

# Neighborhood Effects on the Production of Human Capital: Evidence from Movers in North Carolina

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## Abstract

I investigate whether neighborhood exposures during childhood affect academic outcomes observed at the end-of-high school, and whether the effects can be explained by neighborhood schools. I find that neighborhood exposures during childhood affect high-stakes standardized exam scores, 12th grade GPA, the probability of intending to attend college and the probability of dropping out of high school. By leveraging variation in the age at which students move, I estimate exposure effects that encompass the effect of neighborhood schools and other amenities. I demonstrate that these effects can not be fully explained by conventional school quality measures based on test scores or graduation rates, which points to the potential importance of peer effects and other neighborhood amenities as complementary mechanisms.

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# 1 Introduction

There is an emerging consensus that the neighborhoods people live in during childhood can substantially affect their socioeconomic outcomes during adulthood such as earnings, college attendance, marriage and incarceration (Chetty and Hendren, 2018a,b; Chetty et al., 2018; Chyn, 2018; Deutscher, 2020; Laliberté, 2021). Yet little is known about the mechanisms that generate these effects. Can these neighborhood effects be explained entirely by academic achievement and, if so, to what extent are achievement differentials explained by public schools compared to other neighborhood amenities such as public safety and pollution?

This paper investigates how the neighborhood in which a child grows up affects their educational outcomes by the time of high school completion. I adapt the research design from Chetty and Hendren (2018a,b) to derive variation in neighborhood exposures from a sample of children who move between the same neighborhoods at different ages. These data allow me to identify how neighborhood exposures affect a wide range of academic achievement measures. I then calculate how much of the variation in the causal neighborhood achievement differentials can be explained by the quality of schools that a student attends.

I investigate these questions using 10 years of administrative records for the population of K-12 public school students in North Carolina. These data have several features that aid in identifying how neighborhoods affect academic achievement. First, they allow me to follow children from elementary to high school as they move between schools, residential neighborhoods, or both, at a fine spatial scale. Second, the data include several measures of academic achievement such as performance on standardized state and national tests such as the SAT/ACT, GPA, high school completion and college intentions. The diversity of these metrics allows me to estimate effects for students throughout the achievement distribution. Finally, the data include information on schools and standardized testing that allow me to use standard methods to estimate school-specific measures of value-added that I use to complement administrative school quality measures reported by the North Carolina Department of Public Instruction.

I use these rich data to implement a version of the research design developed by Chetty and Hendren (2018a). In particular, I leverage variation in the age at which students move between neighborhoods to identify the effects of exposure. Intuitively, children who move from the same origin neighborhood to the same destination neighborhood, but at different ages, will at the end of high school have had different durations of exposure to this shared destination. If neighborhoods affect academic achievement, then the outcomes of children who move should converge toward those of children who always lived in the destination neighborhood, and this convergence should increase with exposure time. I estimate convergence rates using an origin-by-destination and age-at-move fixed effects specification.

A key feature of this research design is that it identifies neighborhood effects from comparisons between movers within origin-destination pairs, thus alleviating concerns about the endogeneity of families' location decisions. The identifying assumption is that after conditioning on origin and destination neighborhood, as well as the age at which a move takes place, the selection into neighborhoods does not vary systematically with the timing of a move. I validate this assumption by conducting several robustness checks, including a sibling-fixed effects strategy that leverages within-family variation in the exposure time of siblings.

The econometric analysis yields several insights. First, I find novel evidence that neighborhoods have a causal effect on educational outcomes. I estimate convergence rates of approximately 1 to 3 percent per year for SAT and ACT math scores, 10th grade English exams, the probability of intending to attend college, 12th grade GPA and dropouts. A convergence rate of one percent per year means that for each additional year a child lives in a neighborhood, they are expected to close the origin-destination achievement gap by one percent.<sup>1</sup> This implies that roughly one-fifth to half of the observed differences in key measures of educational achievement between neighborhoods can be attributed to the effects of living there, as opposed to residential sorting on household characteristics that affect achievement.

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<sup>1</sup>As measured by the outcomes of permanent residents, i.e. children who always are observed in the same neighborhood.

These statewide convergence rates for academic achievement in North Carolina are smaller in magnitude but qualitatively consistent with Chetty and Hendren’s (2018ab) national estimates of convergence rates for labor market outcomes in adulthood, and with the Canadian estimates of convergence rates for dropouts and college attendance in Montreal reported by Laliberté (2021).

Second, the economic significance of these estimates varies with the outcome measured. Intuitively, if cross-neighborhood differences in an academic achievement measure are small, then convergence matters less than when these differences are large. I find modest differences in average SAT scores and GPA across the neighborhoods between which students move, and thus estimate a small economic impact of moving, even though convergence rates are between 1.1 and 1.3 percent per year.

For the probability of dropping out and college intentions, however, I find larger and more economically important effects. With a convergence rate of 1.6 percent and relatively large cross-neighborhood differences in dropout rates, a fifth of movers see causal impacts on their probability of dropping out that are larger than 0.1 percentage points per year. For a move at age 4, this corresponds to a change in the probability of dropping out of 1.4 percentage points – a 20 percent change from the base dropout rate among movers of 6.9 percent. Similarly, with a convergence rate of 1.2 percent, the implied effects on college intentions are also over a tenth of a percentage point per year for a fifth of movers. This would correspond to a 2 percent change in the intentions to attend college if they had moved at age 4.<sup>2</sup>

Finally, I find that these neighborhood effects on academic achievement are not fully explained by the schools children attend. I demonstrate that less than 10% of the estimated exposure effects are explained by flexible functions of standard school quality measures such as standardized test scores, graduation rates, derived measures of value added, and other

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<sup>2</sup>Chetty and Hendren (2018a) estimate a national convergence rate of 3.7 percent for college attendance when comparing movers between commuting zones but do not report estimates for educational outcomes at a finer spatial scale.

measures calculated by the NC Department of Public Instruction. Additionally, I obtain similar (but less statistically precise) estimates by repeating the estimation and conditioning on the high school a student attends or by restricting the analysis to children who only move within school districts. These results thus indicate that there is a substantial component of neighborhood effects that is orthogonal to school quality, or that is driven by factors such as peer effects that may not be fully captured by traditional school quality measures. I also conduct a battery of robustness check and find that my main findings are robust to several alternative sample selection rules, fixed effects specifications, and neighborhood definitions that collectively exhaust the data’s statistical power.

Overall, this study makes three main contributions to the literature. First, it provides some of the first evidence on the mechanisms through which neighborhood exposures during childhood may affect adult labor market outcomes. If moving to a better neighborhood can improve academic achievement, this channel would also be expected to increase future earnings, for example. This is also the first study to estimate how neighborhood exposures affect high-stakes standardized test outcomes such as the SAT and ACT, as well as end-of-high school grades.

In a closely related study, Laliberté (2021) uses a similar research design to estimate neighborhood effects in Montreal, Canada for a subset of the educational outcomes considered in this study. Thus, a second contribution of my paper is to provide comparable evidence for an entirely different population in North Carolina. Compared to Laliberté (2021), I find somewhat smaller, but similar convergence rates for the subset of achievement measures analyzed in both studies. The similarity in our estimates is especially striking for the probability of dropping out of high school (convergence rates of 1.6 versus 1.7). Finding such consistent results for Montreal and North Carolina, which greatly differ in their local institutions, physical environments and demographics, underscores the important role of neighborhoods in the production of human capital.

Finally, this study advances the discussion of whether it is purely the schools that a

neighborhood provides access to that are responsible for the neighborhood effects found in this study and previous ones. Laliberté (2021), estimates that between 50 and 70 percent of the effect of moving to a better neighborhood comes from access to better quality schools. My results are consistent with this conclusion and add further nuance by showing that the school component of neighborhood effects is not driven by conventional measures of school quality that are based on test scores or graduation rates.

On the other hand, Baum-Snow et al. (2019) find that school *districts* explain almost all of the neighborhood effect when they flip the research design to focus on the subset of children who never move as neighborhood demographics change around them. This may appear to contrast with my findings for children who move between neighborhoods within school districts as I find evidence of neighborhood effects even for these movers. However, the two sets of results are not mutually incompatible because they apply to different subpopulations: movers versus non-movers. Additionally, Baum-Snow et al. (2019) use local labor market shocks to identify neighborhood effects; these might act through different channels.<sup>3</sup> Future research could usefully test whether neighborhoods have heterogeneous effects on movers and non-movers in the same geographic areas.

## 2 Related Literature

This paper contributes to a growing empirical literature focused on estimating causal neighborhood and school effects. An extensive overview of the findings in this literature is provided by Chyn and Katz (2021). Important studies related to this one, include Chetty and Hendren (2018a,b) and Chetty et al. (2018) who use data on the universe of U.S. tax filers and find substantial effects of neighborhoods on intergenerational mobility and a wide range of other social and educational outcomes at geographical scales ranging from commuting zones to counties and census tracts. Given the close link between school attendance and residential location these estimates reflect differences in both neighborhood and school

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<sup>3</sup>In my sample of North Carolina movers, 92 percent of students move within school districts.

quality. Crucially, however, they show that neighborhood effects for earnings ranks are approximately linear in exposure time to neighborhoods, and do not vary with age of move. I leverage this insight and build upon their approach to analyze how neighborhoods affect educational outcomes rather than earnings ranks.

An open question in this literature, is the extent to which quality differences across schools contribute to the differential impacts of neighborhoods on childrens outcomes. Most studies estimating neighborhood effects do not typically distinguish the “pure” neighborhood – or *place* effect – from school effects. Likewise, estimates of school effects might be picking up neighborhood or peer effects that are correlated with school attendance patterns as noted by Aaronson (1998). Notable attempts to distinguish school and neighborhood effects are Dobbie and Fryer (2011) and Agrawal et al. (2019). The former study the Harlem Children’s Zone and find large effects of attending the participating schools and that these effects do not vary with residential location within the zone; whereas the latter use a control function approach to show that schools account for a larger share of variance in outcomes than neighborhoods do. At a larger spatial scale, however, Rothstein (2019) finds that differences in educational systems explain only a small share of cross-commuting zone variation in intergenerational mobility. My study contributes to this discussion by showing that the school component of neighborhood effects is not driven by measures of school quality that are based on test scores or graduation rates.

