NBER WORKING PAPER SERIES

ON THE DETERMINANTS OF YOUNG ADULT OUTCOMES: IMPACTS OF RANDOMLY ASSIGNED NEIGHBORHOODS FOR CHILDREN IN MILITARY FAMILIES

Laura Kawano
Bruce Sacerdote
William L. Skimmyhorn
Michael Stevens

Working Paper 32674
http://www.nber.org/papers/w32674

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2024

We thank Luke Gallagher for his assistance with the data on military personnel and their children. We thank Mason Parris-Bacon, Chuyang Guo and Matt Whalen for excellent research assistance. The views expressed in this paper are those of the authors and do not necessarily reflect the policy of the U.S. Department of Treasury, the U.S. Military Academy, the Department of the Army, or the Department of Defense. Any taxpayer data used in this research was kept in a secured IRS data repository, and all results have been reviewed to ensure that no confidential information is disclosed. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed additional relationships of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w32674

On the Determinants of Young Adult Outcomes: Impacts of Randomly Assigned Neighborhoods For Children in Military Families
Laura Kawano, Bruce Sacerdote, William L. Skimmyhorn, and Michael Stevens
NBER Working Paper No. 32674
July 2024
JEL No. I0,I24,J0,J01

ABSTRACT

Using the quasi-random assignment of 760,000 children in U.S. military families, we show that neighborhood attributes experienced during childhood have powerful impacts on SAT scores, college-going and earnings. For earnings and college going outcomes, location during high school is twice as important as location during elementary school, and for SAT scores, location during middle school has the strongest impact. There is little evidence of positive interactions in neighborhood quality across ages groups. Importantly, the same locations benefit children with equal potency across race or sex. Twenty years of exposure in a 1 standard deviation "better" county raises SAT composite scores by 10 points (1.8 percentiles), raises college attendance by 1.7 percentage points, earnings by 2.2 percentile points, and lowers EITC receipt by 10%. Impacts are three times more potent when we measure neighborhood quality at the zip code level: twenty years of exposure to a one (county level) standard deviation better zip code raises college going by 6.7 percentage points, SAT composite by 38 points and income percentile at age 25 by 6.1 points. By equalizing average neighborhood quality for Black and White families, we estimate that the Army's quasi-random assignment reduces Black-white earnings gaps among the children of Army personnel by 23%.

Laura Kawano
University of Michigan
laurakawano@gmail.com

Bruce Sacerdote
6106 Rockefeller Hall
Department of Economics
Dartmouth College
Hanover, NH 03755-3514
and NBER
Bruce.I.Sacerdote@dartmouth.edu

William L. Skimmyhorn
Raymond A. Mason School of Business
The College of William & Mary
101 Ukrop Way
Williamsburg, VA 23187
USA
bill.skimmyhorn@mason.wm.edu

Michael Stevens
Department of Treasury
Michael.Stevens@treasury.gov

1 Introduction

Social scientists have long been interested in understanding how changes in one's environment during childhood shapes their future economic opportunities. But identifying the causal effects of place is challenging, in large part because of selection into the population of movers and the endogeneity of neighborhood choice. To overcome these concerns, we exploit the unique opportunity provided by the institutional features of the U.S. Army assignment process. Service members with the same military occupation and rank – particularly junior personnel – are viewed as interchangeable so that, conditional on these characteristics, the location and duration of assignments made in a given year are as good as random. We use these quasi-random shocks to provide new evidence on the causal effects of neighborhood quality during childhood on young-adult outcomes.[1]

To construct histories of plausibly exogenous relocations, we utilize extensive U.S. Army personnel records. These records provide the location, start date, and end date of every assignment between 1990 and 2017, along with the accompanying rank and military occupation, and rich demographic information for service members, their spouses, and children. In addition to relying on the institutional details of the personnel assignment process, we provide statistical evidence that the location of these assignments are conditionally random and uncorrelated with family background, child age, and measures of parental ability.

For each quasi-random domestic assignment, we construct several measures of place quality based on raw means of demographic characteristics of a location, such as the county-level percent of the adult population with at least a bachelor's degree or median income, and estimates of the causal effects of exposure to a given county on college-going and young adult earnings from Chetty and Hendren (2018*b*), which we refer to as the Chetty-Hendren college and income effects. We examine effects of neighborhood quality on SAT scores that are reported in Army administrative data, and to examine effects on college enrollment,

[1]In 2021, the U.S. Army comprised over 480,000 active-duty personnel with, 237,000 spouses and a combined of 396,000 children (DOD, 2021). On average, soldiers in our data enlist at 22 years old, serve for 7.5 years and experienced 3 different locations during their tenure.

earnings, tax credit and safety net program utilization, and household formation outcomes measured in young adulthood, we link military children to their administrative tax records between 1999 and 2020.

We address several research questions. First, we ask whether exposure to "better" neighborhoods causes improved young-adult outcomes. Our estimates confirm previous evidence that the benefits of relocation accrued to a child depends on the length of exposure (Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018*b*; Chyn, 2018). Twenty years of exposure to a county with a 1 standard deviation higher share of residents with a bachelors degree raises composite SAT scores by 10 points and own college going by 1.7 percentage points. In addition, 20 years of exposure to a county that is one standard deviation higher on the Chetty-Hendren income effect measure raises earnings at age 25 by 2.2 percentile points. Measuring the neighborhood at the zip code of residence level instead of the county levels triples the impacts of place: twenty years of exposure to a zip code with the same standard deviation increases raises college attendance by 6.6 percentage points, composite SAT scores by 38 points, and own income percentile by 6.6. points. Our estimated effects county level on college-going and earnings are about a third to half of the Chetty-Hendren effects based on civilian movers, which could be explained as Army families being impacted less by local neighborhoods due to the frequent moves in the military and a built-in community that comes with Army life. Importantly, the local area effects are much stronger for children who live off-base rather than on-base. When we define neighborhood measures at the zip code level our impacts are equal in magnitude to the effects that Chetty and Hendren estimate.

Second, we test whether the age at exposure matters for outcomes. Effects on SAT scores are strongest during middle school years – at ages prior to when students would take these test – and for both college-going and earnings, the importance of location is greatest during high school years. We also find little evidence of complementarities between location qualities experienced in middle school and high school (or between location in elementary school and middle school). When we abstract from the age at exposure differences, effects appear to be linear in the number of years exposed to a "good" location.

Third, we consider whether exposure to a more affluent neighborhood affects the utilization of various tax benefits or social safety net programs. The first set of benefits we consider are federal benefits that subsidize higher education. The increased likelihood of attending college, along with shifts that we find towards enrollment in four-year private and for-profit institutions, could affect the likelihood that students tap into the. Despite the impacts of place on college attendance, we find no corresponding change in the utilization of the federal tax benefits aimed at easing the costs of higher education, perhaps reflecting that students are able to use the GI benefits earned by their parents.

In theory, growing up in a more affluent neighborhood could also translate into lower rates of welfare receipt either through the presence of positive adult role models during one's formative years or the formation of valuable social networks (Wilson, 1987). However, the empirical evidence on this link is mixed. On the one hand, social service and public assistance utilization is linked to overall usage rates in one's neighborhood (Bertrand, Luttmer and Mullainathan, 2000). On the other hand, Chyn (2018) finds no causal effect of relocation due to public housing demolitions on welfare receipt, food stamps, or Medicaid utilization. For our sample, we find that exposure to "better" neighborhoods increases the likelihood of filing a tax return, and decreases the utilization of the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC). Each year of exposure to a county that is 1 standard deviation higher on the Chetty-Hendren income effect measure decreases EITC amounts by \$5. Twenty years of such exposure lowers EITC use by \$100, or 17 percent of the mean EITC received.

Lastly, we ask whether impacts of location differ by race and sex. We find that effects of location are similar for Black and white children. One implication of this finding is that the Army's use of random assignment (without respect to race) likely shrinks the Black-white earnings gaps among young adults. In our sample, there is a 3.7 percentile point Black-white earnings gap at age 25. Had the white (Black) children in our sample instead been assigned the locational inputs for the average white (Black) family in the U.S., our estimates imply a Black-white income gap of 4.8 percentile points. This further supports the finding in Greenberg et al. (2022) that Army service is an engine for social mobility and shrinking Black-white earnings gaps.

We expand on the rich experimental and quasi-experimental literature that points towards a growing consensus that place matters.[2] Analyses of the Gatreaux program (Rosenbaum, 1995; Rosenbaum et al., 1999), the Moving to Opportunity (MTO) Experiment (Chetty, Hendren and Katz, 2016), and public housing demolitions (Chyn, 2018) suggest that neighborhoods have significant beneficial impacts on long-run outcomes. These studies consistently reveal that relocating from a disadvantaged neighborhood to a more affluent one translates into improvements in longer-run outcomes, particularly for those who are moved during their early childhoods.[3] This work also highlights the importance of taking a longer-run lens into how neighborhood exposures during childhood translate into outcomes; the earlier work on MTO (Katz, Kling and Liebman, 2001) (Ludwig, Duncan and Hirschfield, 2001) and public housing demolitions (Jacob, 2004) suggest there is little influence of moving and the corresponding change in neighborhood attributes on short-run academic performance.[4]

Whereas these interventions move children to better neighborhoods by design, an important and growing body of work examines the effects of different types of neighborhoods more broadly. The seminal paper by Chetty et al. (2014) uses administrative tax records to describe differences across localities in the U.S., showing that some cities offer far more intergenerational income mobility than others. Chetty and Hendren (2018*a*) extend this work

[2]There also exists a large set of observational studies that largely suggests that the quality of the neighborhood in which one goes grows up is correlated with longer-run outcomes. See Wilson (1987); Jencks, Mayer et al. (1990); Sampson, Morenoff and Gannon-Rowley (2002); Sharkey and Faber (2014), Massey 1993.

[3]An important exception is Oreopoulos (2003), which finds no evidence that shifts in neighborhood influence earnings at age 30 in Toronto.

[4]Other relevant studies for this work are those that analyze changes in schools (Abdulkadiroğlu et al., 2011), school desegregation (Billings, Deming and Rockoff, 2014), from shocks to income, and adoption into different families (Sacerdote, 2007; Björklund, Lindahl and Plug, 2006).

by examining how exposure to different counties in childhood generates different outcomes in young adulthood, and apply several strategies which suggest these estimates represent causal effects. We contribute to the set of studies that find that these broader neighborhood quality measures are indeed predictive of the causal exposure effects in experimental and quasi-experimental settings (Chetty, Hendren and Katz, 2016; Chyn, 2018; Shoag and Carollo, 2016). Our analysis also documents the importance of these effects in a policy-relevant sample with moderate income and education, and one for which there is significant public interest – the all-volunteer military.

Our variation in neighborhood exposure is plausibly exogenous and we have much larger samples than the experimental work in this area. With these features, we are able to more deeply analyze the functional form and age effects by which exposure translates into outcomes – finding both that a surprisingly simple and linear production function can be used to relate exposure to long run outcomes, and that child age at exposure is important. Nevertheless, the military experience is unique and requires thoughtful consideration when extending the lessons learned to a broader population. The demographic characteristics of Army families differ from those of the average civilian family: they tend to be younger (average age of enlistment of 22 and average age of first child of 26), poorer, and are more likely to be nonwhite (45% nonwhite). Military families also move often, and assignments can come with extended periods where a military parent is absent from the home.

2 Institutional Details and Data Description

2.1 Overview of the Army's Assignment Process

Each year, the Army Human Resources Command (HRC) must fill hundreds of thousands of jobs at bases around the world. To make these assignments, HRC follows Department of Defense policy and Army Regulations,[5] which prioritize the "needs of the army" over individual preferences. These needs are driven by the global landscape that dictates the demand for military personnel across bases, and are fulfilled by the soldiers that can complete specific tasks based on their primary military occupational specialties (PMOS) at each level of the military rank hierarchy. For example, the tasks of an infantryman in the rank of private differs from those of both an infantryman in the rank of sergeant and an attack helicopter repairer in the rank of private. From HRC's perspective, service members with the same PMOS and military rank are interchangeable, and the assignment process treats them as such. Thus, for a given assignment year, a service member's assignment location and duration of an assignment is effectively random, conditional on PMOS and rank.

Given this unique, quasi-random assignment process, economists have exploited military assignments to overcome concerns over the endogeneity of relocation decisions to answer several important questions. Military assignments have been used to show the effects of parental absences due to military deployments on divorce and spousal employment (Angrist and Johnson, 2000), the impact of combat deployments on financial health and deaths of despair (Bruhn et al., 2022; Sabia and Skimmyhorn, 2023), the adverse health effects of exposure to ozone (Lleras-Muney, 2010*b*), the effects on financial or labor market outcomes through access to payday lending (Carter and Skimmyhorn, 2017; Carrell and Zinman, 2014), and peers effects in service member educational choices (Murphy, 2017).

Soldiers may submit their preferences over location assignments, but in practice, assignments are largely independent of such preferences particularly early in one's military career. Longer-serving service members may have some degree of control over their assignments. Officers and more experienced enlisted personnel are more likely to submit preferences for base assignments and may be requested by commanders or obtain specific assignments as part of the reenlistment process. We follow the earlier literature by dropping officers from our sample, and we also focus on junior personnel with ranks of E1 (Private) through E6

[5]These policies are Department of Defense Directive 1315.07 "Military Personnel Assignments" and AR 600-14 "Enlisted Assignments and Utilization Management", respectively.

(Staff Sergeant).

While the institutional rules and previous literature provide a strong prior for the quasi-random nature of Army assignments, we complete a few additional steps. First, we reviewed the publicly available HRC documents and online information detailing assignment process.[6] Second, the anecdotal evidence drawn from discussing assignments with enlisted personnel support the conditional random assignment assumption. Finally and most importantly, we show empirically that there is little correlation between base characteristics and personnel or family characteristics at baseline once we condition on PMOS, rank and year of assignment.

2.2 Data Sources

Army personnel data and Department of Defense (DOD) data come from the Office of Economic and Manpower Analysis (OEMA) at the U.S. Military Academy. These data include information on Army service members and their children from 1990-2017 and comprise nearly 500,000 service members and their 800,000 children. For each service member, we observe the location, start date and end date of every assignment, along with their rank and PMOS at the time of assignment. We also observe when each service member first joined the Army, when he separated from the Army, and the type of discharge (known as Characterization of Service) they received. The demographic information on each service member includes gender, detailed race codes, home state, birth date, marital status, number of dependents, Armed Forces Qualification Test (AFQT) scores, and educational attainment, each measured at the time of enlistment. For each child, we know their date of birth, gender, and race.

For each child, we construct a panel of their location histories spanning from birth through age 17 based on their enlisted parent's assignment records, though these are often incomplete. We can observe in the tax data locations after the parent exits from the Army. However, in our regressions we only include information on child-years while the parent is still in the Army and hence child locations are exogenously assigned. Appendix Figure A.1a shows the

[6] Special thanks to Major Fran Murphy for assisting with this effort.

distribution of the number of years that we observe child locations; on average, we observe children for 6.3 years. The distributions of the age at which we first observe children in the sample and of all observed ages are shown in Appendix Figures A.1b and A.1c, respectively.

For each U.S. assignment, we use two primary county-level measures of neighborhood characteristics; if a child moves during a particular age, this is a weighted average of characteristics for the two neighborhoods based on exposure duration. The first measure is the percent with a B.A. in a county from the 2005 American Community Survey (ACS). The second measure is the causal estimate of the impact of one year of exposure during childhood on percentile earnings from Chetty and Hendren (2018*b*), which refer to as the "Chetty-Hendren income effect." We use their estimates for families at the 25th percentile of parental income distribution, which aligns closely with the incomes of the military families in our sample.

For completeness, we also examine several alternative neighborhood quality measures. County-level percent of families above 150 percent of the poverty line, percent Black, and median income come from the 2005 ACS and from Chetty and Hendren (2018*b*), we collect the causal estimate of one year of exposure in the county of a child's college enrollment probability ("Chetty-Hendren college effect"). Average adjusted gross income (AGI) at the county level come from the IRS Statistics of Income and are measured for the 2008 tax year. There are also a number of neighborhood characteristics that we measure at the zip code level from Census: population, per capita income and percent Black population. Standardized test scores at the district level are obtained from Reardon et al. Fahle et al. (2024) and collapsed to the zip code level weighting by enrollment.

Our first set of outcome variables regards academic achievement. Almost half of military children take the SAT during the period spanning the Army records, and for these students we observe their math, verbal and composite scores. The remainder of our outcome variables come from administrative U.S. federal tax records between 1999 through 2020. We match Army records to tax data using social security numbers of the parents and the children. We determine college attendance using Form 1098-T, which colleges submit to the IRS to report qualified educational expenses in a given year. We construct an indicator variable for having a Form 1098-T in any year between ages 17 and 22 as our primary college-going measure, and sum over the years with a Form 1098-T between ages 17 and 22 as a way to proxy for attaining a bachelor's degree. We match these forms with data from the Integrated Post-secondary Education Data System (IPEDS) to determine the type of institution in which the child is enrolled: two-year public, four-year public, four-year private, or for-profit.

Changes in college-going rates or the types of institutions that students attend also affect the utilization of federal benefits for higher education. The costs associated with college can be subsidized through the use of various tax benefits for higher education: American Opportunity Tax Credits (AOTC), Lifetime Learning Credits (LLC) and the tuition and fees deduction. We capture the these benefits using either the child's own tax return, or their parents tax returns if they are claimed as a dependent, all measured at age 20.

Our second set of outcome variables focus on labor market outcomes. We construct measures of working and earnings using wage and salary income reported on Form W-2, and self-employment income from Schedule SE for tax filers or non-employee compensation from Forms 1099-MISC/1099-NEC for non-filers, which includes income for contract work or temporary jobs. For each individual, we sum over the forms received across multiple employers, if relevant. We focus on work probabilities and earnings at age 25 because we are using the Chetty-Hendren income effect for that same age as a measure of neighborhood quality. We translate these earnings amounts into their corresponding earnings percentiles among 25 year olds based on Chetty et al. (2020).

Changes to earnings and household income can have cascading effects onto the likelihood of tapping into the social safety net, several components of which are administered through the tax code and thus observable to us. We examine the Earned Income Tax Credit (EITC) and Child Tax Credit (CTC) which are observable at the household level. Finally, tax returns allow us to examine some household formation outcomes – marital status and the presence of dependent children – along with tax filing status. All monetary values are adjusted to

2022 dollars and winsorized at the 1st and 99th percentiles.

We limit the sample to enlisted service members (i.e., we exclude warrant and commissioned officers) who have at least one child aged 0 and 17 during their military service. Because senior personnel may have some control over their location assignments, we exclude observations where a location assignment is made when a parent has been in service for at least 10 years or has rank of E7 or higher. We also restrict our sample to children born after 1974 or before 1999. This cutoff is dictated by our desire to observe young-adult outcomes (at least one of college going or earnings at age 25) for children in the sample: all military children in our sample will be at least 21 by the last year of our tax data (2020). These restrictions reduce the sample to 439,000 service members with 760,483 children.

2.3 Description of Military Families and their Relocations

Military families differ from the civilian population in a number of dimensions. In Panel A of Table 1, we provide summary statistics on demographic characteristics for our sample measured at the time of enlistment. These means are calculated at the level of the child, meaning that, for example, a parent with two children will be included twice. The distribution of the number of children for households in our sample, which varies from one to twelve, is shown in Appendix Figure A.1d. Fifty-one percent of these service members have only one child, and the remainder have multiple children.

Ninety percent of the service members are male, and half of their children are male. Thirty-one percent of service members are Black, 8 percent are Hispanic, and 55 percent are white. Eighty-eight percent of service personnel with children are married. The average AFQT score is 55, where the scale is in percentile points and ranges from 1 to 99. Approximately 54 percent of service members graduated from high school or have a GED, 22 percent has some college education or an associate's degree, and an additional 6 percent has a bachelor's degree. To provide a sense for service member jobs that are most typical, we provide tabulations of the 20 most common PMOS codes for our sample in Appendix Table

A.1.

