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Money Matters: Returns to School Quality Throughout a Career

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This paper exploits a newly created longitudinal dataset to evaluate the effect of high school resources on the earnings of male wage earners at mid and late career. Using school quality measures like average teacher salary and average years of experience of the teaching force, our regression results show generally large and significant effects of school quality on earnings throughout the careers of our sample members. The positive effects persist at least until the sample members are in their late fifties. Using the parameter estimates from these regressions, we then show that the returns to public investment in school quality are large and well worth the cost. Our calculations suggest a rate of return to marginal increases in school expenditures on teacher salary of about 11 percent. The data were constructed from the Wisconsin Longitudinal Study of the Class of 1957 augmented with historical records from the Wisconsin Department of Public Instruction.

1. Introduction

This paper exploits a newly created longitudinal dataset to evaluate the effect of high school resources on the earnings of male wage earners at mid and late career. Using school quality measures like average teacher salary and average years of experience of the teaching force, our regression results show generally large and significant effects of school quality on earnings throughout the careers of our sample members. The positive effects persist at least until the sample members are in their late fifties. Using the parameter estimates from these regressions, we then show that the returns to public investment in school quality are large and well worth the cost.

The dataset used in this study combines the Wisconsin Longitudinal Study of the Class of 1957 (WLS) with historical records from the Wisconsin Department of Public Instruction (DPI).[1] The WLS has individual and family information for respondents from 1954-1957 (when the respondents were in high school) as well as earnings data from both 1974 and 1992 (when respondents were in their mid-thirties and early fifties). The DPI records include measures of school quality for the years that the WLS respondents were in high school. The combined WLS-DPI dataset is the only microdataset of which we are aware that follows individuals throughout most of their careers and also includes good information on the high schools and family backgrounds of the same individuals. With this rich data, we are in a unique position to evaluate whether the measured effect of high school quality on earnings persists throughout an individual's career. Measuring the persistence (as well as the existence) of a school quality effect on earnings is particularly important because the extent to which returns persist has an important effect on the expected rate of return to public investments in school quality. Therefore, our estimates provide unique and valuable information on the returns to public investments in education.

[1] For a detailed description of the Wisconsin Longitudinal Study, see Hauser et al. (1993) and Sewell et al. (2001). For a detailed description of the Wisconsin Department of Public Instruction data and the collection process, see Olson and Ackerman (2000).

This is the second study to use the combined WLS-DPI data. In the first study to use this data, (Olson & Ackerman 2000 (O&A)), we find significant and positive effects of measurable features of school quality on the earnings of Wisconsin male graduates seventeen years following graduation. The estimates are substantially larger than previous estimates in the literature.[2] The specifications in O&A are straightforward: we regress the natural log of annual wage and salary earnings on a set of school quality measures and a limited set of variables that control for family and community characteristics. This simple specification captures the total effect of high school quality on earnings without specifying the precise causal path from school quality to later earnings. We explore the potential effects of measurement error, sample attrition, and omitted variable bias on our coefficient estimates and conclude that the results are robust to these potential problems.

This study estimates a richer model of the earnings determination process by including number of years of post-high school education, migration information and initial cognitive ability along with the variables above. Although including these variables decreases the size of the estimated effects of school quality on earnings, we cannot conclude from the resulting attenuation of the school quality effects that inclusion is appropriate. However, in this paper we elect to include them in order to make the strongest case for the existence of a positive and measurable effect of school quality on earnings. The next paragraphs discuss the arguments for and against the inclusion of educational attainment, migration, and initial cognitive ability in earnings regressions.[3]

Attending a good quality high school could enhance the value of additional years of schooling (and hence, increase attainment) by lowering the costs or increase the benefits of attaining more schooling. Better preparation before enrolling in post-secondary education could reduce the time and effort required to complete additional credits or degrees and better preparation before

[2] O & A report that a one percent increase in average teacher salary adds .33% to earnings of male students 17 years after high school graduation. For reviews of this literature, see Hanushek 1986, and Heckman, Layne-Farrar & Todd 1996.

[3] See also Card 1999 for a full discussion of the econometric relationship between education and earnings.

enrolling could generate relatively more human capital per year of additional schooling.[4] If this is the case, controlling for post-high school years of schooling in regressions will lead to an understatement of the total effect of school quality on earnings.

Attending a good quality high school could similarly influence migration decisions by increasing the returns to geographic mobility (Heckman et al., 1996). This may be especially important in the WLS where a large fraction of the sample grew up on farms or in small, rural communities. The relatively high quality schools in *some* of these small communities may have provided graduates from these communities with the skills and knowledge necessary to compete for higher skilled (and better paying) jobs in larger urban labor markets. Again, controlling for migration decisions may lead to an understatement of the total effect of school quality on earnings.

On the other hand, completed years of schooling and migration decisions could be correlated with school quality but not because of a causal relationship. For example, students who are likely to continue their education past high school may attend resource rich schools because of unobserved parental demand for education that affect both the post-high school educational choices of the children and the location choices of the parents. Similarly, individuals with above-average levels of personal initiative may be more successful in any work environment and also more likely to relocate. In either of these cases, estimates like those in O&A that fail to control for these two variables will be biased upward.

As with educational attainment and migration, there are theoretical arguments for and against the inclusion of test scores in earnings regressions because test scores are not perfect measures of underlying ability. Attending a good quality high school could have an effect on test scores either by increasing the cognitive ability of the students or by improving their test taking skills. If high school quality affects test scores, it is important that the test used to control for underlying ability be

[4] For example, students with stronger high school preparation might have greater access to (and success in) the technical majors, and through those majors, to the higher earnings that the technical fields command (Loury & Garman 1996). See also Ashenfelter & Rouse 2000, and Ackerman 2001.

administered before high school begins. Otherwise, the estimated effects of school quality on earnings will be biased downward to the extent that the high school experience contributed to test performance. Unfortunately, while freshman and junior test scores are available for many of the WLS respondents, the most complete data on test scores are from the junior year, and thus, subject to bias. For this reason, test scores are excluded from the analysis in O&A.

On the other hand, school quality and cognitive ability may be correlated for non-causal reasons. For example, if parents with higher ability children choose to live in districts with better schools, then the estimates of the effects of school quality on earnings will be biased upward in models that fail to control for test scores. This would occur if cognitive ability when measured in high school is heavily influenced by the home environment and if this parental influence on cognitive ability is positively correlated with parental preferences for high quality high schools.

In this study, our basic models control for the effects of completed years of education, migration, and an 11^{th} grade test score. Although inclusion may introduce a negative bias into our measures of the effects of school quality on earnings, this choice also minimizes the possibility of a large positive bias in our coefficients.

To preview our results, we find evidence that the returns to investments in school quality are large and persist late into an individual's work career even after controlling for migration, completed years of education and 11^{th} grade test scores. Also, we show that under a variety of assumptions on wages over the life cycle, and on the effects of school quality on these wages, the rate of return from a marginal investment per child in school quality is approximately 11 percent from a one percent increase in teacher salary.

2. The WLS and the WLS Sub-sample

The WLS is a random sample of 10,317 students who graduated from Wisconsin public high schools in 1957. Data were collected from school and state records in the late fifties and early sixties 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.[5] The survey is broadly representative of white American men and women with at least a high school education (Hauser et al. 1993). Merged to the sample of WLS respondents is information coded by O&A taken from the annual report that each school district was required to file with the Wisconsin DPI in June of each year. Data from annual reports were coded for the 1954-57 period for 336 of the 421 Wisconsin school districts that had students in grades 9-12.[6] These data include information about the training and salaries of the teaching staff, as well as student/teacher ratios, the length of the school year, disbursements, and receipts. A more detailed description of the data set and the data collection procedure can be found in O&A.

The three school quality measures used in this study are the measures O&A find to be significant determinants of 1974 earnings: average teacher salary, the fraction of the teacher workforce with more than four years of post-high school education, and average years of teaching experience. All three variables are measured at the district level, and are averaged over the four year period our sample attended high school (academic years 1953-54 through 1956-57). Although some districts include primary schools as well as high schools, the teacher information used to construct the school quality measures is restricted to high school teachers. Average teacher salary is measured in 1957 dollars. O&A find that average teacher salary captures most of the effect of the two teacher human capital variables on the 1974 earnings for male respondents.

The three family background characteristics included in the earnings model are mother's education, father'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), as well as in other samples

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[5] For a detailed examination of sample collection methods and of the characteristics of the sample see Sewell & Hauser 1975, 1977, and Hauser et al. 1993.

[6] Most of the excluded districts were very small and contained fewer than five sample members. These districts were

(Ackerman 2001, Altonji & Dunn 1996, Betts 1996). Years of education completed by both the mother and father were obtained from the interviews with the respondents in 1975. 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.[7] We assume that average earnings during these four years are highly correlated with family earnings when the WLS respondents were in elementary and secondary school.

Student cognitive ability is measured by the raw score on the Henmon-Nelson Test of Mental Ability (Henmon and Nelson, 1954) administered during the junior year of high school.[8] Migration is measured by variables that indicate if the individual lived in Wisconsin in the year under analysis, elsewhere in the upper Midwest, or if the individual had moved even further from Wisconsin. Finally, we include years of education completed by 1975.

The following steps were used to construct the sample of WLS respondents. The analysis was confined to males (n=4992) who responded to the survey in either 1975 or in 1991 (n=4571). From this sample, 558 were dropped because they attended private or parochial schools and no information is available on the resources used by these schools. A further 215 individuals were eliminated because they attended public schools for which no district level data is available.[9] From the remaining 3798, we eliminated 547 individuals for whom there was no information on parental

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significantly smaller than most school districts we see today.

