A panel analysis of student mathematics achievement in the US in the 1990s: does increasing the amount of time in learning activities affect math achievement?

Tevfik Aksoy a, Charles R. Link b,*

a Investment Banking, Bank Ekspres, Istanbul, Turkey
b Department of Economics, University of Delaware, Newark, DE 19716, USA

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Abstract

Panel estimation techniques are utilized to estimate econometric models of the determinants of mathematics achievement for a nationally representative sample of US high school students from the National Education Longitudinal Studies program (NELS88).

Among the results, several relate to variables discussed as potentially important policy variables. Extra time spent on mathematics homework increases student test scores while extra hours per day of watching television negatively impacts math test scores. The results of the estimations also indicate the positive and significant effect of an increase in the number of minutes of each class period for mathematics. Given the means and standard deviations of these variables, 3 hours per week for math homework, 2.6 hours per day of watching TV, 52 minutes per math class period, as well as the relatively large effects associated with these variables, the potential for manipulating them to enhance math achievement seems like a real possibility. © 2000 Elsevier Science Ltd. All rights reserved.

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1. Introduction

The rapid technological changes and intense global competition in output and labor markets require a country to have an educated and adaptable workforce. Moreover, economic growth undoubtedly is related to the human capital stock of a country’s workforce. Mathematical skills learned in elementary and secondary schools are critical in such an environment. Zeith and Cool (1992) and Lewis and Seidman (1994) emphasize the importance of time spent on active learning as a determinant of achievement. The main objective in the paper is to estimate the effects on mathematics achievement of several different measures of time spent on active learning. Analyzing these issues with panel data from a nationally representative survey of high school students is a unique aspect of our study.

The importance of the education issue is easy to document. According to a recent Wall Street Journal/NBC News poll, Americans rate education as the highest priority for the federal government.¹ This comes after years of complaints about the US system of public education and poor performance on several tests involving math.

¹ This is higher even than Medicare and deficit reduction. See Graham (1997).
science, and reading which have been given to students from different nations. Out of 41 countries participating in the Third International Math and Science Study (TIMSS), 8th grade students from the US ranked 28th in the Third International Math and Science Study from different nations. Out of 41 countries participating in reading which have been given to students from TIMSS is the movement in governments to be more efficient when spending from public coffers. Put simply, governments are trying, given their tight budgets, to obtain more for each dollar spent on public activities. The current fiscal tightening in Europe as well as in Washington are but two examples of this phenomenon. Policy makers want least cost educational strategies which are effective in enhancing student learning.

As a result, educators have proposed that reforms be made in the US school system. Without the knowledge of what causes students to learn, changes are likely to be ineffective. Educational production functions, because they provide estimates of the determinants of student performance, provide insights into the factors which have been discussed by researchers as likely to improve performance on achievement tests. In order to provide this information and fill a gap in the economics of education literature, panel data are employed to estimate the determinants of achievement in mathematics. The data set used in our study, the National Education Longitudinal Study of 1988 (NELS), is a nationally representative data set, including features related to high school students, their schools, parents, and teachers. The variables we have chosen to emphasize are those related to the time students spend in their math classes along with the time they spend doing math homework.

The issue of ‘omitted variable bias’ arises frequently when cross-sectional or single time series data are employed. In order to specify and analyze economic hypotheses correctly, a researcher has to include variables that are thought to influence and explain the outcome. When one or more of these variables are omitted from the behavioral relationship, usually due to the lack of data or a specification error, the effects of these omitted variables appear through the error term and the likely outcome is biased coefficient estimates. In studies of the determinants of achievement, omitted variables in cross-sections include, among others, innate ability, and motivation. If the effects of the omitted variables are constant over time or if they are the same across all individuals at a given point in time, the bias can be eliminated using panel data estimation techniques. Panel analysis automatically lessens the degree of this bias by excluding variables that are specific to the individual but constant over time and including variables that change over time. Therefore, we can obtain unbiased estimates of the determinants of math achievement. We believe a significant gap in the economics of education literature exists concerning the use of panel techniques and our study is aimed at providing results to start filling this gap.

The related literature is surveyed in Section 2. Section 3 discusses the data, estimation procedures, variable definitions, and the actual samples chosen. Section 4 reports the results of the estimations. The summary and conclusions are presented in Section 5.

2. Literature review

As suggested by Hanushek (1986) and others, relevant determinants of achievement include the student’s innate characteristics along with the cumulative effects of her school, family, environment, and peers. In this section the literature is reviewed which is related to the estimated models discussed in Section 4. In the discussion variables are grouped into two major categories: factors relating to schools and factors relating to the student and his family. The schools category is further broken down into class size, teacher characteristics, time on task, and type of school attended. Characteristics related to the family and individual are the family income and marital status of the student’s parents, and the number of weekly hours the student works for pay.

2.1. School characteristics

Characteristics of schools are used in most educational production functions.

2.1.1. Class size

Teachers associations and educational policy makers have argued for smaller classes as a way of enhancing learning in elementary and secondary schools. However, Hanushek (1986) reported that previous studies generally found no relationship between class size and achievement, and when they did the coefficient was not always

2 See The Economist (1997, p. 21), for a list of the countries and their math and science scores.
3 See Kronholz, 1997.
5 Another excellent and extensive survey of the early literature prior to the early 1980s can be found in Glasman and Binaminov (1981). For more recent discussions see the Federal Reserve Bank of New York (1998).
in the expected direction. Findings in later studies also have been mixed. Cooper and Cohn (1997) found, for mathematics achievement in South Carolina schools, that class sizes had no consistent effect on achievement. Some coefficients were positive and some negative. On the other hand, Goldhaber and Brewer (1997) found a positive and statistically significant relationship between class size and math achievement—larger class sizes were associated with higher achievement! Two studies have found smaller class size is associated with higher achievement. Rouse (1998) in summarizing the current research on the effects on achievement of the Milwaukee Parental Choice Program, suggests that the results are consistent with the hypothesis that children perform better in small classes. Krueger (1997) using data from the Tennessee Student–Teacher Achievement Ratio (STAR) experiment found that students did better in smaller classes. In summary, the findings in the literature are mixed.

2.1.2. Teacher characteristics

Hanushek (1986) reports prior to 1986 there had been 106 studies including teacher education, 109 studies including teacher experience, and 60 studies including teacher salary as variables. The striking result from these early studies is the mixed nature of the effects. Ambiguous results on these variables are also found in more recent studies.6

Race and gender of the teacher and their association with achievement have been analyzed by several researchers because the argument has been made that more teachers from under-represented groups should be recruited to teach students from these under-represented groups. Because achievement and dropout rates of students in these groups tend to be low and high respectively, compared to whites, some believe that teachers of the same minority group would be better able to teach these students.

The teacher’s race and gender do not have consistent effects on achievement. Glasman and Biniaminov (1981) document the mixed coefficients on early studies. Recent studies that find lower achievement when a student’s teacher is black include Cooper and Cohn (1997), Cohn and Teel (1991) and Goldhaber and Brewer (1997). Ehrenberg, Goldhaber and Brewer (1995), found no support for the hypothesis that the teacher’s race had any significant impact on student achievement. Ehrenberg and Brewer (1994) found that an increase in the percent of black faculty is associated with higher test scores.

