USC Dornsife
Dana and David Dornsife
College of Letters, Arts and Sciences

SOCIOLOGY
Ann Owens
Assistant Professor

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Daniel & Beshara, P.C.
3301 Elm Street
Dallas, TX 75226

Dear Mr. Daniel and Ms. Beshara,

I have prepared my expert report for *ICP v. Department of Treasury and Office of the Comptroller of the Currency, 3:14-3013-D.* Enclosed, please find the written report as well as selected peer-reviewed articles included in Appendix 1.

My curriculum vitae is included in Appendix 2, which summarizes my qualifications and publications. In the past 4 years, I have not testified as an expert at trial or by deposition in any trials. I acknowledge that I was compensated by Daniel & Beshara, P.C., at a rate of $200 per hour for preparing this expert report.

Sincerely,

AnnOwens

Ann Owens
Assistant Professor
Department of Sociology
University of Southern California

There is a body of peer-reviewed scholarly research and government reports demonstrating that growing up in a racially segregated, impoverished neighborhood reduces children's well-being on a variety of indicators. Over the past three decades, multiple review articles have summarized the scholarly research across disciplines on neighborhood effects on children and adolescents in the U.S. (e.g., Duncan and Raudenbush 1999; Durlauf 2004; Ellen and Turner 1997; Jencks and Mayer 1990; Leventhal and Brooks-Gunn 2000; Mayer and Jencks 1989; Pebley and Sastry 2004; Sampson, Morenoff, and Gannon-Rowley 2002; Sharkey and Faber 2014). The bulk of the evidence indicates that growing up in a socioeconomically disadvantaged neighborhood with many non-white neighbors reduces children's well-being.

Early Studies of Neighborhoods

The contemporary neighborhood effects literature is characterized by quantitative reports identifying statistical relationships between residents' outcomes and the socioeconomic and demographic characteristics of their neighborhoods. This research base flourished following the publication of William Julius Wilson's widely read and cited book *The Truly Disadvantaged* (1987), which detailed conditions in black inner-city neighborhoods. While this body of research grew considerably after Wilson's book, evidence on the detriments of racial/ethnic segregation and growing up in disadvantaged neighborhoods existed before 1987. In the social science literature, the idea of neighborhood effects dates to at least the 19^{th} century (Briggs, Popkin, and Goering 2010; Sampson 2012), gaining prominence in the U.S. in the late 19^{th} and early 20^{th} centuries via the work of Jacob Riis ([1890] 2010), W.E.B. DuBois ([1899] 1996), and the Chicago School of Sociology (Park and Burgess [1925] 1984). These early scholars documented impoverished conditions in inner-city neighborhoods in New York, Philadelphia, and Chicago, arguing that neighborhood conditions constrained individuals' behavior. In the 1960s and 1970s, a number of studies, many using in-depth ethnographic techniques, documented the conditions of racially and socially isolated, impoverished neighborhoods, arguing that residents' social and economic relationships and outcomes were shaped by their neighborhoods (Clark [1965] 1989; Drake and Cayton [1945] 1993; Liebow 1967; Rainwater 1970; Stack 1974; Suttles 1968).

A prominent government study on the topic of racially segregated neighborhoods—the report of the National Advisory Commission on Civil Disorders ("The Kerner Commission")—was published in 1968. The Commission was convened to investigate the "racial disorder" occurring in the summer of 1967, when nearly 150 cities reported incidents in black neighborhoods ranging from minor disturbances to large-scale uprisings in places like Newark and Detroit. This report was one of a series of White House conferences, commissions, and national policy reports from the early 1960s to the early 1980s on conditions in inner-city neighborhoods.

In their investigation of the causes of the disorder, the Kerner Commission examined the broad conditions of life in racially segregated, poor "ghetto" inner-city neighborhoods, hearing testimony from 130 witnesses ranging from Dr. Martin Luther King, Jr to J. Edgar Hoover, undertaking 1200 interviews and surveys in 23 cities, visiting 8 cities, and reviewing social science evidence. The commission concluded "Our nation is moving toward two societies, one black, one white—separate and unequal… Segregation and poverty have created in the racial ghetto a destructive environment totally unknown to most white Americans" (The National Advisory Commission on Civil Disorders [1968] 1988:1-2). The report details how "segregation and poverty converge on the young to destroy opportunity and enforce failure," describing the lack of economic opportunities, poor sanitation, high crime rates, commercial exploitation, failing schools, and substandard housing in black urban communities (10). The in-depth report argues that disadvantaged neighborhoods have consequences for residents', particularly children's, educational, economic, and health outcomes and strongly advocates the need for policies aimed at integration. The report states:

> "Federal housing policies must be given a new thrust aimed at overcoming the prevailing patterns of racial segregation. If this is not done, those programs will continue to concentrate the most impoverished and dependent segments of the population into the central-city ghettos where there is already a critical gap between the needs of the population and the public resources to deal with them… [Policymakers should] reorient federal housing programs to place more low- and moderate-income housing outside of ghetto areas." (28)

The report was controversial due to its strong language and calls for sweeping change, and it was largely dismissed by President Lyndon B. Johnson and questioned by white Americans. However, it received considerable media and public attention and was published as a paperback by Bantam Books in 1968, quickly becoming a bestseller (Zelizer 2016).

Quantitative Evidence on Neighborhood Effects

The contemporary quantitative neighborhood effects literature flourished following the publications of *The Truly Disadvantaged* (Wilson 1987) and *American Apartheid* (Massey and Denton 1993), both of which detailed increasing poverty in racially segregated black neighborhoods. Building on these seminal works, newly available data and statistical methods for isolating causal effects spurred the growth of the modern neighborhood effects literature.

Researchers have long noted the challenge of identifying causal effects of neighborhoods because households select where to live. People are not randomly assigned a "treatment" of living in a certain neighborhood, as in an experiment. People who move into different neighborhoods have different characteristics, so isolating the effect of neighborhood rather than individual and family characteristics is challenging. For example, high-income households can afford to live in expensive neighborhoods where other households are also high income. When estimating the causal effect of living among high-income neighbors on a person's well-being, his own household's income must be taken into account. Comparisons should be made between individuals with similar characteristics who live in different types of neighborhoods. When they are not, this leads to a problem known as selection bias, where different outcomes may be observed across individuals living in different neighborhoods even when neighborhoods do not in fact have a causal effect.

Past scholarship generally takes three approaches to addressing this problem of selection bias. First, researchers analyze observational survey data with advanced statistical techniques to carefully account for differences in individuals' characteristics across neighborhoods. Recent developments in causal methodology provide approaches that plausibly account for neighborhood selection to generate robust estimates of neighborhood effects. Second, researchers take a macro geographic perspective and examine the association between segregation between neighborhoods at the city or metropolitan area level with individuals' outcomes. Selection bias at the city or metropolitan area level is of lesser concern than at the neighborhood level, as discussed below. Third, researchers analyze data from social programs that randomly induce residential moves, like the Moving to Opportunity (MTO) housing demonstration. Below, I summarize peer-reviewed research that aims to identify causal neighborhood effects on children's and adolescents' outcomes via each of these approaches.

Findings from Observational Data

A substantial body of social science literature, dating from the early 1980s, draws on observational (non-experimental) survey data to document neighborhood effects on residents' outcomes. These studies, which use statistical controls to account for individual and family characteristics, show that growing up in a socioeconomically disadvantaged neighborhood reduces educational success and increases the odds of teenage childbearing (e.g., Aaronson 1998; Ainsworth 2002; Brooks-Gunn et al. 1993; Brooks-Gunn and Duncan 1997; Chase-Lansdale and Gordon 1996; Crane 1991; Crowder and South 2011; Datcher 1982; Duncan 1994; Duncan, Brooks-Gunn, and Klebanov 1994; Ensminger, Lamkin, and Jacobson 1996; Foster and McLanahan 1996; Harding 2003; Hogan and Kitagawa 1985; Klebanov et al. 1998; Owens 2010; South and Crowder 1999; Sucoff and Upchurch 1998). (An earlier body of literature documents the effects of schools' socioeconomic mix, which is closely related to neighborhoods' socioeconomic mix (Jencks and Mayer 1990)). A few studies relying on regression models to statistically control for individual and family characteristics document mixed or null effects of neighborhood characteristics on individual outcomes (e.g., Ginther, Haveman, and Wolfe 2000; Plotnick and Hoffman 1999), perhaps due to the challenge of properly specifying the statistical model to account for selection. However, later studies applying more advanced techniques to these same datasets do find significant causal relationships between neighborhood characteristics and residents' outcomes. The weight of the evidence in these studies indicates an association between neighborhood composition and children's well-being, controlling for characteristics of the children and their families.

Developments in causal methodology provide new approaches to identifying neighborhood effects beyond statistical control in regression analyses, which may be biased by unobserved variables' contribution to neighborhood selection. An approach using inverse probability of treatment weighting directly models selection into and out of neighborhoods over time based on families' characteristics. These models account for changing neighborhood contexts and the timing of exposure to neighborhoods, contending that neighborhood effects may be lagged, not contemporaneous, and cumulative, with a longer length of exposure to a neighborhood having larger effects. This counterfactual approach statistically weights children depending on how similar they are on observable individual and family traits that may contribute to residential choice, mimicking an experiment by creating "treatment" and "control" groups that appear identical on measured background characteristics that affect residential choices. Provided that the most important determinants of residential choices are observed, these studies provide strong causal evidence for neighborhood effects.

In terms of educational outcomes, a nationally representative study using this statistical approach shows that if black children live in the most disadvantaged neighborhoods throughout childhood, about 76% graduate from high school (Wodtke, Harding, and Elwert 2011). If comparable black children live in the least disadvantaged neighborhoods, the graduation rate rises to about 96%. The impact among nonblack children is smaller, a graduation rate of about 87% in the most disadvantaged neighborhoods compared to 95% in the least disadvantaged.[1] Neighborhood effects on the odds of high school graduation are also larger among low-income children than among high-income children. Living in the most compared to the least disadvantaged neighborhood reduces the graduation rate among poor black children by over 20 percentage points; among non-poor black children, the effect size is 9 percentage points (Wodtke, Elwert, and Harding 2016).

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[1] This study measures neighborhood disadvantage by combining neighborhood poverty rate, unemployment rate, welfare receipt rate, proportion of families headed by single females, proportion of residents without a high school diploma, proportion of residents with a college degree, and proportion of residents employed in managerial or professional jobs. Most and least disadvantaged neighborhoods are defined as the highest and lowest quartile in the United States.

Other studies employing causal statistical analyses demonstrate effects on cognitive skills. Among black children growing up in Chicago in the 1990s, living in a neighborhood of concentrated disadvantage reduced later verbal ability (measured ~3 years later) by the equivalent of missing a year of schooling (Sampson, Sharkey, and Raudenbush 2008).[2] There is also evidence that neighborhood environments can have effects on cognitive skills over multiple generations. A child's cognitive ability may be affected not only by his neighborhood, but also by the neighborhood his parent grew up in. His parent's childhood neighborhood may have affected their own educational, economic, or health outcomes, which in turn contribute to the child's well-being directly and via the neighborhood his parent can afford as an adult. Children whose families were exposed to high-poverty neighborhoods in two successive generations had reading and math scores more than half a standard deviation lower than children whose families lived in non-poor neighborhoods over generations (Sharkey and Elwert 2011).[3]

Neighborhood disadvantage also affects the odds of adolescent parenthood: the odds of adolescent parenthood are 80% higher among black children who grew up in high- compared to low-poverty neighborhoods (Wodtke 2013). Among non-black children, growing up in a high-poverty neighborhood more than doubles the odds of adolescent parenthood.[4]

Another approach to estimating causal effects compares siblings who were different ages when their families moved—who were exposed to more or less disadvantaged neighborhoods for different lengths of time. Comparing siblings provides strong causal evidence because it accounts for dimensions of the family environment, including those that may be hard to observe or measure via a survey or interview, e.g., parent-child interaction style. This study design assumes that parents do not purposely time their moves to provide an advantage for one child over another. By treating the timing of moves as random, researchers can estimate effects of exposure to more or less disadvantaged neighborhoods.

Using this approach, analyses of tax record data from over 5 million families across the U.S. with children born between 1980 and 1991 show that spending one's entire childhood in a county whose outcomes are one standard deviation better increases a child's income in young adulthood by 10% (Chetty and Hendren 2016).[5] Each additional year spent in a better county improves outcomes, with similar effects of moving during early and later childhood. Moving to a better neighborhood also increases the likelihood of children attending college and getting married and reduces the likelihood of teen pregnancy.

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[2] This study measures neighborhood disadvantage by combining neighborhood poverty rate, unemployment rate, welfare receipt rate, proportion of households headed by a single female, proportion African-American, and proportion of children under 18. Concentrated disadvantage neighborhoods are defined as the most disadvantaged quartile of neighborhoods in Chicago.

[3] High poverty neighborhoods have poverty rates of at least 20% and non-poor neighborhoods have poverty rates below 20%.

[4] High-poverty neighborhoods have poverty rates above 20%, moderate-poverty neighborhoods have poverty rates of 10 to 20%, and low-poverty neighborhoods have poverty rates below 10%. Moderate-poverty neighborhoods increase the odds of parenthood by about 75% among blacks and 60% among non-blacks, compared to low-poverty neighborhoods.

[5] Counties are categorized by the outcomes of children already living there, specifically by children's expected earnings conditional on their parents' income. Correlates of "better" neighborhoods include fewer black residents, lower racial segregation, lower income inequality, higher K-12 school quality, more social capital, and more two-parent homes. This paper has not undergone final peer review yet; it is a working paper at the National Bureau of Economic Research.

Over the past several decades, researchers have used statistical techniques to estimate neighborhood effects from survey data, and the substantial bulk of the evidence indicates that growing up in socioeconomically disadvantaged neighborhoods is detrimental for children's future outcomes.

Findings from Macro-Level Segregation Studies

Several studies measure the effects of segregation between neighborhoods within metropolitan areas, rather than characteristics of neighborhoods themselves, on the outcomes of black and white residents. Racial segregation produces neighborhoods that are more homogenously black or homogenously white. The logic of these studies is that greater black-white segregation between neighborhoods is disadvantageous for black residents (and potentially beneficially for white residents). Studies of racial (black-white) segregation between neighborhoods indicate that in more highly segregated metropolitan areas, black young adults have lower educational attainment and worse labor market outcomes than they would in less segregated places (Ananat 2011; Cutler and Glaeser 1997; Quillian 2014). Cutler and Glaeser (1997) conclude that "a one standard deviation reduction in segregation … would eliminate one-third of the gap between whites and blacks in most of our outcomes" (865). Similar studies examining economic segregation demonstrate that economic segregation is disadvantageous for poor youths' educational outcomes (Mayer 2002; Quillian 2014).

This approach provides the methodological advantage of reducing concern about selection bias because the independent variable, variation in segregation, is measured for metropolitan areas rather than neighborhoods. Metropolitan areas are much larger and more diverse than neighborhoods, and while families may choose their neighborhoods with their children's well-being in mind, families choose to live in metropolitan areas for reasons like family history or job opportunities.[6] Measuring neighborhood segregation is also an appealing conceptual approach because it directly captures inequality between neighborhoods in a larger geographic space, rather than just estimating the composition of one's immediate surrounding neighborhood. Higher levels of segregation may induce greater prejudice in a city or inequalities in public goods like school funding (Quillian 2014).

Findings from Experimental and Quasi-Experimental Data

Researchers have also identified neighborhood effects from situations in which a random "shock" to a household induced a move to a different neighborhood. These situations reduce concerns about selection bias—a household's neighborhood changed due to something beyond their control, so the population who experiences a residential change is theoretically no different from the population that did not. Several of these situations have arisen from housing subsidy programs. The MTO experiment was a U.S. Department of Housing and Urban Development (HUD) demonstration in five cities (Baltimore, Boston, Chicago, Los Angeles, and New York) beginning in 1994. MTO randomly assigned families living in public housing to one of three groups: the experimental group, which received a housing voucher to be used in a neighborhood with a poverty rate below 10%; the Section 8 group, which received a standard Section 8

[6] Nonetheless, these studies include statistical methods to test for the possibility of selection bias or reverse causality (that inequality in outcomes between black and white residents could lead to later segregation). Specifically, they use an instrumental variables approach, wherein the analyst identifies a variable theoretically related to the treatment (here, segregation) but not to the outcome. In these studies, researchers use characteristics of the metropolitan area (e.g., railroad tracks, municipal government boundaries) that induce segregation but should not affect residents' outcomes through pathways other than segregation as instrumental variables. Instrumental variables analyses confirm that black young adults who grew up in more highly segregated places have reduced educational and economic outcomes compared to those who grew up in more integrated places.

voucher (now known as a Housing Choice Voucher); and the control group, which received no additional housing assistance aside from their public housing unit.

Participants were surveyed and interviewed periodically over a ten-year period (interim and final impacts are summarized in (Briggs et al. 2010; Goering and Feins 2003)). Examining all children, researchers found no evidence of clear impacts on educational outcomes. However, children in the experimental group who were younger than 13 at the time of random assignment have greater educational attainment, higher income in young adulthood and lower odds of single motherhood compared to the control group (Chetty, Hendren, and Katz 2016). Specifically, compared to the control group, young children whose families used the experimental voucher to move to low-poverty neighborhoods had earnings in young adulthood about 30% higher, were about 30% more likely to go to college, went to higher-quality colleges, and were about 25% less likely to become single mothers (for females) compared to young children in the control group. The larger effects found among children who moved early in childhood demonstrate the importance of neighborhoods as a developmental context for young children and also underscore that the amount of time one spends in a disadvantaged neighborhood has cumulative effects, as some observational studies described above also note. Evidence from the MTO study also indicates long-lasting, intergenerational effects: children whose families received MTO vouchers live in lower-poverty neighborhoods as young adults compared to children from the control group (Chetty et al. 2016; Owens and Clampet-Lundquist 2017).

The Gautreaux Housing Demonstration is another program that induced families' residential mobility. The ACLU initiated a lawsuit against the Chicago Housing Authority (CHA) in 1966 alleging the CHA engaged in racial discrimination by building public housing in neighborhoods with high concentrations of minority residents. The consent decree of the Supreme Court case *Hills v. Gautreaux* (1976) required the CHA to provide vouchers for public housing residents to move to private-sector apartments located in areas with fewer than 30% black residents, with moves occurring from the mid-1970s through the 1990s.[7] While the distribution of vouchers was not random—families had to apply and meet tenancy standards—the CHA placed families in city or suburban locations based on the first available housing. Because families did not select their neighborhood, researchers have analyzed Gautreaux as a "quasi-experimental" program, comparing city and suburban movers. The Gautreaux program produced substantial gains in educational outcomes for children who moved to suburban communities with fewer black residents. Children of suburban movers went to higher-quality schools, received higher grades, and were more likely to attend college than city movers (54% of suburban movers compared to 21% of city movers attended college) (Rubinowitz and Rosenbaum 2000). Like MTO, Gautreaux produced intergenerational effects: children who moved to the suburbs via Gautreaux live as young adults in neighborhoods with lower poverty rates and greater racial integration than their origin neighborhood (Keels 2008). Similar causal evidence comes from Denver, where children's families are quasi-randomly assigned to public housing developments in different neighborhoods. While effect sizes vary by children's race/ethnicity, living in disadvantaged neighborhoods generally reduces adolescents' school performance and educational attainment (Galster et al. 2016).

The weight of the evidence from housing programs that induced neighborhood mobility indicate that children benefit from growing up in more advantaged neighborhoods, especially when they move to these neighborhoods when they are young and experience more time in these contexts. Some research on such social experiments has shown null or mixed effects on children's education (Burdick-Will et al. 2011;

[7] The decree allowed up to 1/3 of vouchers to be used in neighborhoods with higher minority populations, so some families that moved did not live in racially integrated areas.

Fauth, Leventhal, and Brooks-Gunn 2007; Jacob 2004). This may indicate heterogeneous effects: that neighborhood contexts matter for certain groups more than others (e.g., younger children compared to older children, or low-income children compared to high-income children) or that, while a child moved, the move did not induce a change in a meaningful neighborhood characteristic that matters for children's well-being, like local school quality or neighborhood violence.

Measuring Neighborhood Characteristics

Studies of neighborhood effects on children and adolescents measure neighborhood conditions in various ways. Conceptually, these studies typically try to capture the degree of opportunity a neighborhood provides for youths' present and future health, educational, and economic well-being. Researchers, including those cited above, often use measures of socioeconomic well-being, like poverty rate, median household income, unemployment rate, welfare receipt rate, residents' occupational status, residents' educational attainment, or rate of households headed by a single mother, to measure social and institutional resources that can shape children's life chances. Neighborhood violence is another characteristic shown to negatively affect children's educational outcomes (Burdick-Will et al. 2011; Sharkey 2010; Sharkey et al. 2014). A study of New York public school students found that if a violent crime occurred on an African American student's street in the week prior to a standardized test, his probability of passing an English language arts assessment decline by about 3 percentage points, equivalent to about one-fifth of the black-white gap in passing rates (Sharkey et al. 2014)

Neighborhood racial composition correlates strongly with measures of neighborhood socioeconomic status. In their study of all metropolitan Census tracts in the U.S. in 2013, Schwartz, McClure, and Taghavi (2016) show that around 90% of majority-black and majority-Hispanic neighborhoods had high or very high levels of distress, compared to 13% of white neighborhoods. Neighborhood distress is an index based on neighborhoods' poverty rate, rate of female-headed households, unemployment rate, rate of households receiving public assistance, and proportion of adults not in school and without a high school diploma. High or very high levels of distress indicate a neighborhood is in the 4^{th} or 5^{th} quintile of the distress index nationally. Over 70% of majority-black neighborhoods were in the very top quintile of neighborhood distress, compared to just 3 percent of majority-white neighborhoods (62% of majority-Hispanic neighborhoods and 17% of racially integrated neighborhoods were in the top quintile of neighborhood distress).[8] Forty percent of majority-white neighborhoods, compared to 1% or less of majority-black or Hispanic-neighborhoods, were in the lowest neighborhood distress quintile. Therefore, even if researchers do not explicitly include neighborhood racial composition in their measure of neighborhood disadvantage, the two are highly related, with most white neighborhoods having low levels of neighborhood distress and high levels of opportunity and most black and Hispanic neighborhoods having high levels of neighborhood distress and low levels of opportunity (Sharkey 2014). Minority neighborhoods are largely detrimental for children's well-being because of the association between residential racial composition and neighborhood resources, rather than because of racial composition per se (Galster and Santiago 2017).

Even comparing neighborhoods with similar poverty rates, however, black and white neighborhoods differ on other opportunity dimensions. In their analyses of neighborhoods where MTO residents moved, Aliprantis and Kolliner (2015) find that low-poverty black neighborhoods (those with poverty rates below

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[8] Majority-black and majority-Hispanic neighborhoods have over 50% black or Hispanic residents, respectively. Majority-white neighborhoods have over 75% white residents. This measurement difference adjusts for the greater share of white residents in the overall population.

10%, the MTO voucher cutoff) are more similar to high-poverty white neighborhoods than low-poverty white neighborhoods in terms of residents' educational attainment, unemployment rates, and single female-headed households.[9] Neighborhood racial composition is a key dimension of contextual inequality, and it may explain why MTO did not produce as large of effects as many expected, particularly for adults' outcomes. While families in the experimental group moved to neighborhoods with poverty rates below 10%, the neighborhoods were still majority-minority (and many were experiencing rising poverty rates) (Orr et al. 2003). In contrast, the Gautreaux program may have produced larger effects because families moved to neighborhoods that were not racially isolated (Rubinowitz and Rosenbaum 2000).

In addition to differences in their own socioeconomic profiles, black and white neighborhoods differ in their spatial proximity to disadvantaged neighborhoods. In 2000, over 64% of majority-black neighborhoods bordered at least one severely disadvantaged neighborhood, compared to only 8% of majority-white neighborhoods (Sharkey 2014).[10] Even black middle- and upper-class neighborhoods are more likely than white middle- or upper-class neighborhoods to be geographically proximate to lower-income neighborhoods with high levels of violence, social problems, and low-quality institutions (Pattillo-McCoy 2000). In 2000, advantaged black tracts were three times more likely than advantaged white tracts to border at least one severely disadvantaged neighborhood (Sharkey 2014).[11]

Researchers measure socioeconomic and demographic characteristics of neighborhood that capture opportunities that may bear on residents' future outcomes. Neighborhood racial composition is a key correlate of opportunity, given the history of racial segregation and inequality in the United States. Black neighborhoods are not only much more likely to be disadvantaged than white neighborhoods, they are also much more likely to be surrounded by disadvantaged neighborhoods, regardless of their own socioeconomic status.

