EXAMINING THE LINK BETWEEN TEACHER WAGES AND STUDENT OUTCOMES: THE IMPORTANCE OF ALTERNATIVE LABOR MARKET OPPORTUNITIES AND NON-PECUNIARY VARIATION

Susanna Loeb and Marianne E. Page*

Abstract—Researchers using cross-sectional data have failed to produce systematic evidence that teacher salaries affect student outcomes. These studies generally do not account for non-pecuniary job attributes and alternative wage opportunities, which affect the opportunity cost of choosing to teach. When we employ the methodology used in previous studies, we replicate their results. However, once we adjust for labor market factors, we estimate that raising teacher wages by 10% reduces high school dropout rates by 3% to 4%. Our findings suggest that previous studies have failed to produce robust estimates because they lack adequate controls for non-wage aspects of teaching and market differences in alternative occupational opportunities.

I.   Introduction

DISCUSSIONS about school policy often focus on the relationship between school resources and student performance, but empirical studies of this relationship have failed to produce systematic evidence that added resources lead to improvements in standard measures of student achievement. In comprehensive summaries of empirical research, Hanushek (1986, 1997) finds that student outcomes are not consistently related to either teacher salaries or per pupil expenditures. Using national, longitudinal data sets, recent papers by Grogger (1996), Betts (1995), and Altonji (1988) have produced similar results.

These findings are at odds with a handful of studies that indicate that schools and teachers matter. Altonji (1988), for example, finds that schools may account for up to 8% of permanent wage variance, and a recent analysis by Hanushek, Kain, and Rivkin (1998) suggests that at least 7% of the variance in students' test scores may be explained by variation in teacher quality. Several recent studies have also produced evidence of a relationship between measurable teacher characteristics and student outcomes. Ferguson (1991), for example, finds that in Texas teacher performance on a statewide certification exam is positively related to student outcomes, and Ehrenberg and Brewer (1994) find that the selectivity of the college a teacher attends positively influences test score growth.

These findings add a new dimension to the puzzle: if teacher quality affects student achievement, then why do studies that regress measures of student outcomes on teacher wages produce such weak results? In the broader labor market, Murnane, Willett, and Levy (1995) have shown that one measure of worker ability—individual test scores—is positively associated with wages, but studies of this relationship in the teacher labor market produce mixed results.[1] One ostensible explanation for the lack of an empirical relationship is that teacher wages are unrelated to teacher quality, because school district administrators are unable to identify the most qualified teachers from the pool of teacher applicants. The problem with this explanation is that, even if school districts are unable to identify teacher quality, one would expect the supply of high-ability teachers to increase with teacher wages. Therefore, even with random selection from the pool of potential teachers, the average quality of teachers should increase as the opportunity cost associated with becoming a teacher falls.

An alternative explanation is that teacher wages do matter, but that the empirical approaches employed to assess these effects have not adequately isolated the elasticity of interest. For example, even if districts can attract more-skilled teachers by offering higher wages, it may be difficult to identify a positive relationship between wages and student outcomes using cross-sectional variation across school districts, because different school districts face different teacher supply curves in quality-wage space, due, for example, to cross-sectional differences in non-pecuniary job attributes. Most existing studies use cross-sectional variation to identify the effect of teacher wages.

In this study, we consider the potential importance of controlling for alternative labor market opportunities and non-pecuniary school district characteristics when trying to assess the degree to which teacher wages affect student outcomes. If these factors influence the average quality of teachers and are correlated with wages, then failure to incorporate them into regression analyses will make it difficult to identify the existence of teacher wage effects. We develop an empirical model of the relationship between teacher wages and student outcomes, measured by students'

[1] Ballou and Podgursky (1997) find that relative wages have no effect on the SAT scores of teachers, but Figlio (1997) finds that, within local labor markets, there exists a significant positive relationship between teacher salaries and teacher quality, measured by undergraduate college selectivity and subject matter expertise. In another paper, Figlio (1996) controls for district-specific effects and finds that raising teacher salaries increases the probability of hiring more-qualified teachers only in non-unionized districts. Manski (1987) finds that salaries do not affect the ability distribution of new teachers but that they do affect the size of the teaching pool, and Murnane et al. (1991) find that increased salaries increase teacher tenure.

educational attainment, that incorporates non-wage attributes associated with teaching together with alternative labor market opportunities. When we estimate this model we produce statistically significant, robust estimates of the effects of teacher wages on high-school dropout rates and college attendance rates, which suggest that raising the wages of teachers by 50% will reduce high-school dropout rates by more than 15% and increase college enrollment rates by approximately 8%. We also employ the methodology used in previous studies and are able to replicate their results. Our comparison suggests that previous studies have failed to find robust effects because they have not adequately controlled for interdistrict variation in the non-pecuniary aspects of teaching and market differences in alternative occupational opportunities.

In the next two sections, we discuss the most common empirical models that have been used to estimate teacher wage effects, outline our empirical approach, and compare it to the approaches used in existing studies. Section IV provides descriptive statistics on the evolution of teacher wages over time, which helps clarify why it may be important to control for alternative wage opportunities. In sections V and VI, we present the results of our analysis of the effect of teacher wages on student outcomes. In the final section, we conclude with policy implications.

II. A Brief Overview of the School Production Function Literature

Hanushek's 1986 and 1997 summaries demonstrate that empirical assessments of teacher wage effects have mostly failed to provide evidence that teacher wages matter. Only nine of the sixty teacher salary studies cited in his 1986 paper, for example, produced wage coefficient estimates that were both positive and statistically significant. One interpretation of the literature is that teacher wages are unrelated to student outcomes. Another possibility is that teacher wages appear to be unimportant because the empirical strategies that have been employed to assess their effects miss some important features of the teacher labor market. Most of the empirical models that have been used to estimate teacher wage effects are based on permutations of the following equation:

$$Y_{is} = \alpha + \beta_1 W_s + Z_s\beta_2 + X_{is}\beta_3 + \epsilon_{is} \qquad (1)$$

where Y_{is} is an outcome measured for student i who attends school s (usually the student's test score), W_s is the log of the average wage paid to teachers at school s, Z_s is a vector of factors common to all students attending school s (such as the average socioeconomic composition of enrollees), and X_{is} is a vector of other factors that are specific to student i (such as family background). Assuming that teacher salaries impact student outcomes because of their effect on teacher quality, $\hat{\beta}_1$ can be interpreted as an estimate of the slope of the supply curve for teachers in quality-wage space.

Equation (1) is typically estimated using cross-sectional data so that $\hat{\beta}_1$ is identified from variation in both outcomes and salaries across schools (or school districts) at a point in time. Relying only on cross-sectional variation, however, may produce misleading estimates. The most commonly acknowledged problem is that, because parents often choose schools based on their perceived quality, teacher wages are endogenous. Parents with a high demand for education quality will spend more on teachers but may also educate their children more at home. This type of endogeneity is likely to bias wage estimates upward.[2] Researchers have invested a great deal of energy into overcoming this problem, and we will return to this issue shortly.

Because this source of endogeniety is likely to bias estimates of β_1 upward, it cannot account for the fact that most studies have not produced evidence that teacher wages matter. Two additional factors have been largely ignored in the literature, and they might lead to underestimates of teacher wage effects. First, like other workers, teachers are likely to care about both the pecuniary and non-pecuniary returns to teaching. Holding all else equal, we would expect to find that districts that pay higher wages are able to attract higher-quality teachers, but all else is not equal. Non-pecuniary job characteristics such as school safety, the length of the school year, and the level of parental involvement, also vary across school districts, and this muddies the degree to which cross-district variation in teacher wages represents variation in the opportunity cost associated with teaching in a particular district. In addition, federal and state governments provide targeted funds for low-income schools with relatively low-achieving students (see Cullen (1997) for evidence of this trend in Texas schools), and these funds may be used to supplement teacher wages. If cross-district differences in non-pecuniary characteristics produce compensating differentials, then estimates of teacher wage effects that do not control for these characteristics will suffer from a negative omitted-variables bias. Antos and Rosen (1975), Kenny and Denslow (1980), and Levinson (1988) provide evidence that compensating differentials for teachers may be substantial, particularly in urban areas. Even the most comprehensive data sets provide limited information on non-wage attributes, and, therefore, most cross-sectional estimates of β_1 may be subject to this bias.[3]

The second factor that might lead to underestimates of teacher wage effects is that existing analyses rarely account for teachers' alternative wage opportunities. If school districts that pay teachers high salaries are located in areas in which wages paid to other professionals are also relatively

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[2] When schools are locally financed, parents can also directly affect teacher wage levels through their support of local property taxes. This phenomenon will also lead to estimates of teacher wage effects that are biased upward.