Table 1: Summary of Personnel Demographics and Base Characteristics

	Mean (1)	Std Dev. (2)	N (3)
Panel A: Demographics			
Male	0.90	0.31	760,483
Child is male	0.50	0.50	760,483
White	0.55	0.50	760,483
Black	0.31	0.46	760,483
Hispanic	0.08	0.27	760,483
Other race	0.06	0.24	760,483
Married	0.88	0.33	760,483
AFQT score	55.03	17.95	760,483
Completed GED	0.04	0.18	760,483
High school graduate	0.50	0.48	760,483
Associate's degree	0.07	0.24	760,483
Some college	0.22	0.40	760,483
College degree	0.06	0.23	760,483
Start year	1987.20	9.33	760,483
Year of birth	1965.68	8.45	760,483
Child year of birth	1989.60	6.48	760,483
Panel B: Moves and Base Characteristics			
Number of moves	2.76	2.01	760,483
County BA rate	0.25	0.06	760,483
Chetty-Hendren income effect	-0.21	0.17	760,483
Chetty-Hendren college effect	-0.0007	0.004	760,483
Average AGI	28.99	9.19	760,483
Ever deployed overseas	0.69	0.76	760,483
Years of overseas deployment	2.14	1.59	760,483
Lives off base	0.90	0.24	760,483

Notes: This table provides means, standard deviations, and number of observations for each child in our sample. Panel A is obtained using Army personnel and Department of Defense data. Panel B provides summary statistics for the total number of observed moves, and the average neighborhood characteristics of assigned bases.

The average number of observed moves and neighborhood attributes that correspond with these Army assignment locations in the U.S. are shown in Panel B of Table 1. Children in military families move frequently relative to their civilian counterparts, with approximately 2.8 moves, on average, within a typical observed 6.3 year window. They also live in a county in which 25 percent of adults have at least a bachelor's degree, on average, quite similar to the U.S. population-weighted average. But these counties also have substantially lower

Chetty-Hendren causal impacts on earnings per year of exposure than the U.S. average of -0.06. We provide the exact assignment numbers at the largest U.S. bases in Appendix Table A.2. Approximately 69 percent are stationed overseas at least once in our sample, for an average of 2.14 years. Our neighborhood quality measures are only defined for domestic assignments, and so we exclude overseas assignments from our analysis.

The relocation and neighborhood information above corresponds to the base to which the service member is assigned but families may choose to live elsewhere. Our data also contain information on family residence location at each assignment, along with an indicator variable for whether the family lives on or off base. Families live off base 90 percent of the time, in the same county as the base 34 percent of the time, and in the same zip code as the base 14 percent of the time. Our baseline estimates use the characteristics of the county containing the randomly assigned Army base, but also provide estimates using the characteristics of the neighborhoods where the family lives, instrumented with the characteristics of the randomly assigned base neighborhood, at both the county and zip code levels.

The correlation between county attributes of the assigned base and residential location is 0.61 for the Chetty-Hendren causal earnings effect and 0.57 for the BA rate. A caveat is that we observe military families during parents' time in service, and so the composition of families changes over time as families enter and exit. As a result, we will observe some children for longer periods if their parents choose to reenlist. How this selection affects our estimates is ambiguous, as it depends on whether and how reenlistment is correlated with the extent to which neighborhood characteristics transmit to military children.

In Table 2, we show summary statistics for our outcomes of interest. Sixty-seven percent of military children attend college at some point between ages 17 and 22. Among the 563,663 children we observe for all years between ages 18 and 22, 46 percent have at least four years with 1098-T. A little over a third of children attend a four-year public institution, and thirty percent attend a community college. Thirteen percent attend a four-year private institute, and only 5 percent enroll in a for-profit institution. When these military children are 20, both parents and children make use of tuition and fee tax deductions and education tax credits.

On average, wage income at age 25 is \$26,264 (in 2022\$), which corresponds to the 48th percentile of the US income distribution at age 25. Eighty-eight percent of those observed at age 25 have positive wage income. The fact that the majority of our sample is working and is in the middle of the income distribution makes this a sensible group to study impacts of child location on use of tax credits and filing. The average amount of EITC claimed is \$585 while the amounts of CTC and ACTC claimed are \$67 and \$147 respectively.

3 Research Design

In each year, our data contains many lotteries among service members that are up for (re)assignment and have the same PMOS and rank. Because of this resulting conditional random assignment, our estimation strategy is intuitive and straightforward. Our simplest specification regresses a child's outcome on characteristics of an assigned location and PMOS $\times$ rank $\times$ assignment year fixed effects – the level at which our quasi-experiments occur. Specifically, these regressions take the following form:

$$Y_i = \beta_0 + \beta_1 Be_{i,t} + \Omega X_i + \gamma_{i,t(a)} + \varepsilon_{i,t}. \tag{1}$$

The key right-hand side variable, B, corresponds to a characteristic of the county of the assigned base that we multiply by the number of years of the assignment to that location, e.[7] Exposure time is calculated using the start and end dates of each Army assignment associated with a particular age. The vector γ represents PMOS-rank-assignment year fixed effects associated with each assignment.

[7]Our preferred neighborhood measures are defined at the county, rather than zip code, level. Zip code based measures would require that we focus on IV estimates because of the smaller geography that sometimes is isolated to a base, which comes with increased es- timation variance. However, we also present results using zip code based measures in Section 4.1.

Table 2: Summary of Outcome Variables

	Mean (1)	Std Dev. (2)	N (3)
SAT Scores			
Composite Score	980	199	187,486
Math Score	486	106	187,486
Verbal Score	494	107	187,486
College Attendance (Ages 17–22)			
Ever Attended College	0.67	0.47	693,857
Public 2-year	0.31	0.46	693,857
Public 4-year	0.34	0.47	693,857
Private 4-year	0.13	0.34	693,857
Private for-profit	0.05	0.22	693,857
Other 2-year	0.02	0.14	693,857
Years of College Attendance (Ages 17–22)			
All	2.40	2.40	693,857
Public 2-year	0.73	1.37	693,857
Public 4-year	1.14	1.93	693,857
Private 4-year	0.40	1.22	693,857
Private for-profit	0.10	0.49	693,857
Other 2-year	0.03	0.24	693,857
Utilization of Federal Benefits for Higher Education (Age 20)			
Parent's education tax credit	218.90	524.49	693,857
Own education tax credit	24.31	146.96	693,857
Parent tuition & fee deduction	106.47	541.32	693,857
Own tuition & fee deduction	20.42	221.50	693,857
Parent refundable education tax credit	113.45	339.80	693,857
Own refundable education tax credit	31.09	162.85	693,857
Earnings and Other Tax Outcomes (Age 25)			
Has labor income	0.88	0.33	504,591
Income percentile	47.75	24.68	504,591
Earnings	26263.71	24764.48	504,591
Earned Income Credit	584.66	1255.72	504,591
Child Tax Credit	66.75	398.92	504,591
Additional Child Tax Credit	147.26	448.62	504,591
Filed Return	0.84	0.37	504,591

Notes: This table provides means, standard deviations, and number of observations for each child in our sample. The differences in the number of observations are due to whether a child is old or young enough to be observed in college board data or tax records for a particular outcome.

We estimate equation 1 using a version of the data in which an observation represents a child–year. We also estimate a version of equation 1 that instruments for the key neighborhood characteristic of the county or zip code of residence using characteristics of the randomly assigned base. The coefficient of interest, β_1, represents the causal effect one year of exposure to a particular location, restricting location effects to be constant across child ages. For each child, we only observe a single set of outcomes measured at particular ages, and so this specification uses variation in a child's locations to which they are exposed throughout childhood but outcomes that are fixed. Although the functional form restrictions and the repetition of the observations for the same child could lead to overstated precision, we cluster standard errors at the child level. To add precision, we include a number of control variables in the vector X: parental AFQT score, and indicator variables for child year of birth, parent year of birth, parent's entry year into the Army, parental education, parent male, Black, and length of assignment.

To allow for heterogeneity and tests of complementarities of location quality effects across ages, our preferred specification uses a cross-sectional version of the data in which there is one observation per child and many right-hand side variables describing the locations experienced at each age a. Formally, this specification is:

$$Y_i = \beta_0 + \sum_{a=1}^{17} \beta_a B_{i,a} + \Omega X_i + \Gamma_i + \varepsilon_i \tag{2}$$

but because this equation generates 17 coefficients of interest, we further simplify our equation by imposing that neighborhood effects are constant within four age groupings that roughly correspond to different educational periods. We refer to ages 0 to 5 as preschool years (P), 6 to 10 as elementary school years (E), 11 to 13 as middle school years (M) and 14 to 17 as high-school years (H). This yields the following specification:

$$Y_i = \beta_0 + \beta_P B_{i,P} + \beta_E B_{i,E} + \beta_M B_{i,M} + \beta_H B_{i,H} + \Omega X_i + \Gamma_i + \varepsilon_i. \tag{3}$$

The key right-hand side variables in equation 3 are the exposure-weighted sums of a location characteristic experienced during a particular age grouping.[8] In this way, our coefficients can be interpreted as the effect of one year of exposure to a neighborhood quality measure 1 point higher during a particular age group. Because we do not observe locations during childhood when parents are not enlisted and do not capture neighborhood characteristics for overseas assignments, we additionally include a series of dummy variables that equal one if a particular age group is missing, and the exposure-weighted neighborhood characteristic is set equal to zero. The matrix Γ contains four vectors of PMOS-rank-assignment year fixed effects that correspond to the first assignment fixed effect observed for each age group, capturing the lotteries that determine the parental assignment during that period.

3.1 Validating the Quasi-Randomness of Military Assignments

The validity of our research design rests on whether assignments made in a particular year are uncorrelated with family characteristics after conditioning on PMOS and rank. While the institutional rules and previous literature provide strong support for the quasi-random nature of Army assignments, we provide empirical evidence for this identifying assumption in our sample.

We estimate the following specification:

$$B_{i,t} = \alpha_0 + \alpha_1 X_i + \gamma_{i,t(a)} + \varepsilon_{i,t} \tag{4}$$

where B represents a base characteristic and X is a vector of characteristics of the Army family. We estimate this equation separately for the four child developmental periods considered The dependent variable is the exposure-weighted average of a neighborhood characteristic to which the service member is assigned during a child's development stage, and γ corresponds

[8] For example if a child spends ages 0 and 1 in a county (or zip code) with a BA rate of 0.50 and ages 2 through 5 in a county (or zip code) with a BA rate of 0.60, our exposure-weighted county BA measure is (2×0.50+4×0.60)/6.

to the first PMOS-rank-assignment year fixed effect for that stage. This specification tests whether children whose parents have the same military occupation and rank and come up for assignment in the same years are indeed sent to locations on a seemingly random basis by examining the extent to which the controls in X explain the characteristics of a base assignment conditional on PMOS $\times$ rank $\times$ assignment year fixed effects. We also run a version of this specification where each observation represents a child-year.

Appendix Tables A.3–A.6 provide evidence of conditional random assignment for each developmental period, and Appendix Table A.7 presents results when each observation represents a child-year. The first two columns correspond to our primary neighborhood quality measures and we examine other measures in the remaining columns. Panel A presents the R-squared statistic for regressions using only PMOS $\times$ rank $\times$ assignment year fixed effects, and Panel B presents results when we additionally include family characteristics.[9]

Comparisons of R-squared statistics across Panels A and B universally show there is very little additional variation in neighborhood characteristics explained by family attributes after we account for the source of random assignment under the Army's procedures. In the bottom two rows, we report the F-statistics and p-values for joint tests that the coefficients on the family characteristics are equal to zero. There are a small handful of cases where we fail to reject that family characteristics are uncorrelated with the types of neighborhoods to which service members are assigned, but the magnitude of any statistically significant relationship between a household characteristic and neighborhood quality is always economically small. For example, during preschool years, the children of Black Army personnel are on average assigned to counties that have a 0.0005 percentage point higher average percent BA, relative to an overall average of 25 percent (Appendix Table A.3). In the spirit of Altonji, Elder and Taber (2005), this general lack of correlation between our observed characteristics and treatment suggests that any unobserved characteristics are also unrelated to treatment.

[9] The relevant fixed effects for conditional random assignment correspond to the PMOS $\times$ rank $\times$ assignment year at the start of a developmental period in Appendix Tables A.3–A.6, and correspond to the PMOS $\times$ rank $\times$ assignment year at the time the relevant assignment was made in Appendix Table A.7.

3.2 Correlation Between Neighborhood Quality and Outcomes

Before turning to our regression estimates, we provide visual representations of the causal effects of exposure to better neighborhoods during childhood and later outcomes. Figure 1 presents a series of scatter plots with ventiles of neighborhood characteristic on the x-axis and average education and earnings outcomes residualized for demographic characteristics and PMOS × rank × assignment year fixed effects on the y-axis.

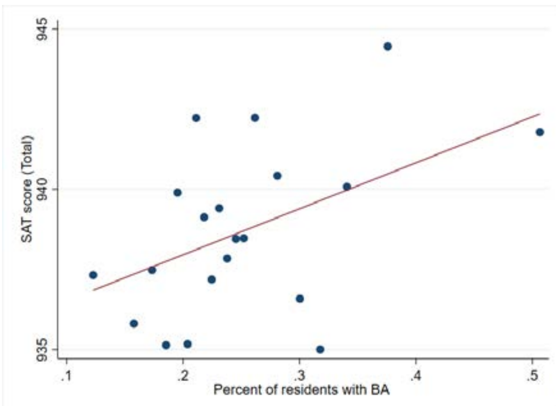
Both composite SAT scores and college attendance probabilities rise linearly with exposure to better counties. A single assignment (typically 3.5 years) to a base in a county with 10 percentage point higher BA rate corresponds to a roughly 3.5 point increase in composite SAT and a 0.4 percentage point increase in the likelihood of attending college. An additional ten years in a county with a BA rate above the 25th percentile increases college attendance by a little more than 3 percentage points.

The relationship between exposure to a county with a higher Chetty-Hendren causal income effect and a child's own earnings at age 25 also shows a striking steep and linear relationship between later outcomes and childhood place characteristic. The slopes imply that moving from a place with a Chetty-Hendren income effect of -0.5 percentiles to 0.5 increases a child's own earnings by $1,300 or roughly 1.3 percentile points. Given the average years of exposure to a location, these results imply a slope on the Chetty-Hendren income measure of 0.4, somewhat smaller than the expected slope relationship of around 1 if the causal neighborhood effects from Chetty-Hendren's sample of all movers similarly applied to military children.

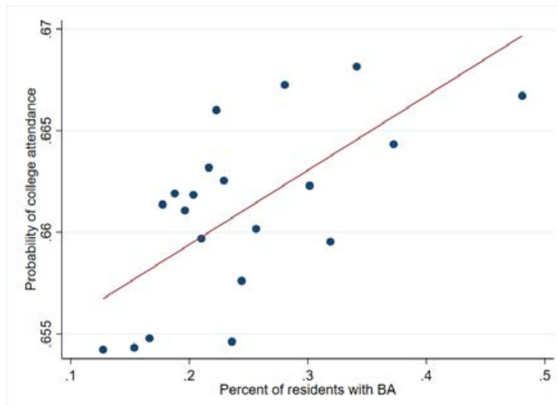
Figure 2 shows that these relationships become much steeper when we define neighborhood as the zip code where the family lives. A single assignment (2.5-3 years) residing in a zip code with a 10 percentage point higher BA rate raises composite SAT scores by about 10 points and own college going by 1.5 percentage points. Residing in a zip code with an annual household income that is $20,000 higher raises own income at age 25 by $1,100.

Figure 1: Correlation Between Exposure to Better Neighborhoods and Outcomes

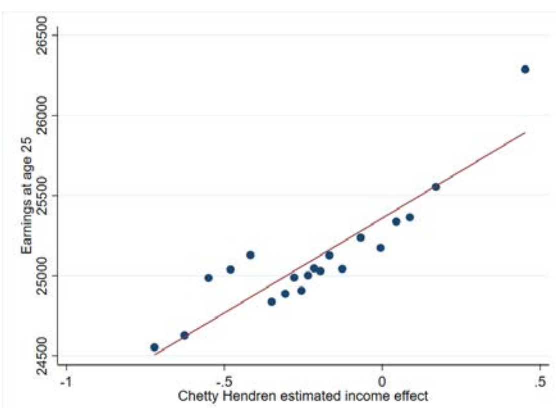
Composite SAT Scores



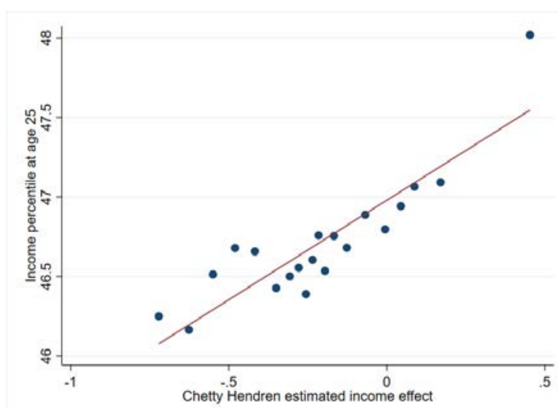
College-Going



Earnings at age 25

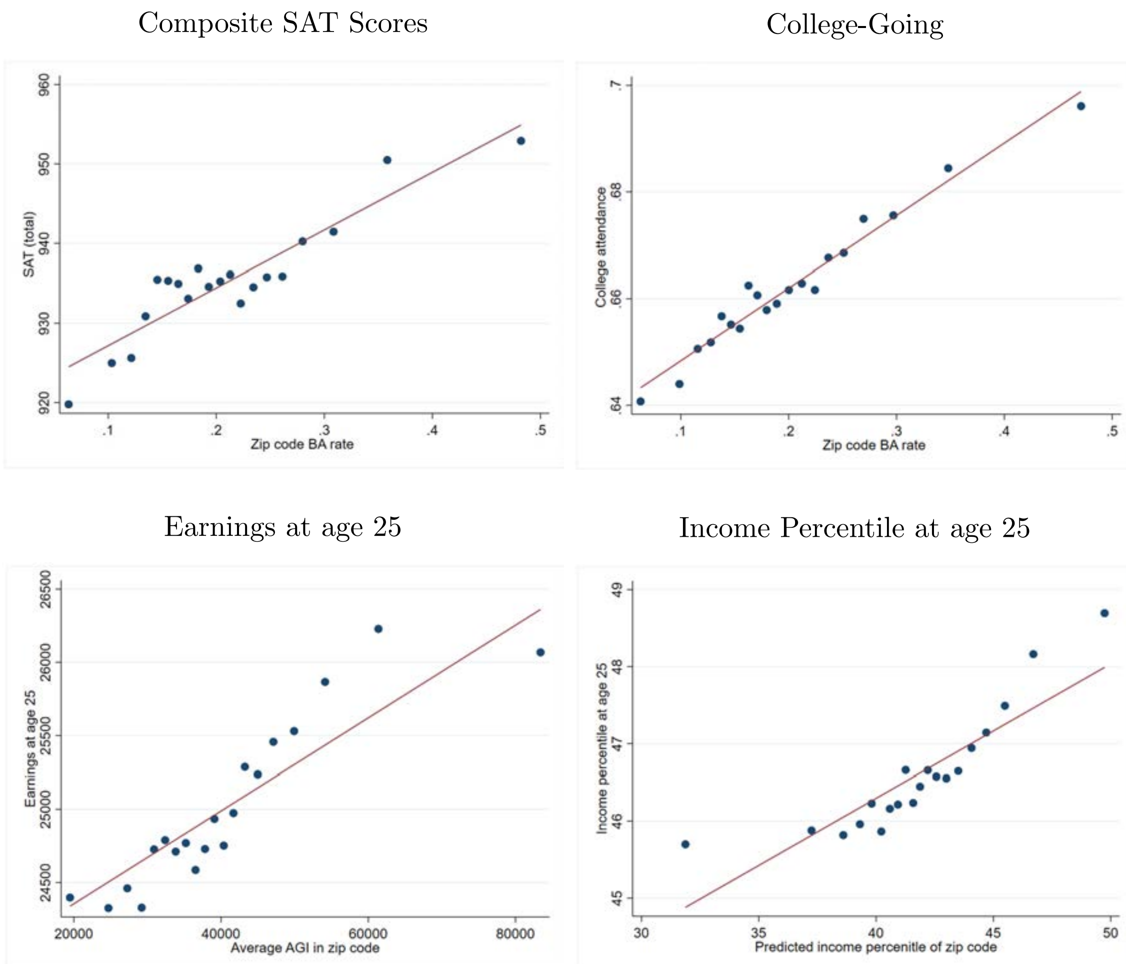


Income Percentile at age 25



Notes: The x-axis bins observations by ventiles of the BA rate for the county in the top row and by ventiles of the Chetty-Hendren causal income effect in the bottom row. The y-axis is the bin-averaged outcome that has been residualized for parent demographics and parents' PMOS, rank, and year of assignment to the base.

