	0.0383	0.0231	-0.0222
	(0.0606)	(0.0631)	(0.0535)	(0.0708)	(0.0720)	(0.0607)	(0.102)	(0.117)	(0.0792)
Standardized AFQT	0.159**	0.161**	0.161**	0.141**	0.146**	0.140**	0.141**	0.154**	0.152**
	(0.0376)	(0.0379)	(0.0345)	(0.0351)	(0.0356)	(0.0318)	(0.0447)	(0.0476)	(0.0437)
Government Employee x Standardized AFQT	-0.124**	-0.147**	-0.130**	-0.137**	-0.173**	-0.135**	-0.178**	-0.159**	-0.129*
	(0.0488)	(0.0497)	(0.0434)	(0.0517)	(0.0537)	(0.0448)	(0.0685)	(0.0732)	(0.0549)
Sample size	1,075	1,024	1,425	1,042	992	1,382	1,066	1,013	1,449
High School Diploma									
Government Employee	-0.00499	-0.0113	-0.0141	0.0692	0.0535	0.0904**	0.0726+	0.0843+	0.0929*
	(0.0453)	(0.0446)	(0.0403)	(0.0474)	(0.0477)	(0.0419)	(0.0421)	(0.0431)	(0.0361)
Standardized AFQT	0.190**	0.199**	0.186**	0.169**	0.164**	0.154**	0.191**	0.185**	0.194**
	(0.0229)	(0.0243)	(0.0217)	(0.0269)	(0.0287)	(0.0242)	(0.0222)	(0.0229)	(0.0202)
Government Employee x Standardized AFQT	-0.0391	-0.0573	-0.0666	0.00107	0.0252	-0.0619	-0.0137	-0.0165	-0.0831+
	(0.0743)	(0.0696)	(0.0654)	(0.0668)	(0.0663)	(0.0541)	(0.0505)	(0.0516)	(0.0433)
Sample size	1,792	1,680	2,852	1,673	1,579	2,702	1,550	1,458	2,517

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each column reports coefficients from two regressions, one those with college educations or more and one on those with a high school diploma only. Dependent variable is log wage of primary job. *Government Employee* is one when respondent works for the state or local government sector, zero otherwise; federal employees are controlled for as a separate category. Each regression controls for age, age-squared, urban-rural, U.S. region (5 regions), race, sex, and marital status and, for those with some college or more, years of education. Standard errors are clustered by family ID. Unweighted and cross-sectional weighted regressions use only the main sample and reflect the probability of attrition in the NLSY. Supplemental weighted regressions use the non-military supplemental sample. Some cross-sectional weights were missing, yielding slightly smaller sample size than in the unweighted results. Supplemental weights are designed to weight the main and supplement samples of the NLSY so as to be reflective of the national population at the time of the NLSY's original interviews (1979) as well as reflect attrition.

Table 4: Probability of Public Sector Employment and AFQT

	High School Graduate	Some College	College Graduate or more	College Graduate or more (no teachers)	College Graduate or more (men)	College Graduate or more (women)
2008 Sample						
Standardized AFQT	0.0147	0.0089	-0.0752**	-0.052**	-0.0715**	-0.0800**
	(0.0123)	(0.017)	(0.0183)	(0.0178)	(0.0242)	(0.0268)
Sample size	*1,550*	*913*	*1,066*	*921*	*518*	*548*
2002 Sample						
Standardized AFQT	0.0164	0.013	-0.0583**	-0.041*	-0.0663**	-0.0385
	(0.0112)	(0.017)	(0.0176)	(0.0167)	(0.0228)	(0.0268)
Sample size	*1,673*	*923*	*1,073*	*1,073*	*517*	*525*
2000 Sample						
Standardized AFQT	0.0210*	0.0027	-0.0702**	-0.046**	-0.0650**	-0.0731**
	(0.0104)	(0.017)	(0.0161)	(0.0164)	(0.0236)	(0.0226)
Sample size	*1,792*	*961*	*1,075*	*1,075*	*533*	*542*

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Regressions are linear probability models, each reported coefficient is from a separate regression. *Public Sector Employment* is one when respondent works for the state or local government sector, zero otherwise. Each regression controls for age, age-squared, urban-rural, U.S. region (5 regions), race, sex, and marital status and, for those with some college or more, years of education. Standard errors are clustered by family ID.