Cooper and Cohn (1997), Cohn and Teel (1991), Ehrenberg et al. (1995) and Hanushek (1992) find that the performance by female and male teachers is comparable when their effects on math achievement are considered. Although Goldhaber and Brewer (1997) found a positive relationship between the student having a female teacher and math achievement, the general conclusion from the literature seems to be that variables which reflect the race and/or gender of a teacher are not usually significant determinants of student achievement. The results for race and sex of the teacher are mixed. The general conclusion regarding the variables discussed to date in the review is that no consistent results jump out of the past literature.

2.1.3. Time on task

Zeith and Cool (1992) emphasize that the literature consistently supports the importance of time spent on active learning as a determinant of academic achievement. Lewis and Seidman (1994) estimate that the amount of time a typical student spends on math in-school and out-of-school by the 8th grade is 30% more in Japan than in the US. They conclude that a 21 day increase in the length of the school year in the US, along with assigned homework during the summer, would cause a major improvement in the performance of US students on achievement tests. The length of the school year also is important because of evidence showing that students in the US forget substantial amounts of what was learned during the year in the following summer and require about four weeks of review in the fall.7 Krueger (1998) also emphasizes that the average school year in the United States is shorter than in many other developed countries, and goes on to stress a well established fact that more years of schooling are associated with higher

6 Dolan and Schmidt (1987) obtained a negative association of teachers salaries and reading achievement for 11th graders but a positive association for 8th and 11th graders in math. Ehrenberg and Brewer (1994) found a positive and occasionally significant relationship between achievement rates and expenditures per pupil for both black and white students.

Hanushek (1986) reported that although many studies found a positive coefficient for teacher experience, it was usually statistically insignificant. Cooper and Cohn (1997) and Goldhaber and Brewer (1997) generally found a positive but statistically insignificant relationship between teacher experience and math achievement. Hanushek (1992) found a statistically significant and positive relationship for vocabulary and reading achievement. Monk (1994) found both positive and negative coefficients.

7 See New York State Department of Education (1978) Moreover, Entwisle, Alexander and Olson (1997) show that disadvantaged children lose ground during the summer while higher socioeconomic status (SES) students actually gain. Krueger (1998) interprets their results as suggesting that both high and low SES students have comparable gains during the school year so that schools are offsetting the negative effects of low SES. However, during the summer months these negative effects cause an increase in the gap between high and low SES students.
labor market earnings—something that should also apply to extending the length of the school year.

Gilby, Link and Mulligan (1993) utilized panel analysis on a sample of 8400 elementary students included in the Sustaining Effects Study. They had observations on each student for three consecutive years and found that extra hours of mathematics instruction per week are associated with small positive gains in mathematics achievement.

Bets (1998) believes that increased expenditures per pupil and the increase in the minimum school leaving age are two of the key reforms in public schooling in the US. He argues that these policies would have been more effective had they been accompanied by increased educational standards. As he puts it

The missing ‘leg’ in these past reforms is a set of academic standards against which both students and schools are measured. (Betts, 1998, pp. 97–98)

From the viewpoint of the present study, one important thing schools can do is heighten their expectations of students. This can be done through a host of actions including stricter grading, curriculum standards and assessment of whether the student is mastering the material, and additional homework. Policy makers must recognize that achievement is influenced by the student’s own effort.

A comprehensive review of more than 100 studies in the literature relating to the effects of homework on academic outcomes (of which achievement is only one) can be found in Cooper (1989). Included in his review were 16 studies involving experiments where some students were placed in a group doing homework and others in a group which did no homework. The overall evidence of these studies lends support to the hypothesis that homework enhances achievement. However, problems with many of these studies included the student assignment strategies (randomness of selection into groups) and small sample sizes (most involved one school and two or less classes). Another literature surveyed by Cooper was denoted correlational. That is, how does the amount of time spent on homework affect achievement as measured by some test score. Results for 17 of these studies are presented. Of these, nine utilized statistical techniques such as multiple regression analysis which controlled for other variables such as family background and previous achievement. The findings generally support a positive correlation between homework and achievement with at least one interesting difference. Homework appears to have larger effects for junior high and high school students compared to elementary school students.

Because of data limitations, most of the earlier studies of homework had inadequate controls for previous achievement. An exception is the study by Keith, Reimers, Fehrmann, Pottebaum and Aubey (1986) which had detailed controls for prior achievement. The upshot of not controlling for previous achievement is the increased probability of omitted variables bias.

The only homework study we uncovered which utilizes panel techniques is by Betts (1996). Analyzing the first 5 years of data from the Longitudinal Study of American Youth, he found a positive association between the amount of homework assigned and student achievement. Betts includes better controls for a student’s previous achievement. This control and the utilization of panel techniques increases the likelihood that the estimated coefficient on the homework variable will be unbiased. In conclusion, the evidence provides support for increased homework as a means of increasing test performance.

As was just noted, the literature on learning suggests that the more time spent studying math, in or out of class, the higher will be math achievement. Other factors the same, the more a student watches TV during the week, the less time there is for doing homework. Gortmaker, Salter, Walker and Dietz (1990) note that many studies have found that increased time watching television is associated with a decline in achievement.8 One of the main studies finding such a result is Keith et al. (1986) who used the first wave of the High School and Beyond Longitudinal Study to estimate the effects of TV time on achievement. In conclusion, prior literature related to time on task supports the argument that more time spent on mathematics, the higher should be the level of mathematical achievement.

2.1.4. Type of school attended

Utilizing the ‘High School and Beyond Study,’ Coleman and Hoffer (1987), Willms (1985) and Alexander and Pallas (1985) found positive effects associated with attendance at Catholic schools and math achievement. However, the results of a study by Filglio and Stone (1997) based on the national educational longitudinal study found that Catholic school attendance did not enhance achievement in the population as a whole.

Neal (1998, pp. 83–84) describes two points of caution when interpreting the previous literature on Catholic school attendance (as well as the results in the our study).

First, none of the studies discussed above fully deals with the fact that some students may be better suited for Catholic schools than others. It is hard to find evidence that urban Catholic school students are simply better students than their public school counterparts

8 More recently, Glenn (1994) discussed the decline in vocabulary at most educational levels in the US. He attributes this to a decline in the reading of newspapers and suggests that part of this decline in reading is due to an increase in the amount of television time.
on some unobserved dimension. However, existing Catholic school students may be the students who have the most to gain from Catholic schooling. We may be safe in concluding that Catholic schools provide real benefits for their current students. Much harder to ascertain is how many other students could benefit from Catholic schooling if given the opportunity. Would students from the Muslim families benefit from Catholic schooling? Given the available data, we cannot answer this question. At best, we may expect significant benefits from Catholic schooling for students who are quite similar to the existing population of Catholic school students.

2.2. Characteristics of the family and student

The home environment and economic status of a student have always been recognized as important determinants of student achievement. As Hanushek (1986, p. 1163) states,

Virtually regardless of how measured, more educated and more wealthy parents have children who perform better on average.\(^9\)

For example, Goldhaber and Brewer (1997) found a positive relationship between family income and math achievement.

An extensive literature exists regarding the effects of divorce on the children in the household. The potential importance is highlighted by Emery and Forehand (1994) who point out that each year 2% of all children are in families going through a divorce, and 40% of children aged 16 reside in a divorced family. Hopper (1997) in his extensive review of the general literature of the effects of divorce on children enumerates several potential negative effects. These include but are not limited to, internalizing problems (e.g. emotional difficulties), externalizing problems (e.g. aggression), prosocial skills (e.g. social competence) and also, difficulties in school (which may be related to the other problems). It is the potential negative effects on academic achievement tests that are of relevance to the present paper.