Conclusion

A large research literature indicates that growing up in socioeconomically disadvantaged, racially segregated neighborhoods is disadvantageous for children's well-being and future life chances. While there is variation in effect sizes depending on the outcome of interest and measurement of neighborhood characteristics, the weight of the evidence strongly indicates negative effects of growing up in impoverished neighborhoods. Some research indicates that neighborhoods may have particularly strong effects for young children who spend more time in these contexts and for lower-income and minority children. Over a century of social science research across methods—qualitative ethnographies, quantitative analyses of observational data, and experimental data—leads to the same overall conclusion that neighborhood disadvantage has deleterious effects on children.

[9] This report is part of a series of commentaries published by the Federal Reserve Bank of Cleveland and may not undergo external peer review.

[10] Neighborhood disadvantage is an index including the neighborhood's rates of welfare receipt, poverty, unemployment, female-headed households, and density of children. Severe disadvantage indicates neighborhoods with disadvantage levels two standard deviations above the mean.

[11] Advantaged tracts are those with neighborhood disadvantage scores (see previous footnote) below the national mean.

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Appendix 1

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Neighborhood Effects in Temporal Perspective: The Impact of Long-Term Exposure to Concentrated Disadvantage on High School Graduation

Geoffrey T. Wodtke,[a] David J. Harding,[a] and Felix Elwert[b]

Abstract

Theory suggests that neighborhood effects depend not only on where individuals live today, but also on where they lived in the past. Previous research, however, usually measures neighborhood context only once and does not account for length of residence, thereby understating the detrimental effects of long-term neighborhood disadvantage. This study investigates effects of duration of exposure to disadvantaged neighborhoods on high school graduation. It follows 4,154 children in the Panel Study of Income Dynamics, measuring neighborhood context once per year from age 1 to 17. The analysis overcomes the problem of dynamic neighborhood selection by adapting novel methods of causal inference for time-varying treatments. In contrast to previous analyses, these methods do not "control away" the effect of neighborhood context operating indirectly through time-varying characteristics of the family; thus, they capture the full impact of a lifetime of neighborhood disadvantage. We find that sustained exposure to disadvantaged neighborhoods has a severe impact on high school graduation that is considerably larger than effects reported in prior research. We estimate that growing up in the most (compared to the least) disadvantaged quintile of neighborhoods reduces the probability of graduation from 96 to 76 percent for black children, and from 95 to 87 percent for nonblack children.

Keywords

neighborhoods, education, causality, marginal structural models, inverse probability of treatment weighting

Contemporary stratification theory posits that exposure to disadvantaged neighborhoods has serious consequences for child educational outcomes (Brooks-Gunn, Duncan, and Aber 1997; Harding 2010; Jencks and Mayer 1990; Massey and Denton 1993; Sampson 2001; Wilson 1987, 1996). Neighborhood effects are central to ecological socialization models, which examine how individuals develop within interconnected social contexts, ranging from families and peer groups to schools, neighborhoods, and communities (Brooks-Gunn et al. 1993). Growing up in a disadvantaged neighborhood is thought to negatively affect educational outcomes because

[a]University of Michigan
[b]University of Wisconsin–Madison

Corresponding Author:
Geoffrey T. Wodtke, Population Studies Center, University of Michigan, 426 Thompson Street, Ann Arbor, MI 48106-1248
E-mail: wodtke@umich.edu

of social, cultural, and linguistic isolation; a breakdown of collective cohesion among residents; scarce institutional resources; and environmental health hazards.

Empirical research, however, has produced mixed results regarding effects of neighborhoods on educational attainment, often finding small effects, and sometimes finding no effects at all (Jencks and Mayer 1990; Sampson, Morenoff, and Gannon-Rowley 2002; Small and Newman 2001). For example, Brooks-Gunn and colleagues (1993) find no effect of neighborhood income on high school graduation among blacks and only small effects among nonblacks. Similarly, Ginther, Haveman, and Wolfe (2000) find few significant effects of neighborhood context on high school graduation after adjusting for a wide range of family characteristics. Other studies document negative effects of disadvantaged neighborhoods on educational attainment, but these effects are typically small and often quite sensitive to the particular contextual measures used in the analysis (Aaronson 1997; Brooks-Gunn et al. 1997; Crane 1991; Harding 2003).

These mixed results, we contend, may be due at least in part to a set of interrelated problems regarding the role of time in neighborhood-effects research (Crowder and South 2011; Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert 2011; South and Crowder 2010; Timberlake 2007; Turley 2003; Wilson 2009). First, although theories of neighborhood effects all specify mechanisms based on long-term exposure to disadvantaged neighborhoods, most previous studies measure neighborhood context only once or over just a short period (e.g., Brooks-Gunn et al. 1993; Harding 2003). This measurement choice conflates children who were recently exposed to disadvantaged neighborhoods with those who experienced long-term residential disadvantage. To the extent that neighborhood effects are lagged or cumulative, estimates based on point-in-time measurements of neighborhood context may substantially understate the effect of sustained neighborhood disadvantage. Second, because neighborhoods are not a static feature of a child's life—many families move in and out of different communities or remain in areas that change around them (Briggs and Keys 2009; Quillian 2003; Timberlake 2007)—estimating neighborhood effects poses difficult methodological problems that are rarely addressed in empirical research. The central challenge is that selection into different neighborhood contexts across time is based in part on time-varying characteristics of the family, such as parental employment status, parental marital status, and family income (Quillian 2003; Sampson and Sharkey 2008; South and Crowder 1997a), that are themselves influenced by previous neighborhood conditions (Wilson 1987, 1996). Prior studies, however, rely almost exclusively on conventional regression models that mishandle this dynamic neighborhood selection process and "control away" indirect effects of neighborhoods that operate through time-varying family characteristics. Such over-control of indirect pathways may further understate the effect of long-term neighborhood disadvantage.

Building on previous work investigating the temporal dimensions of neighborhood effects (Crowder and South 2011; Jackson and Mare 2007; Sampson et al. 2008; Sharkey and Elwert 2011), this study extends research on neighborhood context and child development by (1) measuring duration of exposure to disadvantaged neighborhoods throughout childhood and adolescence, (2) explicitly defining neighborhood effects within a counterfactual causal framework for time-varying exposures, and (3) using novel statistical methods of adjusting for dynamic neighborhood selection that overcome critical shortcomings of conventional regression. Specifically, we estimate effects of sustained exposure to different levels of neighborhood disadvantage on high school graduation, a central dimension of social stratification (Rumberger 1987). Because educational attainment is one of the most extensively studied outcomes in neighborhood-effects research (e.g., Aaronson 1997; Brooks-Gunn et al. 1993; Crane 1991; Crowder and South 2011; Ginther et al. 2000; Harding 2003), we can compare our estimates, which take exposure duration and dynamic neighborhood selection into account, with results of past analyses that neglect these issues.

We begin by reviewing the theoretical mechanisms through which long-term exposure to disadvantaged neighborhoods is thought to affect educational attainment. Next, we review past estimates of neighborhood effects, focusing on limitations of static models and point-in-time measurements, and delineate the dynamic neighborhood selection process. Following this discussion, we specify counterfactual models for the longitudinal effects of neighborhood disadvantage on high school graduation, explain the challenges that dynamic selection processes pose for the estimation of duration-dependent neighborhood effects, and describe procedures to estimate these effects using inverse probability of treatment weights. Then, with data from the Panel Study of Income Dynamics, we follow a cohort of children from birth through early adulthood, measuring neighborhood context once per year, every year, for 17 years, and estimate effects of sustained exposure to different levels of neighborhood disadvantage on high school graduation.

Results of this analysis indicate that exposure to disadvantaged neighborhoods throughout the early life course has a severe negative impact on the chances of high school graduation for black and nonblack children. These effects are considerably larger than estimates from prior research and appear to be mediated by time-varying characteristics of the family. In other words, our findings suggest that neighborhood effects on children operate in part through neighborhood effects on parents. We conclude that a temporal framework is essential for understanding the deleterious effects of disadvantaged neighborhoods on child development.

NEIGHBORHOOD MECHANISMS AND EXPOSURE DURATION

Failure to graduate from high school is the result of a cumulative process of academic and social disengagement that unfolds over time (Rumberger 2004). The proximate determinants of disengagement from school include low educational aspirations, poor academic performance, absenteeism, behavioral problems and delinquency, parenthood, and family economic demands (Cairns, Cairns, and Neckerman 1989; Ensminger and Slusarcick 1992; Rumberger 1987, 2004). Theoretical models of neighborhood effects on educational attainment describe mechanisms through which local communities affect the proximate determinants of school engagement. These theories can be broadly classified into four categories: those based on social isolation, social organization, neighborhood resources, and the physical environment.

Social isolation theories argue that residents of poor neighborhoods are isolated from social networks and institutions that provide access to job information and important links to mainstream culture. As a result, adults in such neighborhoods fail to provide role models that encourage success in school for local children (Jencks and Mayer 1990; Wilson 1987, 1996). According to Wilson (1987), social isolation from mainstream institutions, particularly the labor market, leads to the development of "ghetto-specific" cultural repertoires. Similarly, other researchers contend that social isolation gives rise to alternative or oppositional cultures, which emerge in response to structural constraints on upward social mobility (Anderson 1999; Massey and Denton 1993). According to this perspective, deviant subcultures that emerge in disadvantaged neighborhoods devalue formal schooling and valorize risky behaviors that may lead to poor educational outcomes. Linguistic isolation is another potential consequence of social isolation in disadvantaged neighborhoods. Black children raised in poor, racially segregated neighborhoods are more likely to speak Black English Vernacular, which can impede success in school because it is devalued by mainstream institutions (Massey and Denton 1993). To the extent that disadvantaged, socially isolated neighborhoods affect children's educational outcomes by socializing them into deleterious attitudinal and behavioral patterns, sustained exposure is required for local values and behaviors to become sufficiently internalized.

Social organization theories of neighborhood effects emphasize the role of social cohesion among neighbors and its impact on regulation of crime and other deviant behavior (Sampson et al. 2002). Neighborhood poverty is linked to a breakdown of mutual trust among resident adults, which hampers their ability to monitor and control youth behavior (Sampson 2001). Lacking collective supervision, children in disadvantaged neighborhoods may be more likely to engage in deviant behaviors that disrupt progression through school (Browning et al. 2008; Browning, Leventhal, and Brooks-Gunn 2005). Moreover, exposure to violent crime that results from social disorganization may have a number of harmful effects on cognitive and emotional development through, for example, maladaptive biological responses to stress (Harding 2009; Massey 2004). The cumulative risk of victimization necessarily increases with one's duration of residence in high-crime neighborhoods, and harmful biological processes resulting from stress require sustained exposure. Similarly, the chances that children engage in behaviors that disrupt progression through school increase with the amount of time they spend in social environments that provide few deterrents to such behaviors. Thus, social organization theories also suggest the importance of long-term exposure to disadvantaged neighborhoods.

Resource theories of neighborhood effects contend that poor neighborhoods lack important institutional resources, such as quality schools, daycare centers, grocery stores, pharmacies, and recreational areas, that promote child development and academic achievement (Brooks-Gunn et al. 1997; Small and Newman 2001; Wilson 1987). In the United States, the quality of a child's school environment is often directly linked to neighborhood socioeconomic conditions because public school funding is geographically determined. School quality is likely a primary mechanism through which neighborhood context affects educational outcomes, and research suggests it is important to account for the length of time that children spend in different school environments (Halpern-Manners, Warren, and Brand 2009). Resource deprivation in poor neighborhoods also makes effective parenting more difficult. For example, children of working parents may be left unsupervised for longer periods of time in neighborhoods with fewer recreational programs or daycare centers. Without such institutions, disadvantaged neighborhoods provide less deterrence against problem behaviors that interfere with schooling. Because the harmful consequences of resource deprivation for child development likely compound over time, sustained exposure to disadvantaged neighborhoods ought to have a more severe impact on school progression than would transitory exposure.

Environmental theories of neighborhood effects focus on the poor physical condition of disadvantaged neighborhoods (Crowder and Downey 2010). Much research in this tradition focuses on health outcomes (Schulz et al. 2005). Because of their proximity to major freeways and industrial centers, impoverished urban neighborhoods are disproportionately exposed to air pollution, which has a negative impact on residents' health (Ponce et al. 2005). Poor neighborhoods also contain dilapidated housing, which can affect residents' health through exposure to indoor allergens, toxins, and structural hazards (Rosenfeld et al. 2010). Health problems resulting from residence in disadvantaged neighborhoods may impede timely progression through school. For example, exposure to air pollutants is a major risk factor for asthma, which in turn is associated with school absences (Clark et al. 2010; Moonie et al. 2006). The extent to which individuals are harmed by environmental health hazards depends on the length and severity of exposure (Schwartz 2006), providing yet another reason to consider the amount of time that children reside in different neighborhood contexts. In summary, all four broad theories of neighborhood effects on educational outcomes involve a temporal dimension. Empirical research thus requires careful attention to duration of exposure.

PREVIOUS ESTIMATES OF NEIGHBORHOOD EFFECTS

Previous research on neighborhood effects has used both experimental and observational designs. Experimental studies randomly assign subjects to treatment and control groups and thus avoid the problem of selection bias because random assignment should form groups of subjects identical on all factors, whether observed or not, except for the exposure of interest. Observational studies, on the other hand, estimate effects of neighborhood context from data in which subjects self-select into different neighborhoods.

The most comprehensive neighborhood experiment to date is the Moving to Opportunity (MTO) study, which randomly assigned low-income residents of poor inner-city neighborhoods to receive Section 8 housing vouchers, enabling a subset of participants to move into more affluent suburban neighborhoods. Neighborhood-effect estimates from the MTO experiment are mixed. Early evaluations indicated that children who moved into low-poverty neighborhoods had significantly better developmental outcomes, including higher test scores, fewer behavioral problems, and better mental health (Ladd and Ludwig 1997; Ludwig, Duncan, and Hirschfield 2001). However, a second round of evaluations conducted four to seven years after the intervention revealed few significant differences between treatment and control groups (Ludwig et al. 2008).

Although the MTO experiment contributed important evidence about neighborhood effects on children, it is not without limitations, some of which are particularly relevant to the present study. First, the MTO study provides conservative estimates of neighborhood effects because families who received housing vouchers often moved to highly segregated, mostly black neighborhoods that were only slightly less poor than the neighborhoods in which they lived previously (Clampet-Lundquist and Massey 2008; Sampson 2008). Second, because families in the treatment group had to move to live in neighborhoods with lower poverty rates, results from the MTO study confound effects of neighborhood poverty and residential mobility. Third, both the treatment and control groups were, in fact, exposed to high-poverty neighborhoods for some period of time. Only families from disadvantaged neighborhoods were eligible for the study, so all parents and children who subsequently moved to less disadvantaged suburbs were exposed to high-poverty neighborhoods prior to the intervention. Evaluations of MTO, therefore, compare treatment and control groups that may be quite similar in terms of cumulative exposure to neighborhood poverty. If the impact of neighborhood poverty on child development is cumulative or lagged, then estimates from MTO fail to capture the total effect of sustained exposure to disadvantaged neighborhoods.

Neighborhood-effect estimates from observational studies are also at times contradictory or inconclusive (e.g., Aaronson 1997, 1998; Brooks-Gunn et al. 1993; Evans, Oates, and Schwab 1992; Ginther et al. 2000; Harding 2003; Jencks and Mayer 1990; Sampson et al. 2002; Small and Newman 2001). A serious limitation of previous observational research is the near exclusive reliance on static models and short-term measurements of neighborhood context. Because children move between different neighborhood environments, point-in-time measures cannot capture the time-varying sequence of neighborhood conditions that children experience throughout the early life course (Briggs and Keys 2009; Quillian 2003; Timberlake 2007). By mixing children who were recently exposed with those who lived in disadvantaged neighborhoods for an extended period, previous studies that use static models and point-in-time measurements of neighborhood context likely underestimate effects of long-term exposure.

Several prior studies have attempted to assess the temporal dimension of neighborhood effects. Jackson and Mare (2007) and Crowder and South (2011) analyze effects of neighborhood characteristics averaged over many years during childhood (e.g., from birth to age 18) on a variety of developmental outcomes but ultimately reach opposite conclusions about the importance of longitudinal measurement. Both of these studies, however, use conventional regression-based methods, which do not properly account for the dynamic selection of families into different neighborhood contexts and therefore likely understate, perhaps severely, effects of cumulative exposure—a point we explain in greater detail below (see also Kunz, Page, and Solon 2003; South and Crowder 2010). Another study reanalyzed MTO data, taking into account the amount of time that subjects were exposed to different neighborhood contexts in the years following the intervention (Clampet-Lundquist and Massey 2008). Duration-weighted estimates from this analysis suggest a more substantial influence for neighborhood context on adult economic self-sufficiency, yet these results still do not capture the impact of lifetime neighborhood conditions. Sampson and colleagues (2008), using estimation methods that provide for improved adjustment of confounding when selection is time-varying, find that past exposure to disadvantaged neighborhoods has a severe negative effect on children's verbal ability measured years later, but because this study is based on longitudinal data with only two follow-up waves, the full impact of sustained exposure to disadvantaged neighborhoods throughout childhood could not be assessed.

DYNAMIC NEIGHBORHOOD SELECTION

Previous research on residential mobility and spatial attainment (e.g., Quillian 2003; Sampson and Sharkey 2008; South and Crowder 1997a, 1997b, 1998a, 1998b) foreshadows a central methodological problem for estimating effects of extended exposure to neighborhood disadvantage: the dynamic selection of families into and out of different neighborhood environments, in which determinants of future residential choices are themselves affected by past neighborhood conditions. To estimate longitudinal effects of neighborhood disadvantage, knowledge of the neighborhood selection process is critical (Sharkey and Elwert 2011).

Research on determinants of neighborhood context emphasizes the role of the life course, family structure, and socioeconomic characteristics. Marital status, age, and family size are all linked to neighborhood socioeconomic characteristics (Sampson and Sharkey 2008; South and Crowder 1997b, 1998a; South and Deane 1993; Speare and Goldscheider 1987). Neighborhood attainment is also related to education, income, employment, receipt of public assistance, and homeownership; individuals who have a more advanced education, work regularly, earn higher incomes, do not receive public assistance, and own rather than rent their dwellings are more likely to live in non-poor neighborhoods (Sampson and Sharkey 2008; South and Crowder 1997a, 1997b, 1998a). In addition, longitudinal studies of neighborhood mobility indicate that abrupt changes to the family environment predict future neighborhood attainment. For example, parents who have recently divorced or become unemployed are at greater risk of moving to poor neighborhoods (Sampson and Sharkey 2008; South and Crowder 1997a).

Race is another important determinant of neighborhood attainment, as it constrains residential choices for minorities, especially blacks (Charles 2003; Massey and Denton 1993). Audit studies and survey experiments indicate that blacks face extensive discrimination from realtors, lenders, and white neighbors (Charles 2003; Yinger 1995). Because of extreme residential segregation, neighborhood selection processes operate differently for whites and blacks. A number of studies show that blacks have much more difficulty converting personal resources into improved neighborhood conditions, and blacks of all income levels are more likely to live in high-poverty neighborhoods than are comparable whites (Charles 2003; Iceland and Scopilliti 2008; Massey and Denton 1993). Moreover, blacks are less likely than whites to move, and when blacks do change residences, they are less likely to improve their neighborhood conditions (South and Deane 1993).

Research on spatial attainment therefore shows that a variety of demographic and economic factors are important determinants of residence in different neighborhood environments. Conversely, theory and research also indicate that past neighborhood context in turn affects many of the same characteristics that influence future neighborhood selection. For example, spatial mismatch theories contend that poor neighborhoods are located far from areas with employment opportunities at appropriate skill levels, making it difficult for residents to acquire and maintain jobs (Fernandez and Su 2004; Wilson 1987, 1996). Furthermore, Wilson (1987) argues that the decline of manufacturing during the 1970s diminished the pool of marriageable men (i.e., employed men with income sufficient to support a family) in urban black neighborhoods, leading to delayed marriage and increasing non-marriage among this population.

A number of family characteristics may thus simultaneously confound and mediate the effects of disadvantaged neighborhoods on children's educational outcomes. That is, certain time-varying characteristics of the family, such as parental employment and marital status, affect both children's educational attainment and the chances of living in different neighborhood environments and are in turn affected by prior neighborhood conditions. As we explain below, time-varying confounders affected by past neighborhood context pose unique methodological challenges for estimating neighborhood effects that cannot be addressed with conventional regression models.

METHODS

Data

This study uses data from the Panel Study of Income Dynamics (PSID) and the GeoLytics Neighborhood Change Database (NCDB) (GeoLytics 2003). The PSID is a longitudinal study of families who were first interviewed in 1968. PSID core respondents consist of an equiprobable sample of approximately 2,800 households from the contiguous United States together with a sample of about 2,000 low-income households selected from Standard Metropolitan Statistical Areas (SMSAs) in the North and non-SMSAs in the South. The PSID conducted annual interviews of core family units (FUs) and new families formed by core FU members from 1968 to 1997; interviews were conducted biennially thereafter.

The analytic sample for this study consists of the 4,154 children present at age 1 year in PSID core FUs between 1968 and 1978. We gather information on these children for every year until age 20 or they are lost to follow-up.[1] Two thousand and ninety-three children—834 black and 1,259 nonblack—are continuously present in a responding PSID FU from age 1 to 17 years and are present in, or reported on by, a PSID FU at age 20 (we describe methods used to adjust for sample attrition below). The PSID wave, indexed by k, in which a child is age 1 year defines the baseline time period ($k = 0$) when neighborhood context and a rich set of covariates are first measured. We then measure neighborhood context and all time-varying covariates once per year from age 2 to 17 years. Baseline neighborhood context is not used to estimate neighborhood effects but rather is absorbed into the vector of control variables measured at age 1. Thus, our study uses $K = 16$ post-baseline waves of follow-up—each of the waves in which children are between ages 2 and 17 years—to estimate educational effects of neighborhood context. The outcome of interest in this study, high school graduation, is measured at age 20, defined as the end of follow-up.[2]

Treatment

Measurements of neighborhood context come from the NCDB, which contains nationwide tract-level data from the 1970 to 2000 United States Censuses with variables and tract boundaries defined consistently across time.

