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Abstract:

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To enable separate estimation of sorting and school effects, we use the characteristics of the high school students will attend as an additional indicator of family background. When we compare youth who are at the same junior high school, the above measure is an appropriate 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 is statistically significant.

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Are Measured School Effects Just Sorting?

Identifying Causality in the National Education Longitudinal Survey*

David I. Levine and Gary Painter

Youth sharing a school and neighborhoods often have similar academic achievement. This correlation between neighborhood quality and youth achievement holds even when controlling for many observable features of a family. Nevertheless, the correlation is not entirely causal because families and youth are sorted into relatively homogeneous groups. Thus, the quality of the school or neighborhood in part acts as a proxy for hard-to-measure attributes of the family.

To enable separate estimation of sorting and school effects, we use the characteristics of the high school students will attend as an additional indicator of family background. When we compare youth who are at the same junior high school, the above measure is an appropriate 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 is statistically significant.

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

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

The evidence as to the causal impact of neighborhood effects is mixed. Several careful experiments with vouchers for public housing residents find important causal neighborhood effects for some outcomes (see citations below). These experiments are important as they focus on the disadvantaged segment of the population. At the same time, in many cases, regression analyses that include unusually complete measures of family background or that use one of several clever instrumental variables fail to find statistically significant neighborhood results.

This study uses the National Education Longitudinal Survey (NELS) to identify the causal portion of the correlations. We follow a suggestion of 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 that we 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. Using this additional measure of family background as an instrument permits an unbiased estimate of the role of family background. This procedure will also enable us to obtain estimates of school effects that are not biased by their correlation with family background.

Theory and Literature Review

Youth outcomes can be correlated to school and neighborhood characteristics for many reasons. (Jencks and Peterson [1991], Cook and Goss [1996], and Duncan and Raudenbush [1999] provide reviews.) As noted above, many of the links occur when classmates act as role models, provide information, create norms, and enforce norms with peer pressure. At the same time, adults in a neighborhood also 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, in part due to parents' ability to spend time and money, and in part due to greater political power.¹

Simple cross-sectional regression analyses are unable to disentangle why youth outcomes in a neighborhood are correlated (Manski, 1995). The estimated peer effect will be biased upward because families sort across neighborhoods. Therefore, one needs to explicitly account for this sorting in order to measure the effects of improving schools and neighborhoods.

The most convincing evidence that some of the disadvantages are at least partly causal, not merely due to sorting, 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 those staying in the cities. When the children were approximately 18, those who moved to the suburbs had one-fourth the high school dropout rate of their counterparts who 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

These efforts at deconcentration have been replicated in other cities. Preliminary evidence suggests that movements away from high-crime areas lowers youths' involvement in some juvenile crime, and improves adults' health, rates of crime victimization, and (in some studies) employment and earnings (Ludwig, Duncan and Hirschfield, 1999; Ludwig, et al. 2000; Katz, et al. 2000). At the same time, these experimental populations are not representative of youth in general because they focus on the very poor and on those 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 find gangs, not school, as 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 by their peers than rewarded for academic success. (Cook and Ludwig [1998] review the evidence for and against this claim.)

Other methods provide less consistent evidence of causal neighborhood effects. Typical regressions attempt to control for family background using measurable characteristics such as parental education and income. In many cases, substantial correlations among youth outcomes remain even after controlling for many observable features of the family. Other studies have found that the neighborhood effects become much smaller and often lose statistical significance, suggesting that the correlations may be largely noncausal (Brooks-Gunn, Duncan and Aber, 1997). 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. Due to limited sample sizes, their estimates among urban neighborhoods and among rural neighborhoods are not precise, but are consistent with a low role for neighborhoods.

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

Aaronson (1997) and Plotnick and Hoffman (2000) used the difference in neighborhoods that

sisters lived in to identify the portion of neighborhood effects not due to omitted family characteristics. In some, but not all, specifications they 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 not that many families lived in substantially different neighborhoods while their children are adolescents. Moreover, those families that do 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.

As with Aaronson (1997) and Plotnick and Hoffman (2000), we use information from school changers to distinguish the effects of family background from neighborhood. Like them, our sample for identification includes only movers. In our procedure, "movers" include both families that change residence between junior high and high school and youth that did not attend the high school most of their junior high classmates attend.

Our identification strategy is the converse of Aaronson's (1997) and of Plotnick and Hoffman's (2000). They use the fact that neighborhood location partly is causal (and difference out family effects) to identify the true effect of location. We use the fact that high school quality partly is due to family background (net of junior high effects) to identify the true effect of family background. Importantly, we use information on the schools that youth do not yet attend, which implies high school quality measures only family background, not the causal effect of schools and neighborhoods.²

In a related study, Glaeser, et al. (1996), control for neighborhood fixed effects and look at average differences in youth behavior over time; we control for junior high school and neighborhood fixed effects and look at individual changes in neighborhood and junior high surroundings.

