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# High School Inputs and Labor Market Outcomes for Male Workers in Their Mid-Thirties: New Data and New Estimates from Wisconsin

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## Abstract

This study presents new evidence on the relationship between high school inputs measured at the time male respondents attended high school and the earnings of these same individuals when they were in their mid-thirties. To accomplish this task, we matched newly coded data on the characteristics of Wisconsin high schools in 1954–57 to the Wisconsin Longitudinal Survey. Our estimates show a significant relationship between the characteristics of teachers and the earnings of their students 17 years after graduation. Specifically, a 1 percent increase in the average teacher salary in a district increases the earnings of students by 0.33 percent. The magnitude of this effect is larger than estimates reported in previous research and many times larger than the impact of increasing parents' income by a comparable amount.

**High School Inputs and Labor Market Outcomes for Males in their Mid-Thirties: New Data and New Estimates From Wisconsin**

A substantial literature on the relationship between school inputs and student achievement has developed over the last 30 years. In an influential review, Hanushek (1986) concluded that the evidence did not support a positive relationship between school inputs (class size, per pupil expenditures, etc.) and student outcomes (test scores and earnings).[1] His assessment echoed the conclusions reached 20 years earlier in the Coleman Report (Coleman et al. 1966). This evaluation of the literature has been questioned in a recent review (Hedges, Laine, and Greenwald 1994) and by the results of an influential empirical study by Card and Krueger (1992) that found a positive relationship between primary and secondary school inputs and the wage returns to education. The Card and Krueger study has, in turn, generated additional empirical research on the effects of school inputs.[2] Some of these studies support the Card and Krueger findings while other studies have failed to confirm their results.

This study contributes to the literature by offering new evidence of the effect of high school inputs on earnings from a newly compiled and uniquely rich data set. We explore the effect of high school inputs measured at the school district level or the school level on the earnings of the same individuals when they were in their mid-thirties. In 1957 researchers at the University of Wisconsin–Madison began a 35-year study of a one-third random sample of the 1957 Wisconsin high school graduating class (Sewell and Hauser 1975, 1977; Hauser, Sewell, and Warren 1984; Hauser et al. 1993). This study, the Wisconsin Longitudinal Study (WLS), provides detailed information on respondents collected from public records and major surveys administered in 1957, 1964, 1975, and 1992–93. Here we investigate the relationship between school inputs and labor market outcomes for

[1]See also Hanushek (1996).

[2]For example, see Burtless 1996 and Moffitt 1996.

males using WLS data on parents' education when respondents were growing up, family income taken from state tax records for the 1957–60 period, and WLS respondents' reported earnings in 1974.

The WLS provides limited information on the characteristics of the schools that the WLS respondents attended. Therefore, to supplement the WLS, we have matched to each respondent from a public high school newly coded information on the characteristics of his high school during the 1954–57 period. These "new" data were obtained from annual reports filed by each school district in June of each year with the Wisconsin Department of Public Instruction. The reports are archived in the Wisconsin State Historical Society and provide detailed information at the school district level on expenditures, teacher characteristics, and enrollment. For respondents in districts with only one high school, the information on high school inputs reflects resources provided to the school the respondent attended. For respondents in districts with multiple high schools (e.g., Madison, Milwaukee), the variables are average inputs calculated over all high schools in the district. These annual reports permitted us to construct measures of virtually all of the school input variables used in previous research for each of the 4 years the respondents were in high school.

Yearly employment earnings in 1974 for male WLS respondents were regressed on school characteristics, parents' education, and family income. To preview the results, estimates from a simple reduced-form model of labor market earnings show that high school inputs have a statistically (and practically) significant effect on earnings. A 1 percent increase in average teacher salaries in a district has an effect on the later earnings of graduates that is over 80 times larger than the effect of a comparable increase in family income on children's earnings.

The remainder of this paper is divided into eight sections. The Card and Krueger study and the research this study generated are briefly reviewed in the next section. Section III describes the empirical specification. Section IV describes the selection of our WLS subsample and variables. Section V presents the basic empirical results showing the effect of family background, length of school term, and teacher characteristics on the earnings of the WLS subsample. Section VI presents estimates of the effect of teacher salaries on earnings. Section VII examines the relationship school district size and school inputs in the context of the school district restructuring that was occurring in Wisconsin during the 1950s. The paper concludes with a discussion and summary of our results.

## II. PREVIOUS RESEARCH

Card and Krueger and others have maintained that improved test scores are only one outcome potentially related to school inputs. In their 1992 study, they argue that the effects of primary and secondary educational inputs should also be judged by the relationship between these school inputs and labor market outcomes observed several decades after students leave school. To investigate this relationship, they designed a study that permitted them to estimate the effects of school inputs on labor market outcomes for workers after they had been in the labor market for 20–45 years, without having school- or school-district-specific data on the resources individuals received when they were in school. This design was necessary because there were no existing individual-level panel data sets with earnings when workers were beyond their early thirties that included school or school district information on inputs. For example, the latest wage data on seniors in the High School and Beyond survey were collected when respondents were 24 years old. For respondents in the sophomore cohorts, the “oldest” wage data were collected when respondents were 27 years old. The National Longitudinal Study of the Class of 1972 contains wage data when the respondents were 32, but the school-level information is vague and often incomplete.[3] Faced with these data limitations, Card and Krueger correlated earnings data for older cohorts by state of birth from the 1980 census with state-level average school and teacher characteristics measured at the time respondents were in school. This analysis was done in two steps.

[3]The school questionnaire does not supply expenditure information or salary information. In addition, item nonresponse rates are high.

First, the average returns to a year of education for individuals were estimated by state of birth and birth cohort. This set of estimated returns to education was then regressed on average school inputs for the state during the period when individuals born in a particular state and birth cohort would have been enrolled in school. Thus, by assuming individuals were educated in public schools in their state of birth, Card and Krueger estimated the relationship between average state educational inputs and average returns to education for individuals later in their careers. In some specifications they also controlled for state of birth in the second-stage regression so that the effects of school inputs on earnings are identified by the within-state changes across cohorts in school inputs. They found that the return to a year of education declined with increasing pupil/teacher ratios and was positively related to relative teacher salaries.

A number of studies of school inputs and labor market outcomes have been published since Card and Krueger. Generally speaking, studies that correlate school inputs measured at the school or school district level to career outcomes measured when workers are in their late twenties or early thirties fail to find a significant relationship (Betts 1996; Grogger 1996; Hanushek, Rivkin, and Taylor 1996). On the other hand, studies that replicate the Card and Krueger methodology on other samples find evidence consistent with Card and Krueger (Loeb and Bound 1996). Four major explanations have been offered to account for the different results obtained across the studies.

The first explanation for the conflicting results is that studies using microdata to relate school inputs to the earnings of workers through their late twenties are biased downward because the effects of school inputs are not yet reflected in earnings. This bias occurs if more highly educated workers receive greater schooling inputs in grades 1–12 and also make more investments in postschool training early in their careers than do individuals receiving fewer school resources. Since on-the-job training investments are at least partially financed through a lower wage (Becker 1964), the difference in wages between those with more high school resources and those with fewer inputs reflects both the effects of the inputs and differences in current investments in on-the-job training. This source of bias is less of a problem when studying workers later in their careers, when they are making fewer training investments and are realizing the returns to both their training investments and their investments in formal education.

Different levels of measurement error in the observed values of school inputs provide a second possible explanation for the conflicting results. Classical measurement error in the measures of school inputs will bias the estimated effects of school resources toward zero. Although Card and Krueger lack direct evidence on the amount of measurement error in either district-level measures of inputs or state averages, the measurement error in average school inputs calculated over all districts in a state is smaller than the measurement error in inputs measured for a single school or school district. Such a difference in measurement error implies that the estimates using school-district-level data will be biased toward zero and smaller than the estimated effects using average state-level school input data.

A third explanation offered for the different results involves the effect of using average state input measures on the magnitude of omitted variable bias. Comparing average returns across states will overstate the effect of school inputs on labor market outcomes if the correlation between state average school input measures and unobserved state characteristics is greater than the correlation between school district inputs measured at the district level and omitted family and local community variables that may affect wages (Hanushek, Rivkin, and Taylor 1996). Thus, for example, states with higher than average school inputs may also have a variety of other regulations on curriculum, teacher certification, teacher tenure, and school governance that affect educational quality and, hence, student performance. Models that control for fixed state effects may share this source of bias if changes in school resources are correlated with changes in these other state policies. Since these state policies are not measured by Card and Krueger, their school resource measures will partially reflect the effects these omitted state policies have on earnings. When using school-district-level data, the biasing effect of these omitted state characteristics is likely to be smaller because the estimates are partially based on within-state variability in school inputs across school districts. Moreover, with school- or district-level data, the between-state differences can be controlled for by confining the analysis to a single state (as in this study) or by including a set of state dummy variables in the model.

