THE TEACHING PENALTY

Teacher Pay Losing Ground

Sylvia A. Allegretto Sean P. Corcoran Lawrence Mishel

Economic Policy Institute

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ISBN: 1-932066-30-6

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Introduction

Teacher quality is the most important input schools contribute to the academic success of their students (Hanushek and Rivkin 2006; Rice 2003). Yet for many school officials, recruiting and retaining talented and effective classroom teachers remains an uphill battle. For decades now, a small and declining fraction of the most cognitively skilled graduates choose to become teachers (Corcoran, Evans, and Schwab 2004), while rigorous national standards and school-based accountability for student performance have pushed the demand for talented teachers to an all-time high.

Recent efforts to recruit and retain highly skilled teachers have reenergized the debate over teacher compensation. Many continue to ask whether teacher salaries are sufficient to attract the best graduates into teaching (Stronge, Gareis, and Little 2006; Moulthrop, Calegari, and Eggers 2005), while others minimize the importance of base pay and question whether the current *structure* of teacher compensation is optimal for attracting talent into the profession (Solmon and Podgursky 2000; Hoxby and Leigh 2004; Leigh and Mead 2005). Whatever the case, it is clear that sound evidence on the comparability of teacher pay remains critical to our understanding of the link between compensation and teacher quality and those policies that will ensure a cadre of teachers capable of helping students to meet increasingly higher achievement standards.

In an earlier study (Allegretto, Corcoran, and Mishel 2004) we contributed to this evidence by examining recent trends in the relative weekly earnings of elementary and secondary school teachers. In that study we found that the average weekly pay of teachers in 2003 was nearly 14% below that of workers with similar education and work experience (p. 13), a gap only minimally offset by higher non-wage benefits in teaching. Though teacher earnings have fallen below that of the average college graduate in recent decades, we showed that teachers lost considerable ground during the late 1990s, as earnings of college graduates grew 11% relative to 0.8% growth in teaching.

In this new study, we extend our earlier work by updating this analysis of relative teacher earnings through 2006, further disaggregating these t rends by seniority level. Using decennial Census data, we place the recent trend in relative pay into its long-run context, and consider how long-run trends in teacher pay complicate efforts to maintain a constant level of teacher quality.

In our 2004 study, we highlighted some important methodological issues that frequently arise in the analysis of teacher compensation, and we revisit some of those issues here. For example, we explain how researchers can sometimes arrive at contradictory conclusions about relative teacher pay when relying on different sources of data. These differences depend on (1) whether the analyst uses an employer- or

employee-based survey of earnings, and (2) the pay interval (annual, weekly, or hourly) the analyst elects to compare. Often in hourly wage comparisons (Greene and Winters 2007, for example), quite unrealistic assumptions are made regarding teacher work schedules. All of the data available show that teachers work at least as many hours each work week as comparable college graduates.

The major findings of this report are as follows:

- An analysis of trends in weekly earnings shows that public school teachers in 2006 earned 15% lower weekly earnings than comparable workers, a gap 1 percentage point larger than that reported for 2003 in our original study. The teacher disadvantage in weekly earnings relative to comparable workers grew by 13.4 percentage points between 1979 and 2006, with most of the erosion (9.0 percentage points) occurring in the last 10 years (between 1996 and 2006).
- Recent trends represent only a small part of a long-run decline in the relative pay of teachers. Using U.S. Census data we show that the pay gap between female public school teachers and comparably educated women—for whom the labor market dramatically changed over the 1960-2000 period—grew by nearly 28 percentage points, from a relative wage *advantage* of 14.7% in 1960, to a pay *disadvantage* of 13.2% in 2000. Among all public school teachers the relative wage disadvantage grew almost 20 percentage points over the 1960-2000 period.
- An analysis of the weekly earnings of occupations comparable to K-12 teachers confirms the teacher disadvantage in weekly earnings and the substantial erosion of teacher relative pay over the last 10 years. Teachers' weekly wages were nearly on par with those paid in comparable occupations in 1996 but are now 14.3%, or $154, below that of comparable occupations.
- Improvements in the non-wage benefits of K-12 teachers partially offsets these wage differences, such that the weekly *compensation* disadvantage facing teachers in 2006 is about 12%, about 3 percentage points less than the 15% weekly *wage* disadvantage.
- After disaggregating trends in relative compensation through the 1990s by age, nearly all of the increase in the weekly earnings gap between teachers and comparably educated and experienced workers occurred among mid- and senior-level teachers. Early-career teachers (age 25-34) experience roughly the same wage disadvantage today as in 1990 (about 12%).
- Raising teacher compensation is a critical component in any strategy to recruit and retain a higher quality teacher workforce if the goal is to affect the broad array of teachers—that is, move the quality of the median teacher. Policies that solely focus on changing the composition of the current compensation levels, such as merit or pay-for-performance schemes, are unlikely to be effective unless they also correct the teacher compensation disadvantage in the labor market.

- A broad array of analysts from across the political spectrum have found trends comparable to ours—that teachers face an earnings disadvantage, and that this disadvantage has grown over the long run. Only two widely cited analysts seem to disagree with this finding, but the data they examine are inappropriate for this task, as the Bureau of Labor Statistics clearly warns in a statement on its Web site.
- States vary widely in the extent to which public school teachers are paid less than other college graduates. In 15 states, public school teacher weekly wages lag by more than 25%. In contrast, there are only five states where teacher weekly wages are less than 10% behind, and no state where teacher pay is equal or better than that of other college graduates.

CHAPTER 1

The Long View
Trends in Teacher Pay

This chapter sets the stage for an analysis of recent trends in teacher pay by placing this study in the broader context of change in the labor market for teachers. A long-run perspective is essential for understanding the links between relative compensation and the quality of the teaching force, and for recognizing the structural challenges facing schools seeking to attract highly skilled graduates into the profession. This chapter reviews existing evidence on long-run trends in relative teacher compensation, and then turns to the decennial census to provide some estimates of change in relative teacher wages over a 40-year period.

Perhaps like no other profession, the labor market for teachers was profoundly affected by improvements in work opportunities for women during the mid-20th century. Schools had long enjoyed a captive labor pool in academically skilled women who had few career options outside of teaching, nursing, and social work. As labor market opportunities for women improved, however, college-educated women were much more likely to pursue medicine, law, science, and management than to enter a traditionally female-dominated profession (Black and Juhn 2000; Goldin 2006).

Part of the appeal of these new careers was their earnings opportunities. Wage growth in general for college-educated women outpaced that for men for decades, both in the aggregate (as shown below) and within traditionally male-dominated professions (Murphy and Welch 2001; Bacolod 2007). Given the high economic returns possible in the most lucrative of these occupations, one might expect that the most academically talented women would have the most to gain from choosing a non-teaching profession.

Indeed, Corcoran, Evans, and Schwab (2004) document a sharp reduction in the fraction of the highest-achieving female graduates entering the teaching profession since 1960, and Bacolod (2007) explicitly links trends over the 1970-90 period to relative earnings opportunities. In that paper, Bacolod finds using the National Longitudinal Surveys of Young Men, Young Women, and Youth that, where relative earnings outside of teaching increased, both men and women were less likely to make teaching their occupational choice, with the highest aptitude graduates being the most responsive to outside wage opportunities. Bacolod finds, for example, that a 10% increase in

professional earnings reduced the highest scoring (top 25%) graduates' likelihood of teaching by 6.4%.

Evidence on how the earnings of teachers have fared relative to that of other college graduates over the long run is plentiful (see Temin 2002, 2003; Hanushek and Rivkin 1997; Hurley 2003). All of these studies show that female teachers at one time earned significantly more than other female college graduates, and that this pay premium has sharply eroded over time.

For example, Hanushek and Rivkin (2007) examine changes in the fraction of non-teacher college graduates who earned *less* than the average teacher using Census earnings data between 1940 and 2000, and find:

> Over the period, the salaries of all young teachers relative to those of college-educated non-teachers fell, though gender differences were substantial. For men, relative salaries fell between 1940 and 1960 but then remained roughly constant. For women, relative salaries began high—above the median for college-educated women—but fell continuously. The changes are easiest to see for young teachers, but they hold for teachers of all ages, meaning that growth in late-career salaries did not offset the decline in salaries for younger teachers. Among the explanations for the relative salary decline are technological change, expanded opportunities for women, and growth in international trade—all of which increased the demand for and earnings of highly skilled workers outside of teaching. The long decline in teachers' relative earnings has likely led to a drop-off in average teacher quality. (p. 73)

Paul Peterson (2003) refers to the same trend in falling relative earnings for teachers in diminishing the power of teacher unions:

> Yet for all this political influence, teacher pay, relative to that of other occupations, has been slipping downward for decades. In 1940, female teachers made better than 60 percent of what was earned by the average college-educated woman; by 1990, they were earning hardly 40 percent. Among males, salaries slipped from 52 to 33 percent of the college-educated average.

Table 1 provides similar estimates of long-run changes in relative teacher earnings using individual-level data from the U.S. Census Bureau (Public-Use Microdata Samples, or PUMS) from 1960 to 2000. This table compares the annual earnings of public school teachers with those of similarly educated workers for each Census year using a regression approach that controls for years of education and work experience.[1] This sample of workers is limited to those aged 25 to 55, working full time (30 hours or more) for the majority of the year (at least 27 weeks).[2]

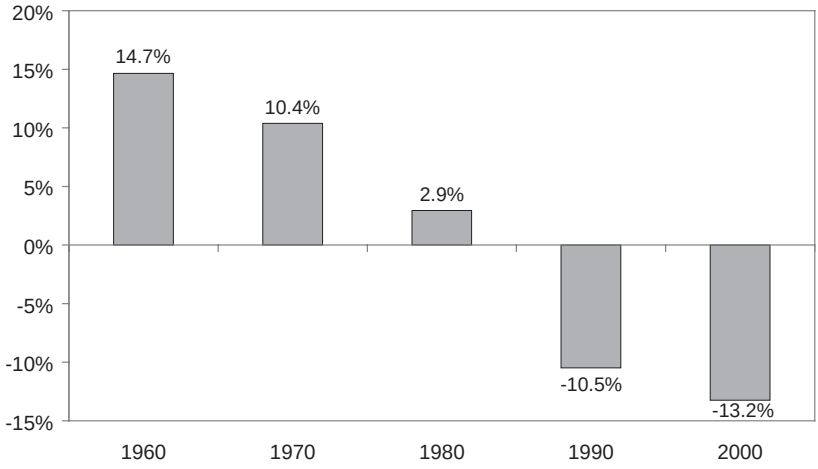
Each calculation in Table 1 is an estimate of the percentage difference in annual earnings between the average public elementary or secondary school teacher and a worker with similar education and work experience. For each Census year, we calculate

Table 1 Regression-adjusted annual wage premium of public school teachers, 1960 - 2000

	All	Women	Men
Teacher wage differential			
1960	0.9%	14.7%	-20.5%
1970	-2.5	10.4	-22.6
1980	-8.8	2.9	-25.9
1990	-16.5	-10.5	-28.0
2000	-19.0	-13.2	-31.2
Percentage-point change			
1960-2000	-19.9	-27.9	-10.8
1980-2000	-10.2	-16.2	-5.4

Source: Authors' analysis using the decennial Census Public-Use Microdata Samples.

Figure A Annual wage premium of female public school teachers, 1960-2000



Source: Authors' analysis using the decennial Census Public-Use Microdata Samples.

this difference for all workers, and by gender. In 1960, annual earnings for female teachers were 14.7% *higher* than that of similarly educated women, while male teacher earnings fell 20.5% below that of similarly educated men (taken together, teachers were comparably paid).

As others have shown, we also find dramatic erosion in relative teacher earnings since 1960. **Figure A** illustrates these changes for female teachers, beginning with a relative pay *advantage* of 14.7% in 1960 and falling to a similarly sized wage *disadvantage* of 13.2% in 2000. Altogether, the annual pay differential between female teachers and female non-teachers has shifted almost 27.9 percentage points over a 40-year period. Male teachers—while always experiencing a negative earnings differential—also experienced a growing pay gap between 1960 and 2000, but to a lesser extent than women (10.8 percentage points). Combining male and female teachers, the overall pay gap grew nearly 20 percentage points over these 40 years.[3] With this steep erosion of relative pay it is not surprising that analysts such as Hanushek and Rivkin, Temin, and others have presumed that there is a likely link between relative wage declines and a "a drop-off in average teacher quality" (Hanushek and Rivkin 2007, 73).

CHAPTER 2

Recent Trends in the Relative Earnings of Teachers

This chapter turns from a long-run view to an examination of weekly earnings disparities and trends from 1979 to the present, with a particular focus on trends since the 1990s. As shown in Allegretto, Corcoran, and Mishel (2004), the late 1990s was a period in which teachers saw no improvement in their inflation-adjusted wages while other college-educated workers saw significant gains. This section updates those estimates through 2006. As in our earlier report, we use weekly wage reports from the Current Population Survey as the primary source of data, and carefully adjust for differences in worker education levels, experience, region, and other relevant differences. This chapter presents separate estimates by highest level of education, examining workers whose highest degree is a B.A. and those with an M.A. or higher. The following section begins by reviewing our data and methodology, noting a few key measurement issues that will be relevant for this work.

Data

This chapter presents trends in the relative weekly earnings of teachers since 1979, focusing in particular on trends since 1996. In doing so, we draw from the Current Population Survey (CPS) of the Bureau of Labor Statistics, specifically the "Outgoing Rotation Groups" sample, or CPS-ORG. The CPS is the monthly survey administered by the BLS to more than 60,000 households to measure and report on unemployment. The CPS-ORG data used here are based on reports from nearly 145,000 workers each year (see the Wage Appendix in Mishel et al. (2006) for details on the development of these data). The CPS-ORG, in addition to the March CPS supplement that collects data on annual earnings, represent the data most extensively used by economists to study wages and employment. The CPS-ORG data are particularly useful due to their large sample and information on weekly wages.[4]

Since 1994, the CPS-ORG survey (see *BLS Description...* on p. 10) asks respondents to report their wages on a weekly, biweekly, monthly or annual basis (whichever the respondent finds most appropriate) from which the BLS then derives the weekly

BLS DESCRIPTION OF WEEKLY WAGE MEASUREMENT IN THE CPS

Prior to 1994, respondents were asked how much they usually earned per week. Since January 1994, respondents have been asked to identify the easiest way for them to report earnings (hourly, weekly, biweekly, twice monthly, monthly, annually, other) and how much they usually earn in the reported time period. Earnings reported on a basis other than weekly are converted to a weekly equivalent. The term "usual" is as perceived by the respondent. If the respondent asks for a definition of usual, interviewers are instructed to define the term as more than half the weeks worked during the past four or five months. (www.bls.gov//opub/mlr/2005/05/art1full.pdf)

wage. More than half of teachers report an annual (as opposed to a monthly, bi-weekly, or weekly) wage to BLS. Respondents also report the hours they worked last week.

This analysis restricts the sample to all full-time workers between the age of 18 and 64 (defining "full time" as working at least 35 hours per week). Teachers are identified using detailed Census occupation codes, and include only elementary and secondary teachers (pre-kindergarten and kindergarten teachers, adult educators, and special education teachers are excluded). This analysis also only focuses on *public* school teachers (private school teachers—who on average earn less than public school teachers—are excluded).

There are several measurement issues that require some further discussion. First, as in our earlier report, we limit our analysis to workers whose wage data was provided by the respondent and not "imputed," or assigned by the Census Bureau, who imputes earnings for the BLS. Second, we justify our choice of comparing *weekly*, as opposed to annual or hourly earnings. Finally, we address concerns that have been raised that the CPS weekly wage measure understates teacher pay due to the non-traditional teacher work year.

Imputed wages

An important innovation of our 2004 report was the choice to restrict the sample to workers with earnings that have not been "imputed." Imputed wages are those that have been inferred, or assigned, by the BLS in cases where the respondent fails to report his or her earnings (Hirsch and Schumacher 2004). In the BLS imputation procedure, earnings are estimated for non-respondents through a "hot deck" method employed by the Census Bureau. This method finds a respondent or "donor" in the survey that closely matches the non-respondent on characteristics such as location, age, race, and education. The problem arises here because occupation is not necessarily one of the criteria used in imputing earnings—non-responding teachers are more often than not assigned the average earnings of non-teacher college graduates. Given differences in the earnings

and work year of teachers and non-teachers, this procedure creates a *systemic* bias in the comparison of teacher earnings with that of other professionals as imputed teacher earnings are systemically overstated. Furthermore, this bias is *growing over time*, as the share of all observations with imputed wages has risen sharply over the last 10 years.