Our identification strategy uses the same source of identifying variation as Gaviria and Raphael (2000). Gaviria and Raphael find that youth 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 (their Table 6). 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 youth not yet attending a high school already resemble those who are not yet their 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 as young teens spend many of their waking hours physically at school, and spend much of their free time with peers who are also classmates. For example, Gaviria and Raphael (2000) 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 their parents' priorities and their own capabilities and preferences are more similar to those of their classmates than to 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 to 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.

Methods

We assume test scores in 8th grade ($test_i$) depend on the academic ability of one's classmates (as measured by their average test scores, $Test_{JH}$), on true family background (FB_i^*), as well as 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 will attribute to neighborhoods only the subset of attributes of a school or neighborhood that correlate with average test scores.

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, to correctly measure school and neighborhood effects requires the life history of neighborhoods a child has lived in (Hanushek, 1986). As is common in the literature, we measure school and neighborhood quality at a single point in time. Correspondingly, our consistent estimates (described below) of the effects of improved neighborhoods measure changes in lifetime neighborhood quality, not merely contemporaneous changes.

Biases from the “standard regression.” Consider estimating equation 1 while ignoring measurement error:

$$3. \quad test_i = b_1 \cdot Test_{JH} + b_2 \cdot FB_i + e_i'.$$

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 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, 2000; Brooks-Gunn, Duncan and Aber, 1997; many others have estimated versions in first-difference form).

Incremental sorting between 8th and 10th grades: Most youth continue on from a junior high school to the main high school it feeds. Other youth live in families that move between 8th and 10th grades. Yet a third group do not move with their classmates to a junior high=s main high school, even when their families remain in the same house. For example, some move from a public junior high school to a nearby private or specialized public high school, while others move from a private junior high to a nearby public school. Our analysis sample consists of youth who do not continue to the main high school fed by each junior high school.

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 have helped motivate some 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 youth who do not attend the main high school end up with a better match between their true family background and their high school quality than if they had

continued 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 new shocks, v_i :

$$4. \quad Quality_{HSi}^* = D_1 \cdot Quality_{JHi}^* + D_2 \cdot FB_i^* + v_i.$$

Identifying family background. Because many youth 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) under the assumption that families that do not attend the high school of their junior high peers partially sort themselves in the years between 8th and 10th grade (equation 4). That is, families that 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 measurement error on family background (u_i); that is, as is assumed in equation 4, we need sorting to depend on 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 will 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). This assumption would not hold, for example, if high-achieving students (given their observable characteristics) were frequently accepted into and given scholarships to selective high schools.

We performed two analyses that suggest this potential problem is not too serious. First, we

estimated the sorting equation (4) 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 attended high schools that were most likely to be selective (specifically, those private high schools that are not Catholic schools). This restriction never affected results.

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

Measurement

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 we create an index of family background equal to the best predictor of test scores. That is, we ran:

$$test_i = C @ X_i,$$

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

$$FB_i = c @ X_i.$$

Junior high quality: When calculating observable junior high school quality ($Test_{JH}$) for student i we used 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 youth who move to a non-standard high school. Thus, we use the characteristics of that high school to create an index of quality. The list of characteristics includes the proportion of students receiving subsidized school lunches, proportion minority, and proportion living in single-parent families. Tables A2 include the complete variable list. The school characteristics are measured when the youth is in 10th grade.

Data

The National Education Longitudinal Study of 1988 (NELS) is sponsored by the National Center for Education Statistics and carried out by the National Opinion Research Center. NELS is 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 tenth grade, in the twelfth 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 for which there are too few students per junior high school, a sample of 11,939 are used for the analysis.³

Defining the Movers Sample: The NELS sample started with an average sample of 25 students per junior high school, sampled in 8th grade in 1988. Matching between youth 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 youth could improve their matches by choosing distinct high schools. 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 job changing for parents to the social disruption parents and children suffer lead most parents to stay at a single house 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, yet at least one student attended a different high school.⁴ We termed the first high school, the main high school. From these junior highs, our sample is comprised of the youth who did *not* attend the main high school. In addition, we dropped junior highs with less than 8 students because we may not be able to identify correctly 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 an important predictor of youth’s educational attainment and behavior. The NELS is beneficial in that it contains multiple measures of family background and family involvement in education that many studies lack.

The measures of socioeconomic status are created from both the 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 included indicators of whether in eighth grade the youth lived in an intact family, a single parent family with the biological mother, a single-parent family with the father present, stepfamilies with either the biological mother or father present, and those families with no biological parent present.

