Contagion, Relative Deprivation, or Selection?
The Contextual Effects of Peer Poverty on Student Test Score Growth

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Abstract

Social scientists and policymakers generally share the widely held belief that impoverished contexts have harmful effects on children. Disentangling the influence of the effects of individual and family background from the effects of context, however, is conceptually and methodologically complex, so the hypothesized existence of contextual effects remains open to scholarly inquiry. This study examines the effects of classroom peer poverty rates on student test score growth on the universe of children in grades three through eight in the state of North Carolina from 2001 to 2006. Multilevel growth models produce a negative effect of peer poverty on initial status, but no difference in test score growth trajectories, which raises doubts about whether the effect of peer poverty on test score achievement is causal. A student fixed effects model also fails to produce differences in test score trajectories and the difference in initial status shrinks to non-significance, which suggests that initial differences are driven by omitted student and family background factors rather than by peer poverty. Exploiting the fact that peer poverty changes most when students change schools, we examine the effects of extreme changes in peer poverty from structural (policy induced) and non-structural (family induced) school mobility. With this approach, we find strong support for relative deprivation and weaker support for contagion explanations, with non-poor students harmed by large decreases in peer poverty from both structural and non-structural school mobility and poor students harmed only by large increases in peer poverty from non-structural moves in middle school.

Scholars have spent decades researching and debating the influence of neighborhood and school context on academic achievement, aspirations and attitudes (Alexander and Eckland 1975; Crosnoe 2009; Felmlee and Eder 1983; Rumberger and Willms 1992; Wilson 1959). The scholarly consensus is that high SES neighborhoods and schools positively affect individual academic outcomes (Brooks-Gunn et al. 1993; Entwisle, Alexander and Olson 1994; Willms 1986), whereas high poverty neighborhoods and schools negatively affect academic outcomes (Crane 1991; Harding 2003; South, Baumer and Lutz 2003). For example, Coleman and colleagues, in their seminal Equality of Educational Opportunity report, argued that peer effects were strong predictors of academic achievement: “the social composition of the student body is more highly related to achievement, independent of the student’s own social background, than is any other school factor” (Coleman et al. 1966: 325). Social science evidence on contextual effects has informed educational policy in the United States, which for the past four decades has sought to mix students by racial background and, more recently, by poverty status. The relevance of contextual effects research is demonstrated by the prominent role such research played in the recent social science statement submitted as an amicus curiae brief in a Supreme Court case about school desegregation.[1]

Despite the scholarly consensus, very little contextual effects evidence shows that 1) context affects *changes* in outcomes rather than simply the *level* of outcomes at one point in time, and that 2) context affects outcomes when factors predicting selection of context are statistically controlled. Selection of context based on unobserved or mismeasured background characteristics is an often noted, and rarely addressed, problem in contextual effect research. Selection bias can give rise to what Hauser (1970) termed the "contextual fallacy": “…the

[1] Statement of American Social Scientists of Research on School Desegregation to the U.S. Supreme Court in "Parents v. Seattle School District" and "Meredith v. Jefferson County" Urban Review. V. 40. No 1.

contextual method rests on the arbitrary identification of residual group differences in the dependent variable with correlated aspects of group composition on an independent variable…The only way to eliminate such correlations is to assign individuals randomly to groups, and this is impossible with observational data" (p. 660). Social experiments such as the Moving to Opportunity program raise important doubts about the effect of changes in school and neighborhood context and academic achievement (Kling, Liebman and Katz 2007; Orr et al. 2003; Sanbonmatsu et al. 2006). Moreover, recent work in sociology (Crosnoe 2009; Harding 2003) and in economics (Hanushek, Kain and Rivkin 2009; Hoxby and Weingarth 2005; Solon, Page and Duncan 2000) attempts to reduce bias in contextual effects through propensity score matching and weighting, comparison of sibling and neighbor correlations, fixed effects, and natural experiments. Some of this recent work raises important questions about whether causal inferences about contextual effects are warranted (Mouw 2006).

This study uses a multiple cohort panel design to estimate the effect of peer poverty rates on elementary school students' test score initial status and growth. Our panel includes interval metric and vertically equated mathematics test scores and variation across time in classroom-level peer poverty rates from the universe of children in grades three through eight in the state of North Carolina from 2001 to 2006. The study contributes to contextual effects research by carefully specifying and accounting for bias from omitted and mismeasured time-invariant student and family background characteristics. We argue that standard estimates of peer poverty effects ignore the endogenous self-selection of both neighborhood and school context. To address this problem, we use student fixed effects to remove between-student variation. This approach controls for time-invariant unmeasured and mismeasured aspects of student and family background that may predict both family choice of neighborhood and school and test score achievement. We then examine whether large upward or downward changes in peer poverty due to school mobility alter test score achievement trajectories of both poor and non-poor students. Our approach reveals a nuanced view of the contextual effect of peer poverty on student achievement. We examine the effects of changes in context at key transition points in child development and contrasts in student experiences as they progress through classrooms through their early educational careers. Our work raises doubts about whether impoverished contexts themselves harm academic achievement, showing instead changing to more affluent contexts has generally negative effects for non-poor students and no significant positive effects for poor students.

THEORY AND EVIDENCE ABOUT CONTEXTUAL EFFECTS

Jencks and Mayer (1990) posited three hypotheses to explain school and neighborhood effects in their seminal review: affluent neighbors are an advantage, affluent neighbors are a disadvantage, and affluent neighbors have no effects on student outcomes. The advantages of affluent neighbors and peers flow from three sources: 1) a *contagion* (or *epidemic*) model in which positive values, attitudes, and behaviors are spread from affluent peers through peer group norms, 2) a *collective socialization* model in which adults other than family members serve as positive role models, and 3) an *institutional* model that states additional resources are available due to the presence of affluent neighbors and peers, such as higher levels of parent involvement and better teaching (Harris 2010; Jencks and Mayer 1990; Willms 2010). Research also points to the inverse of the affluence effect: the epidemic or contagion model predicts a negative effect on aspirations and achievement because high-poverty peers “infect” other students with negative school values, attitudes, and behaviors, and students in high poverty schools also have reduced access to beneficial resources, such as high-quality teachers (Crane 1991; Harding 2003; Harris

2010; Jencks and Mayer 1990; South, Baumer and Lutz 2003). On the other hand, another line of research on school contextual effects argues that affluent peers and neighbors have negative effects on youth. Scholars posit that relative deprivation, sometimes referred to as the "frog pond effect," discourages and depresses the aspirations, achievement, and attainment of students in more affluent schools (Attewell 2001; Bachman and O'Malley 1986; Crosnoe 2009; Davis 1966; Marsh 1987; Marsh and Parker 1984).

On the other hand, peers may have no influence on individual outcomes. Youth may associate with peers like themselves no matter what the social context (Jencks and Mayer 1990; Mouw 2006). School policy, in fact, may facilitate social homophily through tracking (Gamoran 1987; Harris 2010; Kubitschek and Hallinan 1998; Lucas 1999). For example, poor families who move to more affluent neighborhoods may experience no change in access to affluent peers if schools place poor students in classes with predominantly poor peers. It may also be the case that both collective socialization and relative deprivation operate simultaneously, pulling student outcomes in opposite directions. Some contextual effects research has accounted for this possibility by including both school mean SES and school average ability in the same models (Alwin and Otto 1977; Meyer 1970), finding that these effects work in opposite directions to create a null combined effect. Finally, contextual effects of peer poverty and affluence may simply reflect what the literature refers to as self-selection bias or omitted variable bias (Evans, Oates and Schwab 1992; Hauser 1970; Leventhal and Brooks-Gunn 2000; Quigley and Raphael 2008). Important omitted and mismeasured family and student background characteristics may be causal determinants of why individuals are sorted into these contexts (neighborhoods and schools) and test score achievement. Controlling for these factors may greatly reduce the unadjusted difference in outcomes between students from high and low poverty contexts.

Cross-Sectional Designs of Contextual Effects

Cross-sectional contextual effects research finds a positive association between socially desirable youth outcomes and average school and neighborhood socioeconomic status (SES). For example, studies find positive effects of school mean parental education on standardized test scores (Entwisle, Alexander and Olson 1994) and 4-year college enrollment (Choi et al. 2008), positive effects of school mean SES on grades and attainment (Willms 1986), and negative effects of the school mean poverty rate on academic self-esteem, educational aspirations and expectations, and standardized tests scores (Battistich et al. 1995). Neighborhood effects research finds negative effects of high poverty neighborhoods on teenage pregnancy and high school drop-out rates (Crane 1991; Harding 2003), negative effects of early childhood neighborhood poverty on educational attainment measured in adulthood (Entwisle, Alexander and Olson 2005), and negative effects of neighborhood deprivation on educational attainment in Scotland (Garner and Raudenbush 1991). However, neighborhood affluence may be a stronger predictor of youth outcomes than neighborhood poverty. Low levels of neighborhood poverty have been associated with positive effects on educational attainment (Duncan 1994), positive effects on standardized test scores (Entwisle, Alexander and Olson 1994), positive effects on IQ, and negative effects on high school dropout rates (Brooks-Gunn et al. 1993).

Though it may be advantageous to associate with affluent neighbors and peers, high achieving peers may harm aspirations, grades, curricular placement, and other academic outcomes, especially if students must compete for scarce resources. For example, Davis (1966) investigated whether the theory of relative deprivation explained college student career and graduate school application decisions. His results indicate that school mean achievement may have a negative effect on career aspirations, suggesting that students in more competitive environments may remove themselves from contention for high status careers and graduate schools. A more recent study finds that students in elite public high schools suffer a competitive disadvantage in entering elite colleges due to the importance of class rank in the college admissions process (Attewell 2001). This disadvantage may produce an organizational adaptation to triage resources in favor of the top students. Therefore, students in high, but not the highest quantiles of class rank, may receive worse grades and take less advanced courses than they would if they had attended a less elite public high school (ibid). Other studies finding negative effects of school mean achievement include Meyer (1970), Alexander & Eckland (1975), Alwin & Otto (1977), and multiple papers by Marsh and colleagues (Marsh 1987; Marsh and Hau 2003; Marsh, Kong and Hau 2000; Marsh and Parker 1984).

It should be noted that not all contextual effects studies have found clear positive or negative results. Two studies find weak or null effects of school mean SES. Alexander and colleagues investigate the nature of school effects in an attempt to distinguish between the mechanisms of value climate, peer networks, and tracking (Alexander et al. 1979). The authors find that controlling for individual SES reduces the effect of school mean SES on college plans to near zero. Their conclusion is that "the school SES influences are shown to result to a considerable degree simply from SES differences in the kinds of students attending various schools" (235). Another study of high school students finds very minimal and mostly non-significant effects of school mean SES on a variety of test score outcomes (Gamoran 1987). In this case, the author controls for types of coursework and tracking variables and concludes that within-school differences in opportunities are more important than, and perhaps explanations for, school effects.