In most years, the BLS flags observations where earnings have been imputed. Unfortunately for this analysis, the CPS records for 1994 and 1995 are not coded in a way that allows identification of which observations have imputed earnings. As a result, estimates for those years are not provided in this analysis. Instead, to construct historical trends, estimates using annual data for the period 1993 to 1996 are substituted. (Additional details related to wage imputation are provided in Appendix A.)

The use of weekly versus annual or hourly wages

This analysis of the relative wage of teachers relies on comparisons of weekly earnings, rather than annual or hourly earnings, the approach taken by some authors (e.g., Hanushek and Rivkin 1997; Greene and Winters 2007). We elect to use weekly wages to avoid measurement issues regarding differences in annual weeks worked (teachers' traditional "summers off") and the number of hours worked per week that arise in many studies of teacher pay. Later in the chapter we benchmark our findings on relative weekly pay to those found in the annual earnings data (we also used annual earnings in our analysis of Census data in Chapter 1).

It is often noted that the annual earnings of teachers cannot be directly compared to that of non-teachers, given that teachers are typically only contracted to work a nine-month year. But differences arise over exactly how much time teachers devote to their position outside of their nine contracted months of teaching. Teachers spend some of their additional three months in class preparation, professional development, or other activities expected of a professional teacher. Teachers who may wish to earn additional income during the summer months can often do so, but are unlikely to be able to earn at the same rate of pay as in their teaching role.

Similarly, attempts to compare the *hourly* pay of teachers and other professionals have resulted in considerable controversy. As Podgursky has noted: "comparing the hourly pay of teachers and non-teachers just sets off an unproductive debate about the number of hours teachers work at home versus other professionals."[5] In addition to our comparisons of weekly earnings, we have compared the relative hourly pay of teachers using CPS data on hourly wages and find no differences in our results.

Such decisions regarding pay interval (weekly, annual, or hourly) become mostly irrelevant when considering *changes* in relative pay over time. Changes in relative wages can be expected to be similar as long as the relative work time (between teachers and comparable professionals) remains constant. For example, if the ratio of weekly hours worked by teachers relative to those worked by comparable workers remains constant over time, then estimates of changes in hourly wages will be the same as for weekly changes. Similarly, estimated changes in relative annual earnings will parallel those for

Table 2 Weekly wages of all workers, public school teachers, and non-teacher college graduates, 1996-2006

	All		Women		Men		Ratio of teacher to other college graduate weekly wages		
	Public teachers	*Non-teacher college graduates*	*Public teachers*	*Non-teacher college graduates*	*Public teachers*	*Non-teacher college graduates*	***All****	***Women***	***Men***
All college graduates									
Weekly wage ($2006)							Ratio		
1996	$948	$1,098	$917	$922	$1,030	$1,221	96%	99%	84.4%
2001	947	1,249	930	1,038	990	1,401	86	90	70.7
2006	935	1,240	918	1,051	979	1,386	84	87	70.6
Percent change							Percentage-point change		
1996-2001	-0.1%	13.7%	1.5%	12.5%	-3.8%	14.7%	-10.5	-9.8	-13.7
2001-06	-1.3	-0.7	-1.3	1.3	-1.2	-1.1	-1.8	-2.3	-0.1
1996-2006	-1.4	12.9	0.1	13.9	-5.0	13.5	-12.4	-12.0	-13.7
Bachelor's degree only									
Weekly wage ($2006)							Ratio		
1996	833	993	811	831	898	1,113	94%	98%	81%
2001	844	1,139	832	947	874	1,285	84	88	68
2006	824	1,122	813	951	855	1,258	82	85	68
Percent change							Percentage-point change		
1996-2001	1.3%	14.7%	2.7%	13.9%	-2.7%	15.4%	-10.2	-9.6	-12.6
2001-06	-2.4	-1.5	-2.3	0.5	-2.2	-2.1	-2.0	-2.4	-0.1
1996-2006	-1.1	13.0	0.3	14.4	-4.8	13.0	-12.2	-12.0	-12.7

Master's degree									
Weekly wage ($2006)							**Ratio**		
1996	$1,105	$1,244	$1,079	$1,081	$1,163	$1,360	97%	100%	86%
2001	1,091	1,384	1,076	1,166	1,134	1,543	88	92	73
2006	1,062	1,379	1,051	1,175	1,092	1,547	86	89	71
Percent change							**Percentage-point change**		
1996-2001	-1.2%	11.3%	-0.3%	7.8%	-2.5%	13.4%	-8.5	-7.6	-12.0
2001-06	-2.7	-0.4	-2.3	0.8	-3.7	0.2	-2.8	-2.8	-2.9
1996-2006	-3.9	10.9	-2.6	8.7	-6.1	13.	-11.3	-10.4	-14.9

* Computed with gender ratios among teachers at 80% women, 20% men.

SOURCE: Authors' analysis of CPS data.

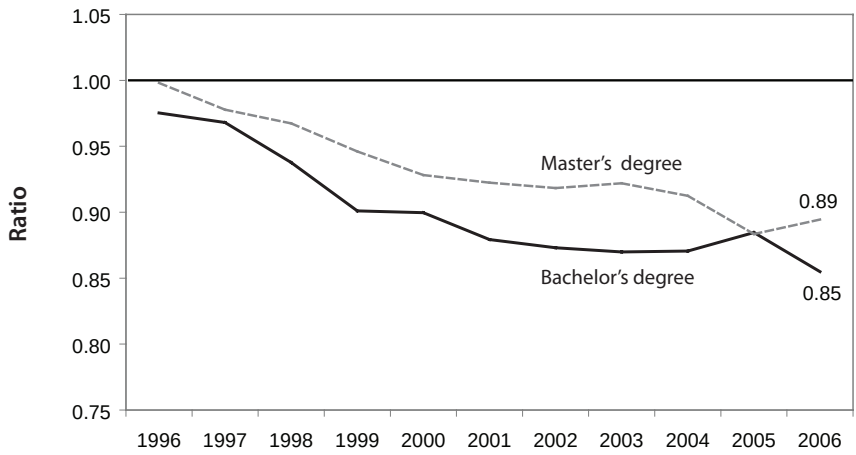
weekly earnings as long as the annual weeks and hours worked by teachers have not changed relative to those of comparable workers.

Recent trends in relative weekly wages

We begin our analysis of recent trends in relative teacher pay with a simple comparison of weekly earnings trends for public school teachers and those of other college graduates. The following section provides "regression-adjusted" estimates of relative teacher wages that control for differences in education, experience, region, and other relevant factors. We also differentiate our results by education level, although our unadjusted raw differences tell essentially the same story as the more refined, disaggregated results. We take our analysis back to 1979, which is the first year for which we have CPS-ORG weekly wage data, but concentrate on the most recent 10-year period from 1996 to 2006.

The basic weekly wage trends for public school teachers and non-teacher college graduates between 1996 and 2006 are summarized in **Table 2,** with the results provided separately for all college graduates, those with a bachelor's degree only, and those with a masters' degree (the entire weekly wage series is presented in Appendix B). These data provide a first cut at the teacher wage disadvantage in 2006, showing that teachers earned 16% less per week than other college graduates, with the disadvantage being 13% among women and 29% among men. The teacher wage disadvantage is somewhat larger among those with terminal bachelor's degrees (18%) than for those with master's degrees (14%). As we will show later, this simple method of computing the teacher

Figure B Teacher/non-teacher weekly wage ratios, women, 1996-2006



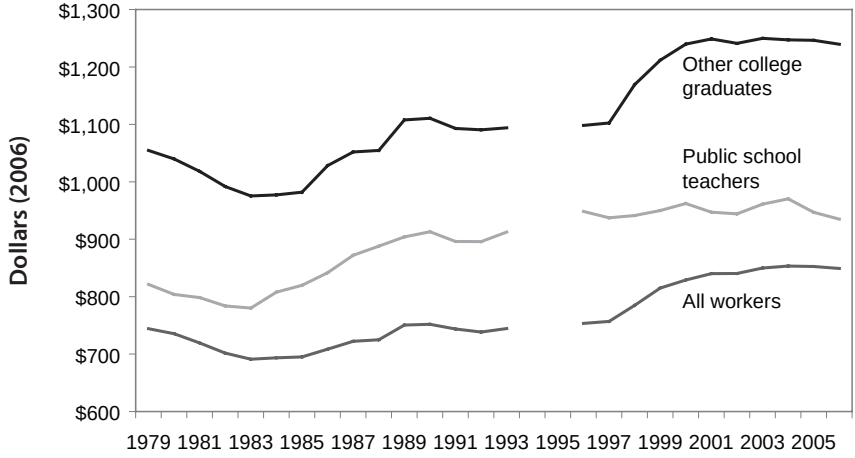
SOURCE: Authors' analysis of CPS data.

wage disadvantage—simple comparisons without adjusting for worker characteristics—provides a very similar answer to that of more sophisticated methods.[6] We also provide a state-by-state breakdown of teacher wages and wages for other college graduates, disaggregated by level of degree, in Appendix B. States vary widely in the extent to which public school teachers are paid less than other college graduates. In 15 states, public school teacher weekly wages lag by more than 25%. In contrast, there are only five states where teacher weekly wages lag by less than 10%, and no state where teacher pay is equal or better than that of other college graduates.

These data also allow an examination of how teachers' wages have fared relative to other college graduates over the last 10 years. The basic story is simple. Weekly wages of public school teachers have barely kept up with or have fallen slightly behind inflation since 1996. This is true for teachers of all education levels and of either gender. By contrast, non-teacher college graduates saw a remarkable 13.7% gain in their inflation-adjusted wages between 1996 and 2001. After 2001, wage growth was unfavorable for teachers and non-teachers alike, though teachers (particularly women) lost ground relative to other college graduates in this period as well.

The basic trend is aptly captured in **Figure B**, which presents the ratio of teacher to non-teacher weekly wages among women college graduates (who comprise 80% of all teachers). The ratio among those with bachelor's degrees fell from 97.5% in 1996 to 85.0% in 2006, a 12.5 percentage point decline in the (unadjusted) wage gap. Female teachers with master's degrees enjoyed wage parity with similarly educated women in 1996, but by

Figure C Real weekly wage trends for teachers and others, 1979-2006



NOTE: Data are missing for 1994 and 1995 due to the lack of imputed wage identifiers.
SOURCE: Authors' analysis of CPS data.

2006 were earning 11% less than other women with MA's. The weekly wages of male teachers also fell behind those of comparably educated men. In 1996, the wage ratio of male teachers to male college graduates was 84.4%; by 2006 this had fallen 13.8 percentage points to 70.6%.

Weekly wage trends from 1979 to 2006 are illustrated in **Figures C, D,** and **E**. Figure C presents wage trends for all workers, public school teachers, and other college graduates (as discussed above, data are missing for 1994 and 1995 due to the lack of imputed wage identifiers). Figures D and E restrict the comparison to teachers and college graduates with bachelor's and master's degrees, respectively, and focus on the 1996-2006 period.[7]

Regression-adjusted estimates of teacher relative wage

The next stage in this analysis is to estimate regression-adjusted teacher relative wages, in order to account for any changes in the composition of the workforce and among college graduates over time. This simple model uses the natural logarithm of weekly wages as the dependent variable, with controls for education (six education categories or five education-level dummy variables), age as a quartic, marital status, region, race, and ethnicity. The coefficient on an additional teacher indicator variable provides an estimate of the relative teacher wage that controls for these other worker characteristics.[8] Note that this analysis assumes teachers and other college graduates have the same returns to education and experience, an assumption relaxed in other regression specifications below.

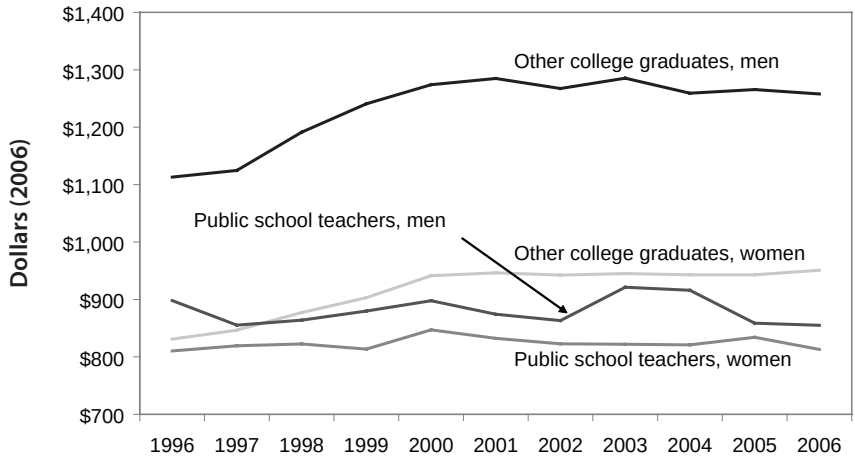
This regression model has been modified somewhat from that used in our earlier study (Allegretto, Corcoran, and Mishel 2004). First, only *public* school teachers are used to identify the relative earnings of teachers. One criticism of our earlier work (Podgursky and Tongrut 2006) was that an estimate of relative teacher earnings using all teachers (public and private) overstates the wage disadvantage faced by public school teachers, as private school teachers generally earn lower wages than public school teachers.[9]

Second, we have incorporated a more detailed list of education level controls. In our earlier study, we employed only four categories (college or higher, some college, high school, and less than high school). Because teachers are more likely to hold a master's degree than other college graduates (Larsen 2006), we include separate identifiers for those with a bachelor's degree alone, those with a master's degree, and those with education beyond a master's degree (i.e., doctorate or professional degree).

The net effect of these two changes is negligible, as the higher estimated relative wage that results from focusing on public school teachers is offset by the effect of additional education-level controls. The *trend* in relative teacher wages is, if anything, a bit stronger in this modified specification, confirming that our earlier analysis did not overstate the erosion in relative teacher earnings. (Additional details on this specification change and its effects can be found in Appendix A.)

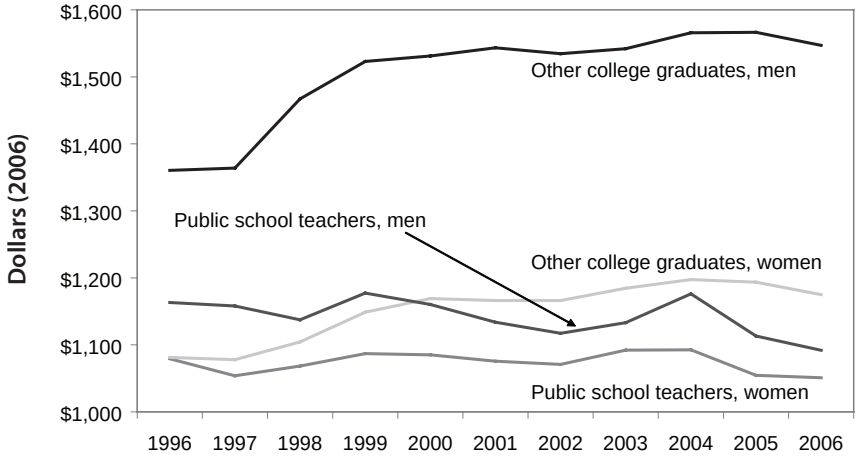
The regression-adjusted estimates of relative teacher wages from the CPS-ORG are presented in **Table 3** and **Figure F**, with estimates presented separately for all teachers

Figure D Weekly wages of public school teachers and non-teacher college graduates with BA, 1996-2006



SOURCE: Authors' analysis of CPS data.

Figure E Weekly wages of public school teachers and non-teacher college graduates with MA, 1996-2006



SOURCE: Authors' analysis of CPS data.

