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SECTOR LABOR MARKETS

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**ABSTRACT**

This paper investigates how state and local fiscal institutions affect the pattern of relative wages between state and local government employees and their private sector counterparts. It focuses on changes in relative wages during the 1979-1986 period. Empirical analysis of data from the Current Population Survey suggests that in places with limitations on local property taxes, and to a lesser extent state-level tax and expenditure caps, public sector wages grew more slowly than the wages paid to comparable workers in the private sector. The differential movement of public sector and private sector wages is particularly pronounced for college-educated women who work in the local public sector. Many of these employees are public school teachers. There is some evidence that the impact of fiscal limits is most pronounced in the years immediately following their adoption, and that the effect of these limits weakens over time.

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Total compensation for state and local government workers in the United States rose eight percent faster than that for civilian workers between 1982, the first year when the Bureau of Labor Statistics published the Employment Cost Index for state and local workers, and 1997. The relative compensation of state and local workers rose particularly rapidly in the 1980s, and there was some private sector "catch up" in the 1990s. In the eight years leading up to 1990, the index of total compensation costs for the state and local sector rose 15.2 percent relative to that for civilian workers.

The growth in the relative pay of public sector workers stimulated a policy debate on whether such pay increases contributed to the fiscal problems of state and local governments in the early 1990s. Applied research on this issue is not conclusive. Cox and Brunelli (1992) focus on aggregate expenditures and conclude that fiscal stress and public sector pay levels are related. Belman and Heyward (1992) follow a different strategy, which relies on individual-level data, and reach the opposite conclusion. They find that wages in the public sector are insignificantly different from those in the private sector. Given the changing relative pay of highly-educated and less-well-educated workers during the 1980s, and the high fraction of public sector workers with college degrees, both analyses could be correct. More generally, the aggregate evidence suggesting substantial changes over time in relative compensation in the public and private sectors raises questions about the factors that determine wages and benefits in the public sector.

Much of the recent fiscal literature on public sector pay levels and state and local budgets has proceeded without regard to a voluminous literature in labor economics, surveyed by Ehrenberg and Schwarz (1986) and Gregory and Borland (1997), on the pay differential associated with working in the public rather than the private sector. Ehrenberg and Smith (1994) summarize these studies as suggesting a public sector wage premium for women, and a small wage penalty for men. Katz and Krueger (1991) find that poorly-educated workers enjoyed a growing public sector wage premium during the 1980s, while better-educated workers faced a shrinking public sector premium. One of the contributions of the present study is to present updated evidence on standard measures of the public sector wage premium. There is some evidence that historical wage patterns have changed during the 1990s.

Recent wage developments in the public and private sectors are of interest for more than simply their link to fiscal conditions. Public sector wages for many occupations, such as teachers and firefighters, are set without information on earnings of workers in

comparable private sector jobs. There are consequently opportunities for rent capture by public sector employees, and for opportunism on the part of taxpayers, who may vote to reduce pay whose skills are in limited demand in the private sector. Questions of rent-seeking have taken on a new urgency in state and local governments in the last two decades, as a wave of "taxpayer revolts" and "tax and expenditure limits" has placed new constraints on fiscal actors. One question that has received relatively little attention is the impact of such fiscal limits on the compensation of public sector employees, particularly in comparison with private sector workers with similar labor market characteristics. Wage and benefit costs account for approximately two thirds of the outlays of state and local governments, so if fiscal limits affect government spending, they are likely to affect public sector labor markets.

This paper presents new evidence on the correlation between state and local fiscal rules, state laws that affect the public sector collective bargaining environment, and the level and growth rate of relative wages in the public and private sectors. It exploits the rich cross-sectional variation in state fiscal institutions and labor laws, as well as the substantial variation over time in the fiscal rules within a number of states, to investigate the correlation between state institutions and relative wages in the public sector. The findings suggest that enactment of tight tax limits at the local level rules reduces the relative earnings of public sector workers. This evidence is particularly strong for college-educated women working in the public sector, many of whom are elementary and secondary educators. We find much weaker evidence linking fiscal rules to relative wages for men, and we find relatively weak evidence on the impact of state-level tax and expenditure limits.

We find evidence that the level and the growth rate of public sector wages for both men and women is related to the state's collective bargaining environment. More generous wage gains have been observed in states with more favorable postures toward collective bargaining by public sector workers. Wage levels at the beginning of our sample, 1979, were also higher in such states.

This paper is divided into five sections. Section one summarizes trends in relative wages and relative compensation in state and local government and in the private sector. It documents the expansion of relative compensation in the public sector during the 1980s,

and the subsequent decline in the 1990s, using data from the BLS Employer Cost Index. This section also reports estimates of traditional human capital models for wages of public and private sector workers. These equations provide a different perspective on relative wage patterns over the last two decades than that provided by aggregate compensation information. The individual-level wage data suggest an expansion of the relative public sector pay of women in the 1980s, followed by contraction in the 1990s. For men, the results from wage equations do not match those from the aggregate data, but suggest a rising public sector wage premium throughout this period. We disaggregate workers by level of education, and find some differences in the relative earning patterns of highly educated and less well educated workers.

Section two documents substantial variation in relative public sector and private sector wages across jurisdictions, and it provides the starting point for our analysis of how fiscal rules and labor market institutions have affected the relative growth of public sector and private sector wages. Section three summarizes variation in the nature of tax and expenditure limits that affect state and local spending in different states. It provides information on the fiscal limits that state and local governments adopted in the late 1970s, as well as the more recently-enacted fiscal limits of the early 1990s.

Section four presents our central results. We investigate the correlation between property tax limits, state tax or expenditure limits (TEs), and the growth rate of public sector wages during two periods: 1979-1996 and 1979-1990. The divergent movements of aggregate compensation data for the public and private sectors in these periods motivates our separate analysis. We find that during the 1980s, localities that adopted tax and expenditure limits in the late 1970s exhibited slower public sector wage growth (relative to private sector wage growth) than localities that did not adopt such limits. In the 1990s, however, localities with TEs from the late 1970s experienced more rapid relative public sector wage growth than other localities. At the same time, jurisdictions that adopted tax or expenditure limits at the beginning of the 1990s experienced slower relative public sector wage growth during the 1990s than comparable jurisdictions that did not adopt such limits. We find generally weaker evidence of any link between state-level fiscal rules and wage outcomes.

Section five concludes and discusses several directions for further work.

## 1. Compensation Comparisons Between Private and State & Local Labor Markets

This section uses two sources of information, the Employer Cost Index (ECI) compiled by the Bureau of Labor Statistics on the basis of employer surveys, and individual wage data reported in the Current Population Survey, to track the evolution of relative pay in the public and private sectors during the last two decades.

### 1.1 Compensation Data From the Employer Cost Index

Employer Cost Index information on the total compensation of state and local government employees has been published since 1982. The ECI does not distinguish between state and local government employees. During our sample period, local governments employed approximately twice as many workers as state governments; the local share is slightly higher for women than for men. The ECI data can be used to compare the average levels of compensation in the public and private sectors at a point in time, or to compare the relative growth rates over time in compensation for a fixed occupational mix of workers.