Hanushek, Rivkin, and Taylor (1996) demonstrate the potential effect of this aggregation bias using test performance data from High School and Beyond. They show that the estimated effects of teacher salaries and the pupil/teacher ratio on test scores are larger when estimated on state averages than when models are estimated on the disaggregated data from individual schools. Whether this bias in explaining test score variability is of the same order of magnitude as the bias obtained from studying long-term labor market outcomes is an unanswered question.

A fourth explanation for the difference between studies stems from Card and Krueger's specification used to estimate the returns to education. Their empirical specification assumes no interactions between state of birth and state of residence on earnings. This constraint implies no selective migration from state of birth to another state (Heckman, Layne-Farrar, and Todd 1996a). If migrants to a particular labor market earn more than migrants coming from other states due to mobility costs (or migration benefits), then Card and Krueger's estimates of the returns to education will partially reflect the effects of this selective migration. Heckman, Layne-Farrar, and Todd (1996a,b) replicate Card and Krueger's results using the 1970 and 1990 census in models that do not include interaction terms between state of birth and state of residence. However, many of the estimates change significantly when interaction terms between state of birth and state of residence are included in the model.[4]

This brief overview of recent research provides the rationale for the present study. The data set we compile enables us to estimate the effect of school inputs on the midcareer earnings of workers while minimizing many of the usual causes of bias. By matching school-district-level data to the WLS, we

[4]Compare the estimates in Tables 8 and 15 in Heckman, Layne-Farrar, and Todd (1996a). With state of birth and state of residence interaction terms, the coefficients on the school quality variables become insignificant or change sign.

create a 20-year panel that includes good measures of family background characteristics, earnings at 35 years old, and school district information from the districts these individuals attended measured at the time they attended high school. Since most of the districts in the sample (95 percent) contain only one high school, there is virtually no aggregation of our school input measures. We also minimize the effects of measurement error by calculating average school inputs over a 4-year period. Finally, potential aggregation bias due to omitted state effects cannot be a confounding variable because the entire sample graduated from Wisconsin high schools.[5]

## III. THE REDUCED-FORM MODEL

The potential effects of school inputs on earnings can be estimated by the following two-equation recursive model:

$$Y_{i,t} = \beta_0 + \beta_1 Ed_{i,t} + \beta_2 Q_{i,t-k} + \beta_3 Z_{i,t-k} + \beta_4 (Q_{i,t-k}*Ed_{i,t}) + \beta_5 (Z_{i,t-k}*Ed_{i,t}) + \varepsilon_{i,t} \qquad (1)$$

$$Ed_{i,t} = \alpha_0 + \alpha_1 Q_{i,t-k} + \alpha_2 Z_{i,t-k} + \nu_{i,t} \qquad (2)$$

$Y_{i,t}$ is earnings in year t for respondent i and $Ed_{i,t}$ is the total years of education received by respondent i beyond high school by year t. $Q_{i,t-k}$ is a measure of school inputs provided k years earlier when the respondent was in high school and $Z_{i,t-k}$ is a vector of family background characteristics affecting both schooling and later labor market outcomes.

In this specification high school inputs influence labor market outcomes through three mechanisms. First, greater secondary school inputs may improve labor market outcomes for all high school graduates by shifting the earnings function upward by a constant amount ($\beta_2 > 0$). Second, high school inputs may lead to greater investments in formal education beyond high school and indirectly influence labor market outcomes by raising the quantity of post-high school education ($\alpha_1 > 0$). Finally,

[5]Although the entire sample graduated from Wisconsin high schools, many respondents (28 percent) were living outside of Wisconsin at the time of the 1975 interview. These respondents are included in the analysis.

school resources may raise the effective return to a unit of formal education ($\beta_4 > 0$). Equation 1 also includes an interaction term between family background characteristics and years of education because family characteristics might also effectively raise the economic return to an additional year of education.

Implicit in equations 1 and 2 is a model describing the schooling decisions by individuals as a function of the marginal costs and benefits of obtaining additional schooling (Becker 1975; Card 1998). Both secondary school inputs and family background characteristics may affect these decisions by altering the costs and/or the benefits of obtaining additional education. We leave the task of estimating equations 1 and 2 as part of a more formal model for future research. Here we undertake the more modest task of estimating the reduced-form earnings equation obtained by substituting equation 2 into equation 1:

$$Y_{i,t} = C_0 + C_1\, Q_{i,t-k} + C_2\, Z_{i,t-k} + C_3\, (Q_{i,t-k} * Z_{i,t-k}) + C_4\, Q_{i,t-k}^2 + +C_5\, Z_{i,t-k}^2 + u_{i,t} \qquad (3)$$

where:

$C_0 = \beta_0 + \beta_1\alpha_0$

$C_1 = \beta_1\alpha_1 + \beta_2 + \beta_4\alpha_0$

$C_2 = \beta_1\alpha_2 + \beta_3$

$C_3 = \beta_1\alpha_3 + \beta_4\alpha_2 + \beta_5\alpha_1$

$C_4 = \beta_4\alpha_1$

$C_5 = \beta_5\alpha_2$

$u_{i,t} = (\beta_1 + \beta_4\, Q_{i,t-k} + \beta_5\, Z_{i,t-k})\, \nu_{i,t} + \varepsilon_{i,t}$

Equation 3 shows that labor market outcomes are a function of high school inputs (Q), family background characteristics (Z), squared terms in Q and Z, and interaction terms between Z and Q. If there are no interaction effects in the earnings equation between family background and education and between education and school resources, then $C_3 = C_4 = C_5 = 0$.

Though estimating equation 3 leaves unanswered questions about the specific mechanisms through which high school inputs may influence later career outcomes, it has the virtue of providing estimates of the total direct and indirect effects of school inputs on earnings without imposing any structure on the data dictated by a particular model. In this paper we present estimates for models that include neither interactions between school and family characteristics nor higher order terms in the two sets of characteristics.[6]

## IV. THE DATA

Our data were obtained by merging newly coded information on the characteristics of Wisconsin high schools in the 1950s with the Wisconsin Longitudinal Study (Hauser and Sewell 1957–77; Hauser et al. 1992/1993). The WLS is a random sample of 10,317 students who graduated from Wisconsin high schools in 1957. Data were collected from school and state records in the late 1950s and early 1960s and directly from the graduates or their parents in surveys conducted in 1957, 1964, 1975, and 1992/93. The data include detailed information on educational achievement and performance, family background characteristics, and labor market outcomes. The survey is broadly representative of white American men and women with at least a high school education (Hauser et al. 1993). One weakness of the WLS for our purposes is the left censoring at 12 years of education. This censoring means the WLS cannot be used to estimate the marginal effect of high school resources on the returns to gaining a twelfth year of

[6]We estimate a model that includes squared terms for mother's education, father's education, family income, and five school resource variables (term length, teacher education, teacher experience, teacher tenure, and the pupil/teacher ratio.) The model also includes 15 interaction terms between the three family background measures and each of the five school resource variables. The estimates for this model are not reported here because the interaction terms were jointly insignificant. The p-value for the significance of the interaction terms between father's education and the five resource variables was .819. The p-value for the hypothesis that the five interaction terms between mother's education and the resource variables are jointly equal to zero was .393. The p-value for the hypothesis that the interactions between family income and the five school resource variables were jointly equal to zero was .106. On the other hand, the squared terms in this model for both family resource and school characteristics were jointly different from zero with a p-value of .001. Based on these results, the model was re-estimated with only the squared terms. The estimates for this specification are shown in Appendix Table 1.

education. Finally, minorities are not well represented in the sample because of Wisconsin demographics in the late 1950s and because the sample is limited to high school graduates.