Figure 2: Correlation Between Exposure to Better Zip Codes and Outcomes



Notes: The x-axis bins observations by ventiles of the BA rate for the zip code in the top row and by ventiles of the average AGI and predicted income percentile effect in the bottom row. The y-axis is the bin-averaged outcome that has been residualized for parent demographics and parents' PMOS, rank, and year of assignment to the base.

4 Results

4.1 Impacts of Location

Table 3 presents estimated neighborhood exposure effects from equation 1 for our education and earnings outcomes. As anticipated by the scatter plots, we find a positive and statistically significant effects of exposure to better neighborhoods during childhood on all outcomes considered. A single year of exposure to a county with a 10 percentage point higher BA rate increases SAT scores by 0.32 points, stemming from roughly equal improvements in math and verbal scores, and raises the probability that a child attends college by 0.09 percentage points. Exposure to a county with a higher Chetty-Hendren causal effect on college attendance increases the probability that that child attends college. However, the coefficient of 0.13 suggests that military children take on college attendance impacts of a county by less than the Chetty-Hendren prediction that includes civilian movers.

The final three columns examine effects on earnings at age 25. Exposure to a county with a higher Chetty-Hendren causal income effect increases a child's own earnings at age 25, but the estimated coefficient of 0.32 is smaller than the neighborhood effects for a broader set of movers. The log-log specification in column 7 suggests that after 20 years of exposure to a county, a child would pick up about 40% of the income advantage (disadvantage) of that county. This estimate is roughly half of the Chetty-Hendren estimate of the rate at which the outcomes for children who move converge towards the outcomes of children in the destination county.

We further explore the relative magnitudes of neighborhood effects by examining the relationship of base fixed effects on child income percentile at age 25 and the Chetty-Hendren causal income effect, shown in Appendix Figure B.2. Base fixed effects are calculated by regressing a child's income percentiles at age 25 on a series of fixed effects for base of assignment, controlling for parent demographic characteristics and PMOS $\times$ rank $\times$ year of assignment fixed effects. These base fixed effects simply measure the effects of an assignment

Table 3: Neighborhood Exposure Effects on Education and Earnings Outcomes

	(1) Total SAT Score	(2) Math SAT Score	(3) Verbal SAT Score	(4) College Attendance	(5) College Attendance	(6) Inc Percentile at Age 25	(7) Log Earnings at Age 25	(8) Earnings at Age 25
Percent of Residents with Bachelors Degree	3.156**	1.590*	1.567*	0.00893***				
	(1.501)	(0.818)	(0.826)	(0.00198)				
Chetty-Hendren College Effect					0.134***			
					(0.0288)			
Chetty-Hendren Income Effect						0.317***		
						(0.0411)		
Log Avg. Income							0.0200***	
							(0.00486)	
Avg. Income								0.0173***
								(0.00284)
Constant	866.8***	425.9***	440.9***	0.594***	0.596***	43.49***	9.407***	21,737***
	(5.196)	(2.812)	(2.824)	(0.00696)	(0.00695)	(0.387)	(0.0538)	(363.5)
Observations	596,463	596,463	596,463	2,650,637	2,650,064	1,592,369	1,458,146	2,017,007
R-squared	0.205	0.196	0.185	0.089	0.089	0.051	0.049	0.086

Notes: This table displays estimates of neighborhood exposure effects on child outcomes based on equation 1. The neighborhood measures refer to the county of the assigned base. Regression coefficients are interpreted as the causal effect of one year spent in the county, controlling for child year of birth, parental AFQT, education, Army start year, and race, length of assignment, and PMOS × rank × year of assignment fixed effects.

to a base on a child's later outcomes. A base's corresponding Chetty-Hendren causal income effect measure predict base fixed effects with a coefficient of 1.05, so after correcting for an average exposure length of 3.5 years, the effect of a single year of exposure to a county is on average about 0.29 times the Chetty-Hendren effects for a year of exposure to that county. This is consistent with the estimate in column 3 of Table 3 of 0.32.

The smaller neighborhood exposure effects on college-going and earnings for military children relative to movers examined in Chetty and Hendren (2018*b*) could be due to several factors. Because children in military families move often, they may be less influenced by each neighborhood experienced. Moreover, roughly 10 percent of military children live on base and military families might have their own Army-based communities whether or not they live on base, dampening the transmission of neighborhood treatment effects. In Appendix Table B.1, we show that while the estimated effects of exposure to a location remains quite similar to our baseline when focusing on children living off base (column 1 and 3), the effects almost completely disappear when we limit the sample to children living on base (columns 2 and 4), suggesting that residing off base and within the community is necessary for counties to have large impacts on children.

Another possible explanation for these muted effects is that our measurement of location is less precise. Our main specifications define neighborhood attributes using assigned base, where conditional random assignment is plausibly achieved, but this may not well measure the actual neighborhood characteristics to which children are exposed. We explore this hypothesis in two ways. We first estimate equation 1 replacing base neighborhood characteristics with residence neighborhood characteristics. These results are shown in Appendix Table B.2 and we reassuringly find that the estimated effects are somewhat larger and more precise. Second, we switch the definition of a neighborhood to the zip code where the child resides during that base assignment in Panel A of Table 4. A single year of exposure to a zip code with a BA rate that is 10 percentage points higher raises college attendance by 0.48 percentage points and SAT composite by 2.6 points, and a year of exposure to a zip code with household AGI that is 10 percent higher raises own earnings by 0.18 percent. One year of exposure to a zip code in which the predicted income percentile at 25 for children who live there is 10 percentile points higher raises own income percentile by 0.46 percentile points, which very similar to that of Chetty and Hendren (2018*a*) who find outcomes of movers to a neighborhood converge to the outcomes of residents at a rate of 4% per year of exposure.

Both of these sets of results are likely biased upwards by selection of families into particular neighborhoods near a base based on preferences for different amenities or income levels. To address this selection, in Appendix Table B.3 we instrument for residential county characteristics with either the average residential county characteristics chosen by other Army families quasi-randomly assigned to that same base (panel A) or assigned base characteristics (panel B). The first stage regressions show that these instruments strongly predict the attributes of the assigned base neighborhood. The exclusion restriction in both IV strategies is that neighborhoods affect outcomes only through the actual residential location, and that the residential choices of other families or the randomly assigned base do not have their own effect on child outcomes. Instrumenting tends to dampen the exposure effects of residential neighborhoods relative to the estimates in Appendix Table B.2, as expected if some families endogenously choose better neighborhoods than those of their assigned base.

In Panel B of Table 4, we repeat our zip code level analysis but instrument for residential zip code characteristics with the corresponding characteristic for the zip code of the randomly assigned base. The impacts of zip code BA rate on college going and of log zip code earnings on own log earnings actually increase when we move from the OLS to IV estimates. Twenty years in a zip code with a BA rate that is 10 percent higher is estimated to raise college going by 11.3 percent (versus 9.6 percentage points in the OLS estimate), and twenty years of exposure to a zip code with average AGI 10 percent higher raises own earnings at age 25 by 5.8 percent (versus 3.6 percent in the OLS). The effect of exposure to higher predicted income percentile at age 25 falls from 0.046 in the OLS to 0.017 in the IV, as expected if there is positive selection of families into higher income zip codes. Using column (3) alone,

Table 4: Effects of Residential Zip Code Characteristics

	(1) College attendance	(2) SAT (total)	(3) Income percentile at age 25	(4) Log earnings at age 25
Panel A: OLS				
Percent of Residents with Bachelors Degree	0.0482***	26.61***		
	(0.00136)	(1.094)		
Predicted Income Percentile			0.0455***	
			(0.00132)	
Log Avg. Income				0.0118***
				(0.000872)
Constant	0.437***	907.3***	43.92***	9.157***
	(0.00349)	(163.6)	(0.719)	(0.0362)
Observations	2,666,922	572,516	1,581,863	1,379,472
R-squared	0.102	0.214	0.059	0.062
Panel B: Instrumental Variables				
Percent of Residents with Bachelors Degree	0.0565***	-9.864**		
	(0.00585)	(4.749)		
Predicted Income Percentile			0.0168**	
			(0.00767)	
Log Avg. Income				0.0292***
				(0.00479)
Observations	2,190,031	452,939	1,305,517	1,081,983
R-squared	0.040	0.098	0.012	0.010
Number of FEs	22,364	14,022	17,580	16,849

Notes: This table displays estimates of neighborhood exposure effects on child outcomes based on equation 1. The neighborhood measures refer to the zip code of the location. Regression coefficients are interpreted as the causal effect of one year spent in the zip code, controlling for child year of birth, parental AFQT, education, Army start year, and race, length of assignment, and PMOS × rank × year of assignment fixed effects.

our estimates of the effect of neighborhood income on own income at age 25 are 0.017 per year and about 43% of the Chetty and Hendren (2018*a*) estimated effect. However, the log-log specification yields a much higher point estimate and we also find very large impacts of neighborhood on college attendance. Overall, our estimated impacts of neighborhood measured at the zip code level appear to be as large as the Chetty and Hendren (2018*a*) estimates of neighborhood effects measured at the commuting zone level. Our IV estimate of the impact of zip code BA rate on composite SAT score is negative and statistically significant, suggesting that the zip code level impacts we find on SAT scores are not robust.

Both the improvements in college attendance and earnings have important implications for federal subsidies for higher education, the utilization of social safety net programs, wealth accumulation, and engagement with the tax system. To understand how these improvements affect the utilization of federal subsidies for higher education, we first explore neighborhood effects on college type. In panel A of Appendix Table B.4, we find that increased college enrollments are concentrated among community colleges, four-year private and for-profit institutions. In these specifications, we use separate dummies for *ever* attending a particular institution type so significant overlap is possible. These effects come into sharper focus when we focus on the number of years spent at each institution type in panel B. The coefficients can be interpreted as the effect of 10 years of exposure in a county with a BA rate that is 10 percent higher. Living in such a county for ten years raises total years of college by 0.04 years, which consists of reducing years in four-year public institutions by 0.02 years and increasing years at two-year public, four-year private and for-profit institutions.

The corresponding increase in qualified educational expenses may come with an increased utilization of federal tax benefits for higher education. However, panel C of Appendix Table B.4 shows that changes in education tax benefits are economically small and statistically insignificant, with some shifts from refundable education credits towards the tuition and fee deduction for children. Perhaps the children of military families tap into the GI benefits earned by their parents in order to cover the additional costs of higher education, rather than turning to these other benefits.

The neighborhood exposure effects on college-going and earnings may also change tax filing behavior and utilization of social safety net programs. In Table 5 we consider the impact of the Chetty-Hendren causal income effect measure on five outcomes: a dummy for filing a tax return and average amounts claimed for the EITC, ACTC, CTC, and claiming a child dependent, each measured at age 25. The impact on tax filing is small: twenty years in a county with a one standard deviation higher Chetty-Hendren income measure (0.34) increases the probability of filing a tax return by 0.8 percentage points.[10] But exposure to a better neighborhood has more meaningful effects in reducing EITC and the refundable portion of the CTC amounts. Twenty years of exposure to a one standard deviation higher Chetty-Hendren income effect lowers EITC amounts claimed by $97, approximately 16 percent of the sample mean. In column (5), we find that exposure to better neighborhoods reduces the probability of claiming a child on one's tax returns and so these effects on EITC and CTC utilization come through reductions in eligibility through both lower probabilities of having eligible children and being above the relevant income thresholds.

Table 5: Neighborhood Exposure Effects on Tax Filing and Safety Net Utilization

	(1) Filed Return at Age 25	(2) Earned Income Tax Credit at Age 25	(3) Additional Child Tax Credit at Age 25	(4) Child Tax Credit at Age 25	(5) Has Child at Age 25
Chetty-Hendren Causal Impact on Income Percentile	0.00115* (0.000623)	-14.27*** (2.163)	-3.750*** (0.796)	0.541 (0.659)	-0.00451*** (0.000841)
Constant	0.873*** (0.00585)	947.4*** (20.50)	213.5*** (7.449)	88.61*** (4.813)	0.488*** (0.00810)
Mean of Dependent Var	0.84	584.66	147.26	66.75	Need to add
Observations	1,620,844	1,620,844	1,620,844	1,620,844	1,357,173
R-squared	0.050	0.075	0.051	0.020	0.086

Notes: This table displays estimates of neighborhood exposure effects on child outcomes based on equation 1. The neighborhood measures refer to the county of the assigned base. Regression coefficients are interpreted as the causal effect of one year spent in the county, controlling for child year of birth, parental AFQT, education, Army start year, and race, length of assignment, and PMOS × rank × year of assignment fixed effects.

[10] This is calculated as 20 years × 0.34 × 0.00115.

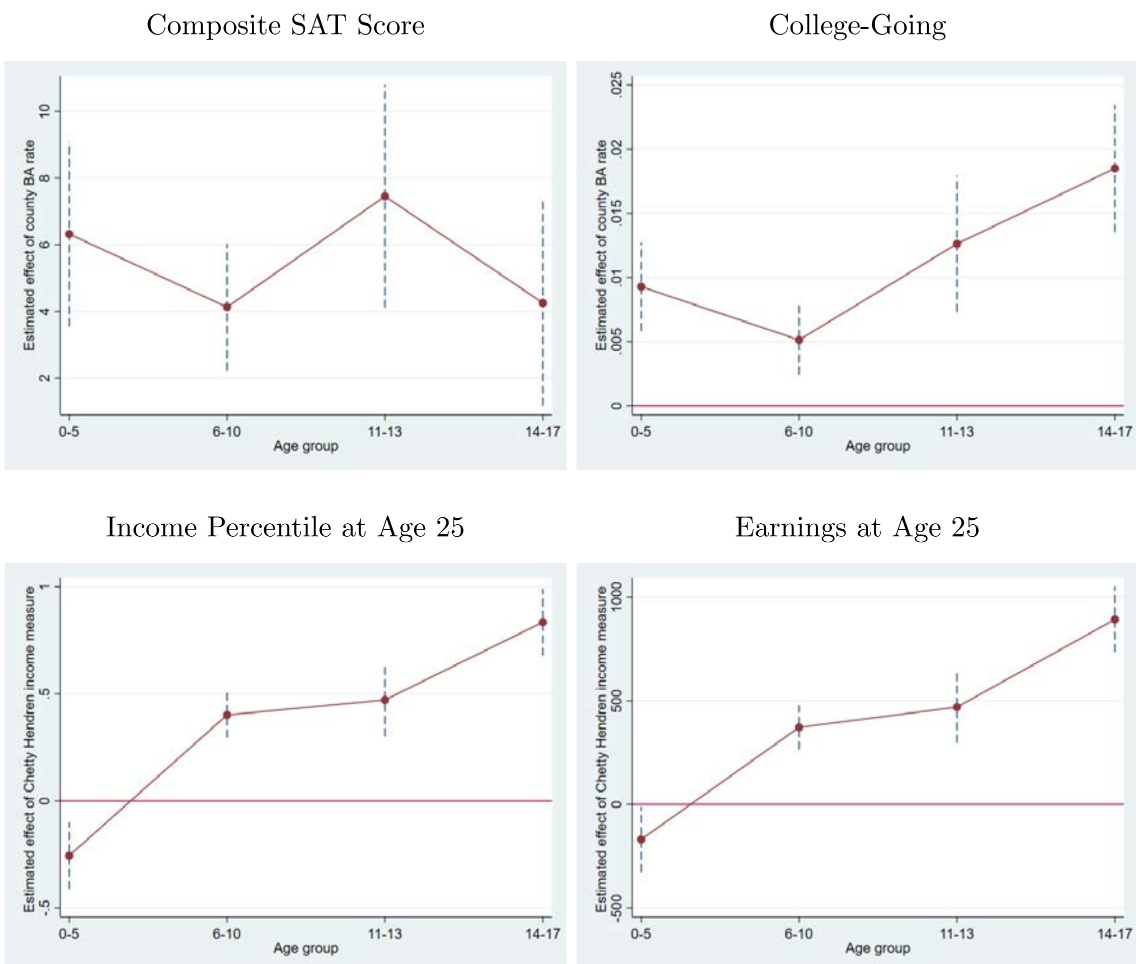
4.2 Impacts of Location by Age

We examine whether the impacts of place vary by age groups in Figure 3 using estimates from equation 3. The top row considers the effects of improvements to the percent in the assigned county with a BA on educational outcomes, and the bottom row considers the effects of upgrades in the Chetty-Hendren causal income effect of an assigned county on a measure of earnings at age 25. The corresponding point estimates are reported in Appendix Tables B.5 and B.6.

Panel (a) shows the effect of exposure to a county with a higher BA rate on composite SAT scores. The greatest neighborhood exposure effect occurs during one's middle school years, but the magnitude of this effects at different age grouping is not statistically significantly different from others. The average effects of exposure to county with a higher percent with a BA on composite SAT scores weighted by years of exposure in each group is 5.4, which implies that 20 years of exposure to a county with a 10 percentage point higher BA rate would raise composite SAT scores by $5.4 \times 20 \times 0.10 = 10.8$ SAT points. At the middle of the SAT distribution, this is approximately equal to 2 percentile points higher in the SAT distribution.

We disaggregate the effects on the composite SAT score into effect on math and verbal scores in Appendix Figure B.3, and additionally show the effects of improvements in the Chetty-Hendren causal income effect. For math SAT scores, exposure to higher BA rate counties in middle school years has the largest impact with a coefficient of 5.4 for each year of exposure to a county with a 100% higher BA rate in middle school. The weighted average effect of exposure to neighborhoods with a higher BA rate across all age groupings on math SAT scores is 3.1, implying that 20 years of exposure to a county with a 10 percentage point higher BA rate raises math SAT scores by 6.2 points. The impact on math scores is 2.2 SAT math percentile points near the middle of the distribution. Because math scores in SAT points contribute half of the SAT composite, 6.2 points on the math exam is a larger effect in percentile points of the math test than is the prior 10.8 point effect on SAT composite

Figure 3: Neighborhood Exposure Effects on Education and Earnings, by Child Age



Notes: This figure displays estimates of neighborhood exposure effects on child outcomes based on equation 3, which interacts the place effect across child development stages. The neighborhood measures refer to the county of the assigned base. In the top row, the neighborhood quality measure is the percent with a BA and in the bottom row, the neighborhood effect is the Chetty-Hendren causal income effect measure. Regression coefficients are interpreted as the causal effect of one year spent in the county, controlling for child year of birth, parental AFQT, education, Army start year, and race, length of assignment, and PMOS $\times$ rank $\times$ year of assignment fixed effects.

when expressed in percentile points. Effects of higher BA counties on verbal SAT scores show more similar effects throughout childhood. When we instead measure neighborhood quality using the Chetty-Hendren causal income effect, we find that improvements to SAT scores are concentrated in elementary and middle school, with effects during middle school being roughly 1.5 to 2 times as large.