The March 1993 Employment Cost Survey, the most recent cross-sectional survey that underlies the ECI data and that is available for public analysis, shows a substantial difference between average compensation in state and local government (\$24.44 per hour) and the private sector (\$16.70 per hour). Nearly two thirds of this disparity is the result of higher wages and salaries in the public sector. Disaggregate information from this survey (U.S. Department of Labor (1993)) shows that part of the disparity between average compensation in the public and private sectors is due to the greater concentration of white collar workers in state and local government<sup>1</sup>:

	<u>White Collar</u>	<u>Blue Collar</u>	<u>Service</u>
Total Compensation			
- State/Local	\$27.67	\$18.78	\$17.04
- Private	19.67	16.43	8.54

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<sup>1</sup>More than half of state and local government employees are employed in the production of educational services. Teachers and most other workers in the education sector are white collar employees. Police, fire, and sanitation workers are classified as service workers. Data on share of employees by occupational categories are drawn from Braden and Hyland (1993).

<b>Wages &amp; Salaries</b>			
- State/Local	19.72	12.13	10.83
- Private	14.32	11.01	6.48
<b>Percent of Employees</b>			
- State/Local	68%	12%	20%
- Private	51	32	17

Even within broad occupational categories, however, both average compensation and average wage and salary payments for state and local employees exceed the comparable magnitudes for private sector workers. The absolute disparities are greatest for white collar and service employees. The percentage difference in compensation is greatest for service workers.

Between the first quarter of 1982 and the fourth quarter of 1997, the index of total compensation for private sector workers rose 86.5 percent, compared with 101 percent for state and local government employees. The corresponding annual growth rates of compensation are 4 and 4.5 percent, respectively. Most of the difference in compensation growth rates occurred during the mid-1980s, a period when strong economic activity led to rapid increases in state and local tax revenues. Between 1982:Q1 and 1990:Q4, public sector compensation grew at an annual average rate of 5.7 percent, compared with 4.4 percent in the private sector.

The ECI data present total compensation costs but they do not control for the characteristics of individual workers. They are nevertheless valuable because, unlike the wage data collected in the Current Population Survey (CPS), they include the cost of fringe benefits. Data from the March 1993 survey indicate that benefit costs averaged 43.8% of wages for public sector workers, and 40.3% for those in the private sector. The ECI data also suggest, however, that both wage and non-wage compensation increased faster for public sector than for private sector workers during the last two decades. This result

suggests that focusing exclusively on the determinants of relative wage levels in the public and private sectors, which we do using CPS data, may capture the broad trends in relative compensation over the last two decades.

### 1.2 Wage Data From the Current Population Survey

Most research on relative pay in the public and private sectors has relied on wage data from the Current Population Survey. Gregory and Borland (1997) summarize the large literature that has estimated human capital wage equations with some allowance for differences in wage levels between the public and private sectors. Interpreting the outcome of such regression models as measuring the pay differential between identical workers in the public and private sectors requires strong assumptions, such as the absence of unobserved characteristics among one group of workers or the other. While some previous studies have used two-step methods in an attempt to control for worker selection into the state-local as opposed to the private sector, the identifying assumptions that such models require are also very strong.

An additional difficulty, which Moore and Newman (1991) note, is that wage equations estimated on individual data typically lack information on precise job characteristics. There may consequently be omitted factors, such as the riskiness of some types of public sector jobs, that contribute to wage differentials. We do not propose any new solutions to these identification problems, but follow in the tradition of previous work in estimating a proportional wage differential between public and private sector workers. Because we are primarily interested in differences in the resulting wage differential either over time or across jurisdictions, our results should be robust to selection effects provided they are constant over time or across places.

Our empirical analysis of the CPS data closely follows the specification used by



Katz and Krueger (1991). Our wage equation relates the logarithm of an individual's hourly wage,  $\ln(w_{it})$ , to a set of individual characteristics ( $X_{it}$ ) that can affect marginal productivity, and an indicator variable ( $SLGOV_{it}$ ) for working in the public sector:

$$\ln w_{it} = X_{it}\beta_t + SLGOV_{it}\delta_t + \varepsilon_{it} . \quad (1)$$

The set of individual characteristics includes education, up to the fourth power of age, marital status, race, and residence in an SMSA.<sup>2</sup> We allow education to affect wages through a set of five categorical variables (EDUC) for number of years of schooling, corresponding to less than twelve years, twelve years, which typically corresponds to completing high school, thirteen to fifteen years, sixteen years (typically completing college), and more than sixteen years. We also include a set of control variables for ten broad occupational classifications, such as managerial and technical, sales, or crafts, in some equations.<sup>3</sup>

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<sup>2</sup>We include age as a proxy for work-experience. In an earlier version of this paper we used a measure of experience equal to age - years of schooling - 6 to capture time in the labor force. The education variable was reclassified in the 1992 CPS sample to represent categories of educational attainment. We use a fourth-order polynomial in age to capture labor force experience to avoid introducing more measurement error into our regressions in this later period. For the 1979-1991 period using experience as defined above or age does not change our results.

<sup>3</sup>The set of variables included in this wage equation is similar to that in Katz and Krueger (1991, 1992), although they estimate separate wage equations for workers in the public and private sectors, and then predict average wages in each sector for hypothetical workers with fixed characteristics. We estimate a single wage equation each year for all men, and all women, and impose the same coefficient vector  $\beta_t$  for the private and public sectors up to a year-specific shift parameter,  $\delta_t$ . In Poterba & Rueben (1994) we disaggregate this premium into that part attributable to differences in the returns to schooling and experience between public and private sectors. We find that while there are differences in the

We estimate equation (1) using data from the merged outgoing rotation groups in the CPS for the years 1979-1996. Changes in the CPS categorization of education in 1992 make exact comparisons of wage equations for the full seventeen year period difficult. We use a respondent's age as a proxy for experience, and we discuss the comparability of education categories in more detail in the appendix. We exclude self-employed individuals from our analysis, because it is difficult to measure their wage rates. We also exclude federal employees, because they are neither private sector nor state and local government workers<sup>4</sup>, as well as part-time workers.<sup>5</sup> We estimate separate wage equations for men and women.

Equation (1) allows the premium for state and local government employees ( $\delta_t$ ), as well as other coefficients in the wage equation, to vary across years. Figures 1a and 1b plot the estimated values of  $\delta_t$  from the wage equations for men, and for women, for the 1979-1996 period. The other coefficients from the estimated wage equations, which are similar to those in other studies using CPS data, are not reported.

Each of Figures 1a and 1b shows two curves, one corresponding to estimates of (1)

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levels of wage premia between sectors, the patterns over time are similar across education and experience groups. Gregory and Borland (1997) discuss these two approaches to analyzing data for workers in two different sectors in further detail.

<sup>4</sup>If we include federal employees, and allow a separate average wage premium for these workers, our results on the relative wages of state-local government and private sector employees are not affected. The average wage premium for federal workers, relative to private sector workers, is positive.