Cross-sectional represent the association of school context on outcomes at one point in time, which ignores the cumulative nature of students' educational development. A point-in-time study captures both the effect of prior educational experiences and student and family background and the effect of schooling in a focal year. Reviews of the literature note the importance of controlling for exogenous factors (i.e., those that do not depend on type of neighborhood/school) and call for more longitudinal designs (Duncan and Raudenbush 1999; Galster et al. 2007; Harris 2010; Jencks and Mayer 1990; Saporito and Sohoni 2007).

Longitudinal Research Designs of Contextual Effects

Two recent studies examine the effect of school SES composition on test score gain in high school using NELS, a nationally representative database. Using a multi-level growth model, Rumberger and Palardy (2005) find that the predictive power of school SES on student test score growth is as strong as family SES, and has particularly strong effects on science test score growth. The authors also find that the effect of school SES is explained by teacher expectations, the amount of homework students do, course taking, and student perceptions of school safety. Palardy (2008) uses a multilevel latent growth curve model and finds that, adjusting for student background characteristics, students in high SES schools enter high school with test scores 3.3 grade levels higher and adjusted learning growth rates 1.1 grade levels higher than students in low SES schools (p. 37). These studies represent a significant advance over prior research. They treat test score gains rather than levels as the outcome of interest, and they use multilevel modeling to compute parameter estimates and cluster corrected standard errors. Although both studies use an impressive array of control variables to adjust for *observable* differences in student populations that could confound the school SES effect, the designs of these studies do not permit ruling out bias from the sorting of students into schools based on *unobservables*.

Neighborhood Relocation Experiments

Although no experiment instituted to date allows for direct examination of school contextual effects, evaluations of two housing relocation programs, Gautreaux and Moving to Opportunity, provide further evidence about the impact of changes in context. The Gautreaux program moved approximately 7,000 black families in Chicago from public housing mostly to housing in suburban neighborhoods that were majority white (Rubinowitz and Rosenbam 2000). Families voluntarily chose to participate in the program and the study had no control group, which makes assessing the causal effect of relocation impossible. Evaluators have used as a comparison group families moving to poor urban areas due to limitations in housing availability (about 20% of participants). Researchers found a number of positive effects for the families that moved to the suburbs compared to this comparison group including better academic outcomes for children and better employment outcomes for mothers (Rubinowitz and Rosenbam 2000). Although the results from the Gatreaux program support the hypothesis that reducing neighborhood poverty has positive effects, “critics were not mollified because assignment to comparison groups was nonrandom” (Clampet-Lundquist and Massey 2008:110).

The Moving to Opportunity (MTO) experiment, which moved approximately 1,700 families in five large metropolitan cities from public housing to housing in census tracts with a poverty rate of less than 10% (Orr et al. 2003), improved upon the design of the Gatreaux program by randomly assigning participants to three groups: a control group that was not offered a voucher to move, a treatment group that was provided a Section 8 voucher and allowed to move without restrictions, and another treatment group that was provided a rental assistance voucher but allowed to move only to a census tract with less than 10% poverty. Early results indicated a number of benefits for the treatment groups including positive academic, behavioral, and health outcomes (see DeLuca and Dayton 2009 for a review of this research). However, later follow-up studies found that some of these positive results dissipated; in some dimensions individuals in the control group fared better than those in treatment (Kling, Ludwig and Katz 2005). Children in the treatment group showed no real academic improvement and were in only marginally better schools than before the switch (Orr et al. 2003; Sanbonmatsu et al. 2006). Recent debates on MTO's applicability to the discussion of generalized neighborhood effects and problems of lingering selection bias raise further questions about measuring and determining contextual effects (Clampet-Lundquist and Massey 2008; Ludwig et al. 2008; Sampson 2008). In particular, at least one study has discussed the potential problem of self-selection within the experiment because a sizable percentage of the families assigned to both treatment groups did not use their vouchers and these families were significantly different in a number of important dimensions from the families who did use their vouchers (Feins and Shroder 2005). Although families were randomly assigned to these three groups, those in the treatment groups were not required to move. Additionally, as Sampson (2008) points out, MTO is not a general test of neighborhood effects but rather an examination of the effect of changing neighborhood contexts in one direction on one dimension for a limited portion of the population who are extremely poor and overwhelmingly minority.

In summary, existing research on school contextual effects rests primarily on a base of cross-sectional designs of correlational evidence. Two studies employ longitudinal designs, but ignore the problem of unobserved heterogeneity. Housing relocation studies provide evidence about changes in neighborhood, which also involve changes in school context, but suffer from limitations of generalizability to non-poor and non-minority populations and leave unexamined the effects of increases in peer poverty. The present study makes a number of important improvements to existing research. First, we employ a longitudinal research design that takes into account unobserved heterogeneity. A large literature in economics and a growing literature in sociology (e.g. England, Allison and Wu 2007; Jacobs and Carmichael 2001; Jacobs and Tope 2007; Kocak and Carroll 2008; Mouw 2003; Schneiberg, King and Smith 2008) uses "fixed effects" methods to control for time-invariant unobserved heterogeneity.[2] These models, which require variation within units over time, remove confounding bias that can emerge from omitted observable, mismeasured, or unobservable time-invariant student or group characteristics (Allison 2009; Halaby 2004; Mouw 2006; Wooldridge 2003). In the present context, this technique accounts for important student-level confounders such as low birth weight, early childhood education, and genetic factors, as well as family-level confounders such as parental IQ and class background. Second, we examine the differential effects on poor and non-poor students of the most important source of variation in peer poverty: "structural" and "non-structural" school mobility. Structural mobility is a possibly exogenous policy-induced variation in peer composition that occurs when students make transitions across schools with different grade level configurations (e.g., elementary schools with grades three to five and middle schools with grades six to eight); non-structural mobility is not due to a change in grade configuration, but instead due to residential mobility or choosing a school outside the family's attendance zone. Third, unlike most prior research, we measure peer poverty at the classroom level rather than the school level, which, due to the non-random sorting of students to classrooms and middle school tracking based on achievement level, permits more precise estimates of peer poverty effects.

[2] Here fixed effects refers to the panel data technique of using differencing or including indicator variables to control for unit-specific effects, not fixing random effects to zero or fixed (versus random) coefficients in random effects model.

Fourth, we examine the effects of both increases and decreases in peer poverty among diverse population of students enrolled in the North Carolina public school system (a population that includes in large numbers whites, blacks, Hispanics, non-poor and poor students).[3] Finally, we focus on elementary and middle school aged student test score growth for three reasons: 1) the effects of peer poverty on younger students is relatively understudied, 2) the effects of family poverty have been shown to be stronger for children relative to adolescents (Duncan et al. 1998), and 3) the effect of peer poverty has been shown to be stronger for cognitive and achievement outcomes than for behavioral and health outcomes (Duncan and Brooks-Gunn 1997).

DATA

This project uses test score and related data for multiple cohorts of students in grades 3-8 in North Carolina between 2001 and 2006. North Carolina is a particularly appropriate state for this analysis because its tests are vertically equated, interval scaled, and consistently administered over this time period. The scores are produced from a three-parameter logistic IRT model and are scored on a developmental scale to allow computation of growth across grade levels.[4] The sample includes about 3.6 million student-years with a valid math score and grade level in our sample (1.25 million students). We focus on math rather than reading because educational research generally finds larger school and program effects on math scores than on reading scores. The choice to analyze math achievement instead of reading achievement,

[3] Relative to national statistics, blacks are overrepresented in North Carolina public elementary and secondary schools (31.5% vs. 17.2% nationally) and Hispanics are underrepresented (8.4% vs. 19.8% nationally). The percentage of whites in the North Carolina system closely mirrors the national percentage (56.6% vs. 57.1%). Data from the 2007 Digest of Education Statistics, table 40.

[4] The test is anchored at grade three and re-centered on grade five. The math test was rescaled in 2006. To compute growth scores for the state's accountability system the North Carolina Department of Public Instruction (NCDPI) conducted equating studies to permit conversion of scores across time. These studies, which used equipercentile equating, produced concordance tables to convert old scores to the new metric for the purposes of the state's accountability calculations. This study used these concordance tables to convert scores to a consistent metric.

however, has no effect on the results we present (results for reading achievement available from authors upon request). To avoid biasing the school poverty estimates, we use all students in the population regardless of retention or promotion status, including a time-invariant covariate for whether the student was ever retained when appropriate. Descriptive analysis indicates that students with low levels of peer poverty have higher third and eighth grade math test scores (table 1). For example, students in the lowest quartile in peer poverty have third grade math scores of 345.9, on average, whereas students in the highest quartile in peer poverty have third grade math scores of 338.1, on average, a gap of nearly eight scale points. By eighth grade, this gap increases significantly, to about eleven scale points. Students with high poverty peers exhibit slightly higher math test score gains in fourth and seventh grade and slightly lower gains in fifth, sixth, and eighth grade. This evidence suggests that peer poverty is negatively associated with initial status. These results present a mixed picture about the association between peer poverty and math test score growth. This table also shows that math test scores grow more in elementary school grades (3-5) than in middle school grades (6-8), which suggests that a non-linear growth model is warranted

[Insert Table 1 about here]

Math scores for students in grades three through eight range from 303 to 388, with an average of 351.6 and a standard deviation of 11.0 (table 2). The average within-grade standard deviation of math scores is 9.5 (not shown). Because initial status in this study is grade three, we subtract three from grade level to produce a regression intercept corresponding to average third grade achievement (i.e., initial status). Peer poverty is defined as the standardized non-self[5] mean

[5] Student *i*'s own free/reduced lunch status is not taken into account in calculating student *i*'s own peer poverty rate. We use this form of peer measurement to avoid introducing a mechanical correlation between student's own poverty level and her peer poverty measure.

level of a student's classroom peers' free/reduced lunch status. Classroom is defined as the group of students with whom the student took their math test in each year.[6] Peer poverty is a time-varying characteristic rather than a fixed characteristic because 1) students can be assigned to classrooms with varying poverty composition over time, 2) students change schools due to residential changes and school choice, and 3) students make structural school enrollment changes (chiefly changing from an elementary to a middle school). Measuring peer poverty at the classroom level rather than the school level permits analysis of within-school variation across cohorts. There is considerable variation in peer poverty both within and between schools. School average peer poverty rates range from 0% to 100%, with an average of 50% and a standard deviation of 23%. About 75% of total variation in peer poverty rates lies between elementary schools, while 25% of variation is between classrooms within schools. Perhaps due to early tracking, the portion of variation that lies between classrooms in middle schools is larger, at 40%, leaving 60% between schools.

In preliminary models we control for race/ethnicity, gender, ever retained (all time invariant), and parental education, status as gifted, special education, limited English proficient, school transitions, and poverty status (all time varying). We include family poverty (free/reduced lunch eligibility) as a time-varying covariate because student free and reduced lunch eligibility changes from year to year due to changes in family income. For the population used in this study, the poverty level of the student changes at least once for about 15% of the students. School moves are separated into structural and non-structural based on whether a school switch was mandated by school district policy. Because these school moves variables are predictors of variation in peer poverty, we discuss them further in the following section.