[7] The household income data in the WLS is of unusually high quality. In most surveys, such income data would have been reported by the respondents and therefore been less reliable.

[8] When this test data was unavailable, the freshman score was used after properly inflating it to account for average gains in cognitive skill between freshman and junior year. For a few respondents, neither score was available. However, for this entire group, percentile rankings for IQ were available from the respondents' high schools. Usually, these rankings were based on relative performance on other ability tests administered by the schools. These scores were converted into raw score equivalencies. All computations were done by the WLS. For 91 percent of the WLS sample (93 percent of our final sample), the junior year raw score was available. For 6 percent of the WLS sample (6 percent of our final sample), the adjusted freshman raw score was available. For the remaining 3 percent, (1 percent of our final sample), the adjusted school reported IQ was used. Details about the test and regarding the estimation of test scores when junior year test data was unavailable can be found in unpublished WLS documentation. See Appendix D, Memo 121 and Appendix D, COR652, WLS.

[9] Most of these students attended very small schools – far smaller than are commonly found in the United States today.

income or parental education (n=3251). Three more individuals were eliminated due to missing education information or migration information. This left a sample of 3248 individuals. Of these individuals, 315 did not report positive wage or salary income in either 1974 or in 1992 and were therefore excluded from our sample; this left us a final sample of 2933 individual males. We observe salary and wage earnings for 2677 individuals in 1974 and 1975 individuals in 1992. Together these observations form our final dataset of 4652 individual/years. Earnings data for respondents were collected for 1974 in the 1975 interview and 1992 earnings were collected in the interviews that took place in 1992-93.

In this study, we report results for models where the dependent variable is the natural log of wage and salary income - we restrict the sample to those for whom wages and salaries were the primary source of income.[10] All earnings data for WLS respondents is presented in 1992 dollars. Sample statistics for the individuals in the 1974 sample and the 1992 sample are reported in Table 1 (note that most individuals appear in both samples). There is little obvious difference between the two samples, although this possibility is more rigorously examined in section 5.

Our final sample of 2933 individuals graduated from 307 high schools in 280 districts. 265 (94 percent) of these districts contained only one high school; nine districts (three percent) contained only two high schools. The data set includes at least one observation from school districts that collectively enrolled over 80 percent of the total population of seniors in Wisconsin in 1957. Across the two years, the number of observations per school district in the sample ranges from one observation for fifteen districts to 267 students from the Milwaukee public school system. The median number of observations per district across the two years is six.

3. The Estimates

[10] Restricting the sample in this way primarily eliminates farmers and self-employed professionals from the sample. O&A also eliminated those who reported weekly employment earnings under $40 in 1974 or who did not report a positive number of weeks worked. Because of differences in the survey questions, we were unable to use this selection

The basic model we estimate is as follows.

$$Y_{it} = X_{i,57}\beta + \beta_s S_{i,75} + \beta_g G_{i,t} + I[1974 = 1] * \{X_{i,57}\beta + \beta_s S_{i,75} + \beta_g G_{i,t}\} + \varepsilon_{i,t} \qquad \textbf{(1)}$$

where t = 1974 and 1992, Y measures the natural log of wage and salary income in year t, the vector X includes all those variables measured in 1957 (parental income and education, school quality measures, and tested ability), S is equal to the level of schooling attained by 1975, and G is equal to geographical location in the year that earnings are observed. I is an indicator variable that is set equal to 1 for all 1974 observations. By including interactions between this indicator variable all of the other variables in the model, this specification permits the impact of each variable on earnings to differ between 1974 and 1992. If we assume that X begins with a column of ones, the specification includes the ordinary constant and an additional intercept parameter to distinguish 1974 from 1992.

Tables 2 and 3 report the key results from estimating equation 1 for a variety of specifications. Table 2 reports results when teacher quality is measured by average teacher salary and Table 3 reports results when teacher quality is measured by the two human capital variables (average years of experience among teachers in the district and the fraction of the teachers in the district who have more than four years of post-secondary education). In both tables, specifications 1 and 2 control solely for parental education and family income. These specifications are analogous to the specifications in O&A estimated on the dataset that includes earnings observations for 1992.[11] In both tables, specifications 3 and 4 also control for migration, cognitive ability and completed years of schooling in 1975.

We commence the discussion of our basic results with a look at the coefficients on the control variables included in our models, beginning with cognitive ability. Across all specifications, this variable has a very strong positive effect on earnings that is not sensitive to the specification of

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criterion with the 1992 data. Therefore, we chose to retain these individuals in the sample.

teacher quality. The estimated effect of this variable on log wages is almost twice as large in 1992 as in 1975. Using the estimates in column 3 of Table 2 as an example, an 11 point difference in cognitive ability measured in 11th grade (about 1 standard deviation) has a 5.6 percent effect on 1974 earnings and a 10.1 percent effect in 1992. Each of these estimates is significantly different from zero and from each other.

The increase in the effect of cognitive ability on earnings from 1975 to 1992 could represent either a cohort or a life-cycle effect of cognitive ability on earnings. A cohort effect would be consistent with a change in the demand for skilled workers between the 1970s and the 1990s that favored workers with greater cognitive abilities (Levy & Murnane 1992). A life-cycle explanation for this effect is generated by the model developed by Farber & Gibbons (1996) where firm uncertainty about worker quality is reduced over time as firms observe more data on the productivity of individual workers. Decreasing uncertainty over time produces a stronger relationship between wages and cognitive ability later in a worker's career. Since the WLS is a single cohort of workers, it is impossible to separately identify these two effects with this data.

A second interesting result is the statistically insignificant effect of parents' education on the earnings of their children after controlling for migration, cognitive ability and completed years of education. When these variables are excluded from the model, parental education is statistically and economically significant. The results in column 1 of Table 2 show that a one-year increase in mother's education raises expected earnings in 1974 by .4 percent and a one-year increase in father's education raises expected earnings in 1974 by .9 percent. In 1992 parents' education had a still larger impact on the predicted earnings of their children: a one-year difference in mother's education led to a significant 1.9 percent change in earnings and a one-year difference in father's education produced a significant 2.3 percent difference in earnings. However, the estimates in column 3 of Table 2 also show that the measured effect of parents' education on children's earnings

[11] The regressions in this paper also include interaction effects for all variables with "year of observation."

goes to zero after conditioning on the three new variables in our main model; the point estimates are very small (less than .1 percent) and no longer significant. When these additional control variables are included in the model, the p-value for the hypothesis that parents' education has no effect on earnings in 1974 is .70 and in 1992 the p-value is .219. These results suggest that parents' education affects the earnings of their children through its impact on cognitive ability and postsecondary educational attainment.[12]

We now turn to the effects of school quality, our variable of interest. The estimates in columns 3 and 4 of Table 2 add controls for cognitive ability, completed years of education and mobility to the models estimated in columns 1 and 2. In column 3 an interaction term is included between the teacher salary variable and the 1974 indicator and in column 4 this interaction term is excluded. The estimates in column 3 show that the effect on the log wage of an additional $100 of pay for teachers on 1975 earnings is .0082 (p-value<.0001) and .0045 (p-value=.035, one-tail) on earnings in 1992. Although the estimated effect is larger in 1975, the hypothesis that the effect of average teacher salary on earnings is equal in the two periods cannot be rejected (p-value=.134) and the joint hypothesis that the estimated effects are jointly equal to zero is easily rejected. These estimates are very similar to the estimates obtained using the specification in O&A that excludes test scores, migration and completed years of schooling. When that specification is estimated with the additional observations for 1992 and the interaction effects, (column 1), we find that the point estimates on the teacher salary variable is .0083 for 1974 and .0056 for 1992. When the effect of school quality on earnings in 1975 and 1992 is constrained to be equal between the two years, the estimated coefficient on school quality is .0072 (p-value=.01). This latter estimate is only slightly smaller than the .0066 estimate reported in column 4 of Table 2 when completed years of education, migration and 11th grade test scores are included in the model.

[12] These results suggest that in this sample parents' education would be a valid instrument for education in a study that seeks to identify the causal effect of education on earnings. This issue is beyond the scope of this study and is left for future research.

The estimates reported in Table 3 examine the relationship between average teacher salary and the two teacher human capital measures. Comparing columns 1 and 2 of Tables 2 and 3 shows that adding the two measures of teacher human capital to the model adds little further information: the coefficients on all control variables are virtually unchanged, and the coefficient on average teacher salary is no longer significant or even of the expected sign. The specifications in columns 3 and 4 of Table 3 replace average teacher salary with the two human capital measures, but are otherwise identical to columns 3 and 4 in Table 2. The results show a significant relationship between earnings and these two measures of teacher human capital in both time periods. The joint hypothesis that these two variables have no effect on earnings is rejected with a p-value of less than .0001 for 1974 and .011 for 1992 earnings in column 3. As with the estimates reported in Table 2 that use average teacher pay, the hypothesis that teacher experience and education have a different effect on earnings in 1974 and 1992 cannot be rejected (p-value= .758). Column 4 constrains the effect of education and experience to have the same effect on earnings in the two periods. A one year increase in average experience level for teachers in the district raises earnings by 2.4 percent and a 10 percentage point increase in the fraction of teachers with at least 4 years of training beyond high school raises student earnings by .8 percent.

The similarity between the point estimates of the effect of teacher salaries on earnings in the models that include and do not include completed years of education, test scores and migration suggests that school inputs have a lasting effect on earnings independent of their effects on these other education and labor market outcomes.[13] The significance of the estimates of the effect of school quality on earnings after controlling for cognitive ability suggests that higher quality high schools provide more of the life-long knowledge and social and intellectual skills that are highly valued in the labor market than are provided by lower quality schools. Regardless of whether 11th grade test scores reflect the effects of the home environment, the school environment, or both, the

[13] The point estimates of the effect of teacher human capital on earnings are also similar between models that exclude

additional human capital students acquire in higher quality schools is valued in the labor market, and is not reflected in a single-dimensioned test of cognitive ability.