<sup>5</sup>If we include part-time workers and an indicator variable for part-time employment, the pattern of our results do not change. Public sector part-time workers in general receive lower relative wages than their full-time counterparts, but higher relative wages than part-time workers in the private sector. The number of part-time workers varies considerably by state

without occupational controls, the other with such controls. The standard error of each year's estimate of the wage premium is approximately 0.009.

Figure 1a shows that after controlling for worker attributes, men employed in the public sector earned less on average than their private sector counterparts for most of the last two decades. Moreover, there has been substantial and relatively continuous reduction in the "pay penalty" for men working in the public sector. While there was an average penalty of nearly 10 percent in 1979, the public and private sector earnings disparity for men with similar characteristics was largely gone by 1996.

Figure 1b suggests a substantially different time series pattern for women's wage differentials. At the beginning of the 1980s, the wage equations without occupational controls suggest a public sector wage premium of about three percent. Wage equations with occupational controls suggest virtually no wage differential. Both specifications suggest relative growth in public sector pay during the mid-1980s, to an estimated premium of 6 percent without occupational controls and 2 percent with occupational controls. After 1988, however, the relative wage in the public sector begins to decline, so that by 1996, the specification without occupational controls yields an estimated pay premium of one percent. The specification with occupational controls yields a 1996 pay penalty of nearly 2 percent for the public sector. In the remaining analysis we will focus on regression results which control for broad occupational categories.

The public sector wage differentials in Figures 1a and 1b contrast with the earlier estimates based on differences in average wages in the Employment Cost Index. This is primarily due to the differences in the characteristics of workers in the public and private sectors. Public sector workers have more education, and are typically older and more experienced, than private sector workers. This underscores the limits of comparisons

based on the ECI data.

## 2. Differences in Public Sector Pay Premia Across States

The foregoing discussion considered changes over time in relative wages in the public and private sectors. It is also possible to consider differences in relative wages across different jurisdictions. This section begins that analysis, which provides the motivation for our analysis of fiscal rules and bargaining institutions.

Three previous studies have explored the geographic differences in the public sector wage premium. First, Borjas (1986) used data from the 1980 Census to estimate state-specific wage premia for several large states. He related his findings to a model of public demand for government services, and thus attempted to provide a structural interpretation of interstate differences in relative wages. Second, Katz and Krueger (1991) plotted public sector versus private sector wage levels across states, implicitly showing heterogeneity in the relative public sector wage. They did not explore the source of these differences. Finally, Gyourko and Tracy (1991), whose analysis is most similar to that reported below, examined data on workers from a cross-section of SMSAs drawn from the 1980 Census of Population. They found that public sector wages were lower in SMSAs with tax or expenditure limits. They also considered variables that affect the bargaining environment between public sector workers and local governments. Our study draws on the much richer set of tax and expenditure limits that have been enacted since 1980. It considers changes in relative wages in the public sector as well as the level of such wages.

To examine the relative wage premia of state and local workers across states we augment equation (1) in two ways. First, we include a set of state specific indicator variables to capture interstate differences in wages for all workers. Second, we interact

these state-specific indicator variables with the indicator variable for working in the state and local government sector (SLGOV). We thereby estimate a set of fifty state-specific public sector wage premia,  $\delta_{jt}$ , instead of the single public sector wage premium  $\delta_t$ , that we estimated in equation (1). The resulting equation is:

$$\ln w_{it} = X_{it}\beta_t + \sum_{j=1}^{48} (\alpha_{jt} + SLGOV_{it} * \delta_{jt}) * STATE_{ijt} + \varepsilon_{it} \quad (2)$$

$STATE_{ijt}$  is an indicator variable set equal to one if worker  $i$ , in year  $t$ , resides in state  $j$ , and zero otherwise. We focus on interstate differences in the conditional means of the public and private sector wage distributions. We have also estimated state effects using quantile regression methods, and the differences across states in the resulting fixed effects coefficients are very similar to the OLS results. As with equation (1), we estimate equation (2) separately for men and women using data from the merged outgoing rotation groups in the CPS for the years 1979-1996.

State effects interacted with the public sector indicator variable improve the explanatory power of the estimated wage equations. If we pool the outgoing rotation groups in the 1995 and 1996 CPS data files, for example, and estimate a cross-sectional wage equation with and without state-specific public sector wage effects, there is a significant improvement in explanatory power from the more general specification. Adding state effects raises the adjusted  $R^2$  from .382 to .403 for men, and from .383 to .406 for women. Throughout our analysis we follow the strategy of pooling two adjacent years of the CPS data files to increase our sample sizes and to obtain more precise estimates of the public sector wage premia for different states.

Figures 2a and 2b plot the distributions of estimated state-specific state and local

sector wage premia from the pooled 1979/1980 and 1995/1996 outgoing rotation groups. We show the comparison to provide some indication of the changing nature of the premium distribution. The figures display the substantial heterogeneity in the estimated public sector wage premia for different states. Over this period there was a substantial right-ward shift in the relative wages of men in the public sector. While in 1979/80 only six states (of forty-eight) showed positive premia for male workers, by 1995/96 the number had increased to twenty-one. Two states had wage premia of more than 15 percent. The distributions for women are less dispersed, but they still show substantial variation. There is substantial dispersion in the estimated state public sector wage effects in both 1979/80 and 1995/96.

State-specific public sector wage premia also display substantial persistence over time. Figures 3a and 3b show scatterplots of the state-specific wage premium for men estimated from the 1979/80 outgoing rotation groups, CPS data, and the corresponding estimate from 1995/96. Both figures show strong positive correlation between the premia estimated in different years. For men, the correlation of the forty-eight state effects is .63, while for women, the correlation is .47.

Figures 3a and 3b identify individual state observations by their two-letter abbreviations, so they also provide information on which states have high, and low, public sector wage premia. For men, California shows the highest premium in 1995/96, followed by Rhode Island, New York, and Connecticut. The states with the lowest wage premium for men are Wyoming, Mississippi and South Carolina. For women, California is also a high premium jurisdiction in 1995/96, but it exhibits a smaller premium than New Jersey and Missouri. The states with the largest pay penalties for women are Colorado, South Carolina, Georgia and Vermont. In 1979/80, the correlation between the state-specific

state and local wage premium for men, and that for women, was .54. This correlation was somewhat greater, .67, in 1995/96.

Table 1 presents more detailed information on the estimated state and local government wage premia for the twelve largest states in three pairs of years: 1979/80, 1990/91, and 1995/96. These are the states for which the CPS data provide the most precise estimates of  $\delta_{j,t}$ . The point estimates show substantial and statistically significant changes in relative wage premia for several states between the early 1980's and the mid-1990's. In California, for example, the estimated premium for men increases 18.5% during the 1979/80 - 1995/96 period, although there is little change in the premia for women. In only one of these twelve states, North Carolina, do men experience a decline in the estimated public sector wage premium during this period. In contrast, the premium for women declines in four of the twelve states.