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[6] Classrooms with five or fewer students (less than two percent of the student-year observations) were dropped from the analysis.

[Insert Table 2 about here]

METHODS

In brief, we estimate multilevel growth models and student fixed effects models to show how the effects of peer poverty are sensitive to key modeling assumptions. To analytically separate the effects of school mobility from classroom assignment, we estimate a series of models that test the effects of extreme changes in peer poverty for subsamples of students who make structural and non-structural school changes. We divide this section and our results into three parts by type of model: multilevel growth, student fixed effects, and school mobility subsample models

Multilevel Growth Models

Using test score data that are interval scaled and vertically equated to allow for growth modeling, we estimate a non-linear growth model with random intercept and slopes. Researchers in sociology, psychology, education, and criminology often use multilevel modeling for growth models because it allows for unbalanced panels, permits a wide range of covariance structures, accounts for within-subject inter correlation, and often uses empirical Bayes estimation, which weights estimates by their reliability (the ratio of the true score variance to the observed score variance) (Bryk and Raudenbush 1987; Singer and Willett 2003). Using the notation of Rabe-Hesketh and Skrondal (2008), the non-linear multilevel growth model of peer poverty effects can be written as:

$$A_{ij} = \beta_0 + \beta_1 Grade_{ij} + \beta_2 Grade_{ij}^2 + \beta_3 PeerPov_{ij} + \beta_4 Grade_{ij}\ X\ PeerPov_{ij} + \beta_5 Grade_{ij}^2\ X\ PeerPov_{ij} + \boldsymbol{\theta XT}_{ij} + \boldsymbol{\gamma X}_j + \zeta_{1j} + \zeta_{2j} Grade_{ij} + \zeta_{3j} Grade_{ij}^2 + \varepsilon_{ij} \quad (1)$$

This random coefficients growth model regresses a math achievement test score, A, for student *j* at occasion *i*, on grade level, grade squared, the student's non-self mean classroom peer poverty rate, interactions between grade and grade squared and peer poverty rate, a vector of time-varying covariates, **XT**, and a vector of time-invariant covariates, **X**. This equation includes three random effects: for the intercept ζ_{1j}, slope of the instantaneous growth rate, $\zeta_{2j}Grade_{ij}$, and slope of the curvature parameter, $\zeta_{3j}Grade_{ij}^2$. The interpretation of these coefficients is as follows: β_0 is student initial status (3rd grade, in our case), β_1 is the instantaneous average growth rate for each increase in grade level, β_2 is the curvature parameter (this is negative in our models, so it represents the rate of deceleration with each increase in grade level), β_3 is the effect of peer poverty on initial status of achievement, β_4 is the change in β_1 from increases in peer poverty, and β_5 is the change in β_2 from increases in peer poverty.

Parameters estimated from this model are unbiased and efficient assuming that given the covariates, the random effects and the student-level residual, ε_{ij}, are normally distributed with zero mean, are independent of one another, with the random effects independent across subjects and ε_{ij} independent across subjects and occasions.[7] We specify an unstructured variance-covariance matrix that allows three random effects to covary without imposed assumptions. We also present a more parsimonious random intercept model for comparison purposes, finding that parameter estimates on the variables of interest are virtually identical to those produced by the random coefficients model:

$$A_{ij} = \beta_0 + \beta_1 Grade_{ij} + \beta_2 Grade_{ij}^2 + \beta_3 PeerPov_{ij} + \beta_4 Grade_{ij}\, X\, PeerPov_{ij} + \beta_5 Grade_{ij}^2\, X\, PeerPov_{ij} + \boldsymbol{\theta XT}_{ij} + \boldsymbol{\gamma X}_j + \zeta_{1j} + \varepsilon_{ij} \quad (2)$$

[7] We specify growth models as two-level models, occasions within subjects, rather than occasions nested within subjects cross-nested in classrooms due to computational limitations, our focus on the parameter estimates rather than the random effects, and the fact that with population level data efficient estimates of standard errors are a secondary concern. Moreover, simulation evidence suggests that ignoring cross-nesting is likely to affect the variance components and not the parameter estimates (Luo and Kwok 2009).

The multilevel growth model produces an unbiased estimate of the effect of peer poverty on test score growth if peer poverty is uncorrelated with the random effects and if family background is adequately controlled and well measured. Omitted variable bias could produce inconsistent parameter estimates, which could threaten the validity of this model. For example, if the model used to estimate the effect of peer poverty on student test score growth omits adequate measures of parenting skill and if more skilled parents are less likely to send their children to high poverty schools and more likely to have high achieving students, then the estimate of peer poverty on student test score achievement would be downwardly biased. Although mixed models can increase efficiency due to the use of both within and between variance, such models provide no solution for this type of confounding bias. To help interpret whether peer poverty effects emerge from within-student across-time variation or between-student variation we estimate a model with the peer poverty effect decomposed:

$$A_{ij} = \beta_0 + \beta_1 Grade_{ij} + \beta_2 Grade_{ij}^2 + \beta_3\left(PeerPov_{ij} - \overline{PeerPov}_{.j}\right) + \beta_4\left(\overline{PeerPov}_{.j}\right) + \beta_5 Grade_{ij}\ X\left(PeerPov_{ij} - \overline{PeerPov}_{.j}\right) + \beta_6 Grade_{ij}^2\ X\left(PeerPov_{ij} - \overline{PeerPov}_{.j}\right) + \beta_7 Grade_{ij}\ X\left(\overline{PeerPov}_{.j}\right) + \beta_8 Grade_{ij}^2\ X\left(\overline{PeerPov}_{.j}\right) + \boldsymbol{\theta XT}_{ij} + \boldsymbol{\gamma X}_j + \zeta_{1j} + \varepsilon_{ij} \quad (3)$$

In the above equation, β_3 is the estimate of within-student peer poverty effect on the intercept; β_4 is the estimate of the between-student peer poverty effect on the intercept. The coefficients β_5 and β_6 are the estimates of the effects of the within-student student peer poverty effect on instantaneous growth and curvature, respectively. The coefficients β_7 and β_8 are the estimates of the effects of the between-student student peer poverty effect on instantaneous growth and curvature, respectively.

If the coefficients on the between-student effects of peer poverty are large relative to the within-student effects, it is possible that the omission of student and family background characteristics biases our estimates of peer poverty contextual effects. In thinking about bias, it is helpful to return to our explanations of peer poverty effects: contagion, relative deprivation, collective socialization, and institutions. Peer effects can emerge either because students affect each other or because adults in schools affect students. The former pertains to contagion and relative deprivation explanations; the latter to an institutional or collective socialization explanation.[8] In either case, the validity of inferences about contagion or institutional effects hinges on removing the confounding effects of student and family background.

Student Fixed Effects Models

We use a student fixed effects model to control for students sorting into schools based on unobservable student and family background characteristics. The fixed effects formulation uses each student as his/her own baseline, which holds constant all observable, unobservable, and mismeasured time-invariant student and family background characteristics. This approach eliminates all time-invariant between-student confounding in the peer poverty effect and produces consistent parameter estimates when there is no within-student confounding of the peer poverty effect (i.e., that the peer poverty effect is uncorrelated with time-varying unmeasured student characteristics). The student fixed effects model is specified as:

$$A_{ij} = \beta_0 + \beta_1 Grade_{ij} + \beta_2 Grade_{ij}^2 + \beta_3 PeerPov_{ij} + \beta_4 Grade_{ij}\ X\ PeerPov_{ij} + \beta_5 Grade_{ij}^2\ X\ PeerPov_{ij} + \boldsymbol{\theta X T}_{ij} + \alpha_j + \varepsilon_{ij} \quad (4)$$

[8] Because we view teachers as both the primary agents of collective socialization within schools, and as the most important resource that schools can make available to students, in this discussion we subsume collective socialization into institutional effects.

Here we treat the subject-specific intercept as a fixed unknown parameter to be estimated, with α_j representing the deviation of subject *j*'s intercept from the mean intercept β_0 with $\sum_{j=1}^{J} \alpha_j = 0$. This model is often estimated by "demeaning" both sides of the equation by the subject's panel mean, which removes between-student confounding by using only within-subject variation to estimate parameters. We have omitted from equation (4) any time invariant covariates because these have no within-subject variance and are therefore not estimable with this approach (though their effects are subsumed into the subject-specific intercept).

In studies of peer achievement effects, but not peer poverty effects, the direction of causality may be quite difficult to determine because student achievement at time *t* and peer achievement at time *t* are simultaneously determined (Manski 1993). Determining the causal direction between peer poverty and achievement is more straightforward. We posit that peer poverty level affects student achievement initial status and growth and that student achievement at time *t* does not affect peer poverty at time *t*. This seems like a reasonable assumption given that student academic performance has no bearing on their parents' earning power.

The student fixed effects approach requires within-student variation on peer poverty to identify parameters and is relatively inefficient relative to the random effects models. Due its large sample size and the six-year panels within it, however, our data are well suited to this approach. We identify the peer poverty effect from year-to-year variation in the poverty composition of students' classrooms. This changes due to school mobility and due to variations in the poverty compositions of student's assigned classrooms as they progress through grade levels in the same school. Because peer poverty rates vary more between schools than within schools, school movers are somewhat more likely to experience to change in peer poverty than students who remain in the same school. For example, for the group of sixth graders who changed from an elementary school to a middle school, the standard deviation of their fifth to sixth grade peer poverty difference score is 24%; for the group of fifth graders who remained in the same elementary school, the standard deviation of their fourth to fifth grader peer poverty difference score is 14%. This evidence suggests that across time variation exists to analyze for both school stayers and school movers, but that a larger portion of the variation that is analyzed appears to come from movers.

Due to the multiple cohort design of the study, many students in the sample do not appear in the dataset long enough to be at risk of a school move. About 57% of the students in the sample never make a school move. However, those students who are in the sample for four or more years (one-third of the sample) are likely to make one or more school moves: about one-third of this subset of students have one school move and four in ten have one or more school moves. As noted above, there are two types of school moves: structural (due to school district policy about grade configuration) and non-structural (due to residential mobility and school choice). The most common form of school move is structural. About two-thirds of sixth graders in North Carolina made a structural move between 5th and 6th grade (between elementary and middle school). Of all the structural moves, 87% occurred between these two grade levels. All told, 32% of the sample made one structural move, 66% made no structural move, and 1% made more than one. In addition, about ten percent of a grade level made a non-structural move in any given year. This type of move becomes slightly less common as students age, but is not a deterministic function of grade level. In total, about 17% of the sample made one or more non-structural moves.

