# A Three-way Error Components Analysis of Educational Productivity

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ABSTRACT *Previous research on educational productivity has decomposed the variance in student test scores into school and class effects. In this paper, we extend this work to include differences attributable to teachers as well as to schools and classes. Using data drawn from the National Educational Longitudinal Study of 1988, we find that unobservable school, teacher, and classroom characteristics are important factors in explaining 10th-grade mathematics achievement, and account for the majority of the variation that is explained by educational variables.*

## Introduction

Over the past 25 years, research on how educational resources affect student outcomes has failed to reveal strong relationships.[1] In general, these studies show that individual and family background traits explain the vast majority of the variance in student test scores, and observable school characteristics, such as per pupil spending, teacher experience, or teacher degree level, have at best a weak relationship with student outcomes. These findings suggest that while there is little relationship between observable educational resources and student outcomes, it is possible that unobservable aspects of schooling, like teacher motivation and school climate, may be quite important.

Montmarquette and Majseredjian (1989), who estimated a two-way nested-error components model of educational achievement (in grades 1 and 4), find that unobservable school- and class-level effects explain a negligible portion of the variance in achievement in mathematics and French. Class-level unobservables explain less than 1% of the variation in grades 1 and 4, and unobservable school-level effects explain about 1% in grade 1, and 6% in grade 4. Likewise, they find observable school and class characteristics to be relatively unimportant, with observable school and class characteristics contributing between 2.5 and 7% of the variation in achievement. In contrast, Goldhaber and Brewer (1997) find observable and unobservable school and teacher effects to be important, explaining about 12% of 10th-grade mathematics achievement. Both studies show that the

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overwhelming majority of the variance in student test scores is explained by individual and family background characteristics, while relatively little is explained by observable educational variables. In this paper, we extend this research by estimating a three-way error components model that allows us to apportion the variance in 10th-grade mathematics achievement between school-level variables, teacher-level variables, and class-level variables. The methodology employed here also allows us to determine the relative importance of both observable and unobservable school, teacher and class effects.

## Data and Econometric Methodology

Our data are from the first two waves of the National Educational Longitudinal Study of 1988 (NELS), a nationally representative survey of about 24 000 8th-grade students, about 18 000 of whom were resurveyed in the 10th-grade. Students provided comprehensive information on themselves and their families (their race/ethnicity, gender, family structure), supplemented by a parental survey in 1988 (providing information on, for example, family income). At the time of each survey, a sub-sample of students took one or more subject-based tests in mathematics, science, writing and history. The data therefore permit the estimation of 'value-added' or gain score production functions which control for previous knowledge or ability. We confine our attention to students who took the mathematics test in the 8th and 10th grade, the 10th-grade score being modeled as a function of the 8th-grade score and other variables.

The unique feature of NELS is that it provides detailed teacher- and class-level information that is tied directly to individual students. In other words, the characteristics of each 10th-grade mathematics teacher (sex, race/ethnicity, degree level, experience, certification, etc.) who taught students taking the 10th-grade mathematics test are known. In addition, some characteristics of the classes which that teacher taught are also available (for example, class size and composition). School administrators provided information on their schools such as the percentage of the teaching staff with a Master's degree. We confine our attention to a sample of 1570 10th-grade public-school students.[2] These students are drawn from 490 schools, with 1089 individual mathematics teachers and 1340 classes. The presence of multiple teachers per school, and multiple classes per teacher, is a unique advantage of the NELS data which permits the estimation of a broader class of models and allows inference regarding the relative importance of observable and unobservable effects. Complete variable definitions and sample statistics for the four levels of variables we use in this study (individual and family background, school, teacher, and class) are reported in Table 1.