To more explicitly test for movements in state-specific relative wage premia over the 1979/80-1995/96 period, we pooled the outgoing rotation group data from all four of these years, and estimated a single equation allowing for different state-specific effects in the two pairs of years. We also allowed for differences in the coefficients on the other explanatory variables in between 1979/80 and 1995/96. We rejected the null hypothesis that the state effects are the same in 1979/80 and 1995/96; this result applies to our estimates for both men and women. Moreover, t-tests on the differences in individual state effects between the two sets of years (i.e. for the constancy of the California state effect between 1979/80 and 1995/96) reject the null hypothesis of constant effects in 27 of 48 states for men, and 27 of 48 states for women. This provides motivation for our subsequent analysis, which parameterizes the cross-state differences in public sector wage premia and allows them to depend on state fiscal rules and collective bargaining laws.

### 3. State Fiscal Limits and Labor Organizing Provisions

One potential explanation for differences in the relative public and private sector wage across states is that there are differences in the institutional environment in which public sector wages are determined. Fiscal institutions are one source of such differences; state labor laws relating to public sector unionization are another. A number of previous studies have investigated the link between collective bargaining institutions and wages, but, with the notable exception of Gyourko and Tracy (1991), there has been relatively little previous work on fiscal institutions and public sector wages.

#### 3.1 State Differences in Fiscal Rules

There is a small but growing literature on the effects of fiscal institutions on the level of state and local government spending. Examples include Abrams and Dougan (1986), Ichniowski and Preston (1991), Poterba (1994), and Rueben (1995). Because employee compensation is a large share of state and local government spending, if fiscal institutions affect the level of spending, they are likely to have some effect on the aggregate earnings of state and local government employees. Such an effect could operate through changes in the number of state and local government workers, through changes in the relative wages of these workers in comparison to their private sector counterparts, or through both channels. Our analysis focuses on the relative wage effect.

Ehrenberg (1979) presents a theoretical discussion of how tax and expenditure limits could affect relative wages in the public and private sectors. He argues that while it is straightforward to show that such laws could reduce the demand for public sector labor, he observes that they may also reduce the supply of labor to this sector, as potential employees conclude that public sector jobs are prospectively less attractive than positions in the private sector. In his model, the net effect of fiscal restrictions on public sector



wages is ambiguous. Assessing the impact of fiscal rules on relative wages in the public and private sectors is therefore an empirical matter.

We focus on two types of fiscal institutions: overall limits on state taxes or expenditures, and limits on local property tax collections or local revenues. Limits that apply to state government spending should have their primary impact on workers who are employed by the state, while limits on local taxes or expenditures should primarily affect those who work in the local public sector. We build this restriction into our empirical analysis.

One of the difficulties with analyzing the impact of fiscal rules is that many states and localities have adopted nominal, but non-binding, fiscal limits. An overview of the fiscal institutions in the U.S. states may be found in the Advisory Council on Intergovernmental Relations (1987) and the National Association of State Budget Officers (1991), but neither of these sources distinguishes binding from "incidental" limits. Rueben (1995) presents a detailed summary of the various fiscal limits that affect sub-federal governments in the United States, and she classifies limits into binding and non-binding categories. We follow this categorization in our analysis.

Rueben's (1995) analysis treats states with "advisory" fiscal limits as having non-binding limits. In addition, if a state legislature can override or exceed a limit with a simple majority vote, it is classified as non-binding. Most of the states with binding limits enacted them in the "taxpayer revolt" of the 1978-1982 period, although several states passed such limits in the "echo taxpayer revolt" of the early 1990s.<sup>6</sup> Similar criteria are used to

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<sup>6</sup>Continental states with binding state tax or expenditure limits are Michigan, Missouri, Massachusetts (1986+), New Jersey (1976-83), South Carolina, Texas, Tennessee, Oklahoma, Montana, Idaho, Arizona, Washington and California.

determine whether restrictions on local taxes and expenditures are binding or advisory.

We define two variables corresponding to state-level tax and expenditure limits. The first, FRACTION, equals fraction of the 1979-1996 period over which the state had a binding fiscal limit in force. A state that had an binding limit in place for the entire time period would have a FRACTION value of 1; a state that adopted a binding limit in 1992, and therefore had a limit in place for only five years in our sample, would have a FRACTION value of 5/18. FRACTION is set to zero for states without any binding fiscal limits during our sample period.

The second variable that we define, STATE90, is an indicator variable for states that adopted fiscal limits in the "echo taxpayer revolt" of the early 1990s. States that adopted binding fiscal limits in the 1990s are North Carolina, Nevada, Colorado, Utah, Louisiana and Florida. The STATE90 variable allows us to test the hypothesis that recently-adopted state fiscal limits have different effects on the public sector wages than the earlier set of tax and expenditure restrictions.

We analogously define two variables to capture the impact of local tax limits, which typically restrict property tax revenues. The first, FRACTIONLOC, is the fraction of the 1979-1996 period over which localities in a state were affected by limits that were adopted after 1970. We also only classify states as having binding local limits if overall local revenues are constrained or property tax revenues are constrained. (This means we exclude states that constrain either the assessed property value or the property tax rate, but not both.) We focus on limits enacted since 1970 because many states have long-standing limits on local property tax collections, dating to the 1930s or before, that do not represent binding restrictions on current property tax revenues. Twenty-four states have

adopted some type of property tax limits since 1970.<sup>7</sup>

As with state tax and expenditure limits, local property tax limits were enacted in two waves, one in the late 1970s, the other in the early 1990s. We therefore define an indicator variable, LOCAL90, for those states that adopted local property tax restrictions in the early 1990s. States that passed binding local limits in the 1990s are Montana, Colorado, Florida, Illinois, Nevada, West Virginia and Oregon. We investigate whether property tax limits have different effects on public sector wages in the years immediately after they are enacted than after they have been in force for some time.

### 3.2 State Variation in Labor Market Institutions

We also consider cross-state variation in the labor market institutions that affect the bargaining power of public sector employees. The incidence of unionization among state and local workers is high, but there are substantial differences across states in the relative power of public sector unions. A large previous literature has explored the impact of state labor rules, generally finding that wage levels are higher in states that grant greater bargaining power to public sector unions (see the many references in Gregory and Borland (1997)).

There are many different labor market institutions that vary across states, but we focus on only two summary variables. The first is an indicator variable for the presence of public sector right-to-work laws. Such laws restrict the ability of public sector unions to compel membership in the union as a requirement of certain public sector jobs. The

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<sup>7</sup>States with post-1970 property tax limits are Massachusetts, Rhode Island, New Jersey, Ohio, Indiana, Illinois, Michigan, Minnesota, Iowa, Missouri, North Dakota, Maryland, North Carolina, Kentucky, Alabama, Louisiana, Texas, Montana, Arizona, Nevada, Washington and California.

presence of such laws typically reduces the power of public sector unions.