[3] The Schools and Staffing Survey contains substantial information on school characteristics, but it does not contain information on student outcomes. Also, an exceptional data set based on Texas schools includes substantial district attribute information as well as student measures. Using this data set, Ferguson (1991) finds significant and robust teacher wage effects.

high, then the geographic variation in teacher wage levels that is typically used to identify teacher wage effects may not accurately reflect geographic variation in the opportunity cost associated with choosing to teach. Many estimates of school production functions are based on cross-sectional data from a single state and will inherently control for alternative labor market opportunities if a state comprises one labor market.[4] But, if a state has multiple markets or if researchers use data gathered from multiple states, then studies should also address the possibility that alternative job opportunities vary across these markets.[5] The teacher wage variables that have been used in multiple-state studies are not typically measured relative to other workers' wages, and therefore may be unreliable indicators of the true variation in the opportunity cost of teaching. (See Ehrenberg and Brewer (1994), Jencks and Brown (1975), and Ribich and Murphy (1975) as examples.)[6] As a result, estimates of β_1 will be biased.

Figure 1 illustrates how failure to account for these factors compromises our ability to estimate the magnitude of teacher wage effects. What we wish to estimate is β_1, the slope of the supply curve in quality-wage space, but variation in non-wage attributes and non-teaching opportunities across school districts implies that different districts face different supply curves. Because data from a cross section of school districts provides only one observation point on each supply curve, the best-fit regression line through these observations may be substantially different from the true slope.
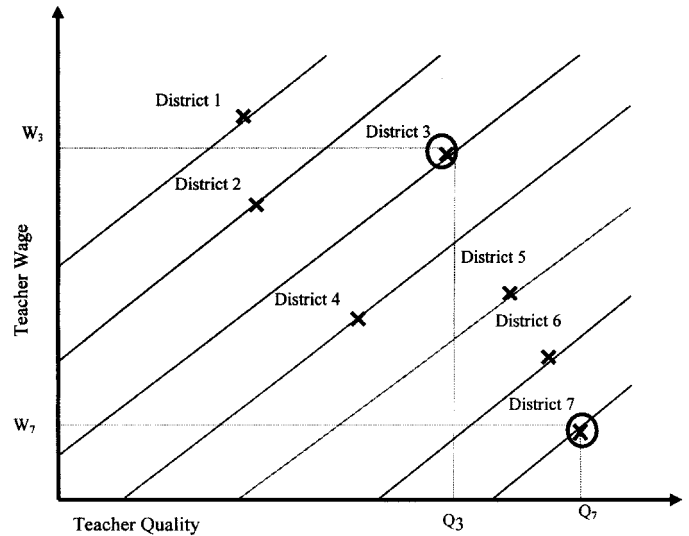
The purpose of this study is to control for variation in non-pecuniary attributes and alternative wage opportunities so that the relationship between teacher wages and student outcomes can be identified by variation in demand along a single supply curve. We do this by using panel data to control for cross-sectional differences in non-wage characteristics, and by replacing teacher wages with the wages of teachers relative to the wages of other college-educated workers in the surrounding area. Although the endogeneity of teacher wages is not the focus of our study, we will also address this estimation problem by applying a two-staged, least-squares procedure using a novel instrument, which can be exploited only because our wage measures take alternative labor market opportunities into account.

FIGURE 1.—THE IMPACT OF SUPPLY DIFFERENCES ACROSS DISTRICTS ON THE ESTIMATED WAGE-QUALITY RELATIONSHIP

III. Accounting for Non-Pecuniary Job Attributes

One way of controlling for cross-sectional differences in non-pecuniary attributes is to make use of variation across multiple dimensions. Panel data allow us to include district- or state-level fixed effects, which capture everything, including non-wage characteristics, that are constant over time. A number of recent studies (Hoxby, 1996; Heckman, Layne-Farrar, & Todd, 1995; Card & Krueger, 1992; Andrews, Fayissa, & Tate, 1991) have made use of panel data to identify the effect of wages on student outcomes, and all produce estimates that are positive and statistically significant.

None of these studies fully address the issues raised in section II, however, and so it would be premature to generalize their results. For example, Hoxby uses real average teacher salaries rather than relative salaries and, thus, does not control for changes in alternative job opportunities that are not correlated with district-specific trends, such as those produced by local economic shocks.[7] The studies by Andrews et al. and Card and Krueger do use relative wage measures, but the Andrews et al. study is based on data from a single state and the Card and Krueger study focuses on birth cohorts from the 1920s, 1930s, and 1940s, so their results may not be applicable to the current U.S. population. Additionally, the Card and Krueger study, which uses state-level data, includes only a few state-specific, time-varying characteristics. Two characteristics that are omitted from their regressions but that are correlated with both teacher wages and student outcomes are the poverty rate and the fraction of the state's population who are immigrants. Several researchers have suggested that Card and Krueger's results are driven by omitted-variables bias

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[4] See Beiker and Anschel (1973), Borland and Howsen (1992), Cohn (1968), Deller and Rudnicki (1993), Dolan and Schmidt (1987), Dynarski, Schwab, and Zampelli (1989), Ferguson (1991), Fowler and Walberg (1991), Keisling (1967), Raymond (1968), Sander (1993), Sander and Krautmann (1991), Sebold and Dato (1981), and Stern, 1989.

[5] See Akin and Garfinkel (1977), Altonji (1988), Betts (1995), Dynarski (1987), Ehrenberg and Brewer (1994), Grogger (1996), Jencks and Brown (1975), Johnson and Stafford (1973), Morgenstern (1973), Perl (1973), Ribich and Murphy (1975), and Wachtel (1976). While a number of these papers (Akin and Garfinkel, Dynarski, Johnson and Stafford, Morgenstern, and Wachtel) use data over multiple years, they do not include state controls in their analyses and are thus largely identifying using cross-state variation.

[6] Expenditure studies also need to adjust for alternative labor markets because wages make up such a large portion of operating costs.

[7] The focus of Hoxby's paper is somewhat different from our own. Hoxby's analysis centers on the effect of teachers' unionization on school inputs and student performance.

which may be affected by their use of state-level data. Betts (1995), Grogger (1996), Hanushek, Rivkin, and Taylor (1996), and Loeb and Bound (1996) have all shown that omitted-variables biases may be exacerbated when state-level data are used instead of district-level data, although, in theory, aggregation could either lessen or exacerbate this bias.

Although it is encouraging that estimates based on panel data are statistically significant and in the expected direction, it is unclear whether the difference in results between these studies and earlier cross-sectional analyses is due to anomalies associated with the recent studies or to systematic differences in approach. Our goal is to shed light on these questions by directly comparing cross-sectional and panel data estimates and by directly comparing estimates based on real versus relative wage measures. We also consider the possibility that Card and Krueger's results are driven by aggregation biases by applying an IV procedure.

IV.   Wage Trends: A Descriptive Analysis

In order to illustrate why it may be important to include alternative wage opportunities when estimating teacher wage effects, we begin by looking at national wage trends among teachers and non-teachers. We base our descriptive analysis on a sample of women from the 1964–1995 March Current Population Surveys (CPS) files who had completed sixteen or more years of education[8] and had worked full time[9] during the previous year. Figure 2 plots real annual earnings and figure 3 plots weekly wages for female teachers and non-teachers from 1963 through 1994. We focus on women here because throughout this period, approximately 70% of elementary and secondary school teachers were women. Weekly wages are calculated in the usual way, by dividing annual wage and salary income by the number of weeks worked.[10,11]

Both teachers' and non-teachers' wages exhibit a downward trend during the 1970s, followed by increases in the 1980s. Throughout these years, wages for teachers remain above the average wage of non-teachers. However, the premium associated with teaching decreases substantially over time. At the beginning of the period, the average weekly teacher wage was $610 (in 1994 dollars) and the average weekly wage of non-teachers was $500. Thirty

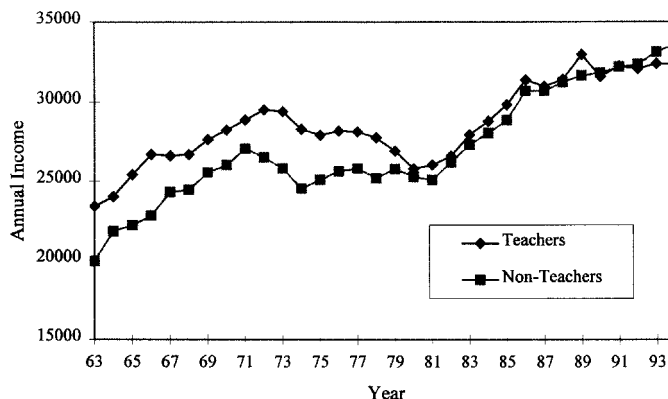Figure 2.—Mean Annual Wage and Salary Income (1994 dollars)

Figure 3.—Mean Weekly Wages (1994 dollars)

years later, wages for the two groups are virtually the same.[12]

Trends in the education and experience of teachers relative to other college graduates exacerbate the relative decline in teacher wages. Figures 4 and 5 depict the change in potential work experience[13] and in the percentage of workers with advanced degrees. Beginning in the early 1970s, both the proportion of teachers with advanced degrees and teachers' average experience levels began to increase at a faster rate than comparable measures for non-teachers. In 1963, for example, the fraction of teachers with advanced degrees was below that for non-teachers; by 1994, the proportion of teachers with advanced degrees was 20% higher than the proportion of non-teachers with advanced degrees.

