

**Are Measured School Effects Just Sorting?
Causality and Correlation in the
National Education Longitudinal Survey**

by

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and

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Abstract

Youths who share a school and neighborhood often have similar academic achievement, but some studies find all or most of this apparent effect is due to sorting, not to the neighborhood itself. We present a collage of evidence from the National Educational Longitudinal Survey (NELS) indicating that a significant fraction of the apparent correlation is causal, rather than solely due to sorting.

We first show that the importance of school effects is robust to very rich measures of family background. We then use the fact that the characteristics of the high school that students will attend are an additional indicator of family background. This measure can be used as an instrument to identify family background separately from neighborhood and junior high school effects. Even after this correction, the point estimate of school effects on student achievement remains large and statistically significant. Finally, we use regression and semi-nonparametric matching methods to show that the test scores of youths who change schools begin to converge with those of their new classmates.

Many families routinely pay tens and even hundreds of thousands of dollars to live in desirable neighborhoods with desirable schools. The features that make a school or neighborhood desirable typically include whether youths in the neighborhood study hard and avoid gangs, and whether adults in the neighborhood provide good role models and offer valuable networks of information and contacts. Conversely, disadvantaged families are often clustered in neighborhoods where many youths behave in ways that are not socially accepted, and where early drug use, high rates of teen pregnancy, high dropout rates, and low employment rates are common (Wilson, 1987, 1996).

A key question in the social sciences concerns the extent to which the correlations within a neighborhood are causal. It is possible that youths' success within a neighborhood is correlated merely because the children of advantaged families live near one another. When the correlations are purely due to sorting, then regardless of location, the children would do well because of their families' advantages.

The evidence for the causal impact of neighborhood effects is mixed (see citations below). Some (but not all) careful experiments with vouchers for public housing residents have found important causal neighborhood effects for some outcomes. At the same time, some regression analyses that include unusually complete measures of family background or that use one of several clever instrumental variables have failed to find statistically significant neighborhood results.

This study uses the National Education Longitudinal Survey (NELS) to present a series of analyses that shed light on the causal portion of the correlations. We first show that schools appear to affect outcomes, in that a student's test scores are well predicted by those of his or her classmates. We then show that merely enhancing the measurement of family background with a much richer set of measures than other analysts have used has almost no effect on the role of a school.

Extending the more traditional analyses, we devise a more structural estimate using an instrumental variable. In this analysis, we follow a suggestion made by Edward Glaeser (1996) to use information from people who change neighborhoods to help separate the causal from the merely correlational portions of estimated neighborhood

effects. Our innovation is to use the quality of a future high school as an additional measure of family background. Because the characteristics of a future high school cannot *cause* 8th grade academic achievement, their ability to predict achievement in 8th grade is (under conditions specified below) due to their correlation with measured and unmeasured family background variables. Using this additional measure of family background as an instrument permits an unbiased estimate of the role of family background. This procedure also enables us to obtain estimates of school effects that are not biased by their correlation with family background.

Our final set of analyses demonstrates that the test scores of youth who move to a new school and neighborhood tend to converge with their new neighbors over the next several years. We conduct these analyses using both standard regression techniques and matching techniques.

The next section outlines the theory behind each of these four tests and provides a brief literature review for each. We then describe the data. The results section again steps through the three methods. We conclude with a summary and discussion of our collage of results.

Theory and Methods

Youths' success in school and other spheres of life can be correlated with school and neighborhood characteristics for many reasons. (Jencks and Peterson, 1991; Cook and Goss, 1996; and Duncan and Raudenbush, 1999, provide reviews.) Many of the links occur when classmates act as role models, provide information, create norms, and enforce norms with peer pressure. Similarly, adults in a neighborhood can contribute to youths' success in and outside of school when they act as role models; provide information about schools, families, and careers; and create and enforce norms. In addition, advantaged neighborhoods typically have better infrastructure, such as school quality, due in part to parents' ability to spend time and money and in part to greater political power.¹

At the same time, some of the correlation is surely due to sorting, not causality. That is, the presence of clusters of good role models is due to the choice of people who make good role models to live near each other. (In cases of racial discrimination, the co-

location of disadvantaged groups may not be their own choice (Massey and Denton, 1993).

Formally, we assume that test scores in 8th grade ($test_i$) are depend on the academic ability of students' classmates (as measured by their average test scores, $Test_{JH}$), true family background (FB_i^*), luck, and measurement error (e_i):

$$1. \quad test_i = B_1 \cdot Test_{JH} + B_2 \cdot FB_i^* + e_i.$$

Throughout, variables subscripted JH and HS refer to characteristics of the junior high and the high school. Because we rely on average test scores as our measure of school and neighborhood quality, our procedure attributes to neighborhoods only the subset of attributes of a school or neighborhood that correlate with average test scores. This restrictive set of school and neighborhood attributes is not a problem as long as we focus on test scores as our outcome of interest.

We are concerned that family background is measured with error, so we observe an imperfect proxy for true family background FB_i , where:

$$2. \quad FB_i = FB_i^* + u_i.$$

Conceptually, correctly measuring school and neighborhood effects requires the history of the neighborhoods that a child has lived in (Hanushek, 1986). But as is common in the literature, we measure school and neighborhood quality at a single point in time. Correspondingly, our cross-sectional estimates of the effects of schools and neighborhoods largely measure the role of a lifetime increase in neighborhood quality, not merely short-term changes.

Because of the difficulty in measuring family background (as in equation 2), the most convincing evidence that some of the disadvantages of a poor neighborhood are at least partly causal comes from experiments that moved a subset of disadvantaged families out of a ghetto neighborhood and into the suburbs. In the Gatreux experiment, the Chicago Housing Authority provided rent vouchers that moved a number of central-city Chicago public housing residents into the suburbs or elsewhere in the central city

(Rosenbaum, 1995). Because the assignment of families to suburban or city apartments was almost completely random, the Gatreux experience provided a natural experiment for understanding the gains from housing desegregation.

The children who moved to the suburbs had much better academic success than did those who stayed in the cities. When the children were approximately 18, those who had moved to the suburbs had one-fourth the high school dropout rate of their counterparts who had moved within the poor neighborhoods of Chicago (5 percent vs. 20 percent). Moreover, children who grew up in the suburbs were more than twice as likely to attend college.

Preliminary evidence from similar experiments in other cities suggests that movement away from high-crime areas lowers youths' involvement in some juvenile crime and improves adults' health, reduces rates of crime victimization, and (in some studies) increases employment and earnings (Ludwig, Duncan, and Hirschfield, 1999; Ludwig, Duncan, and Pinkston, 2001; Katz et al., 2000). At the same time, many of the estimated neighborhood effects are not statistically significant, and in some studies better neighborhoods predict worse outcomes. Moreover, these experimental populations are not representative of youths in general because they focus on the very poor and on households that have chosen to participate.

The anthropological literature on very disadvantaged neighborhoods also provides evidence that within-neighborhood correlations are at least partly causal. In the nation's worst neighborhoods, young men are more likely to consider gangs, not school, the dominant institution. In these settings, many students perceive that "playing by the rules" by working hard in school and staying out of trouble with the law does not pay; schools are often quite bad, and employment prospects even after graduating from high school are poor. Moreover, youths are more likely to be punished than rewarded by their peers for academic success. (Cook and Ludwig, 1998, review the evidence for and against this claim.)

The Standard Method

When experimental data are not available, it is necessary to estimate a specification similar to equation 1 with field data. One advantage our regression analyses based on a national sample have over experiments is that they cover a representative sample of Americans, not just those who are eligible for housing vouchers. A disadvantage is that the results are often biased because of measurement problems (equation 2).