The second variable we consider is a summary statistic for state posture toward public sector unions that was constructed by Valletta and Freeman (1988). This "summary bargaining environment statistic," which is based on the state rules in place in 1984, combines information on many aspects of state public sector labor legislation. Higher values of this variable correspond to more favorable environments for public sector unions.<sup>8</sup> Unfortunately, our data do not capture time-series variation in the labor market institutions in different states. We nevertheless explore whether states with more favorable environments in 1984 had higher or lower wage levels, and experienced faster or slower wage growth, than other states with different laws.

### 3.3 Institutional Endogeneity

One issue that is an immediate concern in studies that relate state laws, such as fiscal rules or labor market institutions, to economic outcomes such as wage levels or growth rates, is that the institutions may be correlated with unobserved third factors that are the fundamental determinants of the outcome variables. This is a clear concern for the institutional variables described above, since states with voters who dislike public spending may enact fiscal limits. Such voters may also support elected officials who try to restrict relative wages in the public sector. Similarly, a state in which voters are favorably inclined toward workers in the public sector may enact supportive labor legislation to bolster public

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<sup>8</sup>Valetta and Freeman (1988) and Zax and Ichniowski (1990) find that pro-labor legislation increases the probability that public sector workers will be represented by a union. Ichniowski and Zax (1991) study the effects of right-to-work laws on union representation. Gregory and Borland (1997) and Ehrenberg and Schwarz (1986) survey the voluminous literature on the relationship between union representation and public sector wages, which generally suggests that such representation raises wages.

sector unions, and also support generous public sector wages.

It is extremely difficult to devise a satisfactory solution to this endogeneity problem. Such a solution would require finding instrumental variables that are correlated with the presence of state fiscal or labor market institutions, but that are not correlated with unobserved variables such as voter tastes. We have experimented with some potential instrumental variables, such as the state constitutional provisions that determine whether state voters can use "direct democracy" methods to enact legislation. The variables that we tried had very limited explanatory power in the first stage of our two-stage least squares analysis, so we do not report any two-stage least squares results in what follows. We rather present our OLS findings, note the potential danger of institutional endogeneity, and leave the potential solution of this problem for future work.

#### 4. Empirical Results on Institutional Variation and Wage Patterns

We focus on how fiscal and labor market institutions affect the growth rate of relative public and private sector wages during our sample period, as well as the relative wage level. Focusing on wage changes reduces the risk that correlation between institutions and unobserved state factors drives the results, but it also makes it difficult to evaluate the long-term effect of different fiscal or labor bargaining institutions.

We begin by considering relative wage changes over two different time periods. The first is our full sample period, 1979/80 - 1995/96. This sample period captures the effect of changes in fiscal rules in the late 1970s, as well as changes in the early 1990s. The second sample period we consider is 1979/80 - 1989/90. This shorter sample corresponds to the period of most rapid relative growth in both male and female public sector wages. It also permits us to analyze the short-run effect of tax limits passed in the

1970s. An additional attraction of this shorter sample is that it avoids the need to splice data from Current Population Surveys with different definitions of the education variables. Our procedure for such splicing in the longer sample period is detailed in the appendix.

The basic specification that we estimate assumes that the state and local public sector wage premium in state  $j$  in either 1989/90, or 1995/96, can be written as a function of the corresponding state premium in 1979/80 and a variety of institutional

$$\bar{\delta}_{j,t} = \bar{\delta}_{j,1979} + \sum_{k=1}^N \theta_k * INST_{kj} \quad (4)$$

factors that influence the change in the state wage premium during the sample period. Equation (4) describes how we parameterize a set of restrictions on a model like that in equation (2). Because we allow the INST variables to affect the difference between the state-specific public sector wage effect in 1979/80 and 1995/96, the  $\theta$  coefficients measure the impact of these institutions on the change in the log of relative wages, or the growth rate of the public sector wage premium.

Table 2 presents the results of estimating equation (2), with the specification modification (4), over the period 1979/80-1995/96. We report only the coefficients on the fiscal limits and bargaining environment variables, and we present separate estimates of wage equations for men and women. We also include in the specifications a measure of the average state unemployment rate for the 1980-1994 period. This is designed to control for any differences in the sensitivity of public and private sector wages to aggregate macroeconomic activity. We assume that limits on state taxes or expenditures affect state government workers, while limits on local taxes affect those who are employed in the local public sector, but not vice versa.

Table 2 shows several different specifications for the equations for both men and women. The specification in the first column of Table 2 includes only the fiscal limitation variables FRACTION and FRACTIONLOC in explaining the cross-state patterns of public sector wage premia. The results suggest a mixed pattern of effects across men and women, with no statistically significant effects of fiscal rules on relative wages.

The second column of Table 2 reports a specification that includes the indicator variables for tax limits that were adopted at the beginning of the 1990s. The pattern of results is much more consistent across men and women, and across the two types of limits. All of the estimated coefficients are negative, suggesting that relative public sector wages have grown more slowly in states and localities with tax limits than in those without such limits. Only the coefficient linking local property tax limits to the relative growth of women's wages is statistically significantly different from zero at conventional confidence levels.

The point estimate suggests that in 1995/96, relative public sector wages for women were 6.2 percent lower in states with post-1990 local property tax limits than in states without such limits. The substantial effect of local tax limits on women's wages may reflect the greater presence of women in teaching, which is locally-financed, than in other parts of the state-local sector.

The specifications in the next two columns of Table 2 include the variables for collective bargaining institutions as well as the indicators for fiscal institutions. These labor market institutions have statistically significant effects on the relative growth of public and private sector wages. States with more generous legislative environments for public sector collective bargaining experience more rapid wage growth during our sample, while relative public sector wages grow more slowly in states with right to work laws.

The effects are substantively large: a right to work law is estimated to reduce public sector relative wage growth by as much as 8.2% for men, and 3.5% for women, during our eighteen year sample period. These findings are broadly consistent with many earlier studies, using similar explanatory variables to study the level of public sector wages, that find more union-friendly institutions to be correlated with higher relative wages in the public sector. Our results provide limited new information on the effect of these rules, so we focus our attention on the findings with respect to fiscal variables.

Controlling for the collective bargaining environment affects the pattern of coefficients on the fiscal institutions. In the fourth column of Table 2, seven of the eight fiscal limit variables have negative coefficients, although we still find that only one of these variables, the LOCAL90, is statistically significant at standard confidence levels.

The specification in the final column of Table 2 includes the average unemployment rate in each state during our sample period, and finds that higher unemployment rates lower relative public sector wages, but this effect is not statistically significant.

Table 3 presents similar regression results for the shorter 1979/80-1995/96 sample period. The results for this period provide more consistent evidence of a relative wage effect from fiscal limits than do the results for the full sample. In virtually all of the specifications, we now find negative and (usually) statistically significant effects of the fiscal limit variables. The point estimates (using those in column five as an illustration) suggest that relative wages for men employed in the local public sector declined 3.5 percent more, between 1979/80 and 1990/91, in states with binding local property tax limits than in states without such limits. The effect is even larger for women: a 5.4 percent decline. These results support a substantively, as well as statistically, significant effect of local fiscal rules on public sector wages.