Divorce can have detrimental effects on children for at least two reasons. According to Hernandez and Myers (1995, p. 57) and other researchers, a short run effect of a divorce is a substantial drop in family income for many children. Such a decline may require the mother go back to work and at the same time lead to heightened tension in the relationship with the child. Moreover, and related to the first point, with one parent removed from the household, children may receive less care and attention each day than would have been the case in a two parent family.

Amato and Keith (1991a,b) and Forehand, Armistead and Klein (1995) provide surveys of the literature regarding divorce and student achievement which cover more than 40 studies of the issue. When analyzing the effects of divorce on a child’s achievement, Amato and Keith (1991a) found that students from divorced families score lower on measures of academic achievement by about one-sixth of a standard deviation when compared to children from intact families. Amato and Keith (1991b), however, noted that even though significant in the statistical sense, the sizes of the effects would be considered trivial by many educational researchers. Forehand, Armistead and Klein (1995, p. 256) summarize the literature in the following statement.

In conclusion, parental divorce is related to multiple areas of children’s school performance. However, contrary to the image portrayed in the public media, the scientific data suggest that the magnitude of these effects, as well as effects in settings other than school, is relatively small. (p. 256)

Their explanation of this overall conclusion is based on three major points. First, divorce may well be a painful experience for youngsters but it apparently does not hurt their ability to function in school (Emery & Forehand, 1994). Second, some students are probably affected negatively by divorce but others are not. Or, as they put it

Therefore, one should not conclude that divorce is harmless—or that it is so harmful that all children should automatically receive psychological treatment. (Forehand et al., 1995).

Finally, and related to the previous point, it is not divorce itself but the conditions in the home that go along with such a situation that affect a child’s ability to function (Amato, 1993).

The panel nature of the NELS data and associated panel estimation techniques used in the current study should provide controls for the accompanying factors just noted. Based on the divorce literature, we do not expect divorce to have a large effect on the math achievement of high school students.\(^10\)

\(^9\) See Gyimah-Brempong and Gyapong (1991) for a justification for including family background as a determinant of achievement. Pungello, Kupersmidt, Burchinal and Patterson (1996), provide a survey of the literature on the effects of low income on student academic performance.

\(^10\) However, negative results are not guaranteed. Marsh (1990), using the High School and Beyond Study (HSB) found no effects on achievement due to family dissolution. Mulkey, Crain and Harrington (1992) found negative effects of divorce on vocabulary and science tests although the effects of dissol-
Many students are involved with part time work while in high school. Primarily, students are involved in labor market activity to provide money for current living expenses and discretionary items. Most studies have found negative associations of student work with academic performance and subsequent educational attainment. Interestingly, this variable should provide an excellent indicator of student motivation.

3. Data and methodology

We now discuss the data, estimation procedures, variable definitions, and the actual samples chosen.

3.1. National Education Longitudinal Study of 1988

The National Center for Education Statistics (NCES) instituted the National Education Longitudinal Studies (NELS) program in response to the need for policy-relevant, time series data on nationally representative samples of elementary and secondary students in the US. The general aim of the NELS program was to study the educational, vocational, and personal development of students at various grade levels, and the personal, family, social, institutional, and cultural factors that may affect that development. The base year of the National Education Longitudinal Study of 1988 (NELS 88) represented the first stage of a major longitudinal effort designed to provide panel data about the critical transitions experienced by students as they leave elementary school and progress through high school and into college and then on to their careers. Beginning in 1988, a cohort of 8th graders was followed at two-year intervals as this group passed through high school. The data were collected in four separate waves beginning in the fall of 1988: the base year (1988), the first follow-up (1990) and, the second follow-up (1992). A third follow-up was conducted in 1994 as these individuals were followed up was implemented in two stages. Because some of these 10th graders in 1990 were not in the US, or were not in the 8th grade in the spring term of 1988, the representative subsample of the 8th grade cohort was augmented through a process called ‘freshening.’ The goal was to provide a representative sample of students enrolled in the 10th grade in the 1989–90 school year. The second follow-up in 1992 repeated all components of the first follow-up study. Each student and dropout selected for the first follow-up in 1990 was included in the second follow-up in 1992. From within the schools attended by the school members, 1500 high schools were selected as sampled schools. Of these schools, the full complement of component activities occurred in 1374 schools, i.e. some schools did not meet certain criteria to provide school and teacher related information to be included in the study. For students attending schools other than those 1374 schools, only the student and parent questionnaires were administered. See Section 3.4 for a discussion of the particular samples used in the estimations.

3.2. Estimation procedure

This section describes the basic statistical framework and estimation procedures used in the estimations. A unique feature of the NELS data is that individual students are followed in the 8th, 10th, and 12th grades in 1988, 1990, and 1992. How to exploit this information raises important questions. The first is whether or not ‘individual effects’ are an important factor in the data underlying our achievement models. If present, one then must choose between the proper specification—fixed effects or random effects.

The panel analysis builds on the regression model of the form

\[ y_{it} = \alpha + \beta' x_{it} + \epsilon_{it} \]

where \( E[\epsilon_{it}] = 0 \) and \( \text{Var}[\epsilon_{it}] = \sigma^2_{it} \) (1)

where \( y_{it} \) measures math achievement, there are \( K \)

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13 This section follows Greene (1997).
regressors in $x_{it}$, not including the constant term, and $\varepsilon_{it}$ has the usual characteristics for the $ith$ individual and $ith$ time period. Introduction of the $\alpha_i$ allows for potential heterogeneity (individual effects) across students and these effects are assumed to be constant over time, $t_i$ and represented by $\alpha_i$. If the $\alpha_i$ do not vary across individuals, and thus there is only one constant, $\alpha$, ordinary least squares (OLS) provides efficient and consistent estimates of $\alpha$ and $\beta$.

One may find it helpful to think of these individual specific student effects as those factors which affect achievement and which may or may not be known by the student, but which clearly are not known by the researcher. These might include such things as the student’s mathematical ability, industriousness, and unmeasured characteristics in the student’s home environment which affect achievement. In order to test economic hypotheses, one has to include variables that are thought to explain the outcome. When variables are omitted from the equation, usually due to the lack of data, the effects of these omitted variables appear through the error term. If the effects of these omitted variables are correlated with variables included in the regression equation, parameter estimates will be biased. This is the well known ‘omitted variable bias’ in econometrics. Panel data provide a way to account for this problem when the individual specific effects vary between students but not over time. If ‘individual effects’ don’t exist, ordinary least squares in a pooled cross-section environment will yield consistent and efficient estimates of relevant parameters. When individual effects are present biased results will occur. In this case, a decision must be made as to which of two ‘individual effects’ specifications will yield consistent estimates, the fixed effects or the random effects model. Let’s first look at the question of whether or not there are individual effects, and then the tests, if heterogeneity exists, to determine whether the fixed or random effects specification is appropriate.