### *Assessing the Effect of Unobservable Schooling Variables on Student Outcomes*

We assume the achievement of student $i$ at school $j$, $Y_{ij}$, is a function of a vector of individual and family background variables (including some measure of prior ability or achievement), $\mathbf{X}_i$, a vector of schooling resources, $\mathbf{S}_j$, which do not vary across students, and a random error term:

$$Y_{ij} = \beta\mathbf{X}_i + \gamma\mathbf{S}_j + \varepsilon_{ij} \tag{1}$$

$\mathbf{S}_j$ may consist of school, teacher or class-specific variables, $\beta$ is the return to individual and family background characteristics, and $\gamma$ is the return to schooling

**Table 1.** Sample statistics

| | Mean | Standard deviation |
|---|---|---|
| *Individual and family background variables* | | |
| 10th-Grade mathematics test score | 43.79 | 13.64 |
| 8th-Grade mathematics test score | 36.45 | 11.67 |
| Female | 0.50 | 0.50 |
| Black | 0.10 | 0.29 |
| Hispanic | 0.10 | 0.30 |
| Asian | 0.05 | 0.22 |
| Family income | 35374.84 | 32613.08 |
| Parental education | 14.13 | 2.35 |
| Student lives with mother only | 0.13 | 0.34 |
| Student lives with father only | 0.02 | 0.12 |
| *School-level variables* | | |
| Urban | 0.19 | 0.39 |
| Rural | 0.41 | 0.49 |
| Northeast | 0.14 | 0.34 |
| North-central | 0.34 | 0.47 |
| West | 0.17 | 0.37 |
| School size | 1200.99 | 699.52 |
| Percentage of students in school who are white | 74.44 | 31.18 |
| Percentage of teachers in school who have an MA or more | 38.85 | 28.20 |
| Percentage of students in school from single-parent families | 24.99 | 17.83 |
| *Teacher-level variables* | | |
| Teacher is female | 0.46 | 0.50 |
| Teacher is black | 0.04 | 0.19 |
| Teacher is Hispanic | 0.02 | 0.13 |
| Teacher is Asian | 0.02 | 0.12 |
| Teacher's years of experience at secondary level | 15.22 | 8.93 |
| Teacher is certified | 0.96 | 0.19 |
| Teacher has an MA or higher degree | 0.49 | 0.50 |
| *Class-level variables* | | |
| Class size | 22.93 | 7.56 |
| Percent minority in class | 20.38 | 28.43 |
| Sample size | 1570 | |

resources. If equation (1) is correctly specified, ordinary leasr-squares (OLS) estimation will yield consistent estimates of $\beta$ and $\gamma$; however, it is quite likely that the available data will necessitate the omission of important variables. This will lead to an understimate of the total effect of schooling resources on student outcomes and, if the omitted factors are correlated with the included variables, biased estimates of the effects of particular schooling resources on student outcomes.

These unobservable effects may represent variables such as the ability of the students' peers or the effectiveness of the teacher. However, an alternate possibility is that students or teachers are not randomly distributed within or across schools. For instance, Akerhielm (1995) shows that students are not randomly distributed

across classes within schools. This can lead to biased estimates of the effects of class-size on student achievement. Additionally, students from families who have unobservable preferences for education may tend to live in the same school districts. In this case, a school effect may simply capture unobservable family background characteristics that are not adequately controlled for by the included family background variables.

One might assume that the omitted effects are specific to the school, teacher or class. In this case, the model is:

$$Y_{ij} = \beta \mathbf{X}_i + \gamma_1 \mathbf{S}_j + \mathbf{Z}_j + \varepsilon_{ij} \qquad (2)$$

This is a standard random effects model in which there are $N$ groups (say schools) with $T$ student observations per school ($NT$ total observations) and which can be estimated by generalized least-squares to yield consistent, efficient and unbiased estimates of the effect of observed schooling resources on student achievement. However, it is predicated on the orthogonality of the random effect to the included regressors, and requires a distributional assumption about $\mathbf{Z}_j$.