Even a cursory analysis of these figures suggests that, over time, real teacher wage levels have exhibited substantively different trends from the ratio of teacher wages to non-teacher wages. The diagrams indicate that, although the

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[8] Beginning in 1992, the focus of the education question in the CPS changed from years of education to degree receipt. Our sample of women from 1992 forward, therefore, includes only women who indicated that they had graduated from college.

[9] The women in our sample may have worked either part of the year or for the full year.

[10] Prior to 1975, the weeks-worked variable was clustered. We use the average weeks worked for respondents in each cluster over the years with complete data as our estimate for respondents in the years with clustered data. We do this separately for teachers and non-teachers.

[11] Teachers' wages and salaries include income from non-teaching jobs as well.

[12] Female teachers' annual earnings in 1963 were $23,415 (1994 dollars), whereas female college graduates in other professions had annual earnings of $19,942. Thirty years later, annual earnings of non-teachers had surpassed those of teachers: $33,994 to $32,369.

[13] Potential experience is defined as age minus years of education minus six.

FIGURE 4.—TRENDS IN POTENTIAL WORK EXPERIENCE

FIGURE 5.—TRENDS IN THE PERCENTAGE OF WORKERS WITH ADVANCED DEGREES
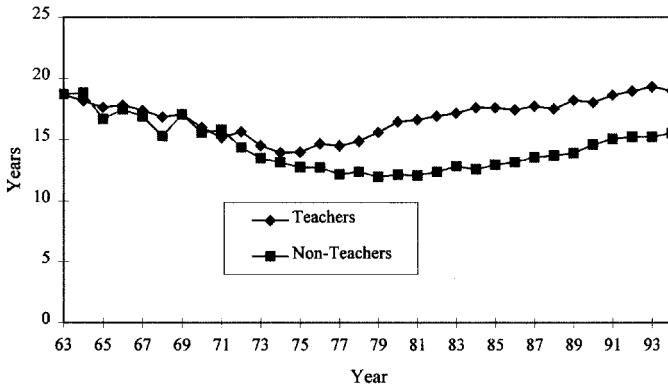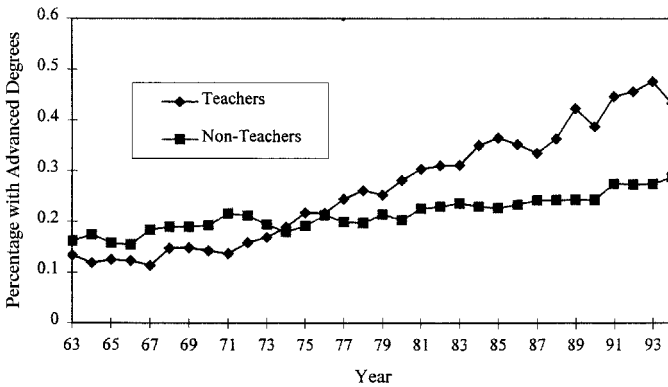
real wages of teachers grew substantially during the period, the actual opportunity cost associated with teaching (in terms of the foregone wage) increased. This provides evidence that using real wage levels in studies of school input effects is likely to lead to inaccurate estimates of the true effect of wages on teacher quality and student outcomes.

V. Empirical Approach

Essentially, our goal is to ascertain the slope of the supply curve for teachers in quality-wage space. The slope of the supply curve tells us the extent to which districts can improve student outcomes (through improving the quality of their teachers) by raising wages. Because real wage variation across school districts reflects variation in both the supply and demand for teachers, estimates that rely on cross-sectional data or that fail to account for alternative wage opportunities are likely to be misleading.

A. Basic Specification

Our analysis is based on the following empirical model:

$$Y_{s(t+10)} = \beta_{0(t+10)} + \beta_1 W_{st} + X_{s(t+10)}\beta_2 + Z_{st}\beta_3 \\ + \eta_s + \mu_s T + \epsilon_{s(t+10)} \quad (2)$$

where $Y_{s(t+10)}$ is a measure of student outcomes in state s at time $(t + 10)$,

W_{st} is the log of the wage for teachers in state s at time t,

$X_{s(t+10)}$ is a vector of control variables specific to state s at time $(t + 10)$,

Z_{st} is a vector of control variables specific to state s at time t, and

the error term has three components: a state-specific, time-constant component, η_s; a state-specific, constant-time-trend component, μ_s (T identifies time); and a state-specific, time varying component, $\epsilon_{s(t+10)}$.

We regress student outcomes on teacher wages ten years earlier because it takes time for wage changes to lead to higher average teacher quality. Even if the quality of new district hires is very responsive to wage changes, tenured teachers who entered teaching under a different wage regime will have little incentive to leave teaching when wages are increased, and this will result in slow turnover. We also use ten-year wage lags because the effects of teacher quality may be greater for students at younger ages (Keisling, 1967).

Our estimates are based on state-level data. Individual-, classroom-, or school-level data may be preferable to more-aggregate data when school inputs other than teacher wages are of interest because micro data can provide more-accurate information than aggregate data on the resources that are available to a particular student. For example, the classroom-level student/teacher ratio is a more accurate reflection of the resources available to a given student than is the district or state average of this variable. Wages differ from other school inputs, however, because wage scales are set at the district level and, thus, exhibit no independent variation at the individual, classroom, or school level. Within-district analyses of teacher wage effects will capture only differences in teacher experience and education.

The school district might, therefore, appear to be the logical level of aggregation for our analysis. We choose to use state-level data instead, because very little information on either district-level teacher quality or district-level student outcomes is available for the nation as a whole. The measures that are available are unreliable. For example, district-level dropout rates can be estimated for a particular age group using census data, but, because many dropouts switch district of residence after dropping out, this measure is likely to be inaccurate. In addition, district-level data on teacher wages are available nationally for only independent school districts, which eliminates all districts in Maryland, North Carolina, Tennessee, and Virginia, as well as many districts in other states. The data that are available for dependent districts is of questionable quality.[14] Because of these problems, we choose to use data at the state level. One advantage of state-level data is that it may help reduce the endogeneity problems mentioned earlier if intrastate location decisions are more affected by school quality than are

[14] Sheila Murray has done extensive work in this area.

interstate location decisions (an assumption that seems reasonable given that roughly three-quarters of young adults live in the same state in which they grew up).[15]

As mentioned in section III, however, using more-aggregate data has a potential drawback. If the true model includes an unobserved state component that is correlated with teacher wages, then failure to include this component in the regression analysis will lead to coefficient estimates on teacher wages that are biased.[16] Empirical estimates suggest that aggregation magnifies this bias. In addition to including numerous time-varying, state-specific control variables, we address this potential problem by first-differencing equation (7), which eliminates static unobserved state characteristics, and by including time and state dummies, which capture time-specific factors and state-specific trends. These controls alone may not be adequate, so we will also use a two-staged, least-squares analysis. Finally, we supplement our main analysis with an analysis based on district-level data from California, to further demonstrate that our results are not driven by the use of state-level data.

B. Non-pecuniary Attributes

We control directly for constant non-pecuniary attribute differences across states by first-differencing equation (2):

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\begin{aligned}
Y_{s(t+10)} - Y_{st} = {} & (\beta_{0(t+10)} - \beta_{0t}) + \beta_1(W_{st} - W_{s(t-10)}) \\
& + (X_{s(t+10)} - X_{st})\beta_2 \\
& + (Z_{st} - Z_{s(t-10)})\beta_3 \\
& + \mu_s + (\epsilon_{s(t+10)} - \epsilon_{st}).
\end{aligned}
\tag{3}
$$

We control for state-specific time trends (including non-pecuniary attributes that change at a constant rate) by including dummy variables for each state. We also include a number of control variables that vary across states and over time to help adjust for changes in job attributes that are not correlated with the time trends. If these control variables sufficiently capture variation in non-pecuniary attributes, then we will be able to identify the relationship between teacher wages and teacher quality by using (unobserved) variation in demand to trace out the supply curve in quality-wage space.

C. Alternative Opportunities

We incorporate alternative labor market opportunities into our empirical model by including relative wage measures. Although some studies use relative wages to more accurately reflect the opportunity cost associated with teaching,

we have found virtually nothing in the literature that discusses the appropriate means for constructing these measures.[17] To be useful, relative wage measures must be based on an appropriate comparison group. Three factors are particularly important to consider: the contribution of non-pecuniary job attributes to workers' utility, the effect of experience and education on individual productivity, and the extent of gender differentiation in the labor market.

