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Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes

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Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes

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Abstract

Studies that attempt to measure the impact of neighborhoods on children's outcomes are susceptible to bias because families choose where to live. As a result, the effect of family unobservables, such as the importance parents place on their children's welfare, and other unobservables that are common to geographically clustered households, may be mistakenly attributed to neighborhood influences. Previous studies that attempt to correct for this selection bias have used questionable instrumental variables.

This paper introduces an approach based on the observation that the latent factors associated with neighborhood choice do not vary across siblings. Therefore, family residential changes provide a source of neighborhood background variation that is free of the family-specific heterogeneity biases associated with neighborhood selection. Using a sample of multiple-child families whose kids are separated in age by at least three years, I estimate family fixed effect equations of children's educational outcomes. The fixed effect results suggest that the impact of neighborhoods exists even when family-specific unobservables are controlled. This finding is robust to many changes in estimation techniques, outcome measures, neighborhood measures, variable definitions, and samples.

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I. Introduction

There is substantial evidence that family background characteristics play an important role in the educational development of children. However, how neighborhoods, schools, and peer groups affect children remains unsettled. Although a fairly large cross-disciplinary literature on neighborhood effects has emerged over the last fifteen years, the empirical findings are not robust to data issues, outcome measures, and estimation techniques.[1] The lack of consistent evidence could stem from a number of factors. Perhaps survey data cannot adequately represent the complex nature of how communities influence children. Another possibility, often discussed in the literature, is the role of bias in the estimating equations.

Bias may arise because families are not randomly placed in neighborhoods but rather choose their location based on a variety of factors, including the importance they put on their children's development.[2] For a variety of reasons, the result is that neighborhoods are stratified along socioeconomic lines. This is reflected in the fact that key family characteristics, such as household income, the proportion of single parent families, and average education levels vary substantially across neighborhoods. Studies that ignore the endogeneity of neighborhood selection risk over- or understating the importance of neighborhoods for children's outcomes. The direction of the bias is related to the way the unobservables associated with neighborhood selection are correlated with the unobservables associated with children's outcomes. It is generally thought that this bias is positive, reflecting the potential of attributing family characteristics, such as parental competence, tastes for education, or time spent with their

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[1] Significant neighborhood effects have been found in, among others, Summers and Wolfe (1977), Case and Katz (1991), Crane (1991), Brooks-Gunn et al. (1993), Duncan (1994), and Borjas (1995). However, other studies, most notably Evans, Oates, and Schwab (1992) and Corcoran et al (1992), have found no evidence that neighborhoods matter. While Jencks and Mayer (1990) conclude that no robust evidence of neighborhood effects exists, their extensive cross-disciplinary summary points to a number of studies where neighborhoods seem to matter.

[2] To minimize the self-selection issues involved in residential location choice, this research has wisely concentrated on children and teenagers. A notable exception, which uses quasi random assignment, is the work by Rosenbaum and Popkin (1991) on Chicago's Gautreaux program.

Other econometric problems discussed in the literature are measurement error and the 'reflection problem' (Manski 1993). The reflection problem concerns the possibility that individuals affect or are directly part of the neighborhood aggregate and therefore it is difficult to discern cause from effect. This problem is particularly severe in cases like Case and Katz (1991) where neighborhood variables are aggregates of a small number of individuals. In this paper, neighborhoods constitute roughly 4,000 individuals and therefore should not suffer from this endogeneity problem.

children, to the neighborhood measures. The bias might be enhanced if the unobserved heterogeneity that is common to a group of clustered individuals is correlated with measured neighborhood characteristics.

Studies that attempt to correct for this selection bias have used an instrumental variables approach. However, this method requires use of a variable that is a determinant of neighborhood choice but not of the outcomes of children. Such an instrument is difficult to find. Only three papers that I am aware of attempt to do so.

Case and Katz's (1991) study of peer influences surmise that children are influenced by peers in their own neighborhood and surrounding neighborhoods but not directly by those living in noncontingent communities. This assumption allows them to use neighbors' neighbors as an identifying variable but is susceptible to the criticism that children are likely to be influenced by peers at school who do not necessarily live in the same or adjoining neighborhoods. A second paper by Evans, Oates, and Schwab (1992) employs a similar strategy, using a geographically different measure of the community-level variable to identify the selection process. They use metropolitan area unemployment, median family income, poverty, and educational attainment characteristics as instruments, arguing that these variables are likely to be correlated with their peer group variable, the log of the number of disadvantaged children in a teen's school, but do not directly affect their child outcome measures. To make this assumption, the authors must believe that metropolitan area characteristics do not affect schooling choice and that parents take their metropolitan area as given when choosing a particular neighborhood or school. There are reasons to question both assumptions.[3] Finally, Duncan, Connell and Klebanov (1994) use the neighborhood that the mother lives in after all the children leave the parental home as an instrument, under the premise that once children move into their own households, parents' residential choice is no longer based on concern about neighborhood influences on their own children. However, inertia in residential choice makes these instrument choices suspect.

[3] Defending their choice of instrument, EOS note that two-thirds of family moves in a five year period were within the same metropolitan area. However, this evidence, which is also borne out in the PSID, suggests that families are quite mobile and thus may be selecting into metropolitan as well as census tract areas.

An alternative approach pursued in this paper is based on the observation that the latent factors associated with neighborhood choice are sibling-invariant; households rarely move due to the differential ability of their children. As a result, family residential changes provide a source of neighborhood background variation within families that can be used to identify neighborhood influences. The key advantage to using sibling neighborhood background differentials is it offers a natural way to eliminate the family-specific heterogeneity biases associated with neighborhood selection. Furthermore, unlike other studies that use twins or siblings, such as the rate or return to education or teenage motherhood debate, the potential endogenous variable is not being chosen by the individual. However, sibling-based fixed effect models are no panacea. They may accentuate problems with measurement error and still leave open the possibility of omitted variable bias due to unobserved time-varying family characteristics and within-family heterogeneity. Using a sample from the Panel Study of Income Dynamics (PSID) of multiple-child families whose kids are separated in age by at least three years, I estimate family fixed effect equations of children's educational outcomes. The fixed effect results suggest that the impact of neighborhoods exists even when family-specific unobservables are controlled. This finding is fairly robust to changes in estimation techniques, outcome measures, neighborhood measures, variable definitions, and samples.

The paper is organized as follows. Section II explains the empirical strategy used in this paper. First, a model where communities matter to childhood educational opportunities is introduced to show how endogeneity, heterogeneity and functional form assumptions might influence empirical results. Some concerns about the fixed effect estimates are explained, including the critical notions of measurement error and within-family heterogeneity. Section III describes the data used to develop the sample and the neighborhood characteristics. Section IV discusses the results, including linear probability, logit, instrumental variables, and fixed effects estimates of neighborhood influences on the likelihood of children graduating from high school. Section V outlines a number of tests of the robustness of the findings. The models are rerun using different neighborhood proxies, different outcome measures, different samples, and different neighborhood variable definitions. Many of these tests are used to reconcile the different findings of this paper and Plotnick and Hoffman (1995). Plotnick and Hoffman also use PSID siblings to identify neighborhood effects but conclude that family fixed effects eliminate the impact of neighborhoods on post-secondary schooling, teen births, and welfare recipiency. Our results can be partly reconciled by differences in sample and variable definitions. Some concluding remarks are offered in the last section.

II. Empirical Strategy

<u>Model</u>

To help clarify these issues, I present a simple variant of Becker's child quality model with the additional assumption that communities influence a child's future outcomes.[4] Suppose a parent maximizes a CES production function over her m children's future outcomes (education, say), k_{t+1}, current consumption, c_t, and the quality of the neighborhood, n_t,

(1) $$u(k_{i,t+1}, c_t, n_t) = \sum_{i=1}^{m} \delta_1 k_{i,t+1}^{\rho} + \delta_2 c_t^{\rho} + \delta_3 n_t^{\rho} \quad \text{where } \rho<1.$$

Children are indexed by i and time by t. Quality of neighborhood enters the utility function independently and additively to account for the importance that households place on crime, ethnicity, services, housing conditions, and other neighborhood-specific factors. The parent faces a budget constraint of the form $I(n_t) = c_t + P_n n_t$. Neighborhood conditions are allowed to influence the household's income. The parent uses income to purchase consumption items and better neighborhood conditions at a price relative to consumption of P_n. Finally, each child's future education is determined by a production function of the form

(2) $$\log k_{i,t+1} = \beta_g \log G + \beta_a \log a_{it} + \beta_n \log n_t \text{ where } \beta_g, \beta_a, \beta_n \varepsilon [0,1).$$

Family-specific variables are captured in the G term. This would include parents' interest in education, and any family background characteristic and ability component that is constant across siblings. The vector a_{it} measures any variables that exhibit heterogeneity between siblings, most

[4] For examples of more complicated models with peer effects, see deBartolome (1990) and Epple and Romano (1993). Many other literatures have argued that production functions should include characteristics of the population, including growth theory (Jacobs 1969, Romer 1986, Benabou 1993) and local public finance (Brueckner and Lee 1989).

notably differences in ability, ambition, or, for siblings separated by age, family conditions. It would also include differences in parental expectations or treatment of siblings. A vector of neighborhood and peer measures also enters the production function.

An important element of this model is the reason for family moves. When a family moves into a community, it has an expectation about the quality of the neighborhood. However, there is also uncertainty about the caliber of the family-neighborhood match since the neighborhood good is, at least in part, an experience good. Therefore, the actual neighborhood good is a sum of the random error component that measures uncertainty in match quality and the expectation of the neighborhood prior to the move. An unexpected negative shock in the match quality parameter may cause households to migrate to a new neighborhood. This uncertainty is used to obtain the variation in sibling background that is needed for the model to work. However, another source of neighborhood migration may be due to changes in the family's background. Changes in marital, income, or employment states may cause families to reevaluate their neighborhood choice. In the empirical work, it will be imperative that these family changes are controlled in order to decompose the effect of neighborhood change from other family changes that might affect children's outcomes. The empirical strategies used to do this are discussed more fully below.

For simplicity, assume neighborhood location does not affect parents' income ($\partial I/\partial n = 0$).[5] Also assume there are two siblings that are separated in age by one period. Sibling *i* lives during period 1 and sibling *j* lives during period 2. Maximizing $U(c_t, k_{t+1}, n_t)$ subject to the budget constraint and equation (2) leads to equilibrium ratios of future outcomes and neighborhood conditions that are a function of the heterogeneous components between siblings and the functional form assumptions associated with ρ.

$$(3) \qquad \log\frac{n_1}{n_2} = \frac{\beta_a \rho}{2-\rho(1+\beta_n)}\log\frac{a_{i1}}{a_{j2}}$$

$$(4) \qquad \log\frac{k_{i2}}{k_{j3}} = \frac{\beta_a (2-\rho)}{2-\rho(1+\beta_n)}\log\frac{a_{i1}}{a_{j2}}$$

[5] This assumption pertains to the spatial mismatch hypothesis. Most researchers believe that the affect of neighborhoods on adult's income is negligible. Regardless, the assumption does not affect the main results of the paper.