The results presented so far consider the change in the public sector wage premium during our eighteen year sample. We can also ask whether the level of the wage premium is affected by either fiscal or bargaining institutions. Table 4 presents results relating the level of the wage premium in 1979/80 to the variables we considered in Table 2. Studying this relationship provides one way of evaluating whether stringent fiscal rules were more likely to be enacted in states with high relative wages in the public sector at the beginning of our sample period.

The results in Table 4 suggest a positive association between the level of public sector wages among state government employees and the enactment of binding state fiscal limits. These effects are statistically significant for women, but not for men. Voters in states with higher public sector wages may have found it more attractive to enact spending limits to reign in public sector labor costs. The results for local property tax limits nevertheless cast some doubt on this interpretation, since it appears that there is a negative, although statistically weak, correlation between relative local public sector wages and the enactment of local tax limits.

The results in Table 4 also suggest a positive effect of the generosity of the collective bargaining environment on the level of public sector wages in 1979/80. There is no evident relationship between right to work laws and the level of relative wages. It is noteworthy that effects we find in Tables 2 and 3 for the change in the relative public sector wage during the last two decades build upon the differences in these wages at the beginning of our sample. Our consistent findings for the impact of labor market institutions on public sector relative wages provide an important warrant for further work, addressing the endogeneity issues raised above, to evaluate the causal links between these institutions the determination of public sector wages.

## 5. Conclusions

This paper has presented suggestive evidence on the link between tax and expenditure limits, the public sector bargaining environment, and the evolution of public sector wages. The results imply that the relative rate of growth of public sector wages, by comparison to private sector wages, is slower in states with more restrictive fiscal environments, and in states with less favorable environments for public sector unions, than in other states. These results are particularly evident for local public sector workers, whose wages appear to be influenced by the enactment of local property tax limits. The findings also suggest that the effect of fiscal limits is strongest in the years immediately following their enactment. We find a stronger effect of property tax limits enacted in the late 1970s on the growth of relative public sector wages over the 1979-1990 period than over the 1979-1996 period. We also find a substantial effect of property tax limits enacted in the early 1990s on the relative growth of local public sector wages over the 1979-1996 period.

Our findings suggest many directions for future work. First, we have not considered the potential selection biases that plague studies of inter-sectoral wage differences, whether between the public and private sectors or the union and non-union sectors. This is because we have not found variables that are likely to affect the probability of public sector employment, but not public sector wages, and that could consequently be used to identify selection models. Further analysis of the potential importance of such effects, and in particular of whether such effects could be more important in some states than others, is an important topic for future investigation.

A second issue that warrants investigation is the effect of fiscal and labor market institutions on public sector employment. While fluctuations in the relative wage paid to

public and private sector workers can affect total spending by state and local governments, changes in employment could have even larger effects. There are many dimensions on which employment can be affected by fiscal and labor market institutions: downsizing existing departments, increased reliance on volunteers, privatization of government functions, and elimination of services are all examples of adjustments that could affect the size of the public sector workforce. There are important differences across states in the rate of growth of per capita state and local government employment. Poterba and Rueben (1995) present some preliminary evidence suggesting that property tax limits and labor market institutions can interact in important ways to affect the growth of public sector employment, but this topic deserves further analysis.

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Figure 1A: Wage Premia for Male State and Local Workers

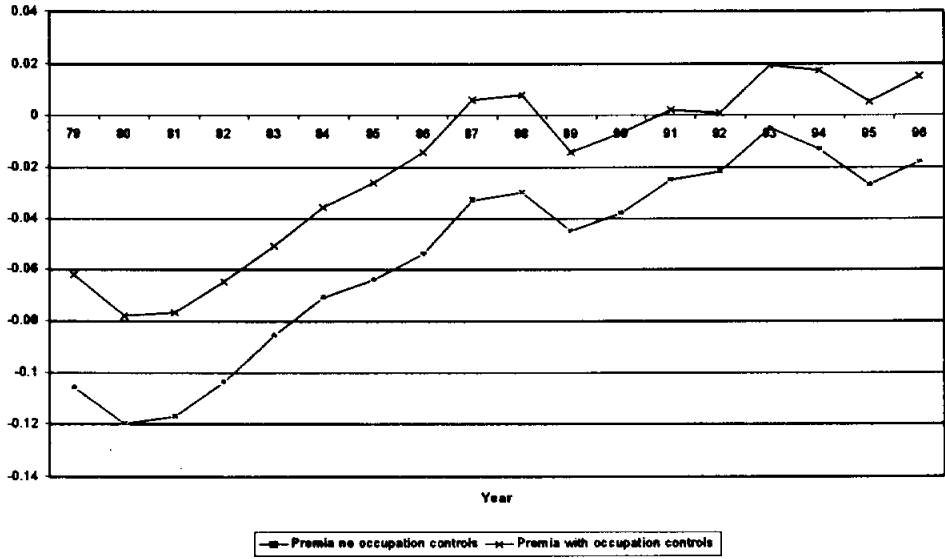
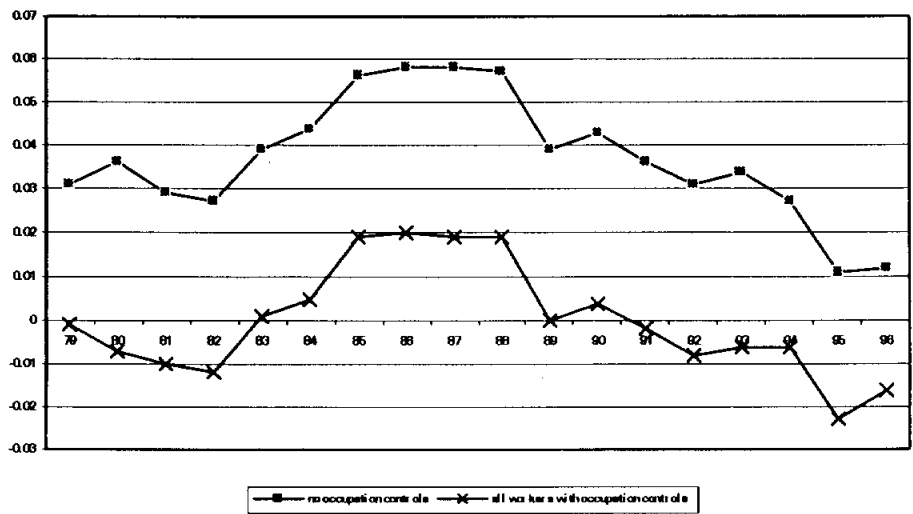
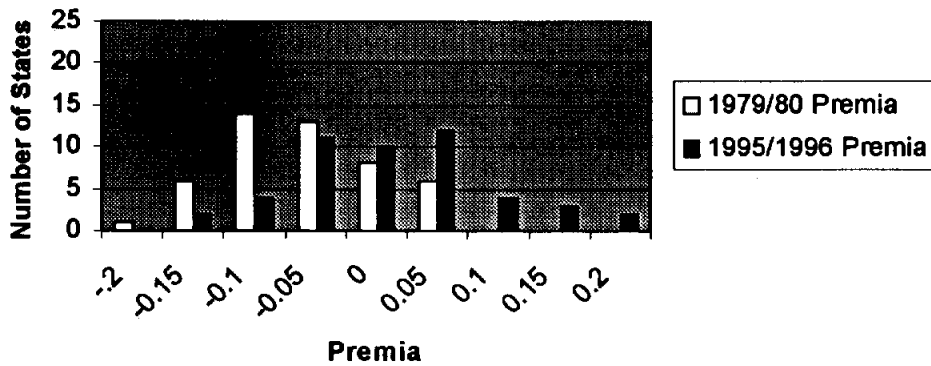