Tract data for intercensal years are imputed using linear interpolation. We use principal component analysis to generate a composite score of neighborhood disadvantage based on seven tract characteristics: poverty, unemployment, welfare receipt, female-headed households, education (i.e., percent of residents age 25 years or older without a high school diploma; percent of residents age 25 or older with a college degree), and occupational structure (i.e., percent of residents age 25 or older in managerial or professional occupations).[3] We then divide census tracts into quintiles based on the national distribution of the composite disadvantage score and create a time-varying ordinal treatment variable, coded 1 through 5, that records the neighborhood quintile in which a child resides at each wave. The first quintile contains the least disadvantaged census tracts, and the fifth quintile contains the most disadvantaged census tracts. For example, in the average first quintile tract, less than 5 percent of residents are poor, only 2 percent receive welfare, and about 40 percent of adults are college graduates. By contrast, in the average fifth quintile tract, nearly 30 percent of residents are poor, about 19 percent receive welfare, and less than 50 percent of adults have graduated from high school (see Part A of the online supplement for details). In the analysis below, we use this ordinal wave-specific treatment variable to compute a measure of duration-weighted exposure to different levels of neighborhood disadvantage between ages 2 and 17 years.[4]

Covariates

This study includes an extensive set of covariates to control for potential confounding of neighborhood effects on high school graduation. The time-invariant baseline covariates are race, gender, birth weight, mother's age at birth, mother's marital status at birth, and FU head's education.[5] Race is coded 1 for black and 0 for nonblack; gender is coded 1 for female and 0 for male; birth weight is expressed as a dummy variable equal to 1 if the child was less than 2,500 grams at birth and 0 otherwise; mother's age at birth is measured in years; and a dummy variable is used to indicate whether the mother was married at the time of childbirth. FU head's education is expressed as a series of dummies for "less than high school," "high school graduate," and "at least some college." The time-varying covariates in this analysis, measured at each wave k, include marital status, employment status, and work hours of the FU head, as well as family size, homeownership, receipt of Aid to Families with Dependent Children (AFDC), total family income, and residential mobility. Marital status is coded 1 for married and 0 for unmarried; employment status is coded 1 for employed and 0 for not employed; and work hours is equal to the average number of hours worked per week during the preceding year. Family size is defined as the total number of people present in a child's family at wave k; homeownership is expressed as a dummy variable indicating whether the family owned the residence they occupied at the time of the interview; and AFDC receipt is coded 1 if a family received AFDC income during the past year and 0 otherwise. Total family income is measured as the sum of taxable income the family head, partner, and other FU members earned over the past year, inflated/deflated using the Consumer Price Index (CPI-U) to 1990 dollars. We define residential mobility as the total number of times a child moved prior to wave k. For all variables, we use multiple imputation to fill in missing values due to item-specific nonresponse.[6]

Counterfactual Models for Time-Varying Neighborhood Exposures

This study relies on potential outcomes notation to define causal effects of time-varying neighborhood exposures on high school graduation (Holland 1986; Robins, Hernán, and Brumback 2000; Rubin 1974). Let $A_k \in \{1,2,\ldots,5\}$ represent neighborhood exposure status at the kth wave since start of follow-up, where $A_k = 1$ denotes residence in the least disadvantaged quintile of neighborhoods and $A_k = 5$ denotes residence in the most disadvantaged quintile. The sequence of neighborhood contexts experienced by a child through wave k is written as $\bar{a}_k = (a_1, ..., a_k)$, and $\bar{a} = \bar{a}_K$ represents a child's complete treatment trajectory from age 2 to 17 years (overbars in this notation signify covariate history). Let Y be the observed outcome equal to 1 for children who graduated from high school by age 20 and 0 for those who did not. $Y_{\bar{a}}$, then, is the potential outcome indicating whether a child would have graduated from high school had she been exposed to the sequence of neighborhood contexts $\bar{a}$, possibly contrary to fact. For example, $Y_{(5,5,...,5)}$ is a child's outcome had she been continuously exposed to the most disadvantaged quintile of neighborhoods, $Y_{(4,5,...,5)}$ is the child's outcome had she been exposed to a 4th-quintile neighborhood during the first follow-up wave and neighborhoods in the most disadvantaged quintile thereafter, and so on. The observed outcome, Y, equals the potential outcome, $Y_{\bar{a}}$, for the one exposure trajectory the child did in fact experience; all other $Y_{\bar{a}}$ are not observed (i.e., counterfactual).

The average causal effect of some specific neighborhood exposure trajectory $\bar{a}$ compared to another exposure trajectory $\bar{a}'$ is defined as the expected difference between corresponding potential outcomes,

$$\begin{aligned} E(Y_{\bar{a}} - Y_{\bar{a}'}) &= E(Y_{\bar{a}}) - E(Y_{\bar{a}'}) \\ &= P(Y_{\bar{a}} = 1) - P(Y_{\bar{a}'} = 1), \end{aligned} \quad (1)$$

where $P(Y_{\bar{a}} = 1)$ is the probability of high school graduation had all children experienced the neighborhood exposure trajectory $\bar{a}$, and $P(Y_{\bar{a}'} = 1)$ is the analogous probability of high school graduation if all children had experienced the exposure sequence $\bar{a}'$. Because the same individual cannot simultaneously be exposed to two different treatment trajectories, the effects of interest are impossible to observe directly and must be estimated. In principle, effects of neighborhood context could be analyzed non-parametrically by comparing expectations, as in Equation 1, for all possible exposure trajectories. However, with a five-level ordinal treatment and $K = 16$ waves of follow-up, there are 5^{16}, or more than 150 billion, possible exposure trajectories and the same number of potential outcomes. Data limitations thus force the imposition of simplifying functional form assumptions about the exposure-outcome relationship.

To investigate effects of sustained exposure to different neighborhood contexts on high school graduation, we specify the following parametric model for counterfactual probabilities:

$$logit(P(Y_{\bar{a}} = 1)) = \theta_0 + \theta_1 \left(\sum_{k=1}^{16} a_k / 16 \right). \quad (2)$$

Equation 2 is called a *marginal structural model* (MSM) because it models the *marginal* distribution of potential outcomes and because causal models are referred to as *structural* in the treatment-effects literature (Robins 1999; Robins et al. 2000). In this model, the probability of high school graduation is a function of duration-weighted exposure to different levels of neighborhood disadvantage (i.e., the average of ordinal wave-specific treatments from wave $k = 1$ to 16). The log odds ratio θ_1 captures the effect of growing up in neighborhoods that are, on average, located in quintile q of the composite disadvantage distribution rather than the less disadvantaged quintile $q - 1$. This parsimonious specification allows for contrasts between exposure trajectories of key theoretical interest, for example, between children who spend their entire childhood in the least disadvantaged quintile of neighborhoods and children with long-term exposure to neighborhoods in the most disadvantaged quintile.[7]

The causal effect defined earlier can be identified from observational data if the level of neighborhood disadvantage at each wave k is independent of potential outcomes given observed covariate history and past treatments (Robins 1987, 1999). This condition is expressed formally as

$$Y_{\bar{a}} \perp A_k \mid \bar{L}_k, \bar{A}_{k-1}, \quad (3)$$

where $\bar{L}_k = (L_0, ..., L_k)$ represents observed covariate history up to wave k, $\bar{A}_{k-1}$ encodes treatment history through the prior wave, and

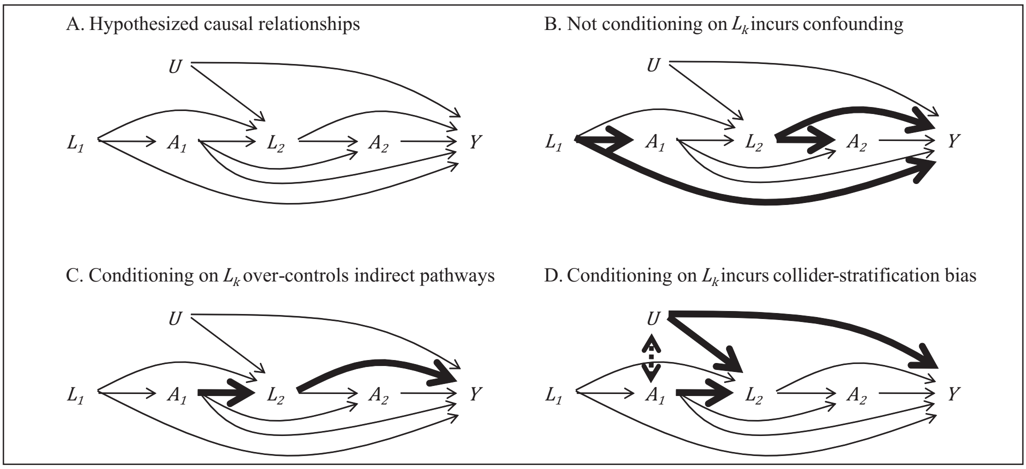

Figure 1. Causal Graphs for Exposure to Disadvantaged Neighborhoods with Two Waves of Follow-up

Note: A_k = neighborhood context, L_k = observed time-varying confounders, U = unobserved factors, Y = outcome.

$\perp$ denotes statistical independence. Substantively, this says that children with the same combination of observed covariate values do not systematically select into different neighborhood contexts based on factors predictive of the outcome. Condition 3 is satisfied if there are no unobserved covariates that affect both neighborhood exposure status and high school graduation (i.e., if there is no unobserved confounding of treatment).

Failure of Conventional Regression Estimators

To understand the limitations of conventional regression models for estimating effects of time-varying neighborhood exposures, consider the simplified two-wave example depicted in Figure 1A. This figure contains a directed acyclic graph (Pearl 1995, 2000) that shows the hypothesized causal relationships between neighborhood disadvantage, time-varying characteristics, high school graduation, and unobserved factors. All arrows between the temporally ordered variables represent direct causal effects, and the absence of an arrow indicates there is no causal effect. In Figure 1A, neighborhood selection is affected by prior time-varying covariates, and neighborhood context in turn affects future time-varying factors. Exposure to neighborhood disadvantage at each wave, then, has a direct effect on high school graduation and also an indirect effect that operates through future levels of observed time-varying covariates. Note that we permit the existence of unobserved factors that directly affect time-varying covariates and the outcome but do not affect treatment.[8] In other words, there is no unobserved confounding of treatment, and following from Equation 3, the causal effect of any neighborhood exposure trajectory on high school graduation is therefore identifiable from the observed data.

Given this set of relationships, suppose that our goal is to estimate the effect of duration-weighted exposure to neighborhood disadvantage on high school graduation. The problem with conventional regression models is their inability to properly handle time-varying confounders affected by past treatment, specifically, L_2. As Figure 1B highlights, L_2 is a confounder of treatment at wave $k = 2$ and thus must be controlled for. However, conditioning on L_2 in a conventional regression model (i.e., including L_2 as a regressor) creates two distinct problems. First, Figure 1C shows that time-varying confounders measured at the second follow-up wave, L_2, are on the causal pathway from past treatment, A_1, to high school graduation, Y. Thus, conditioning on L_2 will remove from our treatment-effect estimate the indirect effect of past treatment, which operates through future time-varying factors. The neighborhood-effects literature refers to this problem as over-control of indirect pathways (see Sampson et al. 2002). Figure 1D depicts the second problem with regression adjustments for time-varying confounders: L_2 is a "collider" variable; that is, L_2 is a common effect of unobserved factors, U, and prior exposure status, A_1. Conditioning on a collider necessarily induces an association between its common causes, in this case, unobserved factors and prior treatment, as illustrated by the dashed arrow in Figure 1D (Pearl 1995, 2000). Because unobserved factors also affect high school graduation, conditioning on L_2 creates a new biasing path for the effect of past treatment. This problem is called collider-stratification bias in the literature on causal inference (see Greenland 2003).

This two-wave example demonstrates that conventional regression models cannot consistently estimate effects of a time-varying treatment when time-varying confounders are affected by past treatments, *even if there is no unobserved confounding* (i.e., no direct arrow from U into A_k). In this situation, both an unadjusted regression model that does not condition on time-varying confounders and an adjusted regression model that does condition on these factors yield biased estimates of the desired treatment effect. Alternative methods are thus needed to adjust for dynamic neighborhood selection.

Estimation Using Inverse Probability of Treatment Weights

Inverse probability of treatment (IPT) weighting is an alternative approach specifically developed to adjust for confounding by time-varying covariates (Robins 1999; Robins et al. 2000).[9] This method has important advantages over conventional regression models because it resolves the problems outlined in the previous section without making additional assumptions about the dynamic selection process. Intuitively, the method involves weighting observations to generate a pseudo-population in which treatment is no longer confounded by measured covariates. An unadjusted model for the observed outcome can then be fit to the weighted pseudo-population to obtain unbiased and consistent treatment-effect estimates under assumptions described below.

The IPT weight for the ith child is given by

$$w_i = \prod_{k=1}^{K} \frac{1}{P(A_k = a_{ki} \mid \bar{A}_{k-1} = \bar{a}_{(k-1)i}, \bar{L}_k = \bar{l}_{ki})}. \quad (4)$$

The denominator of the weight is the probability that a child was exposed to her actual neighborhood quintile at wave k conditional on past treatment and confounders. At each wave, IPT weighting "balances" treatment assignment across prior confounders by giving more (or less) weight to children with covariate histories that are underrepresented (or overrepresented) in their current treatment group. Figure 2 illustrates the effect of weighting by w_i in our simplified two-wave example. In the weighted pseudo-population, treatment at each wave is independent of prior confounders; that is, exposure to different neighborhood contexts behaves as if it were sequentially randomized with respect to prior observed covariates. Conditioning on confounder history, therefore, is no longer necessary, and an unadjusted model for the observed outcome can be fit to the weighted observations to estimate the treatment effects of interest.

In practice, the weights defined in Equation 4 yield imprecise effect estimates with non-normal sampling distributions. To improve the formal properties of our estimates, we use stabilized IPT weights,

$$sw_i = \prod_{k=1}^{K} \frac{P(A_k = a_{ki} \mid \bar{A}_{k-1} = \bar{a}_{(k-1)i}, L_0 = l_0)}{P(A_k = a_{ki} \mid \bar{A}_{k-1} = \bar{a}_{(k-1)i}, \bar{L}_k = \bar{l}_{ki})}, \quad (5)$$

which are less variable than w_i and are centered around 1. Estimates based on stabilized IPT weights have smaller variance and an

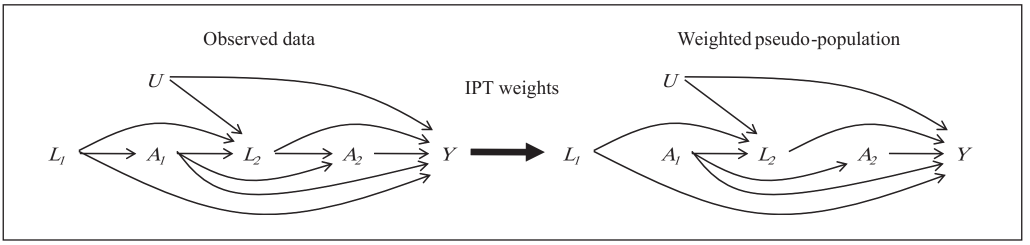

Figure 2. Causal Graphs for the Effect of Weighting by the Inverse Probability of Treatment (IPT)
Note: A_k = neighborhood context, L_k = observed time-varying confounders, U = unobserved factors, Y = outcome.

approximately normal sampling distribution (Hernán, Brumback, and Robins 2002; Robins et al. 2000). Because confounders measured at baseline are included in both the numerator and denominator of the stabilized weight, the outcome model fit to the weighted pseudo-population must condition on these factors to obtain unbiased estimates of the desired treatment effects. Note that regression adjustments for baseline covariates do not suffer the limitations described in the previous section.

Because the true IPT weights are unknown, they must be estimated from the data. We estimate the denominator in Equation 5 from an ordinal logistic regression model for the probability of exposure to different levels of neighborhood disadvantage. Specifically, the probability of treatment at each wave is modeled as a function of treatment status at wave $k-1$, covariates measured at baseline (including baseline treatment status), time-varying covariates measured at wave k and wave $k-1$, and a flexible dummy specification for birth year and age. We also include interactions between measures of marital status and employment status at waves k and $k-1$ to permit unique effects for recent divorce and job loss on neighborhood selection. We compute treatment probabilities in the numerator of the stabilized weight from a constrained version of the denominator model that excludes time-varying covariates. We estimate all models separately by race because prior research on spatial attainment suggests that neighborhood selection processes differ for blacks and nonblacks (Charles 2003; Iceland and Scopilliti 2008; Massey and Denton 1993; South and Deane 1993).[10] Part B of the online supplement reports coefficient estimates from the treatment models.

To demonstrate the importance of using methods that can properly adjust for dynamic neighborhood selection, we compute unadjusted, regression-adjusted, and stabilized IPT-weighted estimates for the effect of duration-weighted exposure to neighborhood disadvantage on high school graduation, separately by race. Unadjusted estimates come from conventional logit models fit to the observed data in which the probability of high school graduation is a function of only duration-weighted exposure to neighborhood disadvantage. The regression-adjusted estimates are from logit models that condition on duration-weighted exposure, baseline covariates, and time-varying covariates averaged over ages 2 to 17 years. Stabilized IPT-weighted estimates are based on logit models of high school graduation fit to the weighted pseudo-population. We use Huber-White robust standard errors to account for clustering of siblings within families.[11]

The IPT-weighted estimator is unbiased and consistent under assumptions of no unmeasured confounders, no model misspecification, and positivity (i.e., there is a nonzero probability of treatment for every level and combination of confounders) (Cole and Hernán 2008; Robins et al. 2000). These are strong assumptions, but they are the same assumptions required to make causal inferences about time-varying treatments using conventional regression methods. Regression-adjusted estimators, however, require the additional assumption that observed time-varying confounders are not affected by past treatment. This assumption, which is untenable in neighborhood-effects research, is not necessary when estimating MSMs using IPT weights.

Sample Attrition

In our analytic sample, some children drop out of the PSID before age 20 and are said to be lost to follow-up. Of the 4,154 children present at baseline, 2,093 remain in the study continuously until age 20. To correct for potential nonrandom attrition, we use stabilized weights analogous to those derived for selection into treatment, but now the weights adjust for the differential probability of remaining in the study through the end of follow-up (Robins et al. 2000). Let C_k be a binary variable equal to 1 if a child drops out of the study at wave k and 0 otherwise. The stabilized weight that adjusts for nonrandom attrition based on observed covariates is given by

$$cw_i = \prod_{k=1}^{K} \frac{P(C_k = 0 \mid \bar{C}_{k-1} = 0, \bar{A}_{k-1} = \bar{a}_{(k-1)i}, L_0 = l_0)}{P(C_k = 0 \mid \bar{C}_{k-1} = 0, \bar{A}_{k-1} = \bar{a}_{(k-1)i}, \bar{L}_k = \bar{l}_{ki})}, \quad (6)$$

where $\bar{C}_{k-1} = 0$ denotes that a subject remained in the study through wave $k - 1$. Similar to IPT weights, we estimate stabilized attrition weights from logistic regression models for the probability of leaving the study at each follow-up wave (results not shown). The effect estimates reported below are computed exactly as outlined above, except we weight observations by the product of the stabilized IPT weight and the stabilized attrition weight ($cw_i \times sw_i$).

RESULTS

Sample Characteristics

Tables 1 and 2 display descriptive statistics for the time-invariant and time-varying covariates used in this analysis. A comparison between black and nonblack children reveals considerable racial differences: on the majority of measured characteristics, black children were substantially more disadvantaged than nonblack children. Black children were more likely than nonblack children to be part of a family unit in which the head was unmarried, unemployed, and worked fewer than 40 hours per week. The average black child also lived in a family with lower income and a greater number of family members.

Perhaps the most staggering disparity between black and nonblack children is their different rates of exposure to disadvantaged neighborhoods. For example, among blacks, 68.71 percent lived in the most disadvantaged quintile of neighborhoods at age 10, while a mere 3.60 percent lived in the least disadvantaged neighborhoods. By contrast, only 14.93 percent of nonblack children lived in the most disadvantaged neighborhoods at age 10, and 19.14 percent lived in the least disadvantaged neighborhoods. Moreover, the extreme disparities in the types of neighborhoods to which black and nonblack children were exposed widened over the early life course. Although racial differences in exposure to the most disadvantaged neighborhoods narrowed slightly between ages 1 and 17 years, the proportion of nonblacks who lived in the least disadvantaged neighborhoods increased substantially, from 13.34 to 20.65 percent, while, over the same time period, the proportion of blacks who lived in these neighborhoods remained virtually constant at about 3.50 percent.

Longitudinal Neighborhood Exposure Patterns

Table 3 describes long-term exposure to different levels of neighborhood disadvantage throughout childhood, demonstrating the heterogeneity in neighborhood environments across and within children over time. The first panel in Table 3 presents descriptive statistics for our independent variable, duration-weighted exposure to neighborhood disadvantage. This measure is the average of ordinal

Table 1. Time-Invariant Sample Characteristics

Variable	Blacks (*n* = 834)	Nonblacks (*n* = 1,259)
High school graduation, percent		
Did not graduate high school	23.38	11.60
Graduated high school	76.62	88.40
Gender, percent		
Male	52.04	50.99
Female	47.96	49.01
Birth weight, percent		
5.5 lbs or more	90.77	94.52
Less than 5.5 lbs	9.23	5.48
Mother's marital status at birth, percent		
Unmarried	41.97	5.56
Married	58.03	94.44
FU head's education, percent		
Less than high school	55.28	22.72
High school graduate	26.26	26.29
At least some college	18.46	50.99
Mother's age at birth, mean	24.34	26.12

Note: FU = family unit. Statistics reported for children not lost to follow-up before age 20 (first imputation dataset).

wave-specific treatments from age 2 to 17 years; higher values represent sustained exposure to more disadvantaged neighborhoods, and lower values indicate long-term residence in less disadvantaged neighborhoods. Black and nonblack children had starkly different cumulative exposure patterns. About 65 percent of black children were exposed to a sequence of neighborhoods during childhood that were, on average, extremely disadvantaged. Less than 1 percent of blacks experienced long-term exposure to the least disadvantaged neighborhoods. By contrast, among nonblack children, only 8.74 percent spent the majority of their childhood in the most disadvantaged neighborhoods, and about 12 percent were continuously exposed to the least disadvantaged neighborhoods. Black children, therefore, were about seven times more likely than nonblack children to experience long-term residence in the most disadvantaged 20 percent of American neighborhoods. The lower panel of Table 3 describes the number of moves between neighborhoods in different quintiles of the composite disadvantage index (i.e., the number of times a subject moved between levels of the ordinal treatment). Only 37.53 percent of blacks and 16.52 percent of nonblacks never moved between neighborhood quintiles throughout the early life course; 29.86 and 44.24 percent of blacks and nonblacks, respectively, moved between different neighborhood contexts at least three times. These data thus reveal frequent neighborhood mobility.

Two substantive findings emerge from our descriptive analysis of longitudinal patterns of exposure to different neighborhood contexts. First, the racial disparity in long-term exposure to the most disadvantaged neighborhoods is more pronounced than the cross-sectional racial disparities in neighborhood environments observed at any given age. Second, for both blacks and nonblacks, neighborhood context appears to be a fairly transient ecological setting, with many families moving between different neighborhood environments (see also Briggs and Keys 2009; Quillian 2003;

Table 2. Time-Dependent Sample Characteristics

Variable	Blacks (n = 834) Age 1	Blacks Age 10	Blacks Age 17	Nonblacks (n = 1,259) Age 1	Nonblacks Age 10	Nonblacks Age 17
NH disadvantage index, percent						
1st quintile	3.48	3.60	3.48	13.34	19.14	20.65
2nd quintile	3.24	3.72	6.00	19.46	18.67	21.84
3rd quintile	5.28	5.88	7.79	26.13	23.27	22.48
4th quintile	14.87	18.11	18.47	26.13	23.99	21.13
5th quintile	73.14	68.71	64.27	14.93	14.93	13.90
FU head's marital status, percent						
Unmarried	33.93	44.84	52.04	5.88	11.36	15.09
Married	66.07	55.16	47.96	94.12	88.64	84.91
FU head's employment status, percent						
Unemployed	27.22	32.61	33.09	8.10	8.02	9.69
Employed	72.78	67.39	66.91	91.90	91.98	90.31
Public assistance receipt, percent						
Did not receive AFDC	81.06	75.66	82.37	96.27	96.19	97.93
Received AFDC	18.94	24.34	17.63	3.73	3.81	2.07
Homeownership, percent						
Do not own home	69.66	53.48	50.12	40.19	22.32	20.73
Own home	30.34	46.52	49.88	59.81	77.68	79.27
FU income in $1,000s, mean	19.68	25.04	27.45	32.59	46.65	57.50
FU head's work hours, mean	30.08	26.82	27.51	42.65	40.84	40.68
FU size, mean	5.75	5.32	4.81	4.22	4.69	4.33
Cum. residential moves, mean	.32	2.48	3.64	.32	2.16	3.02

Note: NH = neighborhood; FU = family unit. Statistics reported for children not lost to follow-up before age 20 (first imputation dataset).

Table 3. Exposure to Neighborhood Disadvantage from Age 2 to 17 Years

Variable	Blacks (n = 834)	Nonblacks (n = 1,259)
Duration-weighted exposure to NH disadvantage, percent		
1.0 to 1.4 (least disadvantaged NHs)	.84	12.31
1.5 to 2.4	2.64	20.57
2.5 to 3.4	6.24	30.26
3.5 to 4.4	24.82	28.12
4.5 to 5.0 (most disadvantaged NHs)	65.47	8.74
Number of moves between exposure levels, percent		
0	37.53	16.52
1	12.83	22.40
2	19.78	16.84
3+	29.86	44.24

Note: NH = neighborhood. Statistics reported for children not lost to follow-up before age 20 (first imputation dataset). NH disadvantage quintiles are based on distribution of the NH disadvantage index across all U.S. census tracts between 1970 and 2000.