A typical study estimates a version of equation 1 while ignoring measurement error on family background:

$$3. \quad test_i = b_1 \cdot Test_{JHi} + b_2 \cdot FB_i + e'_i.$$

A number of analysts attempt to control for family background using measurable characteristics such as parental education and income. In many cases, substantial correlations with youths' educational attainment remain (that is, the estimate of effects of peers, b_1 , is large even after controlling for many observable features of the family. As the number of controls rises, the effect of the school or neighborhood typically declines and sometimes loses statistical significance (Brooks-Gunn, Duncan, and Aber, 1997).

While interesting, these cross-sectional analyses are unable to disentangle why youths' academic and social success are correlated within a neighborhood (Manski, 1995). Coefficient estimates from equation 3 are subject to two important problems. First, because family background is measured with error, the coefficient b_2 is biased down. Second, family background and school and neighborhood quality are positively correlated. Thus, mismeasurement of family background biases the coefficient on the average test scores upward. While acknowledging these important problems, a number of scholars have estimated versions of equation 3 (e.g., Gaviria and Raphael, 2001; Brooks-Gunn, Duncan, and Aber, 1997; many others have estimated versions in first-difference form).

In spite of these limitations, our first set of results follows this method, taking advantage of the uniquely rich measures of family background in the NELS dataset. Thus, if the estimated effect of peers (b_1) diminishes substantially when we move from a

standard set of controls to the richer set of measures, it is likely that continued improvements in measures of family background may eliminate the estimated effects of neighborhoods and schools.

Incremental Sorting between 8th and 10th Grade

To address the potential biases due to sorting, we need to examine how students are allocated to schools and neighborhoods. Most youths continue on from a junior high school to the main high school it feeds into. The families of some youths move between 8th and 10th grades, enrolling their children in new neighborhood schools. Some youths do not move with their classmates to a junior high's main high school even when their families remain in the same location because they enroll in a nearby private or specialized public high school. Still others may move from a private junior high to a nearby public school. The samples for our next analyses consist of youths who did not continue to the main high school fed by their junior high schools.

A first look at sorting

While the standard method described above looks for evidence of school effects, a complementary analysis examines the extent of sorting. Consider the extreme model where school rejects are solely due to sorting but there is some random error in the initial allocations of students to schools. In this case, when a child moves from school₁ to school₂, the child and his or her family would already look like the students at school₂. In contrast, consider the extreme circumstance where some families move randomly to other schools. In this instance, the child would be no more like his or her new classmates in school₂ than like his or her former classmates at school₁.

To see whether the data approximate either of these extreme cases, we look at how much youths who do not attend their junior high school's main high school resemble students at their new school (*Quality_{ActualHS}*), and at how much they resemble the students in the school they would have attended if they had stayed with most of their classmates (*Quality_{MainHS}*). This regression is purely noncausal:

$$4. \quad Test_i = a_1 \cdot Quality_{MainHSi} + a_2 \cdot Quality_{ActualHSi} + a_3 \cdot FB_i + g_i .$$

The results of equation 4 find that for this sample of youths $Test_i = 0.356 (0.079) *$

$$Quality_{MainHS} + 0.289 (.085) Quality_{ActualHS} + 0.685 (0.063) FB_i, R^2 = .292,$$

where the indices of high school and family background quality are defined in Tables A2 and A3, and standard errors are in parentheses.) This suggests that sorting is important, and below we incorporate this insight in a more formal approach.

Instrumental Variables Approach

More formally, when families move, we assume that parents consider the academic qualifications of their children in choosing a new home. In fact, a poor match between a youth's achievement and that of his or her classmates may help motivate some voluntary moves. We also assume that the quality of the student-school match contributes to the decision to start or stop paying for private school. At the same time, we assume that many factors that led to imperfect matches continue to hold; for example, parents with strong opinions about the importance of school quality will continue to hold those opinions.

These scenarios imply that, on average, youths who do not attend their neighborhood's main high school end up with a better match between their true family background and their high school quality than if they had continued on to the main high school. Thus, high school quality ($Quality^*_{HS}$) is an average of true junior high quality and family background, as well as random shocks, v_i :

$$5. \quad Quality^*_{HSi} = D_1 \cdot Quality^*_{JHi} + D_2 \cdot FB_i^* + v_i .^2$$

We use this model of sorting between junior high and high school in the estimation strategy outlined below. Below we also discuss how the model is affected by changes in family characteristics (for example, if the parents divorce) or academic ability (as measured by test scores), as well as the quality of a student's high school.

Given the measurement problems that plague the estimation of equation 3, several researchers have searched for instrumental variables. The challenge here is to find a

factor that is correlated with neighborhood quality, but is not correlated with the family background or unmeasured variables that affect the outcome measure. Although each of the instrumental variable studies has much to recommend it, none passes this stringent test completely convincingly.

For example, Evans and his co-authors (1992) have instrumented for neighborhood quality using metropolitan-level variables. Using the assumption that parents choose neighborhoods but not metropolitan areas, they have found no significant neighborhood effects. At the same time, the validity of their instruments is open to question. As Duncan and Raudenbush (2001) have noted, the procedures require that characteristics of the city and metropolitan area must not influence youths' success. In addition, the procedures may lead to biased results if families choose their metropolitan area in part on the basis of average school or neighborhood quality.

Aaronson (1998) and Plotnick and Hoffman (1999) have used the difference in neighborhoods that sisters live in to identify the portion of neighborhood effects not due to omitted family characteristics. In some, but not all, specifications they have found that, in the PSID, a sister who spent more of her life living in a relatively advantaged neighborhood had a higher rate of high school graduation. The difficulty with this approach is that many families live in more than one neighborhood type while their children are adolescents. Moreover, families that move to quite different neighborhoods are often motivated by events such as divorce that have independent effects on youths' outcomes. Thus precision was often low, and results changed with modest changes in specification.

Like Aaronson (1998) and Plotnick and Hoffman (1999), we use information from school changers to distinguish the effects of family background from those of the neighborhood. Like these studies, our sample for identification includes only "movers." In our procedure, movers include both youths whose families change residence between junior high and high school and youths who do not attend the high school most of their junior high classmates attend.

In our instrumental variable approach, our identification strategy is the converse of Aaronson's (1998) and of Plotnick and Hoffman's (1999). They use the fact that neighborhood location is partly causal (net of family background) to identify the true effect of location. We use the fact that high school quality is due partly to family background (net of junior high effects) to identify the true effect of family background. Importantly, we use information about the schools that youths do not yet attend; this implies that high school quality measures only family background, not the causal effect of schools and neighborhoods.³ In a related study, Glaeser et al. (1996) control for neighborhood fixed effects and look at average differences in youths' behavior over time; we control for junior high school and neighborhood effects and look at individual changes in neighborhood and junior high surroundings.

Our identification strategy uses the same source of identifying variation used by Gaviria and Raphael (2001). Gaviria and Raphael (2001, table 5) find that youths who move to a new high school report smoking, using drugs, and other behaviors at roughly similar rates to students in the new high school. They interpret this result as mixed evidence of sorting (which they associate with a higher correlation for movers than for those who stay). In contrast, we read their evidence as consistent with substantial incremental sorting between junior high school and high school, in that youths not yet attending a high school already resemble those who will be their future peers. Throughout this paper, we study the influence of school-based interactions on a sample of junior high school students. In contrast, much of the literature on peer effects analyzes neighborhoods, often measured for convenience by census tracts. Schools are more natural units of analysis young teens spend many of their waking hours at school and much of their free time with peers who are also classmates. For example, Gaviria and Raphael (2001) document the importance of within-school friendships in the NELS dataset we study. To the extent that students who live near each other attend different schools, it is likely that their parents' priorities and their own capabilities and preferences are more similar to those of their classmates than to those of their neighbors. For example, a student at an elite private school probably has more in common with and is more influenced by his or her classmates than by neighbors. Moreover, his or her parents

probably have more in common with his or her classmates' parents than with neighbors. Below we refer to school and neighborhood effects interchangeably because school and neighborhood go together for the vast majority of the sample; thus our measures cannot distinguish the individual effects of the two sets of influences.