Basing the estimating equations on within-family differences provides the empirical advantage of eliminating the impact of any family-constant component, including decisions about neighborhood selection. Selectivity has an impact only if parents choose neighborhoods based on the differential ability of their children (equation 3).[6] If this selection process exists, the sign of the bias is dependent on the functional form assumptions. If $\rho > 0$, parents use a reinforcing strategy in their choice of neighborhood by taking special effort to live in better neighborhoods for more able children. As ρ approaches zero (Cobb-Douglas), ability is independent of neighborhood choice. Finally, if $\rho < 0$, parents adopt a compensating strategy where the parents try to place kids with lesser ability in better communities.

Econometrically, this preference can be seen more clearly by looking at the error term of an educational production function where I rewrite (2) as:

(5) $$k_{if,t+1} = \beta_{xf}x_f + \beta_x x_{ift} + \beta_a a_{ift} + \beta_n n_{ift} + \varepsilon_{ift}$$

where *f* indexes families. Here the family and individual factors in the educational production function are separated. The *x* terms represent family characteristics that vary over time and siblings (x_{ift}) and those that are time and sibling invariant (x_f). The error term is broken into three components

(6) $$\varepsilon_{ift} = \eta_i + n_{ift}\varphi_f + \mu_{ift}.$$

φ_f is a sibling invariant error component. If φ_f is correlated with family residential preferences, then first differencing equation (6) across *i* will eliminate selectivity concerns. However, if the selection of neighborhood is correlated with the individual-specific error components, then selection bias remains a problem. In the results presented below, child ability is assumed to be

[6] This differential selectivity could be due to other factors related to neighborhood choice. For example, suppose a parent decides whether to work based on the ability of their children. In this case, the budget constraint and the educational production function includes the parent's decision on how much effort to put into their work and their children.

(2a) $$(1 - s_{it})Rk_{i,t} = c_t + P_n n_t$$

(2b $$\log k_{i,t+1} = \beta_g G + \beta_a \log a_{it} + \beta_s \log s_{it} k_{i,t} + \beta_n \log n_t$$

The parent with human capital $k_{i,t}$ chooses whether to work at wage R or to put more effort and time into the production of the child's human capital. If this work effort is used to buy better neighborhood goods for their children, then the activities are substitutes. Because of the endogeneity of s_{it}, we would need to worry about another simultaneous equation that maps the differential effort of the parents as a function of the differential ability of the children.

independent of neighborhood choice. This seems to be an innocuous assumption, although I hope to better understand the correlation between the neighborhood good and the error term and its importance in this system in future work.

First differencing equation (5) eliminates the family-specific unobservable and leaves a reduced form equation of the general form

(7) $\Delta_i k_{ift} = \beta_n \Delta_i n_{ift} + \beta_x \Delta_i x_{ift} + \beta_a \Delta_i a_{ift} + \Delta\varepsilon_{ift}$

where Δ differences across siblings. Without differential neighborhood selectivity, then $\Delta_i n_{ift}$ can be thought of as an element of the a_{it}/a_{jt} vector in equation (4). Equation (7) is the main estimating equation used in this paper.

However, at least four main complications remain in the estimation of equation (7): unobserved heterogeneity within families, measurement bias, the discrete nature of the outcome measures used, and complications with the sample due to age restrictions placed on the siblings. The latter two estimation problems are discussed first as they are handled by conventional methods.[7]

Complications with the Fixed Effect Estimator

The first complication is that the outcome variable used in much of this analysis (high school graduation) is discrete.[8] In a linear regression framework with continuous dependent variables, one can handle family fixed effects by applying OLS to the data after taking deviations from group means.[9] However, the nonlinearity of discrete choice models excludes this strategy. Furthermore, the asymptotic properties of the logit model depend on the number of observations per group increasing. Therefore, as shown by Chamberlain (1980), discrete choice fixed effect equations with small numbers of observations per group are inconsistent. Instead, he proposes a logit fixed effect model which is estimated using conditional likelihood functions. An alternative approach that uses within-group variation employs a specification suggested by Mundlak. He

[7] A fifth complication arises from the effect of neighborhood specific error components on the standard errors. This is accounted for using Huber's formula.

[8] In the final section, I run the fixed effect estimator on a continuous variable--grades completed--as well.

[9] Of course, the linear probability model introduces other well-known problems, including heteroskedasticity and predicted probabilities that are not constrained to the zero-one interval.

allows individual effects to enter the probit model by simply specifying separate within-family and across-family variables

(8) $$k_{ift} = \beta_1 x_{ift} + \beta_2 \bar{x}_{ft} + \beta_3 a_{ift} + \beta_4 \bar{a}_{ft} + \phi_1 n_{ift} + \phi_2 \bar{n}_{ft} + \varepsilon_{ift}$$

where variables with bars represent family averages and ϕ_1 is the within-family neighborhood measure of interest. Results are presented using conditional logit, Mundlak probit, and linear probability models. However, it appears to make little difference whether logit or linear probability techniques are used in the fixed effect models. Furthermore, because coefficients from conditional logit equations are in different units than the simple logit coefficients, they are not comparable. As a result, because of their ease of comparability and use, linear probability equations are employed to conduct much of the estimator comparisons.

A second concern is sampling restrictions. In order to get meaningful variation in the residential location of siblings, the sample includes only individuals with a sibling three or more years younger or older than themselves.[10] Most of the difference in neighborhood background between siblings who are close in age is likely to be composed of measurement noise. The further siblings are apart in age, the more likely they will experience true differences in neighborhood composition and enable me to identify real differences in background influences. Choosing the age restriction is a bit ad-hoc, with sample size considerations balanced against the advantages of using more age-separated siblings. I use a cutoff of three years. Some experimentation suggests that using four or five years does not make a significant difference but decreases the precision of the estimates due to the smaller sample sizes. Regardless of the cutoff choice, this sampling restriction complicates the computation of the fixed effect estimator since one family fixed effect will combine siblings that do not fit the age criterion. Therefore, I construct four fixed effect estimators to see how assumptions about grouping observations affect the evaluation.

The first three estimators are constructed by pairing siblings that fit the age criterion for selection into the sample. For example, in a family with three kids, aged x, $x+2$, and $x+5$, I would

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[10] Other important restrictions are: the individual turn 18 by 1985, the individual be in the PSID sample for two years between the ages of 10 and 14 , and be in the sample for one year after age 18, so that high school graduation can be ascertained. If it is not possible to ascertain grades completed and the individual has data only through age 19, then he is dropped from the sample.

include two pairs of siblings in the sample: the oldest with the third child (5 years apart in age), and the oldest with the second child (three years apart in age). This setup might oversample certain individuals and therefore could introduce bias to the estimates. Therefore, a second estimator weights the variables by the number of times each individual in a sibling pair is in the total sample or $(1/n_1+1/n_2)$, where n_i is the number of times individual i is included in any sample pair. If there is concern that this weighting procedure will not compensate for the over sampling of individuals, as a third alternative, I select one pair of siblings--the oldest and youngest--from each family. Finally, as a fourth option, I estimate an equation that includes a single fixed effect for each family. This alternative eliminates the multiple sampling problem but is problematic in my example because it contains groupings of siblings who do not fulfill the age criterion.[11] If inference is similar for all four estimators, I can be more confident in the robustness of the results.

Unobserved Heterogeneity within Families

A more serious concern relates to the reasons for neighborhood change and the individual error components that describe the unobservable differences between siblings. In particular, two problems could potentially complicate the interpretation of the sibling difference estimator. First, at some level, siblings may differ in, among other factors, ability, ambition, or parental expectations and treatment; these unobserved factors may be correlated with neighborhood characteristics.[12] However, this is likely to be a serious concern only if parents choose neighborhoods based on these sibling differentials, an unlikely scenario. While there appears to be scant evidence on the impact of siblings' differential ability on neighborhood choice, the research that exists places little significance on differential selection. Altonji and Dunn (1995) estimate the effect of IQ scores on school choice and find little evidence that ability matters to this decision within families.

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[11] For example, child x and $x+2$ would be included in the same family fixed effect, which is inconsistent with the restriction that only siblings three years apart be compared in the fixed effect models.

[12] For example, Summers and Wolfe (1977) find that lower skilled students are more affected by classmates and school quality than their more able peers. Other omitted differences between siblings, such as parents expectations or treatment, might have opposing effects. Plomin and Daniels (1987) review the genetics and psychology literature and find a wide range of components--including the closeness to the mother, the friendliness of the siblings, the role of siblings in family decision making, and parental expectations--that might affect the outcomes of siblings, particularly those with differing abilities, in distinct ways.

To be safe, it would be useful if some measure of sibling differences could be controlled. Unfortunately, the PSID has no test score reports or other useful childhood measure. A potential partial solution to the missing ability measure is to use whether the child works during his youth as a measure of unobservable ambition or drive. However it is also possible that this variable would pick up the availability of jobs[13] or itself be a component of other neighborhood influences. If this latter interpretation is correct, including the 'whether worked' variable will bias the neighborhood coefficient downward.

One characteristic where sibling differentials might matter is age. If parents learn how to care for their children over time, it is possible that their younger children will benefit by being placed into better neighborhoods than their older siblings. Fortunately, this possibility is easily observed and controlled for in the analysis by including the birth order of the children.

A second complication arises because siblings separated by age may experience different family environments due to, say, marital changes, different family income circumstances, or less measurable changes in household characteristics. As mentioned above, this heterogeneity is particularly worrisome if the neighborhood variable is simply picking up changes in family conditions that precipitate changes in residential location. In other words, sibling differences in neighborhood conditions may be a function of changes in unmeasured family conditions and not changes in community influences.

To provide insight into whether this issue is important empirically, table 1 reports *measurable* changes in total family income, labor income, marital status, and employment status for residential stayers and movers, by type of geographic move,[14] to see if residential moves are correlated with changes in family conditions. The last four columns explore family changes corresponding to moves into better (columns 6-7) and worse (columns 8-9) neighborhoods as crudely measured by the poverty rate in the origin and destination neighborhoods. The sample includes multiple child families in the PSID during 1971-1974 or 1980-1983. These time periods

[13] See Holzer (1991) for a good summary of the spatial mismatch literature.

[14] Moves are categorized by geographic level: state, county, neighborhood, and residence. Neighborhood is the census tract. If the person does not live in a census tract, then enumeration district (the rural equivalent of census tracts) is used. If the person does not live in an enumeration district, then five digit zip codes are used.

were chosen so that it would be clear when moves occur. Because the geocode database is missing addresses for 1969, 1975, 1977, and 1978, the level of a move cannot be identified for these years and the year that follows without making some ad hoc assumptions about timing.

As can be seen in the income, marital status, and employment status variables, longer distance moves are more likely to occur among better-off families. State and county movers look like stayers with regard to income, marital status and employment status. But neighborhood and residential movers are poorer, less attached to the labor market, and less likely to be married.