The study that is perhaps most closely related to this paper, both in terms of research question and methodology, is Laliberté (2021). He uses data from Montreal to estimate total neighborhood exposure effects, and what share of these are attributable to schools. Following the approach pioneered by Chetty and Hendren (2018a), that I also adapt in this study, Laliberté (2021) first estimates exposure effects to neighborhoods (at the ZIP-code level) that encompass both school and place effects using a sample of movers. Then, he estimates causal school effects by implementing a boundary discontinuity design in the spirit

of Black (1999), using a sample of permanent residents.<sup>4</sup> With estimates of total exposure effects and school effects in hand, the school share of the total exposure effect can then be estimated under an assumption that the degree of sorting into schools, conditional on residential location, is equal for movers and permanent residents. Given this assumption, Laliberté (2021) finds that 50 to 70 percent of total exposure effects are due to access to better schools, rather than the neighborhoods themselves. Compared to Laliberté (2021), my study uses a more diverse set of achievement measures and documents evidence for the entire public school system of a US state. Additionally, I provide further nuance to the discussion of school-vs-place effects, by showing that if schools are responsible for the causal effects of neighborhoods, these effects are likely due to factors not captured by traditional school quality measures such as school value-added.

### 3 Data

I use 2007-2017 administrative education records from North Carolina throughout this study.<sup>5</sup> These records cover the full population of students in public and charter schools in the state and are maintained by the North Carolina Education Research Data Center (NCERDC). Unique features of these data are geocoded address identifiers that let me identify the census tract each student lives in every school year, and longitudinal links that allow tracking of students as they move between neighborhoods. The data also contain rich information on students, teachers and schools. Demographic information about students that can be linked to the address data include birth date, gender, race and economically disadvantaged status.<sup>6</sup>

For end-of-high school outcomes, I consider 12th grade GPA and indicators for college

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<sup>4</sup>These school effects are thus identified from school catchment areas that intersect ZIP codes, which induces variation in school inputs among permanent residents who arguably receive the same neighborhood inputs.

<sup>5</sup>The records begin in 1995, but it is not until 2007 that all districts report address data.

<sup>6</sup>Indicates participation in a federally funded, means tested, free or reduced price lunch subsidy program.



intentions, high school graduation, ever taking the SAT and dropping out.<sup>7</sup> I also consider students' performance on the SAT and ACT. The SAT outcome I use is the percentile rank in the national distribution for the math subsection. I do this to ensure comparability over time, given that the SAT and its scoring scale were redesigned in 2016. SAT scores are available in the data from 2009 onward and are available for 40% of students in my main sample. Since the SAT is not compulsory, the SAT-takers are a selected sample and estimates should be interpreted with this in mind. The data on ACT scores, however, does not suffer from this selection issue since all 11th graders in North Carolina were required to take the ACT in 2013, 2014 and 2015. To be comparable with the SAT data, I therefore also use the students' ACT math scores. A caveat to the ACT data, however, is the smaller sample size due to the few numbers of years it was mandated – only 25% of students in the main sample are observed taking the ACT.

### *3.1 Sample Description*

The main sample includes students who are observed in 2007 or later and for whom birth date, address and school-grade data is complete in every year until they graduate or drop out of school. Students who ever attend private (but not charter) schools or move out of state are thus excluded. I use census tracts to operationalize the notion of a neighborhood, and classify students who are always observed in the same neighborhood as permanent residents. My sample selection criteria are further outlined in the appendix. The main sample contains 568,212 permanent residents and 232,667 students who move exactly once during the sample period 2007-2017. A feature of the data is that addresses are only recorded at the beginning of an academic year so the exact timing of a move is unknown. This introduces measurement error when I calculate childhood exposure times to neighborhood. To avoid compounding this measurement error, I exclude students who move more than once, and explore the

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<sup>7</sup>A student's stated intention of whether to attend college after high school or not is recorded through a survey of all graduates. While I do not observe whether students follow through on their intentions, stated intentions could be more responsive to peer effects and the environment in which one grows up in, and are still informative of student aspirations. Dropouts can be observed directly in the data and are recorded separately from graduation. This is important since it avoids conflating dropouts with students who graduate early, transfer to a private school, move out of state or die.

Table 1: Summary Statistics by Mobility Status

	Main Sample (1)	Permanent Residents (2)	One-time Movers (3)	Movers (4)	All (5)
A. Student Characteristics					
Female	50%	49%	50%	51%	50%
White	58%	61%	51%	47%	55%
Economically Disadvantaged	36%	32%	45%	52%	41%
Disability	18%	17%	21%	23%	19%
Limited English Proficiency	4%	3%	4%	3%	3%
B. Mobility					
Mean age at move, conditional on moving	14.74	-	14.74	15.49	15.49
SD age at move, cond.	2.70	-	2.70	2.55	2.55
Mean number of moves, cond.	1.00	-	1.00	1.76	1.76
SD number of moves, cond.	0.00	-	0.00	1.04	1.04
Mean number of moves, unconditional	0.29	0.00	1.00	1.76	0.76
C. Educational Outcomes					
Mean ACT Math Score (36pt scale)	19.87	20.12	19.24	18.69	19.50
Percent with Outcome	25%	25%	25%	25%	25%
Mean SAT Math percentile	49.79	51.00	46.30	43.42	48.22
Percent with Outcome	40%	42%	35%	32%	38%
Mean College Intentions	0.84	0.85	0.81	0.79	0.82
Percent with Outcome	93%	93%	92%	90%	92%
Mean Dropouts	0.05	0.05	0.07	0.08	0.06
Percent with Outcome	98%	98%	98%	98%	98%
Mean 12th grade GPA (4pt scale)	2.86	2.90	2.75	2.67	2.80
Percent with Outcome	79%	78%	79%	77%	78%
Mean 10th grade English test	0.12	0.16	0.05	-0.02	0.08
Percent with Outcome	32%	31%	33%	34%	32%
Observations ( $N \times T$ )	5,351,046	3,757,926	1,593,120	3,079,067	6,836,993
Individuals ( $N$ )	800,879	568,212	232,667	432,968	1,001,180
Mean observations per student	6.68	6.61	6.85	7.11	6.83

This table reports summary statistics for student characteristics, educational outcomes and moves between census tracts by mobility status. The main sample (1) consists of permanent residents (2), who are never observed moving, and one-time movers (3), students who move exactly once between the ages of 5 and 20. Column 4 reports the same information for all movers (one-time and multiple movers) and (5) pools all movers (4) with permanent residents (2). All columns exclude students who live in census tracts where fewer than 50 permanent residents are observed and students who move out of state or transfer to a private school.

robustness of my results to this choice in the appendix.

Summary statistics by mobility status are presented in Table 1. It appears that students who move differ substantially from permanent residents. Compared to permanent residents,

movers tend to be less white and more economically disadvantaged. They also on average have worse educational outcomes which highlights the importance of an empirical strategy that does not rely on comparisons across the two groups. There are substantial differences in outcomes across census tracts in North Carolina, even if attention is restricted to the pairs of tracts I observe students moving between. Figure A1 plots the distribution of realized moves for one-time movers in the main sample as measured by the difference in the outcomes of permanent residents in the origin and destination neighborhoods. For all outcome measures, the median mover moves between neighborhoods of similar quality, and the distributions are roughly symmetric.<sup>8</sup> This indicates moves to “better” neighborhoods are about as equally likely as moves to “worse” neighborhoods. There is, however, substantial heterogeneity – e.g., 40% of movers move between neighborhoods where the difference between origin and destination college attendance intentions is at least nine percentage points (for the permanent residents) and differences in GPA are larger than 20% of a standard deviation. These data are also presented in tabular form in Table A3.

## 4 Empirical Framework

If longer exposure to better neighborhoods leads to better outcomes, then moving to a better neighborhood at an earlier age will be more beneficial than moving at a later age.<sup>9</sup> Therefore, I focus on estimating the rate at which the outcomes of movers converge to the outcomes of those that always live in the same neighborhood (the “permanent residents”). To ease interpretation, and motivate this convergence rate as the object of empirical interest, I first outline a simple model of human capital production before presenting the estimation strategy.

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<sup>8</sup>Quality here is defined in terms of the outcomes I measure; there are of course many other neighborhood amenities that families value.

<sup>9</sup>See Chyn and Katz (2021) for an overview of the evidence for this exposure hypothesis.

#### 4.1 Human Capital Production

Suppose educational investment takes place over  $T$  years, after which point an outcome  $y_i$  is realized.<sup>10</sup> For simplicity, assume investment is additively separable in family and neighborhood inputs, where the latter includes the schools that living in a certain neighborhood provides access to. Also assume that each input has a constant, cumulative effect. For a student from a given family,  $y_i$  then depends on how long he or she lives in each neighborhood,  $n$ , and the quality of the amenities in those neighborhoods:

$$y_i = \sum_n t_{in} \Phi_n + \theta_i, \quad (1)$$

where  $t_{in}$  is the number of years student  $i$  lives in neighborhood  $n$  and the cumulative effect of family inputs over  $T$  years is denoted  $\theta_i$ . The causal impact of a year's exposure to each neighborhood is denoted  $\Phi_n$ .

Now consider the set of students who from birth to year  $T$  live in exactly one neighborhood. I denote them permanent residents (PRs) and the average of their outcomes in each neighborhood  $n$  is then given by  $\bar{y}_n^{PR} = T\Phi_n + \bar{\theta}_n$ . Observed differences in average outcomes among permanent residents across two neighborhoods,  $\Delta\bar{y}_{od} = \bar{y}_d^{PR} - \bar{y}_o^{PR}$ , can thus reflect both differences in causal neighborhood quality ( $\Phi_n$ ) and residential sorting which causes differences in the types of families who choose to live in certain neighborhoods ( $\bar{\theta}_n$ ).