Identifying family background

While the previous three tests are intended to be interesting and descriptive, we now add more structure to the model. Under the assumptions we outline, our strategy will lead to consistent estimates of the causal effects of neighborhoods.

Because many youths move to a high school that fits their characteristics more closely than did their junior high's main high school (as in equation 5), high school quality is a potential instrument for family background. To see that our measure of high school quality, $Quality_{HS}$, is an appropriate instrument, note that it correlates with our measure of family background (FB_i) on the assumption that students who do not attend the high school of their junior high peers partially sort themselves in the years between 8th and 10th grade (equation 5). That is, students who appeared advantaged relative to their junior high classmates on average attend above-average high schools. We will show that this correlation holds in the data.

High school quality must also correlate with true family background (FB_i^*) but not with measurement error on family background (u_i); that is, as is assumed in equation 5, we need sorting to reflect true family background, not merely the observable portion of it. This assumption is plausible, given the positive correlations of high school quality and observable family background that we report below.

Less obviously, we need $Quality_{HS}$ to be uncorrelated with academic ability that is not due to school quality or family background (e_i : equation 1). This assumption would not hold, for example, if high-achieving students (given their observable characteristics) were more frequently accepted into and given scholarships to selective high schools.

We performed two analyses that suggest this potential problem is not serious. First, we estimated the sorting equation 5 and included an estimate of e_i (specifically, the residual from estimating equation 3 using our index of observable family background,

FB_i). The coefficient on the residual was small and not significant. Second, we reran all analyses, dropping students who attend high schools that are most likely to be selective (specifically, private high schools that are not Catholic). This restriction never affected the results.

Catch-up at new schools

If the estimated effects of school quality are largely causal, then the test scores and other outcomes of a youth's move to a new school should quickly converge to the outcomes of the youths already there. Intuitively, if a student attends a somewhat better high school (in terms of academic achievement) and quickly attains the academic level of his or her new classmates, it appears that school effects are largely causal. We estimate the changes in test scores of students who do not attend the same high school attended by most of their junior high classmates:

$$6. \quad Test12_i = c_0 \cdot Test8_i + c_1 \cdot Quality_{MainHS_i} + c_2 \cdot Quality_{ActualHS_i} + v_i,$$

where $Test12_i$ and $Test8_i$ are the student's achievement scores in 12th and 8th grades respectively.

Hanushek and his colleagues (forthcoming) report a similar specification. Consistent with the theory we present here, they find significant catch-up among students who move to a new school of markedly higher or lower quality than their previous school. Their very large sample permits them to look at the effect of the changes in the test scores of peers (by using variation in classmates who move in addition to those who simply change schools). Moreover, they find convergence when new students arrive in an existing school, in that students who do not change schools have test scores that slowly converge with those of their new classmates.

A youth's test scores could appear to converge with those of his or her new classmates regardless of the effects of peers and a new school if the move to a new school is due to a shock to the student's family. For example, if parental divorce leads to a job loss, then a family may move to a less advantaged neighborhood and a youth may not learn as much in high school; this correlation would not necessarily be due to the

neighborhood change. Thus we rerun these results on families without a divorce or remarriage and control for changes in family income.

Case-control matching

It is possible that schools have different effects on youth with different ability and achievement levels (as measured by test scores). For example, a small high school might be best for the average child in a junior high, but might not have the enrichment or advanced courses that are important for the best students and that are only available at a large high school. To see if these factors are important, we match each youth with a student who has very similar academic achievement in 8th grade.

For each child who left a junior high, we chose another child in that junior high with the closest test scores in eighth grade. We then ran the case-control regression controlling for which child moved away from the main high school (*mover_j*) fed by that junior high *j*:

$$7. Test12_j = c_0 Test8_j + c_1 Quality_{HSj} + c_2 mover_j + \text{fixed effects for each matched pair}_i + error_j.$$

Measurement Issues

Family background: Each of our constructs, such as family background, consists of a vast array of attributes, including family income, parental education, family structure, and many others. To facilitate estimation of the instrumental variable technique, we create an index of family background equal to the best predictor of test scores. That is, we run:

$$test_i = C * X_i,$$

where *X* is the vector including family income, parental education, and so forth. The complete variable list and results are listed in Table A1. The predicted value of test scores is used as the index of observable family background:

$$FB_i = \hat{c} * X_i.$$

Results were similar if we created the index of family background as the first principle component of the various measures.

Junior high quality: When calculating observable junior high school quality ($Test_{JH}$) for student i , we use the average test score of all students other than student i .

The measures of high school quality used as instruments: We typically have only one test score for youths who move to an alternative high school. Thus we use the characteristics of that high school to create an index of quality by regressing school characteristics against student test scores and using predicted test scores. The list of characteristics includes the proportion of students receiving subsidized school lunches, proportion minority, and proportion living in single-parent families. Table A2 includes the complete variable list. The school characteristics are measured when the youth is in 10th grade.

Estimating standard errors: The dataset has multiple observations within a junior high school. We estimate jackknifed standard errors that correct for the clustering of the data (Stata Press, 2001: 15).

Data

The National Education Longitudinal Study of 1988 (NELS) was sponsored by the National Center for Education Statistics and carried out by the National Opinion Research Center. NELS was designed to provide trend data about critical transitions experienced by young people as they develop, attend school, and embark on their careers. The base year (1988) survey was a multifaceted study with questionnaires for students, teachers, parents, and the school.

Sampling was first conducted at the school level and then at the student level within schools. The data were drawn from a sample of 1,000 schools (800 public schools and 200 private schools, including parochial institutions). The three follow-ups revisited (most of) the same sample of students in 1990, 1992, and 1994; that is, when the respondents were typically in the 10th grade, in the 12th grade, and roughly two years after high school graduation. We use data from the 1988 and 1990 surveys, and obtain a

sample of approximately 14,000 students. After dropping students from junior high schools in which there were too few students, we use a sample of 11,939 for the analysis.⁴

Defining the movers sample: The NELS started with an average sample of 25 students per junior high school, sampled in 8th grade in 1988. Matching between youths and schools (as best we can observe) is imperfect in 1988. Moreover, junior high schools contain substantial heterogeneity. Thus, in a world of no transaction or moving costs, it appears that many youths could improve the match between themselves and their school [by choosing a specific high school]. Nevertheless, in the 1990 survey, the majority of students from each junior high attended a common high school. Many forces ranging from the transaction costs of selling a home to the costs of changing jobs for parents to the social disruption parents and children suffer lead most parents not to move during the years when the focal youth is in 8th to 10th grade, even if the family-school match is imperfect. Thus we restrict our sample to “movers,” those youths who did not attend the main high school.

We first identified junior high schools in which a plurality attended a single high school but at least one student attended a different high school.⁵ We termed this high school the main high school. Drawn from these junior highs, our sample consists of the youths who did *not* attend the main high school. In addition, we dropped junior highs with fewer than eight students because we were not certain of being able to correctly identify the main and alternative high schools in this sample.

Socioeconomic status and family background: In the construction of the index of family background, a number of variables are used that previous research has determined are important predictors of youths’ educational attainment and behavior. The NELS contains multiple measures of family background and family involvement in education that many studies lack.