Most telling for this paper is the way these variables change in relation to moves. In the rows titled "Change (*t-i,t*)", I calculate the difference between the year after the move (*t*) and the two years preceding the move (*t-1,t-2*). Asterisks represent whether these changes are significantly different from the changes prior to stayer years. A pound sign represents whether changes preceding moves to better neighborhoods are significantly different from changes preceding moves to worse neighborhoods at the five percent level. I also calculate transitions between marital and employment states in the two years before a move in the rows directly below the "change" rows. These transitions represent the percentage of family-year moves (or stays) that are prefaced in the two previous years by that particular change. For example, 11.4 percent of state changers experience a divorce (married→divorced) in the previous two years before the state move.

The results suggest some significant change in observable family environment preceding moves, although these changes vary by the distance and type of mobility. The two income categories show fluctuations in years before moves but these changes are generally not significantly different from the years before stays. There is a decline in total income and labor income preceding moves to neighborhoods with higher poverty rates (column 6) and an increase in total income but not labor income in years preceding moves to neighborhoods with lower poverty rates (column 8). Moves into higher poverty neighborhoods are also preceded by a small spike in the variance of total and labor income, suggesting that income instability could be a factor in these community changes. However, the overall picture from these income changes seems to suggest little relationship between family changes and moves. Other work using larger samples is consistent with this conclusion.

There is much more activity in marital and employment status changes prior to moves. Transitions into and out of marriage are significantly different for move years than stay years among every move category. Changes preceding moves into high poverty neighborhoods are especially noticeable, with strong evidence of transitions into divorce but much less evidence into marriage. Transition into marriage is often followed by moves into lower poverty neighborhoods and different counties and states. However, there also appears to be high levels of recently divorced households moving into lower poverty neighborhoods. Employment status changes play an important role in short distance moves (residential and neighborhood), particularly by those moving to lower poverty neighborhoods. Moves into lower poverty neighborhoods are often preceded by transitions into retirement from full-time employment and into employment from unemployment or temporarily laid off. Transitions into and out of retirement are often related to moves to higher poverty neighborhoods.

In sum, the information in table 1 offers evidence that moves are associated with changes in family background. However, the evidence is fairly weak in two ways. First, income changes are not highly correlated with moves. Second, negative shocks to family composition are as likely to be followed by moves into better neighborhoods as worse neighborhoods. As a result, there does not appear to be a consistent pattern in the relationship between changes in observable family environment and changes in neighborhood choice.

Nevertheless, because a large part of the variation used to identify the neighborhood effect is from moves, the analysis must control as much as possible for these different circumstances. One important strategy is to directly control for family mobility. Another promising feature of the fixed effect estimates is that if the unobserved family change is constant across siblings, then the fixed family effect should eliminate this concern. However, if these changes affect siblings in different ways, which is possible given the age differences in the sample that I will work with, then some discretion must be used in interpreting the results as caused by changes in community influences rather than differing latent family conditions.

Measurement Error

Since the neighborhood inputs, n_i, are imperfect measures of the true effects of communities, a final concern with the fixed effect (and simple linear probability and logit) neighborhood equations is classical measurement error. In particular, assume that $n_i = n^* + v_i$, where v_i is an iid random variable and n^* is the true community measure. Differencing across siblings exaggerates the measurement bias by creating a correlation between the differenced inputs, n, and the differenced idiosyncratic shocks, v. As a result, much of the true variance is eliminated while the noise remains. The direction of this measurement error bias is toward zero. The size of the bias would be proportional to the difference between the signal to noise ratio in the estimator without fixed effects $(\sigma_v^2/(\sigma_v^2 + \sigma_{n^*}^2)$ and the fixed effect signal to noise ratio $\sigma_{\Delta v}^2/(\sigma_{\Delta v}^2 + \sigma_{\Delta n^*}^2)$. A common solution to this problem is to estimate fixed effect, instrumental variable (FE-IV) equations. But this reintroduces the problem of finding believable instruments that are related to neighborhood differences but not to differences in outcomes between siblings that arise from other sources. I tried some specifications using a differenced version of Evans, Oates, and Schwab's county-level measures, although this instrument is susceptible to the same criticisms as before. If this is a classic measurement error story, one neighborhood measure could be used as an instrument for another, but this relies on the precarious assumption that the measurement error is uncorrelated between variables. Fortunately, the results presented in the next section show no evidence that first differencing increases the importance of classical measurement error.[15] In fact, most of the fixed effect results are of the same size or larger than the simple OLS or logit models without fixed effects.

[15] Measurement error in the other right hand side variables could introduce upward bias in the neighborhood estimates if there is a correlation between these measures and the neighborhood variables. For example, if there is measurement error in the change in family income and a correlation between family income changes and neighborhood changes, then the neighborhood measures will be positively biased. In most cases, however, several years of family data are available before and after the moves. Under the assumption that measurement error is iid, one could exploit this fact to construct an IV estimator.

III. Data

The data used in this paper are from the Panel Study of Income Dynamics (PSID) and its accompanying geocode file for 1968-1985. Individuals are included in the sibling sample if they have a sibling at least three years apart in age and are in a respondent household for at least two years while they are between ages 10 and 14. Furthermore, the individual must have at least one year of data after age 18 so that high school graduation can be ascertained. These constraints result in a sample of 2,178 individuals from 742 families.

A problem with using the age restriction is that the sample is composed solely of children from larger families. This can bias the results in an unknown direction when fixed effects are excluded. In this paper's model, parents invest less per child in large families, resulting in a positive bias in the neighborhood effects measure. However, if there are spillovers from large families that make it easier for children to relate to community externalities, the bias might work in the opposite direction. Fixed effect specifications sweeps out this family-specific heterogeneity. Nevertheless, to gauge the importance of this nonrandom sampling on the model without fixed effects, I also construct a sample that includes all individuals that fit the data demands except the sibling requirements. The all-youth sample includes 4,410 people from 1,199 families.

Table 2 includes descriptive statistics on the main variables used in the analysis for the sibling sample and the all youth sample. All statistics are weighted using the PSID-constructed probability of selection into the sample. The sibling sample appears to be roughly comparable to the all youth sample. Some small differences reflect the larger family sizes of the sibling sample. In particular, the sibling sample has lower education levels for the parents and children, lower household income, lower mobility, more minorities, and more two parent families.

The Neighborhood Measure

The believability of the neighborhood proxy is key to the measurement of neighborhood effects. Previous studies have used many different measures, including the fraction of disadvantaged students in the individual's school (Evans, Oates, and Schwab 1992), the percentage of families living in the neighborhood with incomes below $10,000 and above $30,000 (Brooks-Gunn et al. 1993), the percent of families on welfare (Corcoran et al. 1992), the percentage of female-headed households (Corcoran et al 1992), and racial composition (Summers and Wolfe 1977). Duncan (1994) tests many of these measures within the same data set and specification. Case and Katz (1991) aggregate household data to derive neighborhood averages of the numerous outcomes they study.

I concentrate the analysis on two variables that should pick up many of the dimensions hinted at in the above analyses. First, because the outcome measure of interest is high school graduation, I use a similarly defined variable that measures the percentage of young adults in a census tract who were aged 16 to 19 in 1980 (16 to 21 in 1970) and who had not graduated from high school and were not in school. This variable can be thought of as an *extremely* rough proxy for peer effects. Second, I use a variable that measures the percentage of households below the federal poverty threshold. This variable might be thought of as a proxy for the effect of adult neighbors and relative neighborhood conditions on youth achievement. I make no claims that these two variables will pick up all community-level influences. However, as much as the various influences are highly correlated, these two measures should be representative of the size of the neighborhood effects on educational achievement. In the final section, I also report some preliminary results on three other neighborhood proxies -- percentage of female heads, average family income, and percentage of population that is white -- to see if any patterns emerge when using these different measures.

The data for these neighborhood variables come from two sources. Geographic identifiers are reported in the PSID's geocode file. The geocode file is a set of addresses collected from mailings to respondents. From these addresses, identifiers are assigned for various levels of geographic aggregation. The smallest geographic area classified by the Census bureau is the census tract or block numbering area (BNA), which is the basis for the neighborhood measure used in this paper. A census tract is an area of, on average, 4,200 people that local authorities deem to be a 'neighborhood.' BNAs are the equivalent of census tracts for untracted urban areas. When census tracts are unavailable, enumeration districts (ED), the rural equivalent of census tracts, are used. When tracts, BNAs, and EDs are unavailable, I employ five digit zip codes, which tend to encompass a larger area than the other identifiers.

These geographic identifiers are matched to 1970 and 1980 Census identifiers and data on numerous area dimensions, including family structure, income, employment, race, education, housing, and mobility. I linearly interpolate neighborhood variables for the years 1970 to 1978 and set 1980 to 1985 and 1968 to 1969 values equal to their closest census year. The effect of this imputation scheme is examined in section V.

The main neighborhood measure used is an average of the community conditions that the person lived in from ages 10 to 18. This averaging technique implicitly weights each age equally in the neighborhood impact estimate. It does not pick up any additional effect that may occur from neighborhoods lived in prior to age 10.[16] As an alternative computation, I also explore the robustness of the results to using the age 14 measure of neighborhood conditions. This latter measure is more commonly used in the literature but does not describe the full history of background influences that the person experiences. Therefore, it may be more susceptible to measurement error relative to the averaged variable.

<u>The Outcome and Control Variables</u>

Although there are many dimensions in which peer and neighborhoods might be influential, I concentrate on educational outcomes. Most of the analysis focuses on whether the individual graduated from high school. Section V reports some findings using college attendance and grades completed.[17] I chose these education variables because, unlike teenage pregnancy, they do not limit the sample to a single gender. Other variables that are often studied in this literature, such as crime rates and drug use, are not available in the PSID.

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[16] For evidence of neighborhood effects on younger children, see Brooks-Gunn et al (1993). No outcome measure currently exists for younger children in the PSID, which makes it difficult to determine neighborhood effects on children prior to age 10.

[17] Using the PSID education variables might cause an attrition problem because grades completed are not reported until an individual has finished full-time schooling. As a result, a number of individuals over age 18 leave the sample without ever reporting data on grades completed. The high school graduation variable is coded as 1 if the individual ever reports completing 12 grades or the individual remains in the sample after age 20 but still reports being a student. Individuals who attrite from the sample before age 20 without reporting grades completed are excluded. The analysis using college attendance and grades completed includes only individuals who report grades completed after age 19. Grades completed are the greatest number of grades reported by age 25, unless the individual has not reported a grade by age 25. In this case, the first reported grade completed is used.