Figure 1B: Wage Premia for Female State and Local Workers



**Figure 2a: Distribution of Male State and Local Public Sector Worker Wage Premia Across States**



**Figure 2b: Distribution of Female State and Local Public Sector Worker Wage Premia Across States**

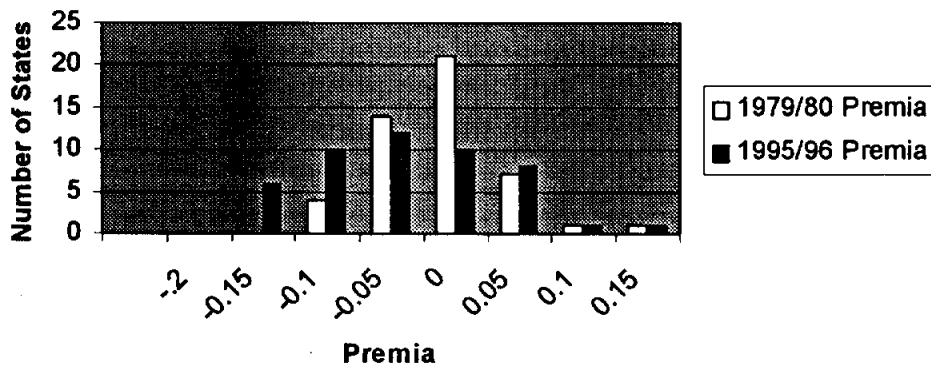






Table 1: State and Local Wage Premia for the Twelve Largest States, Various Years

State	1990 Pop- ula- tion	Men			Women		
		1979/80	1989/90	1995/6	1979/80	1989/90	1995/6
California	29.7	.017 (.011)	.141 (.014)	.202 (.016)	.087 (.010)	.081 (.013)	.092 (.015)
New York	17.9	.033 (.011)	.103 (.013)	.145 (.015)	.031 (.011)	.079 (.012)	.059 (.015)
Texas	16.9	-.087 (.015)	-.031 (.017)	-.031 (.019)	-.035 (.012)	-.040 (.013)	-.038 (.016)
Florida	12.9	.012 (.019)	.076 (.017)	.040 (.021)	.036 (.017)	.051 (.014)	.041 (.018)
Pennsylvania	11.8	-.057 (.016)	-.008 (.019)	.064 (.023)	.024 (.015)	.060 (.017)	.085 (.022)
Illinois	11.4	-.042 (.017)	.034 (.018)	.086 (.022)	.047 (.015)	.036 (.016)	.020 (.020)
Ohio	10.8	-.111 (.017)	-.016 (.018)	.031 (.024)	-.022 (.014)	.007 (.016)	-.001 (.020)
Michigan	9.3	-.057 (.018)	.017 (.018)	.071 (.023)	.051 (.015)	.136 (.016)	.123 (.021)
New Jersey	7.7	.027 (.019)	.062 (.017)	.102 (.023)	.077 (.017)	.035 (.015)	.153 (.021)
North Carolina	6.6	.017 (.022)	-.013 (.017)	-.046 (.026)	.008 (.017)	.020 (.014)	-.065 (.021)
Georgia	6.5	-.104 (.022)	-.002 (.037)	-.087 (.037)	-.031 (.019)	-.034 (.028)	-.111 (.027)
Massachusetts	6.0	-.021 (.019)	.020 (.018)	.026 (.027)	.016 (.018)	-.009 (.017)	.037 (.025)

Notes: Results are from regressions run on data from the Outgoing Rotation Groups of the CPS for full-time employees. Regressions also controlled for educational attainment, experience, marital status, race, SMSA status and state of residence. Standard errors are reported in parentheses. State populations are reported in millions of persons.

Table 2: Fiscal Institutions, Labor Market Institutions, and Changes in Relative Public Sector Wages, 1979/80 - 1995/96

Variable	Men			
Binding State Limit (FRACTION)	-.022 (.033)	-.017 (.033)	-.030 (.028)	-.027 (.028)
Local Property Tax Limit (FRACTIONLOC)	.019 (.023)	.016 (.023)	.007 (.016)	.008 (.016)
1990 State Limit (STATE90)		-.032 (.035)	-.038 (.032)	-.040 (.031)
1990 Local Property Tax Limits (LOCAL90)		-.001 (.030)	-.008 (.034)	-.008 (.034)
Average Unemployment Rate, 1980-94				-.006 (.005)
Collective Bargaining Index		.065 (.039)	.063 (.040)	.058 (.040)
Right to Work Law		-.082 (.020)	-.084 (.019)	-.090 (.019)
	Women			
Binding State Limit (FRACTION)	.003 (.023)	.002 (.023)	-.008 (.021)	-.005 (.021)
Local Property Tax Limit (FRACTIONLOC)	-.015 (.020)	-.012 (.020)	-.015 (.016)	-.014 (.016)
1990 State Limit (STATE90)		-.013 (.025)	-.014 (.023)	-.016 (.022)
1990 Local Property Tax Limit (LOCAL90)		-.062 (.019)	-.067 (.023)	-.065 (.022)
Average Unemployment Rate, 1980-94				-.006 (.006)
Collective Bargaining Index		.107 (.038)	.100 (.036)	.094 (.037)
Right to Work Law		-.035 (.021)	-.044 (.020)	-.050 (.020)

Notes: Results are from regressions run on data from the Outgoing Rotation Groups of the CPS for full-time workers. Regressions also control for educational attainment, experience, marital status, race, SMSA status and state of residence, and allow separate coefficients on these covariates for the first (i.e. 1979/80) and second (i.e. 1995/96) sets of years in the sample. A set of state specific indicator variables are also included for state and local employment. Reported coefficients are for interactions of variables with an indicator variable for employment in the state and local sector in 1995/96. Standard errors are reported in parentheses and are corrected for within-state correlation.