4. Are the Standard Errors Biased Downward?

The parameter estimates reported in Table 2 and Table 3 are OLS parameter estimates and the estimated standard errors are Huber corrected standard errors that account for the correlation in the error terms over time for individuals included in both the 1974 and 1992 sample (Rogers, 1994). Although these standard errors account for the error correlation caused by the panel structure of the data, they do not adjust for another potentially important type of correlation that could alter the estimated standard errors. This potential source of bias is caused by the fact that the sample includes multiple respondents from most school districts in the state and, therefore, the error terms in the earnings equations for individuals graduating from the same district may be correlated with one another because of unobserved community characteristics that affect later earnings. Since all individuals from the same school were assigned the same school quality measures, these correlated errors are likely to cause the standard errors on the school quality parameters in tables 2 and 3 to be biased downward (Moulton 1986, 1990). Because O&A found that OLS standard errors and Huber-corrected standard errors (with clustering on the school district level) were virtually the same for 1974 earnings, we do not think within-district correlation is a serious problem. However, we revisit this possibility because of the differences in model specification and slight differences in sample composition.

One way of controlling for the potential bias in the standard errors caused by the within district correlation in the error terms is to estimate the earnings models separately for the two years and compare the Huber-corrected standard errors for each year with the OLS standard errors. If the Huber and OLS estimated standard errors are sufficiently similar, then we conclude that the bias in

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and include measures of completed years of education, test scores, and migration. This regression is not displayed.

the standard errors in Tables 2 and 3 due to the clustering of observations within a district is small and can be safely ignored.

Table 4 reports select coefficients for specifications that include either mean teacher salary (specification1) or teacher education and teacher experience (specification 2) as well as the complete set of controls used in the models reported in columns 3 and 4 of Tables 2 and 3. The models are estimated separately on observations for each year; both the conventional OLS standard errors and the Huber corrected errors that account for the clustering of observations within school districts are reported. In the 1974 earnings regressions, the OLS standard error on teacher salary is about 7 percent smaller than the Huber standard error. However, the null hypothesis that there is no effect from average teacher salary on 1974 earnings is still strongly rejected using the Huber corrected standard errors. Similarly, when the two teacher human capital measures are included in the model instead of teacher salary, the OLS standard errors are smaller than the Huber corrected errors, but the joint hypothesis that these teacher human capital variables have no impact on earnings is strongly rejected using either set of standard errors (p-value<.0001).

By the time WLS respondents are in their early fifties in 1992, the standard errors suggest that the correlation between the error terms for respondents from the same school district has virtually disappeared. In the specification using average teacher salary, the OLS and Huber errors are virtually identical and in the specification using the two teacher human capital measures, the OLS standard errors are only slightly different than the Huber corrected errors. These results suggest that the standard errors reported in tables 2 and 3 are not significantly biased even though within-district clustering of observations is ignored.

Since the estimates for 1974 and 1992 do not come from independent samples, the estimates in Table 4 for the two separate years cannot be used to test hypotheses about the differences in the effects of school quality on earnings between the two time periods. Therefore, we also pursued a second estimation strategy that accounts for both the potential correlation across individuals within the same school district and the correlation within individuals over the two time periods. In this approach we collapse the data for each school district and time period and estimate the earnings models using mean data for each district-by-year data cell. Specifically, we use each district's school quality characteristics and the district-by-year mean values for the other variables in the model. Since a different number of observations are used to calculate the mean earnings value for each district and year, the estimates are weighted by the number of observations in each data cell. Collapsing the data in this way eliminates the correlation between individuals within a district because each observation now refers to mean earnings in either 1974 or 1992 calculated over all respondents from a district. The correlation between the error terms for WLS respondents in 1974 and in 1992 is still likely to be important because of the panel structure of the data – to control for this correlation, we use Huber-corrected standard errors.

Table 5 reports key coefficient estimates for four specifications using the mean data described above. In all four specifications, the complete set of control variables are included, along with interaction effects between each of these variables and year=1974. Column 1 reports estimates where the effect of average teacher salary is constrained to the same value in both years (but the effects of all other variables may differ between 1974 and 1992). Constraining the impact of average teacher salary on earnings produces an estimate of .0055 and a standard error of .0019. Compared to the corresponding estimate using the individual level data in column 2 of Table 2, this point estimate is slightly smaller (.0055 versus .0066) and the standard error is slightly larger (.0019 versus .0015). However, the .0055 estimate and its estimated standard error still leads to a strong rejection of the null hypothesis that the average teacher salary in 1957 has no impact on earnings in 1974 and 1992 (p-value = .003).

Figure 1 provides a visual summary of the relationship between the mean earnings of graduates from a district and average teacher salary for the model reported in column 1 of Table 5. The slope of the line in the figure is the estimated relationship between the average teacher salary and earnings (slope = .0055). The sizes of the data points shown in the figure are proportional to the number of observations in each cell. The figure shows a positive relationship between the earnings of graduates and the earnings of their high school teachers that is not strongly influenced by data outliers.

Column 2 of Table 5 allows teacher salary to affect 1974 and 1992 earnings differently. Like the corresponding estimates reported in column 1 of Table 2, the joint hypothesis that the coefficients on teacher salary and the interaction between the teacher salary and the 1974 indicator is strongly rejected (p-value = .0025). Also, as in Table 2, the hypothesis that the effects are the same in the two time periods cannot be rejected. However, the data also fail to reject the hypothesis that the effect of teacher salaries on earnings in 1992 is zero (p-value = .173).

Columns 3 and 4 of Table 5 report estimates where the average teacher salary in the district is replaced with the human capital measures for the teachers in the district. The model in column 3 constrains these variables to have the same effect in the two time periods. The joint hypothesis that the parameters on these two variables equal zero is strongly rejected using these data (p-value = .0002). In column 4 the effect of the two school quality variables can differ between years; the conclusions are identical to the conclusions we reached earlier. These two human capital variables have a statistically significant joint effect on earnings in both 1974 (p-value = .0025) and in 1992 (p-value = .029) and the joint hypothesis that these two variables have the same effect on earnings in 1974 and 1992 cannot be rejected (p-value = .408).

The conclusion we draw from Tables 4 and 5 is that the standard errors for the earlier estimates reported in Table 2 and Table 3 are not significantly biased due to a correlation among the regression error terms for individuals graduating from the same high school or district. Our conclusions about the effect of school quality on earnings are unchanged regardless of whether we control for this potential source of bias in the analysis. Therefore, all standard errors presented in the remaining sections of this research account for correlations across the same individual over time, but ignore potential correlation across individuals within the same district.

5. The Likely Impact of Sample Attrition on the School Quality Estimates

There was a substantial amount of sample attrition in our data between 1974 and 1992. In this section we explore the likely effect this attrition has had on the baseline estimates of the marginal effect of school quality on earnings. Of the 2677 individuals in the 1974 sample, only 1719 or 64 percent are also in the 1992 sample. On the other hand, only 256 individuals were included in 1992 that were not part of the sample in 1974. The descriptive statistics in Table 6 suggest there is reason to be concerned that the attrition was not exogenous. Compared to respondents included in the sample in both time periods, respondents that attrited between 1974 and 1992 had lower earnings in 1974 (9 percent lower), lower parental income (8 percent lower), lower 11th grade test scores (6.5 percent lower) and fewer years of education (.55 years less). These differences are statistically significant. There was not, however, any significant difference in school quality as measured by the average teacher salary between respondents in the two subgroups.

Table 7 presents coefficient estimates of the probability that a member of the 1975 earnings sample is not included in the 1992 earnings sample – in other words, the probability of attrition. These probabilities are estimated using a probit model where the dependent variable equals 1 if the individual is not in the 1992 earnings sample; the sample is restricted to those included in the 1975 sample. Column 1 reports coefficient estimates where attrition is solely a function of 1974 earnings. Column 2 reports coefficient estimates where attrition is a function of the standard control variables and average teacher salary. Column 3 reports coefficient estimates when attrition is a function of the standard control variables, average teacher salary *and* 1974 earnings.

In general, the regression results are consistent with what we observe in the summary statistics. The estimates in column 3 show a negative relationship between 1974 earnings and attrition in 1992 that remains significant even after controlling for the school quality, family background characteristics and personal characteristics included in column 2. If the correlation between 1974 earnings and attrition reflects an individual specific permanent component to earnings, these estimates suggest that workers with low earnings (conditional on the other covariates) are under-represented in the 1992 sample. The coefficient estimates imply that the probability of attrition by a worker with otherwise average observable characteristics but earning a wage at the 10^{th} percentile is .37 but only .32 if the same worker were at the 90^{th} percentile of the earnings distribution.

One method for investigating the potential impact of sample attrition on the estimated effect of school quality on earnings is to follow the strategy for modeling attrition based on "observables" developed by Fitzgerald, Gottschalk & Moffitt (1998). Throughout this example, we maintain the assumption that the subsample of WLS respondents in our 1974 earnings model is a random sample of the original WLS sample (thus we assume that the probability an individual is in our 1974 subsample is independent of the exogenous variables in the earnings model and of any unobservable characteristics affecting earnings in 1974. Under this maintained assumption, earnings in 1974 (or more precisely, log earnings) can be estimated as a function of teacher salary and the same set of family and individual control variables we have been using throughout this paper.[14] More formally, estimating $E[\ln E_{74} \mid \textit{school quality}, X]$ will provide unbiased estimates of the effect of school quality on earnings. E_{74} measures earnings in 1974; X equals our usual set of control variables and is hereafter suppressed.

