Measuring Primary and Secondary School Characteristics: A Group-Based Modeling Approach

Andrew Halpern-Manners
John Robert Warren
Jennie E. Brand

CCPR-014-08

Measuring Primary and Secondary School Characteristics: A Group-Based Modeling Approach*

Andrew Halpern-Manners
John Robert Warren

Department of Sociology
Minnesota Population Center
University of Minnesota

Jennie E. Brand

Department of Sociology
University of California-Los Angeles

Version: September 2008

Measuring Primary and Secondary School Characteristics: A Group-Based Modeling Approach

Abstract

In this paper we introduce a new way to conceptualize and measure the educational resources that young people encounter as they make their way from kindergarten to high school graduation. Using recent methodological advances in group-based modeling and a unique data set, we empirically test for and identify a series of categorically distinct school quality trajectories. We find that these trajectories vary significantly in terms of their intercept and slope, their prevalence within the sampled population, and in the sociodemographic makeup of their constituent members. We then present an extended empirical example illustrating relationships between school quality trajectories and important post-secondary educational outcomes, both before and after controlling for static, single-year measures of primary and secondary school characteristics. Our results suggest that the chronology of students' exposures to different educational resources is significantly associated with college enrollment, college selectivity, and, in some instances, college completion.

Measuring Primary and Secondary School Characteristics: A Group-Based Modeling Approach

Studies relating schools' resources to students' outcomes have a rich and longstanding history in the social sciences. In fact, it requires only modest exaggeration to describe the "school effects" literature as unrivaled in terms of volume of scholarly output. Researchers in sociology, economics, and education policy have modeled the relationship between a broad range of indices—including pupil-teacher ratio, length of school year, per pupil expenditures, and teachers' education—and an even broader assortment of individual-level outcomes (Alexander and Griffin, 1976; Altonji and Dunn, 1996; Alwin and Otto, 1977; Behrman and Birdsall, 1983; Betts, 1995; Card and Krueger, 1996; Coleman, et al., 1966; Entwisle and Hayduk, 1988; Greenwald, et al., 1996; Grogger, 1996; Hanushek, 1989; Heckman, et al., 1996; Heyneman and Loxley, 1983; Hill, 2008; Johnson and Stafford, 1973; Lloyd, et al., 2003; Meyer, 1970; Rose and Betts, 2004; Ross and Mirowsky, 1999; Sorenson and Hallinan, 1977).

While these analyses have produced an impressive and methodologically diverse body of work, we contend that much of the existing research has failed to sufficiently account for important temporal dimensions of the relationship between schools' attributes and student outcomes. If we start from the premise that the salience of school resources varies by non-trivial amounts depending on the age and grade level of the student (Entwisle, et al., 2003; Entwisle and Hayduk, 1988), then it would seem inadequate to observe school characteristics at only one or two points in time, as is typical in the school effects literature. This problem is further exacerbated if schools themselves change—in the resources they make available to their students, in their enrollment and attendance, and in the size, experience, and composition of their teaching staff—over the course of a given student's career. The primary contribution of this paper is to empirically test for the presence of distinct school characteristic trajectories, to describe the shape

and prevalence of these trajectories, and to consider whether they are predictive of subsequent attainments.

We draw extensively on group-based modeling techniques (or latent class growth models) to propose a new way of conceptualizing youths' exposure to different types of schooling resources and environments (Jones and Nagin, 2007; Jones, et al., 2001; Loughran and Nagin, 2006; Nagin, 1999; Nagin, 2005; Nagin and Land, 1993; Nagin, et al., 2003; Nagin and Tremblay, 2005; Nagin and Tremblay, 1999). Rather than observing school characteristics at a particular moment in time, or taking the average over a fixed period, the group-based approach adopts a finite mixture modeling strategy to assign individuals to a limited number of "developmental trajectories" or "latent longitudinal strata" on the basis of continuities and differences in the primary and secondary schools they attended (Haviland and Nagin, 2005). In addition to identifying a finite number of categorically distinct trajectories, the semi-parametric technique provides the statistical capacity to relate one's trajectory group to distal outcomes like post-secondary enrollment, college completion, and so forth.

The remainder of this paper is organized into five sections. In the section that follows, we present an abbreviated summary of research on school effects, focusing in particular on common conceptual and methodological limitations and opportunities for elaboration. We then go on to describe our data set and key indicators of school characteristics. In the third section, we introduce the group-based modeling technique, review its statistical foundation, and show how it can be used to identify and probabilistically assign students to distinct school characteristic trajectories. This exposition of trajectory-based thinking and its practical application to school effects research is, in our view, the main contribution of the paper. In the fourth section, we present results from the group-based models, describe the characteristics of each group's constituent members, and, in a series of exploratory analyses, relate trajectory membership to a

variety of post-secondary educational outcomes. Here we use the term "exploratory" decidedly, as our multivariate models of school effects are properly regarded as a starting point for subsequent data collection efforts, analyses, and discussion. We conclude by recapitulating our main findings and remarking on various avenues for further research.

RECONCEPTUALIZING SCHOOL EFFECTS

Questions concerning the efficacy of school resources are not new. Since at least the release of the Coleman Report (Coleman, et al., 1966) social scientists have sought to link school quality to a variety of outcomes, including achievement on standardized tests (Elliott, 1998; Hanushek, 1986; Wenglinsky, 1997), labor market experiences and lifetime earnings (Card and Krueger, 1992; Grogger, 1996; Grogger, 1996; Heckman, et al., 1996), and educational and occupational attainment (Alexander and Eckland, 1977; Betts, 1995; Dearden, et al., 2002; Greenwald, et al., 1996; Griffin and Alexander, 1978; Hill, 2008; Kerckhoff, et al., 1982; Meyer, 1970; Sander, 1993). The results of these efforts have produced a number of counterintuitive, and often controversial, findings (see, e.g., Hanushek, 1997; 2003). Academic performance has been shown to be independent of school attributes and resources (Hanushek, 1986; 1989). Similar conclusions have been reached with respect to earnings (Betts, 1995; Grogger, 1996) and employment (Dearden, et al., 2002). Even in those cases where the available evidence suggests that additional resources significantly improve achievement and/or work-related outcomes, the effect size has often been relatively modest (Card and Krueger, 1996; Grogger, 1996; Heckman, et al., 1996; Hedges, et al., 1994).

While it is certainly possible that these findings reflect an underlying reality about the salience of educational resources, we suggest that inadequacies relating to the way investigators conceptualize and measure school characteristics make such claims premature. Despite

longstanding disagreements over aggregation and variation in how administrators allocate resources (see, e.g., Card and Krueger, 1996; Condron and Roscigno, 2003; Hanushek, et al., 1996; Heckman, et al., 1996; Murnane, 1991), school effects scholars have been remarkably consistent in one important respect: they tend to observe school characteristics at a single time point, usually early during the primary years or when students enter high school. Griffin and Alexander (1978), for example, examined the effect of school characteristics during students' sophomore year on their subsequent occupational attainment and earnings. Wenglinsky (1997) investigated dependencies between students' eighth grade school environment and standardized test achievement. Grogger (1996) and Betts (1995) asked whether educational inputs specific to a particular academic year can be used to explain future wage differentials. In fact, in his extensive survey of the school effects literature, Hanushek (1997) reviewed nearly 400 estimates of the impact of school resources on student outcomes—all of which pertained to elementary school characteristics or characteristics at the secondary level, but never both.[1]

The validity of this measurement technique is unclear. If the resources available to students remain stable over time and across institutional boundaries, then a single point in time measurement of school characteristics should suffice. In other words, if students who attend advantaged (disadvantaged) high schools are the same students who attend advantaged (disadvantaged) primary schools, then the difference between a cross-sectional indicator and a variable that captures over-time variation in educational inputs should be empirically immaterial. If, however, education is better understood as a cumulative process (Entwisle, et al., 2003; Entwisle and Hayduk, 1988; Pallas, 2003), in which resources, enrollments, and institutional environments vary in non-trivial ways from one school year to the next, then ignoring the chronological patterning of students' exposures would seem like a gross simplification of a more complicated reality.[2]

The latter perspective is consistent with central analytic themes in the sociological life-course tradition (Elder, 1985; Elder, et al., 2003; Elder and Shanahan, 2007). Rather than adopting a cross-sectional or "snapshot" model of the association between educational resources and subsequent outcomes, a life-course orientation encourages explicit attention to the *duration* and *timing* of individuals' exposures. In recent years, this organizing framework has been profitably applied within a number of problem domains, including such diverse topics as economic deprivation during childhood (Duncan and Brooks-Gunn, 1997; Mayer, 1997; Wagmiller, et al., 2008; Wagmiller, et al., 2006), longitudinal patterns of marital quality and health (Wickrama, et al., 1997), the effect of family instability on the risk of premarital birth (Wu, 1996; Wu and Martinson, 1993), and the relationship between work-related stress and longevity (Pavalko, et al., 1993). We are not aware of any studies, however, that integrate life-course concepts into school effects research.

This failure to apply life course perspectives to school effects research is surprising, as we can imagine a number of instances in which an attention to temporal variability in students' exposures might prove revealing. Although two students may enjoy equivalent educational environments at a particular moment in time, for example, the total *duration* of their exposures to these environments between kindergarten and the end of high school may vary in considerable and consequential ways. Perhaps even more importantly, the *timing* of students' exposures relative to their developmental stage may serve to independently facilitate or constrain subsequent attainments. This may be true even among students who are the same with respect to educational environments at single points in time or who experience the same *average* educational environments across the schooling years. Under these circumstances, we might expect the utility of a snapshot measure vis-à-vis a longitudinally-based methodology to depend on when in a student's career the cross-sectional measure is obtained. Whereas a single-year

7

measure describing school characteristics at one point in time may capture much of the meaningful variation in individuals' exposures, an analogous variable obtained at a different point during students' schooling years may obscure more than it reveals. Our core objective is to understand whether (and in what situations) more dynamic measures of the attributes of students' schools—measures that capture the level, duration, and timing of exposures to various school environments—are more closely related to various outcomes than single point in time measures.

If it is true that the duration and timing of exposures to educational environments matter, then longitudinal school characteristic data and a more dynamic measurement technique are needed to ascertain and model over-time variation in students' resource exposures. We discuss each of these needs in turn in the next section.