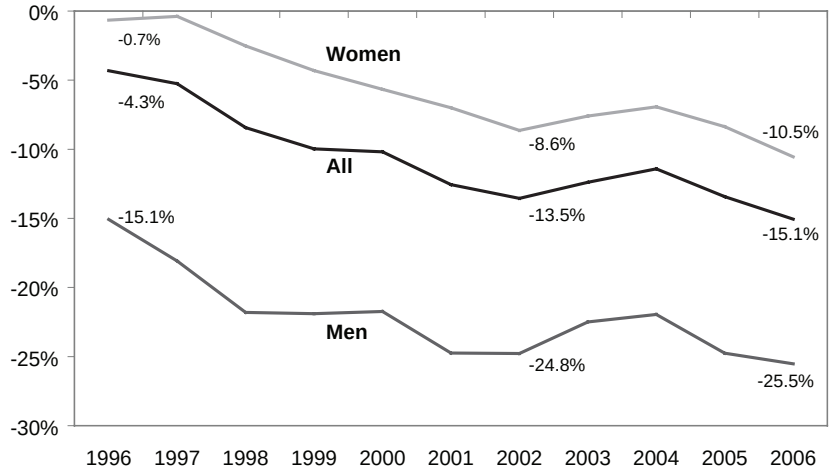
Table 3 Regression-adjusted weekly wage penalty for teachers, 1996-2006

	All	Women	Men
1996	-4.3%	-0.7%	-15.1%
1997	-5.3	-0.4	-18.1
1998	-8.4	-2.5	-21.8
1999	-10.0	-4.3	-21.9
2000	-10.2	-5.7	-21.7
2001	-12.6	-7.0	-24.7
2002	-13.5	-8.6	-24.8
2003	-12.4	-7.6	-22.5
2004	-11.4	-6.9	-22.0
2005	-13.4	-8.4	-24.8
2006	-15.1	-10.5	-25.5
Percentage-point changes, 1979-2006			
1979-93 **	-1.7	-5.5	3.7
1993-96*	-1.0	-4.2	-2.0
1996-2006	-10.7	-9.9	-10.4
1979-2006	-13.4	-19.6	-8.8
Percentage-point changes in annual wage estimates, 1979-2005*			
1979-93	-1.7	-4.5	0.2
1993-96	-1.0	-4.2	-2.0
1996-2005	-9.2	-7.7	-7.1
1979-2005	-11.2	-15.8	-8.0

* Estimated using the March Current Population Survey.
** Estimated for public school teachers with four education controls.

SOURCE: Authors' analysis of Current Population Survey ORG and March data.

Figure F Public school teacher wage premium, by gender, 1996-2006



SOURCE: Authors' analysis of CPS data, regression adjusted.

and for teachers by gender. The regression approach suggests around a 10 percentage point erosion of the teacher relative-wage since 1996, whether one looks at all teachers together or strictly at male or female teachers. This estimate is somewhat smaller than that using unadjusted wage ratios, where relative wages fell about 12.4 percentage points (see Table 2).

Table 3 also presents regression-adjusted estimates of the change in relative teacher wages from 1979 to 1993.[10] As discussed earlier, we do not have usable CPS-ORG data for 1994 and 1995. In its place we have estimated similar regression functions using *annual* March CPS data from 1993 to 1996.[11] For female teachers, the 9.9 percentage point decline in relative wages between 1996 and 2006 is in addition to a 4.2 percentage point erosion between 1993 and 1996, and a 5.5 percentage point erosion in the earlier 1979 to 1993 period. Taken together, the cumulative erosion in the earnings of female teachers relative to women of comparable education and experience since 1979 is 19.6 percentage points. In contrast, male teachers saw their relative wages improve slightly over the 1979-93 period, falling only 2.0 percentage points between 1993 and 1996. The cumulative erosion for male teachers since 1979 has been 8.8 percentage points.[12]

These estimates are benchmarked by comparing them to estimates using annual wage data for the entire 1979 to 2005 period (the latest data are for 2005), as shown in the bottom panel of Table 3. The data used for these estimates, the annual wage data in the March CPS, are the same data relied upon by Temin (2002, 2003), Hanushek and Rivkin (1997, 2007), and Podgursky and Tongrut (2006), and thus should not be subject

to the concern about teacher underreporting of weekly wages or other concerns specific to the CPS-ORG. The estimates with annual wage data confirm the findings based on weekly wage data: there has been a substantial erosion of teacher wages relative to that of comparable workers over the last 10 years or so and over the longer period since 1979. The magnitudes of the erosion of relative teacher pay using weekly and annual wage data differ, but they tell the same general story. A comparison of trends in annual earnings in the March CPS with an analysis of trends in the decennial census (1980 to 2000) confirms this pattern (see Table 1). Census data show at least as great an erosion in relative wages. Taken together, this finding of large erosion in relative teacher pay over the past 10 years (and since 1979) is not dependent on our choice of the CPS-ORG as the primary data source.

So far we have examined the *changes* in the teacher relative wage, both over the longer term since 1960 and with greater detail since 1979. We now turn to regression-adjusted estimates of the *magnitude* of the teacher wage disadvantage in 2006. The estimates presented in Table 3 indicate that in 2006, teachers had 15.1% lower weekly wages than workers with similar characteristics. Male teachers earned 25.5% less than comparable male workers, while female teachers earned 10.5% less. These estimates of teacher wage disadvantage are remarkably close to what was calculated with a simple ratio of teacher to non-teacher college graduate weekly wages, provided in Table 2.[13]

An issue that frequently arises when discussing relative teacher compensation is whether teachers receive better benefits that offset their lower wages. The answer is "a bit," with an overall (wages plus benefits) compensation disadvantage perhaps two percentage points less than the wage disadvantage. This is explored in Chapter 4, which finds that teachers do have somewhat better benefits but not as much as critics claim. Furthermore, the scale of benefits is far too small—only 20% of total compensation—to offset a 15% wage disadvantage.

Digging deeper: Relative wages by age

The erosion in relative teacher pay documented above may ultimately affect teacher quality through its effects on recruitment and retention. We can obtain more insight into the likely effects of declining teacher earnings by examining changes in earnings by age or experience level. This section examines relative teacher wages by age using three age categories: "young" (age 25-34), "middle" (age 35-44), and "senior" (age 45-54).[14] The results are presented in Table 4.

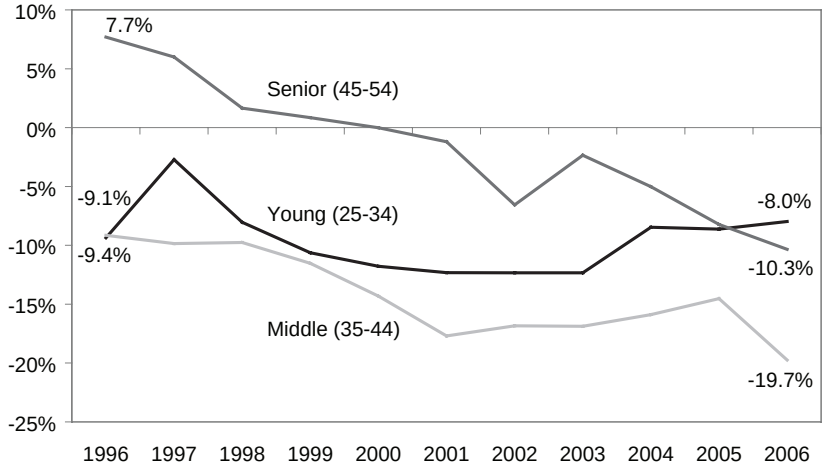
This analysis reveals that younger teachers (age 25-34) earned 11.5% less than comparably educated younger workers in 1996, a wage gap that remained the same in 2006. In fact, the relative wage disadvantage among younger female teachers diminished some over this period (falling from a 9.4% gap to 8.0%). **Table 4** and **Figure G** show that the erosion of relative teacher earnings has fallen most heavily on experienced teachers, age 45 to 54. For instance, senior teachers had a slight 1.9% wage premium over comparable workers in 1996, but by 2006 they earned 15.4% less than comparable workers, an erosion of 17.3 percentage point. The erosion from 1996 to 2006 among

Table 4 Change in teacher weekly wage premium by age and gender, 1996-2006

	All teachers			Women teachers			Men teachers		
	Young	*Middle*	*Senior*	*Young*	*Middle*	*Senior*	*Young*	*Middle*	*Senior*
	25-34	35-44	45-54	25-34	35-44	45-54	25-34	35-44	45-54
Teacher relative wage									
1996	-11.5%	-10.5%	1.9%	-9.4%	-9.1%	7.7%	-19.8%	-19.2%	-11.6%
1997	-5.8	-13.4	-0.5	-2.7	-9.9	6.0	-18.8	-23.4	-15.2
1998	-12.5	-15.1	-3.3	-8.0	-9.8	1.6	-23.8	-28.6	-15.9
1999	-15.0	-16.9	-4.8	-10.6	-11.5	0.8	-25.7	-28.6	-17.5
2000	-14.9	-18.4	-3.7	-11.8	-14.3	0.0	-24.2	-30.2	-14.8
2001	-17.1	-21.7	-8.0	-12.3	-17.7	-1.2	-27.1	-31.0	-23.9
2002	-16.4	-21.3	-11.1	-12 3	-16.8	-6.6	-25.1	-33.1	-22 7
2003	-14.5	-21.5	-7.9	-12 3	-16.9	-2.3	-19.5	-31.2	-21.1
2004	-12.0	-19.2	-9.3	-8 5	-15.9	-5.0	-22.4	-26.4	-19.9
2005	-13.2	-19.0	-12.9	-8.6	-14.5	-8.2	-23.3	-29.1	-24.0
2006	-12.0	-23.8	-15.4	-8.0	-19.7	-10.3	-21.9	-32.0	-29.0
Percentage-point change									
1996-2006	-0.5	-13.3	-17.3	1.4	-10.6	-18.0	-2.1	-12.9	-17.4

SOURCE: Authors' analysis of CPS data, regression adjusted.

Figure G Female teacher weekly wage premium by age range, 1996-2006



SOURCE: Authors' analysis of CPS data, regression adjusted.

middle teachers (age 35 to 44) was less, but still considerable at 13.3 percentage points. The same pattern prevailed among both men and women.

These results suggest that trends in relative teacher earnings over the last 10 years may not have had a substantial impact on the recruitment of new teachers, though recruitment must still overcome the 12% wage gap facing young teachers. However, the doubling of the wage gap teachers experience as they age, from their younger years (25-34) to mid-career (35-44), suggests that retention may have become more difficult. The erosion of pay for mid-career and more-senior teachers might also affect teacher recruitment to the extent that potential teachers evaluate their lifetime earnings as teachers relative to that of other professions.

Are weekly earnings understated in the CPS-ORG?

Critics of the CPS-ORG (e.g., Podgursky and Tongrut 2006) argue that the use of weekly wage data to compare teachers with other workers biases teacher earnings downward, claiming that teachers report a weekly wage that is actually an annual salary divided over a full year rather than the partial year they actually work. To be sure, the measurement of relative teacher pay is bedeviled by the unique work schedule of teachers over the course of the year (e.g., "summers off" and school holidays) as well as the difficult distinction between "hours spent teaching" and actual work hours. These critics often prefer the annual March CPS or the employer-based National Compensation Survey (NCS). But the latter of these is heavily plagued by these measurement

issues. As we discuss in Chapter 5, weekly and hourly wage measures from the NCS are inappropriate for comparing teachers to other workers, an appraisal echoed by the BLS in its clear warning not to use these data for this purpose.

This section takes a closer look at the CPS-ORG weekly wage data, benchmarking *trends* in these data with those in data widely used by other analysts. We also benchmark trends to the widely used decennial census data on annual earnings.

It is useful to separate the discussion of bias into that related to *levels* versus *trends*. In this case there are two issues. The question of bias in levels asks whether our data appropriately measure the degree to which teachers earn less than comparable workers in a given year (a *level* comparison). The second is how relative teacher pay has changed over time (a comparison of *trends*). This distinction is important, as a measure can be biased in terms of levels (a thermometer, say, may be off by two degrees) but could still provide accurate information on trends (how much the temperature rose may be accurately discerned with either a precise or a consistently biased thermometer).

Benchmarking trends

We first compare trends across surveys in the relative weekly earnings of teachers, because the findings on the erosion of teacher pay are our most salient results.

Table 5 Change in teacher relative wage, alternative surveys, 1979-2005

	All	Women	Men
Percentage-point change, 1979-99			
March	-5.1	-10.8	-1.2
PUMS	-10.2	-16.2	-5.4
CPS-ORG	-8.4	-13.3	-5.1
Percentage-point change, 1999-2005			
March	-6.1	-5.0	-6.8
PUMS	n.a.	n.a.	n.a.
CPS-ORG	-3.5	-4.1	-2.9
Percentage-point change, 1979-2005			
March	-11.2	-15.8	-8.0
PUMS	n.a.	n.a.	n.a.
CPS-ORG	-11.8	-17.4	-8.0

SOURCE: Authors' analysis of March CPS, CPS-ORG, and Census PUMS data.

Specifically, we compare changes in relative teacher wages across three different data sources—the CPS-ORG (which reports weekly earnings) and the March CPS and decennial census (which report annual earnings).[14] The decennial census data was described in Chapter 1 and the CPS-ORG data and March annual wage data were described above in the discussion of Table 3. The same regression specifications were employed with the CPS-ORG and March CPS data and a comparable specification was used for the decennial census analysis (to be consistent in the 1960 and in the 2000 data required fewer education controls and some adjustments for race/ethnicity controls).

Census and March CPS data allow for a long-run comparison with the CPS-ORG from 1979 to 1999 (each Census year 1980 to 2000 reports earnings data from the previous year), though the Census is only available once each decade. The March CPS, on the other hand, is available annually, so these data are used to compare trends over the more recent 1999-2005 period. The same regression specification is used to measure relative teacher pay across each survey.

Table 5 reveals that CPS-ORG weekly wages show a deterioration of teacher relative earnings quite in line with the other surveys over the 1979-99 period. This conclusion holds whether looking at teachers as a whole or separately by gender. Specifically, relative teacher earnings fell 13.3 percentage points among women using the CPS-ORG data, which is somewhat below that found using census annual wage data (a 16.2 percentage-point decline) and somewhat above that found in March CPS annual wage data (a 10.8 percentage-point decline).

Over the 1999 to 2005 period, annual wage estimates from the March CPS indicate a larger erosion of relative teacher wages among women (5.0 percentage points versus 4.1 percentage points in the CPS-ORG) and among teachers as a whole (6.1 versus 3.5 percentage points). Table 5 shows that between 1979 and 2005, the longest comparison possible with these two surveys, an analysis of the March CPS annual wage data yields almost identical results to the CPS-ORG weekly wage data. These comparisons suggest that our findings with the CPS-ORG data are robust across surveys, leaving little doubt that there has been deterioration in the relative earnings of teachers, especially over the past decade and since 1979.

Benchmarking levels

Our analyses comparing teacher earnings to that of similar workers also indicates a significant disadvantage in the *level* of teacher relative pay. As discussed above, if teachers report weekly pay in the CPS-ORG as their annual salary divided over an entire year of work (52 weeks), then the weekly wages of teachers will be understated and the pay disadvantage overstated.

Putting aside the issue of whether teachers indeed have summers "off" and do not devote this time to their profession, this measurement issue is a reasonable concern. We do believe that this type of understatement existed to a greater extent prior to the CPS redesign in 1994 when the survey inquired only about weekly wages earned "last week." However, as discussed above the new survey question asks respondents to

provide their wages in any interval that is easiest for them. We calculate that slightly more than half of all teachers in the CPS now provide an *annual* wage for this question; the BLS then computes a weekly wage using reported weeks worked.

The change in the CPS survey question on earnings appears to have resulted in a significantly *higher* weekly wage among teachers, as teacher wages rose 10.2% between 1993 and 1994 (the year the redesigned survey was first used)—far faster than the 2.2% increase among non-teacher college graduates. The additional 8% wage growth among teachers appears to represent the effects of a correction for the underlying bias in the pre-1994 survey. Consequently, the estimates below incorporate the pre-1994 data in a way that does not allow this bias to be built into these results.[15]

It appears, then, that the survey redesign diminished a considerable bias in the measured level of teacher earnings prior to 1994: in fact, the correction is over half the size of the maximum possible bias—the case where every teacher reported their weekly wages as their annual wage divided by 52 weeks.[16] However, the new survey question may not have fully corrected the upward bias of teacher earnings. We examined this issue by benchmarking the relative weekly pay of teachers in the CPS-ORG files against annual wages in the March CPS (see Appendix A on Wage Measurement). The result is that the smaller pay gap in weekly earnings versus annual earnings—76.7% versus 63.1%—corresponds to a reasonable estimate of the earnings impact of a shorter work year (i.e., the effect of "summers off"). This exercise provides confidence in our estimates of the level of the teacher wage disadvantage: we show that the underlying data result in the same relative comparison of teacher to non-teacher, whether examining the March CPS annual wage data or the CPS-ORG weekly wage data—the difference reflects "summers off" for teachers. Thus, to the extent that confidence is extended to estimates based on the March CPS annual wage data, that confidence also should be placed on estimates from the CPS-ORG weekly wage data we employ.