We would like to estimate $E[\ln E_{92} \mid \textit{school quality}]$, the equivalent relationship for 1992 (note that X is suppressed). However, because of sample attrition, we only observe $E[\ln E_{92} \mid \textit{school quality}, A_{92} = 0]$ where A_{92} represents sample attrition (A_{92} equals zero for those sample members who remained in the sample through 1992. Columns 1 and 3 of Table 7 clearly shows that the

[14] These variables are mother's education, father's education, log household income, 11^{th} grade test score, completed

probability of attrition is correlated with 1974 earnings (this is true with and without our set of additional control variables). As a result, E[$ln\ E_{92}$ | *school quality*, $A_{92} = 0$] does not equal E[$ln\ E_{92}$ | *school quality*]. In other words, for individuals in the WLS, membership in the 1992 sample provides information about their expected earnings in that year. If E[$ln\ E_{92}$ | *school quality*, $A_{92} = 0$] is estimated when E[$ln\ E_{92}$ | *school quality*] *should* be estimated, the coefficient estimates of the effect of school quality will be biased due to this correlation.

Fitzgerald et al. provide a method to exploit information in the restricted (but observable) sample in order to estimate E[$ln\ E_{92}$ | *school quality*] in an unbiased manner using weighted least squares. First we estimate a model of sample attrition where the probability of attrition is a function of all of the exogenous variables in column 2 of Table 7 and $ln\ E_{74}$. This is the model estimated in column 3 of Table 7. We also estimate the model, E[$ln\ E_{92}$ | *school quality*, $A_{92} = 0$], which is the model estimated in column 1 of Table 8. Let υ_A equal the error term in the attrition model and let ε_{92} equal the error term in the earnings model. If υ_A and ε_{92} are independent but ε_{92} and $ln\ E_{74}$ are not independent because of an individual fixed component of earnings that is correlated with the probability of attrition, then weighted least squares applied to E[$ln\ E_{92}$ | *school quality*, $A_{92} = 0$] will produced unbiased estimates of the effect of school quality (recall that all regressions also includes the standard set of control variables listed in footnote 13). The appropriate weights are:

$$W = \{\Pr(A_{92} = 0 \mid ln\ E_{74}, school\ quality, X)/ \Pr(A_{92} = 0 \mid school\ quality, X)\}^{-1}$$

The assumption that υ_A and ε_{92} are independent rules out any biases created by sample selection on unobservables (Heckman 1979). This assumption would be violated if, for example, a transitory shock to earnings in the early 1990s were correlated with sample attrition. In our data, the 17 year time difference between 1974 and 1992 and the strong relationship between 1974 earnings

years of education by 1975, and geographic location.

and sample attrition in 1992 leads us to believe that much of the relationship between 1992 earnings and attrition in 1992 is a function of a "permanent" individual specific component rather than a transitory shock to earnings. Therefore we present the "corrected" estimates in Table 8.

Column 1 of Table 8 reports OLS estimates on the 1992 subsample and column 2 reports the weighted least squares coefficient estimates of 1992 earnings for the subsample of respondents that were in both the 1974 and the 1992 samples. The OLS point estimate of the effect of the average teacher salary on 1992 earnings is .0039 and slightly smaller than the .0045 estimate when the sample includes the 256 individuals that were in the 1992 sample but not in the 1975 sample (see Table 2 column 3). The weighted least squares estimate presented in column 2 is .0041 - virtually identical to the OLS estimate in column 1. The coefficient estimates are also nearly identical for most control variables in the model and very similar to the corresponding estimates in column 3 of Table 2.

These estimates suggest that sample attrition between 1974 and 1992 did not have a significant effect on the estimated effect of school quality on late career earnings. However, two caveats apply to this conclusion. First, these estimates do not rule out and do not test for parameter bias created by attrition on unobservable characteristics that creates a correlation between υ_A and ε_{92}. Second, the test assumes that the 1975 sample is representative of the original 1957 cohort of graduating high school seniors. Given these caveats, the similarity between the OLS and weighted least squares estimates strongly suggests that sample attrition between 1974 and 1992 has not significantly biased the estimated effect of school quality on the late career earnings of 1957 Wisconsin high school graduates.

6. Estimates of the Private Returns to Public Investments in School Quality

The estimates reported in the previous sections provide strong evidence of a positive and significant effect of school quality on the earnings of male Wisconsin high school graduates: all else equal, the students in school districts that paid higher teacher wages to obtain better educated and more experienced teachers were rewarded in the workplace. We have shown that the positive effect of good quality schools on earnings was true 17 years after graduation and that they persisted for at least another 15 years. In this section, we quantify what these estimated effects imply for the private rate of return wage and salary earners would receive from marginal increases in public expenditure on schools.

The smallest point estimate of the effect of school quality on 1992 wages is from the model that includes 11th grade test scores, completed years of education and geographic location and does not constrain the effects of school quality to be the same in both 1974 and 1992. The coefficient estimate from this model implies that an additional $100 in teacher salaries has a .0045 estimated effect on log earnings in 1992 (column 3 of Table 2). Evaluated at the sample mean for teacher salaries, (which is $4,600 in 1957 dollars in our sample), this estimate implies that a one percent increase in the average teacher salary increases earnings for a respondent in his early fifties by .21 percent. The same specification implies that this expenditure raises 1974 earnings by .38 percent.[15]

We can use these estimates to ask the following: if local taxpayers wished to spend more on their children's education, what would be the rate of return from these expenditures as measured by the higher wages their children could expect to receive throughout their careers? Evaluated at the mean teacher salary, the internal rate of return associated with a one percent (e.g. $46 per year) increase in the average teacher salary is the value of r that solves the following equation:

$$\sum_{t=6}^{17} \frac{-46/k}{(1-r)^{(t-6)}} = \sum_{t=25}^{52} \frac{e^{\ln E_t + \beta_t} * (.46) - e^{\ln E_t}}{(1+r)^{(t-6)}} \qquad \textbf{(2)}$$

where:

[15] Consistent with earlier decisions, we choose from among smallest point estimates so that our estimates of the private return to public investment are more conservative. We believe this makes the strongest case.

k = average pupil/teacher ratio (21.8 in our sample),[16]

$ln\ E_t$ = average log earnings in year t for a male Wisconsin high school graduate from the class of 1957, and

β_t = the estimated effect of an additional $100 spent on teacher salaries on the log wage of a graduate in year t (our coefficient estimates).

The left hand side of Equation (2) is the discounted cost of spending an additional $2.11 in 1957 dollars per student per year for 12 years beginning when a student enters the first grade at age 6. $2.11 is the share of the $46 in increased salaries per teacher allotted to each student. The $2.11 share in 1957 is equivalent to just under $14 in 2004 dollars and $46 in 1957 is equivalent to $301 in 2004 dollars. The right hand side of Equation (2) is the discounted benefit individuals could expect to receive throughout their career from this additional spending (assuming no returns until age 25). Equation (2) sets the student-teacher ratios and teacher salaries the same for all twelve years of schooling even though our results are based only on data from high schools. Essentially, we are assuming expenditures by a school district at the high school level are highly correlated with expenditures in grades 1-8 and the effect we have estimated represents the impact of better quality teachers through a student's entire tenure in the district. Note that we assume the earnings differential doesn't begin until age 25 and continues until age 52 when we last observe their earnings. Finally, note that we are assuming that the coefficients we have estimated on the sample of male earners apply equally to the female members of the WLS. We will investigate the effect of this last assumption on our estimates later in this section.

Since the WLS and the estimates reported above only provide information about $ln\ E_t$ and β_t for two points in time, r can only be calculated from Equation (2) after making additional assumptions about the time path for $ln\ E_t$ and β_t between ages 25 and 35 and between ages 35 and

[16] Please see the discussion later in this section regarding the assumption that both male and female students would benefit from increases in school quality in the manner predicted by our estimated coefficients.

52. Since there are a variety of plausible assumptions about the time-paths for these two variables, we calculate estimated rates of return using several different estimates for $ln\ E_t$ and β_t.

We use three different estimates of the time path for $ln\ E_t$ based on data from the 1950 to 1990 Censes. The first time path for $ln\ E_t$ models what a social planner in Wisconsin in the late fifties might have used to predict earnings: the observed cross-sectional relationship between age and earnings in the labor market at that time. If districts in Wisconsin were making judgments about what to spend on education based on the wage returns these investments would generate for their students, then cross-sectional age-earnings profile would have been a plausible basis for calculating these rates of return. Therefore, the first method employed to estimate $ln\ E_t$ in equation 2 uses data from the 1960 Census on male white workers born in Wisconsin with at least 12 years of education who were between the ages of 25 and 64. Estimating the natural log of yearly wages as a quadratic function of age on this 1960 sample produced the following estimates:

$$Ln\ E_{1t} = \underset{(.1747)}{6.6036} + \underset{(.0087)}{.0905} * Age - \underset{(.0104)}{.0976} * Age^2/100 \qquad \textbf{(3)}$$

R^2 = .043

N = 3536

The first *ln Et* profile uses the predicted values from this equation.