Table 4. Stabilized Treatment and Attrition Weights

			Percentiles			
Weight	Mean	SD	1st	25th	75th	99th
Blacks (n = 834)						
Stabilized treatment weight (SW)	1.03	.58	.27	.73	1.18	4.62
Stabilized attrition weight (CW)	1.00	.12	.76	.92	1.06	1.44
SW x CW	1.04	.61	.26	.71	1.18	4.80
Nonblacks (n = 1,259)						
Stabilized treatment weight (SW)	1.00	.31	.32	.84	1.10	2.54
Stabilized attrition weight (CW)	1.00	.14	.71	.93	1.03	1.70
SW x CW	1.00	.37	.32	.81	1.10	3.00

Note: Statistics reported for children not lost to follow-up before age 20 (first imputation dataset).

Table 5. Effects of Duration-Weighted Exposure to Neighborhood Disadvantage on High School Graduation (log odds ratios)

	Blacks (n = 834)			Nonblacks (n = 1,259)		
Model	Coef	SE		Coef	SE	
Unadjusted	–.703	(.170)	***	–.581	(.109)	***
Regression-adjusted	–.416	(.196)	*	–.212	(.125)	
Stabilized IPT-weighted	–.525	(.190)	**	–.274	(.128)	*

Note: Analyses based on children not lost to follow-up before age 20. Coefficients and standard errors are combined estimates from five multiple imputation datasets.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-sided tests of no effect).

Timberlake 2007). This dynamic neighborhood selection demonstrates the need for IPT-weighted estimation.

Weights

Table 4 shows descriptive statistics for the stabilized IPT weights, attrition weights, and final weights, separately by race. Stabilized IPT weights adjust for selection into different neighborhoods across time based on observed time-varying covariates. We estimate them from the ordinal logistic regression models of treatment status at each wave k, reported in Part B of the online supplement. The treatment weights are well-behaved—centered around 1 and not highly variable. We also compute stabilized attrition weights to adjust for nonrandom loss to follow-up. These weights exhibit desirable properties with observed means close to 1 and small variance. The product of the stabilized IPT weight and the stabilized attrition weight give the final weight used to compute neighborhood-effect estimates. Weights are truncated at the 1st and 99th percentiles to improve efficiency and avoid disproportionate influence from outlying observations (Cole and Hernán 2008; Sharkey and Elwert 2011).

Neighborhood Effect Estimates

Table 5 shows unadjusted, conventional regression-adjusted, and stabilized IPT-weighted estimates for the effect of duration-weighted exposure to different neighborhood contexts on the probability of high school graduation. The unadjusted logit estimates suggest that long-term exposure to disadvantaged neighborhoods is strongly and negatively related to high school graduation for black ($\theta_1^u = -.703$, $p < .001$) and nonblack ($\theta_1^u = -.581$, $p < .001$) children.[12] Specifically, the unadjusted point estimates indicate that long-term exposure to neighborhoods in quintile q of the composite disadvantage distribution, rather than neighborhoods in the less disadvantaged quintile $q - 1$, is associated with about 50 percent lower odds of graduation for blacks (exp(–.703) = .495) and about 45 percent lower odds for nonblacks (exp (–.581) = .559). These estimates are biased, however, because they do not adjust for nonrandom selection into different neighborhood contexts.

The next set of results comes from conventional logit models that condition on duration-weighted exposure, all covariates measured at baseline, and time-varying covariates averaged over ages 2 to 17 years. These adjustments substantially reduce the estimated effect of long-term exposure to neighborhood disadvantage for blacks ($\theta_1^r = -.416$, $p = .034$) and nonblacks ($\theta_1^r = -.212$, $p = .091$). The problem with these estimates is that the models from which they are derived include time-varying covariates as regressors. Because neighborhood effects are likely mediated by time-varying characteristics of the family, these regression-adjusted estimates are biased due to over-control of indirect pathways and collider stratification.

The stabilized IPT-weighted estimates come from logit models for high school graduation fit to the weighted pseudo-population in which neighborhood exposure status at each wave is independent of prior time-varying covariates. Regression-adjusted and stabilized IPT-weighted estimates differ only in the way they adjust for confounding by time-varying factors. Under assumptions of no unmeasured confounders, no model misspecification, and positivity, IPT weighting—but not conventional regression—provides unbiased estimates of average causal effects. The stabilized IPT-weighted estimates indicate that living in a more disadvantaged neighborhood throughout childhood has a substantial negative effect on the chances of high school graduation among blacks ($\theta_1^w = -.525$, $p = .006$) and nonblacks ($\theta_1^w = -.274$, $p = .033$). For blacks, sustained exposure to the most disadvantaged quintile of neighborhoods between ages 2 and 17 years, compared to residence, on average, in 3rd-quintile neighborhoods, reduces the odds of high school graduation by about 65 percent (exp((5 – 3)×(–.525)) = .350). For nonblacks, these estimates indicate that long-term exposure to the most disadvantaged neighborhoods reduces the odds of high school graduation by about 40 percent, compared to residence, on average, in 3rd-quintile neighborhoods (exp((5 – 3) × (–.274)) = .578). Compared to growing up in the least disadvantaged quintile of neighborhoods, sustained exposure to the most disadvantaged neighborhoods reduces the odds of high school graduation by nearly 90 percent for blacks (exp((5 – 1) × (–.525)) = .122) and by about 70 percent for nonblacks (exp((5 – 1) × (–.274)) = .334). Stabilized IPT-weighted estimates for the effect of duration-weighted exposure to neighborhood disadvantage on high school graduation are substantially larger than corresponding estimates from conventional regression models.[13]

Figure 3 displays predicted probabilities of high school graduation by neighborhood exposure history. We computed these probabilities from the stabilized IPT-weighted estimates with baseline covariates set to their race-specific means. The graph describes how the probability of high school graduation would be expected to change if children had experienced one neighborhood exposure sequence compared to another. Estimates indicate that if black children had been continuously exposed to the least disadvantaged quintile of neighborhoods from age 2 to 17 years, about 96 percent would have graduated from high school by age 20. If the same population of black children had been exposed to the most disadvantaged neighborhoods in the United States for an extended period, only an estimated 76 percent would have graduated. Among nonblack children, an estimated 95 percent would have graduated from high school if they had been exposed to the least disadvantaged neighborhoods throughout the early life course, compared with 87 percent if

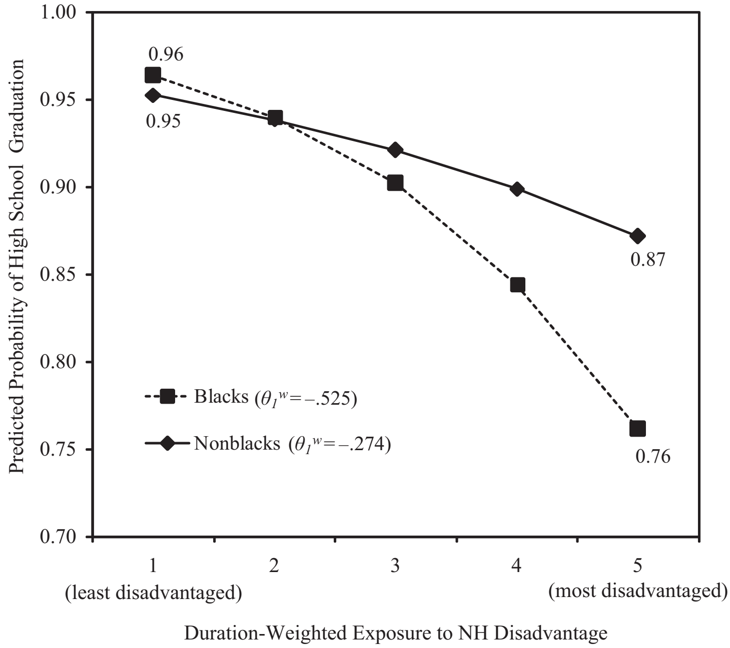

Figure 3. Predicted Probability of High School Graduation by Neighborhood Exposure History
Note: NH = Neighborhood

they had grown up in the most disadvantaged neighborhoods.

IPT weighting allows for improved adjustment of observed confounding when risk factors for selection into different neighborhood contexts are also intermediate variables. Even though IPT-weighted estimation avoids problems associated with conditioning on observed time-varying confounders, selection bias may still occur if unobserved factors simultaneously affect decisions about where to live and the chances that a child graduates from high school. The assumption of no unobserved confounding is not testable with observed data, but we address this challenge by adjusting for an extensive set of observed covariates. A second threat to the validity of causal inferences based on IPT-weighted estimates is the possibility that treatment models are misspecified. Extensive experimentation with different specifications, however, indicates that our effect estimates are remarkably robust—estimated neighborhood effects hardly change across 14 different treatment model specifications and remain statistically significant (see Table S4 in Part C of the online supplement). Related to correct model specification, IPT weighting also requires a positive probability of treatment for every level and combination of prior confounders. Because the United States does not formally restrict neighborhood choice based on economic or demographic characteristics, there is no reason to expect zero treatment probabilities in subgroups of children defined by their confounder history, except for the inherent limitations of sampling. Descriptive analyses of the empirical treatment distribution indicate that all exposure categories occur with positive probability across levels of several key confounders (see Tables S5 and S6 in Part C of the online supplement).

DISCUSSION

The consequences of growing up in disadvantaged neighborhoods are central to the study of social stratification. However, despite considerable theoretical motivation for the importance of long-term exposure and dynamic selection, past studies of neighborhood effects have neglected to take proper account of the duration for which children live in different neighborhood contexts, as well as the complex processes of selection, exposure, and feedback that link the neighborhood environment to children's developmental outcomes. This study addresses the paucity of research on neighborhood effects within a temporal framework, using counterfactual models for time-varying treatments and estimating the impact of exposure to different neighborhood contexts throughout childhood on the chances of high school graduation.

Our results indicate that sustained exposure to disadvantaged neighborhoods—characterized by high poverty, unemployment, and welfare receipt; many female-headed households; and few well-educated adults—throughout the entire childhood life course has a devastating impact on the chances of graduating from high school. Effect estimates presented in this study suggest a more substantial influence for neighborhood context than do estimates reported in prior research (e.g., Brooks-Gunn et al. 1993; Crane 1991; Ginther et al. 2000; Harding 2003). For example, Harding (2003) reports propensity score matching estimates from the PSID indicating that exposure to high-poverty neighborhoods (greater than 20 percent poverty) during adolescence, compared to living in low-poverty neighborhoods (less than 10 percent poverty), reduces the odds of high school graduation by about 50 percent for blacks and nonblacks. Brooks-Gunn and colleagues (1993), also using PSID data, provide regression-adjusted estimates based on point-in-time measures of neighborhood context that suggest that moving from a less disadvantaged neighborhood (5 percent with incomes less than \$10,000, 60 percent with incomes greater than \$30,000) to a more disadvantaged neighborhood (30 percent with incomes less than \$10,000, 10 percent with incomes greater than \$30,000) is associated with only 4 percent lower odds of high school graduation for blacks and 30 percent lower odds for nonblacks. By contrast, similar comparisons based on IPT-weighted estimates reported here indicate that *sustained* exposure to disadvantaged neighborhoods is associated with about an 80 percent decrease in the odds of high school graduation for blacks ($\exp((5 - 2) \times (-.525)) = .207$) and close to a 60 percent decrease for nonblacks ($\exp((5 - 2) \times (-.274)) = .439$).

Our results exceed previous estimates for two main reasons. First, this study accounts for duration of exposure to different neighborhood contexts. Previous research is frequently criticized for measuring neighborhood characteristics at a single point in time and implicitly viewing these conditions as permanent rather than temporary traits (Clampet-Lundquist and Massey 2008; Quillian 2003; Timberlake 2007). If families live in disadvantaged neighborhoods at one time but reside in advantaged neighborhoods otherwise, or vice versa, then measuring neighborhood context only once will understate the full impact of extended exposure. By measuring neighborhood context repeatedly throughout childhood, we are able to isolate the total effect of sustained exposure. Second, this study draws on novel methods that were specifically developed to resolve the difficult statistical problems related to dynamic selection into time-varying treatments. Estimating duration-dependent effects of exposure to different neighborhood contexts is challenging because moving decisions are affected by time-varying characteristics that are endogenous to previous neighborhood conditions. In this situation, regression estimators are biased. IPT weighting, by contrast, can properly adjust for dynamic neighborhood selection and therefore provide unbiased and consistent estimates of longitudinal neighborhood effects under assumptions that are, in fact, weaker than those required for conventional regression. Scholars can use these methods to analyze effects of other social contexts, such as firms, organizations, or schools, in which mobility is frequent and putative confounders are time-varying.

Evidence presented in this study suggests that a temporal framework is crucial for understanding neighborhood effects on children's educational outcomes. Most past research focuses on contemporaneous effects of neighborhood disadvantage and does not capture the cumulative impact of growing up in America's most disadvantaged communities. This cumulative effect is more consistent with Wilson's (1987) foundational arguments regarding the consequences of spatially concentrated poverty, which motivate nearly all recent studies of neighborhood effects. Our results demonstrate the importance of the neighborhood environment throughout childhood for one educational outcome, high school graduation. These findings resonate with evidence from several studies suggesting that residence in disadvantaged neighborhoods may affect children's cognitive development years, or even generations, later (Sampson et al. 2008; Sharkey and Elwert 2011). This emerging body of research indicates that the developmental impact of neighborhoods should be studied as a longitudinal process (Crowder and South 2011; Quillian 2003; Sampson et al. 2008; Sharkey and Elwert 2011).

Another important implication of the present study is that family background and neighborhood context affect children through a complex time-dependent process of selection, exposure, and feedback. We argue that family characteristics linked to children's educational attainment, such as parental marital status and family income, are not only important determinants of where a family lives but are also affected by neighborhood conditions in the past. The difference between our regression-adjusted and IPT-weighted estimates provides evidence that measured time-varying characteristics of the family mediate the effect of neighborhood context on educational attainment: the stabilized IPT-weighted estimates are larger than estimates from conventional regression models because they do not "control away" indirect effects mediated by family characteristics. This finding suggests a revised interpretation of claims that family environment is a more important determinant of child development than is neighborhood context (e.g., Leventhal and Brooks-Gunn 2000). Because family characteristics are partly the result of past neighborhood conditions, it is misleading to contrast the neighborhood and family as independent, competing determinants of children's outcomes; rather, neighborhood effects operate in part through family effects (Sharkey and Elwert 2011).

Results presented here extend past research by demonstrating the importance of duration of exposure as well as the time-varying selection and feedback mechanisms that structure neighborhood effects on child development. For a more complete understanding of how the neighborhood environment affects youth, future research should account for the different environments children experience throughout the entire course of development, as well as the dynamic selection processes that influence time-varying exposure patterns. Another important direction for future research on the temporal dimension of neighborhood effects is to investigate the specific mechanisms, including social and cultural isolation, violent crime, social cohesion, institutional resources, school quality, and environmental health hazards, through which disadvantaged neighborhoods affect parents and their children over time. The connection between neighborhood context and school quality, in particular, deserves greater attention. Although adjudicating between different theories of neighborhood effects is beyond the scope of this study, the conceptual and methodological approach developed here can provide a useful framework for investigating neighborhood mechanisms.

The severe educational impact of sustained exposure to disadvantaged neighborhoods illustrates the negative effects of growing up in communities that have suffered decades of structural neglect. The consequences of long-term exposure to disadvantaged neighborhoods documented in this study suggest that neighborhood-effects research is essential to understanding the reproduction of poverty. While the present study does not speak to the efficacy of specific policy interventions, which must be evaluated on their own terms, it seems likely that a lasting commitment to neighborhood improvement and income desegregation would be necessary to resolve the problems identified here. Absent more enduring structural changes, concentrated neighborhood poverty will likely continue to hamper the development of future generations of children.

Authors' Note

Some of the data used in this analysis are derived from Sensitive Data Files of the Panel Study of Income Dynamics, obtained under special contractual arrangements designed to protect the anonymity of respondents. These data are not available from the authors. Persons interested in obtaining PSID Sensitive Data Files should contact PSIDHelp@isr.umich.edu.

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Notes

1. A subject is lost to follow-up at wave *k* if their FU does not respond to the PSID at wave *k*. Subjects who leave the PSID at wave *k* but return to the study several years later are considered permanently lost to follow-up at wave *k*.
2. Due to measurement limitations in the PSID, sample members who earned a general equivalency degree by age 20 are coded as high school graduates.
3. Principal component analysis of the seven tract characteristics reveals a single component representing neighborhood disadvantage. The disadvantage score for each tract-year observation is equal to the first principal component from a pooled analysis using all tract-years between 1968 and 2000. Table S1 in the online supplement (http://asr.sagepub.com/supplemental) reports component weights and correlations with each tract characteristic. Table S2 provides descriptive statistics for census tracts in each quintile of the composite disadvantage distribution.
4. Results from analyses using a binary measure of neighborhood disadvantage (not shown, available upon request) are similar to those based on the five-level ordinal measure. The ordinal treatment variable is preferred because it retains more information about neighborhood context, allows for more flexible contrasts between different exposure trajectories, and attenuates certain technical problems associated with dichotomization, such as loss of statistical power.
5. The PSID does not measure parental education at regular intervals, which limits our ability to track changes over time. We therefore treat parental education as time-invariant and use measurements of this factor taken at baseline or, short of that, the most recent measurement prior to baseline.
6. Multiple imputation replaces missing data with $m > 1$ values that are simulated from an imputation model. Each of the m complete datasets are then analyzed separately, and the results are combined to produce estimates and standard errors that account for the uncertainty associated with missing information (Little and Rubin 2002; Rubin 1987). We use $m = 5$ datasets with simulated missing values from multiple imputation by chained equations (Royston 2005). Neighborhood effect estimates are based on combined results from these five datasets; for simplicity, we report descriptive statistics for only the first imputed dataset.
7. MSMs that relax the linearity assumption in Equation 2 by including quadratic and cubic terms for duration-weighted exposure to neighborhood disadvantage provide no evidence of nonlinearity in the treatment-outcome relationship. None of the higher-order polynomial terms in these models are statistically significant. In addition, we fit models that allow the effect of duration-weighted exposure to differ between childhood (ages 2 to 11 years) and adolescence (ages 12 to 17 years). There is no conclusive evidence of effect heterogeneity by developmental stage.
8. The assumption of no unobserved confounding of treatment does not preclude the existence of unobserved factors that affect time-varying covariates and the outcome.
9. For other applications of MSMs and IPT weighting in the social sciences, see Barber, Murphy, and Verbitsky 2004; Hong and Raudenbush 2008; Sampson, Laub, and Wimer 2006; Sampson et al. 2008; Sharkey and Elwert 2011; and Sharkey and Sampson 2010.
10. Estimating separate models by race is equivalent to fitting a pooled model that includes interactions between race and all other covariates.
11. Robust standard errors such as those computed here are conservative (i.e., too large) because they do not account for the fact that the IPT weights are estimated

(Robins, Rotnitzky, and Scharfstein 1999). Conservative standard errors make rejecting the null hypothesis of no treatment effect more difficult, and thus provide for more exacting tests at given levels of statistical significance.

12. We use superscripts on the theta parameters to distinguish the estimand defined in Equation 2 from the different realized estimates reported in this section.
13. Conventional logit models for high school graduation that use point-in-time measurements of neighborhood exposure status (at age 14) yield estimates that are even smaller than those reported here (results not shown, available upon request). These models replicate the analytic strategy most often used in prior research and thus provide further evidence that past studies severely underestimate the effect of sustained exposure to neighborhood disadvantage.

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Geoffrey T. Wodtke is a doctoral student in the Department of Sociology and a Population Studies Center Trainee in social demography at the University of Michigan. His research focuses on the processes through which racial and class inequalities are generated and maintained; the ways that institutions, such as the formal educational system, influence beliefs about racial and economic inequality and how these attitudes challenge or reinforce existing social hierarchies; and methods for causal inference in observational studies.

David J. Harding is Associate Professor in the Department of Sociology and Ford School of Public Policy and Research Associate Professor at the Population Studies Center and Survey Research Center at the University of Michigan. His recent work includes "Collateral Consequences of Violence in Disadvantaged Neighborhoods" (*Social Forces* 2009), "Violence, Older Peers, and the Socialization of Adolescent Boys in Disadvantaged Neighborhoods" (*ASR* 2009), and *Living the Drama: Community, Conflict, and Culture among Inner-City Boys* (University of Chicago Press 2010).

Felix Elwert is Assistant Professor in the Department of Sociology at the University of Wisconsin–Madison. His research focuses on social demography, neighborhood effects, and methods for causal inference. His recent work includes "The Legacy of Disadvantage: Multigenerational Neighborhood Effects on Cognitive Ability" (*AJS* 2011), "Effect Heterogeneity and Bias in Main-Effects-Only Regression Models" (*Heuristics, Probability and Causality* 2010), "Wives and Ex-Wives: A New Test for Homogamy Bias in the Widowhood Effect" (*Demography* 2008), and "Widowhood and Race" (*ASR* 2006).

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The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment†

By Raj Chetty, Nathaniel Hendren, and Lawrence F. Katz*

The Moving to Opportunity (MTO) experiment offered randomly selected families housing vouchers to move from high-poverty housing projects to lower-poverty neighborhoods. We analyze MTO's impacts on children's long-term outcomes using tax data. We find that moving to a lower-poverty neighborhood when young (before age 13) increases college attendance and earnings and reduces single parenthood rates. Moving as an adolescent has slightly negative impacts, perhaps because of disruption effects. The decline in the gains from moving with the age when children move suggests that the duration of exposure to better environments during childhood is an important determinant of children's long-term outcomes. (*JEL* I31, I38, J13, R23, R38)

Individuals who live in high-poverty areas fare worse than those who live in lower-poverty neighborhoods on a wide range of economic, health, and educational outcomes.[1] Motivated by such disparities in outcomes across neighborhoods, the Moving to Opportunity (MTO) experiment of the US Department of Housing and Urban Development offered a randomly selected subset of families

* Chetty: Department of Economics, Stanford University, Landau 323, Stanford, CA 94305, and NBER (e-mail: chetty@stanford.edu); Hendren: Department of Economics, Harvard University, Littauer Center, 1805 Cambridge Street, Cambridge, MA 02138, and NBER (e-mail: nhendren@fas.harvard.edu); Katz: Department of Economics, Harvard University, Littauer Center, 1805 Cambridge Street, Cambridge, MA 02138, and NBER (e-mail: lkatz@harvard.edu). The opinions expressed in this paper are those of the authors alone and do not necessarily reflect the views of the Internal Revenue Service or the US Treasury Department. This work is a component of a larger project examining the effects of tax expenditures on the budget deficit and economic activity. All results based on tax data in this paper are constructed using statistics originally reported in the SOI working paper, "The Economic Impacts of Tax Expenditures: Evidence from Spatial Variation across the U.S.," approved under IRS contract TIRNO-12-P-00374 and presented at the Office of Tax Analysis on November 3, 2014. MTO participant data are highly confidential. HUD allowed the authors special access to the experimental data under Data License DL14MA001, approved March 28, 2014. We thank Joshua Angrist, Jeffrey Kling, Jeffrey Liebman, Jens Ludwig, anonymous referees, and numerous seminar participants for helpful comments and discussions. We are very grateful to Ray Yun Gou, Lisa Sanbonmatsu, and Matt Sciandra for help with the MTO data. Sarah Abraham, Alex Bell, Augustin Bergeron, Jamie Fogel, Nikolaus Hildebrand, Alex Olssen, and Benjamin Scuderi provided outstanding research assistance. This research was funded by the National Science Foundation, NIA grant R01 AG031259, the Lab for Economic Applications and Policy at Harvard, and Laura and John Arnold Foundation. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.
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[1] See, for example, Jencks and Mayer (1990); Brooks-Gunn et al. (1993); Cutler and Glaeser (1997); Leventhal and Brooks-Gunn (2000); and Sampson, Morenoff, and Gannon-Rowley (2002).

living in high-poverty housing projects subsidized housing vouchers to move to lower-poverty neighborhoods in the mid-1990s. The MTO experiment generated large differences in neighborhood environments for comparable families, providing an opportunity to evaluate the causal effects of improving neighborhood environments for low-income families (Ludwig et al. 2013).