Table 3: Fiscal Institutions, Labor Market Institutions, and Changes in Relative Public Sector Wages, 1979/80 - 1989/90

Variable	Men			
	Binding State Limit (FRACTION)	-.000 (.025)	.009 (.025)	-.012 (.023)
Local Property Tax Limit (FRACTIONLOC)	-.035 (.021)	-.029 (.021)	-.040 (.018)	-.035 (.017)
Average Unemployment Rate, 1980-89		-.012 (.006)		-.015 (.006)
Collective Bargaining Index	.044 (.032)		.036 (.031)	.012 (.029)
Right to Work Law	-.044 (.016)		-.053 (.016)	-.070 (.019)
	Women			
Binding State (FRACTION)	.000 (.021)	.009 (.023)	-.013 (.017)	-.003 (.019)
Local Property Tax Limit (FRACTIONLOC)	-.055 (.014)	-.048 (.014)	-.059 (.011)	-.054 (.009)
Average Unemployment Rate, 1980-89		-.014 (.006)		-.017 (.006)
Collective Bargaining Index	.069 (.031)		.056 (.030)	.032 (.028)
Right to Work Law	-.030 (.018)		-.044 (.019)	-.062 (.020)

Notes: See Table 2.

**Table 4: Fiscal Institutions, Labor Market Institutions, and the Level of Public Sector Relative Wages, 1979/80**

Variable	Men		Women	
	Binding State Limit (FRACTION)	0.042 (0.026)	0.039 (0.025)	0.056 (0.018)
Binding Local Tax Limit (FRACTIONLOC)	-0.023 (0.022)	-0.022 (0.020)	-0.015 (0.015)	-0.015 (0.014)
Collective Bargaining Index		0.046 (0.027)		0.025 (0.017)
Right to Work Law		0.001 (0.019)		-0.002 (0.014)

Notes: Results are from regression equations estimated on the Outgoing Rotation Groups of the CPS for full-time workers. Regressions control for educational attainment, experience, marital status, race, and SMSA status. State tax and expenditure limits are only allowed to affect state government workers; local property tax limits only affect local public sector employees. Standard errors reported in parentheses are corrected for within-state error correlation.

### **Appendix: Changes in CPS Coding of Education, Pre- and Post-1992**

Prior to 1992, the Current Population Survey questions concerning educational attainment focused on number of years of school attended, and whether the respondent had completed the final year of schooling. Beginning in 1992, the survey asked about the highest grade of school attended. The modified questionnaire also grouped some potential responses on years of schooling, such as grades 1-4 and grades 5-6.

These changes makes it impossible to estimate experience = age-years of school-6 in a consistent way across the sample years, and to produce the same set of education indicator variables. To estimate a comparable equation, we have included a quartic in age as a proxy for experience. We have assumed that 11 or less years of education corresponds to less than a high school education, 12 completed years of education equals obtained a high school diploma or equivalency certificate. 13-15 years of education is the same as some college, or an associates degree. 16 years of education equals a BA and 17 or more years of schooling is equivalent to a post-graduate degree. Jaeger (1993) presents some evidence on the relative performance of wage equations estimated with the two sets of educational variables. In this appendix, we present summary information on the distribution of responses to the 1990-92 surveys, and the pattern of responses for those who were included in both the 1991 and 1992 Current Population Surveys.

Table A-1 shows the distribution of responses across education categories for respondents in the 1990-92 Current Population Surveys. The pre-1992 respondents are classified by number of years of education completed, while the 1992 respondents are categorized by highest grade attended. There are two differences. First, the fraction of 1990/1991 respondents who are classified as having twelve years of schooling is more than two percent greater than that in 1992. This is offset by a higher fraction of the 1992 respondents who appear to have attended some college, but do not have a college degree. Second, the 1992 survey reveals a higher fraction of respondents with sixteen years of schooling, and a lower fraction with post-graduate degrees.

To provide further information on the nature of the response changes, Table A-2 reports a cross-tabulation of responses to education questions for respondents who were in the Current Population Survey in two consecutive years. The column labeled 1990/1991 shows the degree of agreement between responses to the same survey instrument, pertaining to the same individual, in two consecutive years. For most levels of educational attainment, the agreement rates are greater than 95%, with the notable exception of the 11 or 12 years of schooling (no high school degree) category. The incidence of identical responses is 94% for those completing 12 years of high school, and 97% for those with 16 years of schooling.

Table A-2 also shows the agreement in education responses for individuals who were surveyed with different instruments in 1991 and 1992. The incidence of identical responses for those with 1991 responses showing fewer than twelve years of education completed is less than 70%. Since the 1990/91 cross-tabulation suggests relatively little pure measurement error in these questions, these results suggest substantive differences in the responses to the two sets of questions. There is a higher degree of agreement in responses for those who completed high school, with 90% of the 1991 respondents in this category classified the same way in 1992. For those recorded as having 16 years of schooling in 1991, however, only 79% were coded as having a B.A. degree in 1992.

Appendix Table A-1: Tabulations of Educational Attainment Variables,  
1990-1992 Current Population Surveys

Years of Education Completed (Pre-1992)	Highest Grade Attended (Post-1991)	Men			Women		
		1990	1991	1992	1990	1991	1992
0	0	0.3	0.2	0.3	0.2	0.2	0.1
1-4	1-4	0.6	0.7	0.7	0.4	0.4	0.3
5-6	5-6	1.4	1.3	1.3	0.7	0.7	0.7
7-8	7-8	2.5	2.6	2.2	1.5	1.5	1.5
9	9	2.0	1.9	2.1	1.4	1.3	1.4
10	10	3.6	3.4	3.5	3.0	2.7	2.8
11, 12 (Not Completed)	11, 12 No Diploma	4.1	3.8	5.1	3.7	3.5	4.5
12 (Completed)	High School Degree/GED	37.0	36.2	34.5	39.7	39.1	36.3
13-15	Some College (no degree), Associates Degree	22.3	22.3	25.6	25.7	26.1	29.2
16	B.A. Degree	14.6	15.3	16.4	14.2	14.7	16.2
17+	Post-Graduate Degree	11.4	12.2	8.6	9.6	9.9	6.6

Notes: Levels are from tabulations run on data from the Outgoing Rotation Groups of the CPS. 1990, 1991 CPS's surveyed the number of years of school completed. The 1992 CPS surveyed the highest grade completed. These questions can produce different results.

Appendix Table A-2: Tabulations of Educational Attainment Match Rates, 1990/1991 and 1991/1992 Current Population Surveys, Male Respondents

Years of Education Completed (Pre-1992)	Highest Grade Attended (Post-1991)	1990/1991	1991/1992
0	0	100%	71%
1-4	1-4	97	71
5-6	5-6	96	70
7-8	7-8	97	75
9	9	98	58
10	10	97	66
11, 12 (Not Completed)	11, 12 No Diploma	74	66
12 (Completed)	High School Degree/GED	94	90
13-15	Some College (no degree), Associates Degree	95	77
16	B.A. Degree	97	79
17+	Post-Graduate Degree	92	92

Notes: Match rates are for male respondents who were in the fourth month rotation in the earlier year and the eight month rotation of the second year listed. Individuals were matched based on household identification number, age, race and relation to reference person. In both 1990/1991 and 1991/1992, about half of all possible respondents matched. Percentages listed are the match rates of the latter year category with the earlier year category. For example, the number listed for 16 and BA degree is the percent of respondents who responded that they had a BA in 1992, and who also responded they had 16 years of education. (This entry is not the percent of people who had 16 years of education who have a bachelor's degree.)