Using Equation (3) to predict the earnings of WLS graduates over their careers assumes real wages increase in calendar time only because of life cycle changes in productivity attributable to investments and returns to general and firm specific human capital. If real earnings also increase in response to broader productivity increases generated by technological change, then the earnings profile described by Equation (3) would shift upward over time in response to productivity improvements. The second model used to generate the $ln\ E_t$ profile incorporates this assumption. In this model, we assume the shape of the age-earnings profile is described by equation (3) plus a 1 percent increase per year. Using this method, we calculate the second set of *ln* E_t profile from the first according to the following equation:

$$\ln E_{2t} = \ln\left[\left(E_{1,25} * 1.01^{(t-25)}\right) * \left(E_{1t} / E_{1,25}\right)\right] \qquad \textbf{(4)}$$

where

t = 25, 26…52, and

E_{1t} = exp(*ln* E_{1t}), for all t where the *ln* E_{1t} values are from the first profile (equation 3).

The third method for estimating a *ln* E_t profile is based on the actual earnings of white, male workers born in Wisconsin with at least a high school education who were part of the 1957 high school class. Estimates of these earnings are derived from the appropriate individuals in the 1960, 1970, 1980 and 1990 Censes. In all cases, we use 1957 dollars. Specifically, we calculate the average earnings for workers from each Census who fell within a three-year interval and would have graduated from high school in or near 1957. Thus, we examine:

Census	**Age of Individuals**	**N**
1960	21 years old ± 1 year	337
1970	31 years old ± 1 year	543
1980	41 years old ± 1 year	538
1990	51 years old ± 1 year	517

The following regression relationship was generated when labor market earnings were regressed on a quadratic in age for 12 data points generated (three annual observations for each census):

$$ln\ E_{3t} = 4.6516 + .1993 * Age - .2320 * Age^2/100 \quad \textbf{(5)}$$

$$(.4574) \quad (.0272) \quad (.0375)$$

R^2 = .934

N = 12

The predicted wage from this regression is then used to estimate average wage and salary earnings of a graduate from the 1957 cohort – the third $ln\ E_t$ profile. Figure 2 plots the predicted earnings path for an "average" worker from the WLS cohort over his career using the three methods for computing $ln\ E_t$ for all relevant years.

In addition to the three estimated $ln\ E_t$ profiles, we generate six different sets of βt. The first estimate holds β_t constant over the career and equal to .0072. This corresponds to the estimates in column 2 of Table 2 where the effect of school quality is constrained to be the same in 1975 and 1992 and the model only controls for parental education and income. The second estimate of β_t also assumes that it is constant over the career but equal to the estimate from the model that controls for completed years of education, migration and test scores. This value is .0066 (column 4 of Table 2).

The remaining estimates of βt allow the paths to differ for younger and more mature workers (between ages 25 and 35 and ages 36 to 52, respectively. Two arbitrary but plausible assumptions are made about the time path of school quality effects over the life-cycle. In constructing the third and fourth sets of β_t we assume that β_t is equal to zero at age 25 and increases linearly to the 1975 point estimate and then declines linearly to the 1992 point estimate. Set three uses the point estimates in column 1 of Table 2 (the marginal effects of teacher salary are .0083 in 1974 and .0056 in 1992). Set four uses the point estimates in column 3 of Table 2 (the marginal effects of teacher salary are .0082 in 1974 and .0045 in 1992). In constructing the fifth and sixth set of β_t, we assume β_t is equal to the 1975 point estimate for all of the years between age 25 and 35 and then declines linearly from 1975 to the point estimate in 1992. Set five uses the same point estimates as set three (column 1 of Table 2) and set six uses the same point estimates as set four (column 3 of Table 2). In summary, the six time paths for βt are defined as follows:

Time Path		Value	Range
β_{1t}	=	.00715	for all t from 25 to 52
β_{2t}	=	.0062	for all t from 25 to 52
β_{3t}	= {	[(t-25)*.0083/10]	for all $25 \leq t \leq 35$
		.00833-[(.00833-.00559)/(17*(t-35))]	for all $36 \leq t \leq 52$
β_{4t}	= {	[(t-25)*.0082/10]	for all $25 \leq t \leq 35$
		.0082-[(.0082-.0045)/(17*(t-35))]	for all $36 \leq t \leq 52$
β_{5t}	= {	.00833	for all $25 \leq t \leq 35$
		.00833-[(.00833-.00559)/(17*(t-35))]	for all $36 \leq t \leq 52$
β_{6t}	= {	.0082	for all $25 \leq t \leq 35$
		.0082-[(.0082-.0045)/(17*(t-35))]	for all $36 \leq t \leq 52$

The six time-paths of β_t generated by these different estimates of the effect of school quality on earnings are shown in Figure 3.

The six sets of β_t are crossed with the three different earnings profiles defined by equations 3 through 5 to produce 18 different estimates of r, the rate of return to a $2.11/per student per year expenditure on teacher salaries. These values are shown in Table 9. All of the estimates shown in Table 9 are between 10.4 and 13.7 percent. The smallest value is generated from the regression model with completed years of education, test scores and geographic location and assumes the school quality effect is zero at age 25, peaks at age 35 and then declines to age 52. This estimate of

β_t combined with the wage profile generated from the cross-sectional relationship between earnings and wages from the 1960 Census produces a rate of return equal to 10.46 percent. The 13.7 percent return is obtained using the earnings profile estimated from the 1960, 1970, 1980 and 1990 Censes for the 1957 cohort and a school quality effect obtained from the model that controls only for family background variables, assumes the school quality effect between 25 and 35 years of age corresponds to the estimated effect at age 35 and the impact of school quality to declines between the ages of 35 and 52 to the point estimate for 1992 earnings.

The estimates in columns 1 and 2 are closer to what a social planner in the 1950s might have used if she "knew" the estimates of the returns but was unsure of the time-path of wages over the next 30 years for this cohort of workers used the cross-sectional relationship between earnings and age prevailing at the time. On the other hand, the estimates in column 3 of Table 8 are closest to the actual rates of return realized by this cohort since the wage profile for class of 1957 used in Equation (2) is based on data from this actual cohort over their working careers. The estimates in rows 3 and 4 allow the impact of school quality to increase from zero at age 25 to the point estimate obtained for 1975 and then assume the effect declines until the age of 52 when the impact of school quality on earnings matches the estimates obtained for 1992 earnings. The rate of return estimates in rows 3 and 4 of column 3 are virtually (11.41 versus 11.24 percent) identical even though one calculation is based on a model with only family background characteristics while the other estimate controls for migration, test scores and completed years of education.

Figure 4 illustrates the flow of costs and benefits from age 6 to 52 that generates the 11.24 percent return shown in row 4, column 3. The benefits of the $2.11/student expenditure over 12 years begin at zero at age 25 (by assumption), then increase to $24.75 at 35. The estimated benefits reach a maximum of $25 at age 37 and then decline to $13.01 at age 52.

To highlight the rate of return that is generated by investments in school quality, we compare the rate of return estimates in Table 9 with the rate of return that would be received by children from a transfer payment to families that matches the $2.11 per year used in the calculations above. In other words, we ask what the effect on a community's children would be if instead of increasing teacher salaries by 1 percent, the community provided a yearly tax break to families equal to $2.11 per child. To perform this back-of-the-envelope calculation, we used the coefficient on family income from a model that includes only family income, parental education and the average teacher salary in the district. An increase in family income of $2.11 corresponds to a .424 x 10-3 increase in the log of family income evaluated at the sample mean of log family income.[17] Multiplying this value by the coefficient on family income (e.g., .112) implies an increase in log earnings in 1975 of .427 x 10-4, a trivial increase in earnings. This estimate combined with earnings predictions from $ln\ E_3$ generates an estimated rate of return from this transfer payment that is negative. The net present value of this transfer payment on the earnings of children using a 10 percent discount rate is -$14.05.[18] In one respect this calculation is not surprising. The income a family receives from an income transfer may be spent on any number of consumption items other than investments in the human capital of the family's children. Thus, for at least for this cohort of Wisconsin high school graduates, public policies that invested more resources in schools would have had a much larger impact on children than a tax cut or transfer payment to parents of an equivalent sum of money.

The estimates reported in Table 9 are subject to several qualifications. Most importantly, the calculations are based on estimated school quality effects at only two points in the life cycle. It would certainly be desirable to have the complete earnings histories for this sample to more precisely estimate the impact of school quality on earnings and the returns to investments in school quality. On the other hand, the two point estimates are at very different points in the careers of these workers and the estimated effects of school quality at these points are estimated with significant precision.

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[17] Recall that family income is measured in hundreds of 1957 dollars. Thus, $.424 \times 10^{-3} = \ln(49.799+.0211)-\ln(49.799)$.
[18] The present value of the cost of the $2.11/year transfer payment for 12 years is -$14.38 at a 10 percent discount rate while, the present value of the benefit from the transfer payment is 33 cents.

Second, the rates of return calculations are based on estimates of the effect of school quality on the earnings of males graduating from Wisconsin high schools in 1957 and ignore possible differences in the returns to school quality by gender. Since public high school classrooms include both male and female students, the hypothetical $46/teacher investment used in our calculations would be spread across both female and male students in a district and this investment then needs to be compared to the returns received by both male and female students. Our calculations implicitly assume the earnings returns to school quality are equal for men and women. If, however, the earnings returns to school quality are lower for women than for men, then the average return to school quality as measured by later earnings will be less than the 11 percent we calculate. For example, the rate of return calculation of 11.24 reported in column 3, row 4 declines to 8.36 percent if we assume that school quality has no impact on the earnings of WLS women. Future research will replicate the analysis here on the women in the WLS and carefully consider additional life-cycle outcomes measures that might be influenced by school quality which are meaningful for this particular cohort of women.

While our estimates of school quality effects on earnings are quite large compared to virtually all of the estimates in the literature, the 11 percent rate of return is plausible. This estimate is very close the 10 percent return Heckman (2000) cites as a plausible estimate of the average rate of return to human capital investments across all ability groups.