Previous research evaluating the MTO experiment has found that moving to lower-poverty areas greatly improved the mental health, physical health, and subjective well being of adults as well as family safety (e.g., Katz, Kling, and Liebman 2001; Kling, Liebman, and Katz 2007; Clampet-Lundquist and Massey 2008; Ludwig et al. 2013). But these studies have consistently found that the MTO treatments had no significant impacts on the earnings and employment rates of adults and older youth, suggesting that neighborhood environments might be less important for economic success.

In this paper, we revisit the MTO experiment and focus on its long-term impacts on children who were young when their families moved to better neighborhoods. Our analysis is motivated by recent evidence that the amount of time individuals spend in a given neighborhood during their childhood is a key determinant of that neighborhood's effects on their long-term outcomes.[2] Crowder and South (2011) and Wodtke, Harding, and Elwert (2011) show that the fraction of childhood spent in high-poverty areas is negatively correlated with outcomes such as high school completion. Chetty and Hendren (2015) study more than five million families that move across areas and find that neighborhoods have causal exposure effects on children's outcomes using quasi-experimental methods. In particular, every year spent in a better area during childhood increases college attendance rates and earnings in adulthood, so the gains from moving to a better area are larger for children who are younger at the time of the move.

In light of this evidence on childhood exposure effects, we test two hypotheses in the MTO data. First, we hypothesize that moving to a lower-poverty area improves long-term economic outcomes for children who were young at the point of random assignment (RA). Second, we hypothesize that the gains from moving to a lower-poverty area decline with a child's age at move. Prior work has not been able to study these issues because the younger children in the MTO experiment are only now old enough to be entering the adult labor market. We present new evidence on the impacts of MTO on children's earnings, college attendance rates, and other outcomes in adulthood by linking the MTO data to federal income tax returns.

The MTO experiment was conducted between 1994 and 1998 in five large US cities. The experimental sample included 4,604 families, who were randomly assigned to one of three groups: an experimental voucher group that was offered a subsidized housing voucher that came with a requirement to move to a census tract with a poverty rate below 10 percent, a Section 8 voucher group that was offered a standard subsidized housing voucher with no additional contingencies, and a control group that was not offered a voucher (but retained access to public housing).

[2] The idea that the length of exposure to neighborhoods might matter has been recognized since Wilson (1987) and Jencks and Mayer (1990). Importantly, we focus here on exposure effects during *childhood*; as we discuss below, we find no evidence of exposure effects for adults.

We begin our analysis by evaluating the impacts of MTO on young children, whom we define in our baseline analysis as those below age 13 at RA.[3] These children are eight years old on average at RA. Among these children, 48 percent of those in the experimental voucher group took up the voucher to move to a low-poverty area, while 66 percent of those in the Section 8 group took up the vouchers they were offered. Children growing up in the three groups experienced very different childhood environments. On average from the date of RA until age 18, children below age 13 at RA in the control group lived in census tracts with a mean poverty rate of 41 percent. Children whose families took up the experimental voucher lived in census tracts with 22 percentage point lower poverty rates than those in the control group on average until age 18. Those who took up the Section 8 voucher lived in census tracts with 12 percentage point lower poverty rates than the control group.

We estimate the treatment effects of growing up in these very different environments by replicating the intent-to-treat (ITT) specifications used in prior work (e.g., Kling, Liebman, and Katz 2007), regressing outcomes in adulthood on indicators for assignment to each of the treatment arms. We find that assignment to the experimental voucher group led to significant improvements on a broad spectrum of outcomes in adulthood for children who were less than age 13 at RA. Children assigned to the experimental voucher group before they turned 13 have incomes that are $1,624 higher on average relative to the control group in their mid-twenties ($p < 0.05$). Given the experimental voucher take-up rate of 48 percent, this translates to a treatment-on-the-treated (TOT) estimate for those who took up the experimental voucher of $3,477, a 31 percent increase relative to the control group mean of $11,270. Children assigned to the experimental voucher group before they turn 13 are also significantly more likely to attend college and attend better colleges. The ITT effect on college attendance between the ages of 18–20 is a 2.5 percentage point (16 percent) increase relative to the control group mean attendance rate of 16.5 percent. Finally, children assigned to the experimental voucher group before age 13 also live in lower-poverty neighborhoods themselves as adults and are less likely to be single parents themselves (for females).[4]

Children whose families were assigned to the Section 8 voucher group before they turned 13 generally have mean outcomes between the control and experimental group means. For example, the TOT estimate for individual income is $1,723 for the Section 8 voucher relative to the control among children below age 13 at RA. This impact is 50 percent of the TOT estimate for the experimental voucher, which is consistent with the fact that the Section 8 voucher reduced mean neighborhood poverty rates by approximately half as much as the experimental voucher for those who took up the vouchers. Note that households in the Section 8 group could have chosen to make exactly the same moves as those in the experimental group. The fact that the experimental voucher had larger effects on children's outcomes than the Section 8 voucher therefore suggests that actively encouraging families to move

[3] We limit our sample to children who are at least 21 by 2012, the last year for which we have data from tax returns. Because of this restriction, our "below age 13" sample only includes children who were between the ages of 4 and 12 at random assignment. As we discuss below, we find similar results when defining "young children" using other cutoffs, e.g., those below age 12 or 14.

[4] We define a mother as a "single parent" if she has a child whose father's name is not listed on the child's social security application, which is typically submitted at birth.

to lower-poverty neighborhoods—either through counseling or by restricting their choice set—increases the impacts of housing vouchers on young children's long-term economic success.

The MTO treatments had very different effects on older children—those between 13–18 at RA, who were 15 years old on average at that point. In most cases, we find statistically insignificant differences between mean outcomes in the three treatment arms among older children. The point estimates suggest that, if anything, moving to a lower-poverty neighborhood had slightly *negative* effects on older children's outcomes. For example, the ITT impact of the experimental voucher is −\$967 on individual income among children who were 13–18 at RA. One potential explanation for these negative impacts at older ages is a disruption effect: moving to a very different environment, especially as an adolescent, could disrupt social networks and have other adverse effects on child development (Coleman 1988; Wood et al. 1993; South, Haynie, and Bose 2007).[5]

We explore the robustness of these findings by estimating models that interact age at RA linearly with the treatment indicators. We find robust evidence that the gains from moving to lower-poverty areas decline with the child's age at move, suggesting that every extra year of exposure to a low-poverty environment during childhood is beneficial. We do not find any clear evidence of a "critical age" below which children must move to benefit from a better neighborhood, although one cannot obtain very precise estimates of the age profile of exposure effects from the MTO data because of the small sample sizes at each child age.

Putting the results together, the effects of moving to a better neighborhood on children's long-term economic outcomes can be explained by a simple model featuring a disruption cost of substantially changing one's neighborhood environment coupled with benefits that are proportional to the amount of exposure to a lower-poverty environment during childhood. The exposure effects outweigh the disruption cost for children who move when young, but not for children who move at older ages. Although our findings are consistent with such a model of exposure effects, the MTO experimental design cannot be used to conclusively establish that childhood exposure to a better environment has a causal effect on long-term outcomes because the ages at which children move are perfectly correlated with their length of exposure to a lower-poverty neighborhood. As a result, we cannot distinguish differences in disruption effects by age at the time of a move from an age-invariant disruption cost coupled with an exposure effect. Moreover, the treatment effects for families with young versus old children could differ because the set of families who took up the voucher and the areas to which they moved might vary between the two groups. Nonetheless, regardless of the underlying mechanisms, the experimental results are adequate to conclude that providing subsidized housing vouchers to move to lower-poverty areas produces larger benefits for younger children.

[5] Importantly, these disruption costs appear to be a function of *where* a family moves rather than a fixed cost of moving to a different home. Most families in both the control and treatment groups moved several times while their children were growing up (Ludwig et al. 2013). However, families who moved using the subsidized housing vouchers—especially the experimental vouchers—moved to very different neighborhoods that were further away from the housing projects where they started. Disruption costs are presumably larger for children who move to a very different area, e.g., because of a loss of social networks (Coleman 1988). Consistent with this explanation, TOT estimates show a somewhat larger adverse impact of MTO moves on older children in the experimental voucher group as compared to the Section 8 group.

We find that the MTO treatments had little or no impact on adults' economic outcomes, consistent with prior work (e.g., Ludwig et al. 2013).[6] The experimental voucher TOT estimate on individual earnings is $-\$734$ (a 4.7 percent reduction) and the upper bound of the 95 percent confidence interval is a 12 percent increase, well below the estimated impacts for young children. We find no evidence of exposure effects among adults, in contrast with the observational correlations reported by Clampet-Lundquist and Massey (2008). The earnings impacts for adults do not increase over time after RA, despite the fact that cumulative exposure to lower-poverty areas rose significantly.

Prior studies of MTO have detected heterogeneity in short-run and medium-term treatment effects by child gender and experimental site (e.g., Kling, Liebman, and Katz 2007). We find no systematic differences in the treatment effects of MTO on children's long-term outcomes by gender, race, or site. In particular, the point estimates of the effect of the experimental voucher on earnings are positive for all five experimental sites, for whites, blacks, and Hispanics, and for boys and girls for children below the age of 13 at RA. The corresponding point estimates are almost all negative for children above the age of 13 at RA. Most notably, we find robust, statistically significant evidence that the experimental voucher improved long-term outcomes for (young) boys, a subgroup where prior studies found little evidence of medium-term gains.

Previous explorations of heterogeneity in treatment effects in the MTO data raise concerns that our results—which essentially explore heterogeneity in the new dimension of child's age at move—are an artifact of multiple hypothesis testing. A post-hoc analysis of a randomized experiment will generate some p-values that appear to be "statistically significant" (e.g., $p < 0.05$) purely by chance if one examines a sufficiently large number of subgroups. To address this concern, we test the omnibus null hypothesis that the treatment effects for the main subgroups studied in MTO research to date (based on gender, race, site, and age) are all zero using parametric F-tests and a nonparametric permutation test. We reject the null hypothesis of zero treatment effects in all subgroups with $p < 0.05$ for most outcomes using F-tests of interaction terms of treatment group and subgroup indicators. The permutation test yields $p < 0.01$ for the null hypothesis that the MTO treatments had no effect on any of the outcomes we study for children below 13 at RA, adjusting for multiple hypothesis testing across all the subgroups. The results imply that the significant treatment effects we detect for younger children are unlikely to be an artifact of analyzing multiple subgroups. In addition, we returned to the MTO data with a pre-specified hypothesis that we would find larger impacts for younger children, based on the quasi-experimental evidence in Chetty and Hendren (2015). The fact that the experimental results closely match the quasi-experimental evidence makes it less likely that these results are spuriously generated by multiple hypothesis testing.

We conclude that the Moving to Opportunity experiment generated substantial gains for children who moved to lower-poverty neighborhoods when they were young. We estimate that moving a child out of public housing to a low-poverty area

[6] Our findings regarding the impacts of MTO on children are also consistent with prior research. If we restrict ourselves to the data available in prior work (up to 2008) and do not split children by age at move, we detect no impact of the MTO treatments on children's economic outcomes.

when young (at age eight on average) using an MTO-type experimental voucher will increase the child's total lifetime earnings by about $302,000. This is equivalent to a gain of $99,000 per child moved in present value at age eight, discounting future earnings at a 3 percent interest rate. The increased earnings of children ultimately leads to significant benefits to taxpayers as well. Children whose families took up experimental vouchers before they were 13 pay an extra $394 per year in federal income taxes during their mid-twenties. If these gains persist in subsequent years of adulthood, the additional tax revenue obtained from these children will itself offset the incremental cost of the experimental voucher treatment relative to providing public housing. Thus, our findings suggest that housing vouchers which (i) require families to move to lower-poverty areas and (ii) are targeted at low-income families with young children can reduce the intergenerational persistence of poverty and ultimately save the government money.

The paper is organized as follows. Section I summarizes the key features of the MTO experiment. Section II describes the data sources and reports summary statistics and tests for balance across the experimental groups. We present our main results in Section III. In Section IV, we reconcile our new findings with prior research on MTO. Section V presents a cost-benefit analysis and discusses policy implications. We conclude in Section VI by interpreting our findings in the context of the broader literature on neighborhood effects.

I. The Moving to Opportunity Experiment

In this section, we briefly summarize the key features of the MTO experiment; see Sanbonmatsu et al. (2011) for a more comprehensive description. The MTO randomized housing mobility demonstration, conducted by the US Department of Housing and Urban Development (HUD), enrolled 4,604 low-income families living in five US cities (Baltimore, Boston, Chicago, Los Angeles, and New York) from 1994 to 1998. Families were eligible to participate in MTO if they had children and resided in public housing or project-based Section 8 assisted housing in high-poverty census tracts (those with a 1990 poverty rate of 40 percent or more).

Families were randomized into three groups: (i) the experimental group, which received housing vouchers that subsidized private-market rents and could initially (for the first year) only be used in census tracts with 1990 poverty rates below 10 percent; (ii) the Section 8 group, which received regular housing vouchers without any MTO-specific relocation constraint; and (iii) a control group, which received no assistance through MTO. Those in the experimental group also received additional housing-mobility counseling from a local nonprofit organization. The experimental vouchers became regular Section 8 vouchers after a year and were no longer restricted to low-poverty census tracts. Families assigned to the experimental and Section 8 groups had 4–6 months to lease an apartment and use their vouchers.

Families in all three groups were required to contribute 30 percent of their annual household income toward rent and utilities. Those assigned to the experimental or Section 8 voucher groups received housing vouchers that covered the difference between their rent and the family's contribution, up to a maximum amount known as the Fair Market Rent, defined as the fortieth percentile of rental costs in a metro area. Families remained eligible for these vouchers (or public housing projects) indefinitely as long as their income was below 50 percent of the median income in their metro area.

The proportion of individuals randomly assigned to the three groups at each site was changed during the course of the experiment because take-up of the MTO vouchers turned out to differ from projections. All the statistics reported in this paper use sampling weights in which individuals are weighed by the inverse of their probability of assignment to their treatment group to account for changes in the random-assignment ratios over time.[7]

II. Data

We draw information from two datasets: HUD files on the MTO participants and federal income tax records. This section describes the two data sources and key variable definitions. It then provides descriptive statistics and tests for balance across the MTO treatment groups.

A. *MTO Data*

The MTO dataset contains information on 4,604 households and 15,892 individuals who participated in the experiment. This study examines the impacts of MTO on outcomes typically observed at age 21 or older. Since the last year in the tax data is currently 2012, we restrict the MTO sample to the 13,213 individuals who are 21 or older in 2012 (those born in or before 1991). We focus much of our analysis on MTO children, defined as individuals who were 18 years old or younger at the time of RA and residing at that time in a household that participated in MTO. There are 11,276 children in the MTO data, of whom 8,603 (76 percent) were born in or before 1991.

For each MTO participant, we use two sets of information from the MTO dataset. First, we obtain information on individual and household background characteristics from the MTO Participant Baseline Survey. The baseline survey was administered to each MTO household head at the time of program enrollment (prior to RA). The survey provides demographic and socioeconomic background information on each household member (adults and children) including information on children's school experiences, household criminal victimization, reasons for wanting to participate in MTO, and household income and transfer receipt. See Sanbonmatsu et al. (2011) for more detailed information on the background characteristics of the MTO participants and the baseline survey.

Second, we obtain yearly information on the residential neighborhood (census tract) for each MTO participant using address history data from the MTO long-term survey conducted in 2008–2010, as in Sanbonmatsu et al. (2011). We estimate census tract poverty rates in each year by interpolating census tract poverty rates using the 1990 and 2000 censuses and the 2005–2009 American Community Surveys. We

[7] See Orr et al. (2003) for the details on the variation in random-assignment ratios over time and the construction of the MTO sample weights. The weights prevent time or cohort effects from confounding the results. The weights we use are the same as the weights used for the analysis of administrative data in past MTO work.

use this information to construct a measure of each MTO child's exposure to poverty (mean tract poverty rate) from the time of RA to age 18.

B. *Tax Data*

We link the MTO data to data from federal income tax records spanning 1996 to 2012.[8] HUD collected social security numbers (SSNs) prior to RA for 90 percent (11,892) of the individuals who participated in the MTO experiment and were born in or before 1991. The MTO records were linked to the tax data by SSN. Of the MTO records with a valid SSN, 99 percent (11,780) were successfully linked to the tax data. To protect confidentiality, individual identifiers were removed from the linked dataset prior to the statistical analysis.

The tax data include both income tax returns (1040 forms) and third-party information returns (e.g., W-2 forms), which give us information on the earnings of those who do not file tax returns as well as data on other outcomes, such as college attendance. Here, we define the key variables we use in our analysis. We measure all monetary values in real 2012 dollars, adjusting for inflation using the Consumer Price Index (CPI-U).

Income.—Our primary measure of income is "individual earnings." Individual earnings is defined as the sum of income from W-2 forms filed by employers (summed across all W-2s for the individual in each year) and "non-W-2 earnings." Non-W-2 earnings is defined as adjusted gross income on form 1040 minus own and spouse's W-2 earnings, UI benefits, and SSDI payments, and is divided by two for married households. Hence, non-W-2 earnings reflects income from self-employment and other activities not captured on W-2s. Non-W-2 earnings is recoded to zero if negative and is defined as zero for non-filers. If an individual has no tax return and no W-2 earnings, individual earnings is coded as zero.

We also report effects on household income. For those who file tax returns, we define household income as adjusted gross income (as reported on the 1040 tax return) plus tax-exempt interest income and the nontaxable portion of Social Security and Disability benefits. In years when an individual does not file a tax return, we define household income as the sum of the individual's wage earnings (reported on form W-2), unemployment benefits (reported on form 1099-G), and gross social security and disability benefits (reported on form SSA-1099).[9]

College Attendance.—We define college attendance at age a as an indicator for having a 1098-T form filed on one's behalf during the calendar year in which the child turns age a. Title IV institutions (all colleges and universities as well as vocational schools and other post-secondary institutions eligible for federal student aid) are required to file 1098-T forms that report tuition payments or scholarships received for every student. These 1098-T data are available from 1999–2012.

[8] Here, and in what follows, the year refers to the tax year (i.e., the calendar year in which the income is earned).

[9] For non-filers, our definition of "household income" does not include the spouse's income. This is likely to be of minor consequence because the vast majority of non-filers in the United States who are not receiving Social Security benefits are single (Cilke 1998, Table I).

Because the 1098-T forms are filed directly by colleges independent of whether an individual files a tax return, we have records on college attendance for almost all children.[10] Comparisons to other data sources indicate that 1098-T forms capture college enrollment quite accurately overall (Chetty, Friedman, and Rockoff 2014, Appendix B). In particular, the correlation between enrollment counts based on 1098-T forms and enrollment counts in the IPEDS dataset from the Department of Education exceeds 0.95.

College Quality.—Using data from 1098-T forms, Chetty, Friedman, and Rockoff (2014) construct an earnings-based index of "college quality" using the mean individual wage earnings at age 31 of children born in 1979–1980 based on the college they attended at age 20. For those not enrolled in any college at age 20, the index equals the mean earnings at age 31 of all US residents not enrolled in college at age 20. We define college quality at age a based on the college in which the child was enrolled at age a (inflated to 2012 dollars using the CPI-U).

Neighborhood Characteristics in Adulthood.—We measure the characteristics of the neighborhoods where children live in adulthood using information on zip codes from the tax data.[11] We assign each individual a zip code in each year using a sequential algorithm starting with the location from which the individual files his tax return (form 1040). If the individual does not file a tax return, we obtain their zip code from form W-2, followed by other information returns (e.g., 1099s). We use this information to measure the following characteristics of the individual's zip code using data from the 2000 census: poverty share (share of households below the poverty line), mean income (aggregate income in the zip code divided by the number of individuals 16–64 years old), black share (number of people who are black alone divided by total population in 2000), and single mother share (number of households with female heads and no husband present with own children present divided by the total number of households with own children present).

Marital Status and Fertility.—We define an individual as married if he or she files a tax return as a married individual in a given year. We measure fertility patterns using data through June 2014 from the Kidlink (DM-2) database provided to the IRS by the Social Security Administration, which contains information from applications for SSNs. SSN applications request the SSN of both the mother and father (if present), allowing us to link parents to their children.[12] We define a woman as having a birth if she had a child before June 2014 and having a teenage birth if she had a child between the ages of 13 and 19. Most people apply for SSNs for their children at birth because an SSN is required to claim a child as a dependent on tax returns and for various other purposes. We therefore define an indicator for whether

[10] Colleges are not required to file 1098-T forms for students whose qualified tuition and related expenses are waived or paid entirely with scholarships or grants. However, the forms are typically available even for such cases, presumably because of automated reporting to the IRS by universities. Approximately 6 percent of 1098-T forms are missing from 2000–2003 because the database contains no 1098-T forms for some small colleges in these years.

[11] The tax data do not currently contain information on census tracts, so we are forced to use a broader zip code measure when analyzing children's location in adulthood.

[12] The total count of births in the DM-2 data differ from CDC vital statistics counts by less than 2 percent from 1987–2006, but the DM-2 data misses approximately 10 percent of births starting in 2007.

the father was present at a child's birth based on whether a father is listed on the child's SSN application.

Tax Filing and Taxes Paid.—We define tax filing as an indicator for filing a 1040 tax return and total taxes paid as the total tax field from form 1040 for filers and total taxes withheld on W-2 forms for non-filers.

C. *Balance Tests and Summary Statistics*

Prior research has documented that baseline characteristics are balanced between the treatment and control groups for both MTO adults and children, as would be expected in an experiment with random assignment (Kling, Liebman, and Katz 2007). Here, we replicate these balance tests on the linked MTO-tax data to ensure that we retain balance in the subgroup that we are able to link to the tax data.

In our core analysis, we split children into two groups: those below age 13 at RA and those between ages 13–18 at RA. Table 1 reports summary statistics and balance tests for selected baseline covariates for these two groups. Online Appendix Table 1A replicates Table 1 for the broader set of 52 baseline covariates used in the MTO interim and final impact evaluations (Kling, Liebman, and Katz 2007; Sanbonmatsu et al. 2011).

We match 86.4 percent of younger children and 83.8 percent of the older MTO children to the tax data. The match rates do not differ significantly between the control and treatment groups. This is to be expected because the SSNs that we use to link the MTO data to the tax data were collected *prior* to random assignment and we successfully link 99 percent of the individuals with valid SSNs to the tax data. Thus, there is virtually no scope for differential attrition across the three treatment arms in the linked dataset. Consistent with the lack of differential attrition, the distribution of baseline covariates appears to be balanced in the linked MTO-tax data. Thirteen of the 196 differences reported in online Appendix Table 1A are significant with $p < 0.05$ and 2 of the 196 are significant with $p < 0.01$ based on t-tests that do not adjust for multiple comparisons, in line with what one would expect under random assignment.

The summary statistics in Table 1 show that families who participated in MTO were quite economically disadvantaged. Approximately one-third of the MTO household heads had completed high school, only one quarter were employed, three-quarters were receiving public assistance (AFDC/TANF), more than half had never been married, and a quarter had been teenage parents at the point of random assignment. Around three-quarters of applicants reported getting away from gangs and drugs as one of the most important reasons for enrolling in MTO, and over 40 percent of the households had been victims of crime in the previous five years. The vast majority of the household heads were African-American or Hispanic females. Among the older children (ages 13–18 at RA), nearly 20 percent had been suspended or expelled from school in the past two years.

Online Appendix Table 1B reports summary statistics on children's long-term outcomes. Mean individual earnings is \$11,739 at age 24, rising to \$14,269 at age 28. College attendance rates are 18–19 percent over the ages 19–21. Roughly 22 percent of the females give birth to a child as a teenager. On average, 63 percent of the sample files a tax return (form 1040) in any given year after they turn 24.