DATA AND METHODS

Constructing our data set and measures

We analyze data from the Wisconsin Longitudinal Study (WLS), a long-term study of a random sample of 10,317 men and women who graduated from Wisconsin high schools in 1957. The WLS includes detailed information about social background, youthful aspirations, schooling experiences, military service, labor market experiences, family characteristics and events, social participation, psychological characteristics, retirement, and health. Survey data were collected from the original respondents by mail and/or telephone in 1957, 1964, 1975, 1993, and 2005 and from a randomly selected sibling in 1977, 1994, and 2004. Retention across survey waves has been remarkably high, with more than three quarters of living respondents participating in the most recent round of surveys.

To supplement the limited amount of early school-characteristic data offered in the WLS, we collected archival data from annual district reports filed with the Wisconsin Department of

Public Instruction at the conclusion of every academic year. These data are housed at the Wisconsin State Historical Society, and can be obtained for all years in which the WLS cohort was enrolled in primary and secondary schools (e.g., 1945-1957). For each year our records include information (separately by level of schooling) on the number of pupils and number of teachers per district; the duration of the school year; the number and type of school facilities available; teachers' salaries; expenditures on instruction; enrollment by grade and gender; number of graduates in 1957; and teachers' education. Because the vast majority of Wisconsin school districts were comprised of one elementary school, one middle school, and one high school, these district-level data are largely synonymous with school-level records.[3]

Two variables in the WLS allowed us to link the annual district data to members of the cohort: (1) a question from the 2005 telephone survey that asked a random subset of respondents to provide the name and location of the elementary school they attended the longest; and (2) high school district identifiers from the original 1957 survey (when members of the WLS cohort were high school seniors). Using only these two pieces of information to match respondents to 13 years of district-level data required us to make strong—but, as we discuss below, empirically defensible—assumptions about respondents' geographic mobility. Namely, we were forced to assume that individuals remained in a single district—the district they reported attending the longest—for the *entirety* of their primary years, and that they spent *all* of their high school years in the secondary district where they were observed during their senior year. In order to make these assumptions more tenable, we selected only those individuals who attended primary and secondary school in the same district. The logic behind this criterion is straightforward: a respondent who can be placed in the same district for the majority of their primary years and for the completion of their secondary years would seem less likely to have changed districts at any point during the intervening period.[4] Given the exceptionally high levels of geographic stability

among Wisconsin residents during the 1940s and 1950s, it is not surprising that this sample

eligibility constraint eliminated only a minority of cases (9.5 percent).

After harmonizing the yearly district reports over time, we computed a number of

frequently cited measures of school quality. These time-varying variables, which represent the

focal point of our analysis, include indicators of classroom characteristics (percent of teachers

with 4 or more years of schooling beyond the 12th grade, and pupil-teacher ratio) and financial

resources and outlays (per pupil expenditures, which we express in constant 2000 dollars). In

very rare instances, we utilized logical edits and imputation procedures to handle implausible or

missing values.[5] In the next section we describe how we applied these variables within a group-

based modeling framework.

Measuring school quality as a group-based trajectory

Our analytic strategy capitalizes on recent advances in group-based trajectory modeling. Below,

we briefly outline group-based modeling techniques as they will be used to identify and assign

school districts (and, by extension, the students who attend them) to different school quality

trajectories.

Since the attributes under consideration belong to schools—not the individual students

who make up their student body—the unit of analysis in our group-based models is the school

district. Accordingly, let $Y_s = \{y_{s1}, y_{s2}, y_{s3}, \ldots, y_{sT}\}$ represent the longitudinal sequence of

measurements of some attribute of school district *s* (e.g., pupil-teacher ratio or per pupil

expenditures) as observed over the *T* time periods ($t = 1, 2, 3,\ldots, 13$) between 1945 and 1957 in

which WLS respondents were enrolled, and let $P(Y_s)$ represent the probability of observing Y_s.

Group-based trajectory models assume that there are *J* underlying trajectory groups such that

$$P(Y_s) = \sum_{j=1}^{J} \pi_j P^j(Y_s),$$ (1)

where $P^j(Y_s)$ is the probability of observing longitudinal sequence Y_s given membership in group j and π_j is the probability of group j (Jones, et al., 2001; Nagin, 1999). The model assumes that the random variables y_{st} ($s = 1, 2, 3, \ldots, S; t = 1, 2, 3, \ldots, T;$) are independent conditional on membership in group j. The probability, then, that the sth school district follows a given trajectory of characteristics and resources, conditional on membership in the jth group, is equal to the product of individual probabilities over time:

$$P^j(Y_s) = \prod_{t=1}^{T} p^{jt}(y_{st}).$$ (2)

The values of π_j ($j = 1, 2, 3, \ldots, J$) are estimated by a multinomial logit function:

$$\pi_j = e^{\theta_j} \Big/ \sum_{j=1}^{J} e^{\theta_j},$$ (3)

where the parameters to be estimated, θ_j, can take on any value provided that (1) each of the resulting probabilities (e.g., π_j) properly falls between 0 and 1; and (2) the values of π_j sum to 1 across the J trajectory groups (Nagin, 2005).

The functional form of $p^{jt}(y_{st})$ in Eq. (2) is determined by whether y_{st} is measured on a continuous, count, or binary scale. When y_{st} is a continuous variable (censored or otherwise), as is the case throughout our analysis, $p^{jt}(y_{st})$ is assumed to follow a censored normal distribution, in part to allow for the possibility of clustering at the minimum and maximum (Jones, et al., 2001; Nagin, 1999; Nagin, 2005).[6] The link between time—or year, for our purposes—and the variable in question is modeled as a polynomial relationship; the software that estimates these models allows for up to a quartic relationship. For example, when y_{st} is continuous, the linkage is established via latent variable y_{st}^{*j} such that:

$$y_{st}^{*j} = \beta_0^j + \beta_1^j Year_{st} + \beta_2^j Year_{st}^2 + \beta_3^j Year_{st}^3 + \beta_4^j Year_{st}^4 + \varepsilon_{st}, \qquad (4)$$

where $\beta_0^j, \beta_1^j, \beta_2^j, \beta_3^j$ and β_4^j are parameters that determine the intercept and slope of the *j*th

trajectory, and where disturbance ε_{st} is assumed to be distributed normally with a mean of zero

and constant variance σ^2 (Jones, et al., 2001; Nagin, 1999; Nagin, 2005). The parameter

estimates are permitted to vary freely across the *j* classes, a feature that allows different groups of

students to have distinctive trajectories of school characteristic exposures.

After the model has been fit, the parameter estimates can be used to generate a school

district's posterior probability of group membership, denoted $\hat{P}(j \mid Y_s)$. The posterior probability

records the likelihood that a district with the observed sequence of measurements *Y* belongs to

trajectory group *j*. In this respect, the posterior probability provides a criterion for assigning

districts to their most likely trajectory group and for assessing the precision with which the model

fits the data. As described in considerable detail in Nagin (2005:79-92), the appropriate formula

is:

$$\hat{P}(j \mid Y_i) = \frac{\hat{P}(Y_i \mid j)\hat{\pi}_j}{\displaystyle\sum_j^J \hat{P}(Y_i \mid j)\hat{\pi}_j}, \qquad (5)$$

where $\hat{P}(Y_i \mid j)$ and $\hat{\pi}_j$ are calculated according to Eq. (2) and (3), respectively.

Using the matching procedures outlined in the previous section, we subsequently mapped

the *school*-level estimates obtained from Equations 1-5 onto the *individual*-level person records

contained in the WLS. All models were estimated using maximum likelihood, where the

maximization was performed using a general quasi-Newton procedure (Condron and Roscigno,

2003). As described below, we relied upon the Bayesian Information Criterion (BIC) for model

selection (e.g., to determine the preferred number of trajectory groups). Model estimation was

accomplished using the add-on SAS procedure PROC TRAJ (see Jones, Nagin, and Roeder

(2001) and Jones and Nagin (2007) for more information).

RESULTS

Results from the group-based trajectory models

In this section we present results from our group-based models, describe the characteristics of the

individuals assigned to each trajectory group, and then go on to relate trajectory group

membership to a series of distal outcomes. We began our analysis with the randomly selected

subset of WLS respondents who were asked to provide the name and location of their Wisconsin

elementary school (n = 3,520). We subsequently dropped cases (n = 1,154) where the individual

attended private or parochial schools during either their primary or secondary years;

unfortunately, the district reports that we collected provide no information about the resources

and characteristics of these schools. We then eliminated respondents whose district could not be

located in our district-level data (n = 181) or who attended a district that had more than three

missing district years (n = 208). Finally, as noted above, we deleted 337 cases where individuals

changed districts at some unknown point between their primary and secondary years.[7] The

resulting analytic sample, which includes 1,640 individuals, is representative of WLS men and

women in this birth cohort with no record of school transfers and at least a public high school

diploma.[8]

Following convention, we relied on the BIC to identify the optimal number of trajectory

groups (Raftery, 1995).[9] The criterion, which offers a useful way to assess fit in mixture settings

(D'Unger, et al., 1998; Múthen, 2003; Nagin, 1999; Nagin, 2005), extracts a penalty for adding

additional parameters (e.g., trajectories or higher-order terms) and thus tends to favor more

parsimonious model specifications. To calibrate the improvement in fit between a model with *j*

trajectories and a model with $j + 1$ trajectories, we treated the change in the BIC as an approximation of the Bayes factor (D'Unger, et al., 1998; Kass and Raftery, 1995; Kass and Wasserman, 1995; Raftery, 1995). According to Raftery's (1995) revision of Jeffrey's (1961) scale, a Bayes factor of more than 20 represents "strong evidence" that model M_{j+1} is preferable to model M_j. As illustrated in Table 1, we added groups iteratively until further improvement in model fit (e.g., strong evidence) could not be achieved. Our preferred models are shown in bold.

Figure 1 plots trajectories of per pupil expenditures from kindergarten through 12th grade. We obtained the optimal fit using a three-group model, in which 50.2 percent of respondents were classified as receiving "low increasing" expenditures (group 1), 13.8 percent were grouped into the "high" expenditure trajectory (group 2), and the remaining 36.0 percent were assigned to the "medium-flat" trajectory (group 3).[10] Clearly, for some students, advantages (disadvantages) at the primary level translate readily into advantages (disadvantages) at the secondary level. For instance, the trajectories characterizing groups 1 and 2 appear to run parallel to one another, with members of group 1 enjoying consistently higher levels of expenditures throughout the entire time frame.[11] For other students expenditure levels during elementary school are not as predictive of expenditures during subsequent years. Members of group 3, for example, begin their scholastic careers in relatively affluent primary schools but end up in environments not unlike those of group 1.