School Mobility Subsample Models

It is possible that estimates from student fixed effects models could be biased towards zero if within-student peer poverty rates do not vary enough. In other words, we could be aiming to exploit variation when there is simply not enough for this purpose. About 62% of variation in peer poverty lies between students and 38% is within-student, across time. Whether this glass of variation is one-third full or two-thirds empty is a debatable issue. Exploiting within-student variation has conceptual and methodological advantages, but mixing two sources of within-student variation (classroom assignments for school stayers and between-school differences for school movers), may obscure important effects that only emerge when these two sources of variation are disentangled. By limiting the sample to students who made school moves during elementary school, during middle school, and between elementary school and middle school we exploit the larger source of within-student variation in peer poverty. We compute the difference in peer poverty rates between students' pre-move and post-move classrooms and divide this distribution into quintiles. We use the top and bottom quintiles in regressions to denote students who had the most extreme changes in context due to school mobility. For example, for the structural move equation between 5th and 6th grade, we limit the sample to 4th through 7th graders with complete panels. We focus on this sample to permit estimation of the middle school test score trajectory deflection that occurs between 4^{th} and 7^{th} grade, which requires comparing two gains both immediately before and immediately after the 6^{th} grade structural school move. In addition, it retains the benefits of increased reliability that a multiple cohort design provides (included in this analysis are three cohorts starting 4th grade in the years 2001, 2002, and 2003).[9] We estimate:

$$A_{ij} = \beta_0 + \beta_1 G5_i + \beta_2 G6_i + \beta_3 G7_i + \beta_4 \Delta PPQ1_{ij} + \beta_5 \Delta PPQ1_{ij} X\ G6_i + \beta_6 \Delta PPQ1_{ij} X\ G7_i + \beta_7 \Delta PPQ5_{ij} + \beta_8 \Delta PPQ5_{ij}\ X\ G6_i + \beta_9 \Delta PPQ5_{ij} X\ G7_i + \boldsymbol{\theta} \boldsymbol{XT}_{ij} + \alpha_j + \varepsilon_{ij} \quad (5),$$

where *G5, G6, G7* are year fixed effects for grades four through seven, $\Delta PPQ1$ is an indicator for the first quintile in the change in peer poverty from a structural move between 5th and 6th grade (this denotes a large *increase* in peer poverty, or "downward mobility"), and $\Delta PPQ5$ is an indicator for the fifth quintile in the change in peer poverty from a structural move between 5th and 6th grade (this denotes a large *decrease* in peer poverty, or "upward mobility"). The interaction terms estimate the extent to which large changes in peer poverty context are associated with test score deflections in sixth and seventh grade. If contagion theory is correct, switching to a higher poverty middle school should be associated with negative test score deflections and switching to a lower poverty middle school should be associated with positive test score deflections. Relative deprivation predicts the opposite pattern: switching to a more affluent school should be associated with negative test score deflections and switching to a higher poverty school should be associated with positive test score deflections. To gain more insight into whether contagion or relative deprivation explanations are more plausible, we run separate analyses for poor and non-poor children. We also estimate models similar to (5), but on subsamples of students in grades three through six who make a nonstructural move between 4th

[9] Using samples of students defined by alternative grade ranges (grades 3-8 and grades 5-6) produces similar results.

and 5th grade and on students in grades five through eight who make a nonstructural move between 6th and 7th grade.

RESULTS

Multilevel Growth Model Estimates

Table three displays coefficients from random intercept models (models 1-5; see equation 2, above) and a random coefficient model (model 6; see equation 1, above). The fully unconditional model (1) decomposes math test score variation into between-student (σ_u) and within-student (σ_e) components. This model produces an estimate of intraclass correlation, ρ, of .62, which means that 62% of the variance in math test scores lies between students. Including grade level and the square of grade level (model 2), reduces the within-student variation (expressed here as σ_e, the standard deviation of the error term in equation [2]). in test score by about 42% and has very little effect on the between-student variance component, σ_u. Because all variables in the models shown in table 3 except for the growth parameters are grand mean centered, and the grade level growth parameters are coded 0 to 5, the constant represents initial status for the average 3^{rd} grade student. The average third grade student has a score of 340.6, an instantaneous growth rate of 5.836, and a negative curvature parameter, -0.415, which indicates that students' rate of change in test score growth declines over time. Including fixed and time-varying student characteristics (model 3), reduces between-student variation by about 25% (relative to the fully unconditional model) and increases within-student variation (relative to the unconditional growth model), but only slightly. Most of the coefficients on these control variables conform to expectations, with negative differences in initial status for minorities, and poor, special education, limited English proficient, non-structural school movers, and retained students, and positive differences for males, the academically gifted, and students with highly educated parents. Net of grade level, students making structural moves suffer test score decrements, which suggests that the event of changing schools between elementary and middle school has a negative effect on students that is analytically separable from the effect of age. Including the poverty rate of the student's grade level peers (model 4) explains virtually no additional variation over and above the variation explained by the variables included in model 3. Peer poverty has a negative effect on initial status, with a one standard deviation increase in peer poverty producing a half a scale point decrease in 3rd grade math test score. This represents 0.05 SD of the math test score. The linear growth rate in math is 3.7 (not shown). The peer poverty main effect, therefore, represents about 14% of a year's growth. The interactions of the main peer poverty effect and the growth parameters, .076 and -.025, are statistically distinguishable from zero, but combine to produce only small changes in growth rates and fail to produce a substantial incremental reduction in within-student variance (model 5). This model predicts that at baseline, -.96 scale points separate students with peer poverty levels one standard deviation level above and below 0. The corresponding figure at 8th grade is -1.48, indicating a small widening of the test score gap between students with high poverty and low poverty peers (10% of a standard deviation at baseline and 15% of a standard deviation by 8th grade). These results provide weak support for the contagion hypothesis. Net of their own poverty level, students who attend school with high poverty peers have only slightly slower growth rates than students who attend school with more affluent peers.

The coefficients on the interaction terms with student's own poverty level combine to predict higher growth rates for poor students in math. At baseline, the model predicts a 1.6 point gap between poor and non-poor students. By 8th grade the model predicts that this gap closes to .76 points. Model (6) displays results from the random coefficients growth model (equation (1), above). The parameter estimates are virtually identical to the more parsimonious random intercept model. The statistically significant variance components for grade and grade^2 indicate substantial variation in both the growth rate and the amount of curvature in growth trajectories across students.

[Insert Table 3 about here]

Figure 1 shows the unadjusted prediction of math scores produced from an OLS model with only grade, grade^2, and peer poverty level (not interacted with grade level). This graph shows about a five scale point difference at baseline and at 8th grade. Figure 2 displays the fitted values from the multilevel growth model presented in table 3, model 5, with all control variables held at their respective means. Relative to the unadjusted prediction, this multilevel growth model predicts a much smaller difference in initial status (about one scale point) than that is essentially maintained as students increase in grade level. Although there is some evidence of a slight widening of the gap, the 8th grade difference is only about .5 scale points larger than the 3rd grade difference, suggesting very little effect of peer poverty on student test score growth in math.

[Insert Figure 1 about here]

[Insert Figure 2 about here]

Table 4 displays coefficients from a random intercept model in which the peer poverty effect is decomposed into its within-student and between-student components (see equation 3, above). In a model with the same control variables as in the final model of table 3, the within-student effect of peer poverty is positive and significant, +.196. The between-student effect of peer poverty is negative and significant, -2.259. The interaction terms between the two types of peer poverty effects and grade are distinguishable from 0, but are very small; the interaction terms between the peer poverty effects and grade^2 are non-significant. The fact that these interaction terms are very small suggests that peer poverty, whether defined as a within-student or a between-student effect, has very little effect on test score trajectories. The disparity in size between the within-student and the between-student peer poverty main effects raises concerns about whether a composite of the two effects provides meaningful information. The between-student peer poverty effect on initial status is the result of "pre-treatment" differences in the academic preparation between those with poor and non-poor peers. Some important student characteristics that predict third grade academic preparation are controlled in the model, but many important ones are not. If student characteristics that are correlated with both third grade math test score and peer poverty are omitted, therefore, the peer poverty coefficient would be estimated with bias. We suspect that the between-student estimate is downwardly biased because many, if not all, important omitted confounders are negatively correlated with peer poverty and positively correlated with test score.

[Insert Table 4 about here]

[Insert Table 5 about here]

Student Fixed Effects Estimates

Table 5, model 2, includes a student fixed effect (see equation 4, above) and produces a very small, but positive and statistically significant estimate of the peer poverty effect of .0559. This represents .006 of a standard deviation in math test score and .015 of a year's test score growth. Both of the peer poverty interaction terms are statistically significant, but collectively they predict no meaningful difference in the test score trajectories. Together, these coefficients suggest that once unobservable student and family background are controlled, peer poverty effects are not important factors in predicting either initial status or growth in achievement over time. As shown in figure 3, the student fixed effects specification predicts test score growth trajectories for students with poor and non-poor peers that are virtually identical.

[Insert Figure 3 about here]

Although not comparable in metric terms, it is worth considering the parameter estimates of student's poverty level. The multilevel growth model estimate of a student's own poverty level in model 1 is -1.621. This coefficient and the interaction terms indicate that poor students start out below their non-poor peers, but grow at a faster rate. Since student poverty stands in for unobserved student and family background characteristics, once we account for student unobservables like ability, early childhood experiences, and motivation with student fixed effects, the student poverty effect shrinks toward 0, although it remains negative indicating that poor students start somewhat lower in initial status. The interaction terms in the student fixed effects model indicate that students in poverty grow at a faster rate than students not in poverty, but the predicted rate of growth is slower in the fixed effects specification than in the multilevel growth model specification.

In supplementary analyses (not shown, but available from the authors upon request), we find no substantively meaningful differences in the linear peer poverty effect on test score initial status or growth across the following subgroups: poor, non-poor, white, black, or Hispanic. Moreover, including peer racial and ethnic characteristics does not substantively alter the peer poverty effect reported in model 2. Including peer achievement, which is positively correlated with student test score and negatively correlated with peer poverty, produces a much larger positive peer poverty coefficient.

To determine whether our choice of SES measure affects our findings, we also present in table 5 two alternatives, based on student reports of parental education. The first, peer high parental education, tests whether affluence rather than poverty has a larger effect (Brooks-Gunn et al. 1993). We operationalize this construct as the standardized non-self mean of the percentage of peers with parents holding a BA or higher. Peer high parental education enters the equation with a small negative coefficient, -.0182, which is statistically, but not substantively significant. The second measure, peer average parental education, is the standardized non-self mean of peer parental education (measured on an ordinal scale). This SES measure enters the equation with a coefficient of -.0172, which is neither statistically or substantively significant. The interaction terms shown in models 3 and 4 are not distinguishable from zero and combine to predict no differential in student growth rates. In summary, once we account for student unobservables, peer SES, whether measured by peer poverty, peer high parental education, or peer parental education, has virtually no effect on either test score initial status or growth. In summary, these results provide no support for commonly held theories about peer poverty effects. They instead point to the strong possibility that selection on unobservables may be driving the weak support for the contagion hypothesis suggested by the random effects models.