Students switch neighborhoods exactly once before year  $T$ , i.e., one-time movers, are exposed to their destination neighborhood,  $d$ , for  $t_{id}$  years and to their origin,  $o$ , for  $t_{io} = T - t_{id}$  years. Since movers share the same underlying production function as permanent residents, the outcome of a mover from  $o$  to  $d$  can then be written as a function of the outcomes of the permanent residents in the two neighborhoods:

$$y_{iod} = (\bar{y}_o^{PR} - \bar{\theta}_o) + \gamma_{od}(t_{id} \times \Delta\bar{y}_{od}) + \theta_i, \quad (2)$$

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<sup>10</sup>Empirically, some of the outcomes are realized before time  $T$ , e.g. there is some variation across individuals in when they take the SAT. I account for this when calculating exposure times.

where  $\gamma_{od} = \Delta\Phi_{od}/\Delta\bar{y}_{od}$  is the annual rate at which the outcomes of movers converge towards the outcomes of the permanent residents in the destination they move to. These convergence rates tell us what fraction of the observed gap in outcomes between residents in  $o$  and  $d$  that we should expect student  $i$  to close if he or she spends an additional year in neighborhood  $d$ . In other words, convergence rates provide a measure of the relative importance of sorting and causal effects in explaining disparities across neighborhoods. If neighborhoods do not have causal effects and differences are purely driven by sorting, the convergence rates of movers should be zero. Conversely, without sorting, average familial inputs should be equal across neighborhoods and the rate of convergence is then  $1/T$ . The objective of this paper is thus to obtain estimates of the average convergence rates for movers across a wide range of educational outcomes.

#### 4.2 Estimation

Equation 2 describes the outcomes of movers in terms of variables that are observed in the data and suggests an identification strategy based on variation in the timing of moves. Students who move from the same origin neighborhood to the same destination at different ages receive different amounts of exposure to the destination – variation that can be used to identify convergence rates. The empirical counterpart of Equation 2, and my main estimating equation is thus:

$$y_{iodca} = \beta_0 + \beta_1(t_{id} \times \Delta\bar{y}_{od}) + X_i\psi + \mu_{od} + \mu_a + \mu_c + \varepsilon_{icod}, \quad (3)$$

where  $y_{iodca}$  is an academic outcome of a student  $i$  in cohort  $c$  who moves at age  $a$  from origin  $o$  and is exposed to the destination  $d$  for  $t_{id}$  years. The difference in observed average outcomes of permanent residents in the two neighborhoods is denoted  $\Delta\bar{y}_{od}$  and  $X_i$  denotes a vector of individual characteristics.<sup>11</sup> I cluster standard errors at the origin-destination pairs to account for arbitrary correlation in the error terms. To account for sorting into

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<sup>11</sup>These include gender, ethnicity, economically disadvantaged status, disability status and limited English proficiency indicators.

neighborhoods origin-by-destination fixed effects ( $\mu_{od}$ ) are included. I also run specifications with origin and destination fixed effects included separately. Additionally, age-at-move fixed effects ( $\mu_a$ ), are included to capture the notion that the act of moving itself might induce disruption costs (or benefits) that vary by age. Finally, cohort fixed effects,  $\mu_c$ , are included to account for the differing number of years students are observed in the data, as well as variation in the measurement of some variables across cohorts. Given this fixed-effects strategy, it is important that there is enough variation in the regressor of interest, that is the age at move within origin-destination pairs and the origin-destination differentials among PRs across origin-destination pairs. Reassuringly, Table A4 in the appendix shows that there is still substantial variation in the treatment variable after being residualized with respect to the fixed effects.

Equation 3 is identified under the assumption that selection into neighborhoods does not vary with the age at move. This follows Chetty and Hendren (2018a), and requires that *when* a student moves is orthogonal to other determinants of their outcomes, but crucially allows for *where* they move to not be. Under this assumption, families who move to better areas are allowed to differ from those who move to worse areas, as long as they don't systematically differ in the age of their children when they move. This implies that conditional on origin and destination neighborhoods,  $t_{id}$  can be treated as exogenous. I explore robustness to this assumption in Section 5.

### 4.3 Interpretation

The coefficient of interest is  $\beta_1$ , the annual rate at which movers converge to the outcomes of permanent residents in their destination.<sup>12</sup> Although  $\beta_1$  tells us about the causal effects of neighborhoods on *average*, it should be noted that it is not informative of the effect of an additional year of exposure to any *specific* neighborhood (see Chetty and Hendren, 2018a). As discussed, the convergence rate for movers between a specific origin and destination will depend on the relative magnitudes of causal effects and sorting in the two neighborhoods,

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<sup>12</sup>This is the average effect of moving to a one unit (in terms of observed outcomes among permanent residents) better neighborhood one year earlier.

and it is reasonable to expect this to vary across neighborhoods.

However, it is worth highlighting that the validity of estimating (3) to determine how much a student’s potential outcomes on average would improve from moving to a better neighborhood at an earlier age does not depend on assumptions about the specific functional form of the underlying human capital production function, since  $\beta_1$  still captures the reduced form effect of such an intervention. In particular, (1) abstracts from effects of schools and places that are not proportional to exposure time such as the effect of living near a college (see Card, 2001 for examples), but in practice such effects are absorbed by the origin and destination fixed effects. Additionally, the production function specified in (1) imposes linearity and additive separability and thus rules out complementarities between inputs, as well as adjustment responses to changes in inputs. If, for instance, parental inputs adjust in response to changing neighborhood inputs after a move, estimated convergence rates should be interpreted as a policy effect (see Todd and Wolpin, 2003) that includes these parental responses.<sup>13</sup>

#### 4.4 *Measurement and Variable Definitions*

I define origins as the census tract in which a student was observed prior to moving, and destinations as the tract in which he or she resides when the outcome variable is measured. Exposure time,  $t_{id}$ , is calculated in months using student birth dates and defined as the time spent in the destination neighborhood between moving and the measurement of the outcome variable. For all test score outcomes, I use the actual test date to calculate exposure times whereas for the other outcomes I assume these are measured at age 20.<sup>14</sup> All estimates presented in text and tables throughout this paper will be in terms of annual convergence rates. For each origin-destination pair and each outcome variable, I calculate the difference in average outcomes among permanent residents,  $\Delta\bar{y}_{od} = \bar{y}_d^{PR} - \bar{y}_o^{PR}$ . Since no movers are used in the construction of differences, and since  $\Delta\bar{y}_{od}$  equals zero for all permanent residents,

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<sup>13</sup>E.g. Pop-Eleches and Urquiola (2013) find that parents reduce their inputs when children attend a better school.

<sup>14</sup>Technically the outcome variables should thus be defined as e.g. “the probability of having graduated high school by age 20”.

the reflection problem (Manski, 1993) does not affect the estimates presented below. I follow Finkelstein et al. (2021) who implement a movers design to estimate the effects of places on mortality, by including both movers and permanent residents in my main sample. Though movers are crucial for identifying exposure effects, including permanent residents in the estimation improves precision as long as movers and permanent residents share an underlying human capital production function.<sup>15</sup> This assumption is innocuous since this shared function does not have to be of the form indicated in (1). As a robustness check, in the appendix I replicate the analysis while excluding permanent residents and obtain qualitatively similar results.

## 5 Results

I begin by presenting estimates of exposure effects in the main sample. Finding a non-zero convergence rate for these movers implies that moves between neighborhoods have causal impacts on students educational outcomes. The estimates of  $\beta_1$  from Equation 3 – i.e. the average annual rate at which movers converge toward the outcomes of permanent residents in their destination neighborhoods – are reported for each outcome in Table 2. The magnitude of  $\beta_1$  should be interpreted as the causal effect of spending one additional year in a destination neighborhood where the outcomes of permanent resident are one unit higher than in the origin.

The three columns in this table highlight the importance of controlling for the origin and destination of movers. Column 1 presents estimates where these controls are omitted and shows convergence rates close to zero (and sometimes even negative) for all outcomes. This is indicative of sorting into neighborhoods based on unobserved family inputs (or ability). If students who are likely to have the worst outcomes due to family inputs also tend to live in the worst neighborhoods, then when these students move they will on average, almost mechanically, move to a better neighborhood. If the opposite also holds for those who are

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<sup>15</sup>By definition  $\Delta \bar{y}_{od} = 0$  for permanent residents and there is no variation amongst them in exposure times.



Table 2: Neighborhood Exposure Effects

Outcome	Convergence Rate ( $\beta_1$ )		
	(1)	(2)	(3)
10th grade English	0.008* (0.004)	0.011* (0.007)	0.028** (0.012)
SAT Math	0.008*** (0.003)	0.013*** (0.004)	0.006 (0.007)
ACT Math	0.004 (0.004)	0.012** (0.005)	0.010 (0.011)
GPA	-0.001 (0.001)	0.011*** (0.002)	0.015*** (0.003)
Dropouts	-0.004*** (0.001)	0.016*** (0.005)	0.016** (0.007)
College Intentions	-0.005*** (0.001)	0.012*** (0.004)	0.013** (0.006)
Fixed Effects			
Cohort & Age at move	X	X	X
Origin & Destination		X	
Origin-by-Destination			X

\*p &lt; 0.1; \*\*p &lt; 0.05; \*\*\*p &lt; 0.001

This table reports estimates of annual convergence rates ( $\beta_1$ ) for the main sample of students who move between census tracts exactly once and students who never move. Standard errors are clustered at the origin-destination level and reported in parenthesis. These estimates can be interpreted as the impact of spending an additional year in a tract where permanent residents have one unit higher outcomes. Estimates in each column include cohort and age-at-move fixed effects as well as controls for student characteristics. The specification in column 1 does not control for origin or destination. In (2) and (3), this is controlled for using origin and destination, and origin-by-destination fixed effects respectively.

expected to have the best outcomes so that family inputs are negatively correlated with origin neighborhood, estimates of the convergence rates will be biased towards zero if this sorting is not controlled for. To see this, it is helpful to think of parental education as the one family input and fix a destination,  $d$ .<sup>16</sup> By omitting controls for origin, we are effectively comparing the children of highly educated parents who have grown up in a good neighborhood before moving to  $d$  with the children of parents with less education who have

<sup>16</sup>An analogous argument applies if we fix an origin and do not control for destinations.

lived in a worse neighborhood prior to moving. One could then, erroneously, conclude that moving to a worse neighborhood causes better outcomes and vice versa, and that the average convergence rate is zero.