Suppose we ignore the $\alpha_i$, assume a single constant $\alpha$ and estimate the following regression.

$$y_{it} = \alpha + \beta'x_{it} + \varepsilon_{it}$$

(2)

If there is no correlation between the $x_{it}$ and $\alpha_i$, OLS will yield unbiased coefficient estimates. However, if the $x_{it}$ and $\alpha_i$ are correlated, the estimates are not consistent. As an example of this problem, suppose that family income is one of the variables in the achievement equation and that student motivation is unobserved. Highly motivated students are likely to score higher on conventional achievement tests. If students from higher income families tend to be more highly motivated, $\alpha_i$ and $x_{it}$ will be correlated and coefficient estimates will be biased. How can the problem of bias be solved? One approach to the problem of accounting for individual effects is the fixed effects specification and the other is the random effects specification.\(^\text{14}\)

3.2.1. Fixed effects

The fixed effects model assumes that there is a separate constant term for each unit, i.e. differences across individual students are captured by differences in the constant term where there is a constant term $\alpha_i$ for each student, as in Eq. (1).

$$y_{it} = \alpha_i + \beta'x_{it} + \varepsilon_{it}$$

(3)

$\alpha_i$ in the fixed effects specification in Eq. (3) is non stochastic and constant over time for each individual. This model is commonly known as the least squares dummy variable (LSDV) model. This is a classical regression model which can be estimated using OLS.\(^\text{15}\)

In order to determine whether individual effects are present, the Lagrange multiplier test of Breusch and Pagan (1980) is used.\(^\text{16}\) Time effects can be included in the model via the use of dummy variables.

3.2.2. Random effects

The fixed effects model assumes that differences between individuals are reflected in the regression equation as parametric shifts, i.e. the $\alpha_i$ are fixed. For instance, we may have the total population and are confident that our model is constant and applies to the population under study. However, suppose the $\alpha_i$ are random draws from the population. The individual effects model can be formulated to include the random nature of the data as:

$$y_{it} = \alpha + \beta'x_{it} + \omega_i$$

(4)

$\omega_i$ is the fixed effects specification and the other is the random effects specification.\(^\text{14}\)

\(^\text{14}\) The random or fixed effects specifications may not be completely remove the bias if the individual effects change over time. But the period under consideration in our study is short enough that individual effects like industriousness, mathematics ability, or unmeasured family characteristics are likely to be very stable.

\(^\text{15}\) Unbiased estimates of $\beta$ can be obtained without estimating $\alpha_i$ by taking differences from the variable means. The key is that factors which don’t change difference out as is indicated below. This is illustrated by the following equations where $\bar{x}_i$ and $\bar{y}_i$ are the means for each individual across all time periods.

$$y_{it} - \bar{y}_i = \alpha_i + \beta(x_{it} - \bar{x}_i) + \varepsilon_{it} - \bar{\varepsilon}_i$$

\(^\text{16}\) An equivalent test for OLS versus fixed effects involves standard $F$-tests to determine whether there is one versus $i$ constants. The advantage of the Lagrange multiplier test is that only the restricted model needs to be estimated to carry out the test while the $F$-test requires estimation of both the restricted and unrestricted models.
where there are $K$ regressors not including the constant term, and $\mu_i$ is an individual specific disturbance term that is random and constant through time. What separates the random effects from the fixed effects specification is that in the fixed effects model the individual effects enter into the equations as specific constants while in the random effects specification the individual effects enter through the disturbance term. In the present context, the $\mu_i$ are disturbances specific to the individual student that do not vary over time.

For an individual student, disturbances in different periods are likely to be serially correlated, highly motivated students in period 1 are likely to be highly motivated in period $t+1$. Therefore, the efficient estimator is generalized least squares (GLS). The variance components are estimated using the residuals from ordinary least squares regressions. GLS estimates then are computed using the estimated variances.\(^{17}\)

3.2.3. Choosing the model

As was noted above the first modeling decision is whether there are individual effects. If there are none, pooled cross-section time series OLS yields consistent and efficient estimates. If individual effects are present, panel data improves on the pooled cross-section data in terms of both efficiency and consistency since it extracts more information from the data.

If individual effects are present, the question which arises is whether to employ the fixed or the random effects specification. The basic issue is whether or not the $\mu_i$ and the $x_i$ are orthogonal. Or, stated differently, are the $\mu_i$ and $x_i$ correlated? If they are orthogonal, the random effects specification is appropriate while if they are correlated, the fixed effects specification should be chosen. In fact, if the $\alpha_i$ and the $x_i$ are correlated the estimates in the random effects approach will be inconsistent. Estimates in the fixed effects regime are consistent, although not efficient. Thus, the existence of orthogonality between $\mu_i$ and the $x_i$ is critical when choosing between the fixed effects or the random effects specification. Hausman (1978) devised what has become a well known test for orthogonality of the $\mu_i$ and the regressors. A low value for the chi-square test statistic would lead to acceptance of the null hypothesis of orthogonality. When the test statistic (chi-square) is greater than the critical value, the null hypothesis is rejected and one should use the fixed effects model.

The results reported in reported in Section 4 are based on fixed effects specifications with the exception of the samples for white students observed in years one, two, and three (1/2/3), and years one and two (1/2). For these two samples of white students, random effects specifications are employed since the Hausman test statistic is not significant at the 5% level, i.e. the null hypothesis of orthogonality of the $x_i$ and the error term cannot be rejected. For all other samples the decision to use the fixed effects specification is based on the fact that the Hausman statistic is significant at the 5% level.

3.3. Variable definitions

All models in this study use students’ mathematics item response theory (IRT) scores as the dependent variable. These are the students’ scores from quantitative tests administered by the National Center for Education Statistics (NCES). Test items include word problems, graphs, equations, quantitative comparisons, and geometric figures.\(^{18}\)

Following the literature, we use independent variables which are measures of the school environment, the family environment, and the individual student. The variables we have chosen to emphasize are those which relate to the students time spent learning mathematics. The following variables are used in the models estimated in Section 4.\(^{19}\) Unless noted, the variable was available in all three years, 1988, 1990, and 1992.

- **Minutes per class**: Number of minutes per class period for the student’s mathematics class. (Available only in years 1 and 2)
- **Hours of homework**: Number of hours the student spent on mathematics homework each week.
- **Hours of TV**: Hours per day the student watched TV on Monday through Friday each week.
- **Legal days per year**: Number of legal days in a school year for the school district attended by the student.\(^{20}\)
- **Class hours per week**: Number of hours the student’s math class met each week. (Available only in years 1 and 3)
- **Class size**: Number of students in the student’s mathematics class. (Available only in years 1 and 3)

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\(^{17}\) For a detailed description of the estimation process see Greene (1997) or Aksoy (1995).

\(^{18}\) Students were administered different tests in the 8th, 10th, and the 12th grades which differed in their degree of difficulty. Therefore, unadjusted gain scores cannot be used to determine achievement. In NELS 88 these tests were made comparable through the use Item Response Theory (IRT). By making adjustments based on the student responses to the questions, IRT provided the information necessary to put student scores on a continuous vertical scale. That is, IRT makes scores comparable over the different tests and thus comparable over time. See Rock and Pollack (1990) and Ingels et al. (1990) for a detailed description of the mathematics IRT score.

\(^{19}\) For a detailed description of the variables see Aksoy (1995) and Ingels et al. (1990).

\(^{20}\) The actual number of days of attendance for a particular student may vary due to absenteeism.
Beginning teacher salary: Beginning teacher salary in thousands of dollars in the school district attended by the student.