From the student questionnaire, we also included the youth's sex, 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 was one of five religions, and any of four levels of religious observance. These variables proxy for how closely a family is knit as well as proxy for the social capital available to the children. We also controlled for whether the mother had been a teen when the student was born.

Three variables partially capture parents' involvement in the student's life and education: whether the parent belonged to a parent-teacher association or related organization, or volunteered at school; whether the parent helps the child with homework; and whether the child had 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 full 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) was 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, but 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 depends on the accuracy of our model of changing schools (equation 4). 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 changers, roughly half went to public school in both 8th and 10th grades. 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 changers.⁵

Below we discuss how well our results are likely to hold for the overall population.

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

For those students who started in public junior high schools in 8th grade, 93 percent remained in public schools. Half of those 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 a third) switched to a public high school.

Testing the selection model: Our model of school selection assumes that for youth who do not go to 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 changes that took place in families that changed residence between 8th and 10th grades (218 out of 886).

One test of whether our sorting model is appropriate examines if 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 equation 4 is positive, youth may move partly based on their academic success. Such a selection rule might bias up the estimated effect of family background 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 4. We reran this regression for each cell of the transition matrix with at least 20 observations and 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 (in results available on request) omitting youth who moved from private Catholic junior high to a public high school; results were unchanged.

One final concern is that some youth 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 or lower than the corresponding private high school, if the public school is larger its upper tail may have even higher test scores than the private school, and advanced classes for this upper tail. Such sorting would call into doubt our one-dimensional metric of school quality, as 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 classes.

The Effects of School Quality

Results from a standard version of equation 3 are presented in Table 3, 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 the average test score omits the score 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 load onto the junior high effect. The next column presents the main results of the paper. 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 over-identification, 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 2, 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 they are not statistically significant and the majority of the estimated effect of school quality remains.

The 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 such a move would raise their children’s academic achievement by 0.42 of the test’s standard deviation.

We now turn to consider several potential weaknesses of these findings.

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% move every other year, and 90% 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 is

proxying for past peer effects. Thus, when estimating 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 grades), those who moved between 8th and 10th grade will be far more likely to move than those who did not move in the previous 2 years.

Surprisingly, the sample of movers and stayers were not very different in terms of future mobility. Families that moved from 1988 to 1990 were slightly less likely to move again between 1990 and 1992 than were others (difference not statistically significant). Moreover, 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 2, columns 1 and 2).

It is possible that the relative importance of family vs. school and neighborhood effects differs for those who did and did not attend the junior high=s main high school. It is plausible that those most concerned about neighborhood quality leave low-quality neighborhoods. Conversely, those families that do not expect their children to be affected by a neighborhood, may choose relatively disadvantaged neighborhoods (Duncan and Raudenbush 1998). If so, neighborhood effects are largest for the sample of movers, and our results over-state the typical youth=s neighborhood effects.

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.

Robustness tests

We restricted our sample to only junior high schools with larger samples (10 or 12 per junior high, instead of a cutoff point at 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, results were unchanged.

The junior high average test scores were from a sample of students, not the population. This sampling error can attenuate the bias on 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. Results were almost unchanged.

A youth's test scores and his or her junior high's average test scores can be correlated due to 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 misleadingly show low scores (given family background), and the coefficient on average test scores in a junior high can be biased down. Working in the other direction, the average test score is from a sample within each school, and sampling error tends causes 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, et al. 1995). In any case, 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; for example, entry into selective high schools. This selection 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 the same methodology that we applied to test scores did not apply for 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 (2000) interpretation of their finding that youth engaging in a number of undesirable behaviors in 10th grade tended to be found 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. Thus, although Gaviria and Raphael did not explicitly account for sorting based on family background, their results are not substantially biased.

Discussion

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 3, col. 2). This estimate is biased up to the extent the effects of unmeasured family background "load onto" the measure of neighborhood quality. Importantly, most causal theories of neighborhood effects imply this bias is *guaranteed* to be present, as the true school effects are due largely to the sorting of families.

Studies that have controlled for this endogeneity using natural experiments such as Gatreux and the Moving to Opportunity experiments usually find important causal neighborhood effects (Rosenbaum, 1995; Ludwig et al. 1999; Ludwig et al. 2000; Katz, et al., 2000). As the scholars describing these experiments note, each study has problems with imperfect randomization, sample attrition, and the fact that the subject pool volunteered to participate in a relocation experiment. Moreover, results vary by outcome and (at best) apply to the disadvantaged segment of the population. In contrast, other studies using clever identification strategies applied to more representative samples usually have failed to find statistically significant causal neighborhood effects. (Evans et al. 1992; Plotnick and Hoffman, forthcoming; Aaronson 1998). At the same time, the power of these tests are usually low, results continue to differ by outcome and specification, and each identification strategy (as the various authors note) is subject to concerns about exogeneity.