Table 5: Degree Share in Public and Private Sectors and Annual Earnings (ACS)

Degree type	Share of State & Local Sector	Share of Private Sector	Average Earnings
Engineering	3.08	9.38	95,124
Physical Sciences	2.66	3.34	86,904
Biology	4.07	4.75	86,632
Math	1.56	1.45	84,870
Social Sciences	7.24	7.33	79,837
Computer	1.29	3.74	78,479
Business	10.20	24.02	74,818
History	2.48	2.00	73,985
Technical	0.36	0.97	72,810
Architecture	0.42	0.79	67,326
Medical	4.81	8.29	66,521
Philosophy	0.56	0.75	65,275
Agriculture	0.97	1.13	61,966
Other	0.86	1.29	61,406
Environmental Studies	0.85	0.56	60,905
English	3.74	2.92	60,493
Communications	2.54	4.26	59,290
Linguist	1.27	0.93	59,059
Interdisciplinary Studies	0.64	0.57	58,108
Psychology	5.62	4.42	57,313
Liberal Arts	1.79	1.35	57,218
Criminal Justice	3.17	1.06	56,919
Physical Fitness	0.95	0.78	50,260
Public Administration	2.09	1.18	48,306
Fine Art	2.74	3.92	48,016
Education	32.61	7.30	47,653
Family/Consumer Studies	1.20	0.72	44,062
Theology	0.32	0.78	41,980

Table 6: Public-Private Earnings Differentials (ACS), those with High School or Some College

	High School Degree		Some College	
State Employee	0.0447**	0.0540**	0.00699	0.0151
	(0.0188)	(0.0159)	(0.0182)	(0.0145)
Local Employee	0.0429**	0.0643**	0.0216	0.0452*
	(0.0194)	(0.0170)	(0.0231)	(0.0186)
Occupation Controls		x		x
Sample size	835,667		662,522	

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each regression controls for age, age-squared, urban-rural, a cubic in usual hours worked, sex, race, marital status, and state dummy variables. Occupation controls are for twenty-five different census occupational categories. Sample is from 2009-2012 and includes those aged 25 to 65 who are not self-employed and report working 50 to 52 weeks in the past year.

Table 7a: Public-Private Earnings Differentials (ACS)

	College Degree			Master's Degree			Professional Degree			Doctoral Degree		
Teachers Included												
State Employee	-0.107**	-0.0628**	-0.0315**	-0.159**	-0.106**	-0.0561**	-0.224**	-0.190**	-0.191**	-0.154**	-0.138**	-0.0673**
	(0.0157)	(0.0125)	(0.00979)	(0.0148)	(0.0137)	(0.0123)	(0.0137)	(0.0123)	(0.0157)	(0.0178)	(0.0159)	(0.0135)
Local Employee	-0.124**	-0.0504**	0.0177	-0.136**	-0.0596**	0.0137	-0.339**	-0.223**	-0.222**	-0.143**	-0.0855**	-0.0967**
	(0.0204)	(0.0164)	(0.0134)	(0.0192)	(0.0171)	(0.0151)	(0.0200)	(0.0250)	(0.0190)	(0.0190)	(0.0164)	(0.0156)
Sample Size		589,860			255,751			59,959			41,670	
Teachers Excluded												
State Employee	-0.0902**	-0.0667**	-0.0571**	-0.161**	-0.120**	-0.0857**						
	(0.0144)	(0.0133)	(0.0128)	(0.0137)	(0.0128)	(0.0131)						
Local Employee	-0.0680**	-0.0363**	-0.00991	-0.0893**	-0.0297*	-0.0174						
	(0.0191)	(0.0167)	(0.0163)	(0.0155)	(0.0132)	(0.0139)						
Sample Size		540,171			205,853							
College Major Controls		X	x		x	x		x	x		x	x
Occupation Controls			x			x			x			x

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each column is a separate regression with controls for age, age-squared, urban-rural, a cubic in usual hours worked, sex, race, marital status, and state dummy variables. For those with college and master's degrees, each column reports two regressions, one including those in the teaching profession and one excluding those in the teaching profession. Occupation controls are for twenty-five different census occupational categories. College major controls are for thirty-eight different college majors. Sample is from 2009-2012 and includes those aged 25 to 65 who are not self-employed and report working 50 to 52 weeks in the past year.