TABLE 1—SUMMARY STATISTICS AND BALANCE TESTS FOR CHILDREN IN MTO-TAX DATA LINKED SAMPLE

	< Age 13 at random assignment			Age 13–18 at random assignment		
	Control group mean (1)	Exp. versus control (2)	Sec. 8 versus control (3)	Control group mean (4)	Exp. versus control (5)	Sec. 8 versus control (6)
Linked to tax data (%)	86.4	−0.8	−0.4	83.8	1.5	−0.1
		(1.4)	(1.5)		(2.0)	(2.2)
Child's age at random assignment	8.2	−0.1	−0.0	15.1	0.1	−0.1
		(0.1)	(0.1)		(0.1)	(0.1)
Household head completed high school (%)	34.3	4.2*	0.4	29.5	5.0	0.7
		(2.4)	(2.6)		(3.1)	(3.3)
Household head employed (%)	23.8	1.0	−2.2	25.3	3.0	−0.4
		(2.1)	(2.2)		(2.9)	(3.0)
Household head gets AFDC/TANF (%)	79.5	0.6	1.8	75.0	−0.8	−1.0
		(1.9)	(2.0)		(2.9)	(3.0)
Household head never married (%)	65.1	−4.3*	−3.1	53.0	−3.1	−6.3*
		(2.3)	(2.6)		(3.2)	(3.4)
Household head had teenage birth (%)	28.6	−0.9	−0.3	29.1	−3.6	−2.5
		(2.2)	(2.5)		(2.9)	(3.2)
Primary or secondary reason for move is to get away from gangs or drugs (%)	78.1	−1.8	−4.4*	77.7	3.1	−0.9
		(2.1)	(2.4)		(2.6)	(2.9)
Household victims of crime in past five years (%)	41.3	2.5	0.9	44.8	1.3	−3.3
		(2.4)	(2.7)		(3.3)	(3.5)
Household head African American (%)	66.9	−0.4	−1.4	63.9	−1.9	−5.9**
		(2.0)	(2.1)		(2.7)	(2.8)
Household head Hispanic (%)	29.4	−0.3	−0.5		31.1	0.6
		(2.0)	(2.1)		(2.7)	(2.7)
Child susp./expelled in past two years (%)	4.9	0.7	0.4	17.6	1.0	0.4
		(0.8)	(0.9)		(2.0)	(2.2)
Children in linked MTO-tax data	1,613	1,969	1,427	686	959	686

Notes: This table presents summary statistics and balance tests for match rates and a subset of variables collected prior to randomization; online Appendix Table 1A replicates this table for all 52 control variables we use in our analysis. The estimates in the first row (fraction linked to tax data) are based on all children in the MTO data who were born in or before 1991. The estimates in the remaining rows use the subset of these observations successfully linked to the tax data. Columns 1–3 include children below age 13 at random assignment; columns 4–6 include those above age 13 at random assignment. Columns 1 and 4 show the control group mean for each variable. Columns 2 and 5 report the difference between the experimental voucher and control group, which we estimate using an OLS regression (weighted to adjust for differences in sampling probabilities across sites and over time) of each variable on indicators for being assigned to the experimental voucher group, the Section 8 voucher group, as well as indicators for randomization site. Columns 3 and 6 report the coefficient for being assigned to the Section 8 group from the same regression. The estimates in columns 2–3 and 5–6 are obtained from separate regressions. Standard errors, reported in parentheses, are clustered by family. The final row lists the number of individuals in the control, experimental, and Section 8 groups in the linked MTO-tax data sample.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

III. Analysis and Results

In our core analysis, we split the 7,340 children in our linked analysis sample into two groups: (i) children younger than adolescence (less than 13 years) at RA and (ii) adolescent children (those 13 to 18 years old) at RA.[13] The children in the

[13] MTO moves typically occurred within six months of RA, so a child's age at RA is essentially the child's age at the time of the move.

younger group were 8.2 years old at RA on average, while those in the older group were 15.1 years old on average (Table 1). This split at age 13 yields approximately the same number of observations for the younger and older groups for analyses of outcomes such as earnings in early adulthood. We report estimates with different age cutoffs and use linear interaction models to evaluate the robustness of the results in Section IIIG.

Within each of these two groups, we estimate "intent-to-treat" (ITT) effects of the MTO treatments, which are essentially differences between treatment and control group means. Following the standard approach used in prior evaluations of MTO (e.g., Kling, Liebman, and Katz 2007), we estimate ITT effects on an outcome (y) using OLS regression specifications of the form

$$(1) \qquad y_i = \alpha + \beta_E^{ITT} Exp_i + \beta_S^{ITT} S8_i + \gamma \mathbf{X}_i + \delta \mathbf{s}_i + \epsilon_i,$$

where *Exp* and *S*8 are indicator variables for being randomly assigned to the experimental and Section 8 groups respectively, **X** is a vector of baseline covariates, and **s** is a set of indicators for randomization site. All of our regressions are weighted to adjust for differences in sampling probabilities (randomization rates into the different treatment groups) across sites and over time. We cluster the standard errors by family (allowing for common error components across siblings) because randomization occurred at the family level.

In our baseline specifications, we include randomization site dummies s (since randomization occurred within sites) but no additional covariates **X**, as the choice of which covariates to include is somewhat arbitrary. In supplemental specifications, we evaluate the sensitivity of our estimates to the inclusion of the 52 baseline covariates shown in online Appendix Table 1A. Including these additional covariates affects the point estimates modestly and has little impact on the qualitative conclusions, as expected given that the covariates are balanced across the treatment groups.[14]

The estimates of β_E^{ITT} and β_S^{ITT} in (1) identify the causal impact of being *offered* a voucher to move through MTO. Since not all the families offered vouchers actually took them up, these ITT estimates understate the causal effect of actually moving to a different neighborhood. Following Kling, Liebman, and Katz (2007), we estimate the impacts of moving through MTO—the impact of "treatment on the treated" (TOT)—by instrumenting for MTO voucher take-up with the treatment assignment indicators. Formally, we estimate specifications of the form

$$(2) \qquad y_i = \alpha_T + \beta_E^{TOT} TakeExp_i + \beta_S^{TOT} TakeS8_i + \gamma_T \mathbf{X}_i + \delta_T \mathbf{s}_i + \epsilon_i^T,$$

where *TakeExp* and *TakeS*8 are indicators for taking up the experimental and Section 8 vouchers, respectively. Since *TakeExp* and *TakeS*8 are endogenous variables, we instrument for them using the randomly-assigned MTO treatment group indicators (*Exp* and *S*8) and estimate (2) using two-stage least squares. Under the assumption

[14] Replicating the covariate balance tests discussed in Section IIC on the estimation subsamples (e.g., the subset of children for whom we observe earnings at age 24) yields very similar results to those reported in online Appendix Table 1A.

that MTO voucher offers only affect outcomes through the actual use of the voucher to lease a new residence, β_E^{TOT} and β_S^{TOT} can be interpreted as the causal effect of taking up the experimental and Section 8 vouchers and moving to a lower-poverty neighborhood (Angrist, Imbens, and Rubin 1996).[15]

This section reports estimates of (1) and (2) for various outcomes y_i . We begin by analyzing the "first-stage" effects of the MTO experiment on the characteristics of the neighborhoods where children grew up. We then turn to impacts on children's outcomes in adulthood, such as earnings and college attendance rates.

A. *Voucher Take-Up and Neighborhood Characteristics during Childhood*

Table 2 shows the effects of the MTO treatments on voucher take-up rates and poverty rates in the neighborhoods where children were raised. Panel A considers younger children (below 13 at RA), while panel B considers older children (between ages 13–18 at RA). The estimates in Table 2 include no controls other that randomization site indicators; online Appendix Table 2 replicates Table 2 controlling for the baseline covariates listed in online Appendix Table 1A and shows that the estimates are similar.

Column 1 of Table 2 reports estimates of the specification in (1) with an indicator for taking up a housing voucher as the dependent variable y_i. The control group mean is zero for this outcome because those in the control group were not offered vouchers. Among younger children, 48 percent who were assigned to the experimental group took up the voucher they were offered. 66 percent of those in the Section 8 group took up the less restrictive voucher that they were offered. The corresponding take-up (or "compliance") rates were slightly lower among families with older children, at 40 percent and 55 percent. Families in the treatment groups who chose to take up the vouchers were also more likely to have been dissatisfied with their current apartment and indicate they would be very likely to be able to find a new apartment (Kling, Liebman, and Katz 2007).[16]

Families who took up the MTO housing vouchers moved to a variety of different neighborhoods. Online Appendix Table 1C lists the most common destinations in each of the five sites. For example, many MTO participants in New York were living in the Martin Luther King (MLK) Towers, a housing development in Harlem, at the point of RA. Many families who took up experimental vouchers moved to Wakefield in the North Bronx (near the Westchester County border), about ten miles north of the MLK Towers. Several families who took up Section 8 vouchers moved to Soundview in the Central Bronx, about six miles north of the MLK Towers.

We characterize the neighborhoods to which MTO families moved more systematically by measuring the impacts of the MTO treatments on neighborhood poverty rates. Column 2 reports ITT estimates of impacts on poverty rates in the census

[15] The ITT estimates rely only on the assumption of random assignment, which is guaranteed by the experimental design. The TOT estimates rely on the additional (untestable) assumption that being offered an MTO voucher had no effect on those who did not take it up.

[16] One might be concerned that when MTO household heads (parents) moved, their children may have stayed behind in the old neighborhood with relatives or friends. In practice, virtually all children moved along with their parents. Approximately 95 percent of both younger and older children were still living with their parents one year after RA in the control group. The fraction living with their parents is, if anything, slightly higher in the treatment groups one year post-RA.

Table 2—First-Stage Impacts of MTO on Voucher Take-Up and Neighborhood Poverty Rates (*Percentage Points*)

	Housing voucher take-up	Poverty rate in tract one year post- RA		Mean poverty rate in tract post-RA to age 18		Mean poverty rate in zip post-RA to age 18	
		ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Children < age 13 at random assignment							
Exp. versus control	47.66***	−17.05***	−35.96***	−10.27***	−21.56***	−5.84***	−12.23***
	(1.653)	(0.853)	(1.392)	(0.650)	(1.118)	(0.425)	(0.752)
Sec. 8 versus control	65.80***	−14.88***	−22.57***	−7.97***	−12.06***	−3.43***	−5.17***
	(1.934)	(0.802)	(1.024)	(0.615)	(0.872)	(0.423)	(0.622)
Observations	5,044	4,958	4,958	5,035	5,035	5,035	5,035
Control group mean	0	50.23	50.23	41.17	41.17	31.81	31.81
Panel B. Children age 13–18 at random assignment							
Exp. versus control	40.15***	−14.00***	−34.70***	−10.04***	−24.66***	−5.51***	−13.52***
	(2.157)	(1.136)	(2.231)	(0.948)	(1.967)	(0.541)	(1.113)
Sec. 8 versus control	55.04***	−12.21***	−22.03***	−8.60***	−15.40***	−3.95***	−7.07***
	(2.537)	(1.078)	(1.738)	(0.920)	(1.530)	(0.528)	(0.921)
Observations	2,358	2,302	2,302	2,293	2,293	2,292	2,292
Control group mean	0	49.14	49.14	47.90	47.90	35.17	35.17

Notes: Columns 1, 2, 4, and 6 report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Columns 3, 5, and 7 report TOT estimates using a 2SLS specification, instrumenting for voucher take-up with the experimental and Section 8 assignment indicators. Standard errors, reported in parentheses, are clustered by family. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment The estimates in panels A and B are obtained from separate regressions. The dependent variable in column 1 is an indicator for the family taking up an MTO voucher and moving. The dependent variable in columns 2 and 3 is the census tract-level poverty rate one year after random assignment. The dependent variable in columns 4–7 is the duration-weighted mean poverty rate in the census tracts (columns 4 and 5) and zip codes (columns 6 and 7) where the child lived from random assignment till age 18. The sample in this table includes all children born before 1991 in the MTO data for whom an SSN was collected prior to RA because we were unable to link the MTO tract-level location information to the tax data. This sample is nearly identical our linked analysis sample because 99.1 percent of the children with nonmissing SSNs are matched to the tax data. The duration-weighted poverty rate is constructed using information on the addresses where the youth lived from random assignment up to their 18th birthday, weighted by the amount of time spent at each address. Census tract poverty rates in each year are interpolated using data from the 1990 and 2000 decennial censuses as well as the 2005–2009 American Community Survey, as in Sanbonmatsu et al. (2011); zip code poverty rates are from census 2000 only and are not interpolated.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

tract where the child lived one year after random assignment using the specification in (1). The mean control group family was living in a very distressed census tract one year after RA, with a 50 percent poverty rate—2.92 standard deviations (SD) above the national average in the 2000 census national tract-poverty distribution. The MTO treatments led to large reductions in neighborhood poverty for both younger and older children. For younger children, the MTO voucher offers reduced the census-tract poverty rates in the experimental and Section 8 groups by 17 and 15 percentage points (pp). The ITT estimates of reductions in poverty rates are slightly smaller for the older children, at 14 and 12 pp respectively. This is because the voucher take-up rate was slightly lower among families with older children, as shown in column 1.

Column 3 of Table 2 reports TOT estimates on poverty rates—i.e., the change in poverty rates for families that actually took up the voucher—using the specification in (2). The estimates in this column are essentially the ITT estimates reported in column 2 divided by the impacts on voucher take-up reported in column 1, which is the first stage of the 2SLS regression used to estimate (2).[17] Among younger children, those who moved using the experimental voucher live in neighborhoods with a 36 pp lower poverty rate than those in the control group one year after random assignment. Those who moved with the Section 8 voucher live in neighborhoods with a 23 pp lower poverty rate. The TOT impacts are very similar for older children.

We focus on the effects of the MTO treatments on poverty rates because the experimental vouchers were targeted based on poverty rates and poverty rates are the most common measure of neighborhood quality in the literature on neighborhood effects (Sampson, Morenoff, and Gannon-Rowley 2002). Prior MTO research (Kling, Liebman, and Katz 2007; Ludwig et al. 2012) has shown that the mean neighborhood poverty rate experienced post-RA provides a reliable linear summary index of neighborhood quality (treatment dosage) for explaining variation in MTO treatment impacts by site and treatment group for both MTO adults and children. However, it is important to note that the MTO treatments changed neighborhood characteristics in several other dimensions as well. The MTO treatment groups lived in neighborhoods with more-educated residents and a lower share of single parent households. MTO treatment group households—especially those in the experimental group—experienced large and persistent increases in neighborhood safety, neighborhood satisfaction, and housing quality relative to control group families (Sanbonmatsu et al. 2011). The MTO treatments also modestly improved post-random assignment school quality, but these improvements were substantially smaller than the improvements in residential neighborhood quality (Fryer and Katz 2013). The MTO treatments had more modest impacts in reducing neighborhood racial segregation (percent minority) than neighborhood economic segregation (Ludwig et al. 2013). The treatment effects we report in this paper should thus be interpreted as the effect of changing a bundle of neighborhood attributes rather than any one feature of neighborhood environments.[18]

The effects of the MTO treatments on neighborhood conditions attenuate over time because many control families moved out of high-poverty public housing projects and some families in the MTO treatment groups moved back to higher-poverty areas. Nevertheless, children in the treatment groups experienced substantially different neighborhood environments on average during their childhood. Column 4 of Table 2 shows that on average from the point of RA until age 18, children in the experimental voucher group lived in areas with approximately 10 pp lower poverty rates than those in the control group. Children in the Section 8 group lived in areas with approximately 8 pp lower poverty rates than those in the control group. The corresponding TOT effects, shown in column 5, are a 22–25 pp reduction in mean

[17] The correspondence is not exact because the sample used in column 1 differs slightly from that in column 3, as post-RA locations are not available for all families.

[18] Because all of these neighborhood characteristics are highly correlated with each other, it is difficult to disentangle which attributes of neighborhoods are most predictive of children's success in the MTO data. The quasi-experimental estimates of neighborhood effects reported in Chetty and Hendren (2015) are better suited to studying this question because they are more precise and cover all areas of the United States.

poverty rates for those who took up the experimental voucher and a 12–15 pp reduction in mean poverty rates for those who took up the Section 8 voucher. Thus, the impacts of MTO-induced moves on the average neighborhood poverty experienced during childhood are about twice as large for the experimental group as for the Section 8 group.

Columns 6 and 7 of Table 2 show ITT and TOT impacts on mean zip code–level poverty rates from RA until age 18 (rather than tract-level poverty rates). The impacts on zip code poverty are about half as large as impacts on census-tract poverty because zip codes provide a more aggregated measure of neighborhoods than census tracts. These zip code measures are a useful benchmark because we can construct analogous zip code–level measures (but not tract-level measures) in the tax data to analyze the effects of the MTO treatments on where children live in adulthood.

The key implication of Table 2 for our analysis of exposure effects is that the younger MTO children received a much larger dosage of exposure to improved neighborhood environments than the older MTO children. The TOT effects on post-RA neighborhood poverty rates are similar for the younger and older MTO children. That is, families who took up vouchers moved to similar neighborhoods irrespective of their children's age. However, the younger children got the improvements in neighborhoods starting at younger ages. On average the younger group got 9.8 years of childhood exposure to better neighborhoods up to age 18, because they were 8.2 years old on average at RA. In contrast, those in the older group received only 2.9 years of childhood exposure to better neighborhoods on average, because they were 15.1 years old on average at RA. Our next task is to examine how this exposure to different neighborhood environments affected the long-run economic, educational, and family outcomes of the MTO children.

B. *Income in Adulthood*

Table 3 presents estimates of MTO treatment effects on children's income and employment rates in adulthood. As in Table 2, we divide children into two groups: younger children (less than 13 years at RA) and older children (13 to 18 years at RA).

We begin in column 1 of Table 3 by estimating ITT effects of the MTO treatments on annual W-2 wage earnings between 2008–2012. This regression is estimated with one observation per year per child and includes no controls other than randomization site indicators. To avoid measuring earnings when children are still in college, we only include observations in which a child is 24 or older. The standard errors, which are clustered by family, adjust for the repeated observations for each child.[19]

For children below age 13 at RA, mean W-2 earnings in the control group is \$9,549. Children assigned to the experimental voucher group have annual W-2 earnings that are \$1,340 (14 percent) higher on average than those in the control group. This estimate is significantly different from zero with $p < 0.05$. The estimated ITT effect of the Section 8 voucher is about half as large as the ITT effect

[19] In our baseline analysis, we do not trim the income measures. Top-coding income at \$100,000 yields similar estimates of mean treatment effects (online Appendix Table 3C, columns 9 and 10).

TABLE 3—IMPACTS OF MTO ON CHILDREN'S INCOME IN ADULTHOOD

	W-2 earnings ($) 2008–2012 ITT (1)	Individual earnings 2008–2012 ($) ITT (2)	ITT w/ controls (3)	TOT (4)	Individual earnings ($) Age 26 ITT (5)	2012 ITT (6)	Employed (%) 2008–2012 ITT (7)	Hhold. inc. ($) 2008–2012 ITT (8)	Inc. growth ($) 2008–2012 ITT (9)
Panel A. Children < age 13 at random assignment									
Exp. versus control	1,339.8**	1,624.0**	1,298.9**	3,476.8**	1,751.4*	1,443.8**	1.824	2,231.1***	1,309.4**
	(671.3)	(662.4)	(636.9)	(1,418.2)	(917.4)	(665.8)	(2.083)	(771.3)	(518.5)
Sec. 8 versus control	687.4	1,109.3	908.6	1,723.2	551.5	1,157.7*	1.352	1,452.4**	800.2
	(698.7)	(676.1)	(655.8)	(1051.5)	(888.1)	(690.1)	(2.294)	(735.5)	(517.0)
Observations	8,420	8,420	8,420	8,420	1,625	2,922	8,420	8,420	8,420
Control group mean	9,548.6	11,270.3	11,270.3	11,270.3	11,398.3	11,302.9	61.8	12,702.4	4,002.2
Panel B. Children age 13–18 at random assignment									
Exp. versus control	−761.2	−966.9	−879.5	−2,426.7	−539.0	−969.2	−2.173	−1,519.8	−693.6
	(870.6)	(854.3)	(817.3)	(2,154.4)	(795.4)	(1,122.2)	(2.140)	(11,02.2)	(571.6)
Sec. 8 versus control	−1,048.9	−1,132.8	−1,136.9	−2,051.1	−15.11	−869.0	−1.329	−936.7	−885.3
	(932.5)	(922.3)	(866.6)	(1,673.7)	(845.9)	(1213.3)	(2.275)	(11,85.9)	(625.2)
Observations	11,623	11,623	11,623	11,623	2,331	2,331	11,623	11,623	11,623
Control group mean	13,897.1	15,881.5	15,881.5	15,881.5	13,968.9	16,602.0	63.6	19,169.1	4,128.1

Notes: Columns 1–3 and 5–9 report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Column 4 reports TOT estimates using a 2SLS specification, instrumenting for voucher take-up with the experimental and Section 8 assignment indicators. Standard errors, reported in parentheses, are clustered by family. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment. The estimates in panels A and B are obtained from separate regressions. The number of individuals is 2,922 in panel A (except in column 5, where it is 1,625) and 2,331 in panel B. The dependent variable in column 1 is individual W-2 wage earnings, summing over all available W-2 forms. Column 1 includes one observation per individual per year from 2008–2012 in which the individual is 24 or older. Column 2 replicates column 1 using individual earnings as the dependent variable. Individual earnings is defined as the sum of individual W-2 and non-W-2 earnings. Non-W-2 earnings is adjusted gross income minus own and spouse's W-2 earnings, social security and disability benefits, and UI payments, divided by the number of filers on the tax return. Non-W-2 earnings is recoded to zero if negative and is defined as zero for non-filers. Column 3 replicates column 2, controlling for the characteristics listed in online Appendix Table 1A. Column 4 reports TOT estimates corresponding to the ITT estimates in column 2. In column 5, we measure earnings in the year when the individual is 26 years old. In column 6, we measure earnings in 2012, limiting the sample to those 24 or older in 2012. Columns 7–9 replicate column 1 with the following dependent variables: employment (an indicator for having positive W-2 earnings), household income (adjusted gross income plus tax-exempt social security benefits and interest income for those who file tax returns, the sum of W-2 wage earnings, SSDI benefits, and UI benefits for non-filers, and zero for non-filers with no W-2 earnings, SSDI, or UI benefits), and individual earnings growth (the change in individual earnings between year $t - 5$ and the current year t).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

of the experimental voucher. For children aged 13–18 at RA, the estimated effects of both the experimental and Section 8 vouchers are negative, although they are not statistically significant.

W-2 earnings do not include self-employment income, tips, or earnings from jobs that paid less than $1,800 a year, all of which may be important income sources for individuals in the MTO sample. We therefore turn in column 2 to a broader measure, which we call "individual earnings," that sums W-2 earnings and non-W-2 earnings using data from 1040 tax forms (see Section IIB for further details). For younger children, the ITT effect of the experimental voucher on individual earnings is \$1,624: again a 14 percent increase relative to the control group mean, which is \$11,270. The ITT effect of the Section 8 voucher is \$1,109 and is marginally significant, with $p = 0.101$. Once again, the estimated effects on the older children are negative but statistically insignificant.

The larger treatment effects on individual earnings than on W-2 earnings could potentially be driven by endogenous tax filing responses, as non-W-2 earnings are observed only for individuals who file tax returns.[20] We do find that the experimental voucher treatment increased federal tax filing rates 5.7 pp for younger children (see Table 12 below). However, mean non-W-2 earnings for tax filers in the control group—which is a plausible upper bound for non-filers—is only \$1,721. Hence, the 5.7 pp filing increase accounts for at most $0.057 \times \$1{,}721 = \$98$ of the increase in non-W-2 earnings for younger children, a small portion of the observed increase in non-W-2 earnings of \$284 (online Appendix Table 3C, column 2). Hence, the majority of the increase in non-W-2 earnings appears to be driven by real changes in earnings behavior, consistent with the fact that both non-W-2 and W-2 earnings rise by 14 percent in the experimental voucher group relative to the control group. We therefore use the broader "individual earnings" measure as our preferred measure of earnings in what follows.

Column 3 of Table 2 replicates the specification in column 2 including the baseline covariates used in the MTO final impacts evaluation (Sanbonmatsu et al. 2011), listed in online Appendix Table 1A. For younger children, the inclusion of these covariates reduces the point estimates by about 20 percent, approximately one-third to one-half of a standard error of the baseline estimates.[21] If one includes different subsets of the covariates, one can obtain point estimates that are slightly larger or smaller than the baseline estimates without controls. Importantly, the estimated coefficients generally fluctuate by less than half a standard error when we include different sets of covariates and thus are not statistically significant from each other, consistent with random assignment and balance across the treatment arms.