School Mobility Subsample Estimates

As noted above, we must be cautious about fixed effects estimates. Relying on within-student variation removes between-student confounding, but provides no protection against time-varying confounding. Therefore, a closer examination of the sources of within-student variation helps inform interpretation. As shown in models 1 and 2 of table 5, net of student's grade level, students who make structural school moves (e.g., the change between elementary and middle school, which in North Carolina takes place for most students between 5th and 6th grade), suffer test score declines. Prior research has found that students from a variety of backgrounds and achievement levels face test score declines and other negative academic and social psychological effects during school transitions (e.g. Barone, Aguirre-Deandreis and Trickett 1991; Cook et al. 2008; Eccles et al. 1993; Langenkamp 2010; Simmons et al. 1979). The size of this effect from the student fixed effects model (-.964) is relatively large compared to the other effects we report. It represents .10 SD or about a quarter of a year's linearized growth. It suggests that the elementary to middle school transition is a difficult one for many students.

The fact that the structural move coefficient is virtually identical in the random effects and fixed effects models suggests that the structural school move effect is one of the few effects in this analysis that is not biased by the omission of fixed student unobservables. Therefore, the structural school move between elementary and middle school is a plausible source of exogenous variation, one that is driven by school district school assignment policies rather than family choices and disruptions (such as divorce or residential mobility to a neighborhood with higher or lower poverty). The flaw in this reasoning is that there may be a relationship between the direction of change in classroom peer poverty a student experiences and the student's prior achievement level, which suggests that students with high achieving students may avoid sending their children to middle school with higher poverty levels than their elementary school. Table 6 presents descriptive results test score gains from the sample of students in grades four through six who have complete panels and made a structural school change between 5th and 6th grade. We calculate the change in peer poverty a student experienced between these two grade levels and break this variable into quintiles, where quintile one represents a change to much *higher* peer poverty (*downward mobility* with respect to peer composition), quintile three represents no or minimal change in peer poverty, and quintile five represents a change to much *lower* peer poverty (*upward mobility* with respect to peer composition). The first column records the average 4th grade score for each quintile and the second shows the average 5th grade score for each quintile. Student prior achievement is positively related to making upwardly mobile moves, which is evidence against viewing the elementary to middle school transition as a source of exogenous variation in peer poverty.[10] When peer poverty is defined at the grade level rather than the classroom level, however, there is no relationship between prior achievement and direction of peer poverty change. The positive relationship shown in table 6 between prior achievement and peer poverty change may simply be a function of tracking based on achievement level in sixth grade. Without more detailed data on tracking and geo-coded data (neither of which are available for this study), it is difficult to assess this issue more fully.

[Insert Table 6 about here]

Table 6 shows a sharp decline in test score gains between 5^{th} and 6^{th} grade. For example, for students in quintile 1 of peer poverty change the 4^{th} to 5^{th} growth is 6.52 scale points, whereas the 5^{th} to 6^{th} grade growth is only 2.07 scale points. To examine the relationship between the magnitude of the students change in peer poverty and deflections from student's growth trajectories we compute the "difference in difference" between the 4^{th} to 5^{th} grade gain and the 5^{th} to 6^{th} grade gain (D in D = $[6^{th} – 5^{th}] – [5^{th} – 4^{th}]$), shown in the last column. This column shows that, on average, large changes in peer poverty are associated with larger deflections from a student test score trajectories. In other words, there is some evidence of non-linearities in the effect of peer poverty change on student test score growth, with somewhat more negative effects for students with large increases or large decreases in peer poverty between elementary and middle school. We also find evidence of this pattern of non-linearities among non-structural elementary school movers. Among non-structural middle school movers, however, the difference

[10] In the language of the counterfactual model causal effects (Holland 1986; Morgan and Winship 2007; Rubin 1974), families may be choosing their middle school "treatment" based on their assumptions on how middle schools will affect their children's potential outcomes.

in gains is slightly larger at the extremes than in the middle (results available from authors upon request).

Table 7 displays coefficients from the student fixed effects model specified in equation 5, above. This model is estimated on a subsample of students in grades four through seven who made a structural move between fifth and sixth grade (models control for whether students made non-structural moves between fourth and fifth grade and sixth and seventh grade). Model 1 of table 7 shows that the peer poverty effect for this subsample is equivalent to the effect from the full sample (-.0777 for the subsample and -.0559 for the full sample). Model 2 includes interactions between grade level indicators and the size of the peer poverty change between elementary and middle school.[11] These interaction terms are negative, but small. To determine whether big increases or big decreases in peer poverty have differential effects on the test score trajectories of poor and non-poor students, we estimate this model separately for each group. Model 4, estimated on poor students, shows virtually no effects of peer poverty difference on test score growth. This suggests that poor students are relatively immune to large changes in context between elementary and middle school. A different pattern emerges from model 3, estimated on non-poor students. Non-poor students experience negative test score deflections from both large increases and large decreases in peer poverty between elementary and middle school. The negative effects of downward mobility for non-poor students in both sixth and seventh grade are consistent with a contagion explanation. The negative effects of upward mobility for non-poor students, which emerge in sixth grade and are substantially reduced by seventh grade, are consistent with the relative deprivation hypothesis. These deflections represent between 7% and

[11] Because the size of peer poverty change a student experiences is a fixed characteristic, this variable is subsumed in the student-specific intercept and is not estimable. Because the interaction of a time-varying variable (grade level) and a fixed characteristic (amount of peer poverty change) is itself a time-varying variable, we can consistently estimate it in a fixed effects model (Wooldridge 2003) .

10% of the average gain between fifth and sixth or sixth and seventh grade.[12] The negative effects of downward mobility for non-poor students are perhaps not surprising. They may be driven by time-varying unobservables such as job loss or divorce. The negative effects of upward mobility, however, run counter to common narratives about family unobservables, such as strong values for educational achievement among highly motivated parents, that would other things equal produce positive effects of switching to a more advantaged school.

[Insert Table 7 about here]

To determine whether other types of school moves have the same negative effects on non-poor students, we estimate student fixed effects models similar to the one specified in equation 5, but on subsamples of elementary and middle school non-structural school movers. Table 8a displays coefficients from the elementary school non-structural movers, which includes only third through sixth grade students who make non-structural school changes between fourth and fifth grade (models control for whether students made a structural move between elementary and middle school). As with the structural mover sample, poor students in the elementary non-structural mover sample appear to be relatively immune to changes in context. Non-poor students who make upwardly mobile moves, however, have negative test score deflections which are stronger in 6th grade than in 5th grade. These deflections represent about five percent of the fourth to five grade gain and about one-quarter of the fifth to sixth grade gain. With these results, we find support for the relative deprivation hypothesis, but only among non-poor students. Table 8b displays coefficients from the middle school non-structural movers, which includes fifth through eighth grade students who made a nonstructural school move between 6th and 7th grade (models control for structural moves between fifth and sixth grade). Among poor students, we find that

[12] For example, the difference between the grade 6 and the grade 5 effects is 2.28. The ratio of the PPQ5*Grade 6 coefficient to this gain score is -.217/2.28=.095.

downward mobility is negatively associated with test score gain in eighth grade, which supports the contagion hypothesis. This effect represents about 17% of the seventh to eighth grade gain. Interestingly enough, we do not find evidence that downward mobility has immediate effects on seventh grade scores. Upward mobility is positively associated with test score gain among poor students, but the coefficients are small and imprecisely estimated. Among non-poor students we again find that upward mobility is negatively associated with test score gain, with immediate effects on seventh grade scores stronger than effects on eighth grade scores. The effect on seventh grade scores represents about seven percent of the sixth to seventh grade gain. Negative effects from downward mobility emerge for non-poor students, but these effects are imprecisely estimated.

[Insert Tables 8a and 8b about here]

In summary, examining the effects of structural and non-structural movers reveals some striking patterns. In every subsample, non-poor student test score growth is depressed by upwardly mobile moves, which supports relative deprivation theory and contradicts contagion theory. In the structural moves sample, however, non-poor students are also harmed by downwardly mobile moves, which supports contagion theory. Poor students are relatively immune to extreme changes in context. Therefore, we find consistent support of relative deprivation theory explaining the underperformance of non-poor students who make upwardly mobile school moves and less consistent evidence in support of contagion theory in explaining the effects of downwardly mobile moves. Importantly, we find no statistically significant evidence that poor students benefit academically from attending schools with much lower levels of poverty.

CONCLUSION

Much ink has been spilled searching for evidence of contextual effects on youth outcomes. What do we add to this rich literature? This study is designed to test the hypothesis that school contexts with high levels of peer poverty rates harm both initial status and test score growth. This study moves beyond a conception of contextual effects as correlations estimated on young people at one point in time to context shaping youth development over time. Our findings suggest that most, if not all, of the negative effects of high poverty peers reported in the literature may be downwardly biased by the failure to include unobserved and mismeasured confounders. Multilevel model estimates show evidence of a difference in initial status (3^{rd} grade test score), but no differences in the growth trajectories between those with poor and non-poor peers. In other words, differences in initial status are maintained between 3^{rd} and 8^{th} grade both for students with poor and non-poor peers. Including student fixed effects, which controls for time-invariant student background unobservables, also produces null effects of peer poverty rate on test score growth, and the difference in initial status found in the multilevel growth models shrinks to substantive insignificance. The fact that the peer poverty effects on initial status and growth are effectively null once student background is controlled suggests that selection rather than contagion, collective socialization, institutional, or relative deprivation appears to be the most likely explanation for the negative effect of peer poverty on student test score reported in previous research. The question of whether the effect of school poverty is causal or simply a function of either omitted variable bias or endogenous self-selection is a critical conceptual and empirical matter for both the theory of school effects and policies that seek to integrate students by socio-economic background (Duncan and Raudenbush 1999).

This study also attempts to disentangle the effects of school mobility from year-to-year variation in classroom poverty levels by examining the effects of large changes in poverty context for students who change schools. Large changes in context, whether from large increases in peer poverty or large decreases in peer poverty, generally have effects that are either negative or are indistinguishable from zero. Education transitions from one school to another are detrimental to all students in the form of lower grades and other academic outcomes (Barone, Aguirre-Deandreis and Trickett 1991; Eccles et al. 1993). The negative effect of these large changes in context may stem from reduced social support. During these transitions, reduced social support has a detrimental effect on an individual's academic performance and adjustment to new school environments (Aikins, Bierman and Parker 2005; Dubow et al. 1991; Reyes et al. 2000). Large contextual changes may lead to a reduction in the number or dilution of the density of same grade-level peers across schools during transitions. For some students, retaining peer groups across educational transitions is beneficial (Langenkamp 2010; Schiller 1999). Moreover, some researchers have attributed the lack of long-term positive effects in the dramatic declines in neighborhood poverty experienced by MTO families to the difficulties male youth face in adjusting to their new environments (Clampet-Lundquist et al. 2010; Kling, Liebman and Katz 2007; Kling, Ludwig and Katz 2005). In summary, extreme changes in poverty context, either increases or decreases, may produce negative effects due to a youth's difficulties in adapting to new environments.