Table 7b: Public-Private Earnings Differentials (ACS) Median Regressions

	College Degree			Master's Degree			Professional Degree			Doctoral Degree		
Teachers Included												
State Employee	-0.118**	-0.0593**	-0.0609**	-0.175**	-0.112**	-0.0647**	-0.248**	-0.213**	-0.208**	-0.169**	-0.140**	-0.0616**
	(0.00367)	(0.00345)	(0.00402)	(0.00384)	(0.00378)	(0.00332)	(0.0113)	(0.0100)	(0.00851)	(0.00798)	(0.00602)	(0.00662)
										-0.149**	-0.0890**	-0.0941**
Local Employee	-0.126**	-0.0351**	-0.00975*	-0.150**	-0.0683**	0.000843	-0.334**	-0.247**	-0.212**	(0.0137)	(0.0104)	(0.0107)
	(0.00305)	(0.00296)	(0.00400)	(0.00306)	(0.00318)	(0.00295)	(0.0116)	(0.0106)	(0.00917)	-0.169**	-0.140**	-0.0616**
Sample Size		589,860			255,751			59,959			41,670	
Teachers Excluded												
State Employee	-0.0987**	-0.0664**	-0.0357**	-0.181**	-0.129**	-0.0986**						
	(0.00427)	(0.00419)	(0.00322)	(0.00455)	(0.00454)	(0.00423)						
Local Employee	-0.0668**	-0.0243**	0.0167**	-0.105**	-0.0330**	-0.0218**						
	(0.00409)	(0.00406)	(0.00297)	(0.00448)	(0.00453)	(0.00417)						
Sample Size		540,171			205,853							
College Major Controls		x	x		x	x		x	x		x	x
Occupation Controls			x			x			x			x

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each column is a separate regression with controls for age, age-squared, urban-rural, a cubic in usual hours worked, sex, race, marital status, and state dummy variables. For those with college and master's degrees, each column reports two regressions, one including those in the teaching profession and one excluding those in the teaching profession. Occupation controls are for twenty-five different census occupational categories. College major controls are for thirty-eight different college majors. Sample is from 2009-2012 and includes those aged 25 to 65 who are not self-employed and report working 50 to 52 weeks in the past year.

Table 8: Public-Private Earnings Differentials by Sex

	College Degree			College Degree (no teachers)			Master's Degree			Master's Degree (no teachers)		
Men												
State Employee	-0.148**	-0.115**	-0.0883**	-0.133**	-0.113**	-0.101**	-0.246**	-0.183**	-0.130**	-0.234**	-0.183**	-0.144**
	(0.0204)	(0.0166)	(0.0153)	(0.0184)	(0.0163)	(0.0169)	(0.0156)	(0.0142)	(0.0135)	(0.0163)	(0.0152)	(0.0160)
Local Employee	-0.134**	-0.0749**	-0.0123	-0.0792**	-0.0466*	-0.0167	-0.219**	-0.126**	-0.0483**	-0.149**	-0.0791**	-0.0775**
	(0.0254)	(0.0201)	(0.0196)	(0.0233)	(0.0203)	(0.0216)	(0.0190)	(0.0168)	(0.0178)	(0.0168)	(0.0151)	(0.0184)
Sample Size		300,392			288,254			121,146			108,881	
Women												
State Employee	-0.0695**	-0.0182	0.0125	-0.0513**	-0.0229+	-0.0188	-0.0881**	-0.0444**	-0.00210	-0.0975***	-0.0638***	-0.0407***
	(0.0138)	(0.0116)	(0.00889)	(0.0134)	(0.0134)	(0.0123)	-0.0146	(0.0135)	(0.0117)	(0.0124)	(0.0116)	(0.0117)
Local Employee	-0.106**	-0.0261+	0.0402**	-0.0555**	-0.0256+	-0.00646	-0.0746**	-0.0113	0.0505**	-0.0371**	0.0118	0.0168
	(0.0157)	(0.0135)	(0.00953)	(0.0143)	(0.0131)	(0.0118)	-0.0192	(0.0174)	(0.0141)	(0.0144)	(0.0127)	(0.0119)
Sample Size		289,468			251,917			134,605			96,972	
College Major		x	x		x	x		x	x		x	x
Occupation			x			x			x			x

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each column reports results from two separate regressions, one for men and one for women. Each regression controls for age, age-squared, urban-rural, a cubic in usual hours worked, sex, race, marital status, and state dummy variables. Occupation controls are for twenty-five different census occupational categories. College major controls are for thirty-eight different college majors. Sample is from 2009-2012 and includes those aged 25 to 65 who are not self-employed and report working 50 to 52 weeks in the past year.