If all workers care substantially more about wages than they do about other job attributes, an individual's rank in the distribution of wages and in the distribution of productivity will be identical. We could then measure changes in teacher quality using changes in the average rank of teacher wages in the total wage distribution. Because non-wage attributes are likely to influence job choice, however, the relationship between skill and wages in a cross section may be quite weak and, therefore, teachers' rank is likely to be an uninformative measure of quality. But, even if no relationship exists between wages and quality in a cross section, as long as job attributes remain relatively constant, jobs with faster-growing wages will attract increasingly higher-quality workers. We, therefore, use changes in the ratio of average teacher wages to average non-teacher wages for college graduates as our relative wage measure. This is not a perfect measure, because job attributes are likely to change over time and because the quality of the pool of college graduates may differ both across labor markets and over time. We attempt to control for changes in attributes both implicitly, through the inclusion of state time trends, and explicitly, through the inclusion of additional variables.[18]

The second factor to consider is the effect of experience and education. If the only characteristic that the market rewards is skill (which may or may not be increased through education or experience), simple wage comparisons across groups of workers will provide us with information about differences in skill across groups. Given that teacher wage scales are based on education and experience independent of

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[15] This fraction was calculated using PSID data that were compiled for another project. The young adults were between the ages of thirty and forty in 1992, and their state of residence in 1992 was compared to their state of residence in 1968.

[16] If most of the variation in the omitted variable is within states, then aggregating is likely to reduce the bias due to the variable's omission, but, if most of the variation in the omitted variable is between states, then the aggregation will aggravate the bias.

[17] Three recent papers use different measures of teachers' relative wages to document changes in the opportunity cost that is associated with becoming a teacher. Flyer and Rosen (1994) use CPS data from 1967–1980 to calculate the earnings of teachers relative to all college graduates. They find that relative teacher wages increased during the period, but that this was due to increases in teacher experience and education. When they impute the expected wage for teachers holding sex, education, race, metropolitan status, and experience constant, they find that teacher relative wages fell over time, especially during the late 1970s and early 1980s. Ballou and Podgursky (1997) use the ratio of average teacher pay for both men and women to a gender-weighted average of the earnings of all college-educated workers, and find that in most states relative teacher wages rose by more than 10%. Finally, Hanushek and Rivkin (1997) estimate opportunity costs as the proportion of non-teachers with earnings less than average teacher earnings, and find that the percentage of women college graduates earning less than teachers fell from a high of 68.7% in 1940 to a low of 45.3% in 1990, with most of the decline occurring before 1970. Although some of this change was due to an increase in the age and education of non-teachers, the majority was due to pure wage declines for teachers.

[18] The relative wage will not be a good measure of teacher quality if the wages of non-teachers are changing over time as a result of changes in the average quality of non-teachers. We would like to investigate this possibility in future work.

teacher skill, however, wage comparisons that ignore education and experience will provide misleading information about the true opportunity cost of teaching. School input studies have not typically adjusted relative wage measures to account for these factors, but figures 4 and 5 suggest that they may be important.[19] We create both simple relative wage measures and relative wage measures that adjust for education and experience levels.[20]

We also consider the importance of gender in the labor market for teachers. Because women have comprised the substantial majority of the teaching force throughout the period covered by this study,[21] and because the labor market for college graduates was characterized by institutionalized gender differentiation, our relative wage measures are constructed for women only.

VI. Estimation of Teacher Wage Effects

A. Data

Our regression analysis utilizes a state-level panel data set that we create from the 1960–1990 Public Use Microdata Samples (PUMS). Unlike the CPS, the PUMS data provide us with adequate sample sizes at the state level. Our first dependent variable is the log of the state high-school dropout rate.[22] We define the dropout rate as the percentage of all 16–19 years olds living in the state who are not currently attending high school and do not have a high-school diploma.[23] Because this measure surely includes individuals who conducted the bulk of their education in another state, we create a second measure that includes only those residents who were born in the state in which they are observed at the time of the survey. The disadvantage of this latter measure is that we lose individuals who may have migrated to the state prior to obtaining most of their education.

We also run regressions using the college attendance rate as our dependent variable. We recognize that, relative to dropout rates, the college attendance rate is more likely to be affected by the migration of individuals across states, but we want to demonstrate that our results are not an artifact of one particular measure of student outcomes. We define the college attendance rate as the percentage of 19–20 year olds who have either attended thirteen or more years of school (census years prior to 1990) or who have obtained some schooling beyond completing their high-school diploma (1990 census). Again, we create this variable both for all residents and for only those born in-state.

We would like to be able to look at student test scores in addition to measures of educational attainment, but consistent achievement measures are not available nationally over this period.[24] We do know that, across states, the correlation between 1990 high-school dropout rates and 1992 National Assessment of Educational Progress average math scores is approximately 0.8. Even if no correlation existed between educational attainment and test scores, the well-established relationship between individuals' educational attainment and future earnings justifies an interest in the outcomes used here.

We use three different measures of teacher wages in our analyses: the log of the real teacher wage, the unadjusted relative wage (which is simply the coefficient on a teacher dummy variable in a univariate log wage equation), and the adjusted relative wage (which is created in the same way as the unadjusted relative wage, except that the wage equation includes a binary variable for whether the worker has an advanced degree, and measures of potential experience (defined as age minus years of education minus six) and potential experience squared).[25] Our wage sample is restricted to include workers ages 20–64 who indicated that they had completed college and were working at least 26

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[19] In fact, determining whether to adjust for education and experience is complicated by the possibility that these characteristics may increase productivity in the market for alternative occupations but not in the teacher labor market. If, for example, the type of education and experience that teachers have does not contribute to their productivity in the non-teacher labor market, but the type of education and experience that non-teachers have does contribute to their productivity in the non-teacher labor market, then the correct comparison would be between the adjusted teacher wage and the unadjusted non-teacher wage (essentially comparing teachers to other workers with no experience or additional education).

[20] The best experience measure that we have is potential experience, which will be a noisy measure of teacher tenure. As a result, the coefficient estimates on this variable are likely to be biased downward.

[21] The fraction of the teaching force that are women has remained fairly constant over time: the fraction was 68.7% in 1961, and 72.1% in 1991.

[22] We have chosen to use logs because the distributions of our variables in level form are skewed. Moreover, in our context, the logarithmic specification, which relates percentage changes, may be preferred to one that relates absolute changes. For example, it is probably easier for policymakers to lower the dropout rate from 20% to 19% than from 2% to 1%. Our results are similar whether we use logs or levels, however: using levels, we find that a 50% increase in teacher wages is associated with a decrease in the dropout rate of approximately 1.5 percentage points and an increase in the college enrollment rate of 4.3 percentage points.

[23] The education questions in 1960–1980 census surveys focus on years of education, rather than degree receipt. We classify respondents to these surveys as high-school graduates if they indicated that they had completed twelve years of education. If an individual attended high school through the twelfth year, but did not complete that grade and is not currently in school, then he or she is classified as a dropout. In the 1990 census, individuals are asked specifically about whether they have graduated from high school.

[24] One referee has suggested that we try using SAT test scores as a dependent variable. SAT scores are available by state beginning in 1972 and can be adjusted for cross-state differences in the fraction of students who take the SAT (thereby implicitly controlling for the fact that the average SAT score reflects the inclusion of students from a lower part of the ability distribution when more students take the test). In a single year, the adjusted SAT score has been shown to be positively correlated with a state's average NAEP test score, and it is also positively correlated with our dropout measures. Changes in SAT scores are not positively correlated with changes in the dropout rate, however. (We do not have state-level NAEP scores over multiple years, so we cannot examine this correlation.) It is, therefore, no surprise that changes in SAT scores are unrelated to changes in relative teacher wages. We suspect that the adjustment simply does not work as well over time as it does in a cross section.

[25] Because the educational attainment of teachers is more likely to be endogenous than our measure of experience, we also created a relative wage measure adjusted only by experience and experience squared. The results of analyses using this measure are similar to those presented below for the unadjusted and adjusted relative wage.

weeks during the relevant year.[26],[27] We run separate regressions for each state/year cell for 1960 through 1980, and use the estimated coefficient on the teacher dummy as our estimate of relative wages for a given state in a particular year. This provides us with 150 observations for each relative wage variable.[28]

Our time-varying controls for attribute differences include state demographic variables such as the state unemployment rate, state median income, the percentage of individuals below the poverty line, and the percentage of individuals who are immigrants. The vector also includes variables more directly related to the market for teachers: the number of teachers in the state, school enrollment, the percentage of the state population that lives in urban areas, and three variables that characterize the degree to which teachers are unionized.[29],[30] In addition, we include the demographic variables measured contemporaneously with the dropout rate so we can get a clean estimate of the effect of wages on that portion of the dropout rate that is not influenced by contemporaneous economic conditions.

Table 1 provides the means and standard deviations of the variables used in the regression analysis. The dropout rate decreased steadily over our period, as did its variance across states. Using our first dropout rate measure, the standard deviation across states in 1959 is 0.061. By 1989, the standard deviation had dropped to 0.025. Over the same period, real teacher wages increased by approximately 20%, but teacher wages relative to the wages of other college graduates fell by approximately 12%.

B. Results

Cross-sectional Analysis: In order to place our results in the context of existing studies, we begin by looking at estimates based on cross-sectional regressions. Table 2 displays the coefficient estimates for single-year regressions (1969, 1979, and 1989) of educational attainment on the real teacher wage ten years earlier.[31] Model (I) includes no additional covariates, while model (II) includes all of the control variables listed above. The estimates produced by these regressions are similar to those summarized by Hanushek (1986, 1997): in the univariate regression, all but two of the twelve coefficient estimates are positive and most are significant, but—once additional variables are introduced into the model—there is no evidence that teacher wages affect student outcomes. Not a single coefficient is both in the expected direction and statistically distinguishable from zero. This is consistent with the literature to date, which finds scant evidence of teacher wage effects once family background characteristics are controlled for. Thus far, most studies of school production functions have focused on adjusting for the bias that arises from the omission of these variables, rather than the bias that may arise from improperly using cross-sectional variation in teacher wages to proxy for variation in the opportunity cost of teaching.