Figure 2 displays our preferred three-group model of teachers' schooling trajectories.[12] The form of the expected trajectories—labeled for heuristic purposes as "low" schooling, "medium" schooling, and "high" schooling—reveals appreciably large between-group differences at the primary level, which taper off rapidly as students transition to high school.[13] The attenuation is so substantial, in fact, that the roughly 60 percentage point deficit between members of group 1 and group 3 is erased over a matter of school years. This patterning of

exposures would appear to contradict assumptions that underlie the usage of a single-year approach. Unlike what one would expect to find in an environment characterized by perfect or near perfect stability in resources over time and across grade levels, a measurement pertaining to some point during the high school years produces a markedly different picture than a measurement taken during elementary school, and a different picture still than an approach that considers the entire duration.

One can draw a similar conclusion from Figure 3, which graphs pupil-teacher ratio trajectories for the preferred five-group model. Although the results identify groups that run parallel to one another throughout the entire period, they also contain trajectories that converge, bifurcate, and intersect as students progress from grade school to graduation. Consider the trajectory that characterizes individuals in group 1, for example. Comprising just over 8.1 percent of the sampled population, group 1 consists of respondents whose primary and secondary schools had uniformly low pupil-teacher ratios, ranging from 23 in kindergarten to just over 20 during the twelfth grade. This pattern departs sharply from that of group 5, in which pupil-teacher ratios diminished sharply over time, falling from a maximum of 33 in the third grade to a level in high school that was roughly commensurate with group 1. That these trajectories take variable routes to the same destination is, again, consistent with the notion that timing and the choice of measurement technique may alter how one portrays youths' exposure to school resources.

Individual-level characteristics of trajectory group members

In the foregoing analysis we identified and described distinct school characteristic trajectories. Interesting questions about the individual members of these trajectories, however, remain unanswered. Do persons in different trajectories differ with respect to socioeconomic

background? Does family structure and urban-rural status influence the likelihood of belonging

to one trajectory versus another? The best way to answer these and related questions, at least at

the bivariate level, is to use the maximum posterior probabilities calculated in Equation 5 to sort

WLS respondents into their most likely trajectory group. These assignments, in turn, permit us to

cross-classify trajectory group membership with a number of individual, familial, and geographic

characteristics.[14]

Table 2 provides the resulting cross-classification. We expressed social background,

family structure, and geographic characteristics in terms of father's occupational status (measured

using Duncan's socioeconomic index or SEI) in 1957, father's years of schooling, total parental

income as derived from tax records, religious denomination (Catholic or otherwise), size of the

respondent's sibship as of 1957, whether the respondent's family was intact for the majority of

their youth, and whether the respondent is of farm origins. Our three social background

indicators, as well as our measure of sibship size, are all continuous. Consequently, our

assessments of whether those variables vary significantly across categories of trajectory group are

based on Analysis of Variance *F*-tests. Our assessment of whether intact family status, religious

denomination, and urban/rural status—all of which are categorical—are based on χ^2 tests. The

relevant *p*-values are reported beneath each subset of tabulations.

Comparing the profiles of each of the groups within the three school quality indicators, a

number of significant relationships are apparent. With rare exceptions, group membership is a

correlate of farm residence, number of siblings, and social background. Members of the "low"

teachers schooling group, for example, are more likely to live on farms than members of either of

the other two groups, belong to somewhat larger families, and tend to come from households with

relatively poorly paid and poorly educated parents. In fact, the 14.7 point gap in father's SEI

between members of the "low" teachers schooling group and their more advantaged counterparts in the "high" group represents more than three-fifths of a standard deviation. The only variable that does not correlate significantly with any of the three trajectory types is intact family status. This result is hardly anomalous given the low rates of marital dissolution in Wisconsin during the time period under consideration; just over 91 percent of our sample reported growing up in an intact family.

This descriptive analysis provides evidence that respondents are not distributed randomly to the trajectory groups we identified above. Urban-rural status, socioeconomic background, and family size each correlate with the trajectory of educational resources that student experience from kindergarten through 12th grade. Although we have not yet demonstrated whether trajectories of primary and secondary school characteristics are independently associated with post-secondary outcomes, it seems likely (albeit not especially surprising) that trajectories of school characteristics serve as an important mechanism through which educational advantage and disadvantage are reproduced across generations.

Group-based trajectories, single-year measures, and distal outcomes

In the previous section, we related respondents' trajectory membership to a number of antecedent factors. It remains to be seen whether and to what degree different school characteristic trajectories are associated with distal outcomes. Does one's teachers' schooling trajectory significantly affect their likelihood of attending college? Are per pupil expenditure trajectories independently predictive of post-secondary educational attainments? Do trajectories of pupil-teacher ratio serve as a determinant of the type of post-secondary institution individuals attend? Finally, and perhaps most importantly for the purposes of this paper, are there specific instances where a trajectory-based approach conveys information over and above what more typical cross-

sectional indicators offer? To carefully consider these questions, we turn our attention to a series of multivariate models in which different post-secondary educational outcomes are expressed as a function of trajectory group membership, a set of individual-level covariates, and single-point in time measures of school characteristics.

As we suggested earlier, in some circumstances we expect the difference between trajectories and cross-sectional measures to be empirically immaterial. We speculated that this result ought to obtain when there is a high degree of stability in resource exposures over time (e.g., when two students' trajectories are characterized by parallel lines). There are presumably other instances, we reasoned, where the relative utility of the two techniques is contingent on when the cross-sectional indicator is observed. The exposures of two students who end up in qualitatively similar high schools despite attending widely discrepant elementary schools are not likely to be well summarized by a snapshot measure of characteristics at the secondary level; but may be adequately described by a comparable measure taken at some point during the primary years. Finally, we anticipate a third instance in which the level of over-time instability in students' exposures is sufficient to *require* the use of a dynamic, longitudinally-based methodology. Examples of this kind of variability in exposures are evident in our five-group model of pupil-teacher ratio, which contains a number of complex and crisscrossing trajectories.

Our inferential strategy for testing these expectations is straightforward. For each of our outcomes, which we generically refer to as *Y* in the following discussion, we estimate three subsets of models. We begin with a baseline specification in which we regress *Y* on dummy variables indicating respondents' trajectory group and a vector of exogenous controls, which we describe in more detail below.[15] In the second set of equations we express *Y* in terms of respondents' trajectory group, a vector of exogenous controls, and a cross-sectional measure of school characteristics from their senior year of high school. The third and final group of models

closely resembles the second, except the additional single-year indicator reflects school characteristics as observed when individuals were in the first-grade rather than high school. By comparing the second and third models to the baseline specification, we can make inferences about whether and in what circumstances the trajectory-based approach enhances our ability to predict respondents' subsequent attainments.

Table 3 provides a list of the variables included in our analysis. As shown in the first three rows, we consider three binary educational outcomes: college enrollment, and, conditional on enrollment, college selectivity and degree receipt. Following a coding strategy employed elsewhere using the same data set (Brand and Halaby, 2006), we defined college selectivity using categorical national rankings supplied by *Barron's Profile of American Colleges 1969 College Admissions Selector*.[16] To adjust for potential confounding factors, all of the reported models include four measures of social background (parental income, father's occupation status, and father and mother's years of completed schooling), five demographic variables (the age of the householder, urban/rural status, parents' marital status, and sibship size), and a dummy variable indicating religious denomination (Catholic).[17]

Figures 4 through 6 summarize the main findings. Each panel contains a separate trajectory group comparison, with the vertical axis indicating the predicted effect of group membership on the log-odds of observing the three outcomes of substantive interest (as listed on the horizontal axis). The solid circle reports the point estimate associated with the specified contrast in our baseline model, and the attached line segment provides the 95 percent confidence interval. The hollow circle and hollow triangle represent the same point estimate after including single-year measures pertaining to respondents' high school and elementary school, respectively.[18] The first group listed in each panel heading serves as the reference category.

Figure 4 presents findings for the three per pupil expenditure trajectories. If trajectories of per pupil expenditures are a determinant of post-secondary outcomes we should expect the point estimate indicated by the shaded circle to differ significantly from zero. Although this result is not evident with respect to college enrollment or completion, we do find evidence to this effect in our model of elite college attendance. Respondents in the most advantaged trajectory, group 2, are, on average, three and half times $[e^{1.25} = 3.49]$ more likely to attend a selective institution than their counterparts in the most disadvantaged "low-increasing" category (group 1), all else being equal. Likewise, respondents in the comparatively disadvantaged "medium-flat" expenditure group (group 3) are $[e^{-1.15} - 1 = -0.69]$ 70 percent less likely than members of the "high-increasing" category (group 2) to attend a selective post-secondary institution.

Do the results for elite college attendance change when we enter cross-sectional expenditure measures from first grade or respondents' senior year of high school? In the panel summarizing estimates pertaining to the contrast between groups 1 and 2—two trajectories that exhibit extreme stability, running parallel to one another *throughout the entire time period*—the answer is quite clearly "yes." The effect associated with the trajectory-based indicator decreases in magnitude and fails to reach significance in either of the specifications that include additional cross-sectional expenditure measures. The answer is slightly more complicated, however, for the contrast between groups 2 and 3. Recall that these students begin in roughly the same location, only to bifurcate as they near high school graduation. It seems reasonable to expect, then, that one's expenditure trajectory will matter net of a single-year elementary school measure, but not a cross-sectional indicator from the secondary years. This is in fact the case. The significant result that we observed in the baseline specification is robust to the inclusion of a single-year indicator from the primary years, but, as indicated by the hollow circle, not a measure pertaining to respondents' senior year of high school.

Figure 5 displays the relevant results for teachers' schooling. The estimates suggest that trajectories of instructors' educational background are positively and significantly related to the likelihood of college enrollment; that the magnitude of these effects is pronounced; and that the choice of measurement technique matters. Compared to those in the more disadvantaged "low" schooling trajectory (group 1), respondents in the "high" and "medium" schooling trajectories (groups 3 and 2) are, respectively, $[e^{0.46} - 1 = 0.58]$ 58 and $[e^{0.35} - 1 = 0.42]$ 42 percent more likely to enroll in college. In both instances the relevant point estimates decrease in size and fail to reach significance after we include a single-year measure from the first grade (as indicated by the hollow triangle), but hardly attenuate at all when we enter a cross-sectional measure taken from respondents' senior year (as indicated by the hollow circle). This result is not surprising given the shape of the respective trajectory groups, which, as we described earlier, show substantial inequities throughout the primary years that dissipate almost completely upon students' transition to high school.