In panel (b), we find that the impacts of place on the likelihood of attending college rise significantly with child age. Assignment to a county with a 10 percentage point higher BA rate raises own college attendance by 0.09 percentage points per year of exposure during pre-school ages, and the effect grows to more than twice as large during high school at 0.19 percentage points per year of exposure.

Table 6 analyzes neighborhood effects on years of college enrollment. Column (1) is for overall years of college while the remaining columns divide these years of college at different institution types. The table reveals several striking facts. The impacts of neighborhood on years of college are larger at older child ages. One year in a county with a 100% higher BA rate raises years of college by 0.10 years if that exposure comes during high school, but only 0.03 years if it is instead experienced during pre-school. Four years of exposure to a county with a 10 percentage point higher BA rate during elementary school raises years of college 0.012 years, but a similar exposure during high school raises years of college by 0.04 years. Looking across institution types, about half of the effects on years of college come via increases in years at two year public colleges. The other half of the total impacts on years of college are roughly split between increasing years at four year public and four year private non-profit institutions.

A similar pattern of effects growing with age at exposure emerges when we turn to the effects on earnings at age 25 in panels (c) and (d). Improvements in the county of exposure during pre-school does not improve earnings during young adulthood, and the point estimates suggest a negative relationship during these early years. But by elementary school, the effects of place become positive and grow with age. One year of exposure to a

Table 6: Neighborhood Exposure Effects on Years of College Attendance, by College Type

	(1) Any College	(2) Public 2 Year	(3) Public 4 Year	(4) Private 4 Year	(5) Private For Profit	(6) Other 2 Year
County BA Rate Ages 0-5	0.0309***	0.00836	0.0171**	0.0117**	-0.00381**	-0.00247***
	(0.00878)	(0.00530)	(0.00730)	(0.00470)	(0.00190)	(0.000958)
County BA Rate Ages 6-10	0.0408***	0.0231***	0.0112**	0.00385	0.00334**	-0.000737
	(0.00688)	(0.00415)	(0.00572)	(0.00368)	(0.00149)	(0.000751)
County BA Rate Ages 11-13	0.0616***	0.0470***	0.00787	0.0147**	-0.00914***	0.00121
	(0.0134)	(0.00811)	(0.0112)	(0.00719)	(0.00290)	(0.00146)
County BA Rate Ages 14-17	0.104***	0.0519***	0.0232**	0.0244***	0.00697**	-0.00273**
	(0.0125)	(0.00757)	(0.0104)	(0.00672)	(0.00271)	(0.00137)
Constant	2.385***	0.644***	1.154***	0.430***	0.109***	0.0473***
	(0.0227)	(0.0137)	(0.0189)	(0.0122)	(0.00491)	(0.00248)
Observations	679,616	679,616	679,616	679,616	679,616	679,616
R-squared	0.198	0.095	0.137	0.105	0.089	0.076

Notes: This table displays estimates of neighborhood exposure effects on child outcomes based on equation 3, which interacts the place effect across child development stages. The neighborhood measures refer to the county of the assigned base. Regression coefficients are interpreted as the causal effect of one year spent in the county, controlling for child year of birth, parental AFQT, education, Army start year, and race, length of assignment, and PMOS $\times$ rank $\times$ year of assignment fixed effects.

county with a 1 point higher Chetty-Hendren income effect during high school raises a child's own income percentile by 0.84 – roughly twice the effect observed for a year on exposure to a similar county during elementary school.

4.3 Nonlinearities and Complementarities in Exposure

To examine whether the effects of place are linear in years of exposure, we construct binned scatter plots of our outcome variables and years of exposure to a "good" neighborhood. To simplify the interpretation, we define a good place as those with a characteristic that is a greater than the 75th percentile of that characteristic across assignments.[11] Each figure plots years of exposure to a good neighborhood on the x-axis and a child outcome that has been residualized for family demographic characteristics and PMOS $\times$ rank $\times$ assignment year fixed effects on the y-axis. In Appendix Figure B.1, we show similar figures when we define better neighborhoods as being above the 25th percentile of the sample.

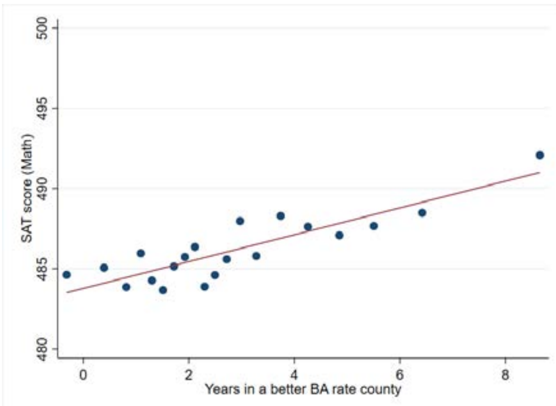
In Figure 4, we examine these relationships between exposure to counties in with a better BA attainment rate (top row) or a Chetty-Hendren causal income effect (bottom row) on SAT scores. The relationships are quite linear in years of exposure to better counties. Ten years of exposure to locations with better BA rates roughly translates to a 12 point increase in math SAT score and a 7 point increase in verbal SAT score, and ten years of exposure to locations with better Chetty-Hendren causal income effects translates to a 7.5 point increase in math SAT score and a 7.5 increase in verbal SAT score. Figure 5 presents similar evidence that effects of place on college-going and earnings are linear in years of exposure to a good neighborhood. Ten years in an above 75th percentile county raises college going by 2.6 percentage points and earnings by 2 percentile points.

Together, these figures imply that improvements in outcomes are roughly linear in years of exposure to a good place. This linearity does not contradict our earlier results that age at exposure matters; the quasi-random assignment of families to bases mean that there is little

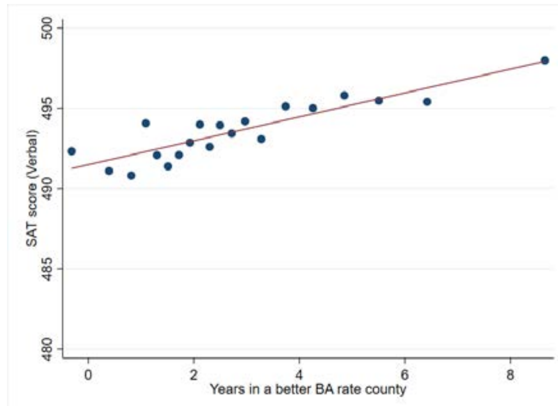
[11] The 75th percentiles are 27.4 percent for county BA and -0.08 for the Chetty Causal effect.

Figure 4: Correlation Between SAT Scores and Exposure to Better Neighborhoods

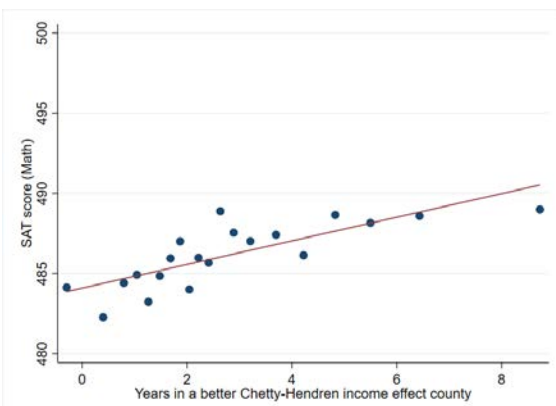
Math Score and County BA Rate



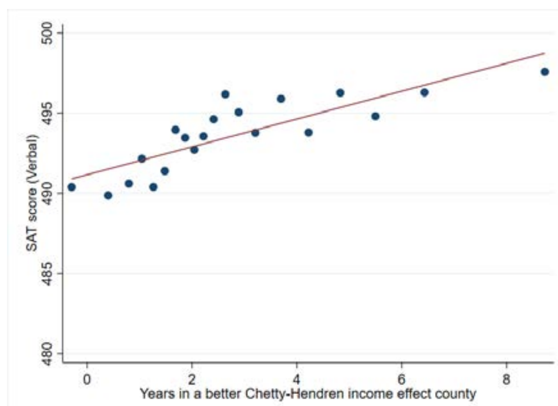
Verbal Score and County BA Rate



Math Score and Chetty-Hendren Income Effect



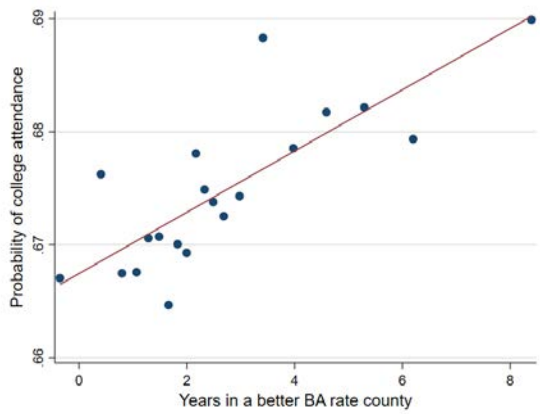
Verbal Score and Chetty-Hendren Income Effect



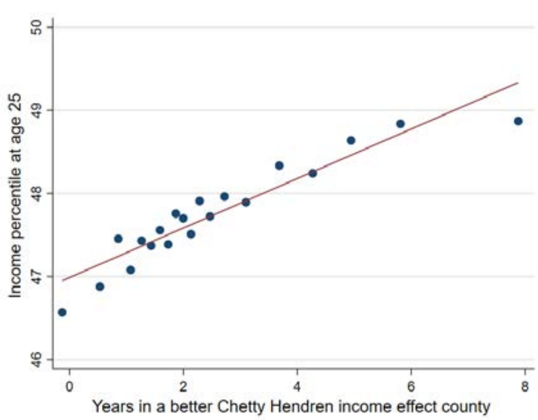
Notes: In panels a and b the x-axis bins observations by years of exposure to a base county with an above 75th percentile BA rate. In panels c and d the x-axis bins observations by years of exposure to a base county with an above 75th percentile Chetty-Hendren causal income effect. The y-axis in panels a and c is the child's math SAT score averaged within the bin. The y-axis in panels b and d is the child's verbal SAT score averaged within the bin. SAT scores are residualized for parent demographics, parents' Professional Military Occupational Specialty (PMOS), rank, and year of assignment to the base.

Figure 5: Correlation Between Income and College Attendance and Exposure to Better Neighborhoods

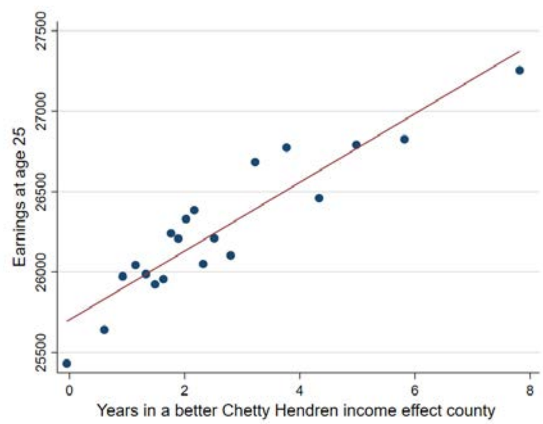
(a) College Attendance



(b) Income Percentile at Age 25



(c) Earnings at Age 25



Notes: In panel a the x-axis bins observations by years of exposure to a base county with an above 75th percentile BA rate. In panels c and d the x-axis bins observations by years of exposure to a base county with an above 75th percentile Chetty-Hendren causal income effect. The y-axes are the probability of attending college (panel a), income percentile at age 25 (panel b), and earnings at age 25 (panel c), residualized for parent demographics, parents' Professional Military Occupational Specialty (PMOS), rank, and year of assignment to the base.

or no correlation between child age and the number of years spent in a good county.

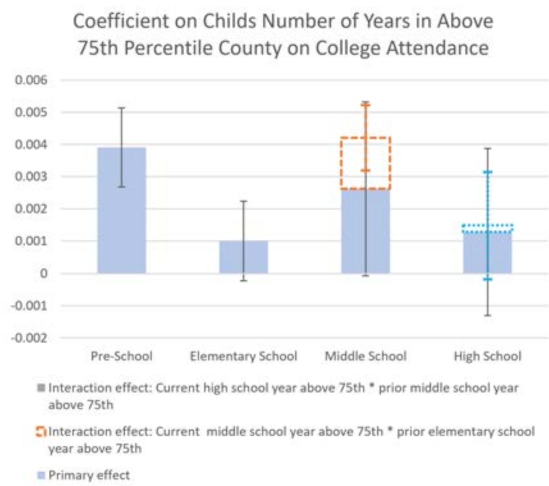
Next, we ask whether there are important interactions of place effects across ages. For example, does an additional year in a good place during middle school increase the effect of exposure to a good place during high school? To test this, we augment equation 3 in two ways. First, our key right-hand side variable is the number of years assigned to a base in a county with an above 75th percentile measure of some characteristic. Second, we include interactions of these exposure variables for different age groups. To simplify the specification we begin by only including the interaction terms between elementary and middle school and middle school and high school, but the fully interacted model is shown in Appendix Table B.7.

Figure 6 shows results for college-going, income percentile at age 25, earnings at age 25. The solid blue bars represent the main effect of an additional year in a good county. The orange open bars (larger dashed outline) represent the incremental effect of an additional year in a good county during middle school for each year in elementary school that was spent in a good county, and the light blue open bars (smalled dashed outline) represent the incremental effect of an additional year in a good county during high school for each year in middle school that was spent in a good county. For the two earnings measures, the main effects remain large and statistically significant, particularly at high school ages. In contrast, the interaction effects are small, and generally statistically insignificant. For earnings at age 25, an additional year in a good county during high school raises earnings by almost \$400, but the additional benefit of a high school year in such a place only increases earnings modestly (by about \$20) for each year of middle school that was also in a good place. And the incremental effect of being in a good neighborhood during high school is negative for each good neighborhood exposure in middle school.

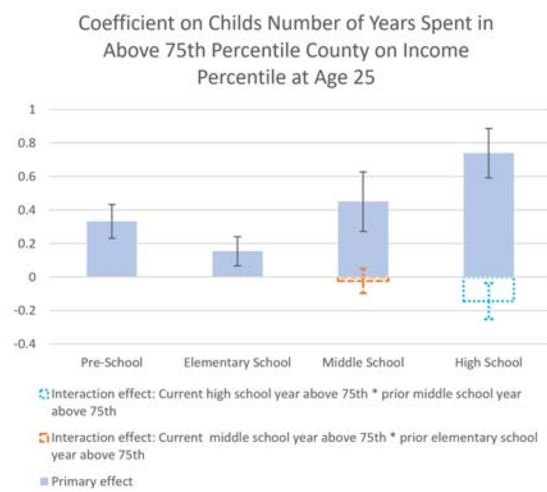
We find a more nuanced story for the interaction effects on college attendance. An additional year assigned to a good place, based on the county BA rate, during middle school raises college attendance by 0.25 percentage points. There is also a 0.15 percentage points

Figure 6: Main Effect of Number Years in a Better County and Interaction Effects of Current Year and Prior Years in Better Counties

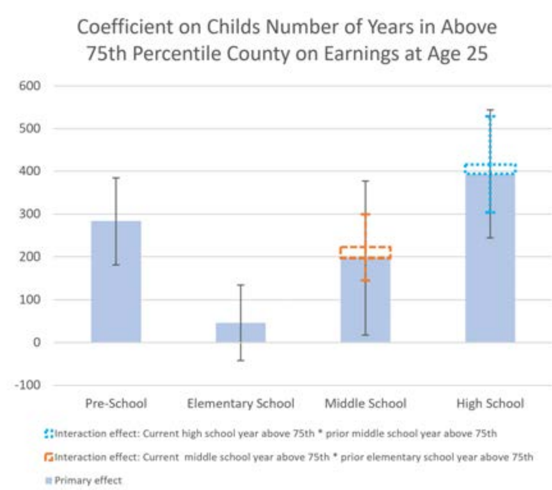
(a) College Attendance



(b) Own Income Percentile at Age 25



(c) Own Earnings at Age 25



Notes: This figure shows the main effect of exposure to a "better" neighborhood in each developmental stage in blue bars, and the interaction effect of exposure to a "better" neighborhood in elementary and middle school or middle school and high school. The regressions control for parent demographics, parents' Professional Military Occupational Specialty (PMOS), rank, and year of assignment to the base.

increase in college attendance for each year in both elementary and middle school that were spent in a top 75th percentile county. However, the interaction effects for middle and high school are small and statistically insignificant.

Overall, we find little evidence that interaction effects of place across child ages are important in determining outcomes during young adulthood. This is consistent with our finding that main neighborhood effects are linear in years of exposure, and further suggest that these exposures add to, but do not compound, if a child experiences a beneficial location across different developmental periods.

4.4 Heterogeneity by Sex and Race

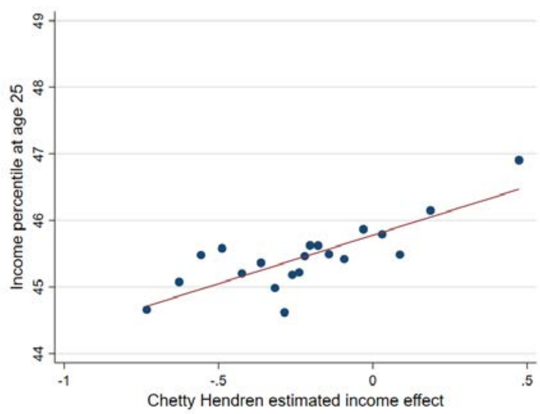
A natural question to ask is whether the effects of location differ by child characteristics. We address this using several tests for whether neighborhood exposure effects differ by child sex or race. We begin by presenting scatter plots of relationships similar to those in Section 3.2 in Figure 7 and estimates from equation 1 in Appendix Table B.8 for different subgroups.

Figure 7 shows the impact of improvements to the Chetty-Hendren causal income effect of a county to which a child is exposed on a child's own income percentile at age 25. Interestingly, panels (a) and (b) show that these effects are quite similar for Black and white children. The slopes are nearly identical, but white children have income percentiles which are on average about 2 percentile points higher than Black children. The estimates in Appendix Table B.8 also reveal a similarity in neighborhood effects on earnings by race. But these regressions also show that the effects on the probability of attending college are over three times larger for Black children than white children, and the effects are only statistically significant for Black children.