The three primary exogenous variables that form the matrix of family background characteristics are each parent's education and family income. Each of these variables has been found to be significant in previous work using the WLS (Hauser et al. 1993),[7] as well as in other samples (Altonji and Dunn 1996; Betts 1996). Years of education completed by both the mother and father were obtained from the 1975 interviews of the respondents. If parents' education was missing from the 1975 interview, then parents' education level was determined from the 1957 interview. Family income was obtained from Wisconsin state income tax records and equals the average reported household income for the years 1957–1960. We assume average earnings during these 4 years are highly correlated with family earnings when the WLS respondents were in elementary and secondary school.

Models were also estimated that included three additional family background characteristics included in previous work using these data (Hauser et al. 1993). These variables were the number of siblings in the family, a dummy variable indicating if both parents were in the household when the respondent was growing up, and a dummy variable indicating if the respondent was from a farm family. For our subsample of males, 24 percent of the respondents were from farm families.

The following steps were used to construct the sample of WLS respondents. The analysis was confined to males (n = 4,992) who responded to the survey in 1975 (n = 4,331). From this sample, 568 were dropped because they attended private or parochial schools; no information is available on the resources used by these schools. A further 169 were eliminated because they attended public schools for which no district-level data are available. From the remaining 3,594, we eliminated 698 individuals for

[7]The WLS also includes an IQ test score given to students when they were high school juniors. Previous research (Hauser et al. 1993) using the WLS shows that this variable is significantly related to earnings. However, we also find that test scores have a small but significant relationship to our school resource measures. Therefore, we exclude the test score in our model, so our estimates capture the effect of school resources on earnings that includes the indirect effect of school resources through test scores.

whom there was no information on parental income, parental education, number of siblings, whether both parents were in the household and whether the respondent was from a farm family. This left a sample of 2,896 individuals.

The three individual earnings measures reported for 1974 in the WLS are: (1) wage and salary income, (2) income from self-employment, and (3) farm income. Here we report results for models where the dependent variable is the natural log of wage and salary income. We further restrict the sample to those whose primary source of income was wages or salary. Restricting the sample in this way primarily eliminates farmers and self-employed professionals from the sample. After eliminating respondents who received less than half of their income from wages and salaries, did not report weekly employment earnings over $40, or did not report a positive number of weeks worked, we were left with a final sample of 2,632 males.[8] Estimation using the sum of earnings from all three sources (n = 2,896) does not significantly alter the results. These estimates are reported in Appendix Table 2.

Merged with our sample of WLS respondents is information taken from the annual report that the Wisconsin Department of Public Instruction required from each school district in June of each year. Data from annual reports were coded for 1954–57 for 336 of the 421 Wisconsin school districts that had students in grades 9–12.[9] The only districts whose reports were not coded were those with fewer than five students in the full WLS. A dozen districts did not provide reliable information over any of the 4 years and were also excluded. Many of these districts were in the process of merging with other districts. These selection criteria produced the final sample of 2,632 students who graduated from 308 different high schools in 278 districts. Of these districts, 264 (94 percent) contained only one high school; nine districts (3 percent) contained only two high schools. Our data set includes at least one observation from school districts that collectively enrolled a substantial share of the Wisconsin senior class of 1957. The

[8]The $40 threshold is equivalent to working 40 hours per week at one-half of the 1974 minimum wage.

[9]In 1956–57 there were 3,822 operating school districts in Wisconsin, including 2,612 one-room rural school districts and 789 “graded” schools that provided education through grade 8.

number of observations per school district in the sample ranges from one observation for 17 districts to 252 students from the Milwaukee public school system. The high schools in our sample enrolled 28,609 seniors, or about 84 percent of the total population of seniors in the state in 1957 (State of Wisconsin 1957).

Because the state provided different levels of financial support for students enrolled in grades 1–8 and grades 9–12, the reports filed with the state provide data separately for grades K–8 and grades 9–12. We use information on teachers and expenditures only for grades 9–12. The inputs received by WLS respondents when they were in grades 1–8 likely are correlated with the resources they received in high school. Thus, the estimates reported here partially reflect the effect of school inputs throughout grades K–12. Although it would be possible to code data on school inputs from earlier annual reports when the WLS respondents were in grade school, there is no information in the WLS on where the respondents went to grade school. Assuming respondents attended school in the same district where they went to high school is likely to be incorrect for many respondents because of the substantial school district reorganization and consolidation that occurred in the state between 1945 and 1957. For example, in 1945–46, when the WLS sample was in the first grade, there were 4,626 one-room school districts in the state that enrolled 18.8 percent of Wisconsin students (State of Wisconsin 1949). By 1956–57, the number of one-room schools had been reduced to 2,811, and these schools enrolled only 9.2 percent of Wisconsin students (State of Wisconsin 1957). This suggests that elementary school input data cannot be meaningfully matched to the WLS without additional information on elementary school attendance because a substantial portion of the WLS sample was probably affected by these consolidations.

Unless the reports were incomplete, the following variables were constructed for each of the four years:

- length of the school term
- percentage of high school teachers with more than 4 years of post-high school training
- mean years of teaching experience of high school teachers
- mean years of district tenure of high school teachers

- pupil/teacher ratio
- mean salary of teachers

For this analysis the mean values calculated over the 4 years were used in the earnings models. This was done for two reasons. First, average values capture any real changes in school inputs over the 4-year period that may have had an impact on student achievement. For example, if teacher tenure was increasing in some districts over this time, then the average value will better capture the experience of teachers in the district when these individuals were in high school than the measure from any single year. Alternatively, if there was no year-to-year variation in true inputs and all the observed year-to-year variation in the data reflects measurement or reporting error, then using a 4-year average will reduce the biasing effects of measurement error. Both of these considerations suggest that using the average data for the 4 years is preferred to using data from a single year. For some districts, data for one or more variables was missing for one or more years. In these cases, school resources were averaged over the years of available data.

Table 1 shows the mean values and standard deviations for each variable calculated over the 4 years of data for each school district. Mean teacher salaries for 1954–1956 were converted to 1957 dollars using the consumer price index before calculating the average salary. Column 1 reports data where the district data are weighted by the number of WLS respondents from the district. Column 2 gives descriptive statistics where the data for all districts are equally weighted. For most variables, the means are similar. However, a comparison of the two columns suggests that the larger districts have more experienced and better educated teachers.

During the 1950s in Wisconsin, there was substantial variation across districts in high school inputs which was generated by limited state support for K–12 education and a heavy reliance on local property taxes to finance education. From 1945–46 through 1956–57, the state paid between 12.8 and 19.5 percent of the cost of K–12 education, and most of this aid was in the form of a flat per pupil payment. For example, in 1956–57 over 60 percent of the nontransportation aid from the state to local

**TABLE 1**
**Descriptive Statistics**
**Mean (SD)**

| Variable | Calculated over the WLS Sample (n=2632) | Calculated over the 278 Districts |
|---|---|---|
| Father's Ed | 9.815<br>(3.373) | 9.195<br>(1.674) |
| Mother's Ed | 10.599<br>(2.769) | 10.392<br>(1.536) |
| Ln(Family Income) | 3.917<br>(0.654) | 3.718<br>(0.386) |
| Farmkid | 0.220<br>(0.414) | 0.387<br>(0.309) |
| Number of Siblings | 2.863<br>(2.211) | 3.254<br>(1.241) |
| Both Parents in Household | 0.938<br>(0.241) | 0.928<br>(0.151) |
| Length of School Year (days) | 178.387<br>(4.166) | 176.251<br>(2.973) |
| Fraction of Teachers with >4 Years Post-HS Ed. | 0.392<br>(0.222) | 0.237<br>(0.174) |
| Mean Years of Teaching Experience | 7.087<br>(1.466) | 5.980<br>(1.441) |
| Mean Years of Tenure in District | 5.805<br>(1.829) | 4.447<br>(1.709) |
| Mean Teacher Salary | 4,629<br>(651) | 4,146<br>(439) |
| Pupil/Teacher Ratio | 21.825<br>(2.422) | 20.250<br>(3.044) |
| No. of Seniors in District | 584<br>(1134) | 103<br>(260) |

districts took the form of a flat per pupil subsidy and only 40 percent of state aid was related (negatively) to the local property wealth of the district (State of Wisconsin 1957).