Figure 6 displays results from comparisons between the five pupil-teacher ratio groups. Containing a number of intersecting pathways, this set of trajectories provides an ideal testing ground for our hypothesis concerning instability and crosscutting sequences of exposures. For this reason and in the interest of space, we present only a select number of contrasts, all of which involve groups whose trajectories crisscross.[19] These comparisons yield two clear findings. The first involves the contrast between groups 1 and 4, in the leftmost panel. Subject to relatively high pupil-teacher ratios during the primary years but comparatively low ratios thereafter, members of group 4 are roughly 60 percent $[e^{-0.90} - 1 = -0.59]$ less likely than students in group 1 to obtain a BA or higher, conditional on college enrollment. The inclusion of single-year

measures does little to dampen or "explain away" this effect: in all three subsets of models individuals in group 4 are at a significant disadvantage with respect to college completion.

The second finding pertains to college selectivity and the comparison between groups 2 and 4. Recall, again, that members of group 4 enjoy lower pupil-teacher ratios during high school despite spending their primary years in comparatively crowded classrooms. As summarized in the middle panel, this sequence of exposures corresponds to a significant reduction in the odds of attending an elite post-secondary institution, holding all else constant. Once again, entering a single-year measure from the primary and secondary years does not appear to moderate this effect: in each model the coefficient associated with group 4 is significant and negative, ranging in magnitude from -0.99 in the baseline specification to -0.93 in the model with a single-year primary measure. Together these findings provide evidence that trajectory-based indicators capture aspects of students' exposures that could not otherwise be ascertained from a more conventional approach, particularly when students' sequences of exposures reflect substantial over-time heterogeneity.

DISCUSSION

In this paper we have suggested that school effects researchers too often pay inadequate attention to temporal variability in educational resources across individuals' academic careers. In an attempt to overcome this inadequacy, we introduced a new way to conceptualize and measure the educational resources that young people encounter as they make their way from kindergarten to high school graduation. Using a finite mixture modeling strategy and a unique longitudinal data set, we empirically tested for and identified a series of categorically distinct school quality trajectories. The results indicate that these trajectories vary substantially in terms of their

intercepts and slopes, their prevalence within the sampled population, and in the socio-demographic makeup of their constituent members.

The results also suggest that a dynamic measurement approach enriches our understanding of whether and in what circumstances indicators of school quality influence students' subsequent attainments. We documented a number of instances in which individuals' trajectories of exposure matter net of their experiences at any one point in time; instances that conformed to our prior expectations. Although this type of observation is hardly novel within the life-course literature (Elder, 1985; Elder, 1998; George, 1999; Moen, et al., 1992), and is becoming increasingly common in other substantive areas (D'Unger, et al., 1998; Laub, et al., 1998; Mayer, 1997; Moen, et al., 1992; Mustillo, et al., 2003; Sampson and Laub, 1995; Wagmiller, et al., 2008; Wagmiller, et al., 2006; Wickrama, et al., 1997; Wu, 1996; Wu and Martinson, 1993), to our knowledge the present analysis is the first to report such a result within the context of school effects research.

These conclusions deserve qualification in at least two ways. First, the generalizability of our empirical findings is limited by the selective nature of the sample and a number of missing data issues (as we described above). The data portray empirical patterns that are specific to a select group of individuals during a particular historical moment. It seems reasonable to suppose that more contemporaneous conditions—in which migration is more commonplace, districts are larger and characterized by more heterogeneity, and school choice is increasingly abundant— would produce an even wider and more variable assortment of student resource exposures and school characteristic trajectories. Future research would thus do well to replicate our analysis using a more modern data source, preferably one that permits investigators to incorporate a fuller array of educational institutions (e.g., public and private schools), a more finely detailed unit of analysis, as well as more complete information pertaining to students' transfer history.

23

Second, we have no misapprehensions about our estimates of school effects being "causal" in the classic counterfactual sense of the word. Although we performed covariate adjustments for a number of easily observed attributes, our specifications were still relatively rudimentary and likely omitted a number of less easily measured traits and motivations that influence selection into different trajectory groups. The absence of these variables in our models and in our interpretation of the results is not meant to indicate their presumed irrelevance, as it is almost certainly the case that unobserved factors meaningfully influence where youth attend school and how they fare upon graduation. Instead, it is a reflection of the exploratory nature of this particular project. Building on the conceptual framework and methodological tools that we sought to introduce and elaborate in this paper, subsequent research stands to benefit by making a more concerted effort to model and adjust for this type of unobserved heterogeneity.

Nevertheless, we believe that our work makes an important conceptual and technical contribution to the well-developed literature on school effects. Our results illustrate the feasibility and potential utility of a life-course orientation to the study of educational inputs and student outcomes. Although there is admittedly much work still to be done in this area, we are optimistic that trajectory-based assessments of school characteristics will provide the scholarly and policy communities with new and valuable information concerning the presence of school effects, the mechanisms through which these effects operate, and the members of the population for whom they are the most relevant.

ENDNOTES

[1] Just over one-quarter of the studies Hanushek (1997) considered employed a "value-added" approach, in which the specified model included information about individuals' prior achievement to "ameliorate any problems arising from missing data about past school and family factors" (p. 147). While this class of studies is clearly an improvement over other approaches, we hesitate to call it a substitute for directly observing school data for each year between kindergarten and high school graduation. Furthermore, it is not immediately obvious how such a design could be usefully applied in studies of other, non-achievement related outcomes, including educational and occupational attainments, earnings, and so forth.

[2] We are not the first to make this observation. Hanushek (2003), for instance, notes that "current schooling inputs will tend to be a very imperfect measure of the resources that went into producing ending achievement. This mismeasurement is strongest for any children who changed schools over their career (a sizable majority in the US) but also holds for students who do not move because of the heterogeneity of teachers within individual schools" (p. F77).

[3] Of the district records that we collected for the 1945 school year, for instance, 91.8 percent contained only a single elementary school. The exceptions, of course, include large urban districts like Madison and Milwaukee, which encompassed a number of neighborhood schools and satellite communities.

[4] Of course, this strategy does not protect against the possibility of temporary transfers from one district to another. For example, if a student attended primary school in Ripon until the fifth grade, then completed the sixth grade in Fond Du Lac, and then returned to Ripon for grades 7-12, our scheme would treat them as if they remained in Ripon for the duration of their primary and secondary years. However, in light of the low level of intra-state migration in Wisconsin

during the time period under consideration, we are not especially concerned about this possibility. According to our own tabulations using public-use microdata from the 1950 census (Ruggles, et al., 2008), for example, less than 2 percent of Wisconsin residents with children between the ages of 10-12—an age range that corresponds roughly to the ages of WLS cohort members in 1950— reported living in a different Wisconsin county during the preceding year.

[5] In some cases the microfiche from which the school district information was obtained was blurred or otherwise hard to read (due to illegible handwriting on the part of the district clerk, smudging, etc.), making it difficult to ascertain the appropriate value. Rather than deleting these districts, a very basic imputation procedure—based on values from the same district in adjacent years, as well as non-missing information from the year in question—was developed. Consider the district of Phillips, where the entry for total expenditures in 1951 was unreadable but the rest of the information (including teachers' salary, which makes up the bulk of district expenditures in any given year) was intact. We were able to impute the missing value by calculating the average ratio of teachers' salaries to expenses for 1950 and 1952 and then dividing the amount given under teachers' salaries in 1951 by the resulting ratio. If, for example, the average salary-expenses ratio in 1950 and 1952 was 0.80, and if total teachers' salary in Phillips in 1951 was $72,000, then the district's total expenses in 1951 would be set equal to $72,000/0.80 = $90,000. Fortunately, instances where this type of imputation was needed were quite infrequent: in total, there are 211 imputed values in the district-level data (out of a possible 193,623 values).

[6] Censoring and/or clustering about the minimum and maximum do not pose a problem for our measures of school characteristics and resources. As Nagin (2005:28) points out, however, the model outlined in Eq. (4) "can also be used for data which is measured on a continuous scale without censoring."

[7] In a supplementary analysis we compared deleted cases to those in our final sample. In terms of social background (father's occupational status and parents' years of schooling), family composition (number of siblings and intact family status), and geographic characteristics (farm versus non-farm), the two groups were statistically indistinguishable from one another. These results are available upon request.

[8] Since there is left censoring at 12 years of schooling, we cannot estimate the effect of one's school characteristic trajectory on their propensity to obtain a high school diploma, or, conversely, to drop out. Thus any observed effect of trajectory membership on post-secondary outcomes may also reflect, at least in part, the influence of trajectory membership on high school completion. Nevertheless, it should be noted that censoring on high school graduation in the WLS sample is less extreme than it would be had the sampling frame spanned all 50 states. Data from the 1960 census indicate that 23.6 percent of Wisconsin residents between the ages of 20-24 did not obtain a high school diploma, compared to 36.5 percent of individuals nationwide (Ruggles, et al., 2008). This places Wisconsin third among all states in terms of lowest high school dropout rate, behind only Nebraska and Iowa.

[9] Following convention (Nagin, 2005, pp. 88-92), we also calculated a number of other more informal measures of fit. These diagnostics, which are available from the authors upon request, suggested satisfactorily close fits for all of our preferred models. For example, the average posterior probability of group assignment for each class never falls below .90, a number that far exceeds the recommended cut-off of .70 (Nagin, 2005, p. 88).

[10] Groups 1 and 2 employ a quartic polynomial, while group 3 was fit using quadratic function in time. For the former two groups the quartic term was significant ($\alpha = .01$), suggesting that further

27

refinement of our specification of the trajectory forms was not necessary. For group 3, the quartic and cubic terms were not significant, and were thus removed from the model.

[11] We were able to confirm the presence of parallel trajectories using a Wald test, which indicated that the intercepts for the two trajectories differ significantly but coefficients on the higher-order terms do not. These tests were executed using the user-written SAS macro TRAJTEST (Jones and Nagin, 2007).