Teacher experience: Total number of years of teaching experience at the primary and the secondary level of the students’ mathematics teacher.

Hispanic teacher: A dichotomous variable which equals 1 if the student had an Hispanic mathematics teacher, and 0 otherwise.

Black teacher: A dichotomous variable which equals 1 if the student had a black mathematics teacher, and 0 otherwise.

Female teacher: A dichotomous variable which equals 1 if the student had a female mathematics teacher, and 0 otherwise.

Private: A dummy variable which equals 1 if the student attended a private school and 0 otherwise.

Urban: A dichotomous variable which equals 1 if the student attended a school located in a city of more than 50,000 people, 0 otherwise.

Rural: A dichotomous variable which equals 1 if the student attended a school located in a rural or farming community, small city, or town with fewer than 50,000 people which is not a suburb of a larger place, an Indian reservation or a military base or station, and 0 otherwise.

Income: Student’s family income from all sources in thousands of dollars.

Parents divorced: A dichotomous variable which equals 1 if the parents of a student are divorced, 0 otherwise.

Hours worked: Number of hours the student spends in the labor market for pay each week.

3.4. Samples

The balanced samples underlying our models are now discussed. In a balanced panel data set every individual (student) in the sample is observed in all available years. Results of three separate estimations using different balanced samples from NELS 88 are reported. These include a balanced panel model using all three years (i.e. base year, first follow-up, and second follow-up), a balanced panel model using only the base year and the first follow-up; and finally, a balanced panel model using the base year and the second follow-up data. These models are based on 2756 students who met the above criteria.

The reason for using balanced panel samples involving two years of data is due to the limitations in the data set—our key variables of interest are available only in the first and second and first and third years. The variable minutes of each math class period appears only in the base year and the first follow-up year. The variable indicating the number of hours her class meets each week appears in the base year and the second follow-up year but not in the first follow-up year. As a result, the sample is probably biased towards students who are college bound and the results should be interpreted accordingly.21

The three balanced panel samples are referred as balanced (1/2/3), balanced (1/2), and balanced (1/3) for the samples including students who are observed in all three years, students observed in the base year and the first follow-up year, and students observed in the base year and the second follow-up years, respectively. The means and standard deviations for the variables included in the all races and white samples are reported in Tables 1 and 2. Due to the small size of the samples, separation of different races is not possible and therefore estimation results include the effects of variables for all races. The only exception is the case of whites since they constitute the majority of the students in the pooled race panel

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21 The balanced samples were constructed by requiring that the students have complete data on the variables of interest in the base year and the years that followed. To have been included in the balanced sample for years 1 and 2, the individual not only had to have been in the base year sample but also had to have complete data for year 2. Another sample included those in the base year and the second follow-up. The 3 year sample required complete data over all three years. Some students drop out of school, move out of the country, or they just get lost for one reason or another. On the other hand, the individual could have still be enrolled in school and had complete data except for the fact that she did not take mathematics. Or, the individual could have taken mathematics but had missing data on one or more of the variables—independent or dependent. The total number of observations in the three mutually exclusive samples is 2756.

In the base year 10.4% of responses to the question ‘number of hours watching TV’ is missing. The percentages for math homework and family income are 5.2 and 12.2%. In the first follow-up the missing data on math homework and hours of TV are 14.8 and 15.8%, respectively. In the first follow-up a full 17% had no test scores at all. If we add to this the fact that these are not the same people it is not surprising that the samples are relatively small. Another point to note is that unless students are college bound, it would not be surprising to observe students who took math in 1988 to not take math classes in one or both of 1990 and 1992. In 1993 about 40% of new high school graduates were not enrolled in college (see Ehrenberg & Smith, 1997).

Some students’ parents, or teachers, or even school administrators in a particular year did not complete the questionnaires, which eliminated them from the sample. NELS added new students to account for the attrition, but for our purposes the same individual is needed in order to perform a balanced panel analysis. Note, however, that the studies in the Special Issue of the Journal of Human Resources (1998) found that attrition leads to little or no bias in estimates of statistical models for many US longitudinal data sets.
Table 1
Means and standard deviations of variables: All races\textsuperscript{a}

<table>
<thead>
<tr>
<th>Variable</th>
<th>Means (Standard deviations)</th>
<th>Years (1/2/3) n=964</th>
<th>Years (1/2) n=1086</th>
<th>Years (1/3) n=706</th>
</tr>
</thead>
<tbody>
<tr>
<td>Math-IRT score</td>
<td></td>
<td>44.82 (14.08)</td>
<td>35.13 (12.48)</td>
<td>43.72 (14.78)</td>
</tr>
<tr>
<td>Weekly hours of math homework</td>
<td></td>
<td>3.06 (3.55)</td>
<td>1.89 (2.85)</td>
<td>2.83 (3.45)</td>
</tr>
<tr>
<td>Number of hours per week student works</td>
<td></td>
<td>7.93 (10.11)</td>
<td>6.40 (10.28)</td>
<td>8.43 (9.28)</td>
</tr>
<tr>
<td>Hours per day watching TV</td>
<td></td>
<td>2.60 (1.51)</td>
<td>2.70 (1.59)</td>
<td>2.56 (1.54)</td>
</tr>
<tr>
<td>Student attended private school</td>
<td></td>
<td>0.03 (0.17)</td>
<td>0.02 (0.14)</td>
<td>0.05 (0.21)</td>
</tr>
<tr>
<td>School in urban area</td>
<td></td>
<td>0.23 (0.42)</td>
<td>0.24 (0.43)</td>
<td>0.26 (0.44)</td>
</tr>
<tr>
<td>School in rural area</td>
<td></td>
<td>0.50 (0.50)</td>
<td>0.41 (0.49)</td>
<td>0.38 (0.49)</td>
</tr>
<tr>
<td>Number of legal days in school year</td>
<td></td>
<td>179.39 (3.06)</td>
<td>178.96 (3.01)</td>
<td>179.07 (3.25)</td>
</tr>
<tr>
<td>Student’s family income (in $1000s)</td>
<td></td>
<td>41.18 (23.80)</td>
<td>33.75 (21.91)</td>
<td>41.17 (24.83)</td>
</tr>
<tr>
<td>Beginning district teacher salary</td>
<td></td>
<td>21.51 (2.94)</td>
<td>19.74 (2.59)</td>
<td>21.76 (2.95)</td>
</tr>
<tr>
<td>Hispanic math teacher</td>
<td></td>
<td>0.10 (0.31)</td>
<td>0.11 (0.32)</td>
<td>0.07 (0.25)</td>
</tr>
<tr>
<td>Black math teacher</td>
<td></td>
<td>0.56 (0.50)</td>
<td>0.51 (0.50)</td>
<td>0.56 (0.50)</td>
</tr>
<tr>
<td>Female math teacher</td>
<td></td>
<td>14.92 (8.06)</td>
<td>14.41 (8.00)</td>
<td>15.24 (8.00)</td>
</tr>
<tr>
<td>Years of teaching experience</td>
<td></td>
<td>21.12 (0.33)</td>
<td>0.12 (0.33)</td>
<td>0.12 (0.32)</td>
</tr>
<tr>
<td>Minutes per math class</td>
<td></td>
<td>52.79 (4.90)</td>
<td>N/A</td>
<td>N/A</td>
</tr>
<tr>
<td>Number of students in student’s math class</td>
<td></td>
<td>N/A</td>
<td>N/A</td>
<td>25.55 (10.49)</td>
</tr>
<tr>
<td>Hours per week of math class</td>
<td></td>
<td>N/A</td>
<td>N/A</td>
<td>4.36 (0.92)</td>
</tr>
</tbody>
</table>

\textsuperscript{a} There are 191 Blacks, 75 Hispanics, and 60 Asian students in the balanced (1/2/3); 184 Blacks, 104 Hispanics, and 37 Asian students in the balanced (1/2); and 115 Blacks, 52 Hispanics, and 64 Asian students in the balanced (1/3).