## V. THE EFFECTS OF SCHOOL TERM, TEACHER CHARACTERISTICS, AND PUPIL/TEACHER RATIO ON EARNINGS

Table 2 reports the ordinary least squares (OLS) estimates of models where the natural log of 1974 yearly earnings is a function of the educational levels of each parent, average parental income from 1957–60, and a variety of school input measures. The models reported in Table 2 include only the linear terms in family characteristics and school inputs. Because many of the observations are from the same school district, more appropriate standard errors might be obtained by accounting for the potential correlation in the error terms for individuals in the same district that is attributable to unmeasured school effects or sorting by families into districts based on variables other than parents' education and income. Random effect models that account for this correlation were estimated but the standard errors were only slightly larger than the OLS standard errors, and none of the substantive conclusions from the analysis were altered. Moreover, a test (Breusch and Pagan 1980) of the hypothesis that the district error variance is equal to zero could not be rejected at the .05 level for all but one specification shown in Table 2 that included both family background and school resource variables. The p-values for this test are shown in the last row of Table 2.

Column 1 of Table 2 reports estimates for a model that includes only family background measures. These estimates show that parents' education and income have a significant effect on the earnings of their children. The point estimate of the effect of mother's education is smaller than for father's education, but the two estimates are not significantly different from one another at conventional significance levels (p-value = .174). The point estimates imply that an additional year of education for both parents increases the expected value of children's earnings by 1.6 percent. Family income over the

**TABLE 2**
**OLS Estimates of the Impact of Family Characteristics, Term Length, and Teacher Characteristics on Log(1974 Earnings)**

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Constant | 8.9580 | 8.7798 | 8.5024 | 7.6272 | 8.9687 | 8.8287 | 8.9039 | 8.9381 |
| | (0.0545) | (0.4229) | (0.4192) | (0.3611) | (0.0543) | (0.0598) | (0.0558) | (0.0894) |
| Father's Ed | 0.0116 | | 0.0100 | 0.0112 | 0.0102 | 0.0103 | 0.0107 | 0.0116 |
| | (0.0029) | | (0.0029) | (0.0029) | (0.0029) | (0.0029) | (0.0029) | (0.0029) |
| Mother's Ed | 0.0043 | | 0.0045 | 0.0050 | 0.0046 | 0.0044 | 0.0047 | 0.0043 |
| | (0.0035) | | (0.0035) | (0.0035) | (0.0034) | (0.0034) | (0.0035) | (0.0035) |
| Ln(Family Income) | 0.1124 | | 0.0879 | 0.1010 | 0.0916 | 0.0921 | 0.0968 | 0.1120 |
| | (0.0136) | | (0.0142) | (0.0139) | (0.0141) | (0.0141) | (0.0140) | (0.0137) |
| Length of School Year | | 0.0035 | 0.0028 | 0.0077 | | | | |
| (days) | | (0.0024) | (0.0024) | (0.0021) | | | | |
| Fraction of Teachers | | 0.1845 | 0.1175 | | 0.2116 | | | |
| w/ >4 Years Post-HS Ed. | | (0.0645) | (0.0640) | | (0.0398) | | | |
| Mean Years of Teaching | | 0.0401 | 0.0266 | | | 0.0312 | | |
| Experience | | (0.0138) | (0.0136) | | | (0.0060) | | |
| Mean Years of Tenure | | -0.0122 | -0.0084 | | | | 0.0207 | |
| in District | | (0.0107) | (0.0106) | | | | (0.0048) | |
| Pupil/Teacher Ratio | | -0.0057 | -0.0052 | | | | | 0.0010 |
| | | (0.0038) | (0.0038) | | | | | (0.0035) |
| $R^2$ | 0.0503 | 0.0334 | 0.0630 | 0.0553 | 0.0605 | 0.0599 | 0.0571 | 0.0504 |

(table continues)

**TABLE 2, continued**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| P-value for Hausman test that the RE and FE coefficients are equal | 0.0010 | NA | 0.9768 | 0.0495 | 0.7532 | 0.6828 | 0.2323 | 0.0001 |
| P-value for Breusch & Pagan test that the variance of school district RE is equal to zero | 0.0001 | 0.9068 | 0.7788 | 0.2006 | 0.8719 | 0.5162 | 0.5003 | 0.0009 |

**Notes:** Standard errors in parentheses. RE = random effect; FE = fixed effect.

1957–60 period is also positively related to children's earnings. A 1 percent increase in family income leads to a predicted 0.11 percent increase in children's income.

The second column of Table 2 regresses earnings on average school term length (in days), the percentage of high school teachers in the district with more than 4 years of post-high school education, the average years of teaching experience for teachers, the average years of tenure in the district for teachers, and the pupil/teacher ratio. Four of the five coefficients on these variables are of the expected sign; the coefficients on the percentage of teachers with more than 4 years of education and on mean years of teaching experience are significant at the 1 percent level. The coefficient on the former implies that a 10 percentage point increase in the fraction of teachers with more than 4 years of post-high school training would increase earnings by 1.8 percent. The joint hypothesis that the five coefficients on the school input variables are all equal to zero is easily rejected with a p-value of less than .0001.

The specification that includes the three family background measures and the five school input variables is shown in column 3. Comparing the estimates in this column with the estimates in columns 1 and 2 shows the bias that occurs when estimating either the direct effects of school resources on earnings without controlling for family background characteristics or the direct effects of family characteristics that omit school resource variables. Comparing column 3 with the estimates in column 1 shows that the effect of father's education is overstated by 16 percent when school characteristics are excluded from the model. On the other hand, including school characteristics in the model has virtually no impact on the effect of mother's education on children's earnings. Controlling for school characteristics reduces the estimated effects of family income by 28 percent from .112 to .088.

A Hausman test was conducted to determine whether the *change* in the estimated effects of parents' income and education was significant when the school resource variables were added to the model (i.e., columns 1 and 3 in Table 2). To construct this test, the three background variables were each separately regressed on the five school district variables and the residuals were computed. The earnings of WLS respondents were then regressed on the vector of residuals plus the three family background characteristics. The hypothesis that the coefficients on the residuals from this "artificial" regression are equal to zero is a test of the null hypothesis that the coefficients on the family background variables did not change when the school district variables were added to the model. The p-value for this hypothesis was less than .0001. The conclusion we draw from this test is that the estimated effects of parents' income and education on children's earnings changes significantly when school district characteristics are controlled.[10]

Columns 2 and 3 also demonstrate how omitting family characteristics biases the estimated effects of school resources on later labor market outcomes. All the school resource coefficients decline substantially when family education and income are included in the model, and the teacher education variable is no longer individually significant. For example, excluding the family background characteristics causes the estimated effect of the length of the school term to be overstated by 26 percent and the effect of teacher education levels to be overstated by over 50 percent.

Although the large standard errors make it difficult to precisely identify the magnitude of the effect of any one of these school resource variables, the data leave little doubt that school resource measures are significantly and positively related to the later earnings of students even after controlling for parents' education and income. The joint hypothesis that all five coefficients in column 3 are different from zero is easily rejected by the data; the *F*-statistic is 7.12 with a p-value less than .0001. Most of the effect of school resources on WLS earnings is due to the three variables measuring teacher education and experience. The joint hypothesis that the three teacher qualification variables are jointly equal to zero for the model in column 3 is rejected with a p-value of .0001.[11] On the other hand, the joint hypothesis that

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[10]See Davidson and MacKinnon (1990) and also Johnston and DiNardo (1997), pp. 338–342, for a treatment of this test.

[11]The estimated standard errors for average teacher tenure in the district and years of teaching experience are large because these variables are highly correlated in this sample (r = .89). The hypothesis that these variables are jointly equal to zero is marginally rejected (p = .1059). The estimated coefficient on tenure is .0062 (SE = .0075)

the effect of the length of the school year and the pupil/teacher ratio is zero is not rejected at conventional levels of significance (p-value = .203).

Columns 4–8 of Table 2 report estimates for models that individually include each of the five school resource variables. Although the school resource coefficient in each of these models is biased upward because the parameter partially reflects the relationship between earnings and the other omitted school resource variables, all these models are nevertheless useful because they correspond to models commonly found in previous research. The coefficients on all the variables in these models are significant and in the predicted direction except for the estimated effect of the pupil/teacher ratio. This coefficient is positive but estimated very imprecisely, and it is not different from zero at conventional levels of significance.