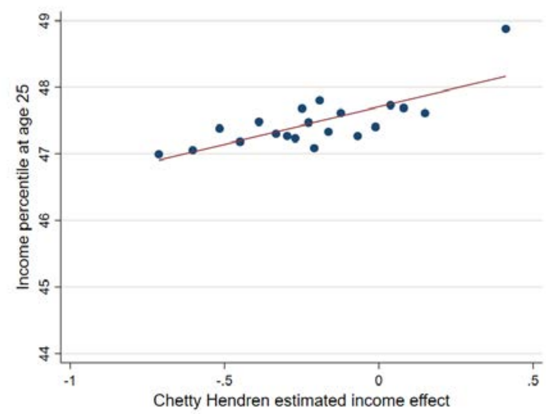
Considering differences in neighborhood effects by sex reveals more significant differences. Panels (c) and (d) of Figure 7 show that the transmission of neighborhood effects begins at a lower base and has a steeper slope for female children relative to male children. Among children who are exposed to a county with a Chetty-Hendren causal income effect of -0.5, at

Figure 7: Neighborhood Exposure Effects on Income Percentile, by Race and Sex

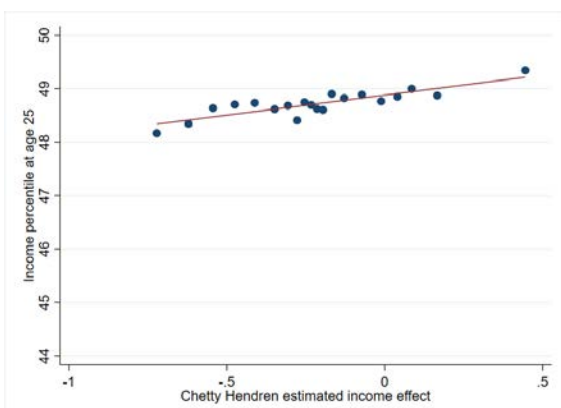
Black Children



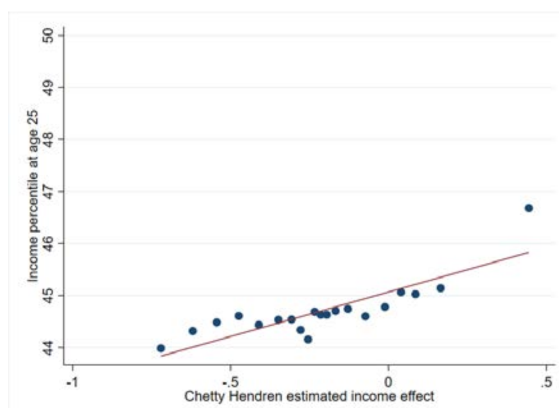
White Children



Male Children



Female Children



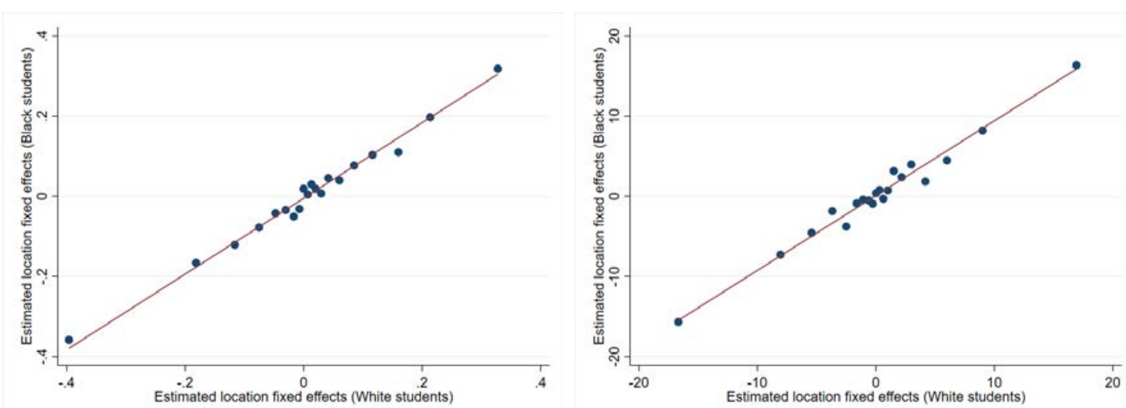
Notes: The x axis bins observations by ventiles of Chetty et. al causal effect of place. The y axis is the child's income percentile at age 25 averaged within the bin. The income percentile is residualized for parent demographics, parents' Professional Military Occupational Specialty (PMOS), rank, and year of assignment to the base. The samples are limited to observations where military parent is black and where military parent is white.

age 25, men are on average are roughly 4 percentile points higher in the earnings distributions than women. But for an exposure to a county with a Chetty-Hendren causal income effect of 0.5, the average gap between men and women closes to 3 percentile points. Estimates from equation 1 similarly reveal a larger effect of place on women's earnings at age 25 than men that is almost 3.5 times as large. But while both are statistically significant, exposures to better neighborhoods has an effect on college-going that is almost twice as potent for men than women. However, when we estimate impacts of place in our one observation per child specification with impacts varying by age (see below) these stark differences between effects for men and women disappear.

Next, we re-estimate our base fixed effects that measure the effect of random assignment on outcomes, but now do so separately by race. Figure 8 presents binned scatter plots of the base fixed effects for Black children relative to the base fixed effects for white children for college attendance (panel A) and income percentiles at age 25 (panel B). These plots show a tight fit around the 45 degree line, implying that a given base has a nearly identical effect on these outcomes for Black and for white children.

Figure 8: Correlation of Estimated Base Fixed Effects across Race

(a) College-Going (b) Income



Notes: This figure presents a scatter plot of estimated base fixed effects from a regression of a child's earning percentile at age 25 on base fixed effects, residualized for parent demographics, parents' Professional Military Occupational Specialty (PMOS), rank, and year of assignment to the base. The x-axis bins observations by ventiles of base fixed effects from regressions including only white children and the y-axis bins observations by ventiles of base fixed effects from regressions including only Black children.

Finally, we estimate exposure effects by age separately for four subgroups: male, female, white, and Black children. Columns 2–5 of Appendix Tables B.6 present heterogeneous estimated exposure effects using equation 3. For college-going, the magnitudes of coefficients and patterns by age groups are remarkably similar across gender and race. For example, the impact of a year of exposure to a county with a 10 percentage points higher BA rate in middle school is 0.001 for white children and 0.0013 for Black children. The corresponding effects for a year of exposure during high school are 0.0017 for white children and 0.0024 for black children. None of the differences across gender or race are statistically significant.

Coefficients are again quite similar across gender and race when considering earnings outcomes. One modest exception that neighborhood effects during high school are larger for white children than Black children (1.04 versus 0.63), although the effects across race during elementary and middle school are quite similar. We do not have an explanation for the anomalous finding that there is a negative correlation between income percentile at age 25 and the Chetty-Hendren income effect measure for the county during pre-school.

The striking similarity in estimated neighborhood effects across groups has potentially important implications for understanding how the effects of place might influence group-based inequalities. In our data, the white-Black gap in percentile earnings at age 25 is 3.7 percentile points. Black children in the U.S. live in counties that have on average a Chetty-Hendren income impact on income percentile that is -0.17 worse per year of exposure. Our estimated coefficient on the Chetty-Hendren income measure of 0.32 suggests that outcomes for Black children in Army families would have been -1.1 (=-0.17×0.32×20) percentiles worse after 20 years of differential exposure, which implies that random assignment to place cuts the black white gap in earnings from a hypothetical 4.8 percent down to the observed 3.7 percent or a 23% reduction. Consider a similar back-of-the-envelope using the sample in Chetty and Hendren (2018*b*). Their data show a 12 percentile point Black-white gap and a coefficient of 1.0 on their measure for one year of exposure, so 20 years of exposure to the existing Black-white differential in place impact yields 3.4 percentile points, or about 28 percent of the overall Black-white earnings gap.

5 Conclusion

The quality of a child's environment during their formative years leaves lasting effects on their development and long-run outcomes. By leveraging the assignment process of the U.S. Army, we are able to exploit the many mini-lotteries that military children face for the types of neighborhoods to which they are exposed, and how these neighborhood characteristics shape their developmental trajectories. Consistent with recent work (Chetty and Hendren, 2018*a*,*b*; Chetty, Hendren and Katz, 2016; Chyn, 2018) we find potent effects of places on SAT scores, children's college attendance, years of college and earnings. These impacts are linear in years of exposure to a location. However, that does not mean that impacts are constant across child ages. In fact, location effects on college-going and young adult earnings are significantly larger during high school than during elementary and middle school. We do not find evidence for large interactions of location quality across age groups. This could be surprising as we think about complex models of human capital formation (Heckman, 2006; Cunha and Heckman, 2007) and the possibilities for complementarities across different ages.

The effects of location on earnings flow through to impact engagements with the tax system and take-up of safety net programs. Exposure to better neighborhoods lead to modest increases the probability that children will file a tax return in their mid-20s. Moreover, boosts to earnings also facilitate reductions in the reliance on safety net programs. We find economically meaningful reductions in both the EITC and the refundable portion of the CTC.

Our estimated impacts of county neighborhood quality are roughly a third to a half the magnitude of the estimated causal effects of place found in Chetty and Hendren (2018*b*) which measures neighborhood at the commuting zone level. However, when measured at the zip code level we find neighborhood exposure effects that are as large as those of Chetty and

Hendren (2018*a*). A plausible explanation for the differences between our baseline results and the existing literature is that the children of Army personnel are not as deeply ensconced in the community as in the earlier studies of civilian movers. Indeed, we find that impacts are much smaller when we limit the sample to families that live on the Army base itself, even though many of those children who live on Army bases attend local schools. Among our more striking results is that the same locations appear to impart similar benefits to children across race and gender. One implication of this result is that the Army's random assignment of families to bases might reduce Black-white gaps in child outcomes because, on average, all children are exposed to similar places. Overall, our results support that location as a child is a powerful determinant of adult outcomes and that differences in location characteristics are a significant contributor to income and educational inequality.

References

Abdulkadiroğlu, Atila, Joshua D Angrist, Susan M Dynarski, Thomas J Kane, and Parag A Pathak. 2011. "Accountability and flexibility in public schools: Evidence from Boston's charters and pilots." *The Quarterly Journal of Economics*, 126(2): 699–748.

Alexander, Karl, Doris Entwisle, and Susan Dauber. 1996. "Children in Motion: School Transfer and Elementary School Performance." *ILR Review*, 90(1): 3–12.

Altonji, Joseph G, Todd E Elder, and Christopher R Taber. 2005. "Selection on observed and unobserved variables: Assessing the effectiveness of Catholic schools." *Journal of political economy*, 113(1): 151–184.

Angrist, Joshua, and John Johnson. 2000. "Effects of Work-Related Absences on Families: Evidence from the Gulf War." *ILR Review*, 54: 41–58.

Audette, Robert, Robert Algozzine, and Michele Warden. 1993. "Mobility and School Achievement." *Psychological Reports*, 72: 701–702.

Bailey, Zinzi D, Nancy Krieger, Madina Agénor, Jasmine Graves, Natalia Linos, and Mary T Bassett. 2017. "Structural racism and health inequities in the USA: evidence and interventions." *The lancet*, 389(10077): 1453–1463.

Bergman, Peter, Raj Chetty, Stefanie DeLuca, Nathaniel Hendren, Lawrence F Katz, and Christopher Palmer. 2019. "Creating moves to opportunity: Experimental evidence on barriers to neighborhood choice." National Bureau of Economic Research.

Bertrand, Marianne, Erzo F. P. Luttmer, and Sendhil Mullainathan. 2000. "Network Effects and Welfare Cultures*." *The Quarterly Journal of Economics*, 115(3): 1019–1055.

Billings, Stephen B, David J Deming, and Jonah Rockoff. 2014. "School segregation, educational attainment, and crime: Evidence from the end of busing in Charlotte-Mecklenburg." *The Quarterly journal of economics*, 129(1): 435–476.

Björklund, Anders, Mikael Lindahl, and Erik Plug. 2006. "The Origins of Intergenerational Associations: Lessons from Swedish Adoption Data." *The Quarterly Journal of Economics*, 121(3): 999–1028.

Bridglall, Beatrice L, and Edmund W Gordon. 2003. "Raising Minority Academic Achievement: The Department of Defense Model. Pedagogical Inquiry and Praxis." *Pedagogical Inquiry and Praxis.*

Brooks-Gunn, Jeanne, Greg J Duncan, Pamela Kato Klebanov, and Naomi Sealand. 1993. "Do neighborhoods influence child and adolescent development?" *American journal of sociology*, 99(2): 353–395.

Bruhn, Jesse M, Kyle Greenberg, Matthew Gudgeon, Evan K Rose, and Yotam Shem-Tov. 2022. "The Effects of Combat Deployments on Veterans' Outcomes." National Bureau of Economic Research.

Carrell, Scott, and Jonathan Zinman. 2014. "In harm's way? Payday loan access and military personnel performance." *The Review of Financial Studies*, 27(9): 2805–2840.

Carter, Susan, and William Skimmyhorn. 2017. "Much Ado About Nothing? New Evidence on the Effects of Payday Lending on Military Members." *Review of Economics and Statistics*, 99: 606–621.

Chetty, Raj, and Nathaniel Hendren. 2018*a*. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." *Quarterly Journal of Economics*, 133: 1107–1162.

Chetty, Raj, and Nathaniel Hendren. 2018*b*. "The Impacts of Neighborhoods on Intergenerational Mobility II: County-Level Estimates." *Quarterly Journal of Economics*, 133: 1163–1228.

Chetty, Raj, John Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan. 2020. "Mobility report cards: Income segragation and intergenerational mobility across colleges in the United States." *Quarterly Journal of Economics*, 135: 1567–1633.

Chetty, Raj, Matthew O Jackson, Theresa Kuchler, Johannes Stroebel, Nathaniel Hendren, Robert B Fluegge, Sara Gong, Federico Gonzalez, Armelle Grondin, Matthew Jacob, et al. 2022. "Social capital I: measurement and associations with economic mobility." *Nature*, 608(7921): 108–121.

Chetty, Raj, Nathaniel Hendren, and Lawrence Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence on the Moving to Opportunity Experiment." *American Economic Review*, 106: 855–902.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States." *Quarterly Journal of Economics*, 129: 1107–1162.

Chyn, Eric. 2018. "Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children." *American Economic Review*, 108(10): 3028–3056.

Clampet-Lundquist, Susan, and Douglas S Massey. 2008. "Neighborhood effects on economic self-sufficiency: A reconsideration of the Moving to Opportunity experiment." *American Journal of Sociology*, 114(1): 107–143.

Coleman, James S. 1968. "Equality of Educational Opportunity." *Equity & Excellence in Education*, 6(5): 19–28.

Coleman, James S. 1988. "Social capital in the creation of human capital." *American journal of sociology*, 94: S95–S120.

Cunha, Flavio, and James Heckman. 2007. "The technology of skill formation." *American economic review*, 97(2): 31–47.

Duncan, Greg J, Jeanne Brooks-Gunn, and Pamela Kato Klebanov. 1994. "Economic deprivation and early childhood development." *Child development*, 65(2): 296–318.

Fahle, Erin M, Sean F Reardon, Benjamin R Shear, Andrew D Ho, Jie Min, Demetra Kalogrides, EM Fahle, SF Reardon, BR Shear, AD Ho, et al. 2024. "Stanford Education Data Archive Technical Documentation SEDA2023 January 2024."

Greenberg, Kyle, Matthew Gudgeon, Adam Isen, Corbin Miller, and Richard Patterson. 2022. "Army service in the all-volunteer era." *The Quarterly Journal of Economics*, 137(4): 2363–2418.

Hanushek, Eric, John Kain, and Steven Rivkin. 2004. "Disruption versus Tiebout Improvement: The Costs and Benefits of Switching Schools." *Journal of Public Economics*, 88: 1721–1746.

Heckman, James J. 2006. "Skill formation and the economics of investing in disadvantaged children." *Science*, 312(5782): 1900–1902.

Heckman, James J, and Dimitriy V Masterov. 2007. "The productivity argument for investing in young children."

Jacob, Brian. 2004. "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago." *American Economic Review*, 94(1): 233–258.

Jencks, Christopher, Susan E Mayer, et al. 1990. "The social consequences of growing up in a poor neighborhood." *Inner-city poverty in the United States*, 111: 186.

Katz, Lawrence, Jeffrey Kling, and Jeffrey Liebman. 2001. "Moving to Opportunity in Boston: Early Results from a Randomized Mobility Experiment." *Quarterly Journal of Economics*, 116(2): 607–654.

Leventhal, Tama, and Jeanne Brooks-Gunn. 2000. "The neighborhoods they live in: the effects of neighborhood residence on child and adolescent outcomes." *Psychological bulletin*, 126(2): 309.

Lleras-Muney, Adriana. 2010*a*. "The needs of the Army: using compulsory relocation in the military to estimate the effect of air pollutants on children's health." *Journal of Human Resources*, 45(3): 549–590.

Lleras-Muney, Adriana. 2010*b*. "The Needs of the Army: Using Compulsory Relocation in the Military to Estimate the Effect of Environmental Pollutants on Children's Health." *Journal of Human Resources*, 35(3): 549–590.

Ludwig, Jens, Greg J Duncan, and Paul Hirschfield. 2001. "Urban poverty and juvenile crime: Evidence from a randomized housing-mobility experiment." *The Quarterly Journal of Economics*, 116(2): 655–679.

Ludwig, Jens, Greg J Duncan, Lisa A Gennetian, Lawrence F Katz, Ronald C Kessler, Jeffrey R Kling, and Lisa Sanbonmatsu. 2012. "Neighborhood effects on the long-term well-being of low-income adults." *Science*, 337(6101): 1505–1510.

Lyle, David. 2006. "Using Military Deployments and Job Assignments to Estimate the Effect of Parental Absences and Household Relocations on Children's Academic Achievement." *Journal of Labor Economics*, 24(2): 319–350.

Moro, Esteban, Dan Calacci, Xiaowen Dong, and Alex Pentland. 2021. "Mobility patterns are associated with experienced income segregation in large US cities." *Nature communications*, 12(1): 4633.

Murphy, Francis X. 2017. "Essays on Human Capital, the Labor Market, and Social Interaction." PhD diss. University of Virginia.

National Academies of Sciences, Engineering, Medicine, et al. 2019. "The promise of adolescence: Realizing opportunity for all youth."

Oreopoulos, Philip. 2003. "The Long-Run Consequences of Living in a Poor Neighborhood." *Quarterly Journal of Economics*, 118(4): 1533–1575.

Reardon, Sean F. 2016. "School district socioeconomic status, race, and academic achievement." *Stanford Center for Educational Policy Analysis.*

Rosenbaum, James E. 1995. "Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program." *Housing Policy Debate*, 6(1): 231–269.

Rosenbaum, James E, Stefanie DeLuca, Shazia R Miller, and Kevin Roy. 1999. "Pathways into work: Short-and long-term effects of personal and institutional ties." *Sociology of education*, 179–196.

Sabia, Joseph, and William Skimmyhorn. 2023. "How do combat deployments affect veterans' health and labor market outcomes? Evidence from the U.S. Army."

Sacerdote, Bruce. 2007. "How large are the effects from changes in family environment? A study of Korean American adoptees." *The Quarterly Journal of Economics*, 122(1): 119–157.

Sampson, Robert J. 2008. "Moving to inequality: Neighborhood effects and experiments meet social structure." *American journal of sociology*, 114(1): 189–231.

Sampson, Robert J, Jeffrey D Morenoff, and Thomas Gannon-Rowley. 2002. "Assessing 'Neighborhood Effects': Social Processes and New Directions in Research." *Annual Review of Sociology*, 28: 443–478.

Sharkey, Patrick, and Jacob Faber. 2014. "Where, when, why, and for whom do residential contexts matter? Moving away from the dichotomous understanding of neighborhood effects." *Annual Review of Sociology*, 559–579.

Shoag, Daniel, and Nicholas Carollo. 2016. "The Causal Effect of Place: Evidence from Japanese-American Internment."

Smrekar, Claire E, and Debra E Owens. 2003. "High performance of minority students in DoDEA schools: Lessons for America's public schools." *Developments in School Finance: 2001–02*, 181.

Wilson, William Julius. 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy.* University of Chicago Press.

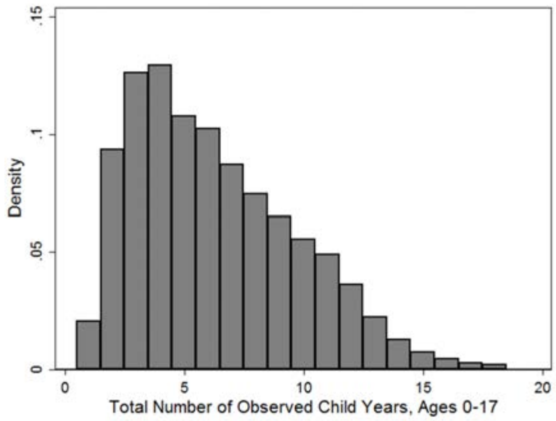
Wilson, William Julius. 1996. "1997,"When Work Disappears,"." *Political Science Quarterly*, 111(4): 567–595.

Wilson, William Julius. 2012. *The truly disadvantaged: The inner city, the underclass, and public policy.* University of Chicago Press.