Table 3 displays the cross-sectional results using our two relative wage measures. Again, the univariate regression produces estimated coefficients that are all in the expected direction, and, again, these estimates are affected by the addition of controls. They are not affected to the same

TABLE 1.—MEANS AND STANDARD DEVIATIONS OF ANALYSIS VARIABLES

	1959	1969	1979	1989
Log real wage	6.122	6.301	6.226	6.315
	(0.140)	(0.103)	(0.103)	(0.116)
Unadjust relative wage	0.305	0.260	0.279	0.227
	(0.138)	(0.106)	(0.087)	(0.081)
Adjusted relative wage	0.294	0.272	0.237	0.160
	(0.164)	(0.115)	(0.090)	(0.098)
Dropout rate (all residents)	0.209	0.143	0.136	0.133
	(0.061)	(0.044)	(0.036)	(0.025)
Dropout rate (those born in-state)	0.200	0.138	0.133	0.105
	(0.071)	(0.051)	(0.037)	(0.027)
College enrollment rate (all residents)	0.281	0.456	0.425	0.561
	(0.068)	(0.091)	(0.055)	(0.057)
College enrollment rate (those born in-state)	0.265	0.437	0.393	0.523
	(0.076)	(0.091)	(0.058)	(0.065)
Unemployment rate	5.194	3.568	5.486	5.118
	(1.695)	(0.858)	(1.320)	(1.329)
Log median income	16.920	17.229	17.305	17.298
	(0.199)	(0.157)	(0.120)	(0.168)
Percentage poor	23.950	13.226	12.656	13.330
	(10.956)	(5.592)	(3.558)	(4.305)
Percentage immigrants	0.039	0.041	0.053	0.069
	(0.022)	(0.033)	(0.043)	(0.061)
Log teacher number	9.718	10.155	10.251	10.333
	(0.976)	(0.980)	(0.970)	(0.955)
Log enrollment	12.982	13.283	13.190	13.151
	(1.015)	(1.003)	(0.992)	(0.987)
Percentage urban	62.994	66.812	67.654	68.830
	(15.627)	(15.065)	(15.136)	(15.345)
Collective bargaining	0.041	0.490	0.857	
	(0.200)	(0.505)	(0.354)	
Agency shop	0.000	0.041	0.245	
	(0.000)	(0.200)	(0.434)	
Strike	0.000	0.020	0.143	
	(0.000)	(0.143)	(0.354)	

* Note: The numbers in parentheses are standard deviations.

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[26] We classify as college graduates in the 1960–1980 census surveys those who have completed sixteen years of education. In the 1990 census, respondents identify themselves as recipients of bachelor's degrees.

[27] This is a different comparison group than the one used in our CPS analysis because, across surveys, PUMS does not have consistent measures of hours worked.

[28] Washington, D.C., is included, but Alaska is not.

[29] The unemployment rate, median income, percentage poor, and percentage urban variables are from *Statistical Abstracts of the United States*. The number of teachers and students are from *Digest of Education Statistics*. We create our immigrant variable from census data. The data on unions are from the NBER data set created by Freeman and Valletta (1988). The three measures we use are a dummy variable for whether the state allows collective bargaining, a dummy variable for whether the state permits agency shops, and a dummy variable for whether strikes are permitted.

[30] We have also run all of our regressions including variables that characterize the racial composition in each state (measured contemporaneously both with the dependent variable and with a ten-year lag). The inclusion of these variables had virtually no effect on our results, nor did the variables exhibit an independent effect on any of the dependent variables.

[31] The weighting technique is similar to that described below for the difference estimations. In this case, our estimates of the error term come from an OLS regression of the inverse of the sample size on the squared residual. There is little difference between the weighted and unweighted results.

TABLE 2.—ESTIMATED REAL TEACHER WAGE COEFFICIENTS BASED ON CROSS-SECTIONAL REGRESSIONS

	Dropout Rate		Dropout Rate (Born In-State)		College Enrollment		College Enrollment (Born In-State)	
	Model (I)	Model (II)	Model (I)	Model (II)	Model (I)	Model (II)	Model (I)	Model (II)
1969	−0.656	−0.163	−0.938	−0.125	0.473	0.175	0.614	0.152
	(0.299)	(0.298)	(0.322)	(0.255)	(0.144)	(0.128)	(0.211)	(0.183)
1979	−0.586	0.723	−0.561	1.138	0.280	−0.225	0.121	−0.303
	(0.281)	(0.408)	(0.313)	(0.413)	(0.145)	(0.207)	(0.189)	(0.186)
1989	0.558	−0.391	0.323	−0.570	0.026	0.237	0.054	0.379
	(0.281)	(0.422)	(0.335)	(0.461)	(0.130)	(0.208)	(0.246)	(0.214)

Note: The numbers in parentheses are White's standard errors. Model (I) has no controls. Model (II) includes all time-variant, state-specific controls. Sample size is fifty.

degree as the real wage estimates, however, and most estimates remain in the expected direction. Consider the adjusted relative wage, for example: all but one of twelve point estimates produced by model (II) suggest that the wage has a positive effect on student outcomes. None of the 1969 or 1979 estimates are significant at conventional levels, however.

Finally, the cross-sectional results show evidence of a time trend. The magnitude of the estimated effects is quite a bit larger for 1989 than for the earlier years. The estimates are in the expected direction and significant at conventional levels. Most previous studies of teacher wage effects have used data from the 1970s and early 1980s, however, and our results for those years are consistent with the failure of

earlier research to find robust evidence that teacher wages matter. It is puzzling that the magnitude and significance of our estimated teacher wage coefficients change so much between 1979 and 1989, but we do not believe that this change is driving the first-differences estimates that we present below, because, when we run our first-differences regression using student outcomes from 1969–1979 only (wage measures from 1959–1969)—our estimates are not statistically or substantively different from those produced using the full sample.

Differenced Analysis—Dropout Rates: Next, consider the results produced by the first-differences analysis. Table 4 presents the estimated effect of teacher wages on our first definition of the dropout rate. Model (I) includes only year dummies, model (II) includes year and state dummies, and model (III) adds the time-varying controls. We weight these regressions to account for heteroskedasticity in our estimates of the dropout rate.[32]

In all three panels of table 4, the estimated coefficients on both the unadjusted and the adjusted relative wage (columns (2) and (3)) are negative and statistically significant. The results from the most inclusive model indicate that raising the wage by 10% would decrease the dropout rate by 3%. If we compare the results of model (I) to the first panel in tables 2 and 3, we can see that differencing reduces the estimated wage effect, indicating that part of the wage effect found in the univariate cross-sectional analysis may be attributed to state-fixed effects. One possible explanation for this result is that states with preferable attributes also have higher wages. An alternative is that an underlying state characteristic, such

TABLE 3.—ESTIMATED RELATIVE TEACHER WAGE COEFFICIENTS BASED ON CROSS-SECTIONAL REGRESSIONS

	Model (I)		Model (II)	
	Unadj. Relative	Adj. Relative	Unadj. Relative	Adj. Relative
Dropout rate				
1969	−0.616	−0.729	0.207	−0.041
	(0.365)	(0.219)	(0.129)	(0.188)
1979	−0.445	−0.682	−0.456	−0.496
	(0.383)	(0.339)	(0.264)	(0.231)
1989	−1.187	−1.387	−0.951	−1.078
	(0.398)	(0.355)	(0.351)	(0.336)
Dropout rate (born in-state)				
1969	−0.805	−0.831	0.167	−0.011
	(0.379)	(0.270)	(0.132)	(0.174)
1979	−0.387	−0.611	−0.314	−0.363
	(0.407)	(0.373)	(0.293)	(0.259)
1989	−1.516	−1.631	−1.187	−1.303
	(0.455)	(0.399)	(0.381)	(0.372)
College enroll.				
1969	0.412	0.387	−0.016	0.067
	(0.147)	(0.097)	(0.078)	(0.091)
1979	0.104	0.172	0.064	0.054
	(0.175)	(0.149)	(0.139)	(0.120)
1989	0.210	0.233	0.411	0.433
	(0.166)	(0.177)	(0.174)	(0.174)
College enroll. (born in-state)				
1969	0.403	0.339	−0.158	−0.073
	(0.205)	(0.170)	(0.119)	(0.108)
1979	−0.009	0.042	0.129	0.081
	(0.193)	(0.180)	(0.167)	(0.146)
1989	0.495	0.500	0.593	0.614
	(0.205)	(0.205)	(0.184)	(0.187)

Note: The numbers in parentheses are White's standard errors. Model (I) has no controls. Model (II) includes time-variant, state-specific controls. Sample size is fifty.