An important issue in the interpretation of the estimates in Table 2 is whether the estimated effects of school characteristics are biased because of omitted community or school district characteristics that are correlated with measured resources and earnings. It is impossible to rule out such a possibility, but comparing estimates from fixed effect and random effect specifications of the earnings model using a Hausman test does highlight a key constraint that would have to be true of any omitted variable. As noted above, the estimates in column 1 of Table 2 that include only family background characteristics are biased because of omitted school district characteristics that are correlated with family background variables and student earnings following graduation. These estimates of the effect of family characteristics may also be biased because of other omitted school district or community characteristics. A formal test of the magnitude of this bias is obtained by estimating the model in column 1 where it is assumed that unobserved district effects are independent of parents' education and income. These random effect (RE) estimates can then be compared with estimates from a fixed effect (FE) specification

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when a model is estimated without the experience variable. The estimated coefficient on experience is .0190 (SE = .0097) when a model is estimated without the average tenure variable.

using a Hausman test where unobserved school district effects are permitted to be correlated with parents' education and income. The second to the last row of Table 2 (column 1) shows that the hypothesis that the coefficients are equal in the RE and FE models is rejected with a p-value of .001 when school inputs are excluded from the model. This result can be compared with the test using the specification in column 3 of Table 2 where the five school resource variables are included in the model. This model can also be estimated as an RE model. However, the coefficients on the school district variables in this RE specification cannot be compared with estimates from an FE model because there is no within-district variation in school resources. However, because there is variation in the family background variables within school districts, the coefficient estimates on these variables in the RE specification that includes the school district variables can be compared with the coefficients in a FE model where the school district variables are absorbed by the set of school district fixed effects. After controlling for the five school characteristics in the model, the hypothesis that the RE and FE coefficients on the family background characteristics are equal cannot be rejected (p-value = .977). These results suggest that the measured school variables are capturing the community characteristics that affect the later earnings of graduates.

These Hausman tests do not completely rule out the possibility that there are omitted school district and community characteristics that may be biasing the estimated effects of school inputs. However, they do imply that any omitted variable cannot be community or school district characteristics correlated with either average parental education or family income. If the omitted variables were correlated with parental education and family income, then the RE and FE coefficients on the family background variables would not be the same after conditioning on school resources.

Indirect evidence of the power of this test is illustrated by looking at the results of the Hausman test in specifications that include only the length of the school term or the pupil/teacher ratio. These specifications are displayed in Table 2, columns 4 and 8, respectively. The Hausman test results for these two specifications support a rejection of the hypothesis that the coefficients on parents' income and education are the same in an RE model as in an FE model that includes only these coefficients and either term length or pupil/teacher ratio.

Table 3 reports results for a model that includes parents' education and income and three additional family background variables—number of siblings, whether both parents were in the household when the child was growing up, and whether the respondent was from a farming household. These three variables are statistically insignificant both individually and jointly (p-value = .3652). Though controlling for these additional family background characteristics reduces the coefficients on all the school resource variables, both the five school resource variables (p-value = .0001) and the subset of the three teacher qualification variables (p-value = .0008) remain statistically significant. Given the insignificance of these variables, in the remaining analysis we control only for parents' education and income.

## VI. TEACHER SALARIES AND EARNINGS

Standard neoclassical theory predicts that teachers will be paid based on their productivity. The results reported in the previous section show that teacher productivity, as measured by later student success in the labor market, is a function of the training and experience of teachers in a district. This suggests that teacher education and experience should be related to their salaries and their salaries will be correlated with the salaries of WLS respondents. Direct evidence that school districts were paying teachers based on teacher characteristics correlated with student achievement is provided by the results from a teacher wage regression model estimated using the 278 school districts and the five school input variables as independent variables:

**TABLE 3**
**OLS Estimates of the Impact of Family Characteristics, Term Length, and Teacher Characteristics on Log(1974 Annual Earnings)**

| Variable | | Variable | |
|---|---|---|---|
| Constant | 8.6322<br>(0.4255) | Length of School Year (days) | 0.0025<br>(0.0024) |
| Father's Ed | 0.0094<br>(0.0029) | Fraction of Teachers with >4 Years Post-HS Ed. | 0.1111<br>(0.0641) |
| Mother's Ed | 0.0042<br>(0.0035) | Mean Years of Teaching Experience | 0.0248<br>(0.0137) |
| Ln(Family Income) | 0.0832<br>(0.0148) | Mean Years of Tenure in District | -0.0088<br>(0.0106) |
| Both Parents in Household | -0.0204<br>(0.0351) | Pupil/Teacher Ratio | -0.0053<br>(0.0038) |
| Siblings (number) | -0.0022<br>(0.0040) | | |
| Farmkid | -0.0345<br>(0.0232) | | |
| $R^2$ | 0.064 | | |

**Note:** Standard errors in parentheses.

$$\text{Teacher Salary (in levels)} = \underset{(1024)}{-3048} + \underset{(5.90)}{35.51}\text{*Days} + \underset{(20.10)}{48.51}\text{*Experience} + \underset{(16.12)}{42.77}\text{*Tenure} + \underset{(121)}{1032}\text{*Educ} + \underset{(5.64)}{10.43}\text{*Pupil/Teacher Ratio} \qquad (5)$$

$R^2 = .65$ (SE in parentheses)

These estimates show that teachers in Wisconsin in the 1950s were compensated based on observed human capital variables that were correlated with their productivity (i.e., student performance) as well as receiving additional compensation based on the length of the school year and a compensating differential for larger classes. Since the average teacher salary is a linear combination of the stock of human capital in a district, we estimate a WLS earnings model that replaces the five school input measures with the average teacher salary in the district. These estimates are reported in Table 4.

Table 4 shows a model in which the mean teacher salary in the district is added to a model that includes parents' education and family income. The coefficient on the average teacher salary variable in this specification is .072 and is estimated very precisely with a t-value greater than 5.00. These estimates imply that a $100 increase in average teacher salaries is associated with a 0.73 percent increase in earnings, and a standard deviation increase in the average teacher salary leads to a predicted 4.39 percent increase in the earnings of graduates.

A Hausman test comparing a school district fixed effect model and a random effect specification that includes average teacher salaries fails to reject the hypothesis that the RE and FE coefficients on parents' income and education are the same (p-value = .872). This suggests that the average teacher salary in districts is capturing key human capital variables affecting teacher productivity and that any omitted variable biasing the effect of teacher salaries would have to be independent of average parents' education and income in a district.

The estimates shown in Table 4 are substantially larger than the largest estimates of the effect of teacher pay reported in previous research. Over the WLS subsample included in our analysis, the average

**TABLE 4**
**OLS Estimates of the Effect of Mean Teacher Salaries on Log(1974 Earnings)**

| | |
|---|---|
| Constant | 8.7257 |
| | (0.0700) |
| Father's Ed | 0.0103 |
| | (0.0029) |
| Mother's Ed | 0.0048 |
| | (0.0035) |
| Ln(Family Income) | 0.0882 |
| | (0.0143) |
| Mean Teacher Salary | 0.0723 |
| ($1,000) | (0.0138) |
| $R^2$ | 0.0602 |

**Note:** Standard errors in parentheses.

teacher salary is $4,626. Thus, a 1 percent increase in teacher salaries at the sample mean implies a predicted 0.33 (.072 x .0463) percent increase in earnings. In contrast, Card and Krueger (1992, Table 6) report that a 1 percent relative increase in teacher salaries increased the expected earnings of males by 0.0477 percent when log weekly earnings were regressed on relative teacher salaries, dummy variables for state of residence and state of birth, and other demographic controls. Thus, our estimated effect of the average teacher salary on the earnings of their students is more than six times larger than the Card and Krueger estimate.

The magnitude of the effect of the average teacher salary on later earnings of students can also be evaluated by comparing it to the estimated effect of parents' income on children's earnings. With an average pupil/teacher ratio of 20 students, the 1 percent increase in average teacher salary (i.e., $46/year) estimated in Table 4 raises per pupil expenditures by $2.31 in 1957 dollars and increases student earnings by 0.33 percent. Alternatively, a $2.31 increase in family income is equal to a 0.046 percent increase in family income evaluated at the sample mean of log family income (i.e., 2.31/exp(3.917)). This increase in income and the coefficient on family income implies that a $2.31 transfer payment to a family raises predicted earnings by .004 percent (.046 x .0882). This estimate is more than 80 times smaller than the effect of spending the $2.31 on teacher salaries. Thus, if a social planner in Wisconsin in the 1950s had been interested in improving the later earnings of students, a policy of raising teacher salaries by 1 percent would have been significantly more effective than providing a transfer payment to parents of an equivalent amount of money.