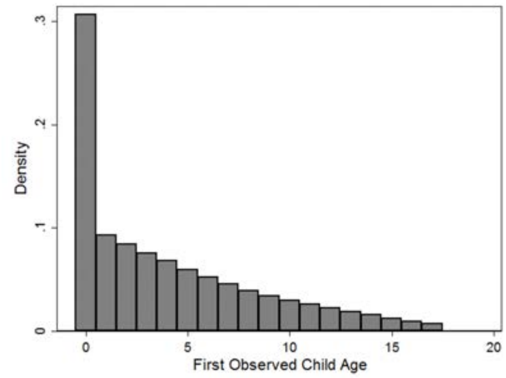
A Additional Background on Army Families and their Locations

Figure A.1: Characteristics of Children in Army Families

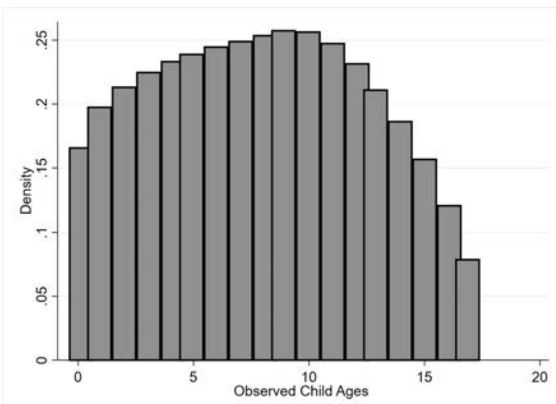
(a) Number of Years Observed



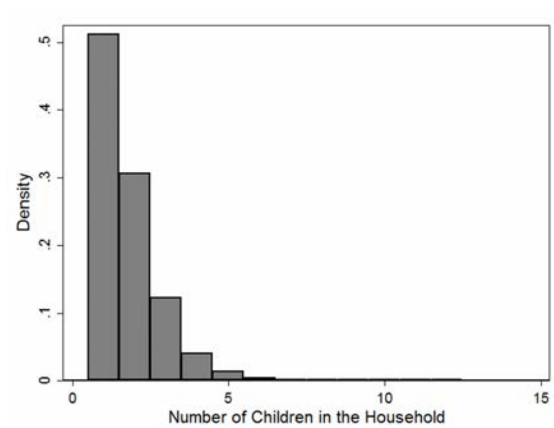
(b) Age at Which Child is First Observed



(c) Distribution of Observed Child Ages



(d) Number of Children in Army Families



Notes: This figure presents histograms on the total number of observed years, the first age observed, and the ages observed per child between ages 0-17, and the total number of children per military household observed. The sample consists of 800,000 children of enlisted members in the years 1990-2017.

Table A.1: Top 20 Primary Military Occupational Specialty (PMOS) Codes

Occupation	Count	Percent
Infantryman	391,895	6.73
Cannon Crewmember	158,677	2.72
Mechanized Infantryman	100,889	1.73
Combat Engineer	89,548	1.54
Fire Support Specialist	65,303	1.12
Indirect Fire Infantryman (Mortarman)	62,613	1.07
Multiple Launch Rocket System (MLRS) Crewmember	40,996	0.7
Infantry Senior Sergeant	27,815	0.48
Heavy Anti-Armor Weapons Infantryman	25,325	0.43
Military Occupational Specialty Inactive	20,313	0.35
Non-Appropriated Fund Instrumental Instructor	19,022	0.33
Cannon Fire Direction Specialist	16,379	0.28
Automated Logistical Specialist	12,223	0.21
Bridge Crewmember	9,670	0.17
Tactical Automated Fire Control Systems Specialist	8,412	0.14
Combat Engineering Senior Sergeant	8,144	0.14
Engineer Senior Sergeant	5,798	0.1
Horizontal Construction Engineer	4,068	0.07
Band Instrument Repairer	2,510	0.04
Healthcare Specialist (Combat Medic)	2,106	0.04

Notes: The table shows the top 20 military occupation codes observed in the estimation sample. The sample consists of 800,000 children of enlisted members in the years 1990-2017.

Table A.2: Top 20 Base Assignments

Base Name	Number of Assignments
Ft. Hood, TX	379,714
Ft. Bragg, NC	339,754
Ft. Campbell, KY	222,503
JBLM Lewis, WA	180,055
Ft. Carson, CO	161,585
Ft. Stewart, GA	144,379
Ft. Benning, GA	134,624
Ft. Bliss, TX	128,168
Ft. Riley, KS	121,693
Schofield Brks., HI	118,761
Ft. Drum, NY	112,547
Ft. Sill, OK	109,678
Ft. Polk, LA	97,886
Ft. Knox, KY	91,983
Ft. Sam Houston, TX	72,077
Ft. Gordon, GA	69,541
Ft. Leonard Wood, MO	62,776
Ft. Rucker, AL	60,198
Ft. Irwin, CA	51,404
Ft. Jackson, SC	50,644
Ft. Eustis, VA	50,150

Notes: The table shows the top 20 base assignments observed in the estimation sample. The sample consists of 800,000 children of enlisted members in the years 1990-2017.

Table A.3: Correlation Between Base Characteristics and Family Characteristics, Ages 0–5

	(1) County BA Rate	(2) Est Impact of County on Income Percentile	(3) Est Impact of County on College Attendance	(4) Free Lunch Participation In County	(5) County Population	(6) Standardized Math Scores In County	(7) Percentage Black Population In County	(8) Average County AGI
Panel A: No Additional Controls								
Constant	0.247***	-0.211***	-0.000719***	0.220***	10,810***	0.576***	0.175***	28.99***
	(0)	(0)	(0)	(0)	(4.38e-10)	(0)	(0)	(0)
Observations	757,623	757,623	757,623	757,623	757,623	757,623	757,623	757,623
R-squared	0.092	0.078	0.091	0.068	0.064	0.049	0.064	0.127
Panel B: With Controls family characteristics								
Child Male	0.000184	0.000536	1.48e-05	-0.000113	-5.751	8.93e-05	-3.32e-05	0.0205
	(0.000147)	(0.000465)	(1.10e-05)	(9.95e-05)	(12.45)	(6.55e-05)	(9.67e-05)	(0.0220)
Black	0.000534*	-0.00304	-6.12e-05*	0.000103	-104.5**	-0.000220	0.00452*	-0.111
	(0.000281)	(0.00187)	(3.71e-05)	(0.000131)	(46.17)	(0.000146)	(0.00248)	(0.0729)
Hispanic	-0.000981*	-0.00631*	1.25e-05	-0.000816*	2.848	0.000270	-0.000121	-0.138*
	(0.000595)	(0.00355)	(1.85e-05)	(0.000464)	(27.45)	(0.000168)	(0.000215)	(0.0826)
Other Race	0.000654	-0.000429	-1.32e-05	-0.000165	72.81	-5.00e-06	-0.000943*	-0.113
	(0.000449)	(0.000981)	(1.96e-05)	(0.000184)	(44.56)	(0.000103)	(0.000501)	(0.0722)
Parent AFQT Score	2.96e-05	5.64e-05	3.33e-07	6.51e-06	1.511**	-1.22e-06	2.01e-06	0.00561
	(1.95e-05)	(3.92e-05)	(3.60e-07)	(4.32e-06)	(0.714)	(1.45e-06)	(3.18e-06)	(0.00373)
Parent Completed GED	0.00762**	0.00846**	5.66e-05	-0.00594**	-526.7**	0.00230**	-0.00238**	1.435**
	(0.00364)	(0.00422)	(4.52e-05)	(0.00254)	(254.7)	(0.000993)	(0.00110)	(0.665)
Parent Graduated High School	0.00307*	0.00600*	3.09e-05	-0.000489*	33.74	0.000285*	-0.000470	0.382*
	(0.00185)	(0.00356)	(2.40e-05)	(0.000293)	(28.63)	(0.000157)	(0.000290)	(0.231)
Parent has Associates	0.00391	0.00881	2.04e-05	-0.000139	-261.2*	0.000163	-0.00101*	0.517
	(0.00241)	(0.00541)	(2.72e-05)	(0.000291)	(152.5)	(0.000154)	(0.000598)	(0.321)
Parent has Some College	0.00327	0.00452	2.44e-05	-0.000149	-125.9	3.31e-05	-0.000496	0.502
	(0.00206)	(0.00301)	(2.45e-05)	(0.000239)	(88.55)	(0.000114)	(0.000315)	(0.317)
Parent has College Degree	0.00283	0.00311	4.59e-05	0.000360	-148.7	-0.000175	0.000174	0.319
	(0.00178)	(0.00235)	(3.56e-05)	(0.000578)	(99.88)	(0.000251)	(0.000334)	(0.230)
Constant	0.242***	-0.217***	-0.000748***	0.220***	10,826***	0.576***	0.174***	28.30***
	(0.00306)	(0.00435)	(2.91e-05)	(0.000259)	(48.81)	(0.000139)	(0.000567)	(0.436)
Observations	757,623	757,623	757,623	757,623	757,623	757,623	757,623	757,623
R-squared	0.093	0.079	0.091	0.069	0.065	0.049	0.066	0.128
F-value	1.295	0.573	0.339	0.802	1.139	0.813	0.640	1.324
p-value	0.226	0.838	0.971	0.627	0.328	0.616	0.781	0.210

Notes: This table displays the regression of base location characteristics and baseline family characteristics using Equation 3, including two panels: one with controls and one without controls. The sample consists of children aged 0-5. The dependent variables in both panels encompass various base location characteristics, such as average adjusted gross income, the percentage of black residents, mathematical achievement in 8th grade, population size, free or reduced-price lunch participation, income related to causality, and the percentage of county residents with a Bachelor's degree or higher. The independent variable for panel A only consists of specialty × rank × assignment years. The independent variables for panel B consist of family characteristics, including the military member's AFQT score, gender, race, parental age, and child age. The correlation coefficients in both panels indicate the strength and direction of relationships while controlling for PMOS, rank, and the year of assignment.

Table A.4: Correlation Between Base Characteristics and Family Characteristics, Ages 6-10

	(1) County BA Rate	(2) Est Impact on Income Percentile	(3) Est Impact on College Attendance	(4) Free Lunch Participation	(5) Population	(6) Standardized Math Scores	(7) Percentage Black Population	(8) Average County AGI
Panel A: No Additional Controls								
Constant	0.248***	-0.206***	-0.000672***	0.217***	12,165***	0.579***	0.176***	29.13***
	(0)	(0)	(0)	(0)	(1.38e-10)	(0)	(0)	(0)
Observations	756,130	756,130	756,130	756,130	756,130	756,130	756,130	756,130
R-squared	0.131	0.100	0.113	0.072	0.188	0.051	0.069	0.142
Panel B: With Controls								
Child Male	0.000253	0.000501	-5.49e-06	-6.33e-06	0.223	-2.21e-05	9.44e-05	0.0181
	(0.000162)	(0.000468)	(8.54e-06)	(0.000100)	(13.61)	(5.58e-05)	(0.000107)	(0.0232)
Black	0.00116***	-0.00627***	-8.28e-05**	0.000436*	-164.7***	-0.000609**	0.00693***	-0.139**
	(0.000443)	(0.00241)	(3.42e-05)	(0.000246)	(56.69)	(0.000259)	(0.00258)	(0.0648)
Hispanic	-0.000556*	-0.00611***	-1.90e-06	-0.00135***	-55.47	5.91e-05	-6.95e-06	-0.130**
	(0.000332)	(0.00229)	(1.89e-05)	(0.000501)	(36.17)	(0.000124)	(0.000262)	(0.0648)
Other Race	0.000905*	0.00126	-1.39e-05	-0.000727**	-22.57	6.65e-05	-0.00138***	-0.0972
	(0.000466)	(0.00104)	(2.11e-05)	(0.000319)	(34.40)	(0.000129)	(0.000427)	(0.0623)
Parent AFQT Score	4.44e-05**	9.79e-05**	8.94e-07*	1.08e-05*	1.399*	-1.93e-06	4.10e-07	0.00650**
	(2.01e-05)	(4.62e-05)	(4.94e-07)	(5.88e-06)	(0.726)	(2.11e-06)	(3.95e-06)	(0.00303)
Parent Completed GED	0.0118***	0.0191***	5.05e-05	-0.00315***	-609.0***	0.000889*	-0.00110**	1.159***
	(0.00367)	(0.00600)	(3.47e-05)	(0.00119)	(232.6)	(0.000500)	(0.000437)	(0.380)
Parent Graduated	0.00398***	0.00738**	5.00e-05**	0.000664	-57.73	-0.000148	-0.000393*	0.375***
High School	(0.00150)	(0.00302)	(2.47e-05)	(0.000495)	(46.49)	(0.000211)	(0.000229)	(0.123)
Parent has Associates	0.00495**	0.0120**	2.89e-05	0.000869	-500.6***	-0.000322	-0.000533	0.678***
	(0.00201)	(0.00500)	(3.09e-05)	(0.000607)	(121.7)	(0.000268)	(0.000337)	(0.240)
Parent has Some College	0.00496**	0.00786**	4.35e-05	0.000974*	-318.6***	-0.000306	-7.87e-05	0.679***
	(0.00197)	(0.00366)	(2.77e-05)	(0.000588)	(61.33)	(0.000239)	(0.000244)	(0.238)
Parent has College Degree	0.00313*	0.00287	7.34e-05*	0.00210**	-410.1***	-0.00105**	0.00107**	0.608**
	(0.00185)	(0.00343)	(3.89e-05)	(0.00106)	(105.7)	(0.000522)	(0.000514)	(0.265)
Constant	0.241***	-0.216***	-0.000724***	0.216***	12,227***	0.580***	0.174***	28.36***
	(0.00307)	(0.00462)	(3.50e-05)	(0.000634)	(53.49)	(0.000306)	(0.000828)	(0.344)
Observations	756,130	756,130	756,130	756,130	756,130	756,130	756,130	756,130
R-squared	0.133	0.101	0.114	0.073	0.190	0.052	0.074	0.143
F-value	2.017	1.954	0.697	1.210	4.963	0.942	1.909	1.593
p-value	0.0278	0.0339	0.729	0.279	3.20e-07	0.493	0.0393	0.102

Notes: This table displays regression of base location characteristics on baseline family characteristics using Equation 3. The dependent variables include various base location characteristics such as the percent of residents with Bachelor's degree or higher, income related to causality, causal impact on college going, free or reduced-price lunch participation, county population, mathematical achievement in 8th grade, the percentage of Black residents, and average adjusted gross income. The independent variable for panel A only consists of specialty $\times$ rank $\times$ assignment years. The independent variables comprise family characteristics, including the military member's gender, race, AFQT score, and parental education. Correlation coefficients indicate the strength and direction of relationships, while controlling for PMOS, rank, and the year of assignment.

Table A.5: Correlation Between Base Characteristics and Family Characteristics, Ages 11-13

	(1) County BA Rate	(2) Est Impact on Income Percentile	(3) Est Impact on College Attendance	(4) Free Lunch Participation	(5) Population	(6) Standardized Math Scores	(7) Percentage Black Population	(8) Average County AGI
Panel A: No Additional Controls								
Constant	0.248***	-0.201***	-0.000597***	0.223***	12,956***	0.577***	0.176***	28.81***
	(0)	(0)	(0)	(0)	(1.27e-10)	(0)	(0)	(0)
Observations	755,557	755,557	755,557	755,557	755,557	755,557	755,557	755,557
R-squared	0.150	0.108	0.119	0.069	0.285	0.052	0.078	0.146
Panel B: With Controls								
Child Male	-5.13e-05	-0.000471	-3.19e-06	5.18e-05	15.02	-4.38e-05	6.35e-05	-0.00523
	(0.000145)	(0.000478)	(8.00e-06)	(9.09e-05)	(15.58)	(5.14e-05)	(9.85e-05)	(0.0214)
Black	0.000866**	-0.00601*	-6.45e-05*	0.000118	-208.5***	-0.000344*	0.00556**	-0.0343
	(0.000384)	(0.00309)	(3.52e-05)	(0.000124)	(79.65)	(0.000186)	(0.00273)	(0.0452)
Hispanic	-0.000237	-0.00389*	-8.10e-06	-0.00107**	-79.78*	5.60e-05	3.59e-05	-0.103
	(0.000276)	(0.00198)	(1.69e-05)	(0.000503)	(46.62)	(9.21e-05)	(0.000260)	(0.0637)
Other Race	0.000686	0.00234*	-3.36e-05	-0.000796**	-75.62*	0.000126	-0.00104***	0.00495
	(0.000438)	(0.00129)	(2.47e-05)	(0.000389)	(38.84)	(0.000105)	(0.000382)	(0.0488)
Parent AFQT Score	3.60e-05*	7.66e-05*	5.32e-07	2.31e-06	0.257	4.21e-07	3.33e-06	0.00309*
	(1.87e-05)	(4.29e-05)	(3.96e-07)	(2.89e-06)	(0.544)	(1.50e-06)	(3.31e-06)	(0.00174)
Parent Completed GED	0.00828*	0.0148*	7.33e-05	0.000216	316.2	-0.000196	-0.000172	0.138
	(0.00484)	(0.00809)	(7.34e-05)	(0.000517)	(404.9)	(0.000275)	(0.000457)	(0.425)
Parent Graduated High School	0.00210	0.00284	5.78e-05*	0.000468	-107.4	-0.000147	-0.000212	0.132
	(0.00132)	(0.00211)	(3.50e-05)	(0.000322)	(68.13)	(0.000134)	(0.000189)	(0.0874)
Parent has Associates	0.00294*	0.00697*	4.53e-05	0.000361	-320.8**	-2.32e-05	-0.000307	0.271*
	(0.00178)	(0.00395)	(3.84e-05)	(0.000350)	(154.7)	(0.000154)	(0.000292)	(0.160)
Parent has Some College	0.00260	0.00262	6.37e-05	0.000416	-245.2**	-0.000171	0.000169	0.330*
	(0.00160)	(0.00233)	(4.03e-05)	(0.000324)	(119.3)	(0.000149)	(0.000216)	(0.174)
Parent has College Degree	0.000455	-0.000333	8.61e-05	0.000545	-149.3	-0.000429	0.000850**	0.286
	(0.00166)	(0.00317)	(5.85e-05)	(0.000502)	(181.8)	(0.000282)	(0.000411)	(0.239)
Constant	0.244***	-0.205***	-0.000641***	0.223***	12,949***	0.577***	0.174***	28.56***
	(0.00262)	(0.00321)	(3.98e-05)	(0.000324)	(107.8)	(0.000184)	(0.000998)	(0.219)
Observations	755,557	755,557	755,557	755,557	755,557	755,557	755,557	755,557
R-squared	0.152	0.109	0.119	0.069	0.288	0.052	0.082	0.147
F-value	0.942	0.776	0.610	0.529	24.68	0.449	1.301	3.686
p-value	0.493	0.652	0.807	0.871	0	0.922	0.223	6.06e-05

Notes: This table displays regression of base location characteristics on baseline family characteristics using Equation 3. The dependent variables include various base location characteristics such as the percent of residents with Bachelor's degree or higher, income related to causality, causal impact on college going, free or reduced-price lunch participation, county population, mathematical achievement in 8th grade, the percentage of Black residents, and average adjusted gross income. The independent variable for panel A only consists of specialty × rank × assignment years. The independent variables comprise family characteristics, including the military member's gender, race, AFQT score, and parental education. Correlation coefficients indicate the strength and direction of relationships, while controlling for PMOS, rank, and the year of assignment.

