No. 9213

MEASURING THE VALUE OF SCHOOL QUALITY

by

Lori Taylor

September 1992

Research Paper

Federal Reserve Bank of Dallas

Measuring the Value of School Quality*

Lori L. Taylor
Federal Reserve Bank of Dallas

August 28, 1992

Preliminary Draft -- Please Do Not Quote

* The author is grateful to the Dallas Independent School District for allowing access to its data, to Stephen P.A. Brown, Thomas B. Fomby, James K. Hightower, Dennis H. Sullivan and Kelly A. Whealan for their comments and suggestions, and to Linda Rueffer for her research assistance.

Most real estate agents will tell you that houses sell for higher prices in areas that have good schools. Economists appear to have confirmed this common wisdom in their analyses of property values (see, for example, Jud and Watts, 1981, or Walden, 1990). However, economists studying property values (and possibly many home buyers) have measured school quality as a function of the achievements of a school's graduates rather than as a function of the value added to those graduates by the school. This definition is at odds with the literature on school quality measurement, which has generally concluded that the preferred measure of school quality is the school's marginal effect on students (see, for example Hanushek and Taylor, 1990).

If specification error in the estimates of school quality capitalization is significant, then policy recommendations that are based on the previous estimates could be misleading. For example, if previous estimates overstate the extent to which school quality differences are capitalized into property values, then analysts trying to judge voter support for a school bond election could substantially over-estimate support among homeowners. In this paper, the author demonstrates that the specification error can be substantial and that previous estimates of school quality capitalization could easily reflect differences in student and parent characteristics rather than differences in school effects.

The Model

To answer questions about the degree to which misspecification has marred estimates of the capitalized value of school quality, one must first construct measures of the marginal impact of schools. Following the methodology outlined in Hanushek and Taylor (1990), the author models student achievement in period T as a function of the student's complete history of school (S) and family (F) characteristics

$$A_{iT} = \alpha_T + \gamma_T S_{iT} + \beta_T S_{iT} + \sum_{t=1}^{T-1} \alpha_t + \sum_{t=1}^{T-1} \gamma_t S_{it} + \sum_{t=1}^{T-1} \beta_t F_{it} + \sum_{t=1}^{T} \epsilon_{it}, \tag{1}$$

where A_{iT} is the achievement of student *i* in period T, S_{it} represents characteristics of the school attended by student *i* in period t, and F_{it} represents family characteristics in period t.

Because equation 1 is recursive, one can extract the total marginal impact of the current school by estimating

$$A_{iT} = \alpha_T + \lambda A_{iT-1} + \beta_T F_{iT} + \sum_{k=1}^{n} q_k s_{ik} + \epsilon_{iT}, \tag{2}$$

where the s_{ik} are dummy variables that equal one if the *ith* student attends school k and equal zero otherwise. In this formulation, q_k represents the marginal effect of (or value added by) school *k*, and

$$\hat{A}_{i,T} = \alpha_T + \lambda A_{i,T-1} + \beta_T F_{i,T} \tag{3}$$

represents the level of student achievement that could be expected regardless of the school attended.

Introducing these measures of the value added by schools and the expected achievement of students into a hedonic model of property values decomposes the capitalization of student achievement into two parts. The first is the part of student achievement that can be attributed to schools and is subject to manipulation by them; the second is the part of student achievement that can be attributed to the characteristics of the student body and is not directly affected by changes in school policy. To the extent that these two components of student achievement are capitalized differently, analyses using the capitalized value of student achievement to proxy for the capitalized value of schools will be misleading.

The Data

Focusing on a single school taxation district avoids complications that might arise from differences in tax rates and tax bases among jurisdictions. With few exceptions, properties within the jurisdiction of the Dallas Independent School District (DISD) are also in the city and county of Dallas. Because these jurisdictions tax uniformly within their boundaries, the properties face the same city, county and school district tax rates. Therefore, differences in property values within the sample studied do not represent capitalized differences in tax rates.