The measures of socioeconomic status are created from both parent and student questionnaires. The set of variables includes occupational status (using Duncan’s index), parental education, and family income. These variables are converted into z-scores with mean zero and standard deviation equal to one. When there are missing values for

parental education because of a missing parent, these are given a z-score of 0, and categorical variables are included to note these missing values. We also include indicators of whether in 8th grade a youth lived in an intact family, a single-parent family with the biological mother, a single-parent family with the father present, a step-family with either the biological mother or father present, or a family with no biological parent present.

From the student questionnaire, we include the youth's gender, whether a foreign language is spoken in the home, whether the mother or father is foreign-born, the number of siblings, and whether the home has a library card, magazines, and many books.

From the parental questionnaire, indicators include whether the family is one of five religions and one of four levels of religious observance. These variables proxy for how close-knit a family is and for the social capital available to the children. We also controlled for whether the mother was a teen when the student was born. Three variables partially capture parents' involvement in the student's life and education: whether the parent belongs to a parent-teacher association or related organization or volunteers at school; whether the parent helps the child with homework; and whether the child participated in clubs such as Boy or Girl Scouts during elementary school.

Dependent variable: The dependent variable is the student's test score, aggregated from a set of cognitive math and reading tests taken in eighth grade (see Levine and Painter, 1999, for a description of the cognitive tests). The tests have high reliabilities. The reliability of each subscore (measured as 1 minus the ratio of the average measurement error variance to the total variance) is greater than 0.80, and often near 0.90 (Rock and Pollach, 1995: 67). We use the sum of the reading and math subscores, further increasing reliability.

Results

Means and summary statistics are presented in Table 1. We analyze data on 11,939 students in total. Many of our analyses focus on the 886 who do not attend the main high school of their junior high. In total we analyze movers from 265 junior high schools who attended 321 high schools.

Who moves?

Our identification strategy in several of the methods depends on the accuracy of our model of changing schools (equation 5). Thus we examine these moves in some detail.

Eighty percent of the entire sample went to public school in both 8th and 10th grade. In contrast, among our sample of school mover, roughly half went to public school in both 8th and 10th grade. Corresponding to the higher rate of attendance at private schools, family incomes (though not parental education) are significantly above average (.2 standard deviations) for school movers.⁶ (In contrast, Hanushek, Kain, and Rivkin, 2001, find that those who changed schools were slightly less economically advantaged than the average family.) Below we discuss how well our results are likely to hold for the overall population.

In Table 2 we present the rates of transition between different types of junior high and high schools. We categorize schools as public, Catholic religious, other religious, and other private categories, and we tabulate the results separately for families that moved and those that did not.

Of the students who started in public junior high schools in 8th grade, 93 percent remained in public schools. Half of the students in Catholic junior highs and 60 percent of those in other private religious schools ended up in public high schools. Among those who attended private non-religious schools in 8th grade, a lower rate (about one-third) switched to a public high school.

The standard method

If we regress 8th grade test scores on junior high average test scores, the coefficient on school mean test scores is 0.82 (Table 3, col. 1). The most common way social scientists analyze the effects of neighborhoods while netting out the sorting of family background is to control for observable features of family background. A number of studies use a narrow set of controls found in the U.S. Census data on census tracts and blocks: race, education, income, and teen mothers (see Case and Katz, 1991; and O'Regan and Quigley, 1991). Controlling for the standard measures found in the census

such as race and family income reduces that coefficient to 0.61 (Table 3, col. 2). These additional controls raise the R^2 of the regression from 11 to 26 percent, which is large but leaves substantial variation in individual test scores unaccounted for.

Many analysts have hypothesized that this residual variance is due to unobserved family background. The key question is whether additional measures of family background would make a difference. When we add 24 additional controls, the R^2 rises to 31 percent and the coefficient on peer test scores declines to .56. The rise in R^2 is statistically significant, while the decline in the point estimate on peer test scores is not. The coefficient on peer test scores remains very large and highly statistically significant ($t > 14$, $P < .001$). Thus, unlike in several previous studies, better measures of family background do not drive the coefficient on peer effects toward zero or statistical insignificance.

Even with much better controls for family background than most past researchers have been able to use, substantial variation across schools remains. These results are consistent with the hypothesis that neighborhoods are very important in shaping the academic achievement of youth.

The instrumental variables method

Results from a standard version of equation 3 are presented in Table 4, using family background and school quality to predict test scores. Column 1 uses the entire sample, while column 2 presents results using the sample of students who will not attend the main high school; this smaller sample is used in the analysis below. Both the index of family background and the average test score at a junior high school strongly predict student achievement. (Recall that these indices omit the characteristics of the focal student.) In the sample of movers, the coefficient on junior high quality equals 0.52 and the family background coefficient is 0.64.

As noted above, the estimated effect of junior high quality is biased up by mismeasurement of family background, which permits some of the true effect of family background to be included in the junior high effect. The next column presents the main results of this section. We address measurement error on family background by using our measures of high school quality as instruments for family background.

Table A2 presents the first-stage estimates predicting the index of family background with junior high average test scores and the list of high school characteristics. The inclusion of the full instrument list passes standard tests of overidentification, and results are largely invariant to the choice of instruments. Moreover, the instrumental variables had an F statistic of 15.91, $P < .001$ (after including junior high average test scores), suggesting sufficient fit for useful estimates.

When we instrument for family background, the coefficient on family background rises from 0.64 to 0.82 (Table 4, col. 3). Although this increase is not statistically significant, its direction is consistent with our prior beliefs about the importance of measurement error on family background.

Because family background is correlated with junior high test scores, a key question is whether this larger effect for family background eliminates the effect of school quality. The answer is no. When we instrument for family background, the coefficient on average junior high test scores declines from 0.52 to 0.42. Although this decline is substantial, it is not statistically significant and the majority of the estimated effect of school quality remains.

The remaining coefficient on average test scores of 0.42 is both economically large and statistically significant. That is, it is rational for parents who grew up in an average neighborhood to pay a substantial sum so that their children are surrounded by peers who have test scores one standard deviation above average (roughly a two standard deviation increase in neighborhood average test scores). Our results imply that such a move would raise their children's academic achievement by 0.42 of the test's standard deviation.

Because the instrumental variable estimates rely on the assumptions discussed above, we now consider several potential weaknesses of the findings.

Catch-up at new schools

The tests in this section are based on the assumption that if schools and neighborhoods have a largely causal effect on a youth, then a youth moving to a new high school should rapidly begin to perform like the youths who are already there.

We first examine the sample of youths who do not attend their junior high

school's main high school using the model in equation 6. We regress 12th grade test scores on 8th grade test scores, an index of the student's current high school characteristics, and an index of the main high school characteristics (the school attended by most former classmates).

The results are consistent with catch-up in test scores (Table 5). If a youth's high school is two standard deviations better than his or her junior high, the youth typically gains about one-third of a standard deviation unit in test scores between 8th and 12th grades ($P < .01$).

We can perform a back-of-the-envelope calculation to compare these results with those in the first method. Assume the effect of school quality on test scores is linear with years of exposure to a better school and that the effect size is similar in all grades. Using these strong assumptions, we can compare this effect size with that of the first method in Table 3, where we looked at the variation in school fixed effects while controlling for a rich set of family background variables. The results in Table 5 indicate that a one standard deviation improvement in school quality for eight years raises scores by .34 standard deviation units. This result is essentially identical to the .35 standard deviation unit increase predicted in Table 3. The two estimates are based on completely different sources of variation, so it is remarkable how similar the results are.

These estimates of how rapidly students at a new school rapidly catch up to the new school's academic achievement are intermediate in the current literature. Our estimates are larger than those in the experimental studies that have looked specifically at disadvantaged families (e.g., Ludwig, Ladd, and Duncan, 2001). At the same time, these results are a bit smaller than Hanushek et al. (forthcoming) find in a representative sample of Texas elementary school students. The results in Texas indicate that a one standard deviation increase in school quality (as proxied by the average test scores of classmates) for eight years raises test scores by between .42 and .67 standard deviations. It is possible that the larger results in Texas are due to the younger age of that sample.