7. Summary and Conclusions

In this paper we investigate the effects of high school characteristics on the career earnings of a random sample of male, Wisconsin high school graduates from the class of 1957. The school quality measures were collected directly from archived reports filed by school districts with the state of Wisconsin over the 1954-57 time period. These data were matched to individuals in the WLS where earnings data were available when respondents were in their mid-thirties and early fifties.

School quality in the district was measured as the average teacher salary in the district, the average years of teacher teaching experience and the fraction of teachers with four or more years of post-high school training. These measures were significantly and positively related to earnings of WLS respondents in 1974 and 1992 after controlling for parents' education, parents' income, completed years of schooling, cognitive ability measured in the 11th grade and migration. The hypothesis that the average teacher salary has an equal effect on earnings in 1974 and 1992 could not be rejected. In some specifications, however, the hypothesis that teacher's salary had no effect on earnings in 1992 could not be rejected. Even for these specifications, when school quality is measured using teacher education and experience, the hypothesis that these two variables have no effect on earnings is rejected for both 1974 and 1992. These results, which are consistent with our earlier study where we only controlled for parents' education and income, suggest the reason the average teacher salary in a district has an impact on the earnings of students is through its impact on the quality of the teaching workforce obtained in districts paying higher salaries.

Our estimates are not only statistically significant, but the magnitudes of the estimates imply an economically meaningful impact of local investments in school quality on the career earnings of students. After making plausible assumptions about the earnings path of Wisconsin graduates from the 1957 cohort and the time-path of the impact of school quality on earnings over the career, we find the estimated increase in earnings generated by a small increase in the average teacher salary in a district generates an internal rate of return of about 11 percent.

One might question the policy relevance of a study that focuses on schooling experiences from over fifty years ago for one relatively homogenous sample of individuals. After all, we have seen large demographic shifts in the characteristics of America's school-age population, as well as changes in school policy, and in the broader economy. However, it is only from studying mature cohorts like this one that we can learn about the potential long-run effects of the policy choices we make today. By necessity, mature workers would have attended school a long time ago. These changes may have diminished the likelihood of seeing such large returns from a similarly small investment in school quality today. Only time and the creation of similar panels to this one will be able to answer this question more completely.

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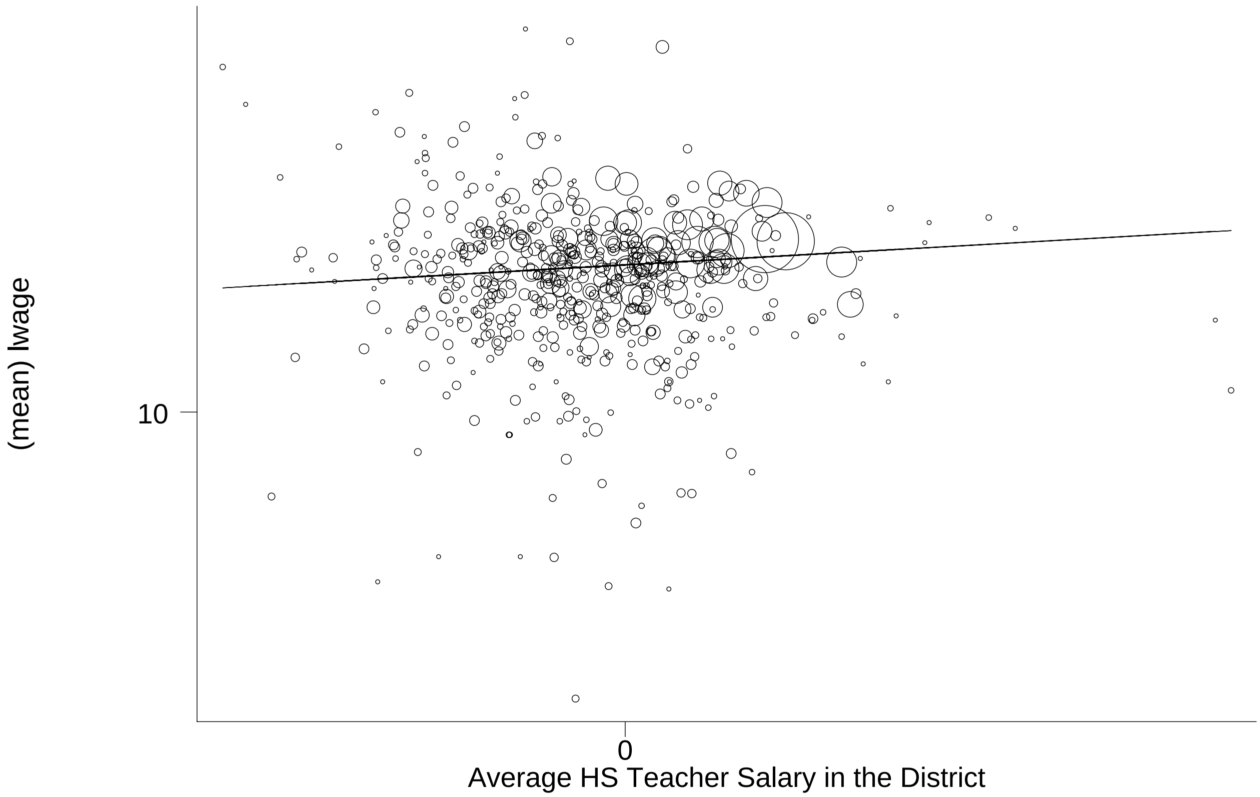
Figure 1
The Relationship Between
Average Teacher Salary and Earnings Estimated on Pooled Data
(Each Observation is the District Mean within Year for Each Variable)



The slope of the regression line is .0055.
The size of the circles is proportional to the number of individuals in each district year.