Covariates in the basic regressions include gender, race, parental education, household income, parents' marital status, the number of children living in the household, whether the teenager worked during her youth, and year born and region dummies. The income measure includes all labor income, transfers and asset income, net of the teenager's income. Like the neighborhood variables, the time-varying family background variables are averaged over the years from age 10 to 18 that the youth lives at home. This averaging technique results in more 'permanent' measures of these variables. However, if temporary changes, such as large income fluctuations, marital changes, or residential moves, are important, then this averaging might miss important factors in the likelihood of continuing schooling. Therefore, I also include controls for a number of transition variables that measure instability in the family environment, including the variance of income during the youth's years in the household, the percentage of years that the household moves, detailed marital transitions, and detailed employment transitions.[18] I also experiment with controls for birth order, whether the individual has an older sibling that graduated from high school, and whether the teenager moves into their own household by age 18 to see if these measures of individual heterogeneity change the estimate of community influences.[19]

IV. Results

<u>How Much Variation Is There in the Within-Family Measures?</u>

Because I elect to use within-family variation rather than across-family variation to identify community influences, I first report some findings in table 3 on the amount of variance that exists within families for four variables: grades completed, family income, and the two neighborhood variables. The first two rows display the mean and standard deviation for the entire sample. The third row gives the standard deviation within families. Within family standard

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[18] The employment transitions are: employment to unemployment, employment to retired, employment to temporarily laid off, unemployment to employment, retired to employment, and temporarily laid off to employment. The marriage transitions are: marriage to divorce, marriage to widow, divorce to marriage, single to marriage, and widow to marriage.

[19] I include the latter variable only as a test of the robustness of the results. Because of the endogeneity of the measure, it is probably best not included.

deviation estimates adjust for degrees of freedom lost in taking deviations from family means.[20] The fourth row reports the fraction of total variance that is within-family.

There appears to be plenty of variation in the education variable, with approximately 56 percent of the total variance in grades completed coming from within-family differences.[21] Unfortunately, there is much less variation in the neighborhood variables. About 7.5 to 13.6 percent of the total variance in the time-averaged neighborhood variables is attributable to within-family differences. Interestingly, this fraction of variance is consistent with other time-varying family variables, such as total averaged family money income, where 10.3 percent of the variance is from within-family differences. Therefore, family changes are not likely to dominate changes in neighborhood background characteristics. This finding, combined with those from table 1 on income and family composition changes before moves, makes it less likely that family changes will drive the neighborhood change parameters. As a result, while I remain cautious about the effect of family changes, there is reason to be optimistic that neighborhood effects can be reliably estimated, especially if observable family changes are controlled.

When the age 14 measure of the neighborhood variable is used, the within-family standard deviation rises to 0.057-0.060 (from 0.034-0.036 with the averaged neighborhood measure), and the fraction of the total variance due to within-family deviations is 18.2 percent for the poverty rate and 30.6 percent for the dropout rate, approximately twice as high as the averaged measures.[22] However, this difference is most likely picking up additional measurement error because of the shorter and less reliable window it measures. Results reported in the next section that use these measures are consistent with this measurement error story.

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[20] The within family standard deviation is calculated as $\sqrt{\mathrm{Var}(N_{if} - N_f) * \frac{I-1}{I-F}}$, where N_{if} is the value of the neighborhood variable for individual i in family f, N_f is the family mean of the neighborhood variable, I is the number of individuals in the sample, and F is the number of families.

[21] The correlation between siblings in grades completed, high school graduation, and college attendance is approximately .35-.38.

[22] The correlation among all siblings is around .9 for the two neighborhood measures. Correlation using the age 14 neighborhood measures are .85 for the poverty rate and .75 for the dropout rate.

The bottom of table 3 presents more information on the amount of differentiation that exists between siblings using the averaged neighborhood variables. Each row displays the percentage of sibling pairs whose average neighborhood background measure differs by 5, 10, 20, 30, or 50 percent.[23] Approximately 71 (76) percent of the sibling pairs live in an average neighborhood during ages 10-18 that is at least 5 percent different in poverty (dropout) rates. The percentage that lives in neighborhoods differentiated by at least 10 percent remains fairly high at 58 percent for the poverty measure and 62 percent for the dropout measure. At 20, 30 and 50 percent differentiation, fewer pairs qualify: 35 to 41 percent of the pairs grow up in 20 percent different average community environments and 7 to 11 percent grow up in 50 percent different average community environments. So while the majority of the variation is clearly across families, a small amount still exists across siblings.

Now, I turn to the estimation. First, I present some base case linear probability and logit models using high school graduation as the outcome measure. Next, I estimate two stage equations that are similar in spirit to Evans, Oates, and Schwab's (EOS from here on) instrumental variables technique for correcting neighborhood selectivity. I then present the main part of the analysis, the fixed effect equations. All results to this point use high school graduation as the outcome measure and poverty and dropout rates as the neighborhood proxy. The next section tests the robustness of the findings to changes in the neighborhood proxy, outcome measure, and sample. Some further tests to determine the importance of data variability are also reported in the following section.

Single Stage Estimates

Table 4 displays neighborhood coefficients from simple, one-stage ordinary least squares and logit high school graduation equations using the 2,178 children in the sibling sample. Full linear probability and logit regression results for a few selected equations are reported in appendices 1a and 1b. The appendices include findings from the sibling sample and the all youth sample but table 4 reports results only for the sibling sample.

[23] This is not an absolute deviation but rather a relative deviation. Therefore, a poverty rate of 13 percent for one sibling versus 10 percent for another is reported as a 30 percent difference in table 3.

The top row of table 4 reports the results from regressions that allow the neighborhood variable to enter log-linearly.[24] For both the neighborhood poverty and dropout rates, higher values signify a reduced probability of graduating from high school. As an interpretation of the size of these (linear probability) coefficients, a 10 percentage point increase in the neighborhood poverty rate would reduce the likelihood of graduating from high school by 2.1 percent. The corresponding impact of a 10 percentage point increase in the dropout rate is 3.6 percent. These effects seem fairly large, so I reran the regressions using the all-youth sample of 4,410 children that does not require the existence of a 3 year age-separated sibling. This sample produces 35 percent smaller point estimates than the sibling sample, but tests of the sample coefficients show that these differences are not statistically significant. The all-youth sample results are in line with the small neighborhood effects findings from previous work. Therefore, it should be kept in mind that the estimates presented below are representative of the impact of neighborhood, family, and individual characteristics on the educational attainment of children from *large* families.

These regressions include controls for race, gender, household income, parents' marital status, whether the father or mother graduated from high school, whether the kid worked, the number of kids in the household, and the county unskilled wage rate. Because these variables are mostly averages over the child's youth from ages 10 to 18, they may not capture important fluctuations in environmental conditions. These fluctuations may be critical if changes in neighborhood conditions are picking up unobserved heterogeneity in family or individual background rather than the true effects of the community. Therefore, I experimented with a variety of such measures to see if omitting them affects the magnitude of the neighborhood coefficient. In particular, I tried the percentage of years that the household moved, the variance of family income over the youth's data, detailed transitions into employment, detailed transitions into marital status, whether the individual moved into her own household by age 18, birth order, and whether the individual has an older sibling that graduated from high school. The results in table 4 include the marital and employment transitions. Many of these factors are important determinants in the probability of graduating from high school, particularly the mobility measures

[24] The analyses to follow are similar if the neighborhood measure is specified linearly.

and whether the individual moved into her own household. However, none significantly affect the magnitude of the neighborhood coefficient. While I may not be picking up other important factors that might be correlated with the neighborhood variable, I am reassured that adding these factors, especially the indicator variable for moves into own household, do not affect the neighborhood parameters.

The bottom of table 4 reports the neighborhood coefficients when they are allowed to enter nonlinearly. A spline is created at the 25th, 50th, and 75th percentile of the neighborhood measure and also at only the 90th percentile to determine if there are nonlinear slopes in the neighborhood coefficient depending on the "quality" of the neighborhood. In both the poverty rate and dropout rate cases, there does not appear to be much evidence that such a nonlinearity exists. A notable exception is that the neighborhoods in the bottom decile of dropout rates (above 28 percent) exhibit stronger effects in the linear probability case. However, no such pattern is detected in the logit model. Therefore, the remaining analysis ignores any possible spline effects.[25]

A Brief Note on IV Estimates

Before presenting the fixed effect estimates, I tried to replicate EOS's two stage estimator that uses a variety of metropolitan area characteristics to instrument for neighborhood selection. They find that single stage equations show a significant impact of neighborhoods on teen birth and high school dropout rates but modeling the selection process using IV eliminates this entire effect. Although I use a different data set than EOS, the results are quite similar when using the neighborhood poverty rate.[26] This is reassuring since their peer variable is similar to the poverty rate variable used here. However, when the dropout rate is used as the neighborhood measure, the

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[25] I also looked at interactions between the neighborhood variables and a number of the family and individual characteristics to see if nonlinearities enter this way. The importance of these nonlinearities is sensitive to the choice of the neighborhood measure. For the poverty variable, only the gender interaction is significant. Females are less likely to be affected by high community poverty rates. The dropout rate interactions appear to be more important. Income and 'whether worked' interactions are positive and significant at the one percent level; the number of kids in the household is negative and significant at the one percent level. These results suggest that kids who do not work during their youth and are from lower income households with more children are more susceptible to negative neighborhoods externalities when the youth dropout rate is higher.

[26] The results are available upon request.

findings are different. Using EOS's instruments, the neighborhood effects are still of the expected sign and the point estimate is bigger than the single stage results, although they are insignificant at any conventional level due to a substantial increase in the standard error. Therefore, my IV results suggest that controlling for selectivity can eliminate the significant effect of neighborhoods on children's high school graduation. But this conclusion is fairly sensitive and prone to substantial increases in imprecision.

Fixed Effect Estimates

As explained earlier, an alternative approach to correcting the selectivity bias, or at least the family-specific component of location decisions, is to estimate family fixed effect equations. Table 5 reports these estimates using eight methods.[27] In row one of table 5, the 2,178 individuals are paired off with siblings that meet the age criterion. This leaves 1,892 sibling pairs that are differenced to eliminate the family constant error term. Full results of this equation are reported in appendix 2. This procedure has a large impact on the poverty rate measures. The point estimate of -0.144 corresponds to a seven percent decrease in the likelihood of graduating when the neighborhood poverty rate increases by ten percent. This result is significant at the two percent level. On the other hand, the dropout rate coefficient is not affected by the first difference estimator. The coefficient increases slightly from -0.061 in the linear probability model to -0.068 in the first difference model. However, a substantial increase in the standard error produces an increase in the p-value to about the six percent significance level.

These findings are robust to adding more controls to account for transitions that may differ across siblings. Like the linear probability regressions, I add controls for the variance of money income and labor income, the percentage of years that the household moves, and whether the teenager moved into her own household by age 18. None of these variables, individually or as a group, affects the neighborhood estimates using either neighborhood measure. Even the 'own household' variable, which is highly significant in the regressions, has no indirect effect on the magnitude of the neighborhood estimates.

[27] The regressions include controls for employment and marital transitions that occur after the older child has left the parents' home. Therefore, these variables should measure changes in employment and family states that are experienced by the younger sibling but not the older sibling in the pair.

These findings form the basic results. However, as noted in section II, the inferences would be more convincing if the estimates were consistent across a number of changes in the specification and assumptions of the model. In the second row, I weight the sample as described in section II using the inverse of the number of times each person is included in a sibling pair. This change has no effect on either neighborhood measure. In row three, a single fixed effect is employed for each family. This lowers the coefficients and standard errors on both measures, but the findings remain similar to the pairwise estimates.