[12] Each of the three trajectories was specified to follow a quartic function in time.

[13] It is not surprising, then, that Wald tests easily rejected the hypothesis of equivalent higher-order terms ($\alpha = .01$), indicating that the trajectories are not parallel.

[14] This procedure is not immune to misclassification (Roeder, et al., 1999). Individuals whose posterior probability places them on the margins of two or more groups, for example, are assigned to their most likely trajectory despite the presence of significant uncertainty. In order to alleviate this concern we tried weighting each case according to its posterior probability of group membership. The results were substantively identical. For a more detailed discussion of this weighting technique see Nagin, 2005.

[15] We ran a parallel set of models in which trajectories were parameterized using individuals' posterior probabilities of group assignment (e.g., Eq. (5)). This approach, which is less prone to misclassification error, produced no material differences.

[16] Only institutions classified as "Most Competitive" or "Highly Competitive" according to the Barron's *Profiles* were considered elite. Of these schools, Lawrence University enrolled the most WLS respondents, followed by Northwestern University. Several respondents attended liberal arts schools such as Carleton College, Dartmouth College, and Wellesley College, while several others enrolled in large research universities like Cornell, Duke, and the University of Chicago.

[17] In total, there were 190 cases where parental income was missing; 15 cases where father's occupational status was missing; and 10 cases where there was no information on father's age. Rather than dropping these individuals, we used hot-deck imputation methods to impute missing values (Mander and Clayton, 2000). In auxiliary analyses (not shown), we included measures of students' intelligence (as measured in 1957 on the Henmon-Nelson test) and senior year class rank. Because these variables did little to alter our substantive conclusions, and since they are each potentially endogenous, we elected to omit them in the models that we report above.

[18] Our choice of single-year indicators was largely arbitrary. In supplementary analyses (not shown) we considered a number of other possible time points, as well as averages generated from multiple years of data. The results from these analyses, which are available from the authors upon request, mirrored those reported above.

[19] With 5 trajectory groups there are [5 x 2 – 1 = 9] 9 potential contrasts per model. It is important to note that a number of the omitted contrasts yielded non-significant coefficients. Nevertheless, the contrasts that were significant were typically large in magnitude and with the expected sign. For example, conditional on having ever enrolled, members of the "high-flat" trajectory (group 5) were less than half as likely [$e^{-.68} – 1 = -0.49$] to obtain a BA than members of the "low-flat" trajectory (group 1), all else being equal. Results for the full set of contrasts are available from the authors upon request.

REFERENCES

Alexander, K.L., Eckland, B.K., 1977. High School Context and College Selectivity: Institutional Constraints in Educational Stratification. Social Forces 56**,** 166-188.

Alexander, K.L., Griffin, L.J., 1976. School District Effects on Academic Achievement: A Reconsideration. American Sociological Review 41**,** 144-152.

Altonji, J.G., Dunn, T.A., 1996. Using Siblings to Estimate the Effect of School Quality on Wages. The Review of Economics and Statistics 78**,** 665-671.

Alwin, D.F., Otto, L.B., 1977. High School Context Effects on Aspirations. Sociology of Education 50**,** 259-273.

Behrman, J.R., Birdsall, N., 1983. The Quality of Schooling: Quantity Alone is Misleading. The American Economic Review 73**,** 928-946.

Betts, J.R., 1995. Does School Quality Matter? Evidence from the National Longitudinal Survey of Youth. The Review of Economics and Statistics 77**,** 231-250.

Brand, J.E., Halaby, C.N., 2006. Regression and Matching Estimates of the Effects of Elite College Attendance on Educational and Career Achievement. Social Science Research 35**,** 749-770.

Card, D., Krueger, A.B., 1992. Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States. The Journal of Political Economy 100**,** 1-40.

Card, D., Krueger, A.B., 1996. Labor Market Effects of School Quality: Theory and Evidence. In: Burtless, G., (Ed., Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success. Brookings Institute, Washington, D.C.

Coleman, J.S., Campbell, E.Q., Hobson, C.J., McPartland, J., Mood, A.M., Weinfeld, F.D., York, R.L., 1966. Equality of Educational Opportunity. Department of Health, Education, and Welfare, Washington, D.C.

Condron, D.J., Roscigno, V.J., 2003. Disparities within: Unequal Spending and Achievement in an Urban School District. Sociology of Education 76**,** 18-36.

D'Unger, A.V., Land, K.C., McCall, P.L., Nagin, D.S., 1998. How Many Latent Classes of Delinquent/Criminal Careers? Results from Mixed Poisson Regression Analyses of the London, Philadelphia, and Racine Cohorts Studies. American Journal of Sociology 103**,** 1593-1630.

Dearden, L., Ferri, J., Meghir, C., 2002. The Effect of School Quality on Educational Attainment and Wages. The Review of Economics and Statistics 84**,** 1-20.

Duncan, G.J., Brooks-Gunn, J., 1997. Income Effects Across the Life-Span: Integration and Interpretation. In: Duncan, G. J., and Brooks-Gunn, J., (Eds.), Consequences of Growing Up Poor. Russell Sage Foundation, New York.

Elder, G., Jr., 1985. Perspectives on the Life Course. In: Elder, G., (Ed., Life Course Dynamics. Cornell University Press, Ithaca.

Elder, G., Jr., 1998. The Life Course as Developmental theory. Child Development 69, 1-12.

Elder, G., Jr., Johnson, M.K., Crosnoe, R., 2003. The Emergence and Development of Life Course Theory. In: Mortimer, J. T., and Shanahan, M. J., (Eds.), Handbook of the Life Course. Kluwer Academic/Plenum Publishers, New York, pp. 3-19.

Elder, G., Jr., Shanahan, M.J., 2007. The Life Course and Human Development. In: Lerner, R., (Ed., The Handbook of Child Psychology. Wiley, New York, pp. 665-715.

Elliott, M., 1998. School Finance and Opportunities to Learn: Does Money Well Spent Enhance Students' Achievement? Sociology of Education 71, 223-245.

Entwisle, D.R., Alexander, K.L., Olson, L.S., 2003. The First-Grade Transition in Life Course Perspective. In: Mortimer, J. T., and Shanahan, M. J., (Eds.), Handbook of the Lifecourse. Kluwer Academic/Plenum Publishers, New York.

Entwisle, D.R., Hayduk, L.A., 1988. Lasting Effects of Elementary School. Sociology of Education 61, 147-159.

George, L.K., 1999. Life-Course Perspectives on Mental Health. In: Aneshensel, C. S., and Phelan, J. C., (Eds.), Handbook of the Sociology of Mental Health. Kluwer Academic/Plenum Publishers, New York, pp. 565-583.

Greenwald, R., Hedges, L.V., Laine, R.D., 1996. The Effect of School Resources on Student Achievement. Review of Educational Research 66, 361-396.

Griffin, L.J., Alexander, K.L., 1978. Schooling and Socioeconomic Attainments: High School and College Influences. The American Journal of Sociology 84, 319-347.

Grogger, J., 1996. Does School Quality Explain the Recent Black/White Wage Trend? Journal of Labor Economics 14, 231-253.

Grogger, J., 1996. School Expenditures and Post-Schooling Earnings: Evidence from High School and Beyond. The Review of Economics and Statistics 78, 628-637.

Hanushek, E.A., 1986. The Economics of Schooling: Production and Efficiency in Public Schools. Journal of Economic Literature 24, 1141-1177.

Hanushek, E.A., 1989. The Impact of Differential Expenditures on School Performance. Educational Researcher 18, 45-51.

Hanushek, E.A., 1997. Assessing the Effects of School Resources on Student Performance: An Update. Educational Evaluation and Policy Analysis 19**,** 141-164.

Hanushek, E.A., 2003. The Failure of Input-Based Schooling Policites. Economic Journal 113**,** F64-F98.

Hanushek, E.A., Rivkin, S.G., Taylor, L.L., 1996. Aggregation and the Estimated Effects of School Resources. The Review of Economics and Statistics 78**,** 611-627.

Haviland, A.M., Nagin, D.S., 2005. Causal Inferences with Group Based Trajectory Models. Psychometrika 70**,** 557-578.

Heckman, J.J., Layne-Farrar, A., Todd, P., 1996. Does Measured School Quality Really Matter? An Examination of the Earnings-Quality Relationship. In: Burtless, G., (Ed., Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success. Brookings Institute, Washington, D.C.

Hedges, L., Laine, R., Greenwald, R., 1994. Does Money Matter? A Meta-Analysis of Studies of the Effect of Differential School Inputs on Student Outcomes. Educational Researcher 23**,** 5-14.

Heyneman, S.P., Loxley, W.A., 1983. The Effect of Primary-School Quality on Academic Achievement Across Twenty-nine High- and Low-Income Countries. The American Journal of Sociology 88**,** 1162-1194.

Hill, L.D., 2008. School Strategies and the "College-Linking" Process: Resoncsidering the Effects of High Schools on College Enrollment. Sociology of Education 81**,** 53-76.

Jeffreys, H., 1961. Theory of Probability. Oxford University Press, Oxford.

Johnson, G.E., Stafford, F.P., 1973. Social Returns to Quantity and Quality of Schooling. The Journal of Human Resources 8**,** 139-155.

Jones, B.L., Nagin, D.S., 2007. Advances in Group-Based Trajectory Modeling and a SAS Procedure for Estimating Them. Sociological Methods and Research 35**,** 542-571.

Jones, B.L., Nagin, D.S., Roeder, K., 2001. A SAS Procedure Based on Mixture Models for Estimating Developmental Trajectories. Sociological Methods and Research 29**,** 374-393.

Kass, R.E., Raftery, A.E., 1995. Bayes Factors. Journal of the American Statistical Association 90**,** 773-795.

Kass, R.E., Wasserman, L., 1995. A Reference Bayesian Test for Nested Hypotheses and its Relationship to the Schwarz Criterion. Journal of the American Statistical Association 90**,** 928-934.

Kerckhoff, A.C., Campbell, R.T., Trott, J.M., 1982. Dimensions of Educational and Occupational Attainment in Great Britain. American Sociological Review 47**,** 347-364.

Laub, J.H., Nagin, D.S., Sampson, R.J., 1998. Trajectories of Change in Criminal Offending: Good Marriages and the Desistance Process. American Sociological Review 63**,** 225-238.