[32] Because we use an estimate of the dropout rate (or college attendance rate) as our dependent variable, the actual equation that we estimate is
$$\hat{Y}_{s(t+10)} - \hat{Y}_{s(t)} = (\beta_{0(t+10)} - \beta_{0(t)}) + \beta_1(W_{s(t)} - W_{s(t-10)}) + (X_{s(t+10)} - X_{s(t)})\beta_2 + (Z_{s(t)} - Z_{s(t-10)})\beta_3 + \mu_s + (\epsilon_{s(t+10)} - \epsilon_{st}) + (\varpi_{s(t+10)} - \varpi_{st}),$$
where $\hat{Y} = Y + \varpi$. Differences in sample sizes across states and over time suggest that ϖ will be heteroskedastic. We, therefore, run a weighted regression, using estimates of the variance of $[(\epsilon_{s(t+10)} - \epsilon_{st}) + (\varpi_{s(t+10)} - \varpi_{st})]^{-1}$ as our weights. Our estimates of the variance of $[(\epsilon_{s(t+10)} - \epsilon_{st}) + \varpi_{s(t+10)} - \varpi_{st}]^{-1}$ are derived in two steps. First, we estimate the equation above using OLS. We then use the squared residuals from this regression as the dependent variable in a second OLS regression that includes $[(1/N_{s(t+10)}) + (1/N_{st})]$ (the sum of the inverse of the sample sizes in each year) as the only independent variable. The estimated coefficient on this variable, multiplied by the variable itself, provides us with an estimate of the variance of $\varpi_{s(t+10)} - \varpi_{st}$, while the estimated intercept provides us with an estimate of the variance of $\epsilon_{s(t+10)} - \epsilon_{st}$. Differences between the weighted and unweighted estimates are minor.

TABLE 4.—REGRESSION ESTIMATES OF THE EFFECT OF TEACHER WAGES ON DROPOUT RATES TEN YEARS LATER (DEPENDENT VARIABLE = CHANGE IN LN DROPOUT RATE)

	Model (I)			Model (II)			Model (III)		
	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative
Δ Relative wage	−0.565	−0.351	−0.316	−0.513	−0.465	−0.374	−0.073	−0.256	−0.301
(t − 10) − (t)	(0.209)	(0.157)	(0.131)	(0.240)	(0.131)	(0.106)	(0.178)	(0.073)	(0.061)
Δ Unemployment							−0.041	−0.041	−0.046
(t) − (t + 10)							(0.014)	(0.013)	(0.012)
Δ Immigrants							6.637	7.120	6.629
(t) − (t + 10)							(2.888)	(2.816)	(2.764)
Δ Median income							0.150	0.077	−0.045
(t) − (t + 10)							(0.321)	(0.259)	(0.253)
Δ Percentage poor							0.023	0.017	0.014
(t) − (t + 10)							(0.008)	(0.007)	(0.007)
Δ Unemployment							0.031	0.031	0.029
(t − 10) − (t)							(0.011)	(0.011)	(0.011)
Δ Percentage urban							0.009	0.005	0.007
(t − 10) − (t)							(0.010)	(0.011)	(0.010)
Δ Immigrants							−1.693	−1.895	−1.904
(t − 10) − (t)							(1.009)	(0.911)	(0.925)
Δ Median income							−0.787	−0.748	−0.799
(t − 10) − (t)							(0.332)	(0.327)	(0.331)
Δ Percentage poor							0.013	0.010	0.010
(t − 10) − (t)							(0.005)	(0.004)	(0.004)
Δ Teachers							−0.006	−0.072	−0.084
(t − 10) − (t)							(0.187)	(0.181)	(0.174)
Δ Enrollment							−0.290	−0.106	−0.119
(t − 10) − (t)							(0.174)	(0.164)	(0.151)
Δ Collective barg.							0.032	0.034	0.025
(t − 10) − (t)							(0.025)	(0.024)	(0.023)
Δ Agency shop							−0.006	−0.023	−0.046
(t − 10) − (t)							(0.050)	(0.049)	(0.049)
Δ Allowed strikes							0.045	0.025	0.008
(t − 10) − (t)							(0.059)	(0.055)	(0.056)
1969–79	0.342	0.1763	0.2032	0.332	0.177	0.208	0.881	0.760	0.805
	(0.067)	(0.034)	(0.034)	(0.072)	(0.029)	(0.029)	(0.093)	(0.081)	(0.075)
Intercept	−0.295	−0.248	−0.265	−0.357	−0.308	−0.334	−0.271	−0.252	−0.260
	(0.028)	(0.019)	(0.020)	(0.051)	(0.059)	(0.065)	(0.052)	(0.044)	(0.043)
State controls?	no	no	no	yes	yes	yes	yes	yes	yes
R-squared	0.299	0.289	0.287	0.448	0.480	0.466	0.843	0.857	0.870

Note: The numbers in parentheses are White's standard errors. Model (I) includes a year dummy only. Model (II) adds state dummies. Model (III) adds time-variant, state-specific controls. N = 100.

as interest in education, increases teacher wages but also increases student outcomes. A comparison across the three tables also shows that differencing reduces the standard-error estimates. This reduction could result from non-pecuniary attribute differences across states which make cross-sectional wage variation a noisy measure of the total utility associated with choosing to teach.

Moving across table 4 from left to right, we see that controlling for state-specific time trends raises the point estimates slightly. States that raise wages may be compensating for a decline in the attractiveness of teaching. Conversely, the inclusion of the time-varying controls tends to decrease the estimated effect of teacher wages and to reduce the corresponding standard-error estimates. This may occur because the inclusion of time-varying controls more completely adjusts for non-pecuniary attribute differences across states, making our wage measures in model (III) better estimates of the true opportunity cost of teaching. The results from model (III) suggest that longitudinal studies that omit time-varying factors may overestimate the impact of teacher wages.

The estimated coefficients on changes in the unemployment rate, immigration levels, and poverty levels are all statistically significant at conventional levels, and the R^2 nearly doubles when these variables are included. We find that the signs on the unemployment rate and immigration coefficients differ depending on whether they are contemporaneous with teacher wages or with student dropout rates. The estimates suggest that an increase in the contemporaneous unemployment rate of one percentage point will reduce dropout rates by nearly 5%, but that an increase in the unemployment rate ten years earlier of one percentage point will increase dropout rates by 3%. One interpretation of these results is that students are more likely to continue their schooling when alternative labor market opportunities are scarce, but that growing up during a recession has a negative impact on educational attainment. Increases in the percentage poor (both lagged and contemporaneous) are associated

TABLE 5.—ESTIMATES OF THE EFFECT OF TEACHER WAGES ON OTHER STUDENT OUTCOMES TEN YEARS LATER

Student Outcomes	Model (I)			Model (II)			Model (III)		
	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative
Δ Dropout rate (born in-state)	−0.538	−0.185	−0.271	−0.513	−0.437	−0.386	0.155	−0.197	−0.282
	(0.211)	(0.188)	(0.151)	(0.233)	(0.139)	(0.109)	(0.209)	(0.103)	(0.079)
Δ College enroll.	0.444	0.227	0.174	0.469	0.298	0.201	0.150	0.164	0.165
	(0.114)	(0.075)	(0.069)	(0.113)	(0.070)	(0.057)	(0.074)	(0.031)	(0.027)
Δ College enroll. (born in-state)	0.498	0.238	0.206	0.494	0.338	0.259	0.028	0.155	0.150
	(0.150)	(0.088)	(0.077)	(0.128)	(0.085)	(0.069)	(0.102)	(0.044)	(0.038)

Note: The numbers in parentheses are White's standard errors. Model (I) includes a year dummy only. Model (II) adds state dummies. Model (III) adds time-variant, state-specific controls. Sample size is 100.

TABLE 6.—ESTIMATES OF THE EFFECT OF TEACHER WAGES ON STUDENT OUTCOMES TEN YEARS LATER: NON-SOUTHERN STATES

Student Outcomes	Model (I)			Model (II)			Model (III)		
	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative
Δ Dropout rate	−0.646	−0.310	−0.283	−0.868	−0.456	−0.360	−0.357	−0.325	−0.359
	(0.277)	(0.176)	(0.137)	(0.257)	(0.142)	(0.105)	(0.214)	(0.076)	(0.050)
Δ Dropout rate (born in-state)	−0.371	−0.202	−0.215	−0.678	−0.373	−0.346	−0.186	−0.193	−0.352
	(0.315)	(0.197)	(0.169)	(0.294)	(0.158)	(0.113)	(0.254)	(0.114)	(0.074)
Δ College enroll.	0.366	0.186	0.137	0.524	0.256	0.159	0.241	0.125	0.137
	(0.132)	(0.077)	(0.070)	(0.141)	(0.079)	(0.064)	(0.069)	(0.026)	(0.019)
Δ College enroll. (born in-state)	0.232	0.166	0.153	0.375	0.269	0.210	0.196	0.085	0.110
	(0.161)	(0.081)	(0.068)	(0.154)	(0.092)	(0.071)	(0.057)	(0.025)	(0.025)

Note: The numbers in parentheses are White's standard errors. Model (I) includes a year dummy only. Model (II) adds state dummies. Model (III) adds time-variant, state-specific controls. Sample size is 68.

with higher dropout rates. The coefficient estimates on school inputs other than the relative teacher wage are typically insignificant.