A simpler method to alleviate concern about over sampling is to choose one random pair of siblings from each family. This is shown in row four, where only the oldest and youngest siblings from the 742 families are used. The point estimate for the dropout variable remains the same but the poverty rate estimate (standard error) is smaller at -.115 (.067) than the other first difference estimates, although still remaining almost one standard deviation higher than the simple linear probability estimates.

All of these estimates use two sources of variation to identify the neighborhood coefficient: time and differences in residential location. Time influences these results because the interpolation of the decennial census figures implies that neighborhood conditions will differ between siblings of different ages even if they live in the same neighborhood.[28] To see whether this time component is driving the results, in rows five and six, I reestimate the unweighted pairwise first difference estimator in row one using only those sibling pairs who lived in different neighborhoods at age 14 (row five) and who have a different average neighborhood during their youth (row six). The latter restriction eliminates only those pairs who never moved. Although the point estimates in the age 14 estimator are very similar in magnitude to the other estimates in table 5, the small sample from using the different neighborhood age 14 estimator results in huge standard errors and thus insignificant estimates. The different average neighborhood restriction results in an increase in the point estimate of the poverty rate and no change in the dropout rate.

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[28] Secular trends in high school graduation rates are controlled with year turned age 15 dummies. Here, I use time to refer to within-neighborhood changes over time (as opposed to across neighborhood changes due to household moves).

In rows 7-9, I return to the logistic framework. Row 8 reports unweighted logit estimates using Chamberlain's fixed effect logit model. Because only pairs of siblings that had different educational outcomes contribute to the likelihood function, 441 of the 1,892 pairs are usable. The conditional logit coefficients are in different units and therefore are not comparable to the single stage logit. But the estimates, like the linear probability estimates in row one, are significant and of the expected sign for both neighborhood measures. Other coefficients in the model seem to react similarly in the conditional logit and linear probability equations.

Finally, in row 9, logit equations were estimated using the Mundlak formulation where separate within-family and across-family variables are defined. For the poverty rate measure, ϕ_1 (the within-family neighborhood coefficient) is -0.872 (0.432) and ϕ_2 is 0.415 (0.456). For the dropout rate, ϕ_1 is -0.407 (0.357) and ϕ_2 is -0.436 (0.402). Not surprisingly given their similarity, the findings on ϕ_1 are very similar in terms of t-values to the one family fixed effect estimates in row 3. In terms of other sibling or time-varying variables, the within-family coefficient is significant for the female indicator, marital status, and whether worked dummy. Family income and marital status are highly significant in the across-family point estimates.

V. Robustness Checks

Using Different Neighborhood Measures

How sensitive are the results to the choice and computation of the neighborhood proxy? To test this measurement issue, I reran the models using different proxies and aggregations of the neighborhood variable, all of which have appeared in the literature in some form. Tables 6 and 7 report the results of this investigation.

In table 6, I examine whether the way the neighborhood measure is calculated has any bearing on the findings. In particular, this table looks at the effect of the imputation scheme and the averaging of the neighborhood variable. Linear probability and unweighted fixed effects estimates are reported for four variants of the neighborhood measure. First, in row one, I report the basic results from earlier tables that use an imputed, time-averaged neighborhood measure. As described in section III, the imputation refers to the linear interpolation between 1970 and

1980 of the decennial Census variables, which allows some variation in the neighborhood measure from time. The time averaging refers to the averaging of variables from ages 10 to 18 for each sibling. In row two, I allow no imputation, setting the neighborhood measure for each year equal to the 1980 Census report for that neighborhood. Therefore, all sibling differences in neighborhood measures will be from neighborhood moves. The result of this change is small. The fixed effect estimator even increases for both the poverty and dropout equations. Therefore, biases caused by the current imputation scheme, if anything, dampen the size of the neighborhood effect, suggesting that this data assumption is probably not a problem.

In rows three and four, I use the age 14 neighborhood measure instead of the average neighborhood characteristic. I run this experiment because one-year windows are a common way to measure the influence of neighborhoods and schools on children. However, this variable may not be a reliable measure of the true effects of neighborhoods (or any other time-varying covariate) since it is ignores the rest of the individual's neighborhood history and thus potentially introduces more measurement noise into the estimation. However, as a methodological and comparative point, it seems to be an useful exercise to compare the results using this measure with the averaged measure.

The magnitude of the age 14 estimates is quite different from the averaged variable. In row three, the census imputation is allowed. Both the fixed effect and linear probability estimators are insignificant for the poverty measure; the dropout measure shows significant effects with the linear probability estimate but slightly smaller and insignificant results using the fixed effect equation due to a doubling of the standard errors. Furthermore, the size of the linear probability coefficients is smaller, although still significant. Row four drops the imputation and finds that the fixed effect estimator is significant at the 10 percent level for the dropout rate but not significantly different from zero with regard to the poverty rate. It would be comforting to find that the results using the age 14 measures match the findings from the averaged variable measures. That this is not the case is not a fatal contradiction. First, the findings are somewhat supportive of the main conclusion of the fixed effect estimates; correcting for selectivity and unobserved family heterogeneity does not completely eliminate the possibility of community influences. While three out of four of the findings are insignificant at even the 10 percent level, the point estimates are in line with the linear probability coefficients, just much less precisely estimated. Second, I would expect that the age 14 measure is not as good a proxy of the youth's full history of neighborhood background influences and thus is more prone to measurement error. Therefore, while it would comforting to find that the age 14 measure and the averaged measure come to exactly the same conclusions, it is not surprising that they do not.[29]

As a second test, the linear probability and unweighted fixed effect estimators were rerun for three other neighborhood variables commonly used in the literature: the percentage of households headed by females, the percentage of the population that is white, and average household income. The findings are reported in table 7. Column one reports linear probability estimates using each of the five neighborhood variables. To gauge the relative size of these coefficients against each other, column two displays derivatives calculated at the mean for each measure. The size of these derivatives is fairly stable across the variables, with the exception being the white composition, which is about the same size as the other variables for white students but zero for nonwhite students.

Column three reports the fixed effect estimates. The three variables not discussed above have point estimates very similar to the linear probability model, but with standard errors roughly three times as large. In all three cases, the increase in standard errors result in insignificant neighborhood effects. However, no cases show the dramatic changes in magnitude, much less sign switches, that are reported in EOS. However, the lack of precision of the within-family estimates does not discount this possibility.

Using Different Outcome Measures

In table 8, I explore how the fixed effect estimator influences two other education outcome variables -- whether the individual attended college and the number of grades completed by age 25. Two points need to be made about variable definitions and the sample. First, in order to maximize sample size and to avoid problems due to attrition, I include all individuals who were

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[29] These findings on single age windows versus averaged values is consistent with those reported in An, Haveman and Wolfe (1992).

in the sample and had a grade completion report after age 19, much like the high school graduation equations. However, this may cause problems, particularly in the grades reported variable, since differences in grades reported may be partly due to differences in age. Therefore, a variable that measures the last age in the sample used to determine grade completed is included. Second, I drop 41 individuals for whom high school graduation was inferred from their status as students well into their twenties but who never report a grade completed due to attrition or the end of the sampling period. This sampling alteration makes little difference to the findings.

In rows 1,2, and 4, I report the neighborhood coefficients for equations that employ high school graduation, college attendance, and number of grades completed as the outcome measure. The regressions were also run for logit models with identical implications. The findings suggest that the effect of neighborhoods on college attendance is much smaller than on high school graduation, especially when looking at a subsample of siblings who graduated from high school (row 3). Once again, the neighborhood proxies seem to be acting quite differently. Fixed effect estimates using the poverty rate measure suggest that unobserved heterogeneity does not eliminate the effect of communities on educational attainment. In all three outcome measures, point estimates (and standard error) increase in magnitude, but remain relatively constant in significance. The dropout rate shows strong support for neighborhood effects in the linear probability and logit specifications, but no evidence of neighborhood effects in the fixed effect models, especially with regard to the college attendance and grades completed outcomes. Further analysis using the percentage of households over $30,000 in 1979 dollars as the neighborhood proxy finds some support for the importance of neighborhood conditions on college attendance decisions.

Using Different Samples--Separating the Responses By Race, Gender, and Income

It may also be of interest to see how stratifying the sample by race and gender affects the magnitude and significance of the neighborhood estimates. Table 9 reports these results using the high school graduation rate as the dependent variable. The first rows classify the sample by race. The nonwhite sample experiences larger neighborhood influences than the white population as measured in the single stage framework when either the poverty rate or dropout rate is used as the neighborhood variable. However, the first difference estimator suggests larger effects in the white sample for both neighborhood measures. In the nonwhite sample, no statistically significant effect is found. None of these results are statistically different across groups at conventional significance levels.

When sisters and brothers are stratified in rows three and four, the results are rather surprising. Row three (four) include all females (males) in the linear probability estimates but only sister (brother) pairs in the fixed effect regressions. The sisters do not seem to react to poverty conditions, especially relative to the brothers. However the sisters have a strong response to neighborhood dropout rates. The brothers respond strongly to both neighborhood conditions, although heterogeneity corrections have quite different effects depending on the neighborhood proxy used. With regard to the poverty rate, the point estimates are extremely large (although so are the standard errors), while the dropout rate parameters are similar to those found in the aggregate. Again, although some differences arise between the two groups, these differences are not statistically significant.

A Test of Data Variability

Given the small variation that I rely on to identify the point estimates, it might be useful to redo the analysis using sibling pairs that experience larger neighborhood differentials.[30] There may be interest in sibling pairs with larger neighborhood differences if there is concern that those with small differences are especially noisy. However, there is a trade-off as the sample with larger differences is more susceptible to bias from latent family characteristics that may have caused large changes in neighborhood location. Furthermore, a priori, it is not clear whether the neighborhood influence should be larger, smaller, or the same size with greater differences in sibling neighborhood backgrounds. If families are selecting neighborhoods based on the differential ability of their children, then larger changes in the quality of the neighborhood would

[30]This issue is essentially one of measurement error. Since, classical measurement error is likely to lead to a larger downward bias in the fixed effect estimates, it is not of great concern in this case. I did run some FE-IV models to formally account for measurement error in the difference neighborhood input, but because of the difficulty in finding a reliable instrument for this model, it is not clear that these techniques will solve any problem. Further, the results are hard to interpret. The standard errors increase dramatically with the loss of efficiency overwhelming any information that might allow better estimates of the neighborhood parameters.

show bigger effects on the outcome measures if the family is following a reinforcing strategy (moving to better neighborhoods to accommodate the more able child) and smaller effects if the family is following a compensating strategy. If there is no differential selection, then the estimates should be roughly the same magnitude regardless of the size of the neighborhood characteristic difference.