Lloyd, C.B., Tawila, S.E., Clark, W.H., Mensch, B.S., 2003. The Impact of Educational Quality on School Exit in Egypt. Comparative Education Review 47**,** 444-467.

Loughran, T., Nagin, D.S., 2006. Finite Sample Effects in Group-Based Trajectory Models. Sociological Methods and Research 35**,** 250-278.

Mander, A., Clayton, D., 2000. Update to Hotdeck Imputation. Stata Technical Bulletin 9**,** 199.

Mayer, S.E., 1997. What Money Can't Buy: Family Income and Children's Life Chances. Harvard University Press, Cambridge.

Meyer, J.W., 1970. High School Effects on College Intentions. The American Journal of Sociology 76**,** 59-70.

Moen, P., Dempster-McClain, D., Williams Jr., R.M., 1992. Successful Aging: A Life-Course Perspective on Women's Multiple Roles and Health. American Journal of Sociology 97**,** 1612-1638.

Murnane, R.J., 1991. Interpreting the Evidence on 'Does Money Matter?' Havard Journal on Legislation 28**,** 457-464.

Mustillo, S., Worthman, C., Erkanli, A., Keeler, G., Angold, A., Costelo, E.J., 2003. Obesity and Psychiatric Disorder: Developmental Trajectories. Pediatricts 111**,** 851-859.

Múthen, B., 2003. Statistical and Substantive Checking in Growth Mixture Modeling: Comment on Bauer and Curran (2003). Psychological Methods 8**,** 369-377.

Nagin, D.S., 1999. Analyzing Developmental Trajectories: A Semi-Parametric, Group-Based Approach. Psychological Methods 4**,** 139-177.

Nagin, D.S., 1999. Analyzing Developmental Trajectories: A Semiparametric, Group-Based Approach. Psychological Methods 4**,** 139-157.

Nagin, D.S., 2005. Group-Based Modeling of Development. Harvard University Press, Cambridge, MA.

Nagin, D.S., Land, K.C., 1993. Age, Criminal Careers, and Population Heterogeneity: Specification and Estimation of a Nonparametric, Mixed Poisson Model. Criminology 31**,** 327-362.

Nagin, D.S., Pagani, L., Tremblay, R.E., Vitaro, F., 2003. Life Course Turning Points: The Effect of Grade Retention on Physical Aggression. Development and Psychopathology 15**,** 343-361.

Nagin, D.S., Tremblay, R., 2005. Developmental Trajectory Groups: Fact or Useful Statistical Fiction? Criminology 43**,** 873-904.

Nagin, D.S., Tremblay, R.E., 1999. Trajectories of Boys' Physical Aggression, Opposition, and Hyperactivity on the Path to Physically Violent and Nonviolent Juvenile Delinquency. Child Development 70**,** 1181-1196.

Pallas, A.M., 2003. Educational Transitions, Trajectories, and Pathways. In: Mortimer, J. T., and Shanahan, M. J., (Eds.), Handbook of the Life Course. Kluwer Academic, New York, pp. 165-184.

Pavalko, E.K., Elder, G.H., Jr., Clipp, E.C., 1993. Worklives and Longevity: Insights from a Life Course Perspective. Journal of Health and Social Behavior 34**,** 363-380.

Raftery, A.E., 1995. Bayesian Model Selection in Social Research. Sociological Methodology 25**,** 111-163.

Roeder, K., Lynch, K.G., Nagin, D.S., 1999. Modeling Uncertainty in Latent Class Membership: A Case Study in Criminology. Journal of the American Statistical Association 94**,** 766-776.

Rose, H., Betts, J.R., 2004. The Effect of High School Courses on Earnings. The Review of Economics and Statistics 86**,** 497-513.

Ross, C.E., Mirowsky, J., 1999. Refining the Association between Education and Health: The Effects of Quantity, Credential, and Selectivity. Demography 36**,** 445-460.

Ruggles, S., Sobek, M., Alexander, T., Fitch, C.A., Goeken, R., Hall, P.K., King, M., Ronnander, C., 2008. Integrated Public Use Microdata Series: Version 4.0 [Machine-readable database]. Minnesota Population Center.

Sampson, R.J., Laub, J.H., 1995. Understanding Variability in Lives through Time: Contributions of Life-Course Criminology. Studies on Crime and Crime Prevention 4**,** 143-158.

Sander, W., 1993. Expenditures and Student Achievement in Illinois: New Evidence. Journal of Political Economics 52**,** 403-416.

Sorenson, A.B., Hallinan, M.T., 1977. A Reconceptualization of School Effects. Sociology of Education 50**,** 273-289.

Wagmiller, R.L., Lennon, M.C., Kuang, L., 2008. Parental Health and Children's Economic Well-Being. Journal of Health and Social Behavior 49**,** 37-55.

Wagmiller, R.L., Lennon, M.C., Kuang, L., Alberti, P.M., Aber, J.L., 2006. The Dynamics of Economic Disadvantage and Children's Life Chances. American Sociological Review 71**,** 847-866.

Wenglinsky, H., 1997. How Money Matters: The Effect of School District Spending on Academic Achievement. Sociology of Education 70**,** 221-237.

Wickrama, K.A.S., Lorenz, F.O., Conger, R.D., Elder, G.H., Jr., 1997. Marital Quality and Physical Illness: A Latent Growth Curve Analysis. Journal of Marriage and the Family 59**,** 143-155.

Wu, L.L., 1996. Effects of Family Instability, Income, and Income Instability on the Risk of a Premarital Birth. American Sociological Review 61**,** 386-406.

Wu, L.L., Martinson, B.C., 1993. Family Structure and the Risk of a Premarital Birth. American Sociological Review 58**,** 210-232.

Table 1. Model Selection

Model and BIC	Number of groups					
	1	2	3	4	5	6
Per pupil expenditures						
BIC	38556.7	38038.4	**37921.4**	38540.6	—	—
Teachers' schooling						
BIC	24467.0	23282.0	**22879.9**	22890.8	—	—
Pupil-teacher ratio						
BIC	15893.0	15399.1	15244.1	15105.5	**15009.7**	15007.5

Note : BIC is calculated as $-2L + k *\ln(N)$, where L is the value of the model's maximized likelihood, N is the sample size, and k is the number of non-redundant parameters. Groups were added iteratively until further improvement could not be achieved. Preferred models are shown in bold. See text for further details.

Table 2. School quality trajectory group profiles, by selected sociodemographic characteristics ($N = 1{,}640$)

School quality indicator and trajectory group	Background characteristics													
	Father's occupational status (SEI)		Father's years of schooling		ln(family income/100)		Catholic (Catholic = 1)		Number of siblings		Two-parent household (Intact = 1)		Farm residence (Farm = 1)	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Per pupil expenditures														
Group 1: Low increasing	29.7	(21.7)	9.4	(2.9)	3.8	(0.7)	.21	(.40)	3.9	(2.1)	.91	(.28)	.31	(.46)
Group 2: High increasing	40.6	(26.2)	10.8	(3.6)	4.1	(0.7)	.18	(.39)	3.4	(1.9)	.89	(.31)	.09	(.29)
Group 3: Medium flat	39.5	(23.7)	10.2	(3.5)	4.1	(0.7)	.16	(.37)	3.3	(1.8)	.92	(.27)	.08	(.27)
p-value from *F*-test or χ^2-test	*.00*		*.00*		*.00*		*.11*		*.00*		*.34*		*.00*	
Teachers' schooling														
Group 1: Low	24.4	(19.2)	8.8	(2.8)	3.5	(0.7)	.30	(.46)	4.3	(2.1)	.88	(.33)	.45	(.50)
Group 2: Medium	30.6	(22.0)	9.4	(3.0)	3.8	(0.7)	.21	(.41)	3.9	(2.1)	.92	(.27)	.31	(.46)
Group 3: High	39.1	(24.2)	10.3	(3.5)	4.1	(0.7)	.15	(.36)	3.3	(1.8)	.92	(.27)	.09	(.28)
p-value from *F*-test or χ^2-test	*.00*		*.00*		*.00*		*.00*		*.00*		*.15*		*.00*	
Pupil-teacher ratio														
Group 1: Low flat	33.9	(26.5)	10.0	(3.6)	3.8	(0.7)	.27	(.44)	4.0	(2.1)	.93	(.25)	.35	(.48)
Group 2: Medium	36.3	(24.0)	10.0	(3.4)	4.0	(0.7)	.16	(.37)	3.5	(1.9)	.92	(.27)	.17	(.38)
Group 3: Low declining	27.8	(21.6)	9.5	(2.8)	3.7	(0.7)	.34	(.48)	4.1	(2.1)	.88	(.32)	.29	(.46)
Group 4: Sharply declining	31.5	(22.3)	9.7	(3.2)	3.7	(0.8)	.17	(.38)	3.9	(2.2)	.92	(.27)	.34	(.47)
Group 5: High declining	34.4	(22.3)	9.7	(3.1)	4.0	(0.6)	.19	(.39)	3.4	(1.8)	.88	(.32)	.13	(.33)
p-value from *F*-test or χ^2-test	*.00*		*.39*		*.00*		*.00*		*.00*		*.12*		*.00*	
Entire sample	34.7	(23.6)	9.9	(3.3)	3.9	(0.7)	.19	(.39)	3.6	(2.0)	.91	(.28)	.20	(.40)

Note: The *p*-values reported beneath each set of tabulations correspond to tests for association between the relevant column and row variables. The *p*-values in columns with continuous variables were derived from *F*-tests; the *p*-values in columns with categorical variables were derived from a χ^2-tests. In total, there were 190 cases where parental income was missing; 15 cases where father's occupational status was missing; and 10 cases where there was no information on head's age. Rather than dropping these individuals, we used hot-deck imputation methods to impute missing values. See text for further details.