The importance of controlling for alternative labor market opportunities, even in the differenced analysis, is underscored when we compare the results using our relative wage measures to the results based on real wage levels. While the coefficient on the real wage is significant and in the expected direction in the univariate regressions, this pattern does not hold up to the inclusion of time-varying controls. In contrast, the relative wage measures, especially the adjusted relative wage, are only mildly affected by the inclusion of these variables.

It is useful to know whether our relative wage results are driven by changes in the teacher wage or in the non-teacher wage. A simple way to assess the effects of the two components of our relative wage measure is to enter them separately in the regression. When all controls are included, the magnitude of the point estimates for teacher and non-teacher wages are strikingly similar but with opposite signs.[33] This result provides evidence both that the teaching wage and the wage of alternative occupations affect students' attainment and that the use of relative wage measures is appropriate.

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[33] For the dropout rate, the estimated coefficient on the teacher wage is −0.32 while that on the non-teacher wage is 0.48. The standard errors are 0.32 and 0.16, respectively. For the college enrollment outcome, the corresponding coefficients are 0.25 and −0.20 with standard errors of 0.14 and 0.09.

Other Outcomes: The results corresponding to our three additional definitions of student attainment are presented in table 5. Much like the estimates in table 4, these results imply that a 10% increase in wages would reduce dropout rates for natives of the state by approximately 3%. Our estimates suggest that this improvement in teacher compensation would increase college attendance by 1.6%. Moving across the panels of table 5, we see patterns that are similar to those discussed above. Controlling for alternative labor market opportunities and other factors that are correlated with teacher wages affects the magnitude and precision of our estimates.

VII. Specification Tests

In order to investigate the robustness of our results, we perform three specification tests.

A. Southern States

Because substantial changes in the quality of education were occurring in the South during the period we analyze, one possible concern is that our results are driven by the North/South convergence of both school inputs and student outcomes. We investigate this possibility in table 6, which shows the results of estimating our equations using only the 34 northern states. We find teacher wage effects that are similar in magnitude and significance to those based on the full sample.

TABLE 7.—FIRST-STAGE, INSTRUMENTAL-VARIABLES ESTIMATES

Student Outcomes	Model (I)			Model (II)			Model (III)		
	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative
Coefficient	0.176	−0.795	−0.947	0.164	−0.814	−0.979	0.071	−0.907	−0.988
Standard error	(0.115)	(0.149)	(0.161)	(0.173)	(0.221)	(0.236)	(0.19)	(0.239)	(0.286)
Partial R-squared	0.013	0.324	0.387	0.008	0.243	0.296	0.001	0.217	0.217

Note: The numbers in parentheses are White's standard errors. Model (I) includes a year dummy only. Model (II) adds state dummies. Model (III) adds time-variant, state-specific controls. Sample size is 100.

B.   Two-Staged, Least-Squares Analysis

Our biggest concern is that, even though we include numerous time-varying controls, we may not be adequately adjusting for all of the factors that are collinear with wages and that influence student outcomes. As noted earlier, the biases resulting from this omission may be aggravated when more-aggregate data (such as data measured at the state level) are used. To systematically assess potential endogeneity and omitted-variable problems, we apply two-stage least squares using as our instrument the change in log wages for non-teaching women college graduates.

This instrument is highly correlated with changes in our relative wage measure because it is, essentially, the denominator of our relative wage measure. Although this is a nontraditional instrument choice, it does allow us to isolate substantial variation in the wage measure that is not subject to the endogeneity and omitted variables bias that we are concerned about. In particular, this instrument is unlikely to lead to upward-biased wage estimates. Because the non-teacher wage is negatively correlated with the relative teacher wage, an upward-biased estimate will result only if there are omitted variables that are both positively (negatively) correlated with the non-teacher wage and negatively (positively) correlated with student outcomes. Although it is not difficult to think of factors that might be positively correlated with both non-teacher wages and students' educational attainment (such as parents' socioeconomic status), it is difficult to think of omitted variables that would lead to a wage estimate that is biased upward. The inclusion of state time trends as well as median income, the poverty rate, and the state unemployment rate should remove much of the correlation between our instrument and the error term, but, if any omitted variables remain, the IV estimates will be conservative. Before proceeding with the 2SLS analysis, we enter the instrument into our differenced equation to check for correlation with the error term. The estimated coefficients are never conventionally significant, which suggests that the non-teacher wage of female college graduates is an exogeneous instrument.[34]

Table 7 reports the coefficient on the instrument—the real wage of non-teaching female college graduates—for all first-stage regressions. When the wage of non-teachers increases by 10%, the relative wage of teachers falls by just under 10%. The partial R^2 is over 0.2 for both the adjusted and unadjusted wage. This provides evidence that our instrument is strongly correlated with our explanatory variable, and we do not need to be concerned that our IV estimates will display an inconsistency of the type discussed by Bound, Jaeger, and Baker (1995).

Table 8 displays the results for the structural equations. The IV strategy yields point estimates for the full model that depict a greater effect of wages on student outcomes than those in tables 4 and 5. The coefficients on adjusted relative wage, for example, are −0.454 and −0.432 for the dropout rates, indicating that a 50% increase in wages corresponds to more than a 20% decrease in the dropout rate. For college enrollment, a 50% increase in the wage corresponds to between a 9% and 13% increase in the college enrollment rate. Although we can not reject the possibility that these estimates are equal to the estimates in tables 4 and 5, table 8 provides evidence that our earlier estimates are not biased upward by the use of an endogenous regressor. In fact, it may be the case that these biases are in the other direction, as would be the case if compensating differentials were at work.

Table 8 also provides real wage estimates using our IV strategy. These estimates are smaller than those in tables 4 and 5, which is expected, because non-teachers' real wage changes are not a good instrument for changes in teachers' real wages. If districts raise teacher wages more when the wages of female college graduates increase, the instrument will actually pick up wage changes that are negatively associated with changes in the opportunity cost of teaching.

Although the potential endogeneity of teacher wages is unlikely to carry over to non-teacher wages, it is possible that, when wages of women college graduates increase, female high-school students have more of an incentive to remain in school, which, in turn, reduces dropout rates and increases college enrollment.[35] Because our instrument is negatively correlated with the relative wage measures, we would expect the potential bias resulting from this effect to be towards finding no impact of teacher wages on student outcomes. Thus, the true effect of teacher wages on student outcomes may be greater than our previous point estimates suggest.

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[34] For example, using our first dropout measure and the adjusted relative wage, the coefficient on the non-teacher wage is −0.18 (0.26 standard error) in model (I), 0.13 (0.36) in model (II), and 0.23 (0.27) in model (III).

[35] Recall, however, that, when our instrument was included directly in our regressions, we found no evidence that it was correlated with the dependent variable.

TABLE 8.—INSTRUMENTAL-VARIABLES ESTIMATES OF THE EFFECT OF TEACHER WAGES ON STUDENT OUTCOMES

Student Outcomes	Model (I)			Model (II)			Model (III)		
	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative	Real	Unadj. Relative	Adj. Relative
Δ Dropout rate	1.358	− 0.301	− 0.253	3.125	− 0.628	− 0.523	6.328	− 0.494	− 0.454
	(1.820)	(0.280)	(0.229)	(4.256)	(0.319)	(0.253)	(17.47)	(0.218)	(0.189)
Δ Dropout rate (born in-state)	0.793	− 0.176	− 0.148	2.815	− 0.566	− 0.471	6.029	− 0.471	− 0.432
	(1.659)	(0.306)	(0.250)	(3.875)	(0.350)	(0.265)	(16.51)	(0.273)	(0.236)
Δ College enroll.	− 0.164	0.036	0.031	− 0.805	0.162	0.135	− 2.540	0.198	0.182
	(0.706)	(0.141)	(0.120)	(1.292)	(0.103)	(0.098)	(7.335)	(0.087)	(0.082)
Δ College enroll. (born in-state)	0.052	− 0.011	− 0.010	− 1.168	0.235	0.195	− 3.642	0.284	0.261
	(0.738)	(0.167)	(0.140)	(1.504)	(0.170)	(0.136)	(9.871)	(0.137)	(0.133)

Note: The numbers in parentheses are White's standard errors. Model (I) includes a year dummy only. Model (II) adds state dummies. Model (III) adds time-variant, state-specific controls. Sample size is 100.