## VII. SCHOOL DISTRICT SIZE AND SCHOOL RESOURCES

As noted earlier, during the 1950s school consolidations in Wisconsin were very common as the state encouraged mergers of smaller districts to create larger districts. This consolidation included closing many of the one-room schools that educated children in grades K–8, as well as the consolidation of smaller high schools. The state's initiative for district consolidation was developed following a 1948 report to the legislature which concluded that small schools and districts in the state should be consolidated because of economies of scale in the production of educational services through specialization that were not available to smaller districts (State of Wisconsin 1948). Small schools could not afford to employ a broad range of teachers specialized in many subjects. One section of the report concluded that "it is apparent that it costs up to 20 percent more to operate a very small school with a limited program than it does to operate a larger school with a broader program."

A 1959 report by the Department of Public Instruction suggested that the minimum size for a high school was 300 students (State of Wisconsin 1959), and a 1955 citizens' study report (Wisconsin Conference on Education 1955) provided additional detail on this point:

> The smaller the high school the fewer the electives and the more limited the offerings, while the larger the high school the more enriched and extensive the curriculum offered. In schools with enrollments under 100 . . . (t)here is . . . practically no foreign language, no mathematics beyond geometry, little or no speech or journalism, little art, only band and vocal music and no remedial work in any field. As school enrollments increase from 100 on up, the mathematics offerings increase, with schools over 300 offering as much as two full years beyond geometry. Schools of this same size and up also offer courses in speech, journalism and remedial reading, two years of foreign language, . . . more courses in science, in social studies, more art and more music. (p. 12)

The legislation following the initial report in 1948 led to a significant reduction in the number of Wisconsin high schools from 457 in 1947–48 to 421 in 1956–57. Despite the consolidations that occurred during the 1950s, in 1957 there was still a large number of relatively small high schools in the state. In our sample, 78 percent of the 278 districts had fewer than 100 seniors/year during the 1954–57 period. Among WLS respondents, 38 percent of our sample graduated from districts with 100 or fewer seniors.

The Department of Public Instruction data for our sample of districts are consistent with other reports of the time. The data show that the smaller high schools in the state were lower in average quality along the dimensions found important for student economic success. Figures 1–4 show the scatter plots