Podgursky and Tongrut (2006) argue that estimates based on the March annual wage data are inconsistent with those based on the CPS-ORG weekly wage data, which they suggest indicates a severe bias in the weekly wage data. Specifically, estimates with annual wage data should, according to them, be larger than those with weekly data because they reflect a gap in weeks worked (summers off). In fact, our own estimates confirm this, with regression-adjusted estimates of the teacher wage disadvantage using the March annual wage data for 2005 at 19.4% while that based on the weekly wage data being 13.4%: the gap of just 6 percentage points is much less than what one would expect for the annual data reflecting the wage disadvantage as well as "summers off." However, we show in Appendix A that this small difference is an artifact of the regressions comparing wages measured in natural logs, which shrinks the gap by 6 percentage points. Thus, the seemingly small gap does not reflect any bias in the underlying data.

CHAPTER 3

The Earnings of Teachers Relative to "Comparable" Occupations

Chapter 2 compared the weekly pay of public school teachers to other college graduates, in some cases differentiated by gender, age, or level of education. This was accomplished using two methods: simple weekly wage comparisons and regression-adjusted comparisons that control for worker characteristics such as experience, region, and race. However, teacher salaries are frequently compared directly with those of specific professions thought to be "comparable" to teaching (this is particularly useful for longer time series or for comparisons at the local or state level).[17] Unfortunately, these professions are typically chosen based on limited data availability or are chosen arbitrarily without reference to any selection criteria.[18] One innovation of our earlier study (Allegretto, Corcoran, and Mishel 2004) was to systematically and empirically identify professions that represent "proper" comparison groups to the teaching profession. This was done using occupational "skill level" data from the Bureau of Labor Statistics National Compensation Survey (NCS) to identify professions that are similar to teaching in terms of specific skills used on the job. Our earlier study identified 16 professions that were "comparable" to teaching, based both on their raw skill requirements and upon the market valuation of these skills, and then compared their weekly wage levels and trends to those of teachers.[19]

This section updates that earlier analysis as a complement to the analyses in Chapter 2. We briefly review the methodology used to identify comparable occupations and refer readers to the earlier study for more details. As explained below, because of a recent change in occupational coding, we have had to modify our original list somewhat. We describe this change and the results.

As part of the National Compensation Survey, the BLS collects specific occupational skill information (via field visits to establishments employing roughly 84 million workers) for a sample of occupations within each surveyed establishment. Each occupation studied in an establishment is rated for the level of skill required along 10 different dimensions (or "generic leveling factors," as the BLS refers to them) such as "knowledge" or "complexity." These skill ratings can explain 75% of the variance in wages across occupations (Pierce 1999).

Table 6 Teacher weekly wages relative to comparable occupations

			Teachers to comparables	
	Revised comparables	Teachers	Revised	Prior
1983	$429	$384	89.5	89.1
1984	455	415	91.4	90.8
1985	470	439	93.4	92.8
1986	499	467	93.5	93.0
1987	524	496	94.6	94.1
1988	550	522	94.9	94.7
1989	593	551	93.0	92.9
1990	620	580	93.6	93.0
1991	652	600	92.0	92.0
1992	679	621	91.4	91.1
1993	701	646	92.2	91.9
1994	n.a.	n.a.	n.a.	n.a.
1995	n.a.	n.a.	n.a.	n.a.
1996	728	721	99.1	98.1
1997	750	736	98.2	97.0
1998	793	753	94.9	94.5
1999	837	770	92.0	91.6
2000	872	791	90.7	90.3
2001	908	812	89.4	89.5
2002	950	836	88.0	87.8
2003	992	870	87.7	n.a.
2004	1,007	894	88.8	n.a.
2005	1,037	902	87.0	n.a.
2006	1,073	920	85.7	n.a.
1983-93			2.7	2.8
1993-96*			-1.0	-1.0
1996-2002			-11.1	-10.4
2002-06			-2.3	n.a.
1983-2006			-10.7	n.a.
1993-2006			-13.4	n.a.

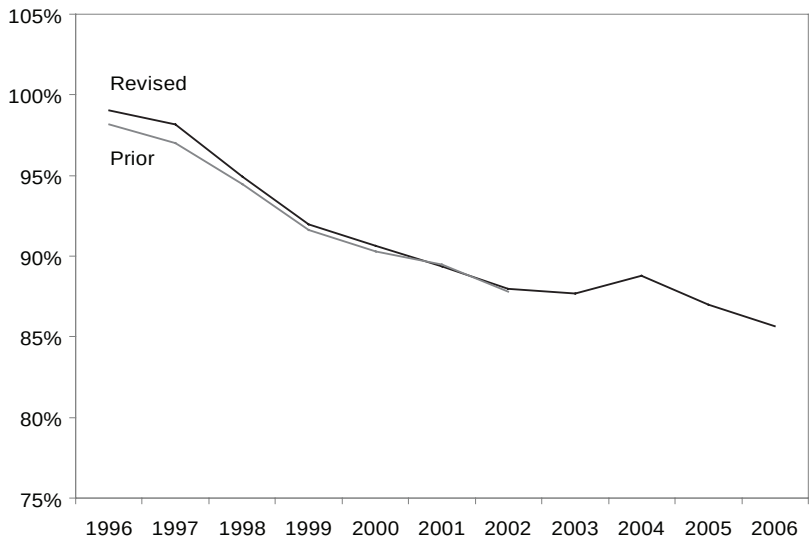
* Authors' analysis of March CPS.
SOURCE: Authors' analysis of CPS data.

Using the BLS skill rating, we compute two summary measures of overall skill for each occupation—a "point" measure and a "market value" measure. These skill measures are based on Pierce's (1999) estimated returns to skill within each generic leveling factor, the distribution of employment across skill levels within each occupation, and a "point rating system" for skill levels. (The point measure is based on the federal government's "factor evaluation system" used to rate white-collar jobs within the federal sector.) Points are assigned to higher skill levels within each skill dimension; some skill dimensions (such as knowledge) are weighted more heavily than others (like complexity).[20] Total points for each occupation in the NCS were provided to us by the BLS from an unpublished tabulation. These data allow us to compute summary measures of overall skill for each occupation and identify occupations that have comparable skills (and skills similarly valued by the marketplace) to teaching.[21]

The market value measure of skills essentially draws on information that identifies how much more in wages workers with particular skill levels earn (from Pierce 1999), and then uses information on the skill levels of each occupation to calculate how much more workers in that occupation earn compared to other occupations where workers have different skills. This provides a ranking of occupations.

Based on these two summary measures of occupational skill, 16 professional and managerial occupations were identified as comparable to K-12 teachers (that is, they

Figure H Teacher wages relative to comparable occupations, prior and revised, 1996-2006



SOURCE: Authors' analysis of NCS and CPS data.

were found to have similar skill ratings as teachers using both measures).[22] Unfortunately, it is not possible to link current occupational wage data to historical wage data because of changes in occupational coding. Fortunately, there is comparability for the six largest occupations in our list (accountants, reporters, registered nurses, computer programmers, clergy, and personnel officers) that comprised 83% of the aggregate employment of the initially selected 16 occupations. We use these six occupations as the "comparable" group in our analysis here. Given the dominance of this group in the earlier computations, it should not be surprising that the relative teacher wage in 2002 (the year of analysis in our prior study and a year for which all data are available) is the same using the revised group definition, and that the change in relative wages between 1983 and 2002 is very much the same.[23] To facilitate a comparison of the prior results to our new results, we examine all teachers, rather than just public school teachers, as we did in the earlier study.

Table 6 and **Figure H** present the historical trend in teacher wages relative to a comparable group of occupations. In 2006, teachers earned 85.7% as much (14.3% less or $154 less) in weekly wages as did those in the group of comparable occupations, representing a 2.3 percentage point erosion in relative teacher pay since 2002, the last year of our earlier study. (Overall, teacher relative wages fell 13.4 percentage points since 1996 and 14.4 percentage points since 1993.[24]) The period between 1983 and 1993 saw small increases in relative wages for teachers. The erosion of teacher relative wages using comparable occupations (11.1% from 1996 to 2006) parallels the erosion found using regression estimates (10.7%) in Chapter 2.

CHAPTER 4

The Role of Differences in Non-Wage Benefits

This analysis of relative teacher compensation thus far has focused entirely on the *wages* of teachers compared to other workers. Yet, fringe benefits such as pensions and health insurance are an increasingly important component of the total compensation package. Many observers argue that teachers enjoy more attractive fringe benefit packages than other professionals, suggesting that our measure of relative teacher pay may overstate the teacher disadvantage in total compensation.[25]

This chapter examines whether and how much our estimates of relative teacher pay should be adjusted to reflect differences in total compensation; that is, it will attempt to measure the size of any "fringe benefit bias."

We are not aware of any prior estimates of the "fringe benefit bias," although the data are readily available. Unfortunately, in many discussions of teacher pay it is simply noted that benefits are a sizable part of total compensation, usually re-iterating the popular assumption that, because teachers' benefits packages are more attractive than that of comparable professions (implying a fringe benefit bias in relative wage comparisons), there is no problem with teachers earning lower wages. Some, such as Vedder (2003), get the basic math wrong by assuming you can add the wage gap and the benefits gap to obtain the compensation gap (see Allegretto, Corcoran, and Mishel 2004 for further detail). In fact, because benefits comprise only 20% of compensation, one must construct a weighted average of these gaps.

Two recent reports attempt to quantify the monetary value of the benefits gap between teachers and other professionals. In *Frozen Assets*, for instance, Roza (2007) calculates the value of "above-average health and retirement benefits." In *Tough Choices*, Tucker (2007) focuses on retirement benefits and claims that pension contributions for teachers can be cut in half and remain at the level of better private employers. Both of these analyses are flawed, for reasons explained in detail below.

Basic facts on non-wage benefits and the "benefits bias"

Table 7 provides the basic information necessary to compare benefit packages—as a share of overall compensation—of K-12 teachers and other professionals. This table

uses the BLS Employer Costs for Employee Compensation (ECEC) series from June 2006 to disaggregate the overall cost of compensation into its various components, such as wages, benefits, and employer taxes. These shares allow us to estimate the "benefits bias" inherent in a wage-only comparison of compensation, which we do for 2006 and for various years between 1994 and 2006.[26] These estimates will, in turn, allow us to assess the extent to which the growing teacher wage disadvantage over this period was partially offset by a change in teachers' relative fringe benefits packages.

Table 7 divides total compensation into several categories of pay, including wages, benefits, and payroll taxes. (Although workers do not perceive payroll taxes as "compensation," they do represent a part of an employer's labor costs; hence they should affect the overall compensation package a worker receives). Wages, in turn, are divided into two components. The first is "direct wages," defined by the BLS as "regular payments from the employer to the employee as compensation for straight-time hourly work, or for any salaried work performed." This definition of wages is what the ECEC and the NCS refer to as wages, and is what those who have used these data (Podgursky

Table 7 Employer compensation costs as a percent of total compensation: Civilian workers, by occupational group, June 2006

	Share of compensation (%)	
Items	**K-12 teachers**	**Professionals**
Direct wages	73.2%	71.6%
Paid leave	5.1	7.5
Supplemental pay	0.2	1.8
Total W-2 wages	**78.5**	**80.9**
Insurance	9.8%	7.6%
Pension	6.1	4.8
Legally required	5.4	6.7
Other	0.2	0.0
Total non-wage benefits	**21.5**	**19.1**
Total compensation	**100.0%**	**100.0%**
Memo:		
Pension/legally required	**11.5**	**11.5**

SOURCE: Authors' analysis of ECEC data, June 2006.

2003; Vedder 2003; Roza 2007) have analyzed. "Direct wages" exclude the second component—"other wages"—which includes premium pay for overtime, bonus pay, paid leave, and profit-sharing. The sum of basic and other wages is identified as "W-2 wages," a wage measure that corresponds to earnings in the CPS data we used above and to wages reported to employees and to the Internal Revenue Service. It is important to note that one will obtain a *different* teacher wage differential depending on which wage measure is employed. This follows from the fact that "other wages" are a larger part of compensation for other professionals (7.5% in 2006) than for teachers (5.1%), as teachers rarely receive bonuses or paid vacation (although they may receive additional wages for extra-curricular activities). Consequently, analysis of direct wages alone will tend to *understate* the teacher wage differential disadvantage by five percentage points relative to an analysis of total (W-2) wages.[27]

Table 7 also presents the shares of the two non-wage (or fringe benefit) components of compensation—health and pension benefits—as well as payroll taxes (social security, unemployment insurance, and workers' compensation). Teachers have a greater share of their compensation in health and pension benefits in 2006, comprising 15.9% of overall teacher compensation and 12.4% of professional compensation. One reason health and pension costs are higher for teachers is that teacher health benefits are provided for a full year for workers *who receive salaries for less than a full year*.[28] These greater costs would also reflect teachers having better benefit packages.[29]

Note that payroll taxes, on average, are also less for teachers nationwide, the result of some teachers not being in the Social Security system. This is an important observation, as recent claims of "excessive" teacher pension costs (e.g., Roza 2007 and Tucker 2007) do not take into account the fact that some teachers have lower payroll taxes because they are not in the Social Security system and will not earn Social Security benefits based on their work as a teacher. Pension plans where participants are not also covered by Social Security have higher pension costs. This is why pension costs should not be considered without also examining Social Security at the same time.[30] The "memo" line at the bottom of Table 7 shows that combined pension and payroll tax costs for teachers is the *same* share of overall compensation as for professionals (11.5%). That is, by this more inclusive measure, teachers do not have "unusually generous" pension costs (a term used by Roza 2007). Non-wage compensation as a whole was more important for teachers (21.5%) than for professionals (19.1%).

Table 8 uses the data in Table 7 (and comparable data for earlier years) to compute the "fringe benefits bias" for a calculation of relative earnings based on the W-2 measure of wages. This "bias" estimate tells us to what extent an estimated relative wage disadvantage will be offset by a relative benefits *advantage*. The analysis is presented for several years between 1994 and 2006 so that we can identify the bias in 2006 and any changes in the bias over time. These data allow an examination of the extent our estimates of the teacher wage disadvantage in 2006 and the change in this disadvantage over the last 10 years have been offset by (relative) benefit improvements.

According to Table 8, overall K-12 teacher compensation was 27.5% greater than teacher wages alone, while overall professional compensation was 23.5% greater than

Table 8 The bias from excluding benefits

	1994	2003	2004	2006
Ratio of compensation to wages				
All teachers	1.249	1.239	1 243	1.256
K-12 teachers	n.a.	n.a.	1 255	1.275
Professional specialty	1.230	1.218		
Professional and related			1 230	1.235
Bias from excluding benefits (%) relative to professional specialty:				
All teachers	1.400	1.500	-	-
K-12 teachers	n.a.	n.a.	-	-
Relative to professional and related:				
All teachers	-	-	1.0	1.4
K-12 teachers	-	-	1.8	2.8

SOURCE: Authors' analysis.

professional wages. These differences in benefit shares translate into a benefits "bias" of 2.8 percentage points in 2006.[31] Given our estimate of the teacher weekly wage disadvantage in 2006 of 15.1% (Table 3), these results together imply a benefits-inclusive compensation disadvantage of 12.3% (15.1 less 2.8) in 2006.

Estimating the extent to which the benefits bias has changed over time is more difficult for two reasons. First, data on K-12 teachers was not reported separately from all teachers (including university professors) until 2004 (the series on "all teachers" began in 1994). Second, the "professional" category reflects a somewhat different group in 2004 than in 2003, shifting from "professional specialty" occupations in 2003 to "professional and related" occupations in 2004.[32] We had concluded in our earlier study that, based on the fact that the benefit bias for teachers did not increase between 1994 and 2003, the growth of the wage disadvantage over that period was not offset by any increased benefits advantage, and these results are replicated here. We also concluded that the size of the overall benefits bias was small—only 1.5 percentage points—over this period.

However, here we find a clear increase in the benefits bias between 2004 and 2006, increasing from 1.8% to 2.8%. If we assume there was no change in the benefit bias from 2003 to 2004 (which we are unable to measure due to measurement inconsistencies), then we observe an overall increase in the "benefits bias" of 1 percentage point since 1994. Our estimate of the erosion of relative teacher pay since 1993 from Table 3 is 11.7 percentage points; given a change in the benefits bias of one percentage point, it

is appropriate to say there was an erosion of relative *compensation* of between 10 and 11percentage points.