Table 9: Public-Not for Profit Earnings Differentials

	High School Graduate or Some College		College Graduate			Master's Degree			Professional Degree		
No Teachers											
State Employee	0.0870**	0.0737**	-0.0277+	-0.00545	0.00850	-0.001	-0.00389	0.00884	0.0172	-0.00659	-0.0271*
	(0.0164)	(0.0137)	(0.0144)	(0.0130)	(0.0125)	(0.0122)	(0.0124)	(0.0127)	(0.0122)	(0.0101)	(0.0111)
Local Employee	0.0798**	0.0810**	0.00116	0.0236*	0.0365**	0.0777**	0.0783**	0.0706**	0.0556**	0.0541**	-0.0182
	(0.0142)	(0.0112)	(0.0118)	(0.0111)	(0.0097)	(0.0123)	(0.0119)	(0.0120)	(0.0163)	(0.0155)	(0.0172)
Teachers Only											
State Employee			0.167**	0.160**		0.141**	0.134**				
			(0.0113)	(0.0108)		(0.0168)	(0.0170)				
Local Employee			0.160**	0.155**		0.146**	0.141**				
			(0.0110)	(0.0104)		(0.0166)	(0.0169)				
College Degree Controls				x	x		x	x		x	x
Occupation Controls		x			x			x			x
Sample size	146,867		127,677 (teachers excluded) 41,942 (teachers)			77,268 (teachers excluded) 45,383 (teachers)			21,351		

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each column is a separate regression with controls for age, age-squared, urban-rural, a cubic in usual hours worked, sex, race, marital status, and state dummy variables. For those with college and master's degrees, each column reports two regressions, one excluding those in the teaching profession and one with only those in the teaching profession. Occupation controls are for twenty-five different census occupational categories. College major controls are for thirty-eight different college majors. Sample is from 2009-2012 and includes those aged 25 to 65 who are not self-employed and report working 50 to 52 weeks in the past year.

Table 10: Public-Private Earnings Differentials by Union Strength

	High School			College Degree			Master's Degree		
No Degree Controls	Strong	Middle	No Bargaining	Strong	Middle	No Bargaining	Strong	Middle	No Bargaining
State Employee	0.127**	0.0108	-0.0379	-0.0463*	-0.115**	-0.189**	-0.126**	-0.154**	-0.238**
	(0.0241)	(0.0134)	(0.0278)	(0.0192)	(0.0134)	(0.0167)	(0.0250)	(0.0150)	(0.0187)
Local Employee	0.113**	0.00813	-0.0600**	-0.0611**	-0.139**	-0.230**	-0.0897**	-0.144**	-0.253**
	(0.0170)	(0.0149)	(0.0133)	(0.0158)	(0.0110)	(0.00839)	(0.0233)	(0.0155)	(0.0156)
Degree Controls									
State Employee				-0.0175	-0.0651**	-0.131**	-0.0804**	-0.0953**	-0.180**
				(0.0188)	(0.0125)	(0.0112)	(0.0244)	(0.0143)	(0.0171)
Local Employee				-0.00125	-0.0569**	-0.146**	-0.0226	-0.0582**	-0.173**
				(0.00996)	(0.0107)	(0.0122)	(0.0204)	(0.0153)	(0.0154)
Sample size	353,750	333,315	148,602	270,984	208,619	110,257	122,656	86,754	46,341

**sig at <0.01 level; *sig at <0.05 level; +sig at <0.10 level. Each column is a separate regression with controls for age, age-squared, urban-rural, a cubic in usual hours worked, sex, race, marital status, and state dummy variables. For those with college and master's degrees, each column reports two regressions, with and without college major controls. Occupation controls are for twenty-five different census occupational categories. College degree controls are for thirty-eight different college majors. Sample is from 2009-2012 and includes those aged 25 to 65. Strong union states are defined as California, Connecticut, Illinois, New York, Ohio, Pennsylvania, Maryland, Michigan, Minnesota, New Jersey, Rhode Island, and Massachusetts. States that prohibit collective bargaining are North Carolina, Georgia, South Carolina, Texas, and Virginia. All other states are intermediate.

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