Figure 2
Alternative Predicted Earnings Paths for Sample Members

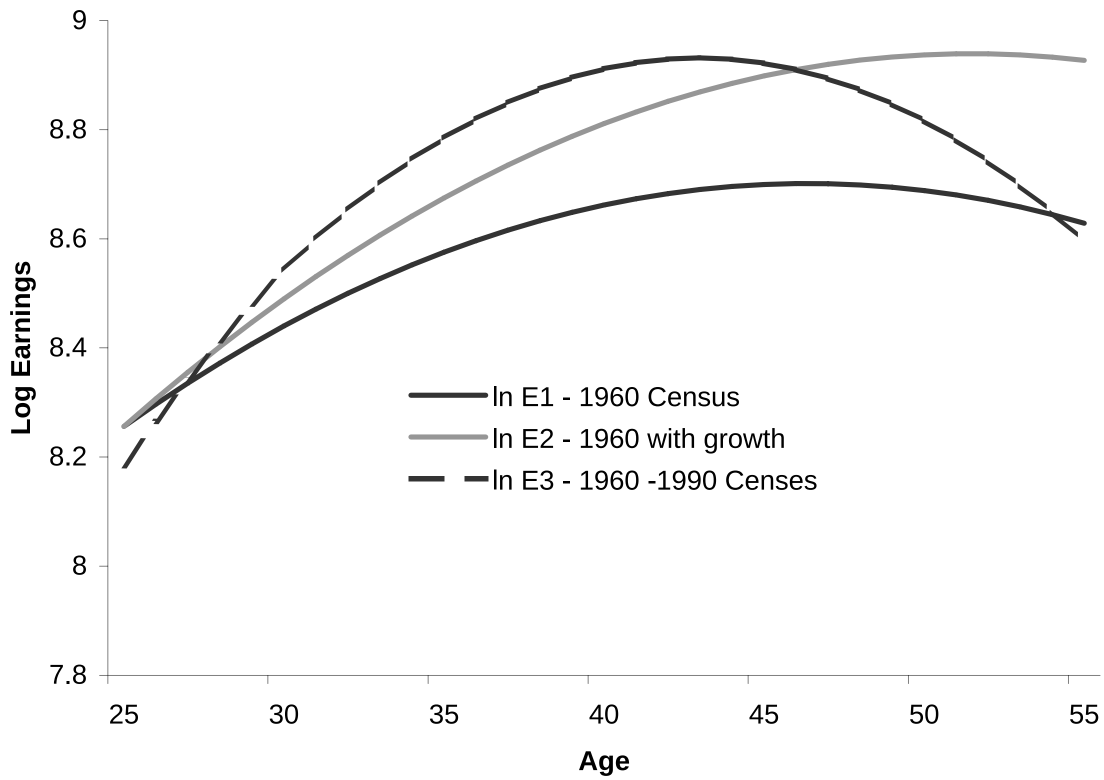


Figure 3
SixTime Paths of the Effect of School Quality on Earnings

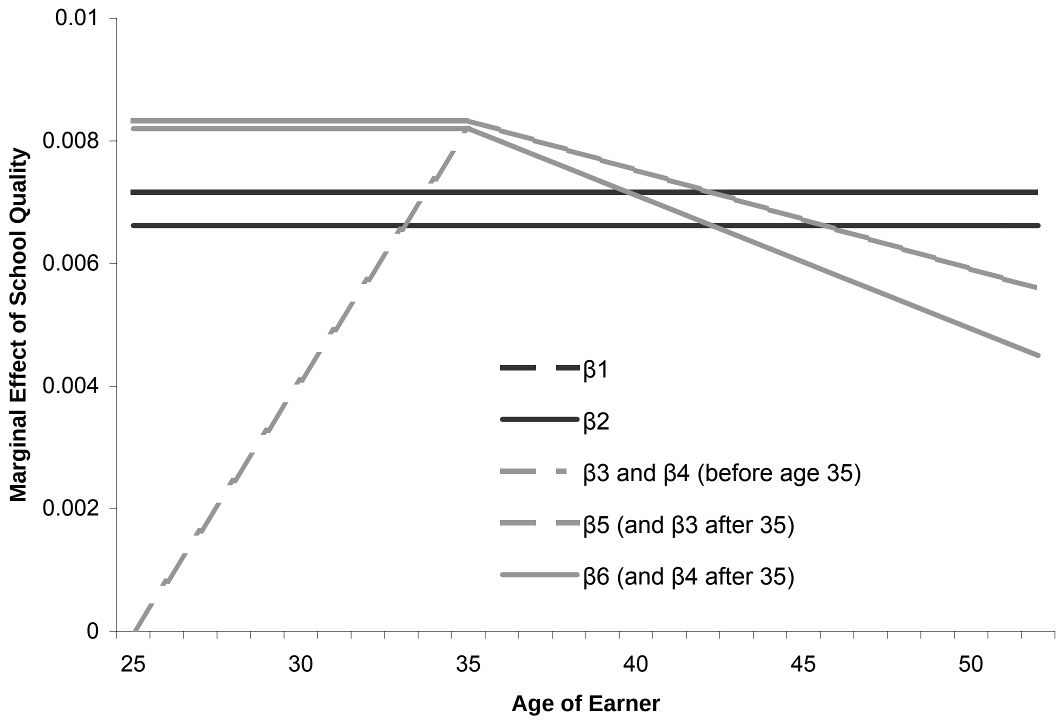
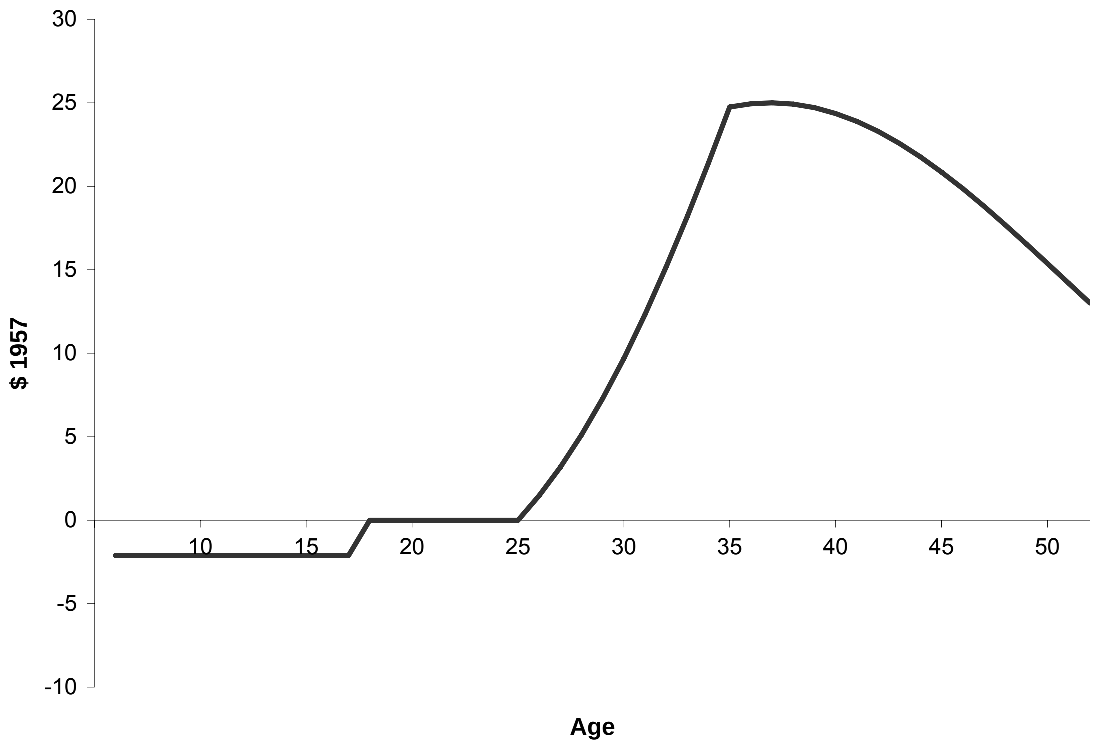


Figure 4
Flow of Costs and Benefits from Investing $2.11 per Student in Teacher Salaries on Student Earnings (all values in $1957)



This figure shows the flow of costs and benefits used to compute the 11.24% rate of return shown in column 3 of Table 9. It uses the coefficients from column 3 of Table 2 and β_4

TABLE 1
Descriptive Statistics
Mean (SD)

Variable	Calculated over the 1975 Sample (N=2677)	Calculated over the 1992 sample (N=1975)
Ln(Earnings of WLS Respondents) ('92 $)	10.600	10.642
	(0.499)	(0.721)
Mother's Education	10.603	10.668
	(2.757)	(2.755)
Father's Education	9.816	9.852
	(3.366)	(3.379)
ln(Family Income measured in $100)	3.908	3.928
	(0.669)	(0.665)
Lives outside the Midwest	0.167	0.212
	(0.373)	(0.409)
Lives in Midwest but outside of WI	0.111	0.089
	(0.314)	(0.284)
Completed Years of Education	13.749	13.939
	(2.413)	(2.492)
11th Grade Intelligence test - raw score	56.602	57.809
	(11.685)	(11.243)
Average HS Teacher Salary (100 of '57$)	46.208	46.126
	(6.494)	(6.467)
Average Yrs of Tcher Tenure in District	5.786	5.768
	(1.834)	(1.813)
Average Yrs of Tcher Experience	7.073	7.061
	(1.477)	(1.468)
Fraction of Tchers with GE 4 yrs post-HS Education	0.39	0.388
	(0.222)	(0.220)
Length of the School Year (Days)	178.4	178.34
	(4.160)	(4.185)
Pupil/Teacher Ratio	21.826	21.862
	(2.437)	(2.454)

TABLE 2
OLS Estimates of the Effect of School Quality on Earnings

Variable	(1)	(2)	(3)	(4)
Average Teacher Salary, ($100)	0.0056 (0.0027)*	0.0072 (0.0016)**	0.0045 (0.0025)	0.0066 (0.0015)**
Aver. Tcher Salary x 1974 indicator	0.0027 (0.0026)		0.0037 (0.0025)	
Mother's Education	0.0193 (0.0065)**	0.0194 (0.0065)**	0.0053 (0.0059)	0.0055 (0.0059)
Father's Education	0.0230 (0.0056)**	0.0226 (0.0056)**	0.0054 (0.0053)	0.0049 (0.0053)
Log Household Income	0.1161 (0.0276)**	0.1115 (0.0269)**	0.0611 (0.0261)*	0.0550 (0.0255)*
11th Grade Test Score of Cognitive Ability			0.0092 (0.0018)**	0.0092 (0.0018)**
Completed Years of Education, 1975			0.0652 (0.0074)**	0.0653 (0.0074)**
Lives in midwest, outside of Wisconsin			0.2022 (0.0487)**	0.2020 (0.0488)**
Lives outside the midwest			0.1829 (0.0396)**	0.1820 (0.0397)**
Indicator for 1974	0.2170 (0.1367)	0.3066 (0.1017)**	0.6588 (0.1548)**	0.7773 (0.1241)**
Mother's Education x 1974 indicator	-0.0152 (0.0062)*	-0.0153 (0.0062)*	-0.0081 (0.0059)	-0.0084 (0.0059)
Father's Education x 1974 Indicator	-0.0138 (0.0053)**	-0.0133 (0.0053)*	-0.0055 (0.0051)	-0.0047 (0.0051)
Log Household Income x 1974 indicator	-0.0216 (0.0272)	-0.0133 (0.0263)	0.0044 (0.0268)	0.0156 (0.0259)
Test Score x 1974 indicator			-0.0041 (0.0016)*	-0.0040 (0.0016)*
Completed Yrs of Educ x 1974 indicator			-0.0344 (0.0078)**	-0.0346 (0.0078)**
Lives in Midwest, Outside of WI x 1974 indicator			0.0034 (0.0487)	0.0023 (0.0488)
Lives Outside Midwest x 1974 indicator			-0.0922 (0.0423)*	-0.0922 (0.0422)*
Constant	9.4959 (0.1416)**	9.4440 (0.1213)**	8.5856 (0.1536)**	8.5163 (0.1364)**
Observations	4652	4652	4652	4652
R-squared	0.056	0.055	0.159	0.158

Robust standard errors in parentheses
* significant at 5% level; ** significant at 1% level

TABLE 3
Additional OLS Estimates of the Effect of School Quality on Earnings

Variable	(1)	(2)	(3)	(4)
Average Teacher Salary, ($100)	-0.0099 (0.0055)	-0.0038 (0.0033)		
Average Yrs of Exper. for Teachers	0.0517 (0.0193)**	0.0353 (0.0127)**	0.0255 (0.0155)	0.0243 (0.0102)*
Fraction of Teachers With 4 Yrs Post-HS Education	0.2127 (0.1402)	0.1558 (0.0872)	0.0477 (0.1059)	0.0832 (0.0670)
Aver. Tcher Salary x 1974 indicator	0.0108 (0.0054)*			
Aver. Tcher Exper x 1974 indicator	-0.1012 (0.1348)		-0.0021 (0.0172)	
Fraction of Tchers with 4 or more Yrs Training x 1974 indicator	-0.0287 (0.0210)		0.0618 (0.1088)	
Mother's Education	0.0186 (0.0065)**	0.0189 (0.0065)**	0.0053 (0.0059)	0.0053 (0.0059)
Father's Education	0.0222 (0.0057)**	0.0221 (0.0056)**	0.0049 (0.0053)	0.0047 (0.0053)
Log Household Income	0.1139 (0.0276)**	0.1102 (0.0269)**	0.0561 (0.0261)*	0.0538 (0.0256)*
11th Grade Test Score of Cognitive Ability			0.0091 (0.0018)**	0.0091 (0.0018)**
Completed Years of Education, 1975			0.0650 (0.0074)**	0.0651 (0.0074)**
Lives in midwest, outside of Wisconsin			0.2006 (0.0487)**	0.2003 (0.0487)**
Lives outside the midwest			0.1792 (0.0397)**	0.1786 (0.0396)**
Constant	9.7871 (0.1987)**	9.6547 (0.1472)**	8.6281 (0.1417)**	8.6347 (0.1317)**