Some recent claims about the "benefits bias"

As mentioned above, two recent and prominent reports have examined the extent to which K-12 teacher benefits exceed those of other workers. We consider them each in turn.

Frozen Assets (2007)

This analysis by Marguerite Roza (2007) for Education Sector, a Washington-based research organization, suggests areas of school expenditure that might be re-directed. In particular, the study aims to identify "common provisions in teacher contracts that obligate schools to spend large amounts of money on programs that lack a clear link to student achievement" (p. 1).

Roza suggests for example that "money spent on seniority-based raises and generous health plans for more veteran teachers might be better used for raising minimum salaries to recruit younger educators who meet high teaching standards."

We will focus here on the savings estimated to be available by cutting "unusually generous" benefits provided teachers. Roza's methodology is designed to estimate "the cost of employee benefits like health care and retirement benefits by looking at the cost differences between typical teacher benefits and the benefits enjoyed by the average worker in the private sector" (p. 2). Roza draws heavily on Podgursky's (2003) assessment of teacher benefits. On health insurance, for instance, Roza concludes:

> Based on data from the federal Bureau of Labor Statistics, Podgursky's analysis suggests that teacher health and other insurance benefits amount to 9.1 percent of the average salary, compared to 6 percent for other professionals. Table 7 shows that the difference between 6 percent and 9.1 percent translates into annual costs of $106 per student, or 1.28 percent of school budgets.

Thus, Roza computes the money saved by eliminating differences in the share of a benefit in overall wages between teachers and professionals. The benefit shares Roza uses are comparable to those we presented in Table 7 except that hers are relative to *wages* (i.e., *direct wages* in our terminology), while ours are relative to total *compensation*.

It is peculiar that Roza's analysis labels teacher benefits "unusually generous" without any examination of salary levels of teachers relative to comparable workers such as professionals. Her argument is wrong on two counts. First, even if benefits are judged excessive, teacher pay can be too low as long as salaries put teachers at a disadvantage. Note that salaries are four times as large as benefits when calculating total compensation. Yet, *Frozen Assets* never addresses salaries, except mentioning that schools may want to raise entry level salaries. Secondly, compensation packages reflect the preferences of those involved as to how much should be devoted to benefits versus wages. For

instance, teachers may have preferred a lower growth of their wages rather than suffer higher health insurance premium contributions or higher co-pays and deductibles. That is, there is a wage/benefit tradeoff that operates in the labor market through both individual and collective bargaining. This being the case, it is curious to suggest cutting benefits without any consideration of restoring some or all of the foregone wages.

The lack of wage comparisons in Roza's analysis prohibits any meaningful interpretation of differences in benefit shares as reflecting higher compensation. Take, for instance, the health benefits shares used by Roza, 9.1% of wages for teachers and 6.0% for professionals. If teachers face a 15% wage disadvantage, as we have found, then one should calculate benefit shares after equalizing wages before concluding there is any "excess." For example, a 9.1% share of health benefits in wages is only a 7.7% share if wages are brought to parity. Simply put, *Frozen Assets*' failure to assess salaries makes its analysis of benefits incomplete and fundamentally flawed: one cannot reason from benefit share differences alone whether teachers have higher or lower compensation than other professionals.

Even if somehow one were to ignore this fundamental point that analyzing benefit shares independent of any wage comparison prohibits meaningful interpretation, then there are other errors of judgment and computation in the *Frozen Assets* analysis to consider. First, Roza's methodology is inconsistent: she applies a difference in the benefit shares of *wages* to total c*ompensation* rather than to total wages when computing

Table 9 A closer look at 'excess' health benefits

	Calculations			
	Roza	***Adjust to wages***	***Adjust for summer****	***Update to 2006***
	(1)	**(2)**	**(3)**	**(4)**
Health share of wages				
Teacher	9.1%	9.1%		9.8%
Professional	6.0%	6.0%	6.0%	7.6%
Difference	3.1%	3.1%		
Teacher, corrected for summer			7.7%	8.3%
Difference, corrected for summer			1.7%	0.7%
'Excess' per student ($)				
Compensation base	$106.50			$25.10
Wage base		$85.20	$47.70	
As share of school budget	1.3%	1.0%	0.6%	0.3%

* Adjust teacher benefit share assuming teacher work-year is 85% as long as other professionals.
SOURCE: Authors' analysis.

cost savings. Since compensation is about 25% greater than wages, this mistake exaggerates the estimated savings by 25%. This is shown in **Table 9**: the first column replicates Roza's analysis where the difference in shares of wages is applied to compensation, and the second column applies the difference in benefit shares to wages, not compensation. The savings per student falls to $85 from $106.50. Second, as discussed above, one reason why health benefits as a share of total compensation in teaching would be expected to be higher than that of comparable occupations is that teachers are provided health insurance year-round, including summers when there are no scheduled work days, which necessarily raises health benefits relative to wages. A methodology that imposes equal shares of health benefits relative to compensation or wages for teachers as for professionals as a policy goal or standard necessarily implies that teachers should pay for their own health insurance in the summer. The third column shows that, when the teacher benefit share accounts for the summer factor, the estimated savings fall even further to less than half the original estimate. Last, in the fourth column of Table 9 we employ the benefit shares for 2006 (from Table 7) in the analysis, which brings the savings to just $25 per student and 0.3% of the school budget (as calculated by Roza). Ultimately, this is much ado about nothing.

Roza also addresses retirement costs in the same manner, again relying on Podgursky's calculation of shares of wages:

> Podgursky's analysis, based on Bureau of Labor Statistics data, suggests that retirement costs amount to 5.9 percent of the average teacher's salary, compared to private sector retirement costs of 3.8 percent of the average salary.

The error in this analysis is clear: Roza examines retirement costs for teachers without considering that some teachers are not in the Social Security system, and thus some school districts have savings from avoiding the employer share of Social Security payroll taxes. As shown in Table 7, once you aggregate retirement costs and payroll tax costs, the shares of compensation are equal for teachers and other professionals. That is, there is no evidence of "unusually generous" pension benefits for teachers once the absence of Social Security benefits is taken into account. In fact, if teacher wages were brought to parity with those of other professionals, then recalculated benefit shares would actually show teachers have lower retirement benefits.

Frozen Assets' analysis of savings from "unusually generous" teacher health and retirement benefits is fundamentally flawed and marred by errors in computations and concepts (such as examining pensions independent of Social Security and ignoring substandard teacher wages).

"Tough Choices or Tough Times"[33]

The New Commission on the Skills of the American Workforce authored a report in 2007, *Tough Choices or Tough Times* (Tucker 2007) that properly observes that, if we want to recruit and retain better teachers, then we must pay them more. But, in an

attempt to accomplish this without fully paying the price, the commission—like the Education Sector report—creates a myth that teacher benefits are so generous that we can reduce them and use the savings to boost salaries. Believing this, the commission proposes to end teachers' defined-benefit pension plans (funded in advance to pay retirees a guaranteed annuity) and substitute defined-contribution or cash-balance plans (in which employers make contributions to teachers' individual retirement accounts, but in which the size of each teacher's pension depends on how savvy an investor he or she becomes). This is supposed to cut schools' contributions in half, from 12% to 6% of salaries, thus matching the plans of "better private employers."

These data from *Tough Choices* are again contradicted by the data presented in Table 7, which showed employer pension contributions for teachers being 6.1% of compensation, very far from the 12% claim above (even if we adjust to shares of wages, which would be higher). In this light, the 6 percentage-point cut in pension contributions urged in *Tough Choices* is roughly equivalent to the entire contribution of schools to teacher pensions. It is also troubling that *Tough Choices* provides no evidence on the pension contributions of "better private employers" (however that is defined) to support the claim that teacher pension contributions are double that level.

Tough Choices also errs by not addressing the Social Security dimension of retirement. As noted above, pension contributions of many school districts do exceed those of many private employers, but *all* private employers also pay Social Security taxes on salaries paid to professional workers, who receive a defined-benefit Social Security retirement annuity to supplement their 401(k)s. Many teachers, however, are still not covered by Social Security, a fact that reduces the national average cost of teacher benefits. The appropriate comparison would be between teacher and private-sector *total* retirement costs, including Social Security. As Table 7 shows, K-12 teachers and all other professionals (most of whom are in the private sector) now have the same share of compensation—11.5%—in overall retirement and payroll tax costs. If teachers must give up defined-benefit plans, those without Social Security will be alone among professionals in lacking any defined-benefit safety net. There is simply no painless, cost-free way to boost salaries by raiding a teacher benefits piggy bank.

The *Tough Choices* analysis also suffers from the same flawed logic as *Frozen Assets* when it assesses the generosity of teacher benefits by examining benefit shares of wages independent of a salary comparison. In fact, it is a bit more curious because the *Tough Choices* report explicitly urges significantly higher wages for teachers. Should it not, then, assess benefit shares at the wage levels it recommends? Simply put, because teachers earn less salary, a dollar in benefits will count as a larger share of teacher compensation than it will for other professionals. So comparing the cost of benefits as a percentage of these uneven salary bases or total compensation can be rather misleading.

CHAPTER 5

Misuse of the National Compensation Survey, Revisited

Our 2004 report demonstrated how hourly wage data from the employer-based National Compensation Survey (NCS) has been frequently misused in the comparison of teacher compensation with that of other professionals (see Podgursky 2003 and Vedder 2003, for prominent examples). Unfortunately, the misuse of these data continues, despite our report and explicit warnings from the Bureau of Labor Statistics—the publisher of the National Compensation Survey—*not* to use NCS hourly rates of pay to compare teacher pay to the pay of other occupations (see *The BLS on NCS Hourly Wage Data* on p. 40).[34]

For example, a report by Greene and Winters (2007) purports to show that teachers are better compensated than editors, reporters, architects, psychologists, chemists, economists, mechanical engineers—and most other white-collar professional workers—when hourly pay is compared. While such comparisons are clearly made with the intention of making apples-to-apples comparisons of pay for a unit of work, our 2004 report demonstrated plainly how hourly rates of pay in the NCS are not measured in the same way for teachers as they are for other professionals and are therefore inappropriate to use for this purpose.

The NCS is a survey responded to by employers (e.g., school districts in the case of teachers) for which the Bureau of Labor Statistics randomly selects employers and occupations within selected firms.[35] (In their study, Greene and Winters rely on data from the 2005 wave of the NCS.) Although the NCS presents earnings on an *hourly* basis, *they are not necessarily collected in this way*. Employers are asked to report occupational earnings on an annual, weekly, or hourly basis as appropriate, together with scheduled hours worked per day or per week, and weeks worked per year.[36] For salaried workers not on a rigid work schedule, the "typical number of hours actually worked [is] collected."[37] For full-time, professional salaried workers, hourly earnings would be calculated by the BLS as the annual salary divided by weeks worked per year, divided again by the number of hours worked per week.

Comparing the compensation of teachers to that of other professional on a basis other than annual earnings does make some sense. After all, school teachers are not paid for the same length work year as full-year workers, making annual earnings problematic

THE BLS ON NCS HOURLY WAGE DATA

When compared with other occupations the hourly earnings for some occupations, such as teachers and airline pilots, seem higher than expected. Why is this?

Answer: Hourly earnings are just one means of comparing the wages of different occupations. This method has the advantage of treating all occupations with a common denominator—a single hour. Unfortunately, this method may not work well for certain occupations with unusual hours. Teachers who often work only nine or 10 months per year are an example of this problem. Another example is the airline pilot occupation. In addition to flight hours, which are highly regulated and carefully recorded, airline pilots also spend time preparing for flights. The preparation time may not be as closely monitored as flight hours. In occupations such as these, total work hours may have to be estimated. Because of these issues, comparisons of annual salaries published by the National Compensation Survey (NCS) might be more appropriate when considering certain occupations.

The National Compensation Survey publishes the number of weekly and annual hours worked for occupations. How is the number of hours worked determined for occupations with unusual work schedules such as airline pilots and teachers?

Answer: The Bureau of Labor Statistics collects data on earnings and associated hours directly from employers (typically human resource professionals) either through a personal interview or telephone conversation. BLS requests that employers provide the appropriate hours that comprise all the duties of the occupation. The collection of hours is more difficult for some occupations than for others and in some cases an estimate must be accepted. In addition to flight hours, which are highly regulated and carefully recorded, airline pilots also spend time preparing for flights. In the case of elementary and secondary teachers, hours of work include preparation time, administrative time, and professional days. For college and university professors, research time and office hours are included with class time in the total number of hours worked.

Source: http://www.bls.gov/ncs/ocs/peoplebox.htm#Q01

(particularly when comparing earnings at a single point in time). The problem lies in the inconsistent manner in which weeks worked per year and hours worked per week are reported for teachers and other professionals—an inconsistency both we and the Bureau of Labor Statistics have pointed out.

For example, a BLS bulletin notes the following about the measurement of weeks worked: "Teachers are typically employed for a fixed number of days—for example, 180—over a 9- or 10-month contract. For many teachers, *school holidays are not in-*

cluded in the days contracted for and are therefore not designated as paid holidays" [emphasis added].[38] In other words, during Thanksgiving an architect and a teacher might both not work Thursday and Friday. The NCS day would show an architect as having worked five days, while the teacher is shown as having worked three.

Weeks worked per year is critical for weekly and hourly earnings calculations for workers paid annually. If one uses published NCS tables to assess the number of annual weeks worked by occupation (dividing annual hours by hours worked per week) the above inconsistencies become clear.[39] For professional occupations broadly defined, there are 46.0 weeks worked per year (obtained by dividing the annual hours of 1,829 by the average weekly hours of 39.0). Similarly, architects work 52.0 weeks each year (2,155 / 41.4), mechanical engineers work 52.0 weeks (2,122 / 40.8), and lawyers work 52.0 weeks (2,157 / 41.5). In other words, according to the NCS the typical professional is considered to have worked (paid holidays included) about 52 weeks per year. This constitutes the denominator in the calculation of weekly (and by extension hourly) pay for these groups.

Public school teachers, on the other hand, are shown to work an average of 38.4 (1,403 / 36.5) weeks per year (38.0 for elementary teachers). These numbers represent the denominator in the calculation of weekly (and thus hourly) pay for teachers.

Why should these varied denominators matter if teachers do indeed have a shorter work year than other professionals? The answer is that weeks worked for professionals *includes* paid time off, while the same statistic for teachers *excludes* days not spent working even if paid. To illustrate further, assume a teacher works a 180-day school year and eight non-instructional days. This accounts for 37.6 (188 / 5) of the 38 reported NCS work weeks, leaving only two to four days "unaccounted for." What results is a fairly precise measure of weeks spent working for teachers but an inflated number of weeks of work for other professionals, unless one assumes that professionals are in fact working a full 52 weeks a year. When translating annual salaries to weekly (or hourly) pay, an inflated number of weeks worked will considerably *deflate* compensation for a week (or hour) of work.

Weekly work hours also appear to be generally understated in the NCS. Greene and Winters (2007) argue in their report that the 32.6 to 40 hours per week teachers work on average *includes* all required work outside of school hours, including preparation and grading. This is simply not how work hours for teachers are measured in the NCS, which reports scheduled hours and does not include work before or after school (whether at school or at home). Moreover, this assumption is simply implausible. Other published data documents clearly the number of hours teachers spend in various activities.[40] In its periodic Schools and Staffing Survey, the U.S. Department of Education finds that school teachers were required to work an average of 37.7 hours per week to receive their base pay, with approximately 27 to 29 of these hours devoted to direct instruction.[41] These 37.7 hours correspond closely to those negotiated in the typical teacher union contract. Accordingly, these are the logical figures that a school principal would provide in response to a survey question regarding teacher contracted (scheduled) work hours. When asked to include other school-related activities (including

grading), teachers in the Schools and Staffing Surveys report an average of 52.4 hours of work per week.

Taken together, hourly pay comparisons from the NCS like those in Greene and Winters (2007) rely on a fundamentally flawed measure of relative compensation, and are of little use in policy discussions surrounding teacher pay.

IF YOU BELIEVE NCS HOURLY WAGE DATA (FOR 2002), THEN YOU BELIEVE…

Economics professors ($61.73) are the second highest paid professionals after pilots and make far more than business professors ($42.58).

English professors ($43 50) make more than dentists ($33.34) or nuclear engineers ($36.16).

Law professors ($51.71) make the same as judges ($51.67) and more than doctors ($50.69).