While previous research has focused on the negative contextual effects of poverty on the poor, we find more consistent negative effects of changes in poverty context on the non-poor than on the poor. In particular, we find negative effects from upwardly mobile school moves for the non-poor, which supports the hypothesis that non-poor students may suffer from relative deprivation when changing to more affluent schools. We find less consistent evidence that downwardly mobile moves harm academic test score growth. Negative effects of large increases in peer poverty emerge for non-poor students in one of three subsamples and for poor students in another one of the three subsamples. We find no statistically or substantively significant evidence that poor students benefit from attending more affluent schools.

This study has important implications for both research and public policy. These findings suggest that standard estimates and prevailing theories about social influence among pre- and early-adolescents may not hold for test score achievement, one of the most important educational outcomes. Second, the fact that the peer poverty effect disappears in fixed effects models suggests that mixing students by poverty level may not have the intended effect of increasing achievement because achievement and achievement growth are not a function of peers but of student and family background. The policy goal of mixing students by race-ethnicity or social background has been a mainstay in educational policy since the Brown vs. Board decision. Since the 1980s, school desegregation orders have been vacated by an increasingly conservative judiciary. The changing legal landscape has contributed to a resegregation of American schools (Orfield, Eaton and Desegregation 1996; Reardon and Yun 2005; Rumberger and Palardy 2005) These trends are likely to continue given that in the 2007 case of *Parents Involved v. Seattle School District No. 1*, the Supreme Court ruled that school districts may not use race in assigning students or granting transfers to achieve or maintain school integration. In response to increases in school racial segregation and the Supreme Court's prohibition on the use of race in making school assignments, some advocate for integrating students based on socio-economic background (Kahlenberg 2001), which is constitutionally permissible. Kahlenberg (2001) argues that the best way to ensure the presence of high standards, highly qualified teachers, and less crowded classes is to ensure a critical mass of middle class families to advocate for these resources. Various forms of SES integration have been implemented in more than 50 districts across the U.S., including Lacrosse, WI; Wake County, NC; Cambridge, MA; and San Francisco, CA. The findings of the present study suggest that simply mixing students by social background may not have the intended effects, unless such mixing can garner increased resources and support for proven practices that can increase student achievement in impoverished contexts.

There are some limitations to this study that point the way for future work on peer poverty effects. Although North Carolina is racially and economically diverse, the study covers only the public school students from one state, which limits the generalizability of our findings. Using population-level administrative data, we have pursued an identification strategy that privileges reduction of bias over national representativeness. The external validity of these results will hinge on cross-state replications using administrative data, or preferably, a large nationally representative survey with rich contextual information and interval metric test scores designed to measure growth over time. The estimates in this study are based on observational data, not an experiment, so we cannot claim them to be causal estimates. We have been careful to stress that we use a research design that reduces, but probably does not entirely eliminate, bias from unobservables. For example, our inability to account for time-varying student or school unobservables could prove these estimates to be biased. Future work should carefully theorize and measure time-varying factors that predict test score growth. We cannot empirically examine whether changes in peer poverty correspond to substantial differences in micro-level interaction between students and students and teachers. Within-school sorting processes – tracking, social homophily, and inequitable access to resources – may limit the actual changes in context students experience from changes in peer poverty rates due to school transitions, although our use of classroom-level data is a benefit of our study relative to those measuring contextual effects at the school or census tract level. Finally, test scores may be mostly impervious to the influence of peers, institutional resource allocation, and socialization processes. Other outcomes such as pregnancy, drug use, school completion, and college attendance, may be more amenable to these factors than a test score, which is a discrete cognitive task rather than a behavioral event.

Much of the existing research base on contextual effects has examined the experiences and outcomes of high school students. This study represents one of the first sociological examinations of school contextual effects among elementary and middle school students. Despite this contribution, it may be that by third grade, the earliest time point in this study, early childhood experiences have largely determined a student’s potential for test score growth. If test score gaps among socioeconomic groups are essentially stable by third grade and variations in school quality have little effect on these gaps over time (Heckman 2006), then policies to mix students by social background may be of limited utility. Still, research has found neighborhood effects on birth weight and other early childhood development experiences (e.g. Chase-Lansdale et al. 1997; Masi et al. 2007; Morenoff 2003). Therefore, a promising avenue for future research may be the investigation of younger children's sensitivity to impoverished contexts, preferably with research designs that permit accounting for unobserved family or individual heterogeneity.

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Table 1. Math Level and Gain Scores by Grade and Peer Poverty Quartile

Peerpov Quartile		Grade 3	4	5	6	7	8	Total N
Avg	Level	342.1	346.9	352.0	353.6	356.7	358.4	
	Gain	--	5.37	5.88	2.55	3.61	2.20	
Q1	Level	345.9	350.6	355.9	359.1	362.2	364.1	
	Gain	--	5.11	6.14	2.66	3.56	2.40	898,468
Q2	Level	343.0	347.8	352.9	354.3	357.5	359.3	
	Gain	--	5.37	5.90	2.68	3.64	2.28	892,413
Q3	Level	341.1	346.0	350.9	351.9	355.0	356.8	
	Gain	--	5.43	5.75	2.54	3.62	2.10	888,188
Q4	Level	338.1	343.2	348.1	348.7	351.8	353.4	
	Gain	--	5.58	5.76	2.32	3.63	2.00	867,127
Total N		594,731	590,682	595,596	596,070	591,486	577,631	3,546,196

Note: Average gains are based on students who have valid test scores for two time periods (i.e. grade 3 and grade 4 for gains listed under grade 4 column), whereas average levels are based on all students who have a valid test score for that one time period. Most of the listed math score gains by peer poverty quartile are significantly different than each other (within grade). Five out of thirty comparisons are not significantly different: grade 4, q2 and q3; grade 5, q3 and q4; grade 7, q2 and q3, q2 and q4, and q3 and q4.

Table 2. Descriptive Statistics

	Obs	Mean	SD	Min	Max
Dependent Variables					
Math test score	3,649,921	351.59	10.96	303	388
Student Background					
Parent has high school degree or less	3,683,117	0	0.50	-0.51	0.49
Parent has some postsecondary education	3,683,117	0	0.40	-0.21	0.79
Parent has bachelor's degree or higher	3,683,117	0	0.45	-0.27	0.73
Black student	3,729,940	0	0.46	-0.30	0.70
Hispanic student	3,729,940	0	0.25	-0.06	0.94
Other racial/ethnic background	3,729,940	0	0.23	-0.05	0.95
Male student	3,729,940	0	0.50	-0.51	0.49
Student was designated gifted	3,729,940	0	0.34	-0.14	0.86
Student received special education services	3,716,453	0	0.33	-0.13	0.87
Student showed Limited English Proficiency	3,726,458	0	0.19	-0.04	0.96
Student was ever retained	3,638,828	0	0.27	-0.08	0.92
Student received free or reduced price lunch	3,620,888	0	0.50	-0.45	0.55
Student made a structural school move	3,729,940	0	0.36	-0.15	0.85
Student made a non-structural school move	3,729,940	0	0.28	-0.09	0.91
Peer Poverty					
% of classroom peers who received free or reduced price lunch (standardized)	3,620,659	0	1	-1.90	2.52
Time Variables					
Grade level - 3	3,729,940	2.50	1.70	0	5
(Grade level - 3) ^2	3,729,940	9.14	8.82	0	25
Interactions					
Grade*free or reduced price lunch	3,620,888	-0.04	1.49	-2.24	2.76
Grade^2* free or reduced price lunch	3,620,888	-0.21	6.26	-11.19	13.81
Grade*(% of classroom peers who received free or reduced price lunch)	3,620,659	0	3.02	-9.06	12.61
Grade^2*(% of classroom peers who received free or reduced price lunch)	3,620,659	0	12.69	-45.30	63.07

Table 3. Multilevel Growth Models Predicting Math Standardized Test Score Achievement

	(1) Fully Uncond	(2) Uncond Growth	(3) W/ Stu Char	(4) W/ Peerpov	(5) W/ Interactions	(6) W/ Random Coefficients
Grade		5.836***	5.830***	5.833***	5.821***	5.812***
		(0.00494)	(0.00581)	(0.00582)	(0.00583)	(0.00631)
Grade^2		-0.415***	-0.448***	-0.448***	-0.447***	-0.455***
		(0.000926)	(0.00108)	(0.00108)	(0.00108)	(0.00112)
Parent Has Some			0.814***	0.800***	0.787***	0.757***
Postsec Educ			(0.00757)	(0.00758)	(0.00758)	(0.00741)
Parent Has Bach			1.787***	1.730***	1.713***	1.648***
Degree or Higher			(0.00918)	(0.00916)	(0.00916)	(0.00881)
Black			-5.668***	-5.340***	-5.312***	-5.220***
			(0.0158)	(0.0159)	(0.0158)	(0.0155)
Hispanic			-2.641***	-2.416***	-2.397***	-2.372***
			(0.0307)	(0.0304)	(0.0304)	(0.0301)
Other Race/Eth			-0.712***	-0.543***	-0.528***	-0.487***
			(0.0292)	(0.0289)	(0.0289)	(0.0287)
Male			0.378***	0.393***	0.396***	0.375***
			(0.0133)	(0.0132)	(0.0132)	(0.0130)
Acad Gifted			3.895***	3.863***	3.921***	4.194***
			(0.0135)	(0.0135)	(0.0135)	(0.0126)
Special Education			-2.645***	-2.677***	-2.682***	-2.836***
			(0.0146)	(0.0146)	(0.0146)	(0.0145)
Limited English			-1.766***	-1.742***	-1.692***	-1.678***
Proficiency			(0.0270)	(0.0270)	(0.0270)	(0.0272)
Struct Sch Move			-0.942***	-0.947***	-0.944***	-0.973***
			(0.00725)	(0.00726)	(0.00726)	(0.00683)
Non-Struct Sch			-0.109***	-0.124***	-0.132***	-0.0830***
Move			(0.00960)	(0.00961)	(0.00961)	(0.00924)
Ever Retained			-6.121***	-6.032***	-6.019***	-5.806***
			(0.0270)	(0.0267)	(0.0267)	(0.0264)
Stu Pov			-1.102***	-0.933***	-1.644***	-1.621***
			(0.00913)	(0.00920)	(0.0155)	(0.0173)
Grade*Stu Pov					0.568***	0.556***
					(0.0119)	(0.0128)
Grade^2*Stu Pov					-0.0784***	-0.0812***
					(0.00226)	(0.0023)
Peer Ave Pov				-0.524***	-0.482***	-0.480***
Rate				(0.00409)	(0.00747)	(0.0084)
Grade*Peer Pov					0.0762***	0.0781***
					(0.00598)	(0.00642)
Grade^2*Peer Pov					-0.0255***	-0.0293***
					(0.00114)	(0.0012)
Constant	351.2***	340.6***	341.0***	341.0***	341.0***	341.0***
	(0.00885)	(0.00965)	(0.00882)	(0.00876)	(0.00876)	(0.00969)
σ_u	8.756***	8.778***	6.537***	6.447***	6.434***	7.318***
	(0.00716)	(0.00614)	(0.00580)	(0.00577)	(0.00576)	(0.00848)
σ_e	6.821***	3.932***	4.061***	4.070***	4.069***	3.753***
	(0.00312)	(0.00180)	(0.00202)	(0.00203)	(0.00203)	(0.00238)
SD(Grade)						2.346***
						(0.0096)
SD(Grade^2)						0.3289***
						(0.0024)
Observations	3649921	3649921	3416483	3416276	3416276	3416276

Note: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Models 1-5 include a random intercept as shown in equation 2 in the text. Model 6 includes a random intercept and random components for grade and grade^2. Models estimated in Stata with xtmixed and maximum likelihood.