4. Results

This section reports the panel estimations for the determinants of math achievement based on balanced samples from the National Education Longitudinal Study of 1988 (NELS). Regression results are shown in Table 3 for the pooled all races samples and Table 4 for the white samples. The constant term in the regression captures students who came from families that did not go through a divorce between 1988 and 1992,\textsuperscript{24} who attended public schools located in suburban areas, and who had white male teachers.\textsuperscript{24}

4.1. Teacher and school factors

According to Hanushek (1986) and Ehrenberg and Brewer (1994), the most important exogenous factors affecting student achievement relate to teacher and school inputs. One of the important conclusions to be drawn from our results is that the variable measuring the minutes per each class period spent on mathematics is important in terms of the size of its coefficient and its statistical significance. For the year one and two (1/2) samples (column 2 of Tables 3 and 4) the estimate for the coefficient ranges from 0.20 to 0.23. Increasing the length of the class by 10 min is associated with a gain of 2–2.3 points in mathematics achievement, or a change which equals 5.4–6.2% of the relevant sample averages. Considering that the average class length for the sample is 53 minutes, a 10 minutes increase certainly is possible.

Each additional hour per week spent on mathematics homework increases student achievement scores by 0.67, \textsuperscript{24} We take into account those parents who got divorced and remarried, and also parents who got married and divorced, i.e. multiple divorces and marriages are accounted for. As long as the parents are married the dichotomous variable, DIVORCED, has the value 0, and changes to 1 as a divorce takes place.
Table 2  
Means and standard deviations of variables: white samples

<table>
<thead>
<tr>
<th>Variable</th>
<th>Means (Standard deviations)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Years (1/2/3)</td>
</tr>
<tr>
<td>Math-IRT score</td>
<td>47.66 (13.36)</td>
</tr>
<tr>
<td>Weekly hours of math homework</td>
<td>2.99 (3.34)</td>
</tr>
<tr>
<td>Number of hours per week students works</td>
<td>8.67 (10.73)</td>
</tr>
<tr>
<td>Hours per day watching TV</td>
<td>2.48 (1.42)</td>
</tr>
<tr>
<td>Student attended private school</td>
<td>0.05 (0.21)</td>
</tr>
<tr>
<td>School in urban area</td>
<td>0.20 (0.40)</td>
</tr>
<tr>
<td>School in rural area</td>
<td>0.53 (0.50)</td>
</tr>
<tr>
<td>Number of legal days in school year</td>
<td>179.27 (3.18)</td>
</tr>
<tr>
<td>Student’s family income (in $1000s)</td>
<td>46.20 (22.83)</td>
</tr>
<tr>
<td>Beginning district teacher salary</td>
<td>21.35 (2.74)</td>
</tr>
<tr>
<td>Hispanic math teacher</td>
<td>0.01 (0.08)</td>
</tr>
<tr>
<td>Black math teacher</td>
<td>0.05 (0.21)</td>
</tr>
<tr>
<td>Female math teacher</td>
<td>0.58 (0.49)</td>
</tr>
<tr>
<td>Years of teaching experience</td>
<td>14.12 (7.88)</td>
</tr>
<tr>
<td>Students parents divorced</td>
<td>0.10 (0.29)</td>
</tr>
<tr>
<td>Minutes per math class</td>
<td>N/A</td>
</tr>
<tr>
<td>Number of students in math class</td>
<td>N/A</td>
</tr>
<tr>
<td>Hours per week of student’s math class</td>
<td>N/A</td>
</tr>
</tbody>
</table>

Table 3  
Mathematics achievement: balanced panel estimation results for the all races samples

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficients (1) Years (1/2/3)</th>
<th>Coefficients (2) Years (1/2) n=1086</th>
<th>Coefficients (3) Years (1/3) n=706</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>n=964</td>
<td>n=1086</td>
<td>n=706</td>
</tr>
<tr>
<td>Weekly hours of math homework</td>
<td>0.67* (6.14)</td>
<td>0.36* (3.14)</td>
<td>0.81* (4.24)</td>
</tr>
<tr>
<td>Number of hours per week students works</td>
<td>0.18* (4.66)</td>
<td>0.10* (2.95)</td>
<td>0.25* (3.50)</td>
</tr>
<tr>
<td>Hours per day watching TV</td>
<td>-0.84* (-2.96)</td>
<td>-0.19 (-0.81)</td>
<td>-1.59* (-3.70)</td>
</tr>
<tr>
<td>Student attended private school</td>
<td>-0.68 (-0.16)</td>
<td>9.03* (2.47)</td>
<td>2.42 (0.29)</td>
</tr>
<tr>
<td>School in urban area</td>
<td>1.27 (0.64)</td>
<td>-6.39* (-3.19)</td>
<td>-1.25 (-0.49)</td>
</tr>
<tr>
<td>School in rural area</td>
<td>-2.33 (-1.12)</td>
<td>-1.18 (-0.45)</td>
<td>-2.72 (-0.95)</td>
</tr>
<tr>
<td>Number of legal days in school year</td>
<td>0.003 (0.02)</td>
<td>0.13 (0.80)</td>
<td>0.32 (1.23)</td>
</tr>
<tr>
<td>Student’s family income (in $1000s)</td>
<td>0.04** (1.65)</td>
<td>0.15* (4.49)</td>
<td>0.11* (3.75)</td>
</tr>
<tr>
<td>Beginning district teacher salary</td>
<td>0.61* (3.44)</td>
<td>0.51* (3.23)</td>
<td>0.94* (3.21)</td>
</tr>
<tr>
<td>Hispanic math teacher</td>
<td>6.80* (2.02)</td>
<td>0.35 (-0.19)</td>
<td>-1.36 (-0.33)</td>
</tr>
<tr>
<td>Black math teacher</td>
<td>-4.88* (-3.31)</td>
<td>-2.78* (-2.36)</td>
<td>-7.28* (-2.60)</td>
</tr>
<tr>
<td>Female math teacher</td>
<td>-0.23 (-0.28)</td>
<td>-0.51 (-0.74)</td>
<td>-3.31* (-2.57)</td>
</tr>
<tr>
<td>Years of teaching experience</td>
<td>0.04 (0.87)</td>
<td>0.02 (0.57)</td>
<td>0.15** (1.73)</td>
</tr>
<tr>
<td>Student’s parents divorced</td>
<td>-0.13 (-0.10)</td>
<td>-1.44 (-1.48)</td>
<td>1.08 (0.49)</td>
</tr>
<tr>
<td>Minutes per math class</td>
<td>N/A</td>
<td>0.20* (2.35)</td>
<td>N/A</td>
</tr>
<tr>
<td>Hours per week of student’s math class</td>
<td>N/A</td>
<td>N/A</td>
<td>-0.34 (-0.47)</td>
</tr>
<tr>
<td>Number of students in math class</td>
<td>N/A</td>
<td>N/A</td>
<td>-0.08 (-1.22)</td>
</tr>
<tr>
<td>Constant</td>
<td>29.20 (1.00)</td>
<td>-12.49 (-0.42)</td>
<td>-33.44 (-0.73)</td>
</tr>
<tr>
<td>Lagrange Multiplier test statistic (prob. value)</td>
<td>168.76 (0.000000)</td>
<td>196.03 (0.000000)</td>
<td>32.60 (0.000000)</td>
</tr>
<tr>
<td>Hausman test statistic (prob. value)</td>
<td>26.36 (0.023298)</td>
<td>31.12 (0.008463)</td>
<td>27.15 (0.039809)</td>
</tr>
</tbody>
</table>