Table 10 gives the results when the high school graduation model is rerun on samples that are stratified based on the percent difference in the neighborhood characteristics between the siblings. In row zero, the unweighted pairwise estimator from table 5 is reported as the base case. Rows one through four break down the sibling sample into those pairs with 5,10, 20, and 30 percent differences in their neighborhood measures. The results are consistent, although far from conclusive, that parents do not select neighborhoods based on the ability of their children. There does not appear to be much difference in the point estimates across the categories. For the dropout rate, a Wald test of the equality of coefficients overwhelmingly shows no difference in the magnitude of the neighborhood impact even when the sample is limited to only those pairs whose average neighborhood dropout rate is 30 percent different. As for the poverty rate, the larger differences, especially the 30 percent level, have somewhat smaller effects, but these differences are statistically indistinguishable from all the other categories. When the sample excludes the largest sibling differentials, the results are also comparable, suggesting that outliers are not driving these findings. Therefore, the results seem robust to the neighborhood differential used.

Reconciling The Findings With Plotnick and Hoffman

Plotnick and Hoffman (1995) use the same family fixed effect approach and find no evidence that neighborhoods matter. The discrepancy between our results is partly due to differences in variable and sample definition. This is exemplified in the robustness checks of tables 6 to 9. Many of their specification and variable definition choices are shown in these tables to result in insignificant neighborhood coefficients. In particular, I highlight four issues.

First, Plotnick and Hoffman's neighborhood measures are composed of averages over three years (age 16 to 18). Results reported in table 6 suggest that shorter time frames can lead to a reduction in parameter estimates. An, Haveman, and Wolfe (1992) argue that these shorter windows are consistent with added measurement error, which is likely to bias estimates downward. Furthermore, Plotnick and Hoffman acknowledge that the use of a 16 to 18 window ignores potential effects at earlier ages or the accumulation of neighborhood significance over many years. Second, several of the neighborhood measures employed in table 7, some of which are used in Plotnick and Hoffman's paper, display insignificant neighborhood effects. Therefore, the results are sensitive to the precise neighborhood measure chosen. Third, their education dependent variable is post-secondary schooling, which I show in table 8 to display a much weaker impact from neighborhood conditions. The stronger effects arise in high school graduation outcomes. Fourth, Plotnick and Hoffman include only sister pairs, which I find in table 9 to display smaller neighborhood effects than brothers and brother-sister pairings. Some of these specification, sample, and variable definitions are arbitrary, especially the choice of neighborhood measure. However, the averaging problem seems to be an important measurement issue where more 'permanent' covariates are preferable. Other issues, such as the choice of dependent variable and the sampling of sisters versus brothers, suggests that neighborhoods could matter in certain cases.

VI. Conclusions

A well-known complication of estimating the influence of neighborhoods on children's outcomes arises because families are not randomly assigned to neighborhoods but rather choose their location based on many factors, including the importance they place on their children's welfare. As a result, the effects of family unobservables, such as parental competence, taste for education, and time spent with their children, and other unobservables that are common to geographically clustered households, may be mistakenly attributed to the neighborhood measures. Previous studies that attempt to correct for this selection bias have used questionable instrument variables.

This paper introduces an approach that relies on the observation that the latent factors associated with neighborhood choice do not vary across siblings. Therefore, family residential changes provide a source of neighborhood background variation within families that is free of family-specific heterogeneity biases associated with neighborhood selection. This approach is feasible because of the high levels of residential migration in the United States. Using a sample of multiple-child PSID families where the kids are separated in age by at least three years, I estimate family fixed effect equations of children's educational outcomes. The fixed effect results suggest that the impact of neighborhoods exists even when family-specific unobservables are controlled. In fact, family fixed effect regressions that use the neighborhood poverty rate as the proxy for community conditions show even larger community effects on high school graduation and grades completed compared with the models without fixed effects. When the neighborhood dropout rate is employed, there appears to be little difference in point estimates between the fixed effect high school graduation equations and the simple linear probability or logit results, but the effect on college attendance and grades disappears. Other neighborhood proxies show similar patterns. When stratified by race and gender, whites and males are impacted the most by neighborhood conditions in the fixed effect specifications. Therefore, the results suggest that, contrary to Evans, Oates, and Schwab's findings, corrections for neighborhood selection biases do not necessarily eliminate the potential for significant community effects.

However, the findings are tempered to some degree by large standard errors due to small sample sizes and noise that might arise if there is not enough variation in the differenced neighborhood variables. While attempts to control for family environment are introduced, there is also the possibility that the empirical models have not adequately isolated latent changes in family background or individual sibling heterogeneity. This is exemplified by the surprising finding that parameters sometimes increase when moving from the single stage models to the fixed effect models. Therefore, in future research, I hope to replicate the results on a different sample of the PSID (younger children using grade retention data currently being collected) or a different data set, such as the National Longitudinal Survey of Youth.

Table 1
Changes in Household Income, Employment Status and Family Composition Preceding a Residential Move (1,2
1971-1974, 1980-1983

	Stayers (1)	State Movers (2)	County Movers (3)	Residence Movers (4)	Neighborhood Movers: All (5)	Neighborhood Movers: Absolute poverty rate relative to previous neighborhood: Higher (6)	5% Higher (7)	Lower (8)	5% Lower (9)
Family years (3	3,705	44	109	589	387	164	102	205	147
Families	911	39	82	371	260	143	94	174	127
Family money income (4									
in move year	32.95	32.42	30.50	24.44	25.42	25.51	22.55	24.96	24.97
Change (t-1,t)	0.57	-0.66	-0.80	-0.42 *	-0.57 *	-1.86 **	-0.64	0.28	0.88
Change (t-2,t)	0.99	-0.58	-0.40	0.31	0.30	-0.59	-0.53	1.09	1.27
Head and wife labor income (4									
in move year	24.16	26.20	25.29	17.09	17.42	18.19	13.62	16.32	15.59
Change (t-1,t)	0.05	-2.06	-0.67	-0.59	-0.86 *	-1.46 **	-1.33	-0.58	-0.02
Change (t-2,t)	-0.11	-3.29 *	-0.39	-0.18	-0.47	-0.98	-0.99	-0.26	0.26
Parents' marital status									
in move year	0.737	0.727	0.798	0.565	0.553	0.530	0.402	0.566	0.551
Change (t-1,t)	-0.015	-0.046	0.055 ***	-0.014	-0.018	-0.019	-0.049 *	-0.014	0.014 *
Change (t-2,t)	-0.024	-0.046	0.027 **	-0.051 **	-0.059 **	-0.055	-0.069 *	-0.063 **	-0.020
Married->divorced	0.030	0.114 ***	0.064 **	0.087 ***	0.093 ***	0.091 ***	0.098 ***	0.102 ***	0.082 ***
Divorced->married	0.014	0.114 ***	0.110 ***	0.058 ***	0.054 **	0.037	0.020 #	0.073 ***	0.095 ***
Head's employment status									
in move year	0.800	0.818	0.844	0.696	0.703	0.677	0.588	0.727	0.721
Change (t-1,t)	-0.014	-0.023	0.046 **	0.012 **	0.026 **	0.012	0.010	0.049 ***	0.061 ***
Change (t-2,t)	-0.021	0.000	0.018	-0.014	-0.008	-0.030	-0.039	0.020 *	0.048 ***
Employed->unempl.	0.022	0.023	0.037	0.034 *	0.036 *	0.037	0.039	0.034	0.020
Employed->retired	0.018	0.023	0.037	0.051 ***	0.054 ***	0.043 **	0.039	0.068 ***	0.088 ***
Employed->laid off	0.020	0.068 **	0.064 ***	0.044 ***	0.036 **	0.037	0.049 **	0.039	0.020
Unemployed->empl.	0.013	0.023	0.055 ***	0.031 ***	0.034 ***	0.012 #	0.020	0.054 ***	0.048 ***
Retired->employed	0.022	0.045	0.028	0.037 **	0.041 **	0.043 *	0.059 **	0.039	0.020
Laid off->employed	0.018	0.045	0.018	0.026	0.018	0.012	0.020	0.024	0.027

Notes:

1) Asterisks represent significance levels from mean tests of the mover groups against the stayer group. *(**,***)=mean of the movers' characteristics is different from the stayers at the 10% (5%,1%) level. # means that columns (6) or (7) are significantly different from columns (8) or (9) at the 5 percent level.
2) The sample includes households that have a child under age 17 living in the household. Only years 1971-1974 and 1980-1983 are used to avoid difficulties in determining when geographic moves occurred during periods when geocode data is missing (1969, 1975, 1977, 1978).
3) Family year observations are included for all moves where that period's income can be determined. Since the PSID does not report income until the following year, geographic moves during the final year of a household's response are not included.
4) Income is in thousands of 1982-1984 dollars.

Table 2
Descriptive Statistics of Main Individual, Neighborhood and Family Variables (1
Weighted by PSID Sample Weights

	Sibling sample		All youth sample	
	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)
High school graduate	0.871	0.335	0.878	0.327
College attendance	0.425	0.494	0.445	0.497
Number of grades completed	12.88	1.89	12.99	1.95
Nonwhite	0.186	0.389	0.177	0.382
Female	0.494	0.500	0.493	0.500
Percent worked during youth	0.801	0.400	0.808	0.394
Number of kids in household	3.23	1.56	2.97	1.60
Mom high school graduate	0.631	0.483	0.678	0.467
Dad high school graduate	0.561	0.496	0.595	0.491
Household money income (82-84 $)	40,046	23,737	40,482	24,452
Parents' married all years (2	0.740	0.439	0.722	0.448
Percentage of years that family moved between ages 10 and 18	0.122	0.168	0.131	0.175
Whether ever moved, ages 10 to 18	0.485	0.500	0.514	0.500
Neighborhood Characteristics:				
Percent households in poverty	0.125	0.097	0.127	0.097
Percent youth not employed or in school	0.134	0.094	0.131	0.095
Percent white	0.852	0.252	0.856	0.240
Percent female household heads	0.134	0.088	0.132	0.087
Average income	40,761	14,051	41,309	14,802
Head experienced at least one transition during ages 10-18 of youth:				
Married -> divorced	0.152	0.359	0.166	0.372
Married -> widowed	0.051	0.220	0.055	0.228
Divorced -> married	0.091	0.288	0.114	0.318
Single -> married	0.012	0.110	0.013	0.116
Widowed -> married	0.021	0.145	0.026	0.159
Employed -> unemployed	0.111	0.315	0.120	0.325
Employed -> retired	0.063	0.243	0.073	0.257
Employed -> temp. laid off	0.098	0.298	0.093	0.291
Employed -> disabled	0.022	0.146	0.028	0.165
Unemployed -> employed	0.088	0.284	0.091	0.288
Retired -> employed	0.068	0.252	0.071	0.257
Temp. laid off -> employed	0.115	0.320	0.110	0.313
Disabled -> employed	0.029	0.169	0.031	0.174
Number of unique individuals	2,178		4,410	
Number of unique families	742		1,199	

Notes:

1) Sibling sample includes all individuals with (1) one sibling that is three years apart in age, (2) two years of data between ages 10 and 14, and one year after age 18 that can distinguish whether the individual graduated from high school. All youth sample does not require condition (1). Variables are averaged for each individual between ages 10 and 18. Family background variables are averaged over the years that the person lived at home. Some 9% of the sample moved out of their parents' household by age 18.
2) Equals one if the parents stay married while the child is living at home between ages 10 and 18.