Results from matching methods

The basic results of the matching estimators are consistent with the regression results: On average, students who attended a better high school than their junior high

classmates did had small but statistically significantly better test score gains than the students who attended worse schools.

To keep the matches close, we discard any mover who had no junior high classmate with a standardized test score within 0.4.⁷ Not surprisingly, when we match on initial test scores within a junior high the initial test score means are very close. Nevertheless, going to a high school with higher quality increases a student's test scores significantly (Table 5, col. 3). This implies that four years later the student who ended up at the better high school had standardized test scores .066 higher (SE = .031, $P < .05$) than the control group. Although not enormous, this gap in growth rates is statistically and economically significant. Moreover, results were similar for both the most and the least advantaged youth (as measured by quartile of 8th grade test scores or by family background).

A possible limitation in matching using only test scores is that an advantaged youth who had an unusually low test score due to transitory factors will both match a less advantaged student and on average attend a better high school. Mean reversion in test scores will then show the advantaged youth to have higher average test score gains. To reduce the importance of such advantages, we used a within-school propensity score matching method. In the appendix, we describe this propensity score matching method, which matches each mover with one youth who remained at the main high school, with matching based on the predicted probability of attending the better high school. Results were very similar to results from the case-control matching.

Robustness Tests

In this section, we briefly describe robustness checks on the results above.

Are results from movers representative?

An important concern when studying just the population of movers is that they might be quite different from the population of those who continued on to the main high school. For example, consider a population where 10 percent move every other year and 90 percent rarely move. For the frequent movers, current school characteristics are poor proxies for the schools and neighborhoods they have lived in their whole lives. For this

group, more than for the geographically stable group, family background proxies for past peer effects. Thus, in equation 3, the estimated family background effect should be larger and the current school effect should be smaller for the frequent movers. Moreover, if we look at moves after 10th grade (for example, between 10th and 12th grade), those who moved between 8th and 10th grade will be far more likely to move than those who did not move in the previous two years (35 vs. 11 percent). Further, it is possible that those families that do not expect their children to be affected by a neighborhood may choose relatively disadvantaged neighborhoods (Duncan and Raudenbush, 1999). If this is true for our sample of non-school changers, then our results would overstate the neighborhood effects on the typical youth.

In short, while selection bias remains a concern, the sample we analyze does not appear to self-select visibly in a way that would lead to unrepresentative coefficients. Not only are the family backgrounds similar among stayers and school changers, as discussed earlier; in addition, the effects of observed average junior high school test scores and family background in predicting test scores (equation 3) were similar for movers and stayers (Table 4, cols. 1 and 2).

Sampling error on test scores

Because the NELS samples roughly 25 students per junior high, these school means have sampling error. To address this problem, we used the mean from half the sample as an instrument for the mean from the other half. This instrumental variable technique does nothing to solve the causality problems related to mismeasured family background but should reduce mismeasurement of test scores and, thus, mismeasurement of the effects of school and neighborhood quality. With this technique the coefficient on school mean test scores increases by about 20–25%, but the pattern of the results is the same as reported in the rest of the paper.

Standard method

Instead of using a school's average test scores, we can use a fixed effect to capture school and neighborhood variation. If we regress 8th grade test scores on junior high fixed effects, the standard deviation of school mean test scores is .525, about half as big as the standard deviation of individual scores (Table 3, col. 1). If we control for the

variables in standard census regressions, the standard deviation of the average residuals across neighborhoods drops to .365 (Table 3, col. 2). If we add our complete set of covariates from Table 3, the standard deviation of the average residuals across neighborhoods drops only a small amount, to .351. Thus our results replicate those presented in Table 3: richer measures of family background do not eliminate the between-school variation we observe.

Solon and Page (1999) note that much of the estimated relationship between neighborhood and child outcomes is dependent on which metropolitan area one comes from. Their point estimates indicate that neighborhoods play a small role. At the same time, their estimates are not precise and cannot rule out rather large neighborhood effects. We reran our results on equation 1 separately for rural and urban regions, and they were roughly unchanged.

Finally, we added a number of potentially endogenous variables to equation 3 to test their impact on the school-based peer effect. This list of variables included a student's behavioral or emotional problems, truancy, and parental expectation of future educational success. None of these interesting variables had a statistically significant impact on the peer effect.

Testing the selection model

Our model of school selection assumes that for youths who do not enroll in the main high school, their junior high and family characteristics, but not their idiosyncratic academic ability conditional on those two factors, determine their high school. This assumption is safest for the one-fourth of school changers whose families changed residences between their 8th and 10th grades (218 out of 886).

One test of whether our sorting model is appropriate examines whether estimated idiosyncratic test scores (conditional on observed family background and junior high quality) are useful in predicting eventual high school quality. If the correlation between idiosyncratic test scores and the residual from the sorting in equation 5 is positive, youths may move in part on the basis of their academic success. Such a selection rule might bias the estimated effect of family background upward when using eventual high school quality as an instrument for family background.

Both observed junior high quality and observed family background predict later high school quality, consistent with equation 5 (see endnote 2). We reran this regression for each cell of the transition matrix with at least 20 observations, and the results were qualitatively similar (results available on request). Moreover, in almost all of the transitions, idiosyncratic test scores (conditional on observed family background and junior high quality) are not useful in predicting eventual high school quality. The exception is for those moving from a private Catholic junior high to a public high school. As a robustness check, we reran all results below (results available on request), omitting youths who moved from private Catholic junior high to a public high school; the results were unchanged.

One final concern is that some youths moved from private junior high schools to public high schools to take advantage of the public schools' larger size and, thus, higher number of advanced classes. For example, even if the mean test score of a public high school is the same as or lower than that of the corresponding private high school, if the public school is larger, its highest-scoring students may have even higher test scores than the private school's, as well as more advanced classes for this upper tail. Such sorting would call into doubt our one-dimensional metric of school quality, because students with different academic backgrounds might have very different experiences at one large high school. In contrast to this hypothesis, students were no more likely to move to a school that had more advanced placement (AP) classes.

Instrumental variables estimation

For the instrumental variables estimation, we restricted our sample to junior high schools with larger samples (10 or 12 per junior high), instead of using a cutoff point of 8. As noted above, we restricted the sample to students who did not attend selective high schools (specifically, we eliminated private schools unless they were Catholic). Again, the results were unchanged.

The junior high average test scores were from a sample of students, not from the student body as a whole. This sampling error can attenuate the bias of the coefficient on average test scores. Using the mean from half the sample to instrument for the mean test

score from the other half can resolve the problem. When we implemented this correction for sampling error, the results were almost unchanged.

A youth's test scores and his or her junior high's average test scores can be correlated by common measure bias. For example, a high-quality school may de-emphasize skills measured with paper-and-pencil tests. In that case, both a student's scores and that of his or her peers will be misleadingly low (given family background), and the coefficient on average test scores in a junior high may be biased down. Working in the other direction, the average test score is from a sample within each school, and sampling error tends to cause a downward attenuation bias.

It is possible to solve both problems if we use characteristics of the junior high school (the proportion poor, with single-parent families, etc.) as instruments for average test scores. Unfortunately, using these instruments can introduce new biases if characteristics of the junior high correlate with unmeasured family background. In that case, the instruments will be imperfect, and a modest amount of failure of the assumptions of instrumental variable estimation can lead to large biases (Bound, Jaeger, and Baker, 1995). In any case, the results were unchanged using a list of characteristics of the junior high school to instrument for average test scores.