DISD provided data on student body characteristics and student achievement scores for 87 primary schools in its jurisdiction for the years 1985, 1986, and 1987. The student body characteristics used in the analysis were the percentage of students who were NONWHITE and the best-available proxy for socio-economic status (the percentage of students receiving free or reduced-price lunches, P_LUNCH). The student achievement data used in the analysis were average scores for fourth-grade students on the Iowa Test of Basic Skills (ITBS) in mathematics and reading in 1986 and 1987 and the previous year's average scores for the same cohort (third-grade scores in 1985 and 1986, respectively). The variables POSTTEST and PRETEST represent the average combined mathematics and reading scores in the fourth and third grades, respectively.

Data on 310 Dallas single-family homes that sold in July 1987 came from the SREA Market Data Center's annual publication of residential property transactions. The housing data used in this analysis include the sale price of the property in thousands (SALEPR), the number of bathrooms (NUMBATHS), the year in which the home was built (YRBUILT), the number of square feet in the structure (SQFEET), and dummy variables that take on the value of one if the house has a fireplace or a swimming pool (FIREPLACE and POOL, respectively). From the SREA data on addresses, the author also constructed variables on distance to the central business district (DISTANCE) and a dummy variable for whether or not the property is located south of downtown Dallas (SOUTH_DAL). Table 1 reports summary statistics for the variables used in this analysis.

The Estimation

To provide a frame of reference, the author estimates the relationship between housing characteristics, average student test scores in 1987 and the value of properties sold in July of that year using linear, log-linear, and log-log specifications (see Table 2). Not surprisingly, the estimations indicate that property values in Dallas are an increasing function of the size of a home and the number of bathrooms and a decreasing function of the distance from the central business district. Houses with swimming pools are roughly 20 percent more expensive than houses without swimming pools, and homes in southern Dallas are *ceteris paribus* substantially less expensive than homes in the northern parts of the city. The estimation also indicates that student achievement differences are significantly capitalized into property values. Evaluated at the mean, a 1-percent increase in student achievement in the fourth grade increases property values by between 1.0 and 1.4 percent, depending on the functional form.

However, it is not clear if the relationship between student achievement and property values found in the benchmark regressions represents capitalized school quality. Answering this question requires estimates of value added and average expected achievement for each primary school in DISD. However, privacy concerns make student-specific data unavailable and force equation 2 to be estimated in residual form,

$$POSTEST_k = \alpha + \lambda PRETEST_k + \beta \overline{F_k} + \mu_k, \tag{4}$$

where $POSTTEST_k$ is the average, combined test score for fourth graders in school *k*, $PRETEST_k$ is the average, combined test score for the same cohort in the third grade, F is a vector of student body characteristics (NONWHITE and P_LUNCH), and

$$\mu_k = q_k S_k + \epsilon_k. \tag{5}$$

Unfortunately, estimating school effects as equation residuals introduces serious problems for the second stage of the analysis. Because the value-added residuals measure school effects with substantial error, hypothesis tests based on the estimated covariance matrix of the hedonic equation would be biased (Murphy and Topel, 1985). The author deals with these problems by using additional information in the data set to enhance the estimation of the stage one equations, and by applying the error correction techniques suggested by Murphy and Topel to the second stage hypothesis testing.

Fortunately, the data set contains sufficient additional information to estimate equation (4) for 1986 as well as 1987. Because the residuals are a function of school effects, and one would expect school effects to be highly correlated over time, the two years of data permit one to estimate a system of two equations,

$$\begin{aligned} POSTTEST_{k,87} &= \alpha_{87} + \lambda_j PRETEST_{k,86} + \beta\overline{F_{k,87}} + \mu_{k,87} \\ POSTTEST_{k,86} &= \alpha_{86} + \lambda_j PRETEST_{k,85} + \beta\overline{F_{k,86}} + \mu_{k,86}, \end{aligned} \qquad (6)$$

using seemingly unrelated regression (SUR) techniques.[1] Because the system of two equations incorporates more information than would an estimation of the first equation alone, this approach should reduce the portion of the μ_ks that represents measurement error. Table 3 reports the results of this first-stage estimation for both a linear and a logarithmic specification.