Once origin and destination are controlled for, however, it seems to matter less exactly how this is done, as columns 2 and 3 of Table 2 report qualitatively and quantitatively similar convergence rates while controlling for origin and destination fixed effects, and origin-by-destination fixed effects respectively. For all outcomes, estimated convergence rates range between 1.0 and 2.8 percent per year, implying that approximately one-fifth to half of the variation in observed outcomes across census tracts is due to the causal effects of neighborhoods and their schools.<sup>17</sup> Equivalently, movers can expect to close the gap between permanent residents in their origin and destination neighborhoods by 1.0 to 2.8 percent each year. For a move at age 4 a student would thus pick up 14 to 40 percent of the observed difference in the outcomes of permanent residents between their origin and destination over 14 years of exposure.

### *5.1 Comparison to Other Estimates of Exposure Effects*

These estimates of exposure effects around 1.5 percent per year are consistent with those obtained in other studies, although slightly smaller. Chetty and Hendren (2018a) estimate an annual rate of convergence of 4.4 and 3.7 percent for earnings and college attendance respectively when looking at moves between commuting zones. When repeating the analysis using census tracts as the geographic unit, Chetty et al. (2018) obtain a convergence rate of 2.7 percent for earnings.<sup>18</sup> The fact that this is higher than the convergence rates for educational outcomes that I estimate is consistent with these outcomes being (potentially one of many) channels through which neighborhoods affect earnings mobility. Additionally, it should be noted that the convergence rates in Chetty et al. (2018) refer to the average causal effect of tracts across the US, whereas my estimates are specific to North Carolina.

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<sup>17</sup>Assuming outcomes are realized at age 18 and a convergence rate of 1.5%, causal effects can explain  $1.5\% \times 18 = 27\%$  of the observed differences across neighborhoods.

<sup>18</sup>Exposure effects for college attendance are not reported at the tract level.

For a subset of the educational outcomes considered in this study, Laliberté (2021) provides estimates of convergence rates in Montreal between 4 and 5 percent when using zip codes as the unit of analysis. When using tracts and a sample of one-time movers, the comparable estimates are instead 2.8 for college attendance and 1.7 for high school completion. The former is larger than my estimate (1.3) but the latter is strikingly similar to my estimates for dropouts (1.6). The differences in the estimate for college attendance could reflect sampling error or the differences between stated intentions and actual college attendance, but could potentially also be explained by differences in the causal effects of schools and neighborhoods between Montreal and North Carolina. The relatively low convergence rate in North Carolina could reflect differences in the degree of sorting of permanent residents into neighborhoods across the two locations. For example, even if causal place effects are the same, if there is more sorting based on family inputs that affect college attendance in North Carolina than in Montreal, then we should expect lower convergence rates in North Carolina. Finally, the convergence rates I estimate could also be attenuated by measurement error in the within-year timing of moves since NCERDC data do not provide the precise move date. In Chetty and Hendren (2018a), Chetty et al. (2018) and Laliberté (2021), the exact date of a move is known. For the remaining outcomes (standardized test scores, college admissions exams and GPA), however, this paper is to my knowledge the first to estimate these types of convergence rates.

## 5.2 *Distribution of Treatment Effects*

The distribution of realized moves (Figure A1), together with the estimated convergence rates in Table 2, induces a distribution of implied treatment effects. The magnitude of the effect of moving thus depends both on the size of convergence rates, and the size of actual observed differences across neighborhoods. Though the median student moves between neighborhoods with similar outcomes and should thus see little effect, there is a substantial portion of students who obtain gains (losses) from moving. The 1st and 9th deciles of these

Table 3: Implied Annual Treatment Effects

Outcome	Mean dep. var. (1)	$\beta_1$ (2)	$\beta_1 \times \Delta \bar{y}_{od}$	
			1st Decile (3)	9th Decile (4)
10th grade English <sup>1</sup>	0.050	0.011*	-0.003	0.003
SAT Math	0.463	0.013***	-0.001	0.002
ACT Math <sup>1</sup>	-0.055	0.012**	-0.005	0.005
GPA <sup>1</sup>	-0.076	0.011***	-0.004	0.005
Dropouts	0.069	0.016***	-0.001	0.001
College Intentions	0.813	0.012***	-0.001	0.001

<sup>1</sup> Z-score standardized over full sample.

\*p&lt; 0.1; \*\*p&lt; 0.05; \*\*\*p&lt; 0.001

This table presents the annual treatment effects implied by the estimated total exposure effects and the distribution of realized moves. Column 1 reports the mean of each outcome among one-time movers and (2) reports estimates of the annual convergence rate for the total exposure effect from the second column of Table 2. Columns 3 and 4 report the implied annual treatment effect for movers at the 1st and 9th deciles of the realized move distributio, respectively.

implied distributions of annual treatment effects are presented for all outcomes in Table 3.<sup>19</sup> For the outcomes measured as Z-scores (test scores and GPA), the treatment effects should be interpreted as fractions of a standard deviation whereas for the remaining outcomes the effect can be interpreted as a percentage point change.

Consider, for example, the treatment effect for dropouts. The estimate of -0.1% in column 3 implies that 10 percent of movers experience an annual decrease in the probability of dropping out by 0.1 percentage points after moving. For a move at birth, this would corresponds to a decrease of 1.8 percentage points or a 26 percent change from the base dropout rate among movers of 6.9 percent. Similarly, in column 4, 10 percent of students would experience an increase in standardized 10th grade English test scores of 0.3% of a standard deviation each year after moving. For comparison, the literature on teacher-value added finds that a one standard deviation increase in teacher ability increases English test scores by 10% of a standard deviation (Chetty et al., 2014). Moving in first grade (10 years of exposure) would for these students thus be comparable to the effect of increasing teacher

<sup>19</sup>Implied treatment effects at other points in the distribution can easily be obtained by multiplying the estimated convergence rates with the realized moves presented in appendix Table A3.

ability by a third of standard deviation. Across outcomes, I find that experienced treatment effects generally are modest in magnitude, but that this is primarily driven by the fact that families tend to move between neighborhoods of similar quality, rather than by exposure effects being small.

### 5.3 *School Quality as a Mechanism*

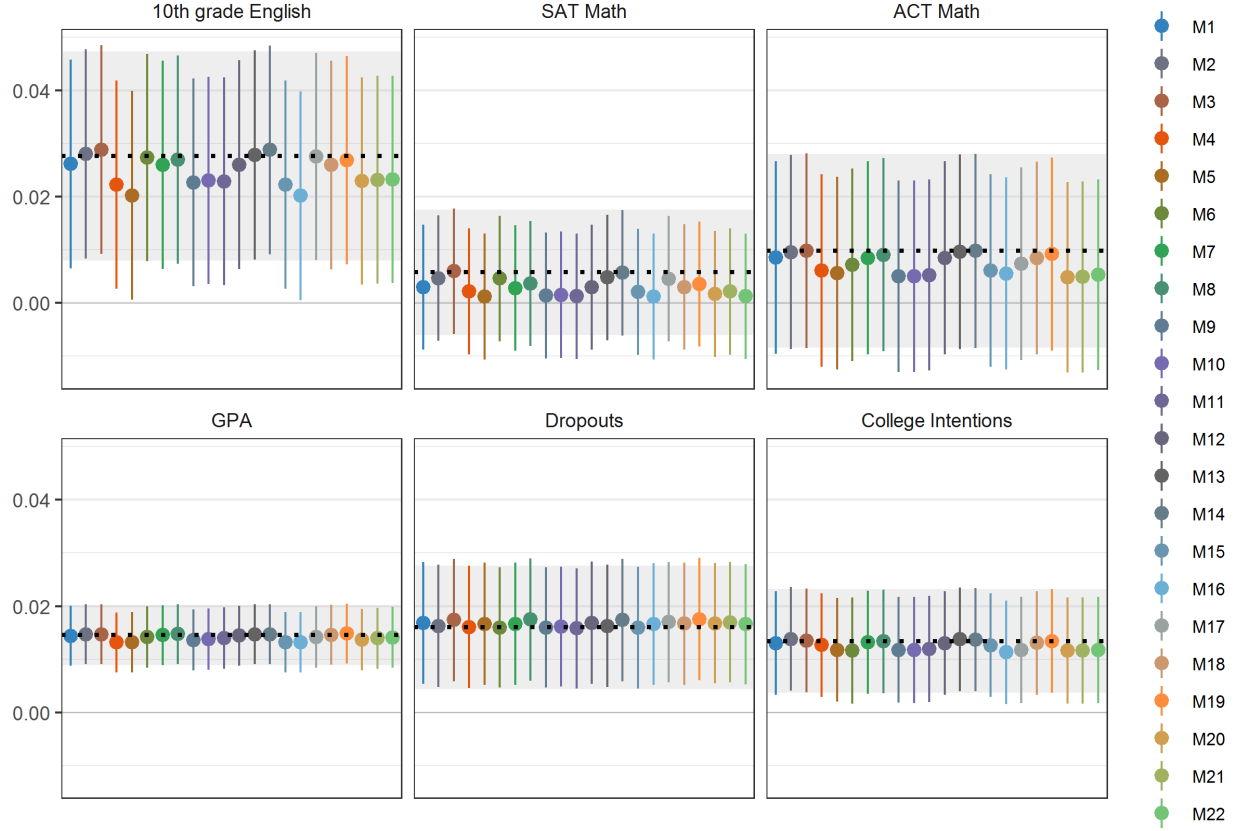
I next proceed to explore whether the exposure effects I find can be explained by observable differences in the quality of the schools that students attend. To see if this is the case, I use a range of school quality measures from the School Report Cards produced by the North Carolina Department of Public Instruction. The measures I use are growth scores, achievement scores, percent of students that are considered grade-level proficient and, for high schools, the percent of students that graduate on time. Growth and Achievement scores are calculated from standardized test scores for each school in the data.<sup>20</sup> The former provide a measure of students' progress based on testing and can be considered a type of school value-added model specified in gains whereas the latter are instead based on absolute performance on these same tests and seek to measure proficiency. The percent of students who are grade-level proficient is thus calculated directly from a school's achievement score. I supplement these school quality measures by estimating school-value added on standardized test scores for each school in the data. My value-added estimates are thus based on the same underlying data as the growth and achievement scores. I follow a large literature on school and teacher effectiveness and estimate a standard value-added model where students' test scores are regressed on lagged test scores, student characteristics and school fixed effects (Koedel et al., 2015; Chetty et al., 2014). Further details about the value-added model and the other school quality measures are provided in the appendix (Table A5).