If panel data are available, the standard technique to account for omitted variables bias is to estimate a fixed effects model. In this case, let $\alpha_j$ be a school-specific effect which does not vary across individuals but is specific to each school. In a sample of $j = 1 \ldots N$ schools with $T$ observations per school, one can then estimate:

$$Y_{ij} = \beta \mathbf{X}_{ij} + \alpha_j + \varepsilon_{ij} \qquad (3)$$

using OLS to obtain unbiased estimates of the effect of individual and family background variables ($X$), and the total effect of schooling resources on student achievement. This model does not require the restrictive assumption that the unobservable schooling variables are uncorrelated with included variables since the effect of all school-specific unobservables is captured by $\alpha_j$. The estimates of $\beta$ are also consistent. However, while the estimates of $\alpha_j$, the schooling effects, are unbiased, their consistency relies on the number of observations being large (infinite). They also may not be fully efficient. Determining whether a random effects model specification (equation (2)) or a fixed effects model (equation (3)) is the 'correct' model depends partly on the relationship between the omitted effects and included regressors. If observable schooling variables are correlated with unobservable schooling effects, then equation (2) is not appropriate.

### *Unobservable Schooling Variables: School, Teacher or Class Characteristics?*

In the discussion so far, we have simply called the unobservables of interest 'schooling' variables, captured in vectors $\mathbf{Z}_j$ in equation (2) and $\alpha_j$ in equation (3). In reality, however, unobservables may be specific to either schools, teachers or classes. Thus, $\mathbf{Z}_j$ and $\alpha_j$ may be decomposed into a portion that is specific to the school, $\zeta$, a portion specific to the teacher, $\tau$, and a portion specific to the class, $\gamma$:

$$Z = \zeta + \tau + \gamma + \varepsilon \qquad (4)$$

We can account for this by including in our models separate school, teacher and class effects, and as earlier, we can view these effects to be either fixed or random.

However, the data requirements for such models are extensive. To include individual level variables in a model with class fixed effects, we must have at least two students per class or the class dummy variable will be perfectly collinear with the individual characteristics. We also cannot include teacher-level observed variables while including class fixed effects, nor can we not include school-level observed variables while including either teacher or class fixed effects.

It is also the case that one effect is nested within another. A teacher effect contains a school effect given that there are multiple teachers per school but not multiple schools per teacher. Likewise, teacher and school effects are nested within class effects. We can only distinguish one effect from the others if there are multiple teachers per school and if teachers instruct multiple classes. Thus, to estimate three-way fixed effects models and obtain distinct class, teacher and school effects, the data must have a pyramid-type structure in which there are at least two students per class, two classes per teacher and two teachers per school.[3] For instance, to estimate three-way effects for a particular school requires that there are at least two teachers from that school who each teach at least two classes in which there are at least two students. Thus, the data requirement for each school is eight students, four classes and two teachers.

## Results

### *Standard OLS Educational Production Functions*

Throughout our analyses, the dependent variable is the 10th-grade mathematics test score. We group our explanatory variables into four sets that parallel Montmarquette and Majseredjian (1989) as closely as possible. These include: individual and family background variables (including sex, race/ethnicity, parental education, family structure, family income, and 8th-grade mathematics test score), school variables (including urbanicity and regional dummies, school size, the percentage of students at the school who are white, the percentage of students at the school who are from single parent families, and the percentage of teachers at the school with at least a Master's degree), teacher characteristics (including gender, race/ethnicity, years of experience at the secondary level, whether the teacher is certified, and the teacher's degree level) and classroom variables (class size and percentage of minority students).

We begin by estimating a standard educational production function, like equation (1), using OLS. The estimated coefficients from this model are reported in the first column of Table 2. The returns to individual and family background variables are generally in line with previous research. There is a statistically significant positive relationship between the achievement measure, 10th-grade mathematics test score, and the 8th-grade mathematics test score and parental education. Only two of the 16 school- and teacher-level variables are statistically significant: the percentage of students in the school who are white, and teacher's gender. Both class size and percent minority in the students mathematics class are statistically significant, but class size is significant with the 'wrong' sign. These rather ambiguous results are consistent with much of the educational production function literature (Hanushek, 1986).