Physical education professors ($40.06) make more than architects ($26.63), accountants ($23.34), and computer programmers ($24.88).

Sociology professors ($32.39) make more than mechanical engineers ($29.84) and architects ($26.63).

Source: National Compensation Survey (2002).

Even authors such as Podgursky (2003)—who once used hourly rates of pay to compare teachers to other professionals—have acknowledged that heated disagreements over hours worked are mostly unproductive, and they now use weekly or annual rates of pay in their own work. However, as we showed above, even comparisons of weekly compensation in the NCS are flawed, as the number of weeks worked are not comparable between teachers and other workers.

Conclusion and Policy Implications

We have established that public school teachers earn less than similarly educated and experienced professionals and that this disadvantage has grown substantially over the last 10 to 13 years. The basic story is that the earnings gains that appeared to benefit all college-educated (and other) workers during the late 1990s appear to have bypassed teachers. Moreover, in recent years the average college graduate has experienced a stagnation in real wages, and teachers appeared to have fared even worse. In this report we have documented this erosion of teacher relative pay in three different surveys using data on both weekly and annual wages.

The longer view is that women teachers enjoyed an earnings *advantage* in 1960 relative to other women college graduates. As women's opportunities have expanded, the earnings of women in teaching have fallen behind those of similarly educated women.

We have also identified flaws in two recent studies related to teacher compensation, *Tough Choices* and *Frozen Assets*, which suggest that teacher benefits are so generous that policy makers can reduce them to achieve a substantial boost in teacher salaries. In fact, a straightforward analysis of teacher compensation data shows that K-12 teacher benefits are better on average than those of other professionals, but that benefits are a small share of overall compensation (about 20%), so that accounting for differences in benefits does not alter the overall disparity significantly.

The real curiosity is that the extensive policy discussions of teacher pay seem to ignore the persistent and growing teacher pay disadvantage. Any effort to alter the quality of the teacher workforce by changing recruitment and retention must address the teacher pay disadvantage if there are expectations of changing the profile of the typical teacher, which is what is required to have a substantial impact on education outcomes. Efforts to provide one-time bonuses to a small minority of teachers (especially small bonuses) would continue to leave the pay of the most effective teachers below that of their comparables in the labor market and could hardly be expected to change retention and recruitment dynamics for the "best" teachers, let alone the typical teacher.

APPENDIX A

Wage Measurement Issues

This appendix provides more detail on three measurement issues regarding estimates of the teacher relative wage, or wage disadvantage. First, we examine the problem with using the full samples from the CPS-ORG data, which includes many observations where the weekly wages are imputed. Second, we examine the issue of whether the CPS-ORG weekly wage data provides biased (too large) estimates of the teacher wage disadvantage. We focus specifically on whether there is a potential bias in the level or trends of the estimated teacher wage disadvantage. Last, we explore how the results from model specification for estimating the teacher wage disadvantage in this study differ from the model specification we employed in our earlier study, *How Does Teacher Pay Compare* (Allegretto, Corcoran, and Mishel 2004).

1. Imputations

The Census Bureau undertakes a careful and complicated process to impute wages where necessary. Imputed wages are those that have been inferred, or assigned, in cases where the respondent fails to report his or her earnings (Hirsch and Schumacher 2004). In the BLS imputation procedure, earnings are estimated for non-respondents through a "hot deck" method employed by the Census Bureau. This method finds a respondent or "donor" in the survey that closely matches the non-respondent on characteristics such as location, age, race, and education. When comparing the pay of teachers to that of non-teachers, a problem arises because occupation is not necessarily one of the criteria used in imputing earnings. Non-responding teachers are often assigned the average earnings of non-teacher college graduates (systematically over-stating their earnings), while some non-teaching professionals have imputed wages based on teacher earnings (systematically understating their pay). For example, the average weekly wage of teachers in our full sample in 2003 (including both imputed and non-imputed observations) was $900, i.e., 3.2% higher than the average of non-imputed earnings ($872). In contrast, the average weekly wage of non-teacher college graduates was 2.1% *lower* in the full sample than among the non-imputed observations ($1,128 versus $1,152). Failure to exclude imputed wage observations, therefore, lowers the ratio of non-teacher weekly wages to teacher weekly wages among college graduates from 1.321 to 1.254, a considerable amount.

There has been a remarkable increase in the share of observations where wages are imputed: wage imputation among teachers rose from 12.3% in 1983 to 15.1% in 1993 to 26.3% in 2003; among non-teacher college graduates imputations rose from 16.3% in 1983 to 17.9% in 1993 to 33.0% in 2003. There was a large jump in imputations associated with the redesign of the Current Population Survey. Among all full-time college graduates, imputations rose from 17.6% of the CPS-ORG sample in 1993 to 23.4% in 1996.[42]

The growing proportion of observations with imputed wages means that the bias in measuring teacher wages imparted by the imputations process has been growing over time. Consequently, imputations are creating a systematic and growing bias in measuring teacher relative wages using the CPS-ORG. As a result, we have elected to use only those observations of full-time workers with non-imputed wages.

Restricting the sample to nonimputed observations raises the issue of whether there is a selectivity involved that biases our estimates. The issue can be thought of as whether there are systematic differences in which observations are given (or are in need of) imputed wages among teachers relative to nonteacher college graduates. We ran OLS and probit analyses, which examined the relationship between wage imputation and the same set of human capital variables we use for our estimates and a teacher indicator. We did not find evidence of any "teacher effect" on imputation, except in a few years where the effect was statistically significant but not meaningful in size. Experiments in which a teacher indicator interacted with other variables did not show any persistent or significant (statistically or economically) effects. All of these models had very little explanatory power. We concluded that selectivity of the imputations does not present a problem for our estimates.

2. Do teachers understate wages in CPS-ORG weekly wage data?

Critics of the CPS-ORG (e.g., Podgursky and Tongrut 2006) argue that the use of weekly wage data to compare teachers with other workers biases teacher earnings downward, claiming that teachers report a weekly wage that is actually an annual salary divided over a full year rather than the partial year they actually work. If true, our estimates would overstate the pay disadvantage for teachers.

It is useful to separate discussions of bias into that related to *levels* versus *trends*. In this case there are two issues. The first is whether our data appropriately measure the degree to which teachers earn less than comparable workers in a given year (a *level* comparison). The second is how relative teacher pay has changed over time (a comparison of *trends*). This distinction is important, as a measure can be biased in terms of levels (a thermometer, say, may be off by two degrees) but could still provide accurate information on trends (how much the temperature rose may be accurately discerned with both a precise or a consistently biased thermometer). In this light, the Podgursky and Tongrut (2006) critique of the CPS-ORG weekly wages of teachers should only apply to our estimates of the extent of the teacher pay disadvantage in a particular year—the

level—and does not necessarily mean that that our findings of an erosion of teacher relative pay over time is biased in any way (again, if a bias is consistent over time, then the trends over time will be accurate). Podgursky and Tongrut make no argument that there has been an increasing bias in the CPS-ORG measure of weekly wages.

3. Benchmarking levels: CPS-ORG weekly wages and the March CPS annual wages

We now turn to assessing a bias in the estimates of the teacher pay disadvantage at a point in time—a bias in the level. It seems that in the CPS-ORG surveys undertaken before the 1994 redesign there was a bias of the sort that Podgursky and Tongrut describe. However, our assessment is that this bias was dramatically reduced in the surveys following the redesign, and our estimates for recent years are not significantly biased. Furthermore, the time trends we present also lack significant bias, as we do not rely on the *level* estimates in the (pre-redesign) 1979-93 period, only the *changes* in these estimates.

Prior to the survey redesign in 1994, the CPS-ORG survey inquired only about wages earned "last week." However, as discussed in the study, the new survey question asks respondents to provide their wages *in any interval that is easiest for them*. We find that slightly more than half of all teachers in the CPS now provide an *annual* wage for this question; the BLS then computes a weekly wage using weeks worked.

The change in the CPS survey question on earnings appears to have resulted in a significantly *higher* weekly wage among teachers. For instance, teacher wages rose 10.2% between 1993 and 1994 (the year the redesigned survey was implemented), far faster than the 2.2% increase among non-teacher college graduates. The additional 8% wage growth among teachers appears to represent the effects of a correction for bias in the pre-1994 survey questions, a bias that would affect estimates of the level of the teacher pay disadvantage in those years. It is worth noting that this 8% correction represents about half the maximum potential bias if all teachers that do not work year-round (say 85% of all teachers) reported their annual earnings over the 52-week work year rather than the roughly 42.5-week work year we estimate below.[43]

Thus, where we discuss the absolute size of the teacher wage disadvantage in this report, we focus only on estimates from recent years, in particular from 2006. In computing *changes* in the pay disadvantage, we do not allow pre-1994 bias to affect our results. For example we do *not* compute changes in relative weekly wages of teachers by differencing a recent estimate with an estimate from any period before 1994 (e.g., calculating the change in the teacher pay premium by subtracting the estimate for 1979 from that for 2006). Rather, we assume the *change* in teacher relative pay in the pre-1994 data is accurately measured (i.e., the bias did not change over this period). We add the change in relative pay between 1979 and 1993 to the change in relative pay between 1996 and 2006 to obtain the changes from 1979 to 2006. As discussed above, the weekly wage data cannot provide relative wage estimates for 1994 and 1995 (imputed wages are not identified), so we bridge these two series by adding the changes

in teacher relative wages in the *annual* wage data for the 1993 to 1996 period to obtain the changes over the full 1979-2006 period.

It may be, however, that the redesigned survey question did not fully correct for the downward bias of teacher earnings. To assess this possibility, we benchmarked relative teacher pay in the CPS-ORG weekly wage data to the March annual wage data. Annual earnings from the March CPS have been relied upon by a number of researchers, including Hanushek and Rivkin (1997, 2004), Podgursky and Tongrut (2006), and Temin (2002, 2003). This exercise shows that the *level* of relative teacher pay is comparable across these two data sources. In the main body of the study we showed that trends in relative teacher pay over the past 10 years are comparable whether using the CPS-ORG or March annual wage supplement.

We benchmark the CPS-ORG and March data by comparing the weekly wage ratio of public school teachers to other college graduates. That is, we are benchmarking *relative* wages and not *absolute* wages, as relative wages are the foremost concern of this analysis. Also, we benchmark reported wage levels rather than a regression-adjusted measure of wages. We do this partly for ease of exposition but also to respond to critics who claim that the underlying survey responses are biased (e.g., Podgursky and Tongrut 2006). Regression-adjusted estimates may differ for other reasons, as the choice of control variables may vary in their impact on weekly versus annual wages. Also, regression estimates are based on logged wages, which generate smaller differentials, as we show below. If there is a bias it should be evident in a simple comparison of relative wages.

Table A1 reports relative wage ratios from both data sources, using data averaged over the 2002-05 period. The annual earnings of teachers reflects those on the *longest job held that year* (rather than earnings from all jobs) in order to provide a proper comparison to observed weekly wages of teachers. It is clear that the pay gap is larger when comparing *annual* wages (teachers earn 62.3% of the earnings of other college graduates) than with *weekly* wages (here teachers earn 76.7% of college graduate earnings). This would be expected, given that teachers work less than a full year—the annual wage comparison reflects "summers off," where weekly wage comparisons do so to a lesser extent. The gap between these two ratios is 14.4 percentage points.

If the differences in the wage gap between the CPS-ORG and March CPS are a fair reflection of differences in time worked over the year, then one could say that the two surveys are consistent with each other. That is, the *annual* wage gap should be related to the *weekly* wage gap as follows:

$$\left(\frac{annual_{tch}}{annual_{ntch}} \right) = \left(\frac{weekly_{tch}}{weekly_{ntch}} \right) \cdot \left(\frac{weeks_{tch}}{weeks_{ntch}} \right)$$

In other words the annual wage gap is just the weekly wage gap multiplied by the ratio of teacher and non-teacher annual weeks worked. According to Table A1, the annual wage gap is 63.1% while the weekly gap is 76.7%, implying—if these surveys are

Table A-1 Benchmarking weekly wages (CPS-ORG) to annual earnings (March CPS)

Year	Public school teachers	Other college graduates	Wage ratio
March CPS annual wages			
2002	$42,134	$65,944	63.9%
2003	43,361	67,244	64.5
2004	42,489	69,299	61.5
2005	44,531	71,053	62.7
Average, 2002-05			**63.1%**
CPS-ORG weekly wages			
2002	$856	$1,126	76.1%
2003	886	1,152	76.9
2004	914	1,175	77.8
2005	916	1,206	76.0
Average, 2002-05			**76.7%**
Gap: weekly vs. annual wage ratio			
Average, 2002-05			**13.6%**

SOURCE: Authors' analysis of CPS-ORG and March CPS data.

consistent—that the ratio of weeks worked should be about 0.82 (or 82%). Is 82% a close approximation of relative weeks worked? We consider this below.

Non-teacher college graduates report in the March CPS that they work an average of 51 weeks per year—a number that includes paid leave such as vacations and holidays (**Table A2**). Teachers' *scheduled* days of work (sometimes called contract days) are typically 188.2 days per year—or 37.6 weeks. Thus, teachers' scheduled work time is 74% that of non-teacher college graduates (or, 26.3% less).[44]

Scheduled days, however, do not reflect paid leave for holidays and vacations, while the 51-week average for other workers *does* include such days. We thus have to estimate the number of paid weeks in a year for teachers to put the comparison on an "apples-to-apples" basis. If we assume that teachers are given nine of the 10 federal holidays (excluding July 4) and accrue 85% of the vacation days provided workers who earn $15 an hour or more (15.6 days per year, an average of those with 10 and 15+ years of service), then the teacher work year extends to 42.6 weeks (meaning a summer hiatus

Table A-2 Teacher work years relative to non-teacher college graduates

	Weeks worked	*Ratio of weeks*	*Gap in weeks*
Non-teacher college grads			
Actual weeks worked*	51	-	-
Teachers			
Scheduled teacher work Days (188.2)	37.6	0.74	-26.3%
Allow for:			
Holidays (9) **	39.4	0.77	-22.7%
Vacation days (15.6) **	42.6	0.84	-16.5%
Assume 15% teachers are year-round***	43.8	0.86	-14.1%

* March CPS sample: full-time with at least 26 weeks of work.
** Nine federal holidays between September and June, and vacations equal to 85% of what private sector workers (with hourly wage of $15 or more) with 10-15 years seniority.
*** 15% work 51 weeks; 85% work 42.6 weeks.

SOURCE: Authors' analysis.

of 9.4 weeks).[45] At 42.6 weeks, the teacher work year is 84% that of non-teacher college graduates.

Finally, we correct for the fact that annual earnings in the March CPS include some teachers who do in fact work year-round as teachers. Assuming 15% of teachers in the sample work 51 weeks per year (comparable to other college graduates), while the others work 42.6 weeks, the average teacher work year is 43.9 weeks—86% that of non-teacher college graduates.

This rough calculation suggests that the ratio of teacher to non-teacher work weeks is about 86%, a bit higher than the 82% implied above, but not drastically different (being greater than 82% confirms the lack of a bias in the CPS-ORG measure). Consequently, the weekly wage data used here is roughly consistent with the March annual wage data that has been widely used. The result is that the smaller pay gap in weekly earnings versus annual earnings—76.7% versus 63.1%—corresponds to a reasonable estimate of the earnings impact of a shorter work year (i.e., the effect of "summers off"). Specifically, these calculations show that the average teacher has a work year (measured by weeks paid) that is about 86% as long as the work year (again, paid weeks) of non-teacher college graduates in full-time jobs. Podgursky and Tongrut (2006) assert that

"summers off" for teachers means that teachers should expect to receive just 75% of an annual salary of comparable non-teachers. They compare teacher scheduled days to an assumed 52-week work year for non-teacher professionals. This is mistaken, as this comparison does not compare teachers and other professionals on an apples-to-apples basis. The work years should be measured as either paid weeks or as actual weeks worked for both teachers and non-teachers. In Podgursky and Tongrut's calculation, teachers work time is measured as scheduled days worked and compared to an assumed 52-week work year for non-teachers, which is a measure that includes time paid but not worked.

This benchmarking exercise provides confidence in our estimates of the level of the teacher wage disadvantage, at least to the extent that confidence is extended to the March CPS annual wage data. Consequently, this study examines weekly wage comparisons, as stated above, which allows us to focus on comparisons of weekly rates of pay rather than generate a discussion of work years, summers off, and so on.