As we described above, some school changes between 8th and 10th grades may be due to students' achievement in 8th grade, which, for example, allowed them to enroll in selective high schools. This selection of schools should be less important if family changes such as divorce or getting a new job led to the move. Perhaps due to our limited sample size of movers, results were not statistically distinguishable when we divided the sample by the distance of the move (same city vs. new city) or by the cause of the move (when we could identify divorce, job change, or job loss).

We repeated this analysis on a number of other youth outcomes, such as having behavioral problems reported by teachers or parents, and self-reported cigarette smoking and drug use. We found that the same methodology that we applied to test scores did not apply to any of these outcomes. While the coefficient on the school-wide average of each behavior was statistically significant, the coefficient on the index of family background was not. This result suggests that sorting is not important for these outcomes. These

results support Gaviria and Raphael's (2001) interpretation of their finding that youths engaging in a number of undesirable behaviors in 10th grade tended to be enrolled in the same school. That is, if observable family background does not predict these behaviors, then unobserved family background probably has a weak relationship as well. Even though Gaviria and Raphael (2001) did not explicitly account for sorting based on family background, their results are not substantially biased.

Catch-up

We entered lagged test scores as a control variable in the regression and the results were unchanged. Because students who change schools score lower on standardized tests in the year after the move (as documented by Hanushek, Kain, and Rivkin, 2001), we dropped families that moved in the twelve months prior to the second test administration. The results were unchanged.

Alternative measures of test scores

The tests above used the combined reading and math scores as a composite test score. We reran all tests on the reading and math subscores; the results were similar.

Discussion

We have used three different measures of the role of schools and neighborhoods. Each is subject to biases typically because we can never identify all the forms of family background that might affect the school a youth attends. At the same time, the methods use quite different sources of identification and do not all suffer from that main bias. Thus, together they provide a fairly convincing collage of evidence that schools and neighborhoods do, in fact, matter substantially for youths.

Summary

Controlling for a good measure of family background, a one standard deviation increase in school test scores raises a student's test scores by 0.52 standard deviations (Table 4, col. 2). To the extent that unmeasured family background is correlated with neighborhood quality, this estimate partly captures the effects of both. Thus the coefficient is likely to be biased upward. Importantly, most causal theories of

neighborhood effects imply this bias is guaranteed to be present, because the true school effects are due largely to the sorting of families.

Our first method asks whether better measures of family background would make a difference. The answer here is clear: Even with far more detailed information about the family than economists usually analyze, school effects are measured as having essentially the same magnitude as when only the standard controls are used.

Our next test uses an identification strategy and instrument that allows a more precise estimate of family background than previous studies. The cost of this precision is the need for very strong structural assumptions. This technique implies that the causal effect of a one standard deviation increase in peer test scores is about 0.42 (Table 4, col. 3). On the one hand, this effect is substantially smaller than the 0.52 estimated in column 2. On the other hand, the estimate is substantively large and suggests neighborhoods do matter, if somewhat less than standard estimates indicate.

Our final method asks whether youths who move to a better high school than most of their peers “catch up” with the academic achievement of their new high school peers. Using both standard regression methods and matching techniques, we find substantial catch-up – our most direct evidence that schools and neighborhoods matter.

Implications

Many readers will already have made important decisions based on the intuition underlying these results. For example, many parents pay substantial amounts to live in neighborhoods with advantaged neighbors. Presumably, higher-cost real estate reflects the ideals of better schools, better peers, and better role models for their children. If the estimated neighborhood outcomes are completely noncausal, such amenities are valueless; that is, the children’s expected achievements would be unchanged if they grew up in a much less advantaged neighborhood. Moreover, urban policies such as the Moving to Opportunity program focusing on deconcentrating the poor would not help poor youths (see the citations in Ludwig et al., 2001; and Katz et al., 2000, for descriptions of this program).

The results here indicate that parents (and others) who pay extra to live near advantaged neighbors are buying valuable improvements in their children's education. Moreover, the concerns of urban policymakers that the government warehouses the poor in massive housing projects are similarly well grounded.

It is important to note what we have not identified in this study. Even if schools and neighborhoods matter, these results tell us nothing about the causal mechanisms. A youth's school or neighborhood could matter because peers influence each other. In such a model, interventions to stop one child from drinking, for example, have multiplier effects throughout the peer group. In contrast, if a youth's neighborhood matters because nearby adults act as role models or because institutions are better, these social multipliers for youth interventions are absent (Manski, 1995). Finally, schools can matter if school and parental policies and institutions are effective. Our estimated school effect captures the sum of these forces. It is left for future research to measure the importance of each channel and to identify cost-effective policies that will improve the lives of all youths.

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Table 1: Describing the Data: Sample Sizes and Summary Statistics

<i>Sample Sizes</i>		
(sample = schools with at least one mover and at least eight students per junior high)		
	Number of students per junior high school	Number of movers from junior high school
Minimum	8	1
Median	15	3
Mean	15.3	3.3
Maximum	54	21
Standard deviation	7.0	4.5
Total students	11,939	886
No. of junior high schools	781	265 (with at least one mover)
<i>Summary Statistics</i> (N = 886 youth who moved)		
	Mean	Standard Deviation
8 th grade test score (Test _i). This variable was z-scored to mean zero and s.d. = 1 for the entire NELS sample.	0.138	0.991
Index of family background (details in Table A1) (FB _i)	0.117	0.531
Junior high average test score (excluding the focal individual, Test _{JH})	0.143	0.514
<i>High School Characteristics</i>		
Percent of school that receives reduced-price lunches ^Q	1.262	0.876
Percent of school that is non-minority ²	3.186	1.420
Percent of school that come from single-parent households ^ϕ	1.644	0.698
Percent of school that has English as a second language ^{>}	1.411	1.211
Percent of previous year's 12 th grade class that dropped out	7.183	9.863
Percent of previous year's graduating class in a 4-year college	53.952	28.767
Percent of previous year's graduating class in a 2-year college	18.568	13.751

^Q This variable is measured on a 0–3 scale (0 = none, 1 = 0–10%, 2 = 11–50%, 3 = 51–100%).

² This variable is measured on a 1–5 scale (1 = 0–25%, 2 = 26–50%, 3 = 51–75%, 4 = 76–90%, 5 = 91–100%).

^ϕ This variable is measured on a 0–5 scale (0 = none, 1 = 1–24%, 2 = 25–49%, 3 = 50–74%, 4 = 75–99%, 5 = 100%).

[>] This variable is measured on a 0–5 scale (0 = none, 1 = 1–9%, 2 = 10–19%, 3 = 20–29%, 4 = 30–39, 5 = 40–100%).

Table 2: Transition Matrix for the Sample of School Changers

School in 8th grade	<i>School in 10th grade</i>			
	Public	Catholic	Private, religious	Private, non-religious
Public	456	28	6	3
Catholic	114	144	0	1
Private, religious	33	7	7	5
Private, non-religious	28	10	9	35

School in 8 th grade	<i>School in 10th grade, after changing residence</i>			
	Public	Catholic	Private, religious	Private, non-religious
Public	145	1	1	0
Catholic	21	23	0	0
Private, religious	8	5	0	1
Private, non-religious	6	0	1	6

School in 8th grade	<i>School in 10th grade, after not changing residence</i>			
	Public	Catholic	Private, religious	Private, non-religious
Public	311	27	5	3
Catholic	93	121	0	1
Private, religious	25	2	7	4
Private, non-religious	22	10	8	29

Table 3: The Standard Method

	(1)	(2)	(3)
Dependent Variable = test scores in 8 th grade ($Test_i$)	OLS	OLS Control for Census measures of family background	OLS Including all controls for family background
Average test scores, $Test_{JH}$	0.817 ** (0.016)	0.606 ** (0.030)	0.557 ** (0.030)
Family background measure	None	Census controls listed in table A1	All controls listed in table A1
Adjusted R ²	0.179	0.262	0.313

Table 4: Instrumental Variable Estimation of School-Based Peer Effects

	(1)	(2)	(3)
	OLS	OLS	Instrumenting for family background
Dependent Variable = test scores in 8 th grade ($Test_i$)	Full Sample	Movers Sample	Movers Sample
Average test scores, $Test_{JH}$	0.444** (0.016)	0.523** (0.054)	0.428** (0.095)
Family background index, FB_i	0.783 ** (0.015)	0.636 ** (0.056)	0.828 ** (0.137)
Adjusted R ²	0.318	0.343	0.335
Instruments			High school quality measures listed below.