Table 3. Descriptions of exogenous controls and post-secondary educational outcomes

Variable	Descriptions
Outcomes	
College enrollment	Dummy variable (ever enrolled = 1)
College selectivity given ever enrolled	Dummy variable (attended selective institution = 1)
Obtained at least a BA given ever enrolled	Dummy variable (Bachelor's or higher = 1)
Measures of social background	
Average annual parental income (1957-1960)	Top coded at $99,800
Father's occupational status	1957 occupation; Duncan SEI 0-96
Father's education	Years of schooling completed
Mother's education	Years of schooling completed
Demographic variables	
Age of householder	Age of householder in 1957
Urban/rural status	Dummy variable (rural = 1)
Parents' marital status	Dummy variable (intact for majority of youth = 1)
Number of siblings	Number of siblings in 1957
Catholic	Dummy variable (Catholic = 1)

Note: In total, there were 190 cases where parental income was missing; 15 cases where father's occupational status was missing; and 10 cases where there was no information on head's age. Rather than dropping these individuals, we used hot-deck imputation methods to impute missing values. See text for further details.
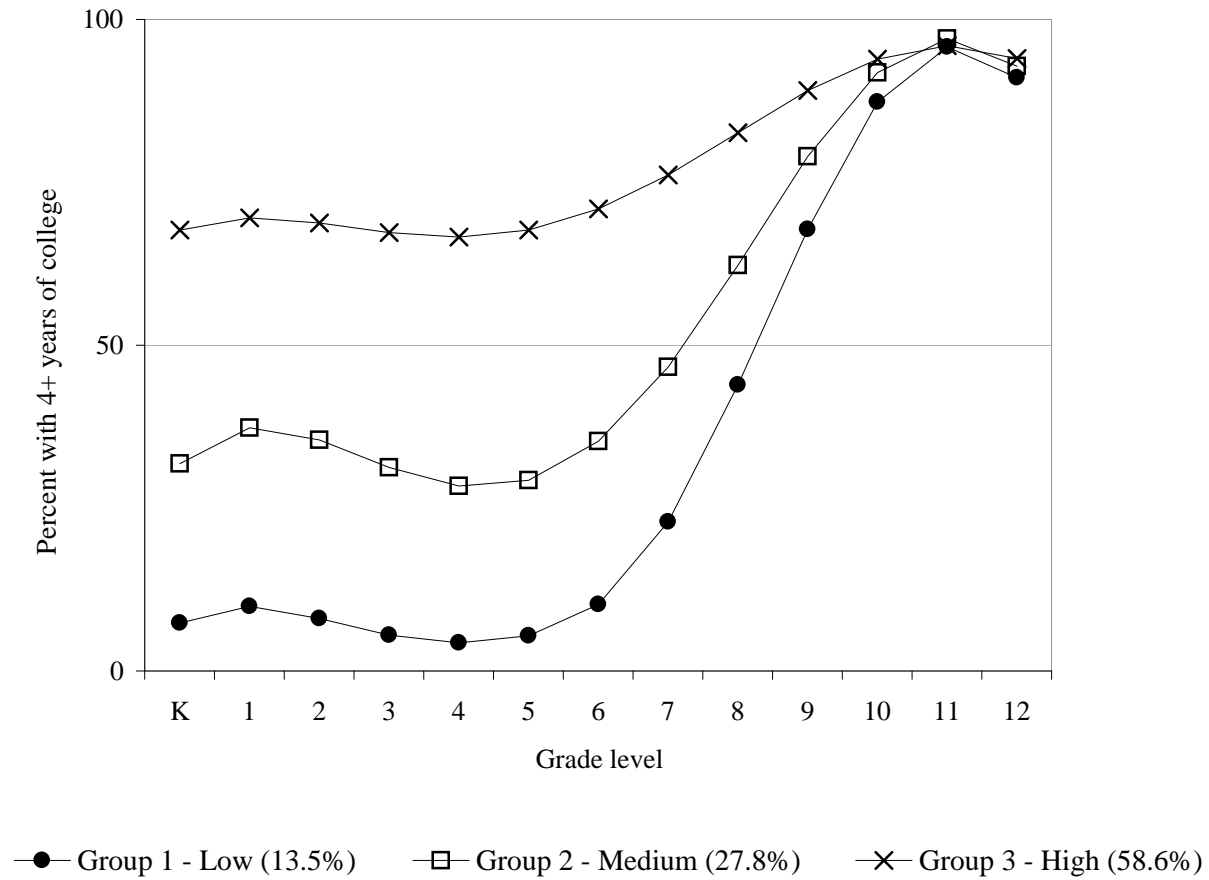
Figure 1. Trajectories of per pupil expenditures
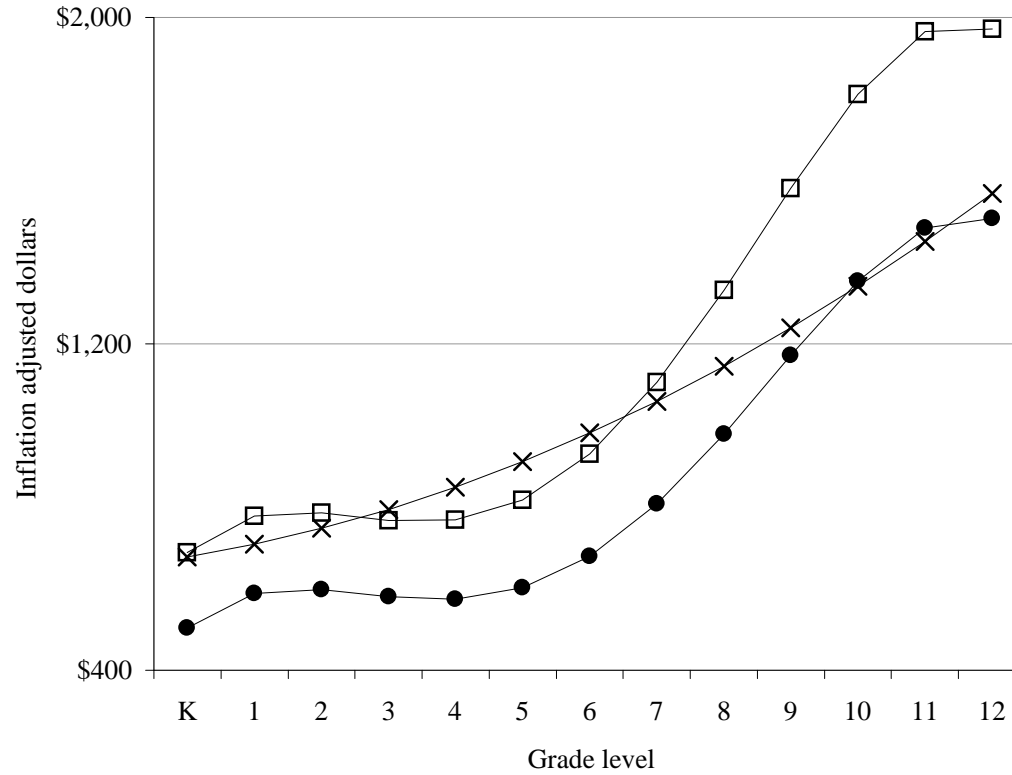
Note : The appropriate number of trajectories was determined according to the BIC, as per Table 1. All values represent predicted values, as opposed to group-specific averages. Please refer to the text for additional details.

Figure 2. Trajectories of teachers' schooling

Note: The appropriate number of trajectories was determined according to the BIC, as per Table 1. All values represent predicted values, as opposed to group-specific averages. Please refer to the text for additional details.
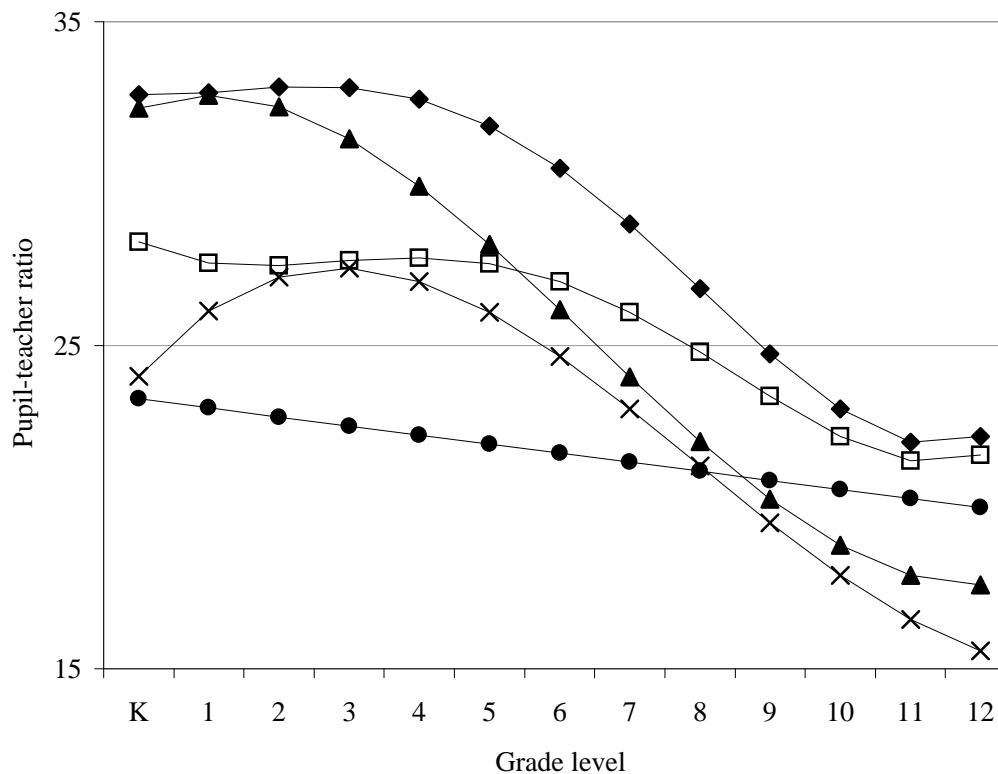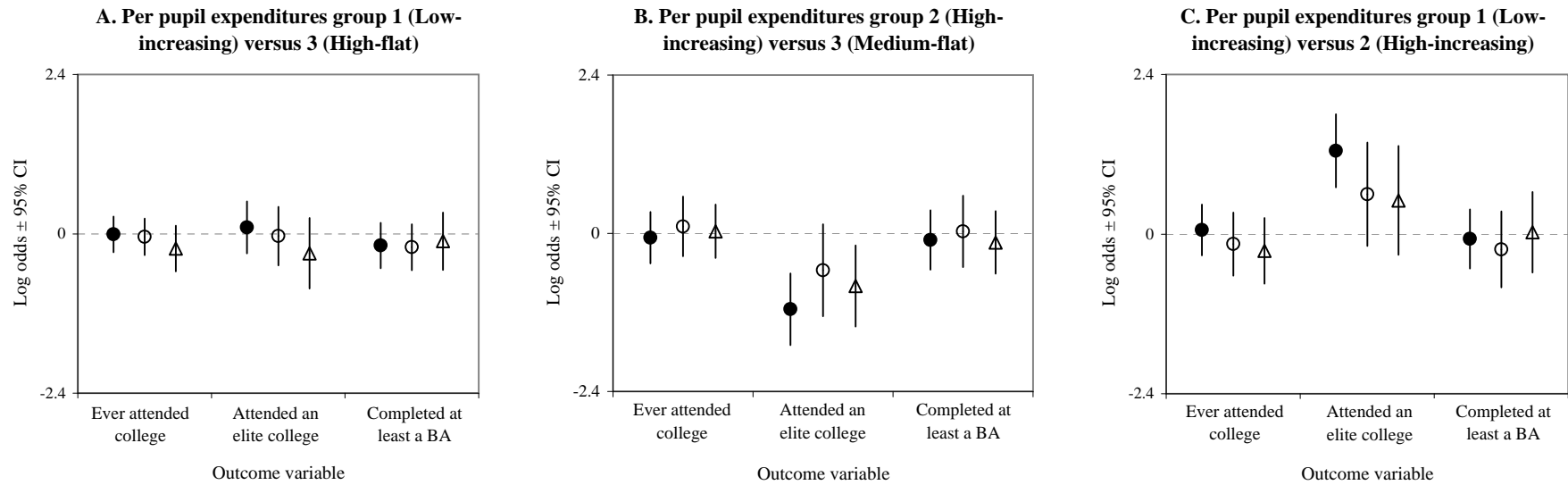
Figure 3. Trajectories of pupil-teacher ratio