Table A.6: Correlation Between Base Characteristics and Family Characteristics, Ages 14-17

	(1) County BA Rate	(2) Est Impact on Income Percentile	(3) Est Impact on College Attendance	(4) Free Lunch Participation	(5) Population	(6) Standardized Math Scores	(7) Percentage Black Population	(8) Average County AGI
Panel A: No Additional Controls								
Constant	0.241***	-0.207***	-0.000721***	0.223***	15,667***	0.578***	0.176***	27.38***
	(0)	(0)	(0)	(0)	(4.29e-10)	(0)	(0)	(0)
Observations	754,820	754,820	754,820	754,820	754,820	754,820	754,820	754,820
R-squared	0.180	0.130	0.129	0.087	0.326	0.062	0.086	0.165
Panel B: With Controls								
Child Male	-0.000293	-0.000533	1.18e-05	1.61e-05	-1.759	7.26e-06	-1.13e-05	-0.0273
	(0.000197)	(0.000445)	(8.69e-06)	(7.44e-05)	(11.12)	(4.35e-05)	(7.64e-05)	(0.0220)
Black	0.000614*	-0.00442*	-3.76e-05*	-6.34e-05	-123.4*	-0.000292*	0.00378*	0.0188
	(0.000343)	(0.00245)	(2.28e-05)	(0.000114)	(63.37)	(0.000171)	(0.00202)	(0.0237)
Hispanic	-0.000226	-0.00266*	-5.07e-06	-0.000908*	-11.19	1.31e-05	-0.000129	-0.0869
	(0.000243)	(0.00152)	(1.38e-05)	(0.000474)	(26.64)	(9.24e-05)	(0.000175)	(0.0538)
Other Race	0.000301	0.00180*	-3.03e-05	-0.000855*	-6.164	0.000158	-0.000689**	-0.0653
	(0.000299)	(0.00105)	(2.16e-05)	(0.000465)	(25.79)	(0.000119)	(0.000298)	(0.0526)
Parent AFQT Score	3.32e-05*	6.64e-05*	2.56e-07	9.41e-08	-0.0375	8.60e-08	-2.25e-06	0.00354*
	(1.79e-05)	(3.78e-05)	(2.49e-07)	(2.62e-06)	(0.419)	(1.43e-06)	(3.21e-06)	(0.00192)
Parent Completed GED	0.00294	0.00582	1.85e-05	0.000667	717.8***	-0.000547	0.000361	-0.474***
	(0.00251)	(0.00467)	(4.21e-05)	(0.000539)	(131.3)	(0.000390)	(0.000343)	(0.132)
Parent Graduated High School	0.00139	0.00162	1.36e-05	0.000257	-99.07	-0.000119	-1.78e-05	0.0428
	(0.00104)	(0.00154)	(1.74e-05)	(0.000229)	(78.38)	(0.000113)	(0.000162)	(0.0550)
Parent has Associates	0.00145	0.00302	1.06e-05	0.000118	-177.0	-9.78e-05	-1.46e-05	0.105
	(0.00120)	(0.00237)	(2.32e-05)	(0.000256)	(121.9)	(0.000144)	(0.000236)	(0.0993)
Parent has Some College	0.00148	0.000657	1.35e-05	0.000157	-155.7	-0.000178	6.92e-05	0.115
	(0.00115)	(0.00141)	(1.96e-05)	(0.000213)	(109.3)	(0.000144)	(0.000185)	(0.0945)
Parent has College Degree	-0.000447	-0.00193	1.39e-05	-0.000588	-57.30	-0.000154	0.000803**	-0.00441
	(0.00107)	(0.00219)	(2.82e-05)	(0.000387)	(149.1)	(0.000193)	(0.000401)	(0.115)
Constant	0.238***	-0.210***	-0.000731***	0.223***	15,614***	0.578***	0.175***	27.24***
	(0.00194)	(0.00232)	(2.13e-05)	(0.000222)	(61.67)	(0.000163)	(0.000644)	(0.150)
Observations	754,820	754,820	754,820	754,820	754,820	754,820	754,820	754,820
R-squared	0.181	0.130	0.129	0.087	0.329	0.062	0.089	0.165
F-value	1.051	0.700	0.520	0.467	20.09	0.355	1.094	5.734
p-value	0.397	0.726	0.877	0.912	0	0.965	0.362	1.19e-08

Notes: This table displays regression of base location characteristics on baseline family characteristics using Equation 3. The dependent variables include various base location characteristics such as the percent of residents with Bachelor's degree or higher, income related to causality, causal impact on college going, free or reduced-price lunch participation, county population, mathematical achievement in 8th grade, the percentage of Black residents, and average adjusted gross income. The independent variable for panel A only consists of specialty $\times$ rank $\times$ assignment years. The independent variables comprise family characteristics, including the military member's gender, race, AFQT score, and parental education. Correlation coefficients indicate the strength and direction of relationships, while controlling for PMOS, rank, and the year of assignment.

Table A.7: Correlation Between Base Characteristics and Family Characteristics, Full Sample

	(1) County BA Rate	(2) Est Impact on Income Percentile	(3) Est Impact on College Attendance	(4) Free Lunch Participation	(5) Population	(6) Standardized Math Scores	(7) Percentage Black Population	(8) Average County AGI
Panel A: No Additional Controls								
Constant	0.246***	0.000643	-0.000570	0.261***	9,367***	0.559***	0.176***	29.07***
	(0.0126)	(0.000757)	(0.000893)	(0.0239)	(1,944)	(0.0102)	(0.00882)	(1.302)
Observations	3,059,254	2,915,963	3,058,575	133,554	2,791,744	132,851	2,789,251	2,707,896
R-squared	0.101	0.109	0.125	0.458	0.079	0.334	0.080	0.146
Panel B: With Controls								
Male	-0.00100	1.96e-05	0.000160	0.0143***	489.2**	-0.00493**	-0.00388***	0.667***
	(0.00159)	(8.35e-05)	(0.000160)	(0.00370)	(192.9)	(0.00218)	(0.00121)	(0.220)
Black	0.00118	0.000375	-0.000181	0.00701	-164.9	-0.00750	0.0127***	-0.109
	(0.00419)	(0.000313)	(0.000273)	(0.00832)	(332.5)	(0.00535)	(0.00361)	(0.406)
Hispanic	-0.000786	0.000104	1.81e-05	-0.0335***	21.12	0.00760	-0.000810	-0.113
	(0.00342)	(0.000146)	(0.000183)	(0.0115)	(504.2)	(0.00512)	(0.00144)	(0.417)
Other Race	0.00244	-0.000110	-6.95e-05	-0.0173*	134.3	0.00386	-0.00346**	-0.0573
	(0.00368)	(0.000146)	(0.000152)	(0.0101)	(413.5)	(0.00571)	(0.00171)	(0.452)
Parent AFQT Score	6.82e-05**	-1.71e-06	1.36e-06	-0.000103	0.624	4.79e-05	-1.09e-05	0.0157***
	(2.72e-05)	(1.72e-06)	(1.38e-06)	(9.24e-05)	(2.179)	(6.42e-05)	(1.74e-05)	(0.00419)
Parent is Married	0.00150*	-2.15e-05	-7.87e-05	0.000954	-269.8***	0.00143	-0.000185	-0.114
	(0.000816)	(3.97e-05)	(9.03e-05)	(0.00330)	(88.79)	(0.00191)	(0.000740)	(0.125)
Number of Dependents	-0.000533	-2.63e-05*	-8.11e-06	-0.00194*	-71.33**	0.000895	-0.000280	-0.118***
	(0.000418)	(1.46e-05)	(2.45e-05)	(0.00117)	(27.79)	(0.000678)	(0.000200)	(0.0433)
Parent has College Degree	0.00193*	1.81e-05	9.42e-05	0.00751	-65.73	-0.00632	0.00169**	0.538***
	(0.00111)	(3.53e-05)	(5.82e-05)	(0.00508)	(123.9)	(0.00393)	(0.000736)	(0.158)
par_HSD	-0.00479	0.000143	-8.86e-05	0.0295	76.97	-0.0212	0.00563*	-0.496
	(0.00343)	(0.000115)	(0.000211)	(0.0229)	(295.5)	(0.0168)	(0.00302)	(0.313)
Parent Graduated High School	-0.00131	4.82e-05	-1.61e-06	0.00488**	236.1*	-0.00188	0.000690	-0.111
	(0.000954)	(4.98e-05)	(9.50e-05)	(0.00217)	(122.6)	(0.00174)	(0.000831)	(0.0998)
Parent has Some College	0.000812	2.76e-05	2.82e-05	0.00523*	68.95	-0.00272	0.00107*	0.398***
	(0.000846)	(4.19e-05)	(5.94e-05)	(0.00304)	(96.98)	(0.00188)	(0.000645)	(0.139)
Constant	0.244***	0.000654	-0.000634	0.257***	9,262***	0.561***	0.176***	28.10***
	(0.0132)	(0.000823)	(0.000974)	(0.0254)	(2,045)	(0.0110)	(0.00856)	(1.247)
Observations	2,916,389	2,779,313	2,915,729	135,149	2,813,222	134,446	2,810,710	2,728,533
R-squared	0.096	0.104	0.120	0.461	0.073	0.332	0.082	0.145
Mean	0.247	0.000418	-0.000672	0.224	12677	0.575	0.175	28.68
F-val	2.197	1.209	1.379	2.264	2.790	0.771	1.645	3.131
P-val	0.0135	0.277	0.179	0.0109	0.00145	0.669	0.0821	0.000385

Notes: This table displays regression of residential location characteristics on baseline family characteristics using Equation 4. The dependent variables include various base location characteristics such as causal impact on college going, income related to causality, percent of residents with Bachelor's degree or higher The independent variables comprise family characteristics, including the military member's gender, race, AFQT score, and parental education. Correlation coefficients indicate the strength and direction of relationships, while controlling for PMOS, rank, and the year of assignment.

B Additional Results

Table B.1: Neighborhood Effects on Outcomes, Off vs On Base

	(1)	(2)	(3)	(4)
	College Attendance		Income Percentile at Age 25	
	Off Base	On Base	Off Base	On Base
County BA Rate	0.00892***	0.00297		
	(0.00211)	(0.00494)		
Chetty-Hendren Income Effect			0.370***	0.0254
			(0.0445)	(0.112)
Constant	0.591***	0.574***	43.99***	43.18***
	(.0079)	(0.0153)	(0.401)	(0.925)
Observations	2,288,586	510,619	1,339,639	233,576
R-squared	0.100	0.165	0.054	0.155

Notes: This table presents regression results from on equation 1 separately for years when children live off base (columns 1 and 3) and when children live on base (columns 2 and 4). The dependent variables are any college attendance between ages 17 and 22 and income percentile at age 25, and the location characteristics are the percent with a BA and the Chetty-Hendren estimated causal income effect for the assigned base county. Regression coefficients are interpreted as the causal impact of one year spent in the county.

Table B.2: Impacts of Residential Location on College-Going and Earnings

	(1) College Attendance	(2) College Attendance	(3) Income Percentile at Age 25	(4) Log of Earnings at Age 25	(5) Earnings at Age 25
Percent BA	0.0221***				
	(0.00215)				
Chetty-Hendren College Effect		0.242***			
		(0.0272)			
Chetty-Hendren Income Effect			0.433***		
			(0.0351)		
Log Avg Income				0.0392***	
				(0.00484)	
Avg Income					0.0310***
					(0.00263)
Constant	-0.113***	-0.107***	46.10***	8.872***	22,957***
	(0.00612)	(0.00611)	(1.007)	(0.0683)	(926.3)
Observations	2,718,047	2,704,228	1,937,629	1,393,013	1,984,633
R-squared	0.128	0.128	0.323	0.061	0.223

Notes: This table displays regressions of child outcome measurements on location characteristics based on Equation 2. The dependent variables include various outcome measurements including college attendance, income percentile at age 25, log of earnings at age 25, and own earnings at age 25. The independent variables are location characteristics, including BA rate of the county, Chetty Hendren estimated causal effect on college, Chetty Hendren estimated causal effect on income, log of the average of who grew up in the county income, and the average of who grew up in the county income. Regression coefficients indicate the impact of one year exposure to locational measure 1.0 higher in the county, while controlling for child YOB, parental AFQT, parental education, parental start year, race, including a full set of dummies PMOS * rank * length of assignment * year of assignment.

Table B.3: IV Estimates of Neighborhood Effects on Outcomes

	(1) College Attendance	(2) College Attendance	(3) Income Percentile at Age 25	(4) Composite SAT Score	(5) Years of College
Panel A: Instrument with Average Residential Characteristics of Other Families Assigned to Same Base					
County BA Rate of Neighborhood County	0.0189***			2.777*	
	(0.00183)			(1.484)	
Chetty Hendren College Effect		0.235***			
		(0.0226)			
Chety Hendren Income Effect			0.508***		
			(0.0331)		
Observations	2,674,541	2,660,923	1,547,485	574,894	
R-squared	0.039	0.039	0.011	0.100	
Number of FEs	23,268	23,255	18,203	15,066	
Panel B: Instrument with Characteristics of Assigned Base Neighborhood					
County BA Rate of Neighborhood County	0.0184***			5.773***	0.0694***
	(0.00196)			(1.598)	(0.00894)
Chetty Hendren Income Effect			0.310***		
			(0.0194)		
Observations	2,310,977		2,181,221	484,721	2,518,579
R-squared	0.039		0.011	0.100	0.044
Number of FEs	22,834		18,241	14,464	23,539

Notes: This table displays regressions of child outcome measurements on location characteristics based on Equation 2 instrumenting for residential location characteristics with the mean of other residential characteristics of people randomly assigned to base (panel A) or the neighborhood of the conditionally randomly assigned base (panel B). The dependent variables include college attendance and income percentile at age 25. The independent variables are Chetty-Hendren estimated causal effect on income, Chetty-Hendren estimated causal effect on college, and county BA rate. Regression coefficients indicate the impact of one year exposure to locational measure 1.0 higher in the county, while controlling for child year of birth, parental AFQT, parental education, parental start year, race, including a full set of dummies for PMOS $\times$ rank $\times$ year of assignment. The first stage F-statistics on the instruments are above 180,000 in each specification.

Table B.4: Impacts of Base Location on College Type and Education Tax Credit Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impacts on College Type						
	Any College	Public Two Year Collage	Public Four Year College	Private Four Year College	Private For Profit College	Other Two Year College
County BA Rate	0.00868***	0.0197***	-0.000292	0.00326**	0.00228**	-0.000963
	(0.00198)	(0.00203)	(0.00200)	(0.00143)	(0.00104)	(0.000600)
Constant	0.592***	0.284***	0.260***	0.0674***	0.0701***	0.0271***
	(0.00696)	(0.00705)	(0.00713)	(0.00479)	(0.00365)	(0.00200)
Observations	2,651,356	2,651,356	2,651,356	2,651,356	2,651,356	2,651,356
R-squared	0.089	0.039	0.061	0.039	0.030	0.026
Panel B: Impacts on Years of College Type						
	Years of Any College	Years of Public Two Year Collage	Years of Public Four Year College	Years of Private Four Year College	Years of Private For Profit College	Years of Other Two Year College
County BA Rate	0.0366***	0.0433***	-0.0207***	0.0106**	0.00514**	-0.00168*
	(0.00916)	(0.00563)	(0.00725)	(0.00447)	(0.00215)	(0.000992)
Constant	1.873***	0.714***	0.845***	0.152***	0.122***	0.0408***
	(0.0338)	(0.0204)	(0.0275)	(0.0155)	(0.00735)	(0.00326)
Observations	2,917,075	2,917,075	2,917,075	2,917,075	2,917,075	2,917,075
R-squared	0.106	0.042	0.065	0.038	0.027	0.024
Panel C: Impacts on Education Tax Benefit Utilization						
	Parents Refundable Education Credits	Parent Tuition & Fee Deduction	Parents Use of Education Credit	Refundable Education Credits	Use of Education Credit	Tuition & Fee Deduction
County BA Rate	0.789	0.778	1.243	-1.504**	0.969	2.621**
	(1.551)	(2.232)	(2.346)	(0.726)	(0.639)	(1.062)
Constant	33.84***	98.58***	147.9***	22.23***	31.08***	27.16***
	(4.901)	(9.451)	(8.454)	(2.166)	(2.330)	(3.632)
Mean of Dependent Var	113.45	106.47	218.90	31.09	24.31	20.42
Observations	2,650,637	2,650,637	2,650,637	2,650,637	2,650,637	2,650,637
R-squared	0.053	0.045	0.054	0.033	0.025	0.025

Notes: This table displays regressions of college attendance and education tax credit utilization at age 20 on location characteristics based on Equation 2. In panel A, the dependent variable college attendance is shown for any college, public two year college, public forum year college, private four year college, private for profit college, and other two year colleges. In panel b, the dependent variable years of college attendance is shown for any college, public two year college, public forum year college, private four year college, private for profit college, and other two year colleges. In panel C, the dependent variables, education tax credit utilization at age 20 include parent refundable education credits, parent tuition and fee deduction, parent education credit, refundable education credits, use of education credits, and tuition and fee deduction. The independent variable in both panels is the percentage of residents with a bachelors degree within the base county. Regression coefficients indicate the causal impact of one year spent in the county while controlling for child YOB, parental AFQT, parental education, parental start year, race, PMOS, rank, length of assignment, and the year of assignment.

Table B.5: Effect of Better Base Locations on SAT Scores, by Age Group

	(1) SAT (Total)	(2) SAT (Math)	(3) SAT (Verbal)	(4) SAT (Total)	(5) SAT (Math)	(6) SAT (Verbal)
County BA Rate Age: 0-5	6.320***	3.215***	3.069***			
	(1.384)	(0.750)	(0.756)			
County BA Rate Age: 6-10	4.130***	2.381***	1.760***			
	(0.952)	(0.516)	(0.520)			
County BA Rate Age: 11-13	7.452***	5.397***	2.056**			
	(1.670)	(0.906)	(0.913)			
County BA Rate Age: 14-17	4.255***	2.085**	2.157**			
	(1.543)	(0.837)	(0.843)			
Chetty-Hendren Income				-0.0642	-0.615	0.573
Effect Age: 0-5				(0.779)	(0.422)	(0.426)
Chetty-Hendren Income				1.490***	0.682**	0.807***
Effect Age: 6-10				(0.543)	(0.295)	(0.297)
Chetty-Hendren Income				2.425***	1.226**	1.201**
Effect Age: 11-13				(0.892)	(0.484)	(0.488)
Chetty-Hendren Income				1.256	0.477	0.785
Effect Age: 14-17				(0.886)	(0.481)	(0.484)
Constant	920.9***	450.5***	470.3***	940.0***	461.1***	478.8***
	(3.586)	(1.943)	(1.958)	(3.178)	(1.722)	(1.735)
Observations	174,198	174,430	174,430	174,198	174,430	174,430
R-squared	0.348	0.325	0.324	0.347	0.325	0.324

Notes: This table displays regressions of SAT scores on location characteristics across age groups based on Equation 3. The dependent variable, SAT score, is shown for total, math, and verbal. The independent variables are the bachelor attainment rate of the base county shown for four age groupings (0-5, 6-10, 11-13, and 14-17), and the Chetty Hendren estimated causal effect on income of the base county shown for four age groupings (0-5, 6-10, 11-13, and 14-17). Regression coefficients indicate the impact of one year exposure to locational measure 1.0 higher in the county while controlling for child YOB, parental AFQT, parental education, parental start year, race, PMOS, rank, length of assignment, and the year of assignment.