In the second stage of the estimation, the author substitutes the predicted values and residuals from the first-stage equations for 1987 for the observed student achievement in the benchmark hedonic equations and uses the techniques suggested by Murphy and Topel to correct the standard errors for hypothesis testing. The author uses the first-stage estimates from the linear specification for the linear and log-linear specifications of the hedonic model, and the first-stage estimates from the logarithmic specification for the log-log specification of the hedonic model. Using a logarithmic specification in the first stage to derive logarithmic estimates of value added and predicted achievement rather than transforming the estimates from

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[1] For simplicity, the author restrict the coefficients on λ and the β vector to be the same across each pair of equations. F-tests of the legitimacy of this restriction do not reject the hypothesis that these coefficients are the same for 1986 and 1987. F-tests do reject the hypothesis that the intercept terms are also equal.

the linear first-stage specification greatly simplifies extraction of the appropriate variance-covariance matrix for the Murphy-Topel correction and does not appear to influence the results. The Pearson correlations between the values for VALUE ADDED and PREDICTED ACHIEVEMENT from the logarithmic specification and log transformations for the same variables from the linear specification are .9823 and .9919, respectively.

The Murphy-Topel error correction involves using the variance-covariance matrix of the first-stage estimation to inflate the standard errors that are used in hypothesis testing in the second stage. Parameter estimates are unaffected by the correction. Specifically, one tests hypotheses using the variance-covariance matrix

$$\hat{\Sigma}_{corrected} = \hat{\Sigma}_{uncorrected} + (Z'Z)^{-1} Z' F^{*} \hat{V}(\hat{\Theta}) F^{*\prime} Z(Z'Z)^{-1}, \quad (7)$$

where Z is the matrix of second-stage regressors, F^* is a matrix of first-stage regressors that is weighted by the square of the difference between the coefficients on the generated regressors (VALUE-ADDED and PREDICTED ACHIEVEMENT) from the second stage, and $\hat{V}(\hat{\theta})$ is the variance-covariance matrix from the first-stage regression. In these examples, the error correction is small and has no impact on the implications of the hypothesis tests.

The estimations reported in Table 4 clearly indicate that the value added by schools and the predicted achievements of students can be capitalized differently and therefore that specification is important. In this example, which is robust to a number of common functional forms, property values are a function of the expected achievement of students and *not* of the marginal effects of schools.

Conclusions

Previous studies of the capitalized value of school quality have been misspecified. Estimates using information on fourth-graders in the Dallas Independent School District suggest that the misspecification is important. In particular, interpreting the relationship between student achievement and property values as evidence that school quality differences are capitalized may be very wrong. Although differences in student achievement in the fourth grade appear to have been capitalized into property values, the estimation indicates that the value added by Dallas schools in the fourth grade is *not* reflected in local property values.

Evidence that estimates of capitalized school quality may be wrong can have serious implications for educational policy. For example, instituting a policy of school choice (which would imply that residence in the neighborhood is no longer a requirement for attending a particular school) would reduce property values by the amount of the capitalized school quality unless transportation costs were substantial. Therefore, the degree of opposition to such a reform would depend on the degree of school quality capitalization. Using misspecified estimates of school quality capitalization could cause analysts to err substantially when estimating voter support for school choice or various other reform proposals.