Table 3
Within-Family Variance in Some Key Variables

	Grades Completed (1)	Neighborhood Poverty Rate (2)	Neighborhood Dropout Rate (3)	Family Money Income (4)
Averaged Variable (1				
Mean of variable	12.52	0.198	0.167	30,433
Total standard deviation in sample	1.87	0.131	0.093	19,975
Standard deviation within families	1.40	0.036	0.034	6,403
Fraction of variance within families	0.560	0.075	0.136	0.103
Age 14 Variable (2				
Mean of variable		0.199	0.169	
Total standard deviation in sample		0.140	0.104	
Standard deviation within families		0.060	0.057	
Fraction of variance within families		0.182	0.306	
Percentage of sibling pairs whose neighborhood measures are different by:				
>5 %		0.71	0.76	
>10 %		0.58	0.62	
>20 %		0.35	0.41	
>30 %		0.21	0.26	
>50 %		0.07	0.11	

Notes:
1) Neighborhood and income variables are averaged over ages 10 to 18 for each sibling. Education outcome variables are based on the highest reported grade completed from age 19 to 25.
2) Variable is the measure at age 14 (or the closest age to 14).

Table 4
Effect of Neighborhood Poverty and Dropout Rate on High School Graduation Rates (1
Neighborhood Measures: log(poverty rate) or log(dropout rate)
Dependent variable: 1 if high school graduate
(Huber standard errors in parentheses) (2

	Neighborhood Measure: log(poverty rate)						Neighborhood Measure: log(dropout rate)					
	Linear Probability			Logit			Linear Probability			Logit		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Linear Specification	-0.042 ***	-0.034 *	-0.042 ***	-0.543 ***	-1.360 ***	-0.596 ***	-0.061 ***	-0.016	-0.047 ***	-0.805 ***	-0.784 **	-0.749 ***
	(0.016)	(0.019)	(0.016)	(0.155)	(0.352)	(0.164)	(0.013)	(0.013)	(0.012)	(0.163)	(0.371)	(0.178)
Spline Specification (3												
Neighborhood measure												
25th percentile		-0.002			0.249 *			-0.023 *			-0.053	
		(0.013)			(0.132)			(0.014)			(0.152)	
50 percentile		0.001			0.153			0.004			0.127	
		(0.015)			(0.109)			(0.017)			(0.118)	
75th percentile		-0.008			0.045			-0.034 *			-0.128	
		(0.016)			(0.096)			(0.018)			(0.121)	
90th percentile			-0.003			0.079			-0.045 **			-0.078
			(0.020)			(0.116)			(0.021)			(0.120)
Sign. level from test of nonlinear coefficients												
All spline slopes=0		0.943	0.867		0.088	0.506		0.009	0.035		0.678	0.516

Notes:
***=significant at 1% level **=significant at 5% level *=significant at 10% level

1) Sample size is 2,178. Regressions control for gender, race, parents' education, parents' marital status, household income, number of siblings, whether the child worked, the year the child turned 15, five marital transition variables, eight employment transition variables, and the wage for unskilled workers in the county of residence.
2) Standard errors corrected for clustering by 1968 neighborhood.
3) Regressions include a spline at the 25th, 50th, and 75th (or 90th) percentiles of the neighborhood measure. The breakpoints for the poverty rate are 8.9,17.5,28.0, and 38.3%. Corresponding breakpoints for the dropout rate are 9.9,16.2,22.6, and 28.0%.

Table 5

Effect of Neighborhood Poverty and Dropout Rates on High School Graduation

Fixed Effect Estimates (1

Neighborhood Measures: log(poverty rate) or log(dropout rate)

Dependent variable: 1 if high school graduate

(Huber standard errors in parentheses)

	Estimators (2	log (poverty)	log (dropout)	Sample Size
		(1)	(2)	(3)
	Linear Probability Models			
(0)	Base case	-0.042 ***	-0.061 ***	2,178
		(0.016)	(0.013)	
(1)	Unweighted pairwise first difference	-0.144 **	-0.068 *	1,892
		(0.060)	(0.036)	
(2)	Weighted pairwise first difference	-0.146 **	-0.068 *	1,892
		(0.060)	(0.037)	
(3)	Single family fixed effect	-0.129 **	-0.045	2,178
		(0.052)	(0.033)	
(4)	Oldest-Youngest pairs	-0.115 *	-0.068 *	742
		(0.067)	(0.038)	
(5)	Unweighted pairwise first difference	-0.110	-0.066	600
	Different age 14 neighborhood variable	(0.074)	(0.064)	
(6)	Unweighted pairwise first difference	-0.166 ***	-0.069 *	1,554
	Different average neighborhood variable	(0.060)	(0.042)	
	Logit Models			
(7)	Base case	-0.543 ***	-0.805 ***	2,178
		(0.155)	(0.163)	
(8)	Conditional logit pairwise sample	-1.250 ***	-0.774 **	441
		(0.408)	(0.390)	
(9)	Mundlak fixed effect logit	-0.872 **	-0.407	2,178
		(0.432)	(0.357)	

Notes:

***=significant at 1% level **=significant at 5% level *=significant at 10% level

1) Regressions control for gender, race, parents' education, parents' marital status, household income, number of siblings, whether the child worked, the year the child turned 15, the wage for unskilled workers in the county of residence, the variance of money income while the youth lives at home, five parent marital transition variables, and eight head employment transition variables. These transition variables are listed in table 2. Only transitions that are experienced by the younger child (ie. after the older child has left the household) are coded as 1 in the difference estimators.

2) See text for explanation of different estimators.

Table 6
The Effect of the Neighborhood Imputation and Averaging
on the High School Graduation Results (1
Neighborhood Measures: log(poverty rate) or log(dropout rate)
Dependent variable: 1 if high school graduate
(Huber standard errors in parentheses)

	log(poverty rate)		log(dropout rate)	
	Linear Probability (1)	Unweighted Fixed Effect (2)	Linear Probability (3)	Unweighted Fixed Effect (4)
Imputed, time averaged (3,5	-0.042 ***	-0.144 **	-0.061 ***	-0.068 *
	(0.016)	(0.060)	(0.013)	(0.036)
No imputation, time avg. (3,6	-0.044 ***	-0.142 **	-0.055 ***	-0.081 **
	(0.017)	(0.064)	(0.014)	(0.041)
Imputed, age 14 (4,5	-0.024	-0.042	-0.048 ***	-0.037
	(0.015)	(0.037)	(0.012)	(0.025)
No imputation, age 14 (4,6	-0.030 **	-0.020	-0.040 ***	-0.053 *
	(0.015)	(0.041)	(0.011)	(0.028)

Notes:
***=significant at 1% level **=significant at 5% level *=significant at 10% level
1) See notes to tables 4 and 5 for list of control variables.
2) Instruments are county poverty rate, unemployment rate, average household income, and percentage of adults who did not graduate from high school.
3) Neighborhood variables are averaged over ages 10 to 18.
4) Neighborhood variables set to age 14 (or closest age to 14) measure.
5) Neighborhood measures are imputed between Census years (1969 and 1979) and held constant at 1969 (and 1979) values before1969 (after 1979)
6) No imputations are calculated. All neighborhood measures are from the 1980 Census reports.

Table 7
Linear Probability and Fixed Effect Estimates
of Neighborhood Impact on High School Graduation
Using Different Neighborhood Proxies (1
Dependent variable: 1 if high school graduate
(Huber standard errors in parentheses)

	Neighborhood Measure (2	Linear Probability (1)	Derivative at Mean (2)	Unweighted Fixed Effect (3)
(1)	Poverty rate	-0.042 ***	-0.0021	-0.144 **
		(0.016)		(0.060)
(2)	Dropout rate	-0.061 ***	-0.0036	-0.068 *
		(0.013)		(0.036)
(3)	Percent white population	0.012	0.0002	-0.001
		(0.012)		(0.029)
(4)	Percent households that are female headed	-0.060 ***	-0.0029	-0.089
		(0.021)		(0.066)
(5)	Average income	0.073 **	0.0021	0.051
		(0.038)		(0.100)

Notes:
***=significant at 1% level **=significant at 5% level
*=significant at 10% level
1) See notes to tables 4 and 5 for list of controls.
2) Neighborhood measures are entered into the high school graduation equations one at a time.

Table 8
The Effect of Neighborhoods on Different Educational Outcome Measures (1
Neighborhood Measures: log(poverty rate) or log(dropout rate)
(Huber standard errors in parantheses)

Outcome Measure		Unweighted Mean of Outcome (1)	log(poverty rate) Linear Probability (2)	log(poverty rate) Unweighted Fixed Effects (3)	log(dropout rate) Linear Probability (4)	log(dropout rate) Unweighted Fixed Effects (5)
(1) High school graduation		0.803	-0.038 **	-0.165 ***	-0.056 ***	-0.064
			(0.017)	(0.060)	(0.013)	(0.040)
(2) College attendance		0.351	-0.037 *	-0.082	-0.081 ***	-0.012
			(0.020)	(0.050)	(0.017)	(0.039)
(3) College attendance conditional on h.s. graduation		0.437	-0.027	-0.058	-0.068 ***	0.033
			(0.022)	(0.067)	(0.018)	(0.048)
(4) Grades completed (2		12.52	-0.180 **	-0.519 ***	-0.362 ***	-0.082
			(0.080)	(0.199)	(0.068)	(0.148)
Sample Size (3	Rows (1,2,4)		2,137	1,822	2,137	1,822
	Row (3)		1,716	1,206	1,716	1,206

Notes:
***=significant at 1% level   **=significant at 5% level
*=significant at 10% level

1) See tables 4 and 5 for list of control variables. Regressions also include the maximum age used to determine an individual's educational outcome measure.
2) Maximum number of grades reported from age 19 to 25. If no grades have been reported by age 25 (ie. the indiviudal is still a student), then the first grade report after age 25 is used.
3) The sample includes only those siblings where grades completed are easily determined. This eliminates 41 individuals who were assumed to be high school graduates in previous tables because they were still students in their early 20s when they attrited from the sample. This assumption makes no difference to the results. Row 3 includes only those siblings who graduated from high school.

Table 9
Effects of Neighborhoods on High School Graduation, By Race, Gender, and Income
(Huber standard errors in parentheses)
Neighborhood Measures: log(poverty rate) or log(dropout rate)
Dependent variable: 1 if high school graduate
[sample size in brackets]

	log(poverty rate)		log(dropout rate)			
Sample	Linear probability (1)	Unweighted fixed effect (2)	Linear probability (3)	Unweighted fixed effect (4)	Sample size (2 (5)	Sample pairs (3 (6)
White	-0.013	-0.204 ***	-0.031 ***	-0.077 **	985	769
	(0.017)	(0.073)	(0.012)	(0.037)		
Nonwhite	-0.055 *	-0.092	-0.097 ***	-0.040	1,193	1,123
	(0.031)	(0.086)	(0.032)	(0.072)		
Female (4	-0.016	-0.030	-0.077 ***	-0.086 *	1,104	495
	(0.019)	(0.083)	(0.015)	(0.051)		
Male (5	-0.057 **	-0.218 **	-0.039 **	-0.083	1,074	449
	(0.023)	(0.087)	(0.017)	(0.074)		

Notes:
***=significant at 1% level **=significant at 5% level *=significant at 10% level
1) Instruments are county poverty rate, unemployment rate, average household income, and percentage of adults who did not graduate from high school.
2) Sample size for linear probability and IV models.
3) Sample size of sibling pairs for pairwise first difference model. See text for explananation.
4) Fixed effect sample includes only those sibling pairs that are sisters.
5) Fixed effect sample includes only those sibling pairs that are brothers.