This study uses an identification strategy and instrument that allows a more precise estimate of family background than have previous studies. This technique leads us to our preferred estimate that the true effect of a one standard deviation increase in peer test scores is about 0.42 (Table 2, col. 3). On the one hand, this effect is substantially smaller than the 0.52 estimated in col. 2. On the other hand, the estimate is substantively large and suggests neighborhoods *do* matter, if somewhat less than standard OLS estimates indicate.

Many readers will already have made important decisions based on the intuition underlying this result. For example, many parents paid substantial amounts to locate in a neighborhood with advantaged neighbors. The higher real estate costs were, presumably, purchasing better schools, better peers, and better role models for one's children. If the estimated neighborhood effects were

completely non-causal, such amenities are valueless; that is, the children=s expected outcomes 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 are not useful in helping poor youth (see the citations in Ludwig et al. 2000 and Katz, et al. 2000 for descriptions of this program).

The results here indicate readers (and others) who pay extra to locate near educated neighbors are buying valuable improvements in their children=s education. Moreover, the concerns of urban policy-makers that the government warehouses the poor in massive housing projects are similarly well grounded.

It is important to distinguish 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 provide role models or because institutions are better, social multipliers for youth interventions are absent. (Manski, 1993, elaborates on these distinctions.) Finally, schools can matter due to school and parental policies and institutions. 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 to improve the lives of all youth.

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Table 1: Describing the Data

Table 1A: Sample sizes

	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	11939	886 movers
# of junior high schools	781	265 (with at least one mover)

Note: Sample = schools with at least one mover and at least 8 students per junior high.

Table 1B: Summary Statistics

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 Appendix 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 [⊖]	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 [⊕]	1.644	0.698
Percent of School that has English as a second language ^{>}	1.411	1.211
Percent of Last Year's 12 th Grade Class that Dropped out	7.183	9.863
Percent of Last Year's Graduating Class in a 4 year College	53.952	28.767
Percent of Last Year's Graduating Class in a 2 year College	18.568	13.751

[⊖] 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%).

Table 2: Transition matrix for the sample of School Changers

School in 8th grade	School in 10th grade			
	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

Moved Residence

School in 8th grade	School in 10th grade			
	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

Did not Move Residence

School in 8th grade	School in 10th grade			
	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: Models of School Effects

Dependent Variable = test scores in 8 th grade ($Test_i$)	OLS (1)	OLS (2)	Instrumenting for Family Background (3)
Sample	Full Sample	Movers Sample	Movers Sample
Average test scores, $Test_{iH}$	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.

Note: Standard errors are in parentheses. They are estimated taking into account the complex sampling structure of the data.

In column 3, measures of (future) high school quality are used as instruments for family background. The first stage is presented in Appendix A2. The instruments are categorical variables that measure the proportion of the high school receiving a free lunch, non-minority, from a single parent household, and speaking English as a second language, and the proportion of last year's 12th grade class that dropped out, entered a 4 year college, or entered a 2 year college.

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

Dependent Variable = Test _i	Mean	Coefficient
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 **
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 **
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 religion	0.152	-0.049
Religious affiliation - Missing religion	0.036	-0.054
Religious affiliation - No religion	0.025	0.079
Religiosity - very religious (Omitted religiosity is ANot 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 **
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 *
Family Income (z)	0.067	0.133 **
Parental Involvement 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_t	Coefficient	Std. Error
Average test scores, $Test_{tJH}$	0.347 **	0.030
Percent of School that receives Reduced Price Lunches [⊖]	-0.059 *	0.026
Percent of School that is non-Minority ²	0.032 *	0.015
Percent of School that come from Single Parent Households [⊕]	-0.003	0.024
Percent of School that has English as a second language ^{>}	-0.027 *	0.016
Percent of Last Year's 12 th Grade Class that Dropped out	0.001	0.002
Percent of Last Year's Graduating Class in a 4 year College	0.004 **	0.001
Percent of Last 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 $Test_{tJH}$	0.075	
F-Statistic on high school characteristics after including $Test_{tJH}$	F(7,877) = 15.9	P < 0.0001

Notes: These estimates the first stage from the instrumental variables estimation in Table 3.

** 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%).

Notes:

¹ Jencks and Peterson also note that youth may also be harmed by having advantaged surroundings when the youth 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 youth and adults (Case and Katz, 1991; NCES 1997).

² While we do not directly control for how family disruptions such as job loss or divorce may simultaneously affect both neighborhood changes and youth outcomes, we do compare outcomes across groups.

³ The NELS sample was stratified and clustered, and over-sampled rare groups. The NELS provides sampling weights to control for the effects of sampling design. While the primary analysis is performed using unweighted estimates, the results do not change when using weighted estimates.

⁴ Results were unchanged when we used a majority rule to define the main high school.

⁵ These tables are available upon request.