Table 4. Multilevel Growth Model Predicting Math Standardized Test Score Achievement

	(1)
Grade	5.815***
	(0.00578)
Grade^2	-0.445***
	(0.00107)
W/in Stu Dev of Peerpov	0.196***
	(0.0124)
Grade*W/in Stu Dev of Peerpov	-0.0714***
	(0.0113)
Grade^2*W/in Stu Dev of Peerpov	-0.00143
	(0.00213)
Peer Pov - Grp Mean	-2.259***
	(0.0108)
Grade*Grp Mean Peer Pov	-0.0607***
	(0.00690)
Grade^2*Grp Mean Peer Pov	-0.00141
	(0.00131)
Constant	341.0***
	(0.00867)
σ_u	6.346***
	(0.00558)
σ_e	4.038***
	(0.00199)
Observations	3416276
R^2	

Note: Model also controls for the time-varying and time-invariant variables shown in table 3. Model includes a random intercept. See equation 3 in the text. Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5. Multilevel and Fixed Effects (FE) Models Predicting Math Standardized Test Score Achievement

	(1) RE-MLE (from Table 3, model 6)	(2) Student FE (peerpov)	(3) Student FE (baplus)	(4) Student FE (pared)
Grade	5.812***	6.283***	6.282***	6.282***
	(0.00631)	(0.00638)	(0.00637)	(0.00637)
Grade^2	-0.455***	-0.476***	-0.476***	-0.476***
	(0.00112)	(0.00112)	(0.00112)	(0.00112)
Struct Sch Move	-0.973***	-0.964***	-0.962***	-0.962***
	(0.00683)	(0.00639)	(0.00639)	(0.00639)
Non-Struct Sch Move	-0.0830***	0.0491***	0.0507***	0.0510***
	(0.00924)	(0.00968)	(0.00968)	(0.00968)
Stu Pov	-1.621***	-0.274***	-0.289***	-0.287***
	(0.0173)	(0.0198)	(0.0190)	(0.0190)
Grade*Stu Pov	0.556***	0.275***	0.298***	0.292***
	(0.0128)	(0.0134)	(0.0126)	(0.0126)
Grade^2*Stu Pov	-0.0812***	-0.0373***	-0.0437***	-0.0422***
	(0.0023)	(0.00238)	(0.00225)	(0.00225)
Peer Ave Pov Rate	-0.480***	-0.0559***		
	(0.0084)	(0.00960)		
Grade*Peer Pov	0.0781***	0.0178**		
	(0.00642)	(0.00675)		
Grade^2*Peer Pov	-0.0293***	-0.00659***		
	(0.0012)	(0.00120)		
Peer High Parent Ed			-0.0182*	
			(0.00889)	
Grade*Peer High Parent Ed			0.00923	
			(0.00617)	
Grade^2*Peer High Parent Ed			-0.000781	
			(0.00109)	
Peer Ave Parent Ed				-0.0172
				(0.00881)
Grade*Peer Ave Parent Ed				-0.000630
				(0.00623)
Grade^2*Peer Ave Parent Ed				0.00152
				(0.00111)
Constant	341.0***	340.3***	340.3***	340.3***
	(0.00969)	(0.00726)	(0.00725)	(0.00725)
σ_u	7.318***			
	(0.00848)			
σ_e	3.753***			
	(0.00238)			
SD(Grade)	2.346***			
	(0.0096)			
SD(Grade^2)	0.3289***			
	(0.0024)			
Observations	3416276	3494963	3495076	3495076
R^2		0.666	0.666	0.666

Note: Model 1 is model 6 from table 3 (with the same covariates, though only a selection are shown here) reprinted here for comparison purposes. Student fixed effects (FE) models (2-4) control for only the time-varying variables and include robust standard errors in parentheses. FE models are estimated in Stata with xtreg. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6. Average Math Scores by Peer Poverty Difference Score, Structural Movers Subsample Only

Δ in Peer Poverty between 5th and 6th grade	4th grade average	5th grade average	4th to 5th grade average gain	6th grade average	5th to 6th grade average gain	D in D*
Quintile 1 (change to greater peer pov)	344.3	350.9	6.52	352.9	2.07	-4.44
Quintile 2	346.0	352.6	6.59	354.7	2.16	-4.43
Quintile 3 (minimal change)	347.0	353.6	6.58	355.8	2.25	-4.33
Quintile 4	347.3	353.7	6.48	355.9	2.18	-4.30
Quintile 5 (change to less peer pov)	347.4	353.9	6.51	356.0	2.08	-4.44
Total	346.4	352.9	6.54	355.1	2.17	-4.37

Note: This table only includes students who make structural changes between 5th and 6th grade and have valid math scores for 4th, 5th, 6th and 7th grades. *D in D is the average 5th to 6th grade gain minus the 4th to 5th grade gain. N=213,596

Table 7. Fixed Effects (FE) Models Predicting Math Standardized Test Score Achievement, 5th to 6th grade Structural Movers Subsample

	(1) Stu FE, 4-7 sample	(2) Stu FE, 4-7 sample	(3) Stu FE, 4-7 sample, non-poor	(4) Stu FE, 4-7 sample, poor
Grade 5	6.514***	6.506***	6.485***	6.613***
	(0.0111)	(0.0111)	(0.0148)	(0.0163)
Grade 6	8.668***	8.712***	8.767***	8.715***
	(0.0125)	(0.0150)	(0.0196)	(0.0229)
Grade 7	11.77***	11.79***	11.90***	11.73***
	(0.0128)	(0.0155)	(0.0201)	(0.0239)
Peer Ave Pov Rate	-0.0777***			
	(0.0080)			
PPQ1*Grade 6		-0.144***	-0.169***	-0.0901*
(change to more peer pov)		(0.0257)	(0.0349)	(0.0380)
PPQ1*Grade 7		-0.151***	-0.206***	-0.0514
(change to more peer pov)		(0.0277)	(0.0371)	(0.0413)
PPQ5*Grade 6		-0.138***	-0.217***	-0.0429
(change to less peer pov)		(0.0257)	(0.0353)	(0.0375)
PPQ5*Grade 7		-0.0448	-0.0687	0.0127
(change to less peer pov)		(0.0272)	(0.0368)	(0.0404)
Stu Pov	0.0620**	0.0462*		
	(0.0196)	(0.0196)		
Constant	346.3***	346.3***	349.5***	342.3***
	(0.0080)	(0.00799)	(0.0127)	(0.0146)
Observations	839355	830210	464188	372929
R^2	0.646	0.646	0.656	0.643

Note: This table only includes students who make structural changes between 5th and 6th grade and have valid math scores and peer poverty measures for 4th, 5th, 6th, and 7th grades. Model 3 includes only students from this sample who spend less than 50% of their panel in the free or reduced price lunch program. Model 4 includes only students from this sample who spend 50% or more of their panel in the free or reduced price lunch program. All models control for the time-varying variables shown in table 3. Robust standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8a. Fixed Effects (FE) Models Predicting Math Standardized Test Score Achievement, 4th to 5th grade Non-Structural Movers Subsample

	(1) Stu FE, 3-6 sample	(2) Stu FE, 3-6 sample	(3) Stu FE, 3-6 sample, non-poor	(4) Stu FE, 3-6 sample, poor
Grade 4	6.225***	6.220***	5.479***	6.656***
	(0.0335)	(0.0335)	(0.0576)	(0.0401)
Grade 5	11.84***	11.84***	11.34***	12.16***
	(0.0364)	(0.0433)	(0.0699)	(0.0543)
Grade 6	14.58***	14.63***	14.03***	15.01***
	(0.0644)	(0.0693)	(0.111)	(0.0874)
Peer Ave Pov Rate	0.0251			
	(0.0198)			
PPQ1*Grade 5		0.0825	0.127	0.0451
(change to more peer pov)		(0.0742)	(0.133)	(0.0894)
PPQ1*Grade 6		-0.0996	-0.000188	-0.0948
(change to more peer pov)		(0.0807)	(0.148)	(0.0965)
PPQ5*Grade 5		-0.135	-0.283*	-0.0819
(change to less peer pov)		(0.0746)	(0.127)	(0.0920)
PPQ5*Grade 6		-0.204*	-0.718***	0.0702
(change to less peer pov)		(0.0805)	(0.137)	(0.0988)
Stu Pov	0.119*	0.118*		
	(0.0511)	(0.0511)		
Constant	338.6***	338.6***	343.6***	335.9***
	(0.0288)	(0.0289)	(0.0461)	(0.0407)
Observations	125984	124864	42922	83033
R^2	0.685	0.684	0.686	0.689

Note: This table only includes students who make non-structural changes between 4th and 5th grade and have valid math scores and peer poverty measures for 3rd, 4th, 5th, and 6th grades. Model 3 includes only students from this sample who spend less than 50% of their panel in the free or reduced price lunch program. Model 4 includes only students from this sample who spend 50% or more of their panel in the free or reduced price lunch program. All models control for the time-varying variables shown in table 3. Robust standard errors in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 8b. Fixed Effects (FE) Models Predicting Math Standardized Test Score Achievement, 6th to 7th Grade Non-Structural School Movers Subsample