Table 10
The Importance of Sibling Differences in Neighborhood Background
on High School Graduation Rates
Unweighted First Difference Equations
Neighborhood Measures: log(poverty rate) or log(dropout rate)
Dependent variable: 1 if high school graduate
(Huber standard errors in parentheses)
[Sample size in brackets]

Row	Neighborhood Differential (1	Poverty Rate (1)	Dropout Rate (2)
(0)	≥ 0%	-0.144 **	-0.068 *
		(0.060)	(0.036)
		[1,892]	[1,892]
(1)	> 5%	-0.151 **	-0.070 *
		(0.059)	(0.037)
		[1,350]	[1,445]
(2)	> 10%	-0.148 **	-0.070 *
		(0.059)	(0.037)
		[1,101]	[1,173]
(3)	> 20%	-0.126 **	-0.070 *
		(0.058)	(0.036)
		[657]	[779]
(4)	> 30%	-0.108 *	-0.072 *
		(0.062)	(0.038)
		[398]	[488]
Significance level from Wald test of difference between row 0 and row 4		0.89	0.98

Notes:
***=significant at 1% level **=significant at 5% level
*=significant at 10% level

1) Neighborhood differential refers to the percentage difference between sibling pairs in their average neighborhood characteristic. For example, the >10% category includes only those sibling pairs whose average poverty rate (or dropout rate) differs by more than ten percent.

Appendix 1a
Linear Probability High School Graduation Regressions
Dependent variable: 1 if high school graduate
(Huber standard errors in parentheses)

	Neighborhood measure: log(poverty rate)			Neighborhood measure: log(dropout rate)		
	Sibling sample		All youth sample	Sibling sample		All youth sample
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.317	0.470 *	0.210	0.320	0.424	0.198
	(0.298)	(0.287)	(0.199)	(0.279)	(0.273)	(0.187)
Neighborhood variable	-0.042 ***	-0.041 **	-0.024 **	-0.061 **	-0.049 ***	-0.034 ***
	(0.016)	(0.016)	(0.011)	(0.013)	(0.012)	(0.008)
Whether female	0.060 ***	0.079 ***	0.061 ***	0.062 ***	0.081 ***	0.061 ***
	(0.015)	(0.015)	(0.011)	(0.015)	(0.015)	(0.011)
Log(household income)	0.056 **	0.046 *	0.067 ***	0.060 **	0.052 **	0.070 ***
	(0.028)	(0.027)	(0.018)	(0.027)	(0.026)	(0.018)
Whether nonwhite	0.086 ***	0.058 **	0.049 **	0.079 ***	0.049 **	0.046 **
	(0.028)	(0.028)	(0.020)	(0.027)	(0.026)	(0.019)
Parents' married	-0.012	-0.020	-0.016	-0.010	-0.019	-0.016
	(0.033)	(0.031)	(0.021)	(0.032)	(0.031)	(0.021)
Dad high school grad.	0.063 ***	0.059 ***	0.060 ***	0.057 **	0.055 **	0.057 ***
	(0.023)	(0.023)	(0.017)	(0.024)	(0.023)	(0.017)
Mom high school grad.	0.084 ***	0.077 ***	0.070 ***	0.081 ***	0.076 ***	0.068 ***
	(0.023)	(0.021)	(0.015)	(0.023)	(0.021)	(0.016)
No. kids in household	-0.031 ***	-0.027 ***	-0.020 ***	-0.031 ***	-0.027 ***	-0.020 ***
	(0.007)	(0.007)	(0.004)	(0.007)	(0.007)	(0.004)
Whether kid worked	0.068 ***	0.053 ***	0.043 ***	0.069 ***	0.056 ***	0.043 ***
	(0.020)	(0.020)	(0.014)	(0.020)	(0.020)	(0.014)
County unskilled wage	0.015	0.015	0.004	0.017 *	0.017 *	0.005
	(0.010)	(0.010)	(0.007)	(0.010)	(0.010)	(0.007)
Variance of income	-0.020	-0.015	-0.004	-0.021	-0.016	-0.006
	(0.015)	(0.015)	(0.006)	(0.016)	(0.016)	(0.006)
Parents divorced while	-0.083 **	-0.082 **	-0.042	-0.080 **	-0.080 **	-0.042
youth was aged 10-18	(0.036)	(0.035)	(0.026)	(0.035)	(0.035)	(0.026)
Percentage of years		-0.202 ***	-0.152 ***		-0.194 ***	-0.148 ***
moved, aged 10-14		(0.047)	(0.034)		(0.047)	(0.034)
Own household by 18		-0.225 ***	-0.195 ***		-0.218 ***	-0.191 ***
		(0.037)	(0.024)		(0.036)	(0.024)
Adusted R-squared	0.133	0.172	0.140	0.139	0.175	0.142
Sample size	2,178	2,178	4,410	2,178	2,178	4,410

Notes:
***=significant at 1% level **=significant at 5% level *=significant at 10% level
1) All regressions include 8 employment transition variables, 4 other marital status transitions, and region and age 15 dummies.

Appendix 1b
Logit High School Graduation Regressions
Dependent variable: 1 if high school graduate
(Huber standard errors in parentheses)

	Neighborhood measure: log(poverty rate)			Neighborhood measure: log(dropout rate)		
	Sibling sample		All youth sample	Sibling sample		All youth sample
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-1.470	-0.309	-2.581	-1.611	-1.058	-2.959 *
	(2.858)	(2.912)	(1.859)	(2.778)	(2.889)	(1.706)
Neighborhood variable	-0.543 ***	-0.586 ***	-0.348 ***	-0.805 ***	-0.726 ***	-0.473 ***
	(0.133)	(0.158)	(0.116)	(0.163)	(0.161)	(0.103)
Whether female	0.463 ***	0.659 ***	0.525 ***	0.495 ***	0.682 ***	0.530 ***
	(0.124)	(0.133)	(0.094)	(0.125)	(0.133)	(0.094)
Log(household income)	0.403 ***	0.343	0.511 ***	0.483 *	0.449	0.579 ***
	(0.157)	(0.280)	(0.176)	(0.277)	(0.289)	(0.170)
Whether nonwhite	0.765 ***	0.556 **	0.458 ***	0.572 ***	0.340 *	0.361 **
	(0.185)	(0.222)	(0.162)	(0.197)	(0.200)	(0.149)
Parents' married	-0.093	-0.153	-0.132	-0.105	-0.172	-0.137
	(0.181)	(0.239)	(0.159)	(0.244)	(0.248)	(0.160)
Dad high school grad.	0.663 **	0.630 ***	0.636 ***	0.604 ***	0.587 ***	0.604 ***
	(0.177)	(0.213)	(0.157)	(0.212)	(0.212)	(0.157)
Mom high school grad.	0.705 ***	0.690 ***	0.581 ***	0.667 ***	0.662 ***	0.561 ***
	(0.144)	(0.174)	(0.123)	(0.178)	(0.173)	(0.124)
No. kids in household	-0.221 ***	-0.195 ***	-0.145 ***	-0.215 ***	-0.190 ***	-0.143 ***
	(0.039)	(0.045)	(0.028)	(0.045)	(0.045)	(0.027)
Whether kid worked	0.437 ***	0.348 ***	0.292 ***	0.438 ***	0.358 ***	0.283 ***
	(0.133)	(0.138)	(0.098)	(0.133)	(0.137)	(0.097)
County unskilled wage	0.144 **	0.145 *	0.028	0.174 **	0.172 **	0.046
	(0.068)	(0.087)	(0.064)	(0.087)	(0.088)	(0.063)
Variance of income	0.019	0.058	0.227	0.045	0.074	0.197
	(0.274)	(0.264)	(0.248)	(0.252)	(0.265)	(0.235)
Parents divorced while	-0.562 ***	-0.593 ***	-0.320 *	-0.553 **	-0.597 ***	-0.334 **
youth was aged 10-18	(0.195)	(0.223)	(0.167)	(0.218)	(0.227)	(0.166)
Percentage of years		-1.329 ***	-0.964 ***		-1.228 ***	-0.919 ***
moved, aged 10-14		(0.282)	(0.211)		(0.287)	(0.210)
Own household by 18		-1.323 ***	-1.169 ***		-1.244 ***	-1.127 ***
		(0.202)	(0.134)		(0.197)	(0.134)
Log likelihood	-867.3	-827.8	-1,678.2	-854.9	-821.0	-1,669.3
Sample size	2,178	2,178	4,410	2,178	2,178	4,410

Notes:
***=significant at 1% level
1) All regressions include 8 employment transition variables, 4 other marital status transitions, and region and age 15 dummies.

Appendix 2

Unweighted Pairwise First Difference High School Graduation Regressions

Neighborhood Measures: log(poverty rate) or log(dropout rate)

Dependent variable: 1 if high school graduate

(Huber standard errors in parentheses)

	log(poverty rate)		log(dropout rate)	
	(1)	(2)	(3)	(4)
Intercept	-0.057 **	-0.053 **	-0.052 ***	-0.057 ***
	(0.028)	(0.027)	(0.028)	(0.027)
Neighborhood Var.	-0.144 **	-0.142 **	-0.068 *	-0.068 *
	(0.060)	(0.060)	(0.037)	(0.036)
Whether female	0.072 ***	0.087 ***	0.070 ***	0.085 ***
	(0.022)	(0.021)	(0.021)	(0.021)
Log(hh income)	0.003	-0.003	0.018	0.012
	(0.053)	(0.051)	(0.055)	(0.054)
Parents' married	0.004	0.011	0.001	0.009
	(0.063)	(0.065)	(0.063)	(0.065)
Number of kids in hh	-0.037 **	-0.029	-0.036 *	-0.029
	(0.019)	(0.018)	(0.019)	(0.019)
Whether kid ever worked during youth	0.070 ***	0.048 **	0.066 **	0.044 *
	(0.025)	(0.024)	(0.026)	(0.024)
County unskilled wage	0.020	0.016	0.022	0.019
	(0.018)	(0.018)	(0.019)	(0.018)
Own household by 18		-0.217 ***		-0.218 ***
		(0.048)		(0.049)
Adusted R-squared	0.058	0.084	0.054	0.080

Notes:

***=significant at 1% level **=significant at 5% level

*=significant at 10% level

1) All regressions also include sibling differences in parents' marital and employment status dummies, variance of household income, whether the child participated in housework, and region and year turned age 15 dummies.

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