We now return to demonstrating that comparisons of the March annual wage data and the CPS-ORG weekly wage data should not be made by comparing regression results with each data set, at least if the purpose is exploring potential bias on one or the other data set. Because wage regressions are made using wage variables that are "logged" (taking the natural log of each individual observation's wage), they necessarily shrink the difference between the teacher wage disadvantage estimated using annual versus weekly wage data.

To identify the effect of "logging" the wage variables on the estimated teacher wage disadvantage, we compared college educated teachers to all non-teacher college graduates. In the 2001 March CPS the gap in annual earnings is -0.375 (37.5%); in the 2001 CPS-ORG the gap in weekly earnings is -0.232, a difference of 14.3 percentage points. Now suppose we take all college educated workers and regress log earnings on a (public) teacher dummy alone. In the March CPS the coefficient is -0.257, and in the CPS-ORG the coefficient is -0.1796. Notice both are considerably smaller than when the percent gap in the levels is used (taking the exponential and subtracting 1 will not affect this much). Now the difference between the March coefficient based on annual wages and the CPS-ORG coefficient based on weekly wages is 7.7 percentage points. Without any additional regressors, the estimated gap when using logs is nearly half the size, and the difference in the estimates is far less than the value of "summers off," which we estimate to be about 14%. In fact, however, this is an artifact of taking logs of wage variables and does not reflect any bias in the underlying data.

Benchmarking trends

This benchmarking exercise was presented in Chapter 2 of this study: we compared trends across surveys in the relative weekly earnings of teachers—the change in the relative weekly wage estimates. We noted that we regard the findings of an erosion of teacher pay as the most salient results in the study. Specifically, we compared changes in relative teacher wages across three different data sources—the CPS-ORG (which

reports weekly earnings) and the March CPS and decennial census (which report annual earnings). These results were presented in Table 5. Our comparison of trends over the 1979-99 period, for which we have estimates from all three surveys, showed that "relative teacher earnings fell 13.3% using the CPS-ORG data, which is somewhat below that found using census annual wage data (a 16.2% decline) and somewhat above that found in March CPS annual wage data (a 10.8% decline)."

As reported in this study (see Chapter 2), other comparisons yielded:

> Over the 1999 to 2005 period, annual wage estimates from the March CPS indicate a larger erosion of relative teacher wages among women (5.0% versus 4.1% in the CPS-ORG) and among teachers as a whole (6.1 versus 3.5%). Table 5 shows that between 1979 and 2005, the longest comparison possible with these two surveys, an analysis of the March CPS annual wage data yields almost identical results to the CPS-ORG weekly wage data. These comparisons suggest that our findings with the CPS-ORG data are robust across surveys, leaving little doubt that there has been deterioration in the relative earnings of teachers, especially over the past decade and since 1979.

Impact of changes in regression specification

As noted in this study, the regression model used to estimate teachers' relative wages has been modified somewhat from that used in our earlier study, Allegretto, Corcoran, and Mishel (2004). First, only *public* school teachers are used to identify the relative earnings of teachers. One criticism of our earlier work (Podgursky and Tongrut 2006) was that an estimate of relative teacher earnings using all teachers (public and private) overstates the wage disadvantage faced by public school teachers, as private school teachers generally earn lower wages than public school teachers.[46] Since the policy discussion about teachers is about public teachers we have shifted our estimates correspondingly.

Second, our specification in this study incorporates a more detailed list of education level controls. In our earlier study, we employed only four categories (college or higher, some college, high school, and less than high school). Because teachers are more likely to hold a master's degree than other college graduates (Larsen 2006), we include separate identifiers for those with a bachelor's degree alone, those with a master's degree, and those with education beyond a master's degree (i.e., doctorate or professional degree).

In this section we show that the net effect of these two changes is negligible. The *level* of the teacher wage disadvantage we estimate is somewhat less in the newer specification, as the lesser estimated teacher wage disadvantage that results from focusing on public school teachers is partially offset by the effect of additional education-level controls. The *trend* in relative teacher wages is, if anything, a bit stronger in this modified specification, confirming that our earlier analysis did not overstate the erosion in relative teacher earnings.

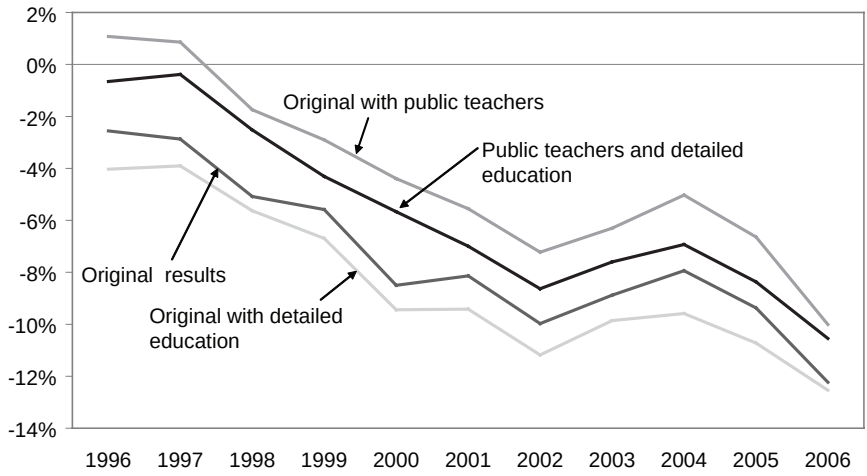
In Allegretto, Corcoran, and Mishel (2004) we focused on estimating the coefficient on a dummy variable that represented a pooled sample of public and private elementary and secondary teachers.

There are considerably more public than private school teachers in the United States. In 1979, 89.5% of teachers worked in the public sector; by 2006, that dropped to 83.5%. The percentages of public school teachers, overall and by gender, have been trending slightly downward since 1979, meaning that there has been an increase in the percentage of private school teachers.

It is well known that private school teachers, on average, earn less than public school teachers (although this gap is closing). Therefore, a more inclusive definition of teachers (i.e., including private with public school teachers) leads to a larger estimated teacher wage disadvantage—the disadvantage is between 2 and 3 percentage points smaller for public rather than all teachers over the 1979-2006 period.

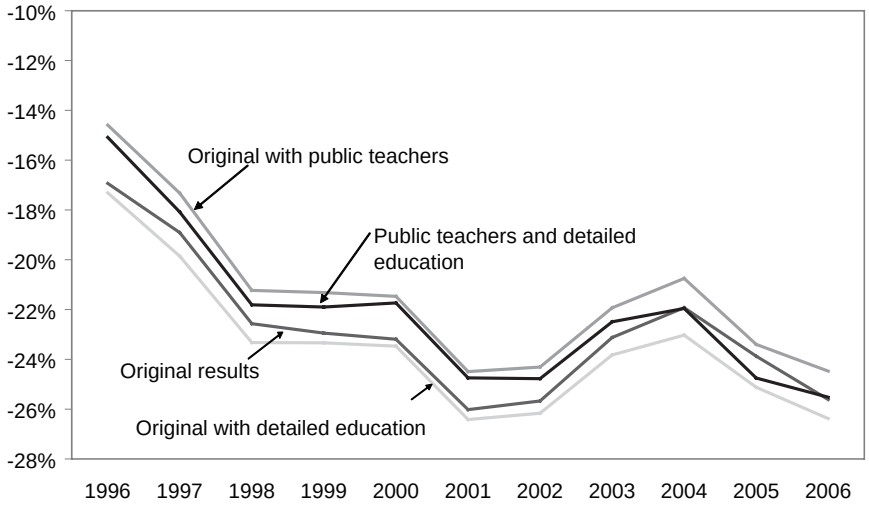
Since the writing of our earlier study we have become increasingly aware of the fact that many more teachers have advanced degrees than do other college graduates. In 2004 over 45% of public school teachers have a master's degree, while about 20% of other college graduates have master's. To compare teachers with others "comparably educated" requires we be able to distinguish among college-educated workers who have or do not have advanced degrees. Our specification in the earlier study had just one control for "college-educated" and therefore could not make this distinction. The specification in this study controls for differences in advanced degrees by having

Figure A-A Book results plus three specification tests (women)



SOURCE: Authors' analysis of CPS data.

Figure A-B Book results plus three specification tests (men)



SOURCE: Authors' analysis of CPS data.

six categorical education variables (less than high school, high school, some college, college but no higher degree, those with a master's degree, and those with education beyond a master's degree, i.e., doctorate or professional degree). We started this new specification in the data for 1992 when new CPS educational coding permits this level of detail. Pre-1992 educational attainment measures remain as four categories.

To gauge the impact of the specification changes, we replicated the results from the earlier study and made changes to the specification, one at a time, before obtaining the results of the new specification focused on public school teachers and detailed education controls. **Figures A-A** and **A-B** plot the exponentiated coefficients from four different model specifications for, respectively, women and men:

1.) *Original*: replication of the results from Allegretto, Corcoran, and Mishel (2004);

2.) *Original with detailed education*: model #1 but with six exhaustive educational categories instead of four;

3.) *Original with public teachers*: controlling for public and private teachers separately with four educational categories;

4.) *Public teachers and detailed education*: controlling for public and private teachers separately with six educational categories—the specification in Chapter 2.

Moving from the book results from model 1 to model 2—which further disaggregated education with the addition of two educational level controls (split 'college-educated' into BA, MA, more than MA)—increased the differential as expected due to the greater proportion of teachers with more than a BA degree. The differential is greatly reduced as one compares model 2 to model 3. Again, this was expected, as we are now parsing out the separate effects of public versus private teachers. When separate coefficients were estimated for public and private teachers the differential on public teachers is greatly reduced while the differential on private teachers is increased (in negative value). Lastly, we compare model 3 to model 4. This specification combined the public/private split with the additional educational categories. As expected, this specification effectively increased the differential. The parameter shifts on teacher pay for the four models from 1992 to 2004 can clearly been seen in the figures.

Shifting from the original specification to the one employed in this study leads to a smaller estimated teacher wage disadvantage for 2006: 15.1% rather than 16.3% for all; 10.5% rather than 12.2% among women; and 25.5% rather than 25.6% among men. Although the *level* of the estimated teacher wage disadvantage is lower in the specification employed in this study, the estimated *trends* show a somewhat larger deterioration since 1996. Specifically, the estimated erosion of the teacher wage disadvantage between 1996 and 2006 reported in this study is 10.7 percentage points, which would have been 9.3 percentage points using the earlier studies specification.

Table B-1 Weekly wages of all workers, public school teachers, and non-teacher college graduates, 1979-2006

	All			Women			Men		
	All workers	*Public teachers*	*Non-teacher graduates*	*All workers*	*Public teachers*	*Non-teacher college graduates*	*All workers*	*Public teachers*	*Non-teacher college graduates*
Real ($2006)									
1979	$744	$821	$1,055	$553	$784	$753	$867	$891	$1,181
1980	736	804	1,040	555	765	759	855	878	1,167
1981	719	798	1,018	549	756	754	833	876	1,137
1982	702	784	992	544	746	747	809	854	1,108
1983	691	780	975	543	748	748	794	842	1,090
1984	694	808	977	549	769	762	793	883	1,092
1985	695	820	982	555	782	773	794	895	1,098
1986	708	842	1,028	564	803	795	812	925	1,156
1987	722	872	1,052	581	834	829	825	957	1,181
1988	725	888	1,055	588	847	835	825	973	1,184
1989	751	904	1,108	610	869	876	854	977	1,250
1990	752	913	1,110	615	885	882	853	974	1,254
1991	744	896	1,093	617	861	882	839	978	1,228
1992	739	896	1,090	621	865	893	827	971	1,219
1993	745	912	1,094	632	887	903	831	977	1,220
1994	-	-	-	-	-	-	-	-	-
1995	-	-	-	-	-	-	-	-	-
1996	753	948	1,098	641	917	922	839	1,030	1,221

1997	$757	$937	$1,102	$645	$917	$931	$843	$995	$1,223
1998	785	941	1,169	665	923	970	878	989	1,310
1999	815	950	1,212	687	930	999	916	1,002	1,364
2000	829	962	1,240	702	942	1,032	930	1,020	1,388
2001	840	947	1,249	713	930	1,038	940	990	1,401
2002	840	944	1,241	720	929	1,037	935	985	1,391
2003	850	961	1,250	731	934	1,046	943	1,026	1,403
2004	853	970	1,247	735	940	1,049	946	1,048	1,395
2005	853	947	1,246	741	930	1,051	939	990	1,396
2006	849	935	1,240	739	918	1,051	935	979	1,386

SOURCE: Authors' analysis of CPS data.

Table B-2 Weekly wages of public school teachers and non-teacher college graduates with bachelor's degrees, 1996-2006

	All		Women		Men		Teacher/non-teacher ratios		
	Public school teachers	*Non-teacher college graduates*	*Public school teachers*	*Non-teacher college graduate*	*Public school teachers*	*Non-teacher college graduate*	*All*	*Women*	*Men*
Real ($2006)									
1996	$833	$993	$811	$831	$898	$1,113	0.839	0.975	0.807
1997	828	1,006	819	846	855	1,125	0.823	0.968	0.760
1998	834	1,059	823	877	864	1,191	0.788	0.938	0.725
1999	832	1,098	814	903	880	1,241	0.758	0.901	0.709
2000	859	1,132	847	941	898	1,274	0.759	0.900	0.705
2001	844	1,139	832	947	874	1,285	0.741	0.879	0.681
2002	834	1,126	823	943	863	1,267	0.740	0.873	0.681
2003	851	1,138	822	945	921	1,285	0.748	0.870	0.717
2004	845	1,122	821	943	916	1,259	0.754	0.871	0.728
2005	840	1,123	834	943	859	1,266	0.748	0.885	0.678
2006	824	1,122	813	951	855	1,258	0.734	0.855	0.680
1996-2006	***-1.1%***	***13.0%***	***0.3%***	***14.4%***	***-4.8%***	***13.0%***			

SOURCE: Authors' analysis of CPS data.

Table B-3 Weekly wages of public school teachers and non-teacher college graduates with master's degrees, 1996-2006

			Women		Men		Teacher/non-teacher ratios		
	All public school teachers	*Non-teacher college graduates*	*Public school teachers*	*Non-teacher college graduates*	*Public school teachers*	*Non-teacher college graduates*	*All*	*Women*	*Men*
Real ($2006)									
1996	$1,105	$1,244	$1,079	$1,081	$1,163	$1,360	0.888	0.998	0.855
1997	1,083	1,244	1,054	1,078	1,158	1,364	0.871	0.978	0.849
1998	1,088	1,310	1,068	1,104	1,137	1,467	0.830	0.967	0.775
1999	1,112	1,360	1,087	1,149	1,177	1,523	0.817	0.946	0.773
2000	1,106	1,378	1,085	1,169	1,160	1,531	0.803	0.928	0.758
2001	1,091	1,384	1,076	1,166	1,134	1,543	0.789	0.922	0.735
2002	1,084	1,376	1,071	1,166	1,117	1,535	0.788	0.918	0.728
2003	1,105	1,381	1,092	1,184	1,133	1,542	0.800	0.922	0.735
2004	1,118	1,404	1,093	1,197	1,176	1,566	0.796	0.912	0.751
2005	1,071	1,397	1,054	1,194	1,113	1,566	0.767	0.883	0.711
2006	1,062	1,379	1,051	1,175	1,092	1,547	0.770	0.894	0.706
1996-2006	***-3.9%***	***10.9%***	***-2.6%***	***8.7%***	***-6.1%***	***13.7%***			

SOURCE: Authors' analysis of CPS data.