To determine whether the observable school-, teacher- and class-level variables are important factors in determining 10th-grade student mathematics achievement, we estimate the base model in stages. We first include only individual and family

**Table 2.** OLS and maximum likelihood models of 10th-grade mathematics achievement (standard deviation in parentheses)[a]

| | OLS (1) | School random effects (2) | School, teacher random effects (3) | School, teacher, class random effects (4) |
|---|---|---|---|---|
| *Individual and family background variables* | | | | |
| Base-year mathematics score | 0.952* | 0.953* | 0.951* | 0.948* |
| | (0.016) | (0.016) | (0.016) | (0.016) |
| Female | –0.160 | –0.191 | –0.179 | –0.172 |
| | (0.336) | (0.329) | (0.329) | (0.329) |
| Black | –0.902 | –1.022 | –1.055 | –1.089 |
| | (0.714) | (0.709) | (0.710) | (0.707) |
| Hispanic | 0.265 | 0.291 | 0.323 | 0.294 |
| | (0.677) | (0.670) | (0.669) | (0.667) |
| Asian | 1.197 | 1.179 | 1.210 | 1.167 |
| | (0.804) | (0.791) | (0.791) | (0.790) |
| Family income[b] | –0.007 | –0.008 | –0.008 | –0.008 |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| Parental education | 0.509* | 0.484* | 0.486* | 0.493* |
| | (0.083) | (0.082) | (0.082) | (0.082) |
| Student lives with mother only | –0.315 | –0.318 | –0.304 | –0.212 |
| | (0.534) | (0.524) | (0.524) | (0.522) |
| Student lives with father only | –0.402 | –0.420 | –0.440 | –0.513 |
| | (1.379) | (1.354) | (1.354) | (1.353) |
| *School-level variables* | | | | |
| Urban | 0.127 | 0.129 | 0.141 | 0.169 |
| | (0.530) | (0.565) | (0.562) | (0.555) |
| Rural | –0.513 | –0.476 | –0.485 | –0.414 |
| | (0.431) | (0.467) | (0.466) | (0.460) |
| Northeast | 0.580 | 0.498 | 0.527 | 0.552 |
| | (0.580) | (0.620) | (0.619) | (0.613) |
| North-central | 0.048 | –0.032 | –0.007 | 0.016 |
| | (0.454) | (0.491) | (0.490) | (0.483) |
| West | 0.097 | 0.009 | 0.019 | –0.001 |
| | (0.567) | (0.603) | (0.600) | (0.593) |
| School size[b] | –0.308 | –0.279 | –0.288 | –0.315 |
| | (0.343) | (0.367) | (0.365) | (0.361) |
| % of students in school who are white | –0.036* | –0.038* | –0.038* | –0.037* |
| | (0.010) | (0.011) | (0.011) | (0.011) |
| % of teachers in school who have an MA or more | –0.001 | –0.003 | –0.002 | –0.001 |
| | (0.010) | (0.010) | (0.010) | (0.010) |
| % of students in school from single-parent families | –0.003 | –0.004 | –0.003 | –0.002 |
| | (0.012) | (0.013) | (0.013) | (0.013) |
| *Teacher-level variables* | | | | |
| Teacher is female | 0.708* | 0.683 | 0.679 | 0.661 |
| | (0.359) | (0.363) | (0.368) | (0.365) |
| Teacher is black | –1.367 | –1.326 | –1.295 | –1.317 |
| | (0.999) | (0.985) | (0.992) | (0.989) |
| Teacher is Hispanic | 1.294 | 1.264 | 1.373 | 1.327 |
| | (1.288) | (1.275) | (1.285) | (1.283) |