To see if exposure effects can be explained by these school quality measures, I estimate modified versions of my main estimating equation (3) where I include flexible functions of the measures as additional controls. If the estimated effects are sensitive to the inclusion

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<sup>20</sup>Students in NC take statewide compulsory reading and math tests annually from grades 3 to 8, and a math and English test in grades 9 and 10 respectively.

Figure 1: Controlling for School Quality



This figure explores the extent to which school quality measures can explain the magnitude of estimated neighborhood exposure effects. For each outcome measure, this figure shows estimated convergence rates with 90% confidence intervals for the main sample of students who move between census tracts exactly once and students who never move, as various flexible functions of school quality measures are included as additional controls. Models M1 to M22 represent different combinations of such controls as follows: In M1-M6, Reading VA, Math VA, growth scores, achievement scores, percent grade-level proficient (GLP) and on-time graduation rates (OTG) are included individually. M7 includes both Reading and Math VA and M8 adds the growth score as well. M9 includes all four measures provided by the NC DPI, M10 includes achievement scores, GLP and OTG. M11 includes all school quality measures. Models M11-M22 repeat M1-M11 but include quadratic terms of each measure as well. These models are further described in the appendix. The main estimates of exposure effects without controls for school quality (dashed black lines) along with 90% confidence intervals (shaded gray regions) are also presented for reference. All specifications use origin-by-destination fixed effects and standard errors clustered at the origin-destination level.

of such controls, this can shed light on to what extent exposure effects are driven by the schools that living in a certain neighborhood gives access to. For each educational outcome measure, Figure 1 shows the estimated convergence rates for 22 different combinations of flexible functions of these additional school-quality controls. Details of these models are fully described in the appendix. For reference, the main results, in which school quality controls

are not included, are indicated by a black dashed line for each outcome.<sup>21</sup>

Figure 1 shows that the estimated convergence rates are remarkably stable after conditioning on observable aspects of school quality. Though not statistically distinguishable from the main estimates, the largest difference in point estimates are on average found in the models that include the percent of students that are grade level proficient. At most, point estimates decrease by 10% when these controls are included. In an analysis presented in the appendix, I obtain similar but statistically imprecise results when I repeat the main estimation while conditioning on the high school a student attends or by restricting the analysis to children who only move within school districts.

Districts could potentially be important determinants of neighborhood effects; Baum-Snow et al. (2019), for example, find that school districts explain almost all of the neighborhood effect when using a research design related to the one in this paper. Instead of focusing on movers, Baum-Snow et al. (2019) flip the design and follow children who never move as neighborhood demographics change around them. To investigate the role of school districts, I replicate my analysis with the addition of school district fixed effects, and on a sample of students who only ever attend schools and live (but move between census tracts) within one school district. The results of these specifications are presented in Table A7 and show that even after controlling for school district the magnitudes of the estimates do not change much.<sup>22</sup> Though contrasting, these results are not mutually incompatible with the findings of Baum-Snow et al. (2019) since both the source of identifying variation (timing of moves versus labor market shocks) and the focal sub-populations (movers versus non-movers) studied differ across the two settings. Taken together, these results indicate that there is a substantial component of neighborhood effects that is orthogonal to school quality, or that is driven by factors such as peer effects that may not be fully captured by traditional school

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<sup>21</sup>Estimates from column 3 of Table 2 are shown, together with shaded gray regions corresponding to a 90% confidence interval around these main estimates.

<sup>22</sup>This similarity is, however, expected given that 92 percent of movers in the main sample stay within the same school district when they move. The exposure effects estimated in the main sample are thus also mainly identified from within school district variation in neighborhood exposure.

quality measures.

#### 5.4 Robustness

I explore the robustness of my findings to the assumption that selection into neighborhoods does not vary with the age at move by adopting an alternative specification that replaces origin-by-destination fixed effects with family fixed effects. Exposure effects are then identified from comparisons between siblings who are of different ages when they move which effectively controls for any fixed (time-invariant) family characteristics such as parental education.<sup>23</sup> A challenge when implementing this analysis is that siblings are not identified directly in the NCERDC data, so siblings have to be inferred from shared addresses.<sup>24</sup> Additionally, a feature of the data is that students who live at the same address, but in different units, are all assigned the same address identifier. Therefore, sibling relationships can not be reliably identified for children who live in apartments or other forms of multi-family housing. This means that families can only be identified for a subset of children in the main sample and that, furthermore, this subset is a selected sample since it consists only of students living in single-family housing. Therefore, the main estimating sample shrinks from 800,879 students (232,667 one-time movers) to 139,101 students with at least one sibling (27,758 one-time movers) when only matched families are included. Statistical power, as well as sample selection issues, thus suggest that there are limits to what we can expect to learn about the main results from adding family fixed effects.

Figure 2 presents the results from this analysis. For each outcome, the main estimates from Table 2 are first presented (circles) for reference. The aim of the next two specifications is to separate the sample selection effects from inferring siblings through matched addresses from the effect of adding family fixed effects. I therefore replicate the main analysis with origin-by-destination fixed effects in the matched sibling (sub)sample (triangles). Here, point

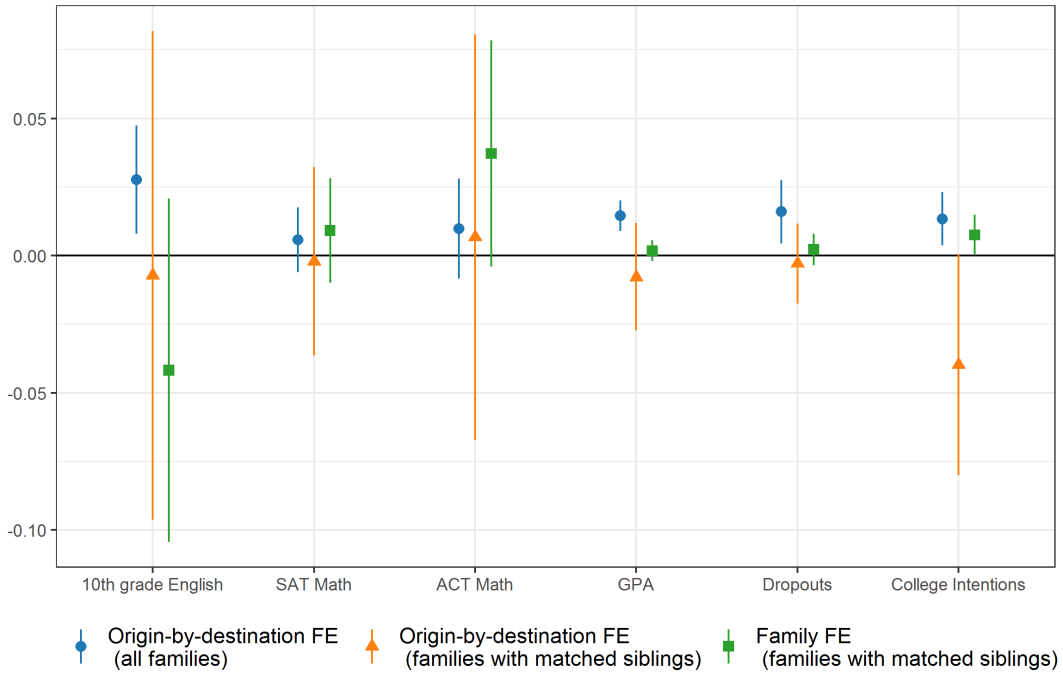
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<sup>23</sup>An important caveat is that this does not account for time-varying family inputs such as income shocks that affect both student outcomes and the decision to move. Chetty and Hendren (2018a), however, have data that allows them to control for such shocks and find that in their setting, exposure effects are not driven by such time-varying family inputs.

<sup>24</sup>To infer sibling relationships, I implement a version of the matching algorithm that Gazze et al. (2021) use to identify siblings in the same NCERDC data. See the appendix for details.



Figure 2: Exposure Effects in the Sibling-Subsample



This figure explores the robustness of the results to using within-family variation to identify exposure effects. For each outcome measure, the circles show estimated convergence rates with 90% confidence intervals for the main sample of all permanent residents and one-time movers with origin-by-destination fixed effects. The next set of estimates, presented with squares, repeat this analysis for the subsample of siblings that can be matched in the data. Due to the structure of the data, these families all live in single-family housing. The final set of estimates, presented with triangles, maintain the sample of matched siblings but replace the origin-by-destination fixed effects with family fixed effects.

estimates decrease and confidence intervals widen substantially which indicates that my main findings are not driven by families with matched siblings (who live in single-family housing). Next, I maintain the matched sibling sample but replace the origin-by-destination fixed effects with a family fixed effect (squares). Compared to the previous specification, point estimates increase and confidence intervals tighten. This provides suggestive evidence that, if anything, my main results might be understated by using origin-by-destination fixed effects instead of family fixed effects.