For completeness, we report estimates with the full set of baseline controls for all the other specifications in Table 3 in online Appendix Table 3A. The inclusion of controls tends to yield slightly smaller estimated MTO treatment effects relative to the specifications without controls, but the differences are not statistically indistinguishable from each other and do not alter the qualitative conclusions. In particular, the experimental voucher treatment has a large, statistically significant positive effect on earnings of younger children, the Section 8 voucher has smaller positive, marginally significant effects on younger children, and the effects of both treatments on older children are negative and statistically insignificant.[22] We also consistently find significant treatment effects ($p < 0.05$) for the younger children, both with and

[20] In contrast, W-2 earnings are observed for all individuals, irrespective of whether they file tax returns or not. Hence, the estimate in column 1 of Table 3 is unaffected by concerns about endogenous reporting.

[21] The changes in the coefficients are due to differences in the characteristics of the treatment and control groups that arise from sampling error. For example, the drop in the experimental treatment effect is driven primarily by the fact that the experimental group has slightly more educated parents (online Appendix Table 1A).

[22] For a few outcomes, such as W-2 earnings, the inclusion of controls attenuates the estimates to the point where the estimates are no longer significant with $p < 0.05$. For example, column 1 of online Appendix Table 3A shows the ITT impact on W-2 earnings for the younger children in the experimental group is \$1,016.8 with a standard error of \$640 ($p = 0.11$). However, the point estimate of \$1,016.8 is not statistically distinguishable from the baseline estimate without controls (\$1,339.8).

without controls, when we pool the Section 8 and experimental groups into a single treatment group (online Appendix Table 11, columns 7–8).

Column 4 of Table 3 reports TOT estimates on individual earnings using the specification in (2). These TOT estimates correspond to the ITT estimates reported in column 2; we report TOT estimates corresponding to all the other ITT specifications in Table 3 in online Appendix Table 3B. The estimates in column 4 show that children whose families took up the experimental voucher and moved when they were young (below age 13, age 8.2 on average) experience an increase in annual individual earnings in early adulthood of \$3,477. This is a 31 percent increase relative to the control group mean earnings of \$11,270 and a 34 percent increase relative to the "control complier mean" (CCM) of \$10,165 (online Appendix Table 3B, column 4).[23]

Section 8 moves lead to a TOT increase in individual earnings of \$1,723 per year (15 percent of the control group mean and 16 percent of the Section 8 CCM) for younger children. The Section 8 TOT effect on earnings is roughly half as large as the TOT effect of the experimental voucher. This mirrors the fact that the Section 8 TOT effect of -12 pp on mean tract-level poverty rates from RA until age 18 was also roughly half as large as the experimental voucher TOT effect of -22 pp on poverty rates (Table 2, column 5). Dividing the TOT effects on earnings by the TOT effects on poverty rates, we infer that growing up in a census tract with a 10 pp lower poverty rate starting at a young age (age 8.2 on average) increases earnings in adulthood by about 13–15 percent.

The TOT estimates for older children are around $-\$2,000$ for both treatments but are not statistically distinguishable from zero. However, we can reject the hypothesis that the effects of the experimental voucher for the older children are the same as those for the younger children with $p = 0.02$ (see Section IVC).

In our baseline specifications, we measure earnings for younger children at an earlier age than for older children. In column 5, we replicate the specification in column 2 measuring earnings at age 26 for all children.[24] This specification yields roughly similar estimates, showing that the age differences are not responsible for the larger effects observed for younger children. In column 6, we measure earnings using data from the most recent available year (2012) for all children to evaluate whether differences in the calendar year when income is measured affect the results. Again, this specification yields similar estimates, with significant gains for younger children and negative point estimates for the older children.

In column 7, we estimate the ITT effects of the MTO treatments on employment rates. This specification replicates column 2 using an indicator for having any W-2 earnings in a calendar year as the dependent variable. MTO treatments have small, statistically insignificant impacts on the extensive margin of employment. The ITT for employment of the young experimental children is 1.8 pp, a 3 percent increase relative to the control group mean of 61.8 percent. Thus, MTO's impacts

[23] The CCM is an estimate of mean earnings for those in the control group who would have taken up the experimental voucher had they been assigned to the experimental group. The experimental CCM is calculated as mean earnings among compliers (i.e., those who took up the voucher) in the experimental group minus the TOT estimate of the experimental treatment effect, as in Kling, Liebman, and Katz (2007).

[24] Earnings comparisons at age 26 limit the sample of younger MTO children to those who were 8 to 12 years old at RA with a mean of 10.7 years as compared to a mean age at RA for the older MTO children of 15.1 years.

on increasing earnings for younger children appear to be driven primary by higher wage rates and/or greater hours worked in a year rather than by changes in whether or not individuals work at all over the course of a year.

Household Income.—In column 8, we estimate ITT effects on household income. Household income expands upon our individual earnings measure by including spouse's income (for married tax filers), unemployment insurance income, and social security and disability (SSDI) income (see Section IIB for details). For younger children, the experimental ITT effect on household income is \$2,231, \$607 larger than the ITT on individual earnings reported in column 2. The experimental ITT on household income is significantly different from zero with $p < 0.01$. The Section 8 ITT effect on household income is \$1,452 and is significantly different from zero with $p < 0.05$. The effects of the treatments on the household income of older children remain negative and statistically insignificant.

We investigate why the MTO treatments have larger effects on household income than on individual earnings for younger children in online Appendix Table 3C, which shows ITT effects on the components of income that contribute to household income. The additional \$607 impact of the experimental voucher on household income relative to individual earnings is predominantly driven by spousal income, which is \$521 higher in the experimental group for younger children. Part of the observed effect on spousal income could be driven by endogenous tax filing, as spousal income is only observed for individuals who file tax returns. However, calculations analogous to those above imply that at most $0.057 \times \$802.1 = \$46$ of the \$521 experimental impact on spousal income can be accounted for by a filing response, assuming that the mean spousal income of married non-filers is no larger than the mean spousal income of \$802 for tax filers in the control group.[25] This increase in spousal income can be entirely accounted for by the effect of the experimental voucher treatment on marriage rates (rather than an increase in a given spouse's level of earnings), as we show in Table 5 below.[26] The experimental voucher treatment also increases unemployment benefits by \$167 per year, possibly because higher labor force participation rates increase eligibility for unemployment benefits. It *reduces* social security and disability benefits by \$98 per year, consistent with increases in labor supply and earnings.

Earnings Trajectories.—Earnings rise steeply in the mid to late twenties as children complete education and enter the labor force (Haider and Solon 2006). Thus, one might expect the treatment effects of MTO to grow as we measure children's earnings at later ages. Figure 1 plots estimates of the ITT effect of the experimental voucher treatment on individual earnings, varying the age at which earnings are

[25] Part of the observed effect on spousal income could be driven by endogenous tax filing, as spousal income is only observed for individuals who file tax returns. However, calculations analogous to those above imply that at most $0.057 \times \$802.1 = \$46$ of the \$521 experimental impact on spousal income can be accounted for by a filing response, assuming that the mean spousal income of married non-filers is no larger than the mean spousal income of \$802 for tax filers in the control group.

[26] For younger children, the experimental voucher increases the fraction married by 1.9 percentage points (Table 5, column 1). The mean individual income of spouses in the control group for married individuals is \$25,568. If the marginal individuals marry individuals with average income, we would predict an increase in household income of $0.019 \times \$25,568 = \$486$, similar to the observed increase of \$521.

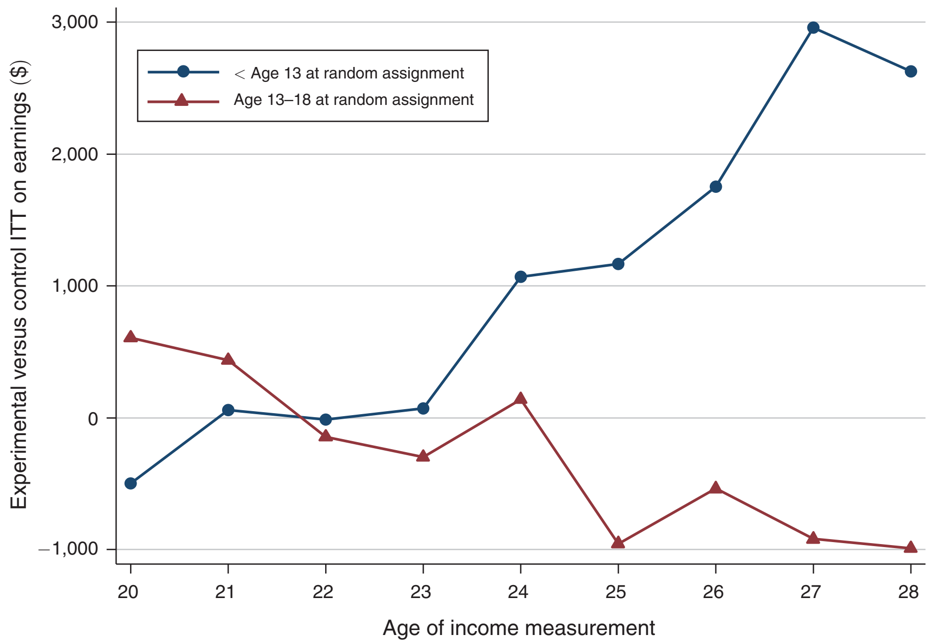

Figure 1. Impacts of Experimental Voucher by Age of Earnings Measurement

Notes: This figure presents ITT estimates of the impact of being assigned to the experimental voucher group on individual earnings, varying the age at which earnings is measured from 20 to 28. The estimate at each age is obtained from an OLS regression (weighted to adjust for differences in sampling probabilities across sites and over time) of individual earnings at that age on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. We plot the coefficient on the experimental voucher group indicator in this figure; the corresponding estimates for the Section 8 voucher group are shown in online Appendix Figure 1. The series in circles restricts the sample to children below age 13 at random assignment; the series in triangles includes children between age 13 and 18 at random assignment. The estimates in the two series are obtained from separate regressions. The estimates at age 26 exactly match those reported in column 5 of Table 3; the remaining estimates replicate that specification, varying the age at which earnings is measured. The null hypothesis that the experimental impacts do not vary with the age of income measurement is rejected with $p < 0.01$ for the below 13 series and $p = 0.06$ for the age 13–18 series. See notes to Table 3 for further details on specifications and variable definitions.

measured from 20 to 28. These effects are estimated using specifications analogous to that in column 5 of Table 3. The MTO experimental impact does in fact rise sharply with age of income measurement for the younger children. The null hypothesis that the experimental impacts do not vary with the age at which income is measured is rejected with $p < 0.01$.[27] In contrast, the treatment effects fall significantly with the age at which income is measured for the older children, implying that they not only have lower earnings but also have less earnings growth in their early career relative to those in the control group. A similar pattern of rising treatment effects with age of income measurement for younger children and declining effects with

[27] To estimate this p-value, we regress earnings on the treatment group indicators linearly interacted with the age of income measurement, controlling for age of income measurement fixed effects interacted with site fixed effects. The p-value is based on the coefficient for the interaction of age at income measurement with the experimental treatment indicator. We estimate this regression in a dataset with one observation per age of income measurement per child and cluster standard errors by family.

age of income measurement for older children is observed for the Section 8 group, although the estimates are noisier and attenuated (online Appendix Figure 1).

Column 9 quantifies the effects of the MTO treatment on earnings growth over a five-year period. The dependent variable in this specification is the difference in individual earnings in year t minus year $t - 5$; as in the other specifications in Table 3, we restrict the sample to observations in which the individual is 24 or older in year t. In the control group, the mean level of income growth over five years is \$4,002 for younger children. The ITT effect of the experimental voucher on five-year income growth is \$1,309 (a 33 percent increase), while the ITT effect of the Section 8 voucher is \$800 (a 20 percent increase). These results suggest that our baseline estimates, which measure income starting at age 24, likely understate the total lifetime earnings impacts of the MTO experimental voucher on children who were young at the point of the move.

Summary.—In sum, our analysis of children's income in adulthood yields three robust findings. First, the MTO experimental voucher treatment substantially increased the earnings of children who were young (below age 13) at the point of the move, with a TOT impact on individual earnings of approximately 35 percent. Second, the Section 8 voucher increased individual earnings of young children about half as much as the experimental voucher, consistent with the fact that it reduced neighborhood poverty rates half as much. Third, the impacts of both treatments on older children are somewhat negative (although not statistically significant).

These three facts are consistent with a simple model that combines positive exposure effects from moving to lower-poverty neighborhoods with a negative disruption cost of moving to such a neighborhood. Such a model would generate our empirical results because the exposure effects outweigh the disruption cost for children who move when young, but not for children who move at older ages. Note that because families in both the control and treatment groups moved frequently, the disruption cost must reflect the cost of moving to a different type of neighborhood (as induced by the MTO voucher treatments, especially the experimental voucher) rather than a fixed cost of moving houses within the same neighborhood or a similar nearby neighborhood (as typically occurred in the control group).

C. *College Attendance and Quality*

In Table 4, we examine MTO impacts on college attendance rates and college quality. Table 4 and the subsequent tables are structured in the same way as Table 3: panel A reports estimates for younger children (below age 13 at RA), while panel B reports estimates for older children (ages 13–18 at RA). We report ITT estimates using the specification in (1), with no additional controls other than randomization site indicators. For this and all subsequent outcomes, the corresponding online Appendix tables with the same number provide ITT estimates with the full set of controls and TOT estimates corresponding to the specifications in the main table.

We begin in column 1 by analyzing treatment effects on college attendance rates between the ages of 18–20. College attendance is measured using 1098-T forms as discussed in Section IIB. This regression includes one observation per child at ages 18, 19, and 20; the standard errors, which are clustered by family, adjust for

Table 4—Impacts of MTO on Children's College Attendance Outcomes

	College attendance (%) ITT					College quality ($) ITT				
	Age 18–20 (1)	Age 18 (2)	Age 19 (3)	Age 20 (4)	Age 21 (5)	Age 18–20 (6)	Age 18 (7)	Age 19 (8)	Age 20 (9)	Age 21 (10)
Panel A. Children < age 13 at random assignment										
Exp. versus control	2.509**	2.213*	2.579*	2.734*	0.409	686.7***	670.2***	800.6***	589.3**	337.8
	(1.143)	(1.200)	(1.452)	(1.464)	(1.474)	(231.2)	(240.6)	(274.3)	(262.3)	(269.9)
Sec. 8 versus control	0.992	1.221	0.502	1.252	−0.371	632.7**	592.0**	604.7**	701.4**	549.2*
	(1.264)	(1.303)	(1.613)	(1.599)	(1.592)	(256.3)	(268.2)	(304.7)	(294.9)	(293.7)
Observations	15,027	5,009	5,009	5,009	5,009	15,027	5,009	5,009	5,009	5,009
Control group mean	16.5	11.3	18.6	19.6	20.1	20,914.7	20,479.6	21,148.7	21,115.7	21,152.3
Panel B. Children age 13–18 at random assignment										
Exp. versus control	−4.261**	−5.866***	−4.460**	−2.995	−3.528*	−882.8**	−1195.7**	−890.0*	−672.6	−687.9*
	(1.712)	(2.180)	(2.162)	(2.077)	(1.972)	(385.5)	(482.8)	(465.0)	(414.2)	(402.6)
Sec. 8 versus control	−3.014*	−3.339	−3.928*	−1.882	−4.455**	−597.2	−581.5	−730.2	−492.1	−603.0
	(1.785)	(2.295)	(2.243)	(2.182)	(2.030)	(434.2)	(546.9)	(511.5)	(465.7)	(446.6)
Observations	5,100	1,328	1,722	2,050	2,234	5,100	1,328	1,722	2,050	2,234
Control group mean	15.6	12.4	16.8	16.6	17.2	21,638.0	21,337.3	21,880.1	21,629.8	21,597.8

Notes: All columns report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Standard errors, reported in parentheses, are clustered by family. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment. The estimates in panels A and B are obtained from separate regressions. The dependent variable in column 1 is an indicator for attending college in a given year (having one or more 1098-T tax forms filed on one's behalf), pooling data over the three years when the individual is ages 18–20 with one observation per year per individual. Years before 1999 are excluded because 1098-T data are available beginning only in 1999. Columns 2–5 replicate column 1, using college attendance at each age between 18 and 21 as the dependent variable. The dependent variable in column 6 is Chetty, Friedman, and Rockoff's (2014) earnings-based index of college quality, again pooling data from ages 18–20 starting in 1999. This index is constructed using US population data as the mean earnings at age 31 of students enrolled in that college at age 20; children who do not attend college are assigned the mean earnings at age 31 of children who are not enrolled in any college at age 20. Columns 7–10 replicate column 6, using college quality at each age between 18 and 21 as the dependent variable.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

the repeated observations for each child. For younger children (panel A), the mean college attendance rate between the ages of 18–20 in the control group is 16.5 percent. Children assigned to the experimental voucher group are 2.5 percentage points (pp) more likely to attend college between the ages of 18–20. The corresponding TOT effect for children whose families took up the experimental voucher is a 5.2 pp increase in college attendance rates, a 32 percent increase relative to the control group mean, and a 34 percent increase relative to the control complier mean (online Appendix Table 4B, column 1).

The Section 8 voucher also has a positive ITT effect of 1 pp, but it is not statistically significant. In contrast, for the older children, both MTO treatments have large and statistically significant negative effects. The experimental ITT is −4.3 pp, while the Section 8 ITT is −3 pp. These findings mirror the patterns observed for earnings, although the negative impacts on college attendance for older children are larger than on earnings.

Columns 2–5 present estimates of impacts on college attendance rates by age, from age 18 to 21. The MTO experimental treatment increased college going for younger children in the period immediately following high school, but had little effect beyond age 20. For younger children, the experimental ITT effects are approximately 2.5 pp from ages 18–20, but fall to 0.4 pp at age 21. The Section 8 ITT estimates exhibit a similar pattern, with positive effects of around 1 pp from ages 18–20 and a small negative estimated effect at age 21.[28] Meanwhile, the ITT effects on older children are consistently negative at all ages for both treatments.

Next, we investigate whether the MTO treatments also changed the types of colleges that students attended. To do so, we use a simple earnings-based index of college "quality," defined as the mean earnings at age 31 of all US residents enrolled in a given college at age 20 (see Section IID for details). For those not enrolled in any college at age 20, the index equals the mean earnings at age 31 of all US residents not enrolled in college at age 20. We define college quality at age a for each child in the MTO sample based on the college in which the child was enrolled at age a.

Column 6 of Table 4 replicates the specification in column 1 using college quality, measured between the ages of 18–20, as the dependent variable. For younger children, the experimental voucher increases mean college quality between the ages of 18–20 by \$687—that is, expected earnings at age 31 are \$687 higher for the experimental voucher group relative to the control group given the colleges that children attend.[29] This estimate is significantly different from zero with $p < 0.01$. This increase of \$687 reflects a combination of extensive-margin responses (higher college attendance rates) and intensive-margin responses (attending a better college conditional on attending). We derive an upper bound on the extensive margin effect by assuming that those who are induced to attend college attend a college of average quality, which is a plausible upper bound for the quality of the college attended by the marginal college student. The mean college quality conditional on attending college for younger children in the control group is \$31,409, while the quality for all those who do not attend college is \$18,867. This suggests that at most $(31{,}409 - 18{,}867) \times 0.025 = \$314$ of the \$687 impact is due to the extensive margin response. Hence, the MTO experimental voucher appears to improve not just college attendance rates but also the quality of colleges that students attend.[30]

The Section 8 voucher also has a large positive effect on college quality for younger children that is significant with $p < 0.05$. The estimated effects on college quality for older children are negative and substantial in magnitude. The difference between the positive MTO experimental impact on college quality for younger

[28] We find small, statistically insignificant estimates on college attendance at older ages (up to age 25), similar to those at age 21. Thus, the positive MTO experimental treatment effect on college attendance for younger children from ages 18 to 20 does not appear to be driven purely by retiming of college attendance. However, our sample size declines when looking at older ages, so we cannot rule out some degree of retiming.

[29] The increase in actual individual earnings of \$1,624 (Table 3, column 2) is larger than the \$686 impact on projected earnings at age 31 based on college attendance. This indicates that the MTO treatment effects on earnings go beyond what one would expect just from the labor market returns to increased college attainment. This is to be expected given the fact that even in the experimental voucher group, more than 80 percent of children do not attend college.

[30] The point estimates of the treatment effects on college attendance and quality are slightly smaller when we include controls (online Appendix Table 4A). With controls, we estimate that the experimental treatment increased college attendance rates by 1.7 pp from ages 18–20 (as compared to 2.5 pp without controls) and increased college quality by \$536.2 (as compared to \$687 without controls).

children and the large negative effect for older children is highly statistically significant ($p = 0.0006$), as shown in Table 11 below. The treatment impacts on college quality by age, shown in column 7–10 of Table 4, are similar to those for college attendance rates. The effects are larger between ages 18–20 and become smaller at age 21, suggesting that most of the marginal children induced to attend college do so immediately after high school.

Overall, the positive MTO treatment impacts on college outcomes for younger children and negative impacts for older children closely mirror the impacts on earnings in Table 3. These results further support the view that moving to lower-poverty areas improves outcomes when one moves as a young child but not at older ages.

D. *Marriage and Fertility*

We next examine MTO treatment impacts on children's marriage and fertility outcomes in Table 5. Columns 1–3 present ITT effects of the MTO treatments on marriage rates, based on whether the individual files a tax return jointly with a spouse. We include one observation per child per year from 2008–2012, limiting the sample to observations where children are 24 or older. In column 1, we pool males and females. For younger children, the experimental treatment increased the fraction married in early adulthood by 1.9 pp, while the Section 8 treatment increases the fraction married by 2.8 pp. These changes are quite large relative to the fraction married in the control group, which is only 3.4 percent.[31] The MTO treatment effects for the younger children are substantially larger for females, for whom the marriage rate nearly doubles, than for males, for whom the effects are small and not statistically significant (columns 2 and 3). There are no detectable treatment effects on marriage for the older children.

In columns 4–7, we study the fertility behavior of the female children in the MTO sample, which we infer from applications from SSNs for children (see Section IIB). These specifications include one observation for each female child because the outcomes are time-invariant. Columns 4 and 5 show that the MTO treatments do not have statistically significant effects on overall birth rates or teenage birth rates for either the younger or older female children. However, the experimental voucher treatment does change the family circumstances of births substantially, in particular the presence of the father at the birth. We measure whether the father is present at the child's birth by whether his name and SSN are listed on the child's SSN application (which is typically submitted when the child is born). In column 6, we restrict the sample to females who have a birth and use an indicator for having a father listed on the first-born child's SSN application as the dependent variable. We find that the experimental voucher treatment increases the share of births in which the father is present by 6.8 pp for younger children. This leads to a significant decline of 4.8 pp in

[31] Because we only observe marital status for those who file tax returns, part of the observed response could be due to the increase in tax filing rates that we document in Table 8 below, but this bias is likely to be very small. The experimental voucher treatment increased federal tax filing rates 5.7 percentage points for younger children. If the marginal filer had the same probability of being married as individuals in the control group (3.4 percent), then endogenous filing accounts for at most $0.057 \times 0.034 = 0.2$ percentage points of the 1.9 pp increase in marriage rates that we observe. The more plausible explanation is that the increase in marriage rates induced by the treatments led to the increase in tax filing rates documented in Table 8, as virtually all married working-age couples file tax returns (Cilke 1998).