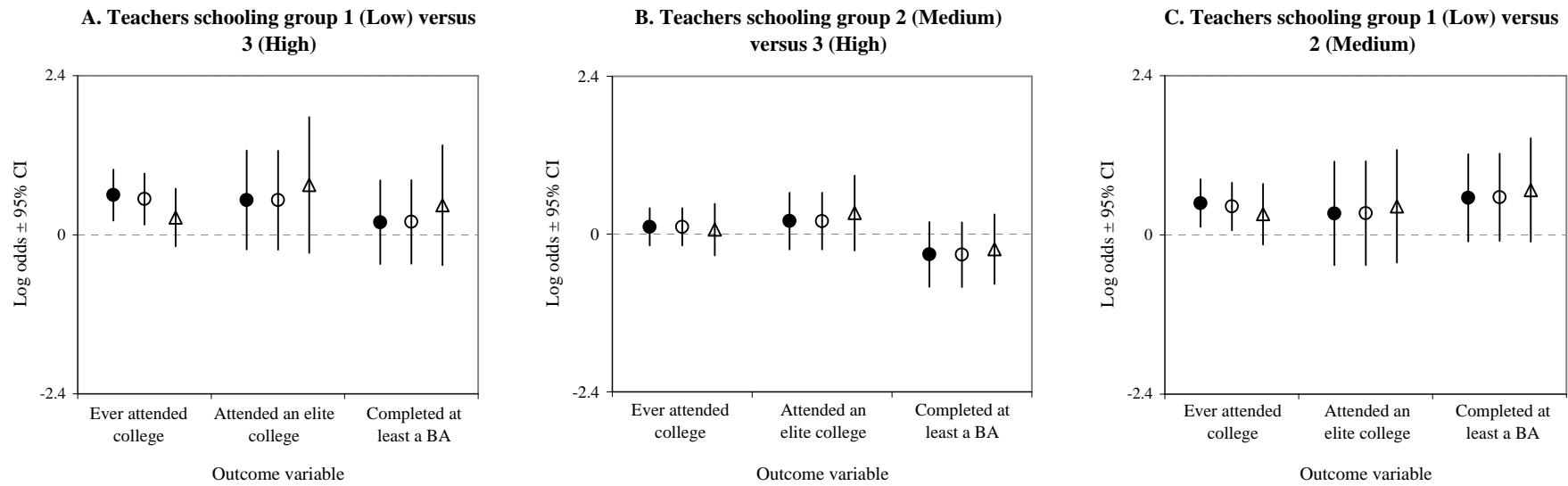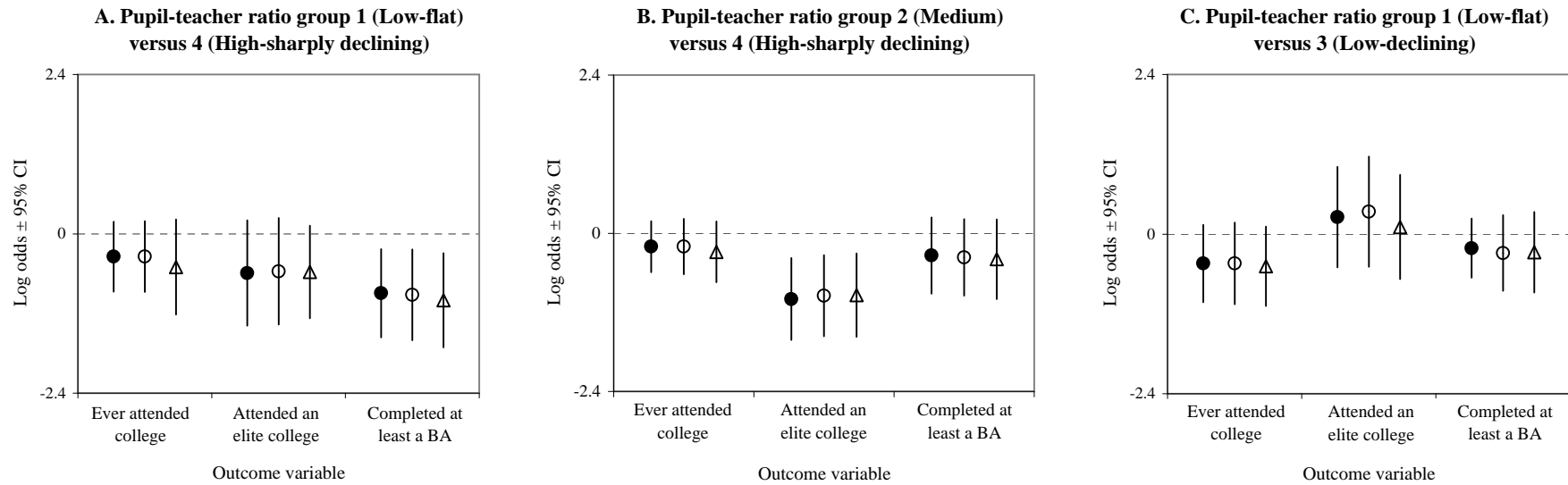

Note: The appropriate number of trajectories was determined according to the BIC, as per Table 1. All values represent predicted values, as opposed to group-specific averages. Please refer to the text for additional details.

A. Per pupil expenditures group 1 (Low-increasing) versus 3 (High-flat)

B. Per pupil expenditures group 2 (High-increasing) versus 3 (Medium-flat)

C. Per pupil expenditures group 1 (Low-increasing) versus 2 (High-increasing)

Y-axis (all panels): Log odds ± 95% CI — 2.4, 0, -2.4

X-axis (all panels): Ever attended college · Attended an elite college · Completed at least a BA — Outcome variable

Figure 4: The first group listed in each panel heading is the reference category for the given trajectory group contrast. The shaded circle indicates the point estimate associated with the specified contrast in a model that only includes trajectory group and individual-level controls. The hollow circle represents the same point estimate after including a single-year measure of teachers' schooling during respondents' senior year of high school. Finally, the hollow triangle denotes the point estimate for the contrast after including a single-year measure of teachers' schooling, as observered when respondents were in first grade. The latter two outcomes—attended an elite college and completed at least a BA—are both conditional on the respondent ever attending a post-secondary institution. All models include controls for respondents' gender, father's education, mother's education, father's occupational status (Duncan SEI score), father's age, parental income, sibship size, intact family status, urban/rural status, and religion. In total, there were 190 cases where parental income was missing; 15 cases where father's occupational status was missing; and 10 cases where there was no information on father's age. Rather than dropping these individuals, we used hot-deck imputation methods to impute missing values.

A. Teachers schooling group 1 (Low) versus 3 (High)

B. Teachers schooling group 2 (Medium) versus 3 (High)

C. Teachers schooling group 1 (Low) versus 2 (Medium)

Figure 5: The first group listed in each panel heading is the reference category for the given trajectory group contrast. The shaded circle indicates the point estimate associated with the specified contrast in a model that only includes trajectory group and individual-level controls. The hollow circle represents the same point estimate after including a single-year measure of teachers' schooling during respondents' senior year of high school. Finally, the hollow triangle denotes the point estimate for the contrast after including a single-year measure of teachers' schooling, as observerved when respondents were in first grade. The latter two outcomes—attended an elite college and completed at least a BA—are both conditional on the respondent ever attending a post-secondary institution. All models include controls for respondents' gender, father's education, mother's education, father's occupational status (Duncan SEI score), father's age, parental income, sibship size, intact family status, urban/rural status, and religion. In total, there were 190 cases where parental income was missing; 15 cases where father's occupational status was missing; and 10 cases where there was no information on father's age. Rather than dropping these individuals, we used hot-deck imputation methods to impute missing values.

A. Pupil-teacher ratio group 1 (Low-flat) versus 4 (High-sharply declining)

B. Pupil-teacher ratio group 2 (Medium) versus 4 (High-sharply declining)

C. Pupil-teacher ratio group 1 (Low-flat) versus 3 (Low-declining)

Figure 6: The first group listed in each panel heading is the reference category for the given trajectory group contrast. The shaded circle indicates the point estimate associated with the specified contrast in a model that only includes trajectory group and individual-level controls. The hollow circle represents the same point estimate after including a single-year measure of teachers' schooling during respondents' senior year of high school. Finally, the hollow triangle denotes the point estimate for the contrast after including a single-year measure of teachers' schooling, as observered when respondents were in first grade. The latter two outcomes—attended an elite college and completed at least a BA—are both conditional on the respondent ever attending a post-secondary institution. All models include controls for respondents' gender, father's education, mother's education, father's occupational status (Duncan SEI score), father's age, parental income, sibship size, intact family status, urban/rural status, and religion. In total, there were 190 cases where parental income was missing; 15 cases where father's occupational status was missing; and 10 cases where there was no information on father's age. Rather than dropping these individuals, we used hot-deck imputation methods to impute missing values.