In the appendix, I further explore the sensitivity of my main estimates of exposure effects to the sample choice and fixed effects strategy, as well as the choice of geographic scale. I find that results are robust to using alternate sample restrictions, ranging from all movers and permanent residents, movers only, and one-time movers only (Tables A10 and A11). A

priori, it is not clear whether neighborhoods should have bigger or smaller effects at finer or coarser spatial scales. However, when replicating the main analysis using 5-digit ZIP codes as an alternative neighborhood specification, I find qualitatively similar results (Table A12) as when using census tracts, though power (larger neighborhoods means fewer movers) and sample selection issues (those moving larger distances might be different) limit the scope of the inference that can be drawn.

## 6 Conclusion

This is the first study documenting neighborhood exposure effects on high-stakes standardized test outcomes such as the SAT and ACT, as well as end-of-high school grades. By comparing students who move at different ages between the same neighborhood origin-destination pairs, I find that moving one year earlier to a neighborhood where the permanent residents have one unit higher outcomes, increases academic achievement by 1.0 to 2.8 percent, depending on the specific outcome measure considered. This suggests that a substantial share (18-50%) of differences across neighborhoods is driven by the causal effects of neighborhoods themselves, as opposed to residential sorting while students are between the ages of 5 and 20. My estimates of convergence rates are broadly consistent with the prior literature, and highlight the importance of neighborhood effects on academic achievement in high school as a potentially important mechanism through which previously estimated neighborhood effects on adult outcomes such as earnings might operate. Furthermore, I contribute novel evidence to the discussion of whether it is the schools that a neighborhood provides access to that are responsible for the neighborhood effects found in this study and previous ones. My results add nuance to this discussion by showing that the school component of neighborhood effects is not driven by measures of school quality that are based on test scores or graduation rates. If schools are largely responsible for the causal effects of neighborhoods, these effects are likely due to factors that may not be fully captured in traditional test-score based school quality measures.

Though the treatment effects actually experienced by movers in general are relatively modest across outcomes, this is primarily driven by the fact that families tend to move between neighborhoods of similar quality, rather than by exposure effects being small. This means that there potentially still is scope for targeted, place-based policies that encourage moves with large origin-destination differentials in neighborhood quality. Furthermore, this suggests that so-called “opportunity bargains” (Chetty et al., 2018) could exist also for educational outcomes. The degree to which these effects are already capitalized into housing prices, however, remains to be explored.

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## A Appendix

### A.1 Main Sample

I classify students who are always observed in the same neighborhood as permanent residents, and students whose address *and* census tract change between two observation years as movers.<sup>25</sup> Using this procedure, I observe between 9 and 19 percent of students moving each year. This is consistent with migration rates reported in the ACS where estimates range from 10 to 13 percent per year for 5- to 17-year olds in North Carolina over this time period. Table A1 provides a year-by-year comparison of moving rates in this sample and the ACS.

Table A1: Moving Rates by Year		
Year	NCERDC Data <sup>1</sup>	ACS <sup>2</sup>
	(1)	(2)
2007	0.123	0.127
2008	0.090	0.117
2009	0.130	0.112
2010	0.196	0.112
2011	0.107	0.119
2012	0.127	0.109
2013	0.113	0.116
2014	0.106	0.111
2015	0.114	0.111
2016	0.119	0.111
2017	0.109	0.103

<sup>1</sup> Student recorded as moving if Address ID and census tract change in the same year.

<sup>2</sup> Within state moving rates for 5-17 year olds in North Carolina, (U.S. Census Bureau, American Community Survey 1-Year Estimates).

Another feature of the data, is that addresses are only recorded at the beginning of an academic year so the exact timing of a move is unknown. I therefore assume all moves occur on July 1st<sup>26</sup> every year when calculating exposure times to neighborhoods. This means that a key variable and main source of variation, will be measured with error. To avoid

<sup>25</sup>This is done to deal with changes in census boundaries over time, as well as the same addresses sometimes being assigned new address IDs.

<sup>26</sup>As well as July 1st being the midpoint of a calendar year, it is likely that most moves occur during the summers between school years. Marlay and Mateyka (2011) use the 2008 Survey of Income and Program Participation to report that summer is the most common time to move with around 30% of moves occurring between June and August.

compounding this measurement error, I restrict the main analysis sample to contain only permanent residents and movers who move exactly once. Students who are not permanent residents but move several times are excluded. Robustness checks to sample choice for the main estimates are presented in Tables A10 and A11. The main sample includes all students who were observed in 2007 or later and for whom birth date, address and school-grade data is complete in every year until they graduate or drop out of school. Students who ever attend private (but not charter) schools or move out of state are thus excluded. Finally, I restrict the sample to neighborhoods in which I observe at least 50 permanent residents, and to movers who are between 5 and 20 years old when moving, provided that this is before the outcome variable is measured. A summary of these sample cuts and their implications for sample size is presented in Table A2.

Table A2: Sample Construction

	Observations ( $N \times T$ )	Individuals ( $N$ )
Raw data	7,323,229	1,052,191
Drop students with missing birthdates and drop years with missing address data.	7,322,152	1,051,173
Keep only one observation per student and year	7,055,407	1,051,173
Remove students with missing grade information or mis-coded birthdates.	7,051,849	1,050,401
Drop students never observed after 2007.	6,910,828	1,012,961
Keep only observations from tracts with at least 50 permanent residents	6,846,911	1,002,669
Keep only permanent residents and those who move between ages 5 and 20	6,836,993	1,001,180
<b>Main Sample:</b>		
Permanent residents and one-time movers only	5,351,046	800,879

## A.2 *Demographics, Outcome Measures and Differences Across Neighborhoods*

The first set of outcomes I consider regards performance in two common college admissions exams, the SAT and the ACT. The SAT is owned, developed and published by the College Board and the ACT is administered by a nonprofit organization of the same name. Both are typically taken at the end of junior year in high school. The SAT data covers the



years 2009 to 2017 and I observe SAT scores for 40% of the students in my main sample. This is because some outcomes are measured at an earlier age than the SAT is taken and because the SAT is not compulsory.<sup>27</sup> The latter reason means that the SAT takers are a selected sample – all estimates should thus be interpreted in light of this. An additional feature of the SAT is that it was redesigned in 2016 with a new scoring scale and the previous writing and reading sections were combined into one verbal section. To ensure comparability across time, the outcome I consider is therefore a student’s percentile rank in the national distribution for the math subsection in each year. The ACT data covers three academic years (2013, 2014 and 2015). During these years, all 11th graders in North Carolina were required to take the ACT which removes the selection issue present among SAT takers. Another advantage for the ACT data is the fact that scores are comparable across years. For comparability with the SAT I report results from the math section of the ACT separately. This is graded on a 36 point scale. A disadvantage of the ACT data, however, is the smaller sample size due to the few years available – only 25% of the main sample are observed taking the ACT.

I also consider post-high school graduation intentions. Specifically, I focus on the probability of intending to attend college, defined as any post-secondary institution. This includes 4-year colleges, as well as community colleges (2 year institutions) and trade, business and nursing schools. Data on post graduation intentions was collected from 2009 to 2017 from all graduating high school seniors.<sup>28</sup> For high school graduates, I also consider 12th grade GPA. This is recorded on a 4 point scale. Another outcome I consider is the probability of leaving high school without a diploma (dropping out). In North Carolina, the reason for any student leaving school before graduation has been recorded since 2004. This means dropouts can be observed directly in the data, instead of being inferred from a student suddenly “missing”. This is important since it avoids conflating dropouts with students who transfer to a private

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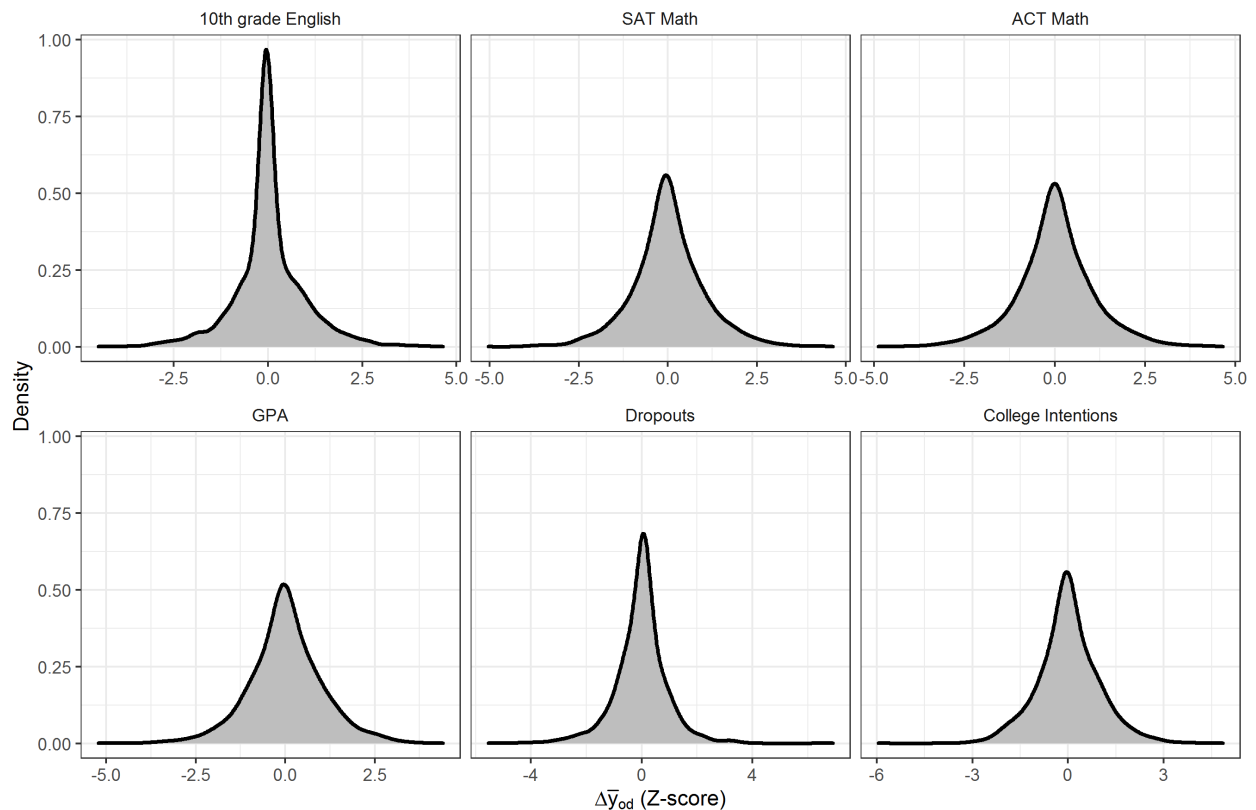
<sup>27</sup>Consider for example a student observed taking a 10th grade math test in 2017 – he or she will be in the main sample, but might not have taken the SAT yet. Conditional on observing a student graduate from high school, SAT scores are available for 67% of the sample.