* Figures in parentheses are t-ratios.  
** Coefficient significant at *5% and **10% level.
Studies of the effects of homework. Quite possible. These results are consistent with previous research. For the all races (1/3) and white (1/3) samples (the only years the data are available), student achievement in mathematics is not affected by how many hours of instruction per week a student received since the coefficient is not statistically significant at the 5% level. The conclusion about the effects of the number of hours of instruction per week is different from the findings of Gilby et al. (1993) who found small positive effects for extra hours of math instruction. However, theirs was a panel sample of elementary school students while ours is high school students. There may be larger returns to the extra hours of instruction in the early years of education which decline as one advances through the school system. But keep in mind that the result we found for extra hours of math per class period is consistent with Gilby et al. (1993).

The length of the school year in a school district is important in both the statistical sense as well as its marginal impact on student achievement. Each additional hour during the school week spent watching the television lowers the math achievement score by between 0.55 and 1.31 points for whites and by between 0.84 and 1.59 points for the pooled race samples. The coefficient is statistically significant for all but the all races years 1 and 2 sample.

For the all races (1/3) and white (1/3) samples (the only years the data are available), student achievement in mathematics is not affected by how many hours of instruction per week a student received since the coefficient is not statistically significant at the 5% level. Each additional hour during the school week spent watching the television lowers the math achievement score by between 0.55 and 1.31 points for whites and by between 0.84 and 1.59 points for the pooled race samples. The coefficient is statistically significant for all but the all races years 1 and 2 sample.

For the all races (1/3) and white (1/3) samples (the only years the data are available), student achievement in mathematics is not affected by how many hours of instruction per week a student received since the coefficient is not statistically significant at the 5% level. The conclusion about the effects of the number of hours of instruction per week is different from the findings of Gilby et al. (1993) who found small positive effects for extra hours of math instruction. However, theirs was a panel sample of elementary school students while ours is high school students. There may be larger returns to the extra hours of instruction in the early years of education which decline as one advances through the school system. But keep in mind that the result we found for extra hours of math per class period is consistent with Gilby et al. (1993).
achievement. An additional 5 days would add 2 points to math achievement, a 5.4% increase compared to the sample mean.

Teacher salary is an important determinant of achievement for all samples. In the pooled all races samples, the effect of each $1000 rise in the beginning salary of teachers is associated with an increase in mathematics achievement of between 0.51 and 0.94 points. Results for whites range from 0.46 to 1.06 points. Students taught by a black math teacher score from approximately 2.8 to 7.3 points lower than those who have a white math teacher. The results for black teachers are similar to those found by Goldhaber and Brewer (1997), who also used NELS. Results for the effects of being taught by an Hispanic teacher were mixed. The all races (1/2/3) and white (1/2/3) samples suggest a positive Hispanic effect while the two year panels suggest no effect at all or a negative effect. The effect on achievement of being taught by females is also mixed. In four of the six models, there was no significant difference in performance of female and male teachers. In the two panels which include the 1st and 3rd years, students with a female teacher scored lower. This is different from Goldhaber and Brewer (1997) who found a positive effect of being taught by a female. However, they did not use the third year, which is where the negative coefficients appear. In conclusion, with the exception just noted these results are consistent with Hanushek (1992), who notes that there is no strong indication of differences in performance for male and female teachers.

In the two panel samples including observations from the first and the second years (1/2), students attending private schools score significantly higher on the math achievement tests. However, private school attendance is not a significant determinant of achievement for any of the other samples analyzed. In fact, the significant results should be interpreted with extreme caution since only 2% of the students in these samples were attending private schools. The sensitivity of the coefficients is undoubtedly due to the low number of private school students in the balanced samples underlying our research.

For the pooled races (1/3) and white (1/3) samples, student achievement in mathematics is not affected by class size. The coefficient is negative, indicating smaller classes are consistent with higher achievement, but does not approach statistical significance. This is consistent with the findings of Hanushek (1992) and Ehrenberg and Brewer (1994). The coefficient of teacher experience, always positive, was statistically significant for two of three samples for white students. When significant, however, the coefficient is only about 0.10, indicating that it takes 10 years of experience to cause a 1 point increase in math achievement. This result is consistent with other findings in the literature. Although students attending schools located in urban and rural areas tended to score lower on the math tests compared to their counterparts in suburban schools, the effects of the variables were not generally significant. The one exception occurred for white students who had data in all three years where the urban students outscored suburban by almost 3.5 points. However, when controls were entered for minutes of the math class period, the coefficient became insignificant.

4.2. Student and family related factors

As expected, the coefficient of the family income variable is always positive and is statistically significant in four of the six models. The coefficient shows up as more important, both statistically and in magnitude, for the all races samples containing only two years of information compared to similar samples containing all three years of information. The insignificant income coefficient in the 1/2/3 and 1/3 samples is surprising. However, earlier in section III, we argued that biased estimates are likely in a cross-section if unobservables such as industriousness or motivation are excluded from the model. To the extent that more motivated and industrious students come from a higher income background, using panel techniques may lessen the effect of income.

Students from a home where there is a divorced parent do not appear to be negatively affected in terms of lower math achievement. This finding is consistent with other studies in the literature, especially since the panel allows for control of unobservables. Working in the labor market does not appear to be an impediment to math achievement when controls are entered for unobservable variables. In fact, the coefficient on the hours worked per week variable is statistically significant and positive in all but the white (1/2) sample where it is not significant. For whites, the coefficient ranges from 0.13 to 0.28.

5. Conclusions and policy implications

Our main objective has been to estimate the determinants of mathematics achievement for three different panel samples of high school students included in the base year of the National Education Longitudinal Study of 1988 (NELS). With few exceptions, studies of the determinants of academic achievement have relied on cross-sectional data. Because data are available in NELS on each student for two, and in some cases three years, we were able to employ panel estimation tech-

With the exceptions of Gilby et al. (1993), Betts (1996), and Goldhaber and Brewer (1997), we have not found studies that have analyzed educational production functions with panel data using samples representative of the elementary and secondary school populations in the US. Only Gilby et al. (1993) examined the effects of time learning on achievement in a panel context.
niques to account for potential individual effects in the data. As a result, we can control for unobservable variables such as motivation and innate ability, and at the same time obtain consistent estimates of the coefficients central to our research. The fact that students had to have taken math at least 2 of the 3 years in order to be included in the balanced samples probably yields a sample which is college bound. Results should be interpreted with this in mind.