Figure 1

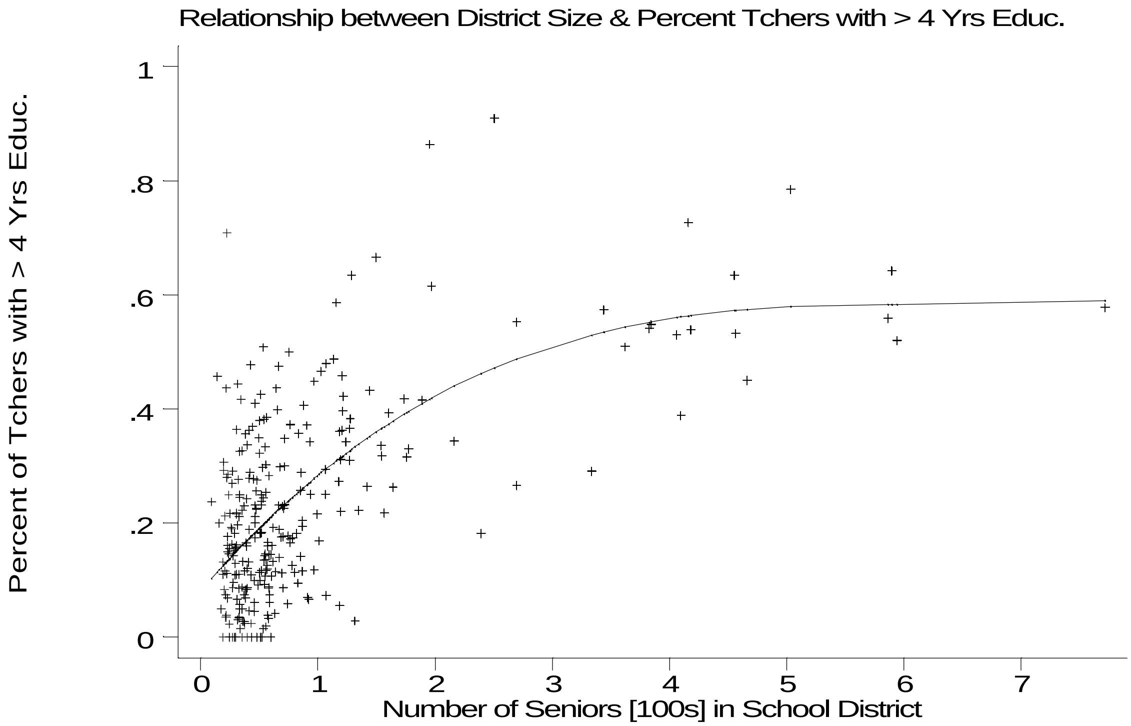



Figure 2

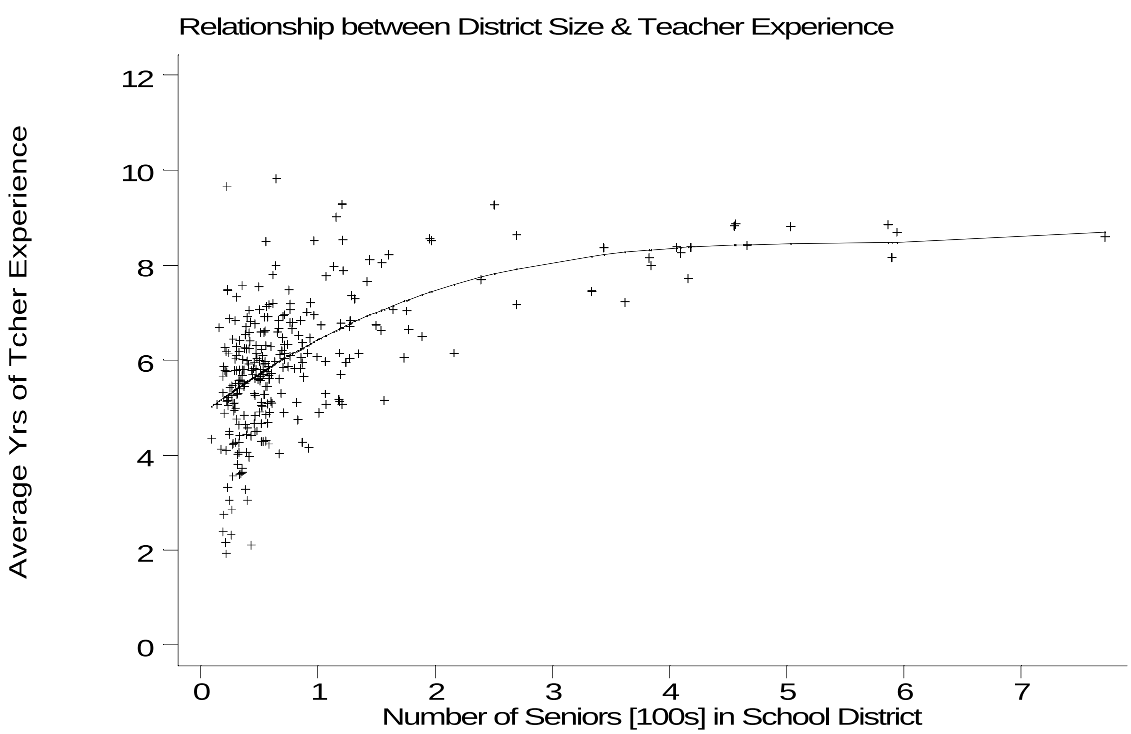


Figure 3

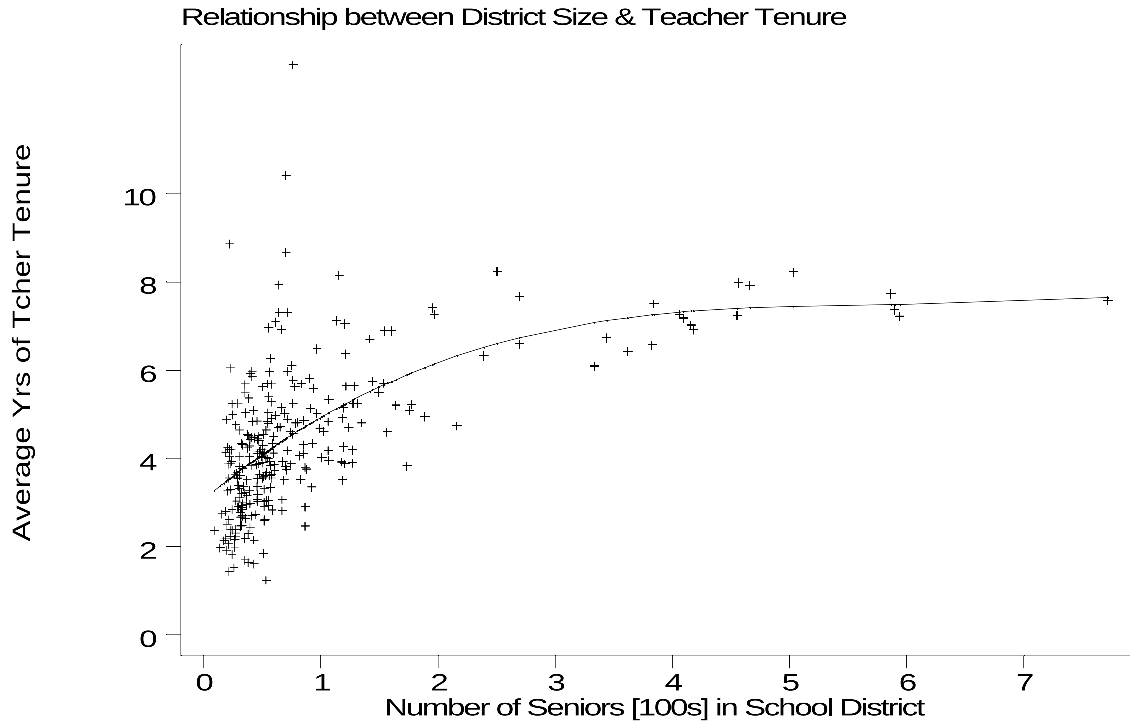
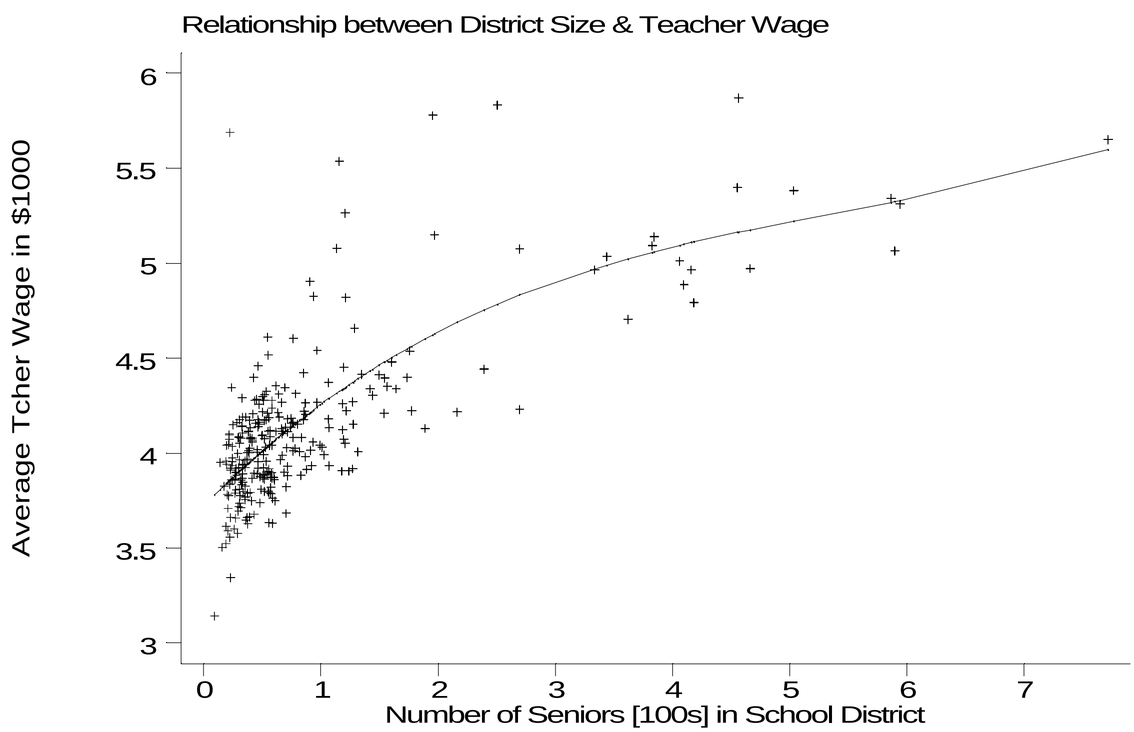



Figure 4



between school district size and the average teacher education, experience, tenure and teacher salary: smaller districts tended to have less-educated teachers and teachers with less experience. The correlation between the school resource variables and district size raises an important question: Are the earlier estimates of the effect of school resource variables on earnings captured by district size? The estimates in Table 5 answer this question in the negative. The first column reports earnings as a function of enrollment, enrollment squared and enrollment cubed, and family background characteristics. Column 2 adds the five resource variables, and the last column shows the effect of average teacher salary on earnings when size is included. Controlling for size, the hypothesis that the coefficients on the five school variables are jointly equal to zero is rejected (p-value = .01). Controlling for the five school resource variables, the hypothesis that the size variables are all equal to zero cannot be rejected at conventional levels of significance (p-value = .671). The joint hypothesis that both the school resource variables and the school size variables are jointly equal to zero is easily rejected with a p-value of less than .0001. In the last column of Table 5, results are reported for a model that includes the school size variables and the average teacher salary. This teacher salary coefficient is similar to the estimates reported in Table 4 that do not control for size. However, the estimated standard error is over twice as large due to the correlation between size and average salary shown in Figure 4.

Since the school districts that did not face the problems confronting smaller districts were primarily located in urban areas of the state, it is also possible that our estimated effects are picking up an urban/rural difference in midcareer earnings which is correlated with but not caused by differences in high school characteristics. Table 6 reports estimates that explore this possibility. In panel A the basic models are estimated with a dummy variable for districts in the greater Milwaukee area. The five school district variables in column 2 remain jointly significant (p-value = .0005), and column 3 shows that the coefficient on mean teacher salary is .065. This coefficient remains significant and declines by only about 10 percent from the .0723 estimate reported in Table 4.

**TABLE 5**
**OLS Estimates of the Effects of High School Size and School Resources on Annual Earnings**

| | (1) | (2) | (3) |
|---|---|---|---|
| Constant | 8.9643<br>(0.0553) | 8.6898<br>(0.4680) | 8.7015<br>(0.1127) |
| Father's Ed | 0.0107<br>(0.0029) | 0.0101<br>(0.0029) | 0.0104<br>(0.0029) |
| Mother's Ed | 0.0050<br>(0.0035) | 0.0045<br>(0.0035) | 0.0048<br>(0.0035) |
| Ln(Family Income) | 0.0940<br>(0.0141) | 0.0876<br>(0.0142) | 0.0877<br>(0.0143) |
| Length of School Year<br>(days) | | 0.0018<br>(0.0026) | |
| Fraction of Teachers with<br>>4 Years Post-HS Ed. | | 0.0893<br>(0.0731) | |
| Mean Years of Teaching<br>Experience | | 0.0295<br>(0.0141) | |
| Mean Years of Tenure<br>in District | | -0.0106<br>(0.0111) | |
| Pupil/Teacher Ratio | | -0.0069<br>0.0042 | |
| Mean Teacher Salary<br>($1,000) | | | 0.0763<br>(0.0285) |
| Size/100<br>(number of seniors) | 0.0476<br>(0.0178) | 0.0196<br>(0.0271) | 0.0142<br>(0.0217) |
| $Size^2/10^5$<br>(number of seniors) | -0.5738<br>(0.3074) | -0.3244<br>(0.3959) | -0.3102<br>(0.3225) |
| $Size^3/10^6$<br>(number of seniors) | 0.0012<br>(0.0007) | 0.0007<br>(0.0008) | 0.0007<br>(0.0007) |
| $R^2$ | 0.0582 | 0.0636 | 0.0607 |