<sup>28</sup>Students could indicate whether they intend to seek employment, join the military, seek further education at trade/business/nursing schools, 2 year post-secondary institutions (community colleges) or 4 year post-secondary institutions.

school or move out of state. A final outcome I consider is the performance on a standardized, statewide and compulsory end-of-course test in high school, specifically an English test taken in 10th grade between 2013 and 2017. To ensure comparability over time, I standardize these test scores over all test takers in North Carolina at the grade-year level.

Restricting attention to the pairs of tracts I observe students moving between, Figure A1 plots the distribution of realized moves for one-time movers in the main sample as measured by the difference in the outcomes of permanent residents in the origin and destination neighborhoods. Table A3 presents the same data in table form. Finally, Table A4 combines these neighborhood differential with exposure times, to explore the variation remaining in the main regressor of interest ( $t_{id} \times \Delta \bar{y}_{od}$ ), after this has been residualized with respect to the fixed effects employed in the main estimating equation.

Figure A1: Distribution of Origin-Destination Differentials ( $\Delta \bar{y}_{od}$ )



For each outcome measure, this figure shows the distribution of origin-destination differentials,  $\Delta \bar{y}_{od}$ , for the realized moves among one-time movers in the main sample. This differential is the difference in average outcomes of the permanent residents in the origin and destination.

Table A3: Origin-Destination Differentials

A. Distribution of Realized Moves											
Outcome	Mean	SD	Decile ( $\Delta\bar{y}_{od}$ )								
			1st	2nd	3rd	4th	5th	6th	7th	8th	9th
10th grade English test	0.050	0.854	-0.270	-0.144	-0.061	0.000	0.000	0.012	0.086	0.173	0.309
SAT Math percentile	46.301	26.876	-10.895	-5.732	-2.659	-0.057	0.000	1.017	3.728	6.955	12.439
ACT Math Score	19.236	4.681	-2.114	-1.149	-0.539	-0.038	0.000	0.093	0.595	1.234	2.211
12th grade GPA (4pt scale)	3.055	0.950	-0.370	-0.202	-0.094	-0.003	0.000	0.040	0.132	0.248	0.428
Dropouts	0.069	0.254	-0.049	-0.028	-0.015	-0.005	0.000	0.000	0.009	0.022	0.041
College Intentions	0.813	0.390	-0.080	-0.045	-0.022	-0.003	0.000	0.007	0.028	0.053	0.088
B. Distribution of Realized Moves – Standardized Measures											
Outcome	Mean	SD	Decile ( $\Delta\bar{y}_{od}$ )								
			1st	2nd	3rd	4th	5th	6th	7th	8th	9th
10th grade English test	0.050	0.854	-0.270	-0.144	-0.061	0.000	0.000	0.012	0.086	0.173	0.309
SAT Math percentile	0.463	0.269	-0.109	-0.057	-0.027	-0.001	0.000	0.010	0.037	0.070	0.124
ACT Math Score	-0.055	0.975	-0.440	-0.239	-0.112	-0.008	0.000	0.019	0.124	0.257	0.460
12th grade GPA (4pt scale)	-0.076	0.983	-0.383	-0.209	-0.097	-0.003	0.000	0.042	0.136	0.256	0.443
Dropouts	0.069	0.254	-0.049	-0.028	-0.015	-0.005	0.000	0.000	0.009	0.022	0.041
College Intentions	0.813	0.390	-0.080	-0.045	-0.022	-0.003	0.000	0.007	0.028	0.053	0.088

For each outcome measure, this table presents deciles of the distribution of origin-destination differentials,  $\Delta\bar{y}_{od}$ , for the realized moves among one-time movers in the main sample. This differential is the difference in average outcomes of the permanent residents in the origin and destination. The mean and standard deviation of each outcome *among one-time movers* is also presented.

Table A4: Isolating Relevant Variation in the Treatment

Outcome	$(t_{id} \times \Delta \bar{y}_{od})$		
	SD	Resid. SD	Resid.SD
	(1)	(2)	(3)
10th grade English	0.396	0.237	0.132
SAT Math	16.522	10.442	5.896
ACT Math	3.014	1.913	0.910
GPA	2.699	0.539	0.356
Dropouts	0.297	0.064	0.041
College Intentions	0.545	0.111	0.077
Fixed Effects			
Cohort & Age at move		X	X
Origin & Destination		X	
Origin-by-Destination			X

This table presents the standard deviation of the treatment variable,  $(t_{id} \times \Delta \bar{y}_{od})$ , for each outcome. Column 1 reports the raw standard deviation. For the remaining columns, treatment variables are residualized with respect to the fixed effects being employed in the various specifications of this paper before the standard deviation is calculated.

### A.3 School-Quality Measures

To see how schools and school quality impact neighborhood effects, I use a range of school quality measures. To produce school-value added estimates I use scores from standardized testing from 2007 to 2017 in math and reading for grades 3 to 9, in addition to the English scores mentioned above. These tests are compulsory for students in all public and charter schools in the state. I follow a large literature on school and teacher effectiveness and estimate the following value-added model (VAM):

$$A_{i,j,t}^s = \alpha_0 + \alpha_1 A_{i,j,t-1}^m + \alpha_2 A_{i,j,t-1}^r + X_i \psi + \delta_j^s + \nu_{i,j,t},$$

where  $A_{i,j,t}^s$  is the standardized test score of student  $i$  in subject  $s \in \{m, r\}$  (math or reading) taken at school  $j$  in year  $t$ .<sup>29</sup>  $A_{i,j,t-1}^s$  denotes the same student's test scores in either subject in the previous year,  $X_i$  denotes the same vector of student characteristics used in (3) and

<sup>29</sup>See Koedel et al. (2015) and Chetty et al. (2014) for an overview of the literature on value-added modeling.

$\delta_j^s$  is a school-fixed effect or the school value-added for school  $j$  in subject  $s$ .

Table A5: Summary Statistics – School Quality Measures

	Math VA	Reading VA	Growth Score	Achievement Score	Grade-level proficient	On-time graduation
Mean	0.31	0.17	77.71	60.36	56.25%	87.94%
SD	0.15	0.12	11.39	15.82	17.89%	11.82%
Correlation						
Math VA	1.00	0.40	0.26	0.26	0.34	0.06
Reading VA		1.00	0.29	0.35	0.54	0.38
Growth Score			1.00	0.25	0.29	0.32
Achievement Score				1.00	0.94	0.71
Grade-level proficient					1.00	0.65
On-time graduation						1.00

This table reports means, standard deviations and correlations for the school quality measures used in this study. Math and Reading VA refers to the estimated school-value added effects. See main text for definitions of the other measures.

Summary statistics and correlations with other school quality measures for the school value-added are presented in Table A5. It should be noted that the small correlation (0.06) between Math value-added and the percent of high schoolers that graduate on time, is likely due to the fact that the latter measure only is calculated for high schools and Math value-added for these schools reflect a lot of sampling variation, given that only one math test is taken during high school (9th grade). In addition to value added estimates I also use measures of school quality from the School Report Cards produced by the North Carolina Department of Public Instruction. States are required by federal law to produce and publish School Report Cards providing information about school- and district-level data in a number of areas. The measures I use from these report cards are growth scores, achievement scores, percent of students that are considered grade-level proficient and, for high schools, the percent of students that graduate on time. The growth and achievement scores are based on the same underlying data as my value-added estimates. Growth scores provide a measure of students' progress based on standardized testing and can be considered a type of value-added model specified in gains whereas the achievement scores are based on absolute performance on these same tests and seek to measure proficiency. The percent of students

who are grade-level proficient is thus calculated directly from a school’s achievement score. These measures from the School Report Cards complement the school value-added estimates since it is likely that these measures are the kind of measures that families making a choice of where to send their children to school have access to.<sup>30</sup>

Table A6: School Quality Control Models

Model	Additional Controls Included
M1	Reading VA
M2	Math VA
M3	Growth score
M4	Achievement score
M5	Percent grade-level proficient (GLP)
M6	On-time graduation (OTG)
M7	Reading and Math VA
M8	Reading VA, Math VA and growth score
M9	Growth and achievement scores, GLP and OTG
M10	Achivement score, GLP and OTG
M11	All measures
M12-M22	Repeat models M1-M11 but include quadratic terms

This table describes the additional controls added to the main specification (Equation 3) for each of the 22 models presented in Figure 1. In all models, measures enter additively as controls.

I use these school quality measures, and flexible functions of them, as additional controls in the analysis presented in Figure 1 of the main text. Table A6 describes the 22 models used to produce this figure.

As discussed in the main text, to further explore the role that schools play in shaping neighborhood exposure effects, I re-estimate the main specification (Equation 3) with the addition of school district fixed effects and on a sample of students who only ever attend schools and live (but move between census tracts) within one school district. The results of these specifications are presented in Table A7, in columns 2 and 3 respectively, and show that even after conditioning on school districts the magnitudes of the estimates do not change much from the main specification (column 1). Similarly, Table A8 present the results of an

<sup>30</sup>E.g. organizations such as GreatSchools.org use information from School Report Cards when constructing their scores.