Notes: ** and * represent statistically significantly different from zero at the 1 and 5 percent levels. Standard errors are in parentheses. They are estimated taking into account the complex sampling structure of the data. The family background index is described in Table A1.

In column 3, measures of (future) high school quality are used as instruments for family background. The first stage is presented in Table A2 in the appendix. The instruments are categorical variables that measure the proportion of the high school student body who receive a free lunch, are non-minority, are from a single-parent household, and speak English as a second language, and the proportion of the previous year's 12th grade class that dropped out, entered a four-year college, or entered a two-year college.

Table 5: Catch-up at New Schools

	(1) OLS	(2) OLS	(3) Case-Control Matching
Dependent Variable = test scores in 12 th grade	Mover Sample	Mover Sample	Mover sample, with each mover matched to a non-mover youth from the same junior high who had the closest test score.
Test scores in 8 th grade (Test _i)	0.807 ** (0.022)	0.789 ** (0.024)	0.889 ** (0.161)
<i>Quality</i> _{MainHS}	0.072 (0.055)	0.044 (0.056)	
<i>Quality</i> _{ActualHS}	0.208 ** (0.058)	0.171** (0.060)	0.171 * (0.076)
Family background index, <i>FB</i> _i		0.113 ** (0.047)	0.174 ** (0.064)
Mover			0.016 (0.031)
Fixed effect	No	No	500 (one for each pair)
N	659	659	500 movers and their matches
Adjusted R ²	0.731	0.733	
R ² within			0.087

Notes: ** and * represent statistically significantly different from zero at the 1 and 5 percent levels. Standard errors are in parentheses. The school quality indices are computed from school and family characteristics such as the proportion receiving a school lunch. Details are in Table A3 in the appendix. The sample in column 3 is movers (youth who did not attend their junior high school's main high school) and, for each mover, the youth in the same junior high with the most similar test scores.

Table A1: Predicting 8th Grade Test Scores with Family Characteristics

Dependent Variable = Test _i	Mean	Coefficient
<i>Family Background: Census Controls</i>		
Female student	0.520	0.048 **
Father foreign born	0.190	0.132 **
Mother foreign born	0.189	-0.023
Oldest child	0.306	0.062 **
Mother was a teen when this student was born	0.103	-0.056 *
Father's education (z)	0.015	0.189 **
Mother's education (z)	0.041	0.121 **
Family income (z)	0.067	0.133 **
<i>Family Background: Additional Controls</i>		
Female-headed household in 8 th grade (omitted family type is two biological parents)	0.127	-0.069 **
Male-headed household in 8 th grade	0.017	-0.086
Stepfather family in 8 th grade	0.083	-0.044
Stepmother family in 8 th grade	0.018	-0.128 *
Resided with no biological parents in 8 th grade	0.081	-0.151 **
Father's occupational status {z}	0.015	0.052 **
Father unemployed	0.059	0.029
Religious affiliation: Baptist (Omitted religion is other Protestant)	0.184	-0.222 **
Religious affiliation: Catholic	0.317	-0.113 **
Religious affiliation: Other	0.152	-0.049
Religious affiliation: Missing	0.036	-0.054
Religious affiliation: None	0.025	0.079
Religiosity: very religious (omitted religiosity is "not at all religious")	0.435	0.141 **
Religiosity: religious	0.164	0.096 **
Religiosity: somewhat religious	0.173	0.115 **
Number of siblings	2.220	-0.031
More than 50 books in home	0.892	0.210 **
Family has at least one magazine subscription	0.762	0.186 **
Family has a public library card	0.773	0.254 **
Mother's occupation status {z}	0.021	0.042 **
Mother unemployed	0.267	0.036 *
Parents are involved in education	0.518	-0.007
Parents and children are involved in child-oriented clubs	0.839	0.058 *
Parents help with homework	0.394	-0.335 **
R ²		0.284

Notes: ** and * represent statistically significantly different from zero at the 1 and 5 percent levels.

Variables marked (z) are z-scored to have a mean of zero and standard deviation of one.

FB_i is the predicted value from this regression, an index of family background.

Table A2: Predicting Family Background with High School Characteristics
 Variables are measured when the student is in tenth grade.

Dependent Variable = FB_i	Coefficient	Std. Error
Average test scores, $Test_{JH}$	0.347 **	0.030
Percent of school that receives reduced-price lunches ^Q	-0.059 *	0.026
Percent of school that is non-minority ²	0.032 *	0.015
Percent of school that comes from single-parent households ^ϕ	-0.003	0.024
Percent of school that has English as a second language ^{>}	-0.027 *	0.016
Percent of previous year's 12 th grade class that dropped out	0.001	0.002
Percent of previous year's graduating class in a 4-year college	0.004 **	0.001
Percent of previous year's graduating class in a 2-year college	0.001	0.001
Constant	-0.167	0.119
Adjusted R ²	0.345	
Incremental R ² of high school characteristics after including $Test_{JH}$	0.075	
F-Statistic on high school characteristics after including $Test_{JH}$	F(7,877) = 15.9	P < 0.0001

Notes: These estimates are from the first stage of the instrumental variables estimation in Table 5.

** represents different from zero at the 1 percent level.

* represents different from zero at the 5 percent level.

^Q This variable is measured on a 0–3 scale (0 = none, 1 = 0–10%, 2 = 11–50%, 3 = 51–100%).

² This variable is measured on a 1–5 scale (1 = 0–25%, 2 = 26–50%, 3 = 51–75%, 4 = 76–90%, 5 = 91–100%).

^ϕ This variable is measured on a 0–5 scale (0 = none, 1 = 1–24%, 2 = 25–49%, 3 = 50–74%, 4 = 75–99%, 5 = 100%).

[>] This variable is measured on a 0–5 scale (0 = none, 1 = 1–9%, 2 = 10–19%, 3 = 20–29%, 4 = 30–39, 5 = 40–100%).

Table A3: Constructing the Index of School Quality

The variables $Quality_{MainHS}$ and $Quality_{ActualHS}$ are computed from the following regression predicting test scores with high school characteristics. Variables are measured when the student is in tenth grade. These estimates are used to construct the school quality measures used in Table 4. (They are not causal regressions in that no controls for family background are present.)

Dependent Variable = 10 th grade test score	Coefficient	Std. Error
<i>High school characteristics</i>		
Percent of school that receives reduced-price lunches [□]	-0.116 **	0.013
Percent of school that is non-minority ²	0.067 **	0.007
Percent of school that come from single-parent households [♠]	-0.062 **	0.015
Percent of school that has English as a second language ^{>}	0.013	0.009
Percent of previous year's 12 th grade class that dropped out	-0.001	0.001
Percent of previous year's graduating class in a 4-year college	0.011 **	0.001
Percent of previous year's graduating class in a 2-year college	0.002 **	0.001
Constant	-0.528	0.065
Adjusted R ²	.139	

** represents different from zero at the 1 percent level.

* represents different from zero at the 5 percent level.

□ This variable is measured on a 0–3 scale (0 = none, 1 = 0–10%, 2 = 11–50%, 3 = 51–100%).