Table B-4 Public school teacher and college graduate weekly wages, by state

Weekly wages ($2006)

	(1) Public K-12 teachers			(2) Other college graduates			Ratios (1)/(2)		
	BA level	*MA level*	*Total**	*BA level*	*MA level*	*Total**	*BA level*	*MA level*	*Total**
National average	**$844**	**$1,098**	**$967**	**$1,133**	**$1,396**	**$1,260**	**74.5%**	**78.7%**	**76.7%**
Alaska	947	1,127	1,020	1,129	1,341	1,215	83.9	84.0	84.0
Alabama	675	873	782	1,076	1,202	1,145	62.7	72.6	68.3
Arkansas	706	905	769	1,002	1,121	1,039	70.5	80.7	74.0
Arizona	750	1,012	884	1,120	1,321	1,223	66.9	76.6	72.3
California	1,115	1,288	1,183	1,286	1,603	1,411	86.7	80.4	83.9
Colorado	735	971	855	1,145	1,426	1,288	64.2	68.1	66.4
Connecticut	867	1,351	1,264	1,324	1,596	1,547	65.4	84.7	81.7
District of Columbia	934	1,059	1,012	1,167	1,492	1,371	80.0	71.0	73.9
Delaware	887	1,180	1,026	1,131	1,380	1,249	78.4	85.5	82.1
Florida	808	1,009	892	1,008	1,243	1,106	80.2	81.2	80.6
Georgia	713	1,030	902	1,064	1,260	1,181	67.0	81.8	76.4
Hawaii	797	1,066	917	998	1,210	1,092	79.9	88.1	84.0
Iowa	734	1,062	835	955	1,180	1,024	76.8	90.0	81.5
Idaho	735	996	808	974	1,228	1,045	75.5	81.1	77.3
Illinois	821	1,165	993	1,141	1,474	1,308	72.0	79.0	76.0
Indiana	804	1,040	956	1,071	1,252	1,188	75.0	83.0	80.5
Kansas	673	914	778	1,040	1,213	1,115	64.7	75.3	69.7
Kentucky	698	935	867	991	1,105	1,072	70.4	84.7	80.9
Louisiana	712	741	723	1,007	1,105	1,044	70.6	67.1	69.2
Massachusetts	1,020	1,168	1,112	1,218	1,547	1,422	83.7	75.5	78.2
Maryland	872	1,205	1,078	1,248	1,533	1,424	69.9	78.6	75.7
Maine	744	907	800	980	1,121	1,029	75.9	80.9	77.8
Michigan	974	1,298	1,183	1,204	1,426	1,347	80.9	91.0	87.8
Minnesota	864	1,098	991	1,146	1,464	1,318	75.4	75.0	75.2

Missouri	$679	$920	$800	$1,008	$1,134	$1,071	67.4%	81.1%	74.7%
Mississippi	686	793	732	914	1,078	985	75.1	73.6	74.4
Montana	682	973	752	769	926	807	88.6	105.1	93.2
North Carolina	738	1,015	821	1,042	1,227	1,098	70.8	82.7	74.8
North Dakota	746	958	788	790	974	827	94.4	98.3	95.3
Nebraska	749	956	836	962	1,197	1,061	77.8	79.9	78.8
New Hampshire	858	1,031	933	1,192	1,419	1,291	71.9	72.7	72.3
New Jersey	1,145	1,440	1,268	1,330	1,603	1,445	86.0	89.8	87.8
New Mexico	767	927	839	967	1,264	1,101	79.3	73.4	76.2
Nevada	825	1,038	930	1,109	1,288	1,197	74.4	80.6	77.7
New York	865	1,284	1,195	1,169	1,477	1,411	74.0	86.9	84.7
Ohio	877	1,103	1,015	1,082	1,316	1,225	81.1	83.8	82.9
Oklahoma	679	749	699	931	1,101	981	72.9	68.0	71.3
Oregon	902	1,017	967	1,070	1,327	1,216	84.3	76.6	79.6
Pennsylvania	880	1,137	1,014	1,090	1,379	1,241	80.8	82.4	81.7
Rhode Island	1,009	1,311	1,180	1,110	1,361	1,252	90.9	96.3	94.2
South Carolina	714	932	843	1,015	1,148	1,094	70.4	81.2	77.0
South Dakota	654	924	757	849	1,048	925	77.0	88.2	81.9
Tennessee	755	853	799	1,075	1,354	1,199	70.3	63.0	66.6
Texas	789	903	818	1,159	1,356	1,208	68.1	66.6	67.7
Utah	846	1,006	889	1,032	1,260	1,093	82.0	79.8	81.3
Virginia	815	1,039	910	1,213	1,541	1,352	67.2	67.4	67.3
Vermont	809	1,097	959	949	1,088	1,022	85.2	100.8	93.9
Washington	885	1,068	1,010	1,195	1,355	1,304	74.1	78.9	77.5
Wisconsin	811	1,031	922	1,055	1,369	1,213	76.9	75.4	76.0
West Virginia	708	895	811	911	1,048	987	77.8	85.4	82.2
Wyoming	803	934	846	879	1,011	922	91.4	92.4	91.8

* Totals are weighted averages of the BA and MA level weekly wages where the weights are the shares of teachers with a BA or MA. This insures that the distribution of education among teachers and other college graduates does not affect the comparison.

SOURCE: Authors' analysis of weekly wages computed from the Current Population Survey ORG files averaged over the 2002-06 period and inflation-adjusted to 2006.

Endnotes

1. As Flyer and Rosen (1997) demonstrated, the average years of experience of teachers and non-teachers diverged during the 1970s and 1980s as student enrollment declined and the teaching force aged.
2. Teachers are defined as elementary and secondary school teachers who were not enrolled in school.
3. The model used with the PUMS data uses only four education categories as controls and does not distinguish between those who have a bachelor's degree and those with a master's or higher degree. As we discuss in Appendix A, teachers are more likely than non-teachers to have further education beyond a four-year college degree. When there are more education controls (distinguishing the college graduate group into BA only, MA, and professional/PhD), the relative wage of teachers falls between 1 and 2 percentage points.
4. The CPS-ORG provides a more accurate measure of weekly earnings than the March CPS, since respondents are asked specifically about their pay 'last week' in surveys before 1994 and for the most relevant pay period in recent surveys. In the March CPS, weekly earnings are calculated using annual wages and weeks worked in the prior year.
5. See http://www nctq.org/nctq/publications/debate.jsp.
6. It should be noted that our method of calculating this disadvantage for all teachers does control for the shares of teachers that have a bachelor's degree or have further education. That is, there is an education level control implicit in our calculation.
7. We limit the comparison with more refined education level comparisons, BA and MA, to the years starting in 1996 for which the new education coding that identifies highest degree attained (rather than years of schooling) in the CPS is available (starting in 1992) as well as years in which we can identify imputed data.
8. We present the exponentiated value of the coefficient less 1.
9. In practice this means having a dummy variable for public school teachers in the model along with a dummy variable for private school teachers.
10. The model specification for these estimates is the one used in the earlier study—having just four education controls. We use estimates for public school teachers. The CPS-ORG data before 1992 does not adequately allow for the finer education controls. Given the correspondence of the results using this model and our newer specification for computing trends in relative wages since 1996, we are confident in using this specification for the 1979-93 periods.
11. We use the same specification as in our CPS-ORG analysis. As we have noted, trends in relative wages in annual and weekly wage data will be comparable as long as there have not been shifts in the relative work time (in this case, weeks worked per year) of teachers and other college graduates.

12. Of course, the wage disadvantage faced by male teachers, 25.5% in 2006, far exceeds that faced by women teachers, 10.5%.

13. The wage ratio for all teachers, however, was calculated by weighting up the gender ratios according to the gender mix among teachers. This is done to keep the impact of each gender group comparable among teachers and those deemed comparable to teachers.

14. As discussed in Appendix A, we compute the change in the teacher relative wage in the ORG in the following manner. We add together three separate estimates. First we use the estimates of teacher relative wages for 1979 and 1993 and difference them. This covers the period before the 1994 CPS revisions and the period for which we can not identify imputed observations (1994 and 1995). We add the differences in the March CPS estimates in 1993 and 1996 to cover the period where the appropriate ORG data are not available. Last, we add the difference in the ORG teacher relative wage estimates between 1996 and 2005. Note that we do not rely on the levels of the teacher relative wage estimates in the ORG in the pre-1994 period where we believe the estimates may be too large because weekly wages were determined only by asking how much you earned last week, which probably did lead to an understatement of teacher weekly wages.

15. For this reason we do not compute any changes in teacher relative weekly wages for teachers by differencing a recent estimate with an estimate from any period before 1994. Rather, we assume the change in teacher relative pay in the pre-1994 data is accurately measured (implying that the bias did not change over this period) and add the changes from, for instance, 1979 to 1993 to the changes from 1996 to 2006. As discussed above, the weekly wage data can not provide estimates for 1994 and 1995 (imputed wages are not identified), so we bridge these two series by adding the changes in teacher relative wages in the annual wage data for the 1993-96 period.

16. See Appendix A for details of this calculation.

17. For example, the AFT in its annual survey of salaries compares teacher salaries to those of accountants, buyers, attorneys, computer systems analysts, engineers, and university professors.

18. Podgursky (2003) questions the AFT's choice of comparable occupations, asking "where, one wonders, are the comparisons with journalists, registered nurses, district attorneys, FBI agents, military officers and other not-so-highly compensated professionals and public-sector employees?" Podgursky (2003) presents occupational comparisons for occupations that require a bachelor's degree, ignoring the fact that public school teachers are far more likely to have, and be required to have, a further degree.

19. In this analysis of comparable occupations our definition of K-12 teachers includes 'special education' teachers as well as elementary and secondary teachers. The addition of special education teachers in this analysis does not affect the results materially and reflects a definition of teachers that we used in the first stages of our work. We also make the comparisons with all teachers, rather than public school teachers, to facilitate comparisons of the prior results to these new results.

20. In this point system the weighting of factors is based on the assumptions built into the factor evaluation system, but they generally reflect market valuations to some extent. Note that no points are given for the 10th factor, supervision, which is not part of the federal system.

21. Each of Pierce's coefficients are an estimate of the wage premium associated with being in an occupation requiring that particular skill level, relative to an occupation with the lowest level of that skill (all else held constant). For example, the coefficient estimate on "complexity level 4" of 0.096 suggests that a worker in an occupation requiring a complexity level of four can expect to receive 9.6% higher wages than a worker in an occupation with complexity level one, all else equal. The largest wage premia are for occupations requiring high levels of knowledge, little oversight, high complexity, and supervisory responsibilities.

22. More details on the selection procedure are provided in Allegretto, Corcoran, and Mishel (2004).

23. In 2002, teacher wages relative to the old and new groups are, respectively, -12.2% and -12.0%. The change in the teacher relative wage between 1983 and 2002 was reported in the earlier study as a 7.6% decline when using the 'old' group of occupations and is a 8.3% decline using the 'new' group.

24. As with the other CPS-ORG analyses, we have no data for 1994 and 1995 and substitute the March annual wage trends in this analysis for those years.

25. Podgursky (2003, 73-4), for instance, introduces his discussion of fringe benefits by saying "Neither AFT nor the NEA makes any adjustments for the fringe benefits associated with teaching in a public school, thus masking an important part of total compensation."

26. The earliest data can be found in U.S. Department of Labor (2000) Bulletin 2526; U.S. Department of Labor, 2003, USDL:03-760. Unfortunately, there are no detailed teacher compensation data available from the ECEC series for years before 1994.

27. The ratio of W-2 wages to direct wages is 1.130 for professionals and 1.072 for K-12 teachers. So, any analysis of a differential between teachers and professionals using a 'direct wages' measure would seriously understate the total wage advantage of professionals—by 5.7 percentage points.

28. Compare, for instance, the benefit share of compensation for a worker with $10,000 of benefits annually and a nine-month salary of $60,000 with a year-round worker with the same annual benefits and a salary of $80,000 (which is the same salary per month as the part-year worker). The summer off generates a different benefit share of compensation by 3 percentage points in this example.

29. Teachers are also more likely to have defined-benefit pensions, which are superior to the defined-contribution plans provided to many professionals (in part because workers bear the risks rather than employers). Our analysis only focuses on employer costs and does not take qualitative differences in benefits into account.

30. The National Education Association's analysis of state pension plans (NEA 2006; http://www.nea.org/takenote/images/char2006.pdf) reviews the Social Security coverage of about 100 state plans in their survey. They report that benefits, assets per active member, and assets per active and retired member are lower (higher) in the 70% (30%) of plans where "most" or "all" participants are covered by Social Security. Obviously, employers that do not contribute to Social Security end up having higher pension contributions. That is why retirement costs, pensions, and Social Security must be examined together. States that have at least one state-level pension plan with "none/few" or only "some" participants covered by Social Security are Alaska, California, Colorado, Connecticut, District of Columbia, Georgia, Illinois, Kentucky, Louisiana, Massachusetts, Missouri, Nevada, Ohio, Rhode Island, and Texas.

31. See the earlier study for details. Basically, we assume a negative teacher wage differential of 14% (i.e., teachers earn 14% less in wages than other professionals) and compute what the comparable differential would be for total compensation, given the data in Table 7—the ratios of compensation to wages. The difference between the wage differential and the compensation differential is the "benefits bias."

32. For instance, the hourly compensation in the fourth quarter of 2004 was $38.52, far below the hourly compensation in the *prior* quarter at the end of 2003 of $42.65.

33. This section draws heavily from articles by Richard Rothstein and Lawrence Mishel in *Phi Delta Kappan* in June and September 2007.

34. This chapter draws heavily on Corcoran and Mishel (2007), available at http://epsl.asu.edu/epru/ttreviews/EPSL-0702-229-EPRU.pdf, and Lawrence Mishel, "Jay Greene's Persistent Misuse of Data for Teacher Pay Comparisons" at http://www.epi.org/content.cfm/webfeatures_viewpoints_teacher_pay_comparisons.

35. That is, in the first stage of the sample selection employers are chosen at random. Then, in a second stage employees (or more precisely, occupations or jobs) are selected for the collection of detailed wage data.

36. Appendix A ("Technical Note") (2006, August). *National Compensation Survey: Occupational Wages in the United States, June 2005*. U.S. Bureau of Labor Statistics. Retrieved Feb. 11, 2007, from http://www.bls.gov/ncs/ocs/sp/ncbl0832.pdf.

37. Appendix A ("Technical Note") (2006, August). *National Compensation Survey: Occupational Wages in the United States, June 2005*. U.S. Bureau of Labor Statistics. Retrieved Feb. 11, 2007, from http://www.bls.gov/ncs/ocs/sp/ncbl0832.pdf.

38. U.S. Bureau of Labor Statistics (1994, Aug. 24), BLS Bulletin 2444. Retreieved Feb. 11, 2007, from http://www.bls.gov/ebs/sp/chp2sl.txt. Quoted on page 4 of Greene and Winters (2007).

39. U.S. Bureau of Labor Statistics (2006, July). *National Compensation Survey: Occupational Wages in the United States, June 2005 Supplementary Tables.* Retrieved Feb. 11, 2007, from http://www.bls.gov/ncs/ocs/sp/ncbl0831.pdf. All of the following calculations use mean annual hours from Supplementary Table 4.2 and mean weekly hours from Supplementary Table 4.1. We perform similar calculations in Allegretto, Corcoran, and Mishel (2004).

40. See also the survey results in Drago et al. (1999).

41. See National Center for Education Statistics (undated) *Overview, Schools and Staffing Survey*. Retrieved Feb. 11, 2007, from http://nces.ed.gov/surveys/sass/

42. The samples for these computations are full-time college graduates.

43. For instance, if teachers earn $50,000 in a year but report their weekly wage as $50,000 divided by 52, then they report a weekly wage of $962, about 18% less than the $1,176 weekly wage computed for a 42.5 week work year (paid days and scheduled days). If 85% of teachers do not work by teaching in the summer, then the maximum bias is about 16% (85% of 18%).

44. These data are based on the average for the 49 districts that had data in the NCTQ data base. The specific way NCTQ refers to the data are: "How many teachers days are on the 2006-2007 calendar (excluding paid holidays)?"

45. See http://www.bls.gov/ncs/ebs/sp/ebsm0004.pdf, Tables 20 and 21 for data on private sector holidays and vacations.
46. In practice this means having a dummy variable for public school teachers in the model along with a dummy variable for private school teachers.

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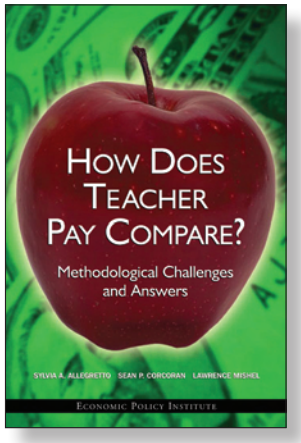

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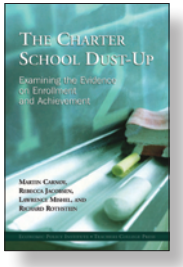

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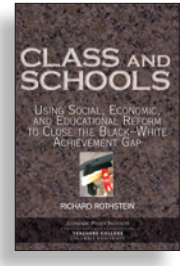

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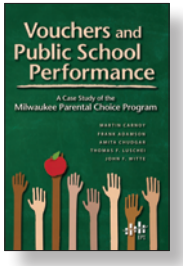

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