	(1) Stu FE, 5-8 sample	(2) Stu FE, 5-8 sample	(3) Stu FE, 5-8 sample, non-poor	(4) Stu FE, 5-8 sample, poor
Grade 6	3.014***	3.018***	2.830***	3.221***
	(0.0704)	(0.0705)	(0.113)	(0.0875)
Grade 7	6.075***	6.098***	6.213***	6.109***
	(0.0330)	(0.0397)	(0.0594)	(0.0521)
Grade 8	8.031***	8.080***	8.242***	8.057***
	(0.0361)	(0.0443)	(0.0663)	(0.0584)
Peer Ave Pov Rate	-0.0321			
	(0.0183)			
PPQ1*Grade 7		-0.0644	-0.162	0.00914
(change to more peer pov)		(0.0710)	(0.121)	(0.0882)
PPQ1*Grade 8		-0.287***	-0.117	-0.326**
(change to more peer pov)		(0.0815)	(0.139)	(0.101)
PPQ5*Grade 7		-0.00875	-0.224*	0.0932
(change to less peer pov)		(0.0698)	(0.112)	(0.0889)
PPQ5*Grade 8		0.0502	-0.0765	0.113
(change to less peer pov)		(0.0795)	(0.127)	(0.102)
Stu Pov	0.0473	0.0371		
	(0.0456)	(0.0458)		
Constant	349.1***	349.0***	352.9***	346.5***
	(0.0244)	(0.0247)	(0.0402)	(0.0355)
Observations	103769	101568	39885	62682
R^2	0.492	0.492	0.513	0.487

Note: This table only includes students who make non-structural changes between 6th and 7th grade and have valid math scores and peer poverty measures for 5th, 6th, 7th, and 8th grades. Model 3 includes only students from this sample who spend less than 50% of their panel in the free or reduced price lunch program. Model 4 includes only students from this sample who spend 50% or more of their panel in the free or reduced price lunch program. All models control for the time-varying variables shown in table 3. Robust standard errors in parentheses; $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

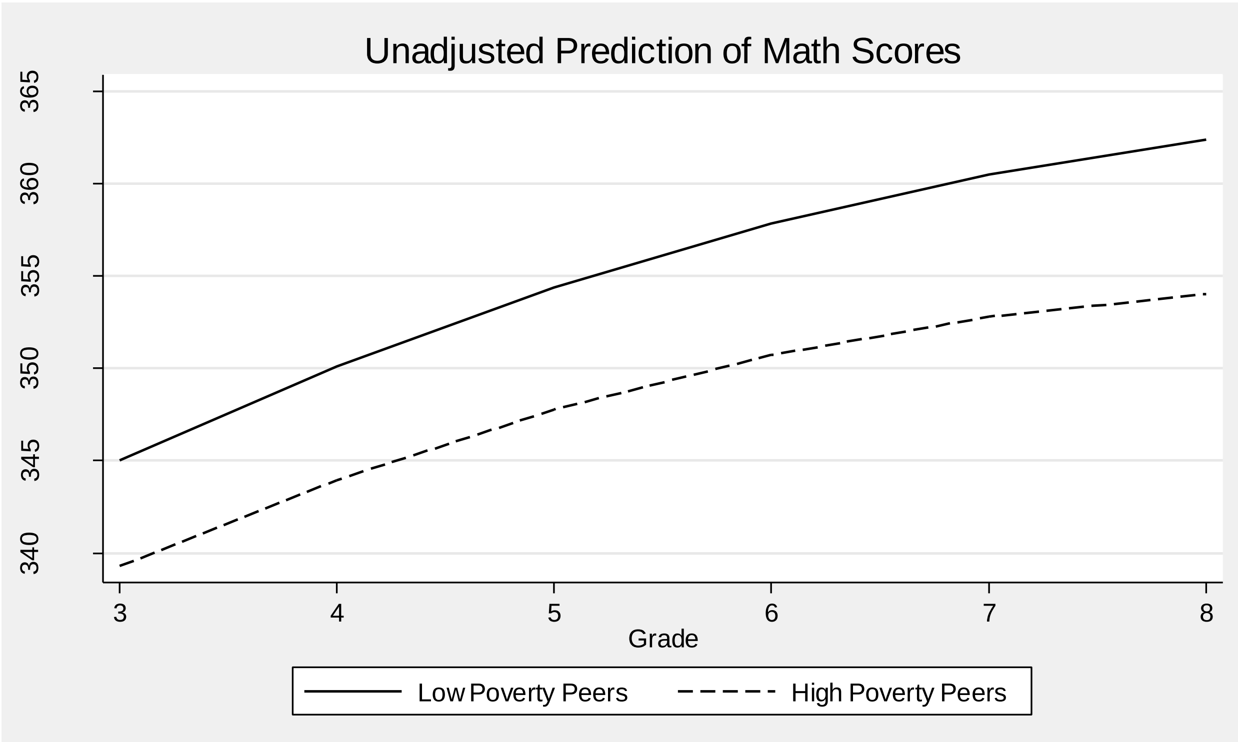

Figure 1. Note: low poverty peers is defined as one standard deviation below the average peer poverty mean and high poverty peers is defined as one standard deviation above the average peer poverty mean.

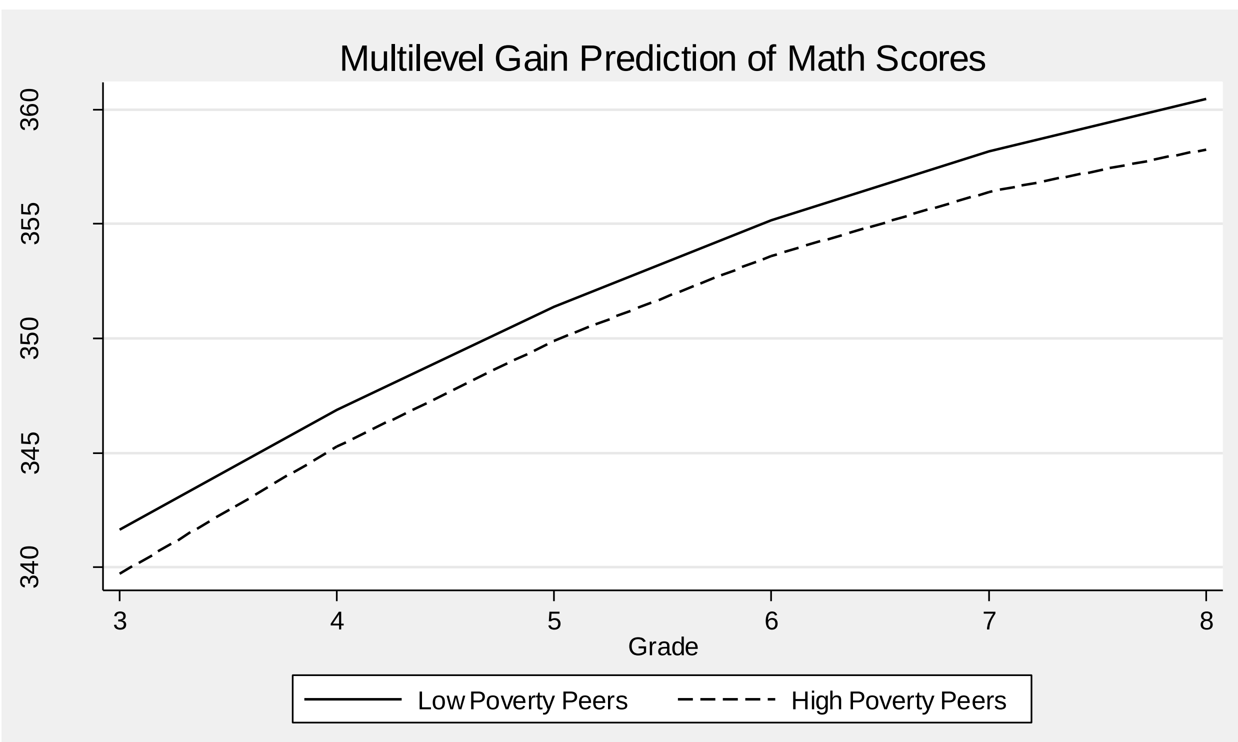

Figure 2. Note: low poverty peers is defined as one standard deviation below the average peer poverty mean and high poverty peers is defined as one standard deviation above the average peer poverty mean.

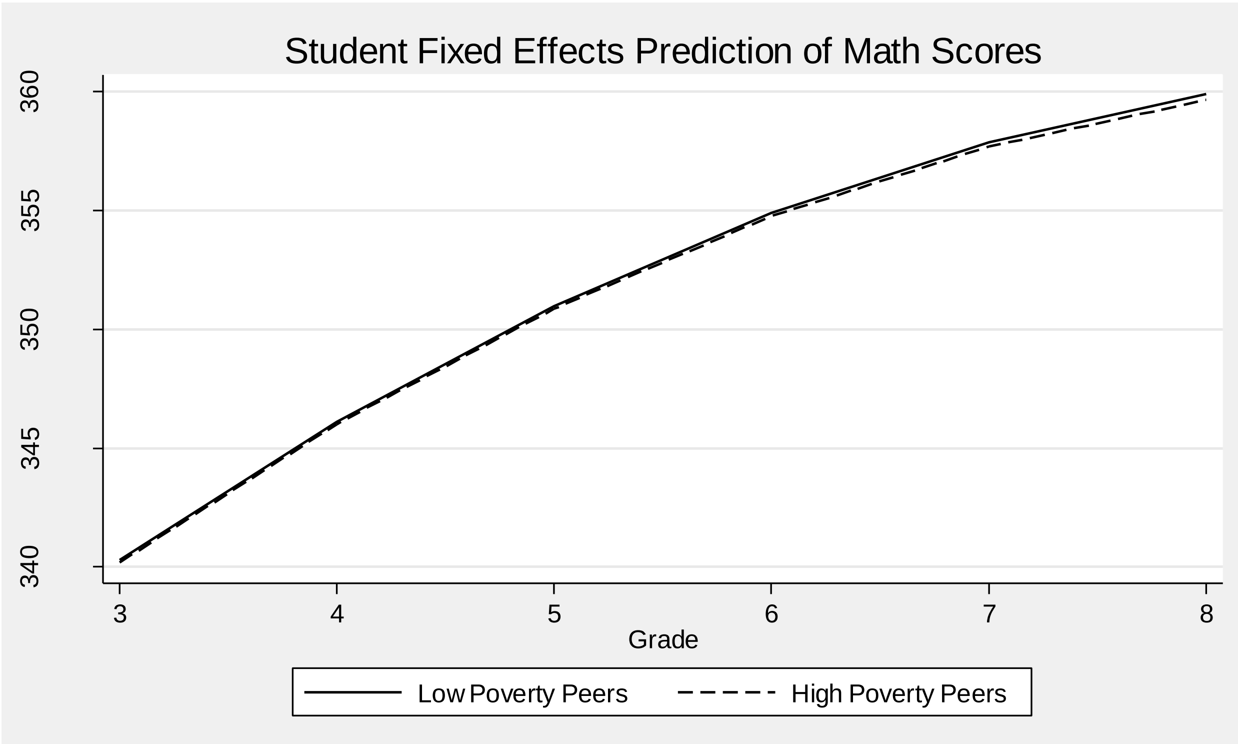

Figure 3. Note: low poverty peers is defined as one standard deviation below the average peer poverty mean and high poverty peers is defined as one standard deviation above the average peer poverty mean.