The main emphasis in our research was to determine the effects on math achievement of variables which measure the amount of time students spend learning mathematics. In this section we highlight results for variables that are central to our stated goal.

Two of the key variables were ‘minutes of the student’s daily math class’ and the ‘number of hours per week for the student’s math class.’ Data availability was the determining factor for constructing the balanced samples underlying our research. The minutes variable was only available in NELS in the base year and the first follow-up survey while the hours per week variable was only collected in the base year and second follow-up survey. Consequently, the sample sizes were smaller than would have been the case had we used cross-sectional data. In spite of this limitation, the benefits of applying panel estimation techniques on NELS far outweigh the sample size limitation. The panel estimation techniques allow us to control for unobserved variables such as ability and motivation which are omitted variables in a typical cross-section data set.

Zeith and Cool (1992) emphasize the importance in the literature of time spent on active learning as a determinant of achievement. Strong results were obtained with regard to the amount of time that students are being taught in and out of class. Our findings with respect to the length of the student’s daily math class, the amount of time the student spent doing homework during the week, and the amount of time the student watched television each day each had the expected effects on achievement. Although the length of the school year exhibited weaker effects on achievement, the coefficients had the expected signs and the coefficient for whites was very important in the panel sample including white students in the base year and first follow-up.

Longer daily math class periods are associated with higher mathematics achievement. Each extra minute of instruction increases math achievement by 0.20–0.23 points on average and, therefore, an additional 10 min is associated with an increase in mathematics achievement scores of 2.0–2.3 points, or approximately 5.4–6.2% compared to the relevant sample mean test scores.

Time spent on mathematics homework is important statistically and in the size of the impact. Extra hours spent on mathematics homework increase student math test scores. This finding lends support to the argument by Betts (1998) that one way to increase achievement is for schools to heighten their expectations about students. Therefore, increasing the amount of homework appears to be a low cost method of improving mathematics achievement. These results about the effects of the length of the daily math class and homework are also consistent with Krueger’s (1998) suggestion that experiments be done with lengthening the school day. His recommendation is based on the fact that crime rates on school days peak between the hours of 2 and 4 p.m. but on weekends the peak occurs in the evening. Thus, he argues for more enriching activities at school during the afternoons. Our results clearly suggest potentially important academic payoffs to such a policy.

Watching television has a negative impact on mathematics achievement. A combination of increased hours of homework while at the same time reducing the amount of TV time appears to be a promising policy with the potential to enhance mathematics achievement.

In spite of the fact the coefficient of the variable measuring the number of days in the school year in the student’s school district was significant for only one of our panel samples, it is still a variable that merits discussion in our concluding section. One reason is the evidence that students in the US forget enough in the summer and require about four weeks of review in the fall of each year. Moreover, increasing the length of the school year is important since many developed countries have longer school years than in the US and, their students have out performed US students on internationally administered mathematics examinations. For the sample of whites included in the base year and the first follow-up, whites (1/2), the impact of each additional day is 0.40 points in math achievement (Table 4 column 2). The results suggest that an increase of 10 days per school year would increase white students’ scores by 4 points or approximately 10% compared to their sample means. Even if the effect is a fraction of this, the result is important for the reasons noted above.26 Lewis and Seidman (1994) as part of their proposed program for increasing mathematics achievement in the US proposed a program which not only increases the number of school days in the year but also includes substantial amounts of homework during the summer. Our results clearly support the last of their recommendations about homework. We argue that at a minimum, our results provide ammunition for Krueger’s (1998) call for extensions of the school year to 210 days in random school districts within a state and then

26 In our sample the range for the number of days in a school year is from 175 to 185 days, and therefore, an increase of 10 days is possible. However, this increase would require extra expenditures which may or may not be justified by the additional benefits of the increased mathematics achievement. A benefit–cost analysis to determine the justification of this proposed change is necessary, but beyond the scope of this study.
carefully evaluating the results. Basically, he calls for experimentation and evaluation.

The one detracting result for our time on task variables was the puzzling insignificance of the coefficient of the variable number of hours per week in math class. The sample underlying the result includes students from the base year and second follow-up of NELS. Given that this variable was available only in the base year and second follow-up and the minutes per daily class was available only in the base year and the first follow-up, it was not possible to try to separate the differences into specification versus sample differences.

Other teacher and school-related variables in our equations, although not our prime variables of interest, provided results of potential importance. Beginning teacher salary coefficients are statistically significant and positive. The impact of a $5000 increase in teacher salary is an approximately 7% increase in the mean score for all samples. Since higher starting salaries not only will cause more but better qualified people to consider teaching, the quality of teachers and instruction should rise. The current policy in Massachusetts of offering a $20,000 bonus to encourage new teachers to consider teaching in the state is consistent with this view. Teacher experience is found to have statistically significant but small positive effects on achievement for some models. Class size was not statistically significant in the achievement equations. The results for the race and sex of the student’s teacher were mixed. Students who had black teachers tended to score lower compared to students who had a white male math teacher, a result consistent with Goldhaber and Brewer (1997) who used the first two years of NELS. In four of the six models, being taught by a female teacher compared to a white male teacher had no effect on math achievement. In the samples for the base year and 2nd follow-up, students of female teachers fared worse than those taught by a white male teacher. Attendance at private schools had no consistent impact on achievement, except for the base year and first follow-up where the private school premium was large and positive. Students in urban and rural areas tended to score lower than suburban students on math tests with one exception, whites in the sample where students are included in all three years of the survey, where the coefficient was significant and positive. But when controls are entered for ‘minutes per class’ the coefficient becomes insignificant.

Socioeconomic and background characteristics had mixed effects. Living in a household where the parents were divorced had no significant effect on student achievement. As expected, the coefficient of family income was always positive and generally was statistically significant. The final result of interest relates to the question of whether or not market work by a student negatively affects achievement. Our results indicate no such effect. The coefficients were either not statistically significant or when significant, the coefficient was positive.

In a data set as rich as NELS hundreds of potential avenues exist for future research. Here are three areas which could add significantly to our knowledge about factors affecting learning. First, the number of legal days in the school year does not measure absenteeism of individual students. Including in econometric models a variable measuring absenteeism would give policymakers insights into the effects of extra days in school on achievement. Second, given our interest in the effects of time in learning math on math achievement, samples of nonwhites were too small to estimate separate models by race. Other studies need to be done which have larger samples of minorities. This would provide tests for structural differences by race in the achievement equations. Along these same lines, it would be possible to test the hypotheses put forth by some that blacks taught by blacks will perform better on achievement tests. Third, NELS contains a wealth of information about gifted students. Extending our models to gifted students would provide useful insights to policy makers about a very important student group.

We conclude the paper by highlighting some of the results as they relate to the recent inter country results from the Third International Math and Science Study (TIMSS) for 8th graders. Results from TIMSS suggest that spending more time on a subject per year does not consistently correlate with the math score. Our results from NELS are consistent in that the number of hours spent on math per week was not statistically significant. However, the number of daily minutes per class period was statistically significant and associated with at least a one point increase in math achievement for each 5 minutes added to the class period. Finally, our results relating to class size are consistent with those found in TIMSS—namely, on the average, class size appears to have a negligible effect on achievement.

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References


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27 See The Economist (1997) for a summary of the 8th grade results.