Table B.6: Impacts of Base Location, by Age Group

	(1) All	(2) Male	(3) Female	(4) White	(5) Black
Panel A: Effects on College-Going					
County BA Rate Age: 0-5	0.00929***	0.0122***	0.00722***	0.00398*	0.0177***
	(0.00173)	(0.00264)	(0.00238)	(0.00229)	(0.00343)
County BA Rate Age: 6-10	0.00513***	0.00610***	0.00455**	0.00148	0.0112***
	(0.00136)	(0.00207)	(0.00187)	(0.00182)	(0.00265)
County BA Rate Age: 11-13	0.0126***	0.0146***	0.0106***	0.0102***	0.0134***
	(0.00265)	(0.00407)	(0.00367)	(0.00360)	(0.00516)
County BA Rate Age: 14-17	0.0185***	0.0196***	0.0171***	0.0173***	0.0236***
	(0.00248)	(0.00381)	(0.00341)	(0.00336)	(0.00486)
Constant	0.660***	0.514***	0.661***	0.630***	0.747***
	(0.00449)	(0.00681)	(0.00613)	(0.00611)	(0.00886)
Mean of Dependent Var	0.67	0.60	0.75	0.67	0.69
Observations	679,464	332,974	328,853	362,382	202,027
R-squared	0.174	0.201	0.190	0.230	0.219
Panel B: Effects on Income Percentile at Age 25					
Chetty-Hendren Income Effect Age: 0-5	-0.256***	-0.339***	-0.109	-0.338***	-0.00457
	(0.0782)	(0.118)	(0.112)	(0.109)	(0.143)
Chetty-Hendren Income Effect Age: 6-10	0.401***	0.317***	0.487***	0.425***	0.467***
	(0.0530)	(0.0787)	(0.0765)	(0.0750)	(0.0951)
Chetty-Hendren Income Effect Age: 11-13	0.472***	0.469***	0.538***	0.426***	0.352**
	(0.0853)	(0.127)	(0.123)	(0.120)	(0.154)
Chetty-Hendren Income Effect Age: 14-17	0.835***	0.717***	0.943***	1.049***	0.628***
	(0.0782)	(0.116)	(0.112)	(0.109)	(0.142)
Constant	48.53***	53.20***	47.36***	46.59***	48.67***
	(0.251)	(0.367)	(0.355)	(0.352)	(0.480)
Mean of Dependent Var	47.75	49.60	45.89	49.20	45.46
Observations	493,592	241,026	238,753	260,575	154,773
R-squared	0.110	0.147	0.152	0.148	0.163

Notes: This table displays regressions of outcomes on location characteristics across age groups based on Equation 3 for male, female, white, and Black children. Regression coefficients indicate the impact of one year exposure to locational measure 1.0 higher in the county while controlling for child year of birth, parental AFQT, parental education, parental start year, race, PMOS, rank, length of assignment, and the year of assignment.

Table B.7: Full Interaction Effects of Years in Above 75th Percentile Place Across Ages

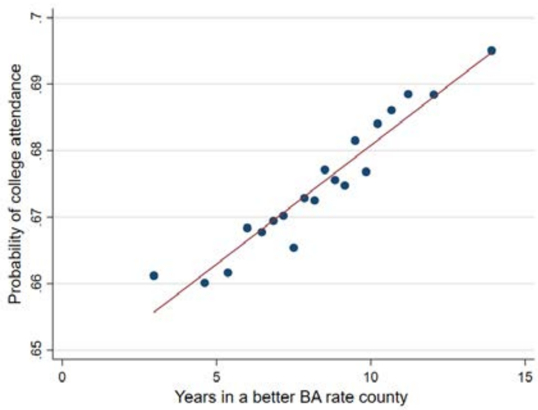
	(1) College Attendance
County BA Rate:	
Age: 0-5	0.00406**
	(0.00185)
Age: 6-10	0.000755
	(0.00126)
Age: 11-13	-0.00134
	(0.00211)
Age: 14-17	0.00416**
	(0.00174)
Interaction Effect:	
(0-5) on (6-10)	-.00056
	(.00040)
(0-5) on (11-13)	-.00032
	(.00098)
(0-5) on (14-17)	-.00216
	(.00113)
(6-10) on (11-13)	.00042
	(.00055)
(6-10) on (14-17)	-.00015
	(.00069)
(11-13) on (14-17)	-.00117
	(.00088)
Constant	0.671***
	(0.00597)
Observations	679,464
R-squared	0.174

	(1) Inc Percentile at Age 25
Impacts of Army Base County on Income Percentile	
Age: 0-5	0.226
	(0.145)
Age: 6-10	0.0384
	(0.0857)
Age: 11-13	-0.0969
	(0.137)
Age: 14-17	0.231*
	(0.119)
Interaction Effect:	
(0-5) on (6-10)	-0.098*
	(0.034)
(0-5) on (11-13)	0.242*
	(0.082)
(0-5) on (14-17)	-0.016
	(0.093)
(6-10) on (11-13)	-0.059
	(0.040)
(6-10) on (14-17)	0.065
	(0.049)
(11-13) on (14-17)	-0.168*
	(0.056)
Constant	46.38***
	(0.399)
Observations	493,592
R-squared	0.110

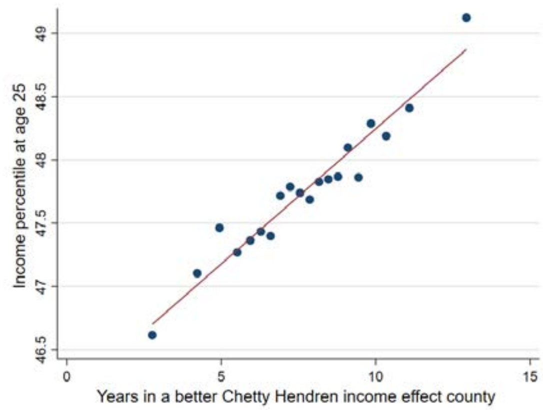
Notes: This table displays regressions of income percentile at age 25 on location characteristics across age groups with iteration effects across groupings. The dependent variable is child's income percentile at age 25. The independent variable is the Chetty Hendren estimated causal effect on income of the base county shown for four age groupings (0-5, 6-10, 11-13, and 14-17) including iteration effects across age groupings. Regression coefficients indicate the causal impact of one year exposure to locational measure 1.0 higher while controlling for child YOB, parental AFQT, parental education, parental start year, race, PMOS, rank, length of assignment, and the year of assignment.

Figure B.1: Correlation of Outcomes and Exposure to Neighborhoods above 25th Percentile of Neighborhood Attribute

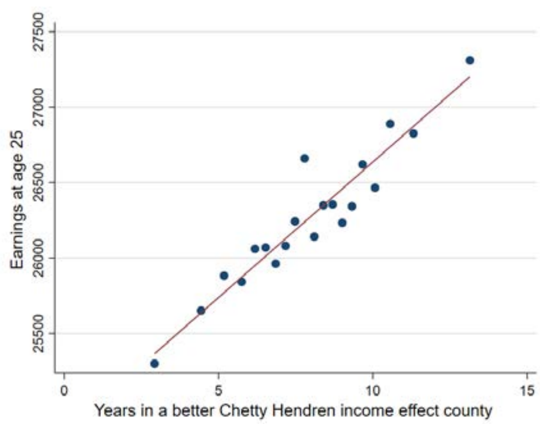
(a) College Going and Percent BA



(b) Income Percentile and Chetty-Hendren Causal Income Effect

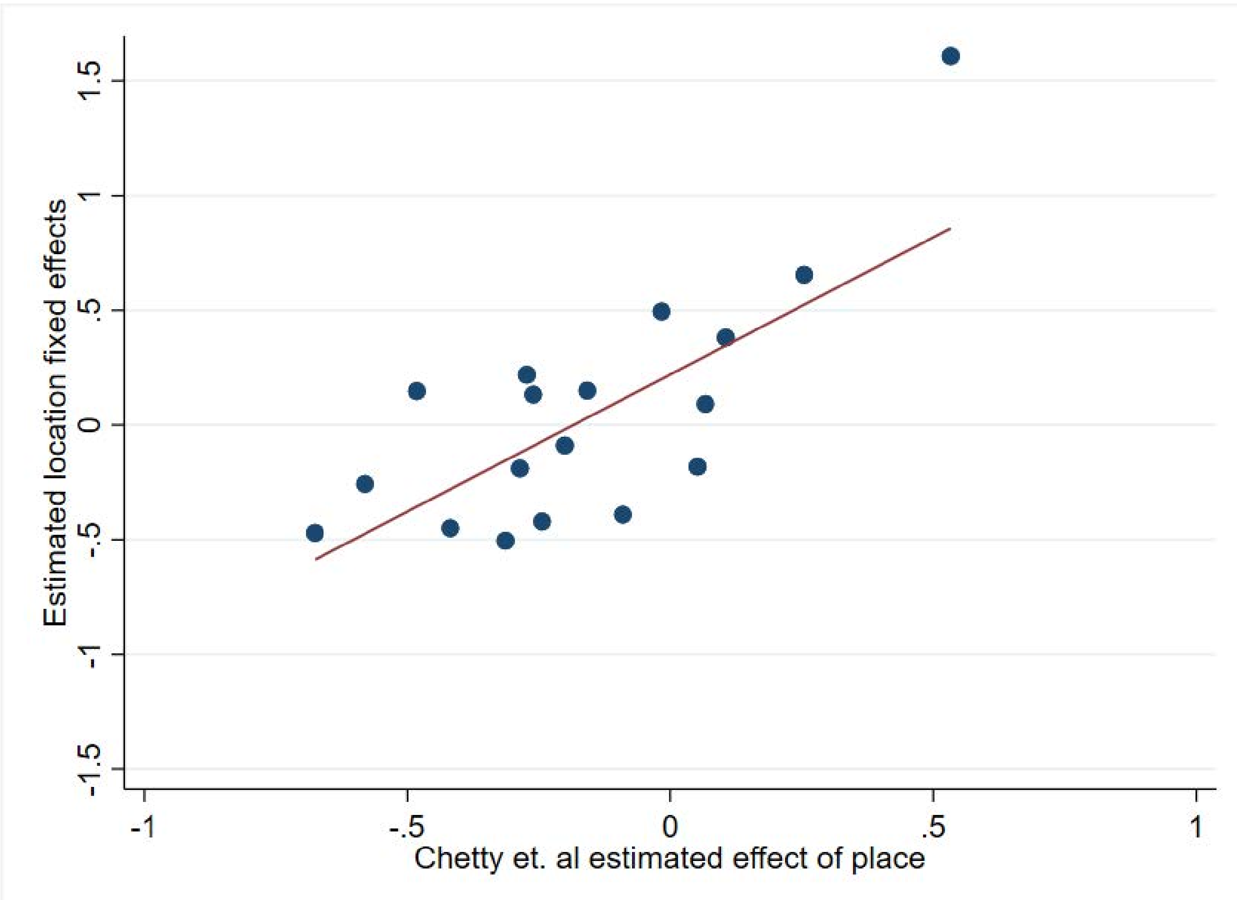


(c) Earnings and Chetty-Hendren Causal Income Effect



Notes: The x-axis bins observations by years spent in a county above the 25th percentile of a neighborhood attribute. The y-axis is the average child outcome within the bin. Child outcomes are residualized for parent demographics, parents' PMOS, rank, and year of assignment to the base.

Figure B.2: Bin Scatter of Base Fixed Effects and Chetty-Hendren Estimated Causal Effects



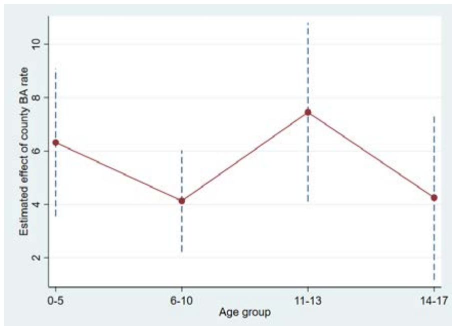
Notes: The x axis bins observations by ventiles of Chetty et. al causal effect of place. The y axis is our estimated fixed effect, calculated by regressing income percentile on random base assignment without accounting for years of exposure. Base fixed effects do not divide by average years in location, which is 2.5. The fixed effects are residualized for parent demographics, parents' PMOS, rank, and year of assignment to the base.

Table B.8: Child * Age Panel: Heterogeneity of County by Sex and Race

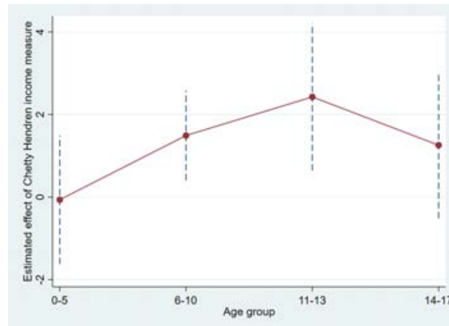
	(1) Female	(2) Male	(3) White	(4) Black
Panel A: College-Going				
County BA Rate	0.00587**	0.0117***	0.00441	0.0138***
	(0.00264)	(0.00295)	(0.00296)	(0.00313)
Constant	0.589***	0.434***	0.526***	0.676***
	(0.00923)	(0.0103)	(0.0103)	(0.0120)
Observations	1,318,484	1,330,574	1,289,760	939,898
R-squared	0.082	0.081	0.111	0.112
Panel B: Income Percentile at Age 25				
Chetty Hendren Causal	0.481***	0.139**	0.294***	0.300***
Income Effect	(0.0568)	(0.0587)	(0.0622)	(0.0634)
Constant	41.48***	49.59***	40.99***	43.83***
	(0.532)	(0.551)	(0.564)	(0.650)
Observations	808,794	809,935	774,076	608,727
R-squared	0.070	0.073	0.089	0.073

Figure B.3: Estimated Neighborhood Exposure Effects on SAT Scores, by Age Groups

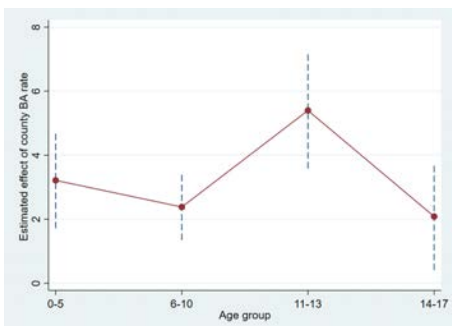
(a) County BA on Total SAT Score



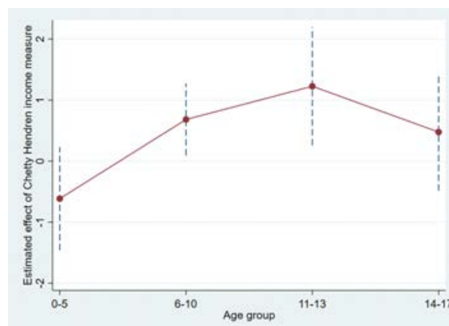
(b) Chetty-Hendren Income Effect on Total SAT Score



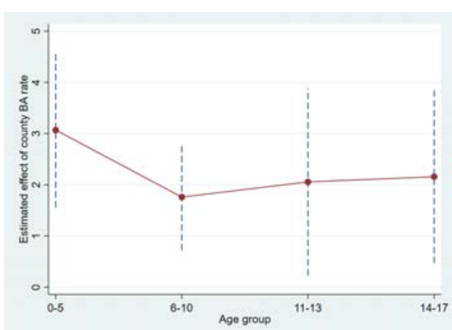
(c) County BA Exposure on Math SAT Score



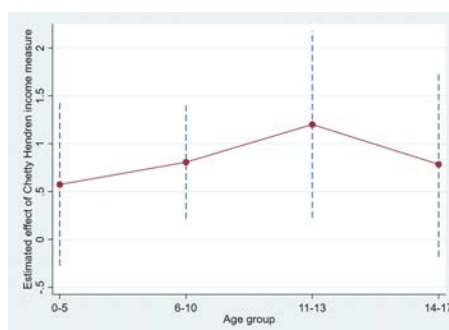
(d) Chetty-Hendren Income Effect on Math SAT Score



(e) County BA on Verbal SAT Score



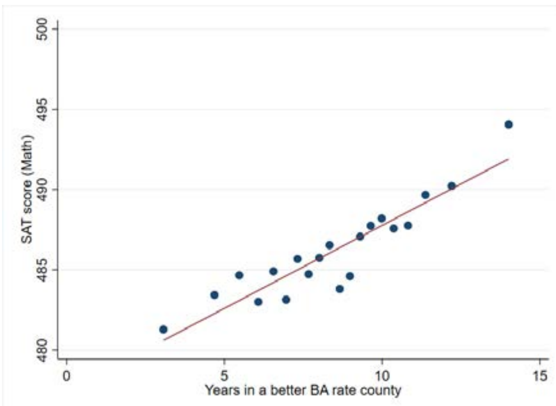
(f) Chetty-Hendren Income Effect on Verbal SAT Score



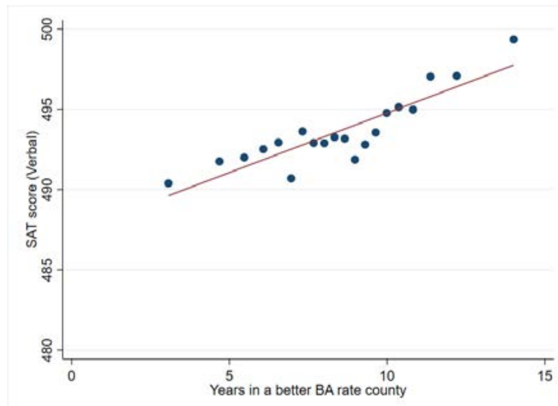
Notes: This panel displays regressions of SAT scores on location characteristics across age groups based on Equation 3. The dependent variable, SAT score, is shown for total, math, and verbal. The independent variables are the bachelor attainment rate of the base county shown for four age groupings (0-5, 6-10, 11-13, and 14-17), and the Chetty Hendren estimated causal effect on income of the base county shown for four age groupings (0-5, 6-10, 11-13, and 14-17). Regression coefficients indicate the impact of one year exposure to locational measure 1.0 higher in the county while controlling for child YOB, parental AFQT, parental education, parental start year, race, PMOS, rank, length of assignment, and the year of assignment.

Figure B.4: Effect of Years Spent in an Above 25th Percentile BA Rate County or an Above 25th Percentile Chetty-Hendren Causal Income County on SAT Scores: Math & Verbal

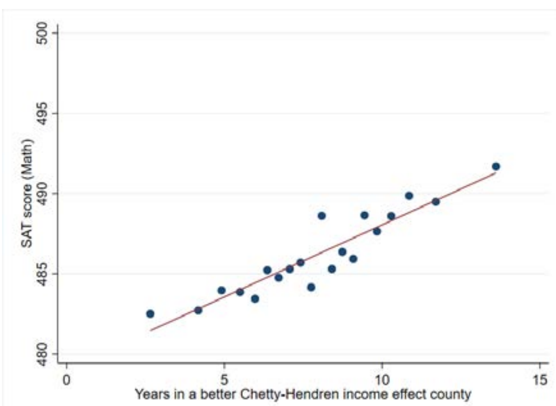
(a) County BA Rate Exposure on Math SAT Scores



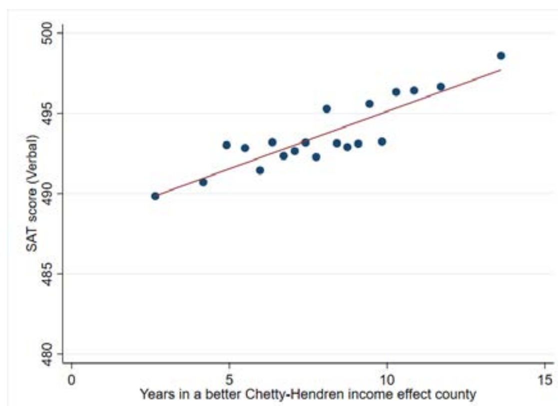
(b) County BA Rate Exposure on Verbal SAT Scores



(c) Chetty Hendren Causal Exposure on Math SAT Scores



(d) Chetty Hendren Causal Exposure on Verbal SAT Scores



Notes: In panels a and b the x axis bins observations by years of exposure to a county with an above 25th percentile bachelor attainment rate. In panels c and d the x axis bins observations by years of exposure in a base county with an above 25th percentile Chetty Hendren estimated causal effect on income. The y axis in panels a and c is the child's math SAT score averaged within the bin. The y axis in panels b and d is the child's verbal SAT score averaged within the bin. The SAT score is residualized for parent demographics, parents' Professional Military Occupational Specialty (PMOS), rank, and year of assignment to the base.