Table A7: Controlling for School Districts

Outcome	Convergence Rate ( $\beta_1$ )		
	Main Sample	Same District	
	(1)	(2)	Sample (3)
10th grade English	0.028** (0.012)	0.028** (0.012)	0.034*** (0.012)
SAT Math	0.006 (0.007)	0.005 (0.007)	0.002 (0.008)
ACT Math	0.010 (0.011)	0.010 (0.011)	0.012 (0.011)
GPA	0.015*** (0.003)	0.014*** (0.003)	0.014*** (0.004)
Dropouts	0.016** (0.007)	0.016** (0.007)	0.015** (0.007)
College Intentions	0.013** (0.006)	0.013** (0.006)	0.014** (0.006)
Fixed Effects			
Cohort & Age at move	X	X	X
School District		X	
Origin-by-Destination	X	X	X

This table reports estimates of annual convergence rates ( $\beta_1$ ) while controlling for the school district a student attends. Columns 1 and 2 use the main sample of students who move between census tracts exactly once and students who never move. Column 1 present the main results for comparison, whereas column 2 augments this specification by additionally including school district fixed effects. Column 3 repeats the analysis from column 1 in a sample of students who always reside and attend schools within a single school district. For all cases, standard errors are clustered at the origin-destination level and reported in parenthesis.

analysis where high school fixed effects are included (column 2), and with a sample restriction limiting the sample of movers to those that only ever attend one high school.

#### A.4 Identifying Siblings in the NCERDC Data

I use students' geocoded home addresses and an algorithm described by Gazze et al. (2021) to identify siblings in the data. Since NCERDC does not provide a family-identifier, I need to infer sibling-status by identifying students who share an address in the same year. One feature of the data, is that the address identifiers do not distinguish between different units that share a street address. This means that students living in multi-family housing

Table A8: Controlling for High Schools

Outcome	Convergence Rate ( $\beta_1$ )		
	Main Sample		Same High School Sample
	(1)	(2)	(3)
10th grade English	0.028** (0.012)	0.028** (0.012)	0.026** (0.012)
SAT Math	0.006 (0.007)	0.002 (0.007)	0.003 (0.008)
ACT Math	0.010 (0.011)	0.009 (0.011)	0.008 (0.012)
GPA	0.015*** (0.003)	0.013*** (0.003)	0.016*** (0.004)
Dropouts	0.016** (0.007)	0.015** (0.007)	0.020** (0.008)
College Intentions	0.013** (0.006)	0.011* (0.006)	0.014** (0.007)
Fixed Effects			
Cohort & Age at move	X	X	X
High School		X	
Origin-by-Destination	X	X	X

This table reports estimates of annual convergence rates ( $\beta_1$ ) while controlling for high schools attended. Columns 1 and 2 use the main sample of students who move between census tracts exactly once and students who never move. Column 1 present the main results for comparison, whereas column 2 augments this specification by additionally including high school fixed effects. Column 3 repeats the analysis from column 1 in a sample of students who only attend one high school. For all cases, standard errors are clustered at the origin-destination level and reported in parenthesis.

can get assigned the same address ID, even though they are not siblings. In the data, I observe multiple addresses where dozens or even hundreds of students live in the same year. For this reason, and following Gazze et al. (2021), I exclude addresses where more than four students are observed in a given year before applying the algorithm. I briefly summarize the steps of the algorithm below. For further details, refer to the data appendix of Gazze et al. (2021).

1. Identify all students who live together or could be living together by transitivity and assign them a temporary family ID.



2. For each pair of students within the temporary family IDs, check if they ever are observed living at different addresses in the same year, or are born within 2 and 240 days of each other. If any of these conditions hold, these students cannot be siblings.
3. Given the checks in the previous steps, there might be multiple potential sibling groups within a temporary family ID. If e.g. A can be a sibling to either B or C, but B and C cannot be siblings with each other, I calculate a score for each potential group based on the number of years the students live together, the number of students in the group, and the span of years that the students are observed. I then pick the subgroup with the highest score and assign them a permanent family ID.

In my main sample, this algorithm identifies 139,101 students who are siblings with at least one other student and 27,758 of these students move between census tracts exactly once. Though I match a smaller number of families than Gazze et al. (2021) due to differences in our sample selection criteria, the distributions of the number of children across identified families is similar. I find that 88% of families have at most two children, and the average number of children per family is 1.8, which is similar to numbers provided by the Census for North Carolina. With the matched sibling sample, Table A9 show the estimates presented graphically in Figure 2.

#### *A.5 Robustness to the Choice of Sample and Geographic Scale*

The following set of tables (A10-A12) replicate my main analysis using alternate sample selection criteria, or choice of geographic scale.

Table A9: Exposure Effects in the Sibling Subsample

Outcome	Convergence Rate ( $\beta_1$ )	
	(1)	(2)
10th grade English	-0.042 (0.074)	-0.007 (0.054)
SAT Math	0.009 (0.015)	-0.002 (0.021)
ACT Math	0.037 (0.053)	0.007 (0.045)
GPA	0.002 (0.002)	-0.008 (0.012)
Dropouts	0.002 (0.003)	-0.003 (0.009)
College Intentions	0.008* (0.004)	-0.040 (0.025)
Fixed Effects		
Cohort & Age at move	X	X
Family	X	
Origin-by-Destination		X

\*p< 0.1; \*\*p< 0.05; \*\*\*p< 0.001

This table presents the estimates of convergence rates from Figure 2 in tabular form and explores the robustness of the results to using within-family variation to identify exposure effects. Column 1 repeats the main analysis in this study using the subsample of siblings that can be matched in the data and origin-by-destination fixed effects. Due to the structure of the data, these families all live in single-family housing. Column 2 maintains the sample of matched siblings but replaces the origin-by-destination fixed effects with family fixed effects.

Table A10: Exposure Effects – Robustness to Sample Choice (Origin-by-Destination FE)

Outcome	Convergence Rate ( $\beta_1$ )			
	All movers and PRs (1)	Main sample (2)	Movers only (3)	One-time movers only (4)
10th grade English	0.012 (0.008)	0.028** (0.012)	0.013* (0.008)	0.029** (0.012)
SAT Math	0.008 (0.005)	0.006 (0.007)	0.009* (0.005)	0.007 (0.007)
ACT Math	0.006 (0.007)	0.010 (0.011)	0.007 (0.008)	0.013 (0.011)
GPA	0.014*** (0.003)	0.015*** (0.003)	0.014*** (0.003)	0.017*** (0.004)
Dropouts	0.022*** (0.005)	0.016** (0.007)	0.025*** (0.006)	0.019** (0.008)
College Intentions	0.010** (0.005)	0.013** (0.006)	0.007 (0.005)	0.008 (0.007)

\*p &lt; 0.1; \*\*p &lt; 0.05; \*\*\*p &lt; 0.001

This table reports estimates of annual convergence rates ( $\beta_1$ ) for the various samples. Standard errors are clustered at the origin-destination level and reported in parenthesis. Estimates in all columns include cohort, age-at-move and origin-by-destination fixed effects as well as controls for student characteristics.

Table A11: Exposure Effects – Robustness to Sample Choice (Origin and Destination FE)

Outcome	Convergence Rate ( $\beta_1$ )			
	All movers and PRs (1)	Main sample (2)	Movers only (3)	One-time movers only (4)
10th grade English	0.013*** (0.005)	0.011* (0.007)	0.014*** (0.005)	0.013* (0.007)
SAT Math	0.016*** (0.003)	0.013*** (0.004)	0.017*** (0.003)	0.016*** (0.004)
ACT Math	0.007* (0.004)	0.012** (0.005)	0.008** (0.004)	0.014*** (0.006)
GPA	0.012*** (0.002)	0.011*** (0.002)	0.015*** (0.002)	0.017*** (0.003)
Dropouts	0.017*** (0.004)	0.016*** (0.005)	0.020*** (0.004)	0.021*** (0.006)
College Intentions	0.009** (0.003)	0.012*** (0.004)	0.008* (0.004)	0.012** (0.005)

\*p&lt; 0.1; \*\*p&lt; 0.05; \*\*\*p&lt; 0.001

This table reports estimates of annual convergence rates ( $\beta_1$ ) for the various samples. Standard errors are clustered at the origin-destination level and reported in parenthesis. Estimates in all columns include cohort, age-at-move and origin-by-destination fixed effects as well as controls for student characteristics.

Table A12: Exposure Effects – ZIP Level Estimates

Outcome	Convergence Rate ( $\beta_1$ )		
	(1)	(2)	(3)
10th grade English	0.010 (0.006)	0.004 (0.007)	0.003 (0.008)
SAT Math	0.018*** (0.004)	0.015*** (0.004)	0.009* (0.005)
ACT Math	0.022*** (0.005)	0.007 (0.005)	0.005 (0.007)
GPA	-0.001* (0.001)	0.008*** (0.001)	0.006*** (0.002)
Dropouts	-0.006*** (0.002)	0.003 (0.003)	0.001 (0.004)
College Intentions	-0.008*** (0.001)	0.010*** (0.003)	0.009** (0.004)
Fixed Effects			
Cohort & Age at move	X	X	X
Origin & Destination		X	
Origin-by-Destination			X

\*p&lt; 0.1; \*\*p&lt; 0.05; \*\*\*p&lt; 0.001

This table reports estimates of annual convergence rates ( $\beta_1$ ) for the sample of students who move between 5-digit zip codes exactly once and students who never move. Standard errors are clustered at the origin-destination level and reported in parenthesis. These estimates can be interpreted as the impact of spending an additional year in a tract where permanent residents have one unit higher outcomes. Estimates in each column include cohort and age-at-move fixed effects as well as controls for student characteristics. The specification in column 1 does not control for origin or destination. In (2) and (3), this is controlled for using origin and destination, and origin-by-destination fixed effects respectively.