**Table 2.** continued

| | OLS (1) | School random effects (2) | School, teacher random effects (3) | School, teacher, class random effects (4) |
|---|---|---|---|---|
| Teacher is Asian | 1.524 | 1.676 | 1.665 | 1.495 |
| | (1.397) | (1.408) | (1.431) | (1.410) |
| Teacher's years of experience at secondary level | −0.009 | −0.006 | −0.007 | −0.008 |
| | (0.021) | (0.022) | (0.022) | (0.022) |
| Teacher is certified | −0.377 | −0.425 | −0.392 | −0.331 |
| | (0.975) | (0.984) | (1.000) | (0.998) |
| Teacher has an MA or higher degree | −0.196 | −0.290 | −0.231 | −0.160 |
| | (0.380) | (0.385) | (0.390) | (0.387) |
| *Class-level variables* | | | | |
| Class size | 0.066* | 0.074* | 0.072* | 0.076* |
| | (0.026) | (0.026) | (0.027) | (0.027) |
| Percent minority in class | −0.049* | −0.050* | −0.050* | −0.050* |
| | (0.011) | (0.011) | (0.011) | (0.011) |
| Sample size | 1570 | 1570 | 1570 | 1570 |
| $R^2$/−2*(log likelihood) | 0.773 | 10333 | 10326 | 10325 |

[a] All models include dichotomous variables for missing values of: family income, family structure, percentage of students in school who are white, percentage of teachers in school who have at least an MA, percentage of students in school from single-parent families, teacher experience, teacher certification, and class size. Column 1 is estimated using OLS, and columns 2–4 are estimated using maximum likelihood.

[b] Coefficient and standard error are multiplied by 1000.

* Variable is significant at the 0.05 level.

background variables, then add school-level variables, then school and teacher variables, and finally, school-, teacher- and class-level variables. This allows us to calculate the incremental contribution of each set of variables to explaining achievement (which we discuss later). At each stage, we test the null hypothesis that the coefficients of the variables added to the model at that stage are jointly equal to zero. We could not reject $F$-tests of the hypothesis, at the 5% significance level, that the coefficients on the school variables were jointly equal to zero nor could we reject this hypothesis for the teacher variables. However, we were able to reject this hypothesis for the class variables at the 1% significance level, indicating that the class characteristics are important predictors of student success.

As discussed in the previous section, the impact of educational resources on student outcomes may act predominately through unobservable effects rather than through observable characteristics that are quantifiable in survey data. To address this possibility, we next use maximum likelihood methods to estimate the random effects models that correspond to equation (2). The estimated coefficients from the school-level random effects model are reported in column 2 of Table 2; the coefficients from the model that includes (two-way) school and teacher effects are reported in column 3; and the estimated coefficients when (three-way) school-teacher- and class-level effects are simultaneously included are reported in column 4.[4]

The estimated coefficients from the random effects specifications of the models (columns 2–4) are very similar to those of the OLS specification reported in column 1 of Table 2. In fact, there is only one case, that of teacher gender in the OLS model, in which a variable is statistically significant (at the 5% level) in one specification of the model and statistically insignificant (at the 5% level) in an alternate specification. There is also very little change in the magnitudes of the estimated coefficients in the four models; thus, estimated returns to the schooling characteristics are relatively insensitive to whether the model is estimated with or without random effects, and insensitive to the specified level of the random effect.

In the case of all three single random effects (school, teacher or class), and models with two-and three-way random effects, likelihood ratio tests suggest that the random effects specifications of the models are preferred to the standard OLS specification. We perform Wald $Z$-tests to determine the appropriate level or levels of the effect. When all three effects are included in the model, we find that the school random effects are significant at the 10% level and the class random effects are significant at the 1% level; however, the teacher random effects are not statistically significant, suggesting that the data show no evidence of correlation (between the error terms) across students within a specific teacher, once school and class random effects are taken into account.

To focus on the relative importance of school, teacher and class unobservables, we estimate our fixed effects educational production function model (equation (3)) in stages, first adding a school effect, then a teacher effect and, finally, a class effect. In these fixed effects models, we cannot include observed variables at the same level as the fixed effect (e.g. teacher observed variables with a teacher-specific effect), nor can we include higher level observed variables than the level of the fixed effect (e.g. school-level observed variables with a class-level effect) due to the nested nature of the effects. An $F$-test of the null hypothesis that the fixed effects are jointly equal to zero is rejected at the 1% significance level for the school-level, teacher-level and class-level variables, respectively.