**Note:** Standard errors in parentheses.

**TABLE 6**
**Alternative Specifications to Control for District Size**

| | Panel A Includes Dummy for Greater Milwaukee Area (n=2632) | | | Panel B Restricted to Urban Areas (n=845) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (1) | (2) | (3) |
| Constant | 8.9858 | 8.6237 | 8.7552 | 8.9749 | 6.6904 | 8.5662 |
| | (0.0549) | (0.4351) | (0.0827) | (0.1044) | (0.8178) | (0.2132) |
| Father's Ed | 0.0111 | 0.0100 | 0.0103 | 0.0133 | 0.0117 | 0.0128 |
| | (0.0029) | (0.0029) | (0.0029) | (0.0046) | (0.0045) | (0.0046) |
| Mother's Ed | 0.0046 | 0.0045 | 0.0048 | 0.0104 | 0.0104 | 0.0107 |
| | (0.0035) | (0.0035) | (0.0035) | (0.0061) | (0.0061) | (0.0061) |
| Ln(Family Income) | 0.1023 | 0.0873 | 0.0884 | 0.0988 | 0.0835 | 0.0901 |
| | (0.0138) | (0.0142) | (0.0143) | (0.0242) | (0.0242) | (0.0245) |
| Length of School Year (days) | | 0.0020 | | | 0.0083 | |
| | | (0.0025) | | | (0.0039) | |
| Fraction of Teachers with >4 Years Post-HS Ed. | | 0.0980 | | | 0.3101 | |
| | | (0.0667) | | | (0.1387) | |
| Mean Years of Teaching Experience | | 0.0285 | | | 0.1019 | |
| | | (0.0138) | | | (0.0484) | |
| Mean Years of Tenure in District | | -0.0100 | | | -0.0393 | |
| | | (0.0107) | | | (0.0399) | |
| Pupil/Teacher Ratio | | -0.0047 | | | 0.0052 | |
| | | 0.0038 | | | 0.0102 | |
| Mean Teacher Salary ($1,000) | | 0.0651 | | | 0.0835 | |
| | | (0.0175) | | | (0.0380) | |
| Greater Milwaukee | 0.0887 | 0.0299 | 0.0200 | | | |
| | (0.0236) | (0.0288) | (0.0299) | | | |
| $R^2$ | 0.0554 | 0.0634 | 0.0586 | 0.0587 | 0.0765 | 0.0641 |

**Note:** Standard errors in parentheses.

In panel B of Table 6 models are reported over the subsample of WLS respondents who attended high schools in urban areas as defined by the 1950 census. Among WLS graduates in urban areas, the estimates show a strong positive effect of school resources on earnings. The coefficients on the teacher education variable (.310), years of teaching experience (.102), and length of school term (.008) are larger than the coefficients in the overall sample reported in column 3 of Table 2. Furthermore, all three are individually statistically significant. The coefficient on the mean teacher salary is also larger than the estimate in the overall sample among WLS graduates from urban areas. We conclude from Tables 5 and 6 that the estimates in Tables 2 and 4 are not due to omitted variables correlated with school district size, geographic region, or urban/rural distinctions. These robustness checks, plus the results from the Hausman tests, lead us to conclude that our estimates reflect the causal effect of school resources on student economic achievement among Wisconsin high school graduates in the late 1950s.

## IX. SUMMARY AND CONCLUSIONS

In this study we have presented new evidence supporting a large positive relationship between high school inputs and the earnings of mature male workers. The analysis is based on a random sample of males graduating from Wisconsin high schools in 1957 and their earnings in 1974. Our estimates of the effect of school resources on earnings are substantially larger than previously published estimates. After controlling for parents' education and income, we find a that 1 percent increase in the average teacher salary in a district leads to a predicted 0.33 percent increase in the earnings of male high school graduates 17 years later. The key human capital characteristics of teachers that are correlated with this effect are their levels of experience and education. Although it is possible that these estimates are biased because of other omitted school or community characteristics correlated with school resources and student achievement, Hausman tests show that these omitted variables would most likely have to be independent of average parents' education and parents' income in the school district.

We believe our estimates reflect a causal effect of school resources on the later earnings of 1957 Wisconsin high school graduates. Schools and education have certainly changed since 1957. Thus, we cannot be certain that these estimates would correspond to the effect of differing resource levels in schools today. This, however, is a shortcoming of any study that seeks to study the long-run effect of any kind of educational policy. If research seeks to evaluate how education affects the adult experiences of children, researchers must wait until the children become adults.

Our estimates leave unanswered a number of interesting questions that we intend to address in future research using the WLS. These issues include the impact of high school resources on post-high school education choices, the relationship between high school inputs, migration, and earnings; the effect of high school inputs on the earnings of WLS respondents later in their careers; and the relationship between high school inputs and labor market outcomes for women.

**APPENDIX TABLE 1**
**OLS Estimates of the Impact of Family and School Resources on Annual Earnings in Models That Include Squared Resource Terms**

| Variable | | Variable | |
|---|---|---|---|
| Constant | 18.7284<br>(17.0183) | | |
| Father's Ed | -0.0088<br>(0.0126) | Father's Ed Squared | 0.0009<br>(0.0006) |
| Mother's Ed | -0.0115<br>(0.0187) | Mother's Ed Squared | 0.0009<br>(0.0009) |
| Ln(Family Income) | -0.0283<br>(0.0833) | Ln(Family Income) Squared | 0.0167<br>(0.0110) |
| Length of School Year (days) | -0.1049<br>(0.1894) | Days Squared | 0.0003<br>(0.0005) |
| Fraction of Teachers with >4 Yeas Post-HS ed. | -0.3246<br>(0.1759) | Teacher Education Squared | 0.4739<br>(0.2008) |
| Mean Years of Teaching Experience | -0.1085<br>(0.0633) | Mean Teacher Experience Squared | 0.0092<br>(0.0046) |
| Mean Years of Tenure in District | 0.0198<br>(0.0390) | Mean District Experience Squared | -0.0016<br>(0.0029) |
| Pupil/Teacher Ratio | 0.0100<br>(0.0416) | Pupil/Teacher Ratio Squared | -0.0002<br>(0.0010) |
| $R^2$ | 0.0693 | P-value for *F*-test for Joint Test That Squared Terms=0 | <.0001 |
| | | P-value for *F*-test for Joint Test That Squared School Inputs=0 | 0.0413 |

**Note:** Standard errors in parentheses.

**APPENDIX TABLE 2**
**Alternative Table 2 with Earnings Sample (n=2896)**

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Constant | 8.9072 | 8.7349 | 8.4124 | 7.6094 | 8.9159 | 8.7942 | 8.8532 | 8.8194 |
| | (0.059) | (0.467) | (0.462) | (0.397) | (0.059) | (0.065) | (0.060) | (0.096) |
| Father's Ed | 0.0126 | | 0.0110 | 0.0121 | 0.0111 | 0.0113 | 0.0114 | 0.0125 |
| | (0.003) | | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Mother's Ed | 0.0073 | | 0.0079 | 0.0080 | 0.0077 | 0.0075 | 0.0078 | 0.0075 |
| | (0.004) | | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Ln(Family Income) | 0.1215 | | 0.0994 | 0.1106 | 0.1028 | 0.1035 | 0.1054 | 0.1194 |
| | (0.015) | | (0.015) | (0.015) | (0.015) | (0.015) | (0.015) | (0.015) |
| Length of School Year | | 0.0037 | 0.0028 | 0.0075 | | | | |
| (days) | | (0.003) | (0.003) | (0.002) | | | | |
| Fraction of Teachers | | 0.1735 | 0.1057 | | 0.1943 | | | |
| w/ >4 Years Post-HS ed. | | (0.070) | (0.069) | | (0.043) | | | |
| Mean Years of Teaching | | 0.0217 | 0.0065 | | | 0.0275 | | |
| Experience | | (0.015) | (0.015) | | | (0.007) | | |
| Mean Years of Tenure | | 0.0026 | 0.0056 | | | | 0.0214 | |
| in District | | (0.011) | (0.011) | | | | (0.005) | |
| Pupil/Teacher Ratio | | -0.0019 | -0.0013 | | | | | 0.0043 |
| | | (0.004) | (0.004) | | | | | (0.004) |
| $R^2$ | | | | | | | | |

(table continues)

**APPENDIX TABLE 2, continued**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| P-value for Hausman test that the RE and FE coefficients are equal | 0.0011 | NA | 0.5641 | 0.0301 | 0.3825 | 0.3102 | 0.1868 | 0.0018 |
| P-value for Breusch & Pagan test that the variance of school district RE is equal to zero | 0.0038 | 0.6724 | 0.3517 | 0.5562 | 0.4850 | 0.9157 | 0.7991 | 0.0138 |

**Notes:** Standard errors in parentheses. RE = random effect; FE = fixed effect.

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