Robust standard errors in parentheses
* significant at 5% level; ** significant at 1% level

continued

TABLE 3
Additional OLS Estimates of the Effect of School Quality on Earnings

Variable	**(1)**	**(2)**	**(3)**	**(4)**
Mother's Education x 1974 indicator	-0.0149	-0.0154	-0.0083	-0.0084
	(0.0062)*	(0.0062)*	(0.0059)	(0.0059)
Father's Education x 1974 Indicator	-0.0135	-0.0133	-0.0051	-0.0048
	(0.0053)*	(0.0053)*	(0.0052)	(0.0051)
Log Household Income x 1974 indicator	-0.0207	-0.0139	0.0109	0.0150
	(0.0272)	(0.0263)	(0.0269)	(0.0259)
Test Score x 1974 indicator			-0.0040	-0.0040
			(0.0016)*	(0.0016)*
Completed Yrs of Educ x 1974 indicator			-0.0346	-0.0346
			(0.0078)**	(0.0078)**
Lives in Midwest, Outside of WI x 1974 indicator			0.0035	0.0035
			(0.0487)	(0.0487)
Lives Outside Midwest x 1974 indicator			-0.0920	-0.0915
			(0.0423)*	(0.0423)*
Indicator for 1974	0.0760	0.3101	0.7922	0.7796
	(0.1935)	(0.1016)**	(0.1516)**	(0.1238)**
Observations	4652	4652	4652	4652
R-squared	0.060	0.059	0.161	0.160

Robust standard errors in parentheses

* significant at 5% level; ** significant at 1% level

TABLE 4
Select Coefficient Estimates of School Quality on Earnings
to Compare OLS and Huber Corrected Standard Errors

	Sample			
	1974 Observations		1992 Observations	
Variable	**(1)**	**(2)**	**(3)**	**(4)**
Average Teacher Salary, ($100)	0.0082		0.0044	
	(0.0015)		(.0024)	
	[.0016]		[.0024]	
Average Yrs of Exper. for Teachers		0.0234		0.0254
		(.0098)		(.0158)
		[.0121]		[.0174]
Fraction of Teachers With More than 4 Yrs Post-HS Educ		0.1094		0.0474
		(.0653)		(.1060)
		[.0826]		[.1016]
P-value for Hypothesis that School Quality Has no Impact on Earnings Using (2-tail):				
OLS Standard Errors	<.001	<.001	0.066	0.014
Huber Standard Errors	<.001	<.001	0.064	0.0098

OLS standard errors are in parentheses & Huber standard errors are in brackets.

Each model also includes mother and father's education, family income, geographic location, completed years of education and the 11th grade test score.

TABLE 5
Estimates of School Quality on Earnings in Regressions on Sample Means
(each observation is a district year)

Variable	**(1)**	**(2)**	**(3)**	**(4)**
Average Teacher Salary, ($100)	0.0055	0.0042		
	(.0019)	(.0031)		
Average Teacher Salary, ($100) x 1974 Indicator		0.0025		
		(.0035)		
Average Yrs of Exper. for Teachers			0.024	0.0274
			(.0114)	(.0184)
Average Yrs of Exper. for Teachers x 1974 Indicator				-0.0061
				(.0208)
Fraction of Teachers With More than 4 Yrs Post-HS Education			0.0726	0.0516
			(.0750)	(.1084)
Fraction of Teachers With More than 4 Yrs Post-HS Education x 1974 Indicator				0.0367
				(.1162)
P-value for Hypothesis that School Quality Has no Impact on Earnings Using (2-tail):	0.003	0.0025	0.0002	0.0017

Huber standard errors are in brackets.
N=550

Each model also includes mother and father's education, family income, geographic location, completed years of education, the 11th grade test score, a 1974 indicator variable and interaction terms between the 1974 indicator and all of the other variables in the model.

Each observation is weighted by the number of WLS respondents in the data cell.

TABLE 6
Descriptive Statistics for Those who Responded in Both Time Periods
and for those who Respondended only in 1992
Mean (SD)

Variable	Calculated over those in 1975 and 1992 samples	Calculated over those in 1975 but not in 1992 sample
Average Teacher Salary, ($100)	46.221	46.184
	(6.479)	6.526
Mother's Education	10.642	10.533
	(2.771)	(2.732)
Father's Education	9.888	9.686
	(3.404)	(3.239)
Log (Household Income in $100 units)	3.938	3.855
	(0.657)	(0.688)
11th Grade Test Score of Cognitive Ability	57.960	54.166
	(11.247)	(12.061)
Completed Years of Education, 1975	13.948	13.394
	(2.491)	(2.225)
Lives in midwest, outside of Wisconsin	0.116	0.101
	--	--
Lives outside the midwest	0.162	0.174
	--	--
Log 1974 Earnings	10.633	10.540
	(0.483)	(0.522)
N	1719	958

TABLE 7
Coefficient Estimates from Three Probit Models of the Probability of Attrition from the 1975 Sample by 1992

Variable	**(1)**	**(2)**	**(3)**
Log 1974 Earnings	-0.2271		-0.1422
	(0.0497)**		(0.0535)**
Average Teacher Salary, ($100)		0.0066	0.0079
		(0.0042)	(0.0042)
Mother's Education		0.0087	0.0082
		(0.0106)	(0.0106)
Father's Education		0.0037	0.0037
		(0.0089)	(0.0089)
Log Household Income		-0.0887	-0.0800
		(0.0423)*	(0.0425)
11th Grade Test Score of Cognitive Ability		-0.0156	-0.0149
		(0.0025)**	(0.0025)**
Completed Years of Education, 1975		-0.0388	-0.0345
		(0.0129)**	(0.0130)**
Lives in midwest, outside of Wisconsin		0.0446	0.0740
		(0.0836)	(0.0844)
Lives outside the midwest		0.2455	0.2589
		(0.0734)**	(0.0736)**
Constant	2.0411	0.9084	2.2261
	(0.5271)**	(0.2423)**	(0.5533)**
Observations	2677	2677	2677

Standard errors in parentheses
* significant at 5% level; ** significant at 1% level

TABLE 8
OLS and "Attrition Corrected" Weighted Least Squares Estimates of the Effect of Family Characteristics, Teacher Salary and Control Variables on 1992 Earnings

Variable	**OLS**	**Weighted Least Squares**
Average Teacher Salary, ($100)	0.0039	0.0041
	(0.0025)	(0.0025)
Mother's Education	0.0074	0.0072
	(0.0064)	(0.0065)
Father's Education	0.0023	0.0018
	(0.0054)	(0.0054)
Log Household Income	0.0607	0.0598
	(0.0260)*	(0.0263)*
11th Grade Test Score of Cognitive Ability	0.0093	0.0093
	(0.0016)**	(0.0016)**
Completed Years of Education, 1975	0.0676	0.0689
	(0.0074)**	(0.0075)**
Lives in midwest, outside of Wisconsin	0.1947	0.1971
	(0.0542)**	(0.0547)**
Lives outside the midwest	0.1885	0.1843
	(0.0409)**	(0.0413)**
Constant	8.5904	8.5639
	(0.1505)**	(0.1520)**
Observations	1719	1719
R-squared	0.187	0.186

Standard errors in parentheses
* significant at 5% level; ** significant at 1% level

TABLE 9
Estimated Rate of Return from a 1% Increase in Average Teacher Salary
Using Alternative Estimates of the Effect of School Quality on Earnings and of the
Wage Profiles for Male Graduates of the Wisconsin High School Class of 1957

		Estimate of the Wage Profile:		
		(1)	**(2)**	**(3)**
	School Quality Effect:	**Cross-Section, 1960 Census** *ln E 1*	**Cross-Section, 1960 Census Adjusted 1%/Year** *ln E 2*	**1957 cohort from 1960, 1970, 1980 & 1990 Census** *ln E 3*
Estimates that constrain school quality Effects to be equal over 1974-1992:				
1	Controls include only family background variables (Table 2, Col. 2) (β_1)	12.55%	12.94%	13.13%
2	Controls include family background, completed ed, geog location, test scores (Table 2, Col 4) (β_2)	12.17%	12.56%	12.75%
Estimates that allow school quality Effects to vary between 1974 and 1992; Est. Quality effect begins at 0 at age 25:				
3	Controls include only family background variables (Table 2, Col. 1) (β_3)	10.65%	11.14%	11.41%
4	Controls include family background, completed ed, geog location, test scores (Table 2, Col 3) (β_4)	10.46%	10.94%	11.24%
Estimates that allow school quality Effects to vary between 1974 and 1992; Est. Quality effect at age 25 equals 1974 Estimate:				
5	Controls include only family background variables (Table 2, Col. 1) (β_5)	13.14%	13.50%	13.70%
6	Controls include family background, completed ed, geog location, test scores (Table 2, Col 3) (β_6)	12.99%	13.35%	13.55%

Note: A 1% increase in the average teacher salary corresponds to $46 in 1957 dollars. Using the average pupil/teacher ratio, this corresponds to an additional expenditure of $2.11/student.

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