² This variable is measured on a 1–5 scale (1 = 0–25%, 2 = 26–50%, 3 = 51–75%, 4 = 76–90%, 5 = 91–100%).

♠ This variable is measured on a 0–5 scale (0 = none, 1 = 1–24%, 2 = 25–49%, 3 = 50–74%, 4 = 75–99%, 5 = 100%).

> This variable is measured on a 0–5 scale (0 = none, 1 = 1–9%, 2 = 10–19%, 3 = 20–29%, 4 = 30–39%, 5 = 40–100%).

Appendix: Propensity Score Matching

Case-control matching captures much of the variation we are interested in; but it does not match for multiple factors, such as parental education and income, youth race and gender, and so forth. In a remarkable result, Rosenbaum and Rubin (1983) show that matching on a single index they call the propensity score can achieve all the benefits of matching on all controls. The propensity score is constructed by estimating the probability that a particular youth receives the “treatment” – in this case, moving to the better high school (also see Dehejia and Wahba, 2002.)

Heckman, Ichimura, and Todd (1997) emphasize the importance of matching within a geographic region to control for unobserved aspects of the labor market, while Levine and Painter (2002) emphasize the advantages of matching within a school to also capture unobserved aspects of the school and neighborhood. Thus, we follow the latter in matching within a junior high school.

For each youth who did not attend the main high school, we created a small sample consisting of that youth and all youth from that junior high who did attend the main high school. We merged all of these small samples. Thus, a youth who attended the main high school from a school that sent several youths to other high schools appears more than once in the larger sample. We then created a dummy equal to 1 if the youth attended the best high school in his sample; that is, if the mover went to a better high school, this dummy is 1 only for the mover. If the mover went to the worst high school, this dummy is one for everyone but the mover.

In this large sample we ran a conditional (fixed-effect) logit, with a separate fixed effect for each subsample predicting that student j is the one from junior high i who attends the better of the two (main or other) high schools ($T_{ij} = 1$).

$$\Pr(T_{ij} = 1, T_{ik} = 0, k \neq j) = \frac{\exp(X_{ij} \cdot \delta)}{\sum_{k=i}^{N_i} \exp(X_{ik} \cdot \delta)} + \text{error.}$$

The control (X) variables here include the long list of family and youth characteristics listed in Table A1.

For each junior high, we then identified the youth who attended the main high school whose propensity score (that is, the predicted probability of being at the better high school) was closest to the propensity score of the youth who moved. We used this closest match as the control person for that mover. We then analyzed test score growth by the senior year of high school for each mover and each control from junior high i :

$$\begin{aligned} \text{Test in 12}^{\text{th}} \text{ gradet}_j &= d_0 \text{Test in 12}^{\text{th}} \text{ gradet}_j + d_1 \text{Better School}_j + d_2 \text{mover}_j \\ &+ \text{fixed effects for each matched pair}_i + \text{error}_j. \end{aligned}$$

where $\text{Better School}_j = 1$ if student i was the one from school j to attend the better high school.

To keep the matches close in terms of test scores, we discard any mover who had no junior high classmate with less than a 0.1 gap in the predicted probability of attending the better high school. (Results were robust to several cutoffs.) As with case-control matching, four years later the student who ended up at the better high school had standardized test scores .09 higher (SE = .02, Table A5, col. 1) than the control.

On average, the better of the two high schools had about .33 higher predicted test scores (our measure of high school quality estimated in Table A3). Thus, students who move to a new high school close about one-fourth of the gap in initial test scores between themselves and their new classmates. That ratio is also what we estimate if we replace the dummy variable for which student attended the better high school with the index of high school quality (Table A5, col. 2).

Results are robust to several variations in matching. The pace of convergence for students who moved to a better high school is also similar to that for students who moved to a worse high school.

Table A4: Predicting Which Student at Each Junior High Attends the Better High School (Fixed effect (conditional) logit)

Denendent Variable = Went to a Better High School:	Coefficient
<i>Family Background: Census Controls</i>	
Female student	0.059
Father foreign-born	0.023
Mother foreign-born	-0.290
Oldest child	0.002
Mother was a teen when this student was born	0.130
Father's education (z)	0.003
Mother's education (z)	0.062
Family income (z)	0.259 **
<i>Family Background: Additional Controls</i>	
Female-headed household in 8 th grade (omitted family type is two biological parents)	0.393 **
Male-headed household in 8 th grade	-0.659
Stepfather family in 8 th grade	0.452 **
Stepmother family in 8 th grade	0.275
Resided with no biological parents in 8 th grade	0.073
Father's occupational status {z}	0.150 *
Father unemployed	0.380
Religious affiliation: Baptist (omitted religion is other Protestant)	0.214
Religious affiliation: Catholic	0.453 **
Religious affiliation: Other	-0.032
Religious affiliation: Missing	-0.214
Religious affiliation: None	0.220
Religiosity: very religious (Omitted religiosity is "not at all religious")	0.116
Religiosity: religious	0.038
Religiosity: somewhat religious	-0.103
Number of siblings	-0.074 **
More than 50 books in home	0.326
Family has at least one magazine subscription	0.018
Family has a public library card	-0.173
Mother's occupation status {z}	0.062
Mother unemployed	0.227
Parents are involved in education	0.468
Parents and children are involved in child-oriented clubs	-0.092
Parents help with homework	-0.059
Test scores in 8 th grade	0.046
Junior high grade point average (on 4.0 scale)	-0.053
Pseudo-R ²	.051

Notes: ** and * represent statistically significantly different from zero at the 1 and 5 percent levels. Variables marked (z) are z-scored to have a mean of zero and standard deviation of one. The dataset from this regression includes each student who did not attend the main high school and each student who did. The regression includes a fixed effect for each student who moved and the pool of students who continued from their junior high to the main high school.

Table A5: Propensity Score Matching Results for Test Score Catch-up

Dependent Variable = Test score in 12 th grade	
Sample	Propensity score matching based on propensity to be at the better school
Test scores in 8 th grade	0.797 ** (0.020)
<i>Quality_{ActualHS}</i>	0.165 ** (0.061)
Family background index, <i>FB_i</i>	0.233 ** (0.058)
Mover	0.003 (0.024)
Fixed effect for each matched pair	Yes
No. of movers and, thus, no. of pairs and no. of fixed effects	1010
Adjusted R ² within	0.639

Notes: ** and * represent statistically significantly different from zero at the 1 and 5 percent levels.

Endnotes

¹ Jencks and Peterson also note that youths may also be harmed by having advantaged surroundings, as the youths may suffer from feelings of relative deprivation, or when success is partly due to relative performance such as grading on a curve or awards based on class rank (1991). Nevertheless, the net result is a high correlation among the behaviors of a single youth and of his or her neighboring youths and adults (NCES, 1997).

² The estimated equation 5 yields

$$Quality_{HS} = 0.539 (.033) Quality_{JH} + 0.208 (0.024) FB_i, R^2 = .387,$$

where the indices of high school and family background quality are defined in appendices A2 and A3, and the index of junior high quality is defined analogously. Thus we claim that sorting on family quality and junior high characteristics appears to be important.

³ While we do not directly control for how family disruptions such as job loss or divorce may simultaneously affect both neighborhood changes and youths' social and academic outcomes, we do compare outcomes across groups.

⁴ The NELS sample was stratified and clustered, and oversampled rare groups. The NELS provides sampling weights to control for the effects of sampling design. Although the primary analysis is performed using unweighted estimates, the results do not change when using weighted estimates.

⁵ Results were unchanged when we used a majority rule to define the main high school.

⁶ These tables are available upon request.

⁷ The results were robust to choice of a bound (from .1 to 1). These results are available upon request.