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TABLE 1
Summary Statistics

Variable	Mean	Std. Deviation
SALEPR	156.12	148.62
YRBUILT	57.70	16.73
SQFEET	1997.87	1013.79
NUMBATHS	2.08	0.94
FIREPLACE	0.65	0.48
POOL	0.16	0.37
DISTANCE	2.46	0.83
SOUTH_DAL	0.25	0.43
$POSTTEST_{87}$	46.91	4.26
$PRETEST_{86}$	40.90	4.27
$NONWHITE_{87}$	77.33	21.51
P_LUNCH_{87}	59.18	21.06
$POSTTEST_{86}$	49.22	3.81
$PRETEST_{85}$	41.45	3.90
$NONWHITE_{86}$	76.84	21.45
P_LUNCH_{86}	57.97	21.75

TABLE 2
Benchmark Regressions

	Linear	Log-Linear	Log-Log
INTERCEPT	-172.28*	2.90*	-6.25*
	(58.18)	(0.26)	(1.20)
YRBUILT	-1.06*	0.001	-0.03
	(0.41)	(0.002)	(0.09)
SQFEET	0.07*	0.0003*	0.98*
	(0.01)	(0.00004)	(0.09)
NUMBATHS	49.16*	0.22*	0.27*
	(10.05)	(0.05)	(0.10)
POOL	30.24*	0.17*	0.20*
	(15.14)	(0.07)	(0.07)
FIREPLACE	14.83	0.14*	0.02
	(12.37)	(0.06)	(0.06)
DISTANCE	-26.94*	-0.20*	-0.37*
	(7.59)	(0.03)	(0.07)
SOUTH_DAL	-14.24	-0.30*	-0.25*
	(12.76)	(0.06)	(0.06)
POSTTEST	4.26*	0.02*	1.00*
	(1.30)	(0.01)	(0.29)
R-SQUARED	.6665	.7504	.7577

* Significantly different from zero at the 5-percent level.

Standard errors are in parentheses.

TABLE 3
First-Stage Regressions

	Linear	Logarithmic
INTERCEPT 1987	35.95	2.93
	(3.10)	(0.24)
INTERCEPT 1986	37.93	2.97
	(3.12)	(0.24)
NONWHITE	-0.04	-0.05
	(0.01)	(0.02)
P_LUNCH	-0.06	-0.05
	(0.01)	(0.01)
PRETEST	0.42	0.38
	(0.06)	(0.05)
SYSTEM R-SQUARED	.5580	.5491
OBSERVATIONS	87	87

All regressors are significantly different from zero at the 5-percent level.

Standard errors are in parentheses.

TABLE 4
Second-Stage Regressions

	Linear	Log-Linear	Log-Log
INTERCEPT	-387.91* ((89.43)) (80.12)	2.49* ((0.38)) (0.37)	-7.57* ((1.49)) (1.47)
YRBUILT	-0.96* ((0.40)) (0.40)	0.001 ((0.002)) (0.002)	-0.02 ((0.09)) (0.09)
SQFEET	0.07* ((0.01)) (0.01)	0.0003* ((0.0004)) (0.00004)	0.97* ((0.09)) (0.09)
NUMBATHS	41.92* ((10.01)) (10.01)	0.20* ((0.05)) (0.05)	0.24* ((0.10)) (0.10)
POOL	39.07* ((15.00)) (14.99)	0.19* ((0.07)) (0.07)	0.21* ((0.07)) (0.07)
FIREPLACE	-8.85 ((12.22)) (12.20)	0.15* ((0.06)) (0.06)	0.04 ((0.06)) (0.06)
DISTANCE	-32.57* ((7.64)) (7.57)	-0.21* ((0.04)) (0.04)	-0.39* ((0.07)) (0.07)
SOUTH_DAL	-7.35 ((12.74)) (12.61)	-0.29* ((0.06)) (0.06)	-0.23* ((0.06)) (0.06)
VALUE ADDED	-2.79 ((2.24)) (2.24)	0.01 ((0.01)) (0.01)	0.40 ((0.49)) (0.49)
PREDICTED ACHIEVEMENT	9.10* ((1.99)) (1.79)	0.03* ((0.01)) (0.01)	1.35* ((0.38)) (0.37)
R-SQUARE	.6820	.7524	.7596

* Significantly different from zero at the 5-percent level.

Corrected standard errors are in double parentheses.
Original standard errors are in parentheses.

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