Following Montmarquette and Majseredjian (1989), we calculate the incremental contribution of each level of observable and unobservable schooling variables to the explanation of variance in student achievement. The incremental contribution of each set of *observed* variables is calculated by taking the difference between the multiple correlation coefficient ($R^2$) with the full set of variables included in the model, and the multiple correlation with each subset of variables (individual and family background, school level, teacher level and class level) excluded from the model. The incremental contribution of each set of unobservable variables is calculated in a similar way. For instance, the contribution of class unobservables is calculated by taking the difference in the $R^2$ in the model that includes class fixed effects and the model that includes teacher fixed effects and observable class variables. The incremental contributions of both observable and unobservable schooling variables are listed in Table 3.

The vast majority of variance is explained by individual and family background characteristics (about 60%). Overall, school, teacher and class variables, both observable and unobserved, account for approximately 21% of the variation in student achievement. Of this 21%, only about 1 percentage point (or 4.8%) is explained by observable educational variables, and the remaining 20 percentage points (or 95.2%) is made up of unobservable school, teacher and class effects. These results contrast with those of Montmarquette and Majseredjian (1989), who find observable class and school characteristics to explain a higher proportion of

**Table 3.** Percentage of variance in student achievement explained by subgroups of variables

| | Observable | Unobservable |
|---|---|---|
| School-level variables | 0.0025 | 0.0837 |
| Teacher-level variables | 0.0020 | 0.0832 |
| Class-level variables | 0.0040 | 0.0372 |
| Total | 0.0085 | 0.2041 |

achievement than unobservable effects. There are several possible explanations for the divergence in findings. First, Montmarquette and Majseredjian focus on 1st- and 4th-grade students, while we focus on students in the 10th grade. Second, there are slight differences in the control variables between the two studies; for instance we use the 8th-grade mathematics score as our prior year test score, while they use a general IQ test. Finally, we use the fixed effects specification of our model in computing the contribution of unobserved effects, while they use the random effects specification of their model.[5]

## Conclusion

In this paper, we extend the work of Montmarquette and Majseredjian (1989) by estimating a three-way nested-error model that allows us to determine how much of the achievement on a 10th-grade standardized test can be explained by observable schooling resources and unobservable school, teacher and class effects. Consistent with previous literature, we find few observable school-, teacher- and class-level variables to be statistically significant determinants of student test scores. In contrast, however, the unobservable school-, teacher- and class-level effects appear to be important in explaining student achievement. Indeed, we find that the majority of the variation in student test scores which is explained by schooling variables is explained by unobservable school, teacher and class effects. Taken together, these unobservable schooling effects explain 20 percentage points, or about 25%, of the total explained variation in 10th-grade mathematics achievement.

## Notes

1. For a review of this literature, see Hanushek (1986).
2. We limit our sample to public-school students to avoid problems associated with selection into private schools (Goldhaber, 1996), and restrict our analysis to approximately 30% of the total sample of public schools who had taken the mathematics test in both the 8th and 10th grades. We randomly select students to restrict the sample due to the computational requirements of estimating three-way random effects. We use the *t*-test to determine if differences in mean test score, and demographic variables such as family income and parental education, between the full and restricted sample, were statistically significant. The characteristics of the restricted sample were not found to be significantly different from the larger sample.
3. If these data requirements are not met, three-way fixed effects models cannot be estimated due to perfect collinearity. In effect, in cases where there is only one teacher per school or one class per teacher, we cannot distinguish between a school and teacher effect, and a teacher and class effect, respectively. There are similar data requirements to estimate two-way fixed effects models.

4. We also estimated specifications of the model that include teacher-level effects, school-level effects, school and class effects, and teacher and class effects. These models yield results similar to the reported specifications and are available from the authors upon request.
5. We could not compute the contribution from the random effects specifications in the same way that Montmarquette and Majseredjian (1989) do because we use maximum likelihood estimation techniques.

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