In order to investigate this issue, we run our 2SLS regression using outcome measures constructed for men only, adding the change in the wages of male college graduates as a control and using the change in the female college-graduate wage as the instrument. When we use the adjusted relative wage measures and include all of our control variables, we find that the estimated effect of teacher wages on the log of the high-school dropout rate is − 0.59 (with a standard-error estimate of 0.19), which suggests that dropout rates would fall by more than 5% if teacher wages were increased by 10%. The larger estimate appears to result in part from the fact that male students are more responsive to changes in teacher quality; the estimated effect of teacher wages on male high-school dropout rates is larger for men than for the sample as a whole (− 0.42, with a standard-error estimate of 0.12), even before applying IV.[36]

C. District-Level Analyses

The 2SLS results provide evidence that our estimates are not driven by omitted-variables or endogeneity bias. Because the concern with state-level data is that it exacerbates these biases, we infer from these results that our use of state-level data is appropriate. However, as an additional specification check, we run an abbreviated version of our analysis using district-level data from California. As discussed in section IV, reliable district-level data are not available nationally over time, but we were able to obtain administrative data on teacher wages and high-school dropout rates for most of the unified districts in California starting with teacher salary information in 1976 and district dropout rate data in 1986.[37] (These are the earliest years for which these measures are available.) We merge these data with 1995 California school district dropout rates taken from the Common Core of Data to create a two-year panel data set that includes teacher salary information from 1976 and 1986 together with student dropout rates for 1986 and 1995. One

nice feature of this data set is that it includes information on both average teacher salaries and the salary typically paid to teachers with different levels of education and experience.

This data set (like the other less-aggregate data sets we considered and rejected) is less than ideal for our study for several reasons. To begin with, the data set goes back to only 1976 and it contains very little information on the types of time-varying attributes that we included in our state-level analyses. Because our main results are driven mostly by the inclusion of state-fixed effects and do not hinge on the inclusion of geographic-specific time trends or additional control variables, we believe that these drawbacks are not serious enough to impede a simple comparison with the state-level results. In addition, the dropout rates for 1986 are defined differently than those from the 1995 Common Core of Data. Because of this difference, we cannot be sure that variation in our outcome is driven by district changes rather than definitional changes. Finally, it is difficult to find an appropriate measure of a teachers' alternative wage for California districts during this period. We average the wages of female college graduates across the 1970 and 1980 decennial census to produce a relative wage measure for 1976 and, across the 1980 and 1990 decennial census to produce a relative wage measure for 1986. In addition to the potential problems that may arise from averaging, the geographic identifiers differ substantially in the 1970, 1980, and 1990 PUMS, and we are not able to match precise labor markets to many of the districts. For districts located in Santa Barbara, San Diego, Los Angeles, and the San Francisco Bay area, we use relatively consistent labor market boundaries over time, but the only way to create alternative wage measures for districts located in the rest of California is to use information from all other counties in the state. This results in a poor matching of teacher salaries with alternative wage opportunities and thus limits the variation in our measure of alternative wages.

Estimates of cross-sectional and first-difference regressions are shown in tables 9 and 10. In keeping with the state-level analysis, we define the relative wage as the difference between the log of the teacher wage and the log of the average female college graduate, non-teacher, wage in the corresponding labor market. The real wage is measured

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[36] For dropouts born in-state, the estimated coefficient is − 0.79, with a standard error estimate of 0.27. For the two college enrollment outcomes, the estimated coefficients are 0.18 and 0.27 with standard-error estimates of 0.11 and 0.16. Before applying IV, the coefficient estimates are − 0.79, 0.17, and 0.15 with standard-error estimates of 0.27, 0.03, and 0.04.

[37] These data were generously provided to us by Thomas Downes of Tufts University. For detailed information on the data, see Downes (1988).

TABLE 9.—ESTIMATES OF THE EFFECT OF TEACHER SALARY SCHEDULES ON
HIGH-SCHOOL DROPOUT RATES USING CALIFORNIA DISTRICTS

Student Outcomes	Real Teacher Wage	Relative Teacher Wage	1976–1985 Change in Real Teacher Wage	1976–1985 Change in Relative Teacher Wage
1985 dropout rates	−5.201 (3.244)	−3.192 (3.603)		
1995 dropout rates	0.0127 (0.0256)	0.010 (0.014)		
1985–1995 Δ dropout rates			−8.748 (3.595)	−7.745 (3.126)

both as the log of the wage for a starting teacher (table 9) and as the log of the average teacher wage in the district (table 10). Unlike in the previous analyses, we use dropout-rate levels instead of logs as our outcome variable.[38] All equations control for the percentage of students receiving AFDC.[39]

As in the state-level analyses, we see that cross-sectional regressions produce estimates that indicate no relationship between teacher wages and high-school dropout rates. In fact, districts with higher wages in 1985 appear to have higher dropout rates ten years later. Once fixed effects are included and relative teacher wage measures are used, however, the teacher wage estimates are positive and statistically significant, suggesting that a 10% increase in the teacher wage will produce a decrease in the dropout rate of 0.5 to 0.8 percentage points (an approximately 10% decrease, on average). Our state-level estimates suggest that the magnitude of these estimates would probably fall slightly if we were to include additional time-varying variables in our analysis. These results back up our conclusion that cross-sectional estimates of the effects of teacher wages on student outcomes have been biased by researchers' inability to control for fixed effects. Nevertheless, we emphasize that, because of the data's imperfections, we do not place confidence in these estimates.

VIII.   Conclusions

In this study, we investigate the relationship between teacher wages and student outcomes. Our results help to explain why previous studies have failed to produce systematic evidence that teacher wages affect student outcomes: the identification of teacher wage effects has been based on cross-sectional variation that reflects both supply and demand factors. Because alternative labor market opportunities and other school district characteristics vary across districts, the supply of teachers in quality-wage space also varies across districts. Therefore, we cannot hope to accurately identify the relationship between teacher wages and

[38] The results are substantively similar using either specification, but the standard errors are somewhat smaller when levels are used. We believe this is due to the difference in the definition of dropout rate between 1986 and 1995.

[39] We use the percentage of students receiving AFDC as a proxy for local income levels. Our results are unaffected by the inclusion of this variable.

student outcomes unless we are able to hold the supply curve fixed. Only a regression analysis that controls for other factors that affect the supply of teachers will produce policy-relevant elasticity estimates of the effect of teacher wages on student outcomes.

Of course, as in other studies, we are not able to directly control for all of the factors that influence the opportunity cost associated with choosing to teach. Instead of conducting a cross-sectional analysis (for which we would have needed to control for all factors affecting the supply decision), we create a state-level panel data set and implicitly control for static differences in non-pecuniary attributes by employing a first-difference estimation strategy. We also account for variation in the opportunity cost of teaching across states by using relative wage measures in our analysis. Our two-staged, least-squares estimates indicate that the results are not driven by endogeneity bias.

Our estimates suggest that, holding all else equal, raising teachers' wages by 10% (which would undo the 10% fall in relative wages that occurred during the 1980s) would reduce dropout rates by between 3% and 6%. Likewise, if the 20% increase in real teacher wages that occurred between 1959 and 1989 had been a relative increase (that is, the alternative opportunities for female college graduates had remained constant), then dropout rates would be at least 8.4% lower than they are today. A back-of-the-envelope comparison of the costs and benefits associated with raising teacher wages by 10% indicates that the increase in individuals' discounted lifetime wages that would result from the additional educational attainment produced by such an increase would be slightly outweighed by the cost. An increase in teacher wages is likely to affect outcomes other than the educational attainment measures that we have focused on in this study, however. Moreover, targeted increases may be more effective than across-the-board increases. A more complete analysis of the total costs and benefits associated with such a policy is certainly warranted.

When we compare our first-differences estimates to those based on data from a single year, we find that the first-differences approach produces standard-error estimates that are smaller and coefficient estimates that are more similar across specifications. In addition, when we include state-specific time trends (which partially account for state-

TABLE 10.—ESTIMATES OF THE EFFECT OF AVERAGE TEACHER WAGES ON
HIGH-SCHOOL DROPOUT RATES USING CALIFORNIA DISTRICTS

Student Outcomes	Real Teacher Wage	Relative Teacher Wage	1976–1985 Change in Real Teacher Wage	1976–1985 Change in Relative Teacher Wage
1985 dropout rates	−1.296 (2.116)	−0.118 (2.541)		
1995 dropout rates	0.005 (0.013)	0.012 (0.014)		
1985–1995 Δ dropout rates			−5.213 (3.134)	−5.387 (2.83)

specific changes in the attributes that are associated with teaching), the magnitude of our wage-coefficient estimates increases. This result is consistent with the existence of compensating differentials.

Our findings have important policy implications. First of all, they suggest that the quality of education can be improved by raising teacher salaries. In addition, they indicate that non-wage attributes are important and should be taken into account by governments that seek to equalize the quality of education. Districts that are unable to increase salaries because of funding limitations may be able to attract higher-quality teachers by improving other job characteristics. Of course, the feasibility of this alternative depends upon how much control districts have over their own characteristics and upon which characteristics affect the opportunity cost of teaching. Our first-differences estimation approach—together with our use of state-specific dummy variables—implicitly control for these characteristics, but does not provide insight into which school- or district-specific factors are most important. Furthermore, we are unable to control for a number of state-specific, time-varying job attributes such as changes in teacher workloads and changes in teacher certification requirements that may affect the degree to which changes in teacher wages reflect changes in the opportunity cost associated with choosing to teach. We hope to investigate these issues in future research.

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