Table 5—Impacts of MTO on Marriage and Fertility (*Percentage Points*)

Sample:	All	Males	Females				
Dependent variable:	Married (1)	Married (2)	Married (3)	Has birth (4)	Teen birth (5)	Father on birth cert. (6)	Birth with no father present (7)
Panel A. Children < age 13 at random assignment							
Exp. versus control	1.934**	0.738	3.341**	−2.253	−0.670	6.849**	−4.807**
	(0.892)	(1.038)	(1.476)	(2.515)	(2.117)	(3.322)	(2.352)
Sec. 8 versus control	2.840***	1.020	4.731***	−0.285	2.409	2.671	−1.318
	(1.055)	(1.092)	(1.831)	(2.679)	(2.375)	(3.523)	(2.562)
Observations	8,420	4,384	4,036	2,409	2,379	1,410	2,409
Control group mean	3.4	2.7	4.1	59.1	19.9	44.1	33.0
Panel B. Children age 13–18 at random assignment							
Exp. versus control	−0.0637	−1.441	1.173	−2.589	−2.404	−8.259**	4.253
	(1.368)	(1.848)	(1.988)	(3.107)	(3.172)	(4.153)	(3.626)
Sec. 8 versus control	0.654	−0.577	1.811	−0.547	−0.635	−0.182	−0.701
	(1.465)	(1.946)	(2.181)	(3.304)	(3.579)	(4.409)	(3.807)
Observations	11,623	5,852	5,771	1,158	1,141	888	1,158
Control group mean	9.3	9.3	9.2	77.8	24.7	46.7	41.4

Notes: All columns report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Standard errors, reported in parentheses, are clustered by family. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment. The estimates in panels A and B are obtained from separate regressions. Column 1 includes one observation per individual per year from 2008–2012 in which the individual is 24 or older. The dependent variable in column 1 is an indicator for filing a tax return as a married individual in a given year. Columns 2 and 3 replicate column 1 for males and females, respectively. Columns 4–7 restrict the sample to females and include one observation per individual. The dependent variable in column 4 is an indicator for having a child before June 2014. In column 5, it is an indicator for having a child before the age of 19. Column 6 restricts the sample to females who have a birth; the dependent variable in this column is an indicator for having a father listed on the first-born child's SSN application. In column 7, the dependent variable is an indicator for having one or more births, with no father listed on the SSN application for the first birth.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

the fraction of females who have a birth with no father present, as shown in column 7.[32] The TOT effect corresponding to this estimate is −10.0 pp, implying that girls whose families moved using the experimental voucher when they were young are 26 percent less likely to become single mothers (online Appendix Table 5B, column 7).

As with other outcomes, the Section 8 voucher has smaller effects on the father's presence at birth than the experimental voucher. And the older female children in the MTO experimental group are much *less* likely to have a father listed on the birth certificate when they have births relative to the control group. Hence, marriage and fertility behavior exhibit what is now a familiar pattern of effects, with significant increases in marriage rates and reductions in single parenthood for children who moved to lower-poverty neighborhoods when young, but no change or opposite-signed effects for children who made the same moves at an older age.

[32] Unlike column 6, where we focus on the endogenously selected sample of girls who have births, the specification in column 7 is estimated on the full sample of all young girls in the MTO data, using an indicator for having a birth with no father present as the dependent variable.

Table 6—Impacts of MTO on Children's Neighborhood Characteristics in Adulthood

	Poverty share in zip 2008–2012 (%) (1)	Mean income in zip 2008–2012 ($) (2)	Black share in zip 2008–2012 (%) (3)	Single mother share in zip 2008–2012 (%) (4)
Panel A. Children < age 13 at random assignment				
Exp. versus control	−1.592***	1,345.9***	−2.852**	−1.812**
	(0.602)	(489.5)	(1.417)	(0.862)
Sec. 8 versus control	−1.394**	1,322.0**	−5.654***	−3.087***
	(0.699)	(558.6)	(1.714)	(1.001)
Observations	6,649	6,649	6,651	6,648
Control group mean	23.8	25,014.3	43.0	42.0
Panel B. Children age 13–18 at random assignment				
Exp. versus control	−0.523	604.2	0.465	−0.294
	(0.643)	(478.5)	(1.654)	(0.940)
Sec. 8 versus control	−0.928	442.0	−2.631	−1.856*
	(0.698)	(524.2)	(1.715)	(0.976)
Observations	9,149	9,149	9,149	9,148
Control group mean	23.6	25,170.5	39.6	40.1

Notes: All columns report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Standard errors, reported in parentheses, are clustered by family. In this table, we only include observations where zip code information in the relevant year is available (based on 1040 tax returns, W-2's, or other information returns). In 2012, zip code data are available for 79.56 percent of observations for children age 24 or older. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment. The estimates in panels A and B are obtained from separate regressions. Outcome variables are defined using zip code–level data from the 2000 census. All columns include one observation per individual per year from 2008–2012 in which the individual is 24 or older and in which zip code information is available. The dependent variable in column 1 is the poverty share (share of households below the poverty line in the 2000 census) in the individual's zip code. Columns 2–4 replicate column 1 with the following dependent variables: mean income in the zip code (aggregate income divided by the number of individuals 16–64), black share (number of people who are black alone divided by total population in 2000) and single mother share (number of households with female heads and no husband present with own children present divided by the total number of households with own children present).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

E. *Neighborhood Characteristics in Adulthood*

The MTO vouchers substantially reduced the degree of neighborhood poverty experienced by MTO children during their childhood (Table 2). Do these childhood improvements in neighborhood environments persist into adulthood, providing better neighborhoods for the next generation (the children of MTO children)? In Table 6, we answer this question using information drawn from tax records on the zip codes where MTO children live in adulthood.[33]

Among younger children (panel A), both the experimental and Section 8 children live in better neighborhoods in adulthood relative to the control group children on a wide range of measures. In column 1, we measure ITT effects on zip code–level

[33] We are unable to obtain zip codes for 20.4 percent of the children because we do not have tax returns or W-2 forms for them. The rate of missing zip code data does not vary across the treatment and control groups.

poverty rates, with one observation per child per year from 2008–2012 (only including observations where children are age 24 or older). The experimental ITT estimate is -1.6 percentage points, about one-third as large as the treatment effect on the average poverty rate in the zip code where the individual lived in childhood (Table 2, column 6). Columns 2–4 examine impacts on other neighborhood characteristics using the same specification as in column 1. They show that children assigned to the experimental group also live in areas with higher mean income, less racial segregation (lower share of black residents), and a lower share of female-headed households. All of these treatment effects are significantly different from zero with $p < 0.01$. In contrast, the MTO treatments on adult neighborhood quality are smaller and typically not statistically significant for the older MTO children, as seen in panel B of Table 6.

Together, Tables 5 and 6 indicate that the improvements in neighborhood environments for the younger MTO children lead to better neighborhood and family environments for the next generation, the grandchildren of the original MTO parents. Relative to the grandchildren in the control group, the grandchildren in the experimental group are more likely to be raised in lower-poverty neighborhoods by two parents who have a higher level of household income and are more likely to have attended college. In short, subsidized housing vouchers produce durable benefits that persist into subsequent generations for children who moved to lower-poverty neighborhoods at young ages.

F. *Heterogeneity of Treatment Effects*

Prior work has found that the MTO treatments had more positive effects on female children than on male children in terms of mental health, physical health, risky behaviors, and educational outcomes during adolescence (Kling, Liebman, and Katz 2007; Sanbonmatsu et al. 2011; Ludwig et al. 2013). In Table 7, we reexamine the heterogeneity of MTO treatment effects by child gender, but look at outcomes in adulthood.

In contrast to the substantially more favorable MTO treatment impacts for female than male children when they were teenagers, we find roughly similar impacts by gender when observing the MTO children as adults. Table 7 shows ITT estimates by gender for individual earnings, college quality, and zip code–level poverty rates in adulthood. Columns 1 and 2 show experimental ITT estimates by gender, while columns 3 and 4 show Section 8 ITT estimates by gender. We show the mean value of the dependent variable for the control group in the relevant estimation sample in square brackets to facilitate interpretation of magnitudes.[34]

For younger children (panel A), the experimental ITT effect on individual earnings in adulthood (age 24 and above) for boys is \$1,679, an estimate that is significantly different from zero with $p = 0.085$. The comparable estimate for girls is a very similar \$1,439 ($p = 0.104$). The Section 8 ITT effects are slightly smaller for both boys and girls, at approximately \$1,100. The experimental and Section 8

[34] An interesting feature of the data presented in Table 7 is that female MTO children have substantially higher adult earnings on average than male MTO children for both the younger and older groups—a striking reversal of the usual gender earnings gap favoring men. The difference arises from much higher employment rates for female than male MTO children, likely reflecting changes in labor market outcomes by gender in US disadvantaged populations.

TABLE 7—HETEROGENEITY OF TREATMENT EFFECTS BY GENDER

	Experimental versus control		Section 8 versus control	
	Male (1)	Female (2)	Male (3)	Female (4)
Panel A. Children < age 13 at random assignment				
Individual earnings 2008–2012 ($) ITT	1,679.2*	1,438.9	1,073.8	1,097.6
	(973.3)	(885.1)	(947.1)	(931.6)
	[10,020.8]	[12,634.2]	[10,020.8]	[12,634.2]
College quality 18–20 ($) ITT	522.3*	819.4**	327.4	895.4**
	(290.1)	(345.0)	(290.2)	(399.2)
	[20,443.8]	[21,440.2]	[20,443.8]	[21,440.2]
Zip poverty share 2008–2012 (%) ITT	−1.498**	−1.519**	−0.733	−2.055***
	(0.720)	(0.674)	(0.793)	(0.766)
	[22.5]	[24.5]	[22.5]	[24.5]
Panel B. Children age 13–18 at random assignment				
Individual earnings 2008–2012 ($) ITT	−1,832.4	−204.9	−1,410.6	−740.6
	(1,298.3)	(1,044.6)	(1,363.6)	(1,165.4)
	[14,814.4]	[16,997.5]	[14,814.4]	[16,997.5]
College quality 18–20 ($) ITT	−590.4	−1,201.4**	−27.44	−1,105.8*
	(496.6)	(582.3)	(596.4)	(618.6)
	[20,935.2]	[22,348.0]	[20,935.2]	[22,348.0]
Zip poverty share 2008–2012 (%) ITT	0.281	−0.627	0.844	−1.241
	(1.069)	(0.954)	(1.149)	(1.059)
	[21.8]	[24.2]	[21.8]	[24.2]

Notes: This table replicates the ITT OLS regression specifications in column 2 of Table 3 (individual earnings), column 6 of Table 4 (college quality), and column 1 of Table 6 (poverty share) separately for male and female children. Columns 1 and 2 report the coefficient on the indicator for being assigned to the experimental voucher group; columns 3 and 4 report the coefficient on the indicator for being assigned to the Section 8 voucher group. The estimates in columns 1 and 3 are for males, and the two estimates in each row of these columns come from a single OLS regression analogous to that in column 2 of Table 3. The estimates in columns 2 and 4 for females are constructed analogously. Standard errors, reported in parentheses, are clustered by family. Control group means for each estimation sample are reported in square brackets. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment. See the notes to Tables 3, 4, and 6 for further details on specifications and variable definitions.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

treatments also improve college quality (at ages 18–20) and neighborhood quality (at age 24 and above) for both boys and girls. Conversely, we find adverse long-term treatment effects for both boys and girls who were above age 13 at RA (panel B).

The positive effects of the MTO treatments on adult outcomes for the younger boys point to an intriguing dynamic pattern of neighborhood effects when combined with results from prior work. Previous work found positive initial impacts of MTO moves on young boys (at 1 to 3.5 years after random assignment), who had a significantly lower incidence of problem behaviors (Katz, Kling, and Liebman 2001). But boys who moved to lower-poverty areas at young ages in the experimental group were doing moderately worse than those in the control group as teens in terms of risky behaviors and education (Sanbonmatsu et al. 2011). This pattern has now turned around to a strongly positive one in terms of labor market and educational outcomes in early adulthood. One speculative explanation for these patterns is that teenage misdeeds may have smaller adverse consequences and second chances may be more available for youth in middle-class neighborhoods than in more-distressed neighborhoods. The positive MTO experimental impacts in adulthood for the younger female youth are less surprising, as previous work found significant positive impacts for the younger females in the experimental group both as teens in the interim evaluation and continued modestly positive effects as older teens in the final evaluation.

We also explored heterogeneity of the MTO treatment effects by race and ethnicity (online Appendix Table 7A) and across the five randomization sites (online Appendix Table 7B). The MTO experimental voucher increased individual earnings in adulthood and college quality for children below age 13 at RA in every racial group (black, Hispanic, and white) and in all five sites (Baltimore, Boston, Chicago, New York, and Los Angeles).[35] The treatment effects on earnings and college quality are larger in the sites where the treatments led to larger reductions in neighborhood poverty rates. In contrast, the estimated effects for the older children (ages 13–18 at RA) are negative in virtually all the subgroups for each of the outcomes.

In summary, the main lesson of the heterogeneity analysis is that the long-term benefits of childhood exposure to lower-poverty neighborhoods are highly robust across genders, racial groups, and geographic locations.

G. *Age Pattern of Exposure Effects*

Thus far, we have split the MTO children into "younger" versus "older" children using a cutoff of age 13 at RA. In this section, we assess the sensitivity of our results to the choice of this cutoff and evaluate how the effects of the MTO treatments vary with a child's age at move more generally.

As a first step, we replicate the baseline specifications in Tables 3–6, varying the cutoff used to split the sample. We find very similar estimates if we define "young" children as those below age 12 at RA or those below age 14 at RA (online Appendix Table 11). In particular, the estimated effects of the experimental voucher on individual earnings, college quality, neighborhood poverty share, and fraction married are all significantly different from 0 at conventional significance levels, with point estimates similar to the baseline estimates. The Section 8 voucher also has positive effects in all cases, most of which are smaller than the experimental voucher impacts but still significantly different from 0.

Linear Exposure Models.—A different way to assess how the MTO treatment effects vary with children's age at move is to estimate models that interact age at move linearly with the treatment indicators instead of splitting children into two groups. Pooling all children, we regress outcomes (y) on the MTO treatment group indicators (Exp and $S8$) and interactions of these treatment group indicators with the age at random assignment ($AgeRA$),

$$y_i = \alpha + \beta_{E0} Exp_i + \beta_{S0} S8_i + \beta_{EA} Exp_i \cdot AgeRA_i + \beta_{SA} S8_i \cdot AgeRA_i + sa_i \gamma + \epsilon_i, \tag{3}$$

[35] Note that the subgroup-specific estimates are naturally much less precise because of the smaller sample sizes, and hence are not statistically significant in many cases.

controlling for randomization site indicators interacted with indicators for age at RA (sa_i). The coefficients on the main effects for treatment group assignment (β_{E0} and β_{S0}) can be interpreted as the ITT impact of being offered a voucher to move to a better neighborhood at birth. The coefficients on the interaction terms with age at RA (β_{EA} and β_{SA}) can be interpreted as the average reduction in the ITT effects per year of reduced exposure to the new area. Note that we observe college outcomes only for children who were four or older at RA and earnings only for those who were six or older at RA. Hence, the estimates of impacts at birth rely on out-of-sample extrapolations based on the linear functional form.

Table 8 presents estimates of (3) for individual earnings (column 1), household income (columns 2 and 3), college quality (column 4), marriage rates (column 5) and the zip code poverty share in adulthood (column 6). Column 7 reports effects on total taxes paid—an outcome that we return to in Section V below. Online Appendix Table 8A replicates the ITT estimates in Table 8 including the baseline controls. Online Appendix Table 8B presents TOT estimates, estimated using a 2SLS specification where we instrument for voucher take-up (and the interactions) using treatment assignment indicators.

The estimates in Table 8 indicate large and statistically significant beneficial impacts of the experimental treatment for all the outcomes, with the gains declining rapidly with age at RA. In other words, the benefits of being offered an MTO experimental voucher increase with potential years of childhood exposure to better neighborhoods. For example, the experimental ITT estimates for individual earnings in column 1 imply an increase in annual adult earnings of $\beta_{E0} = \$4,823$ for those offered an experimental voucher at birth. The estimated effect on earnings falls by $\beta_{EA} = -\$364$ per year, so the predicted effect reaches zero for children who are 13.25 years at RA and becomes negative for children who move as teenagers. This pattern is consistent with positive childhood exposure effects on earnings coupled with a disruption cost of moving to a very different social environment (e.g., moving from a high-poverty to a low-poverty neighborhood or moving a substantial geographic distance) that outweighs the exposure benefits if children move after age 13.

The Section 8 voucher has a similar pattern of effects with attenuated magnitudes. The TOT estimates of both the treatment effects at birth and the interactions with age at RA are about half as large for the Section 8 group as for the experimental group for most outcomes (online Appendix Table 8B). This mirrors the fact that the Section 8 treatment reduced neighborhood poverty rates in childhood half as much as the experimental treatment (Table 2).

Note that one cannot necessarily interpret the interaction effects (β_{EA} and β_{SA}) as the causal effects of an additional year of childhood exposure to lower-poverty areas. The differences in estimated effects by age at RA could be driven by heterogeneity in the types of families who sign up for MTO or comply with MTO treatments by age of children. Conceptually, our ability to identify causal exposure effects is limited by the fact that the MTO experiment only randomized voucher offers; it did not randomize the age at which children moved, which could be correlated with other unobservable factors. Nevertheless, the linear interaction models in Table 8 do provide further evidence supporting our main result that the MTO treatments had significant positive effects on children who were young at the point of random assignment.

Table 8—Linear Exposure Effect Estimates

	Indiv. earn. ($) 2008–2012 ITT (1)	Household income ($) 2008–2012 ITT (2)	Age 26 ITT (3)	Coll. qual. 18–20 ITT ($) (4)	Married ITT (%) (5)	Zip poverty share ITT (%) (6)	Taxes paid ITT ($) (7)
Experimental × age at RA	−364.1*	−723.7***	−564.9**	−171.0***	−0.582**	0.261*	−65.81***
	(199.5)	(255.5)	(282.8)	(55.16)	(0.290)	(0.139)	(23.88)
Section 8 × age at RA	−229.5	−338.0	157.2	−117.1*	−0.433	0.0109	−42.48*
	(208.9)	(266.4)	(302.0)	(63.95)	(0.316)	(0.156)	(24.85)
Experimental	4,823.3**	9,441.1***	8,057.1**	1,951.3***	8.309**	−4.371**	831.2***
	(2,404.3)	(3,035.8)	(3,760.9)	(575.1)	(3.445)	(1.770)	(279.4)
Section 8	2,759.9	4,447.7	−1,194.0	1,461.1**	7.193*	−1.237	521.7*
	(2,506.1)	(3,111.3)	(3,868.2)	(673.6)	(3.779)	(2.021)	(287.5)
Observations	20,043	20,043	3,956	20,127	20,043	15,798	20,043
Control group mean	13,807.1	16,259.9	14,692.6	21,085.1	6.6	23.7	627.8

Notes: This table reports ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group, interactions between the experimental/Section 8 indicators and age at random assignment (RA), and interactions between randomization site indicators and age at RA. All regressions are estimated using all children in the sample with available outcome data. Standard errors, reported in parentheses, are clustered by family. The dependent variable is individual earnings in column 1 and household income in columns 2 and 3. See notes to Table 3 for definitions of these variables. In columns 4–7, we replicate column 1 with the dependent variables used in column 6 of Table 4, column 1 of Table 5, column 1 of Table 6, and column 2 of Table 12. See notes to those tables for definitions of these variables. Columns 1, 2, and 5–7 include one observation per individual per year from 2008–2012 in which the individual is 24 or older. Column 3 includes one observation per individual at age 26. Column 4 includes one observation per individual per year from ages 18–20, as in Table 4. The Experimental × age at RA coefficient can be interpreted as the change in the impact of being assigned to the experimental group for a child who is one year older at random assignment, and the Section 8 × Age at RA coefficient can be interpreted analogously.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Nonparametric Estimates by Age at Move.—In Figure 2, we evaluate how the effects of the MTO treatments vary with children's ages at move using a nonparametric approach. These figures plot ITT estimates of being assigned to the experimental voucher group by a child's age at RA, grouping children into two-year age bins. Given the small sample sizes in each age group, we focus on the two outcomes for which we have the greatest precision: household income and college quality. In panel A, we regress household income on the treatment group indicators using a specification analogous to column 8 of Table 3. In panel B, we regress college quality on these indicators using the specification in column 6 of Table 4. In each panel, we estimate separate regressions using the data within each age bin and plot the experimental ITT estimates along with a 95 percent confidence interval (shown by the dashed lines).

Panel A shows that the experimental voucher increased household income in adulthood by approximately $2,000 for children who were offered the experimental voucher at or before age ten. This effect declines steadily with age at RA and becomes negative around age 13. Similarly, panel B shows significant positive effects on college quality for children who move at young ages, which then become

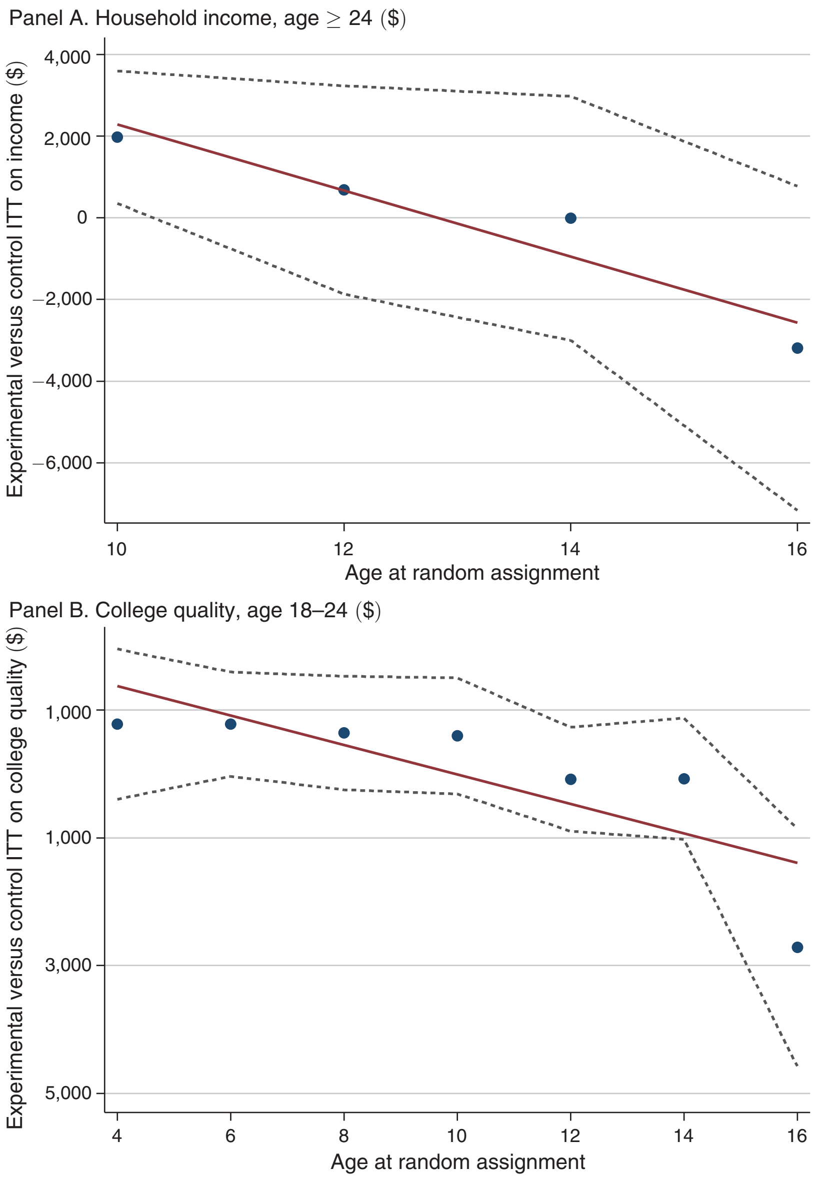

Figure 2. Impacts of Experimental Voucher by Children's Age at Random Assignment

Notes: These figures plot ITT estimates of the impact of being assigned to the experimental voucher group by a child's age at RA. Panel A plots impacts on household income for those above age 24, while panel B plots impacts on the earnings-based index of college quality between ages 18–20. To construct panel A, we first divide children into two-year age groups based on their age at random assignment; for instance, children who were ages 12 or 13 at RA are placed in the "age 12" group in the figure. Since there are few children who are below age ten at RA and whose income is observed at age 24, we include those below age 10 at RA in the age 10 bin; likewise, we include children who are 18 at RA in the age 16 bin. Using data within each age bin, we regress household income on indicators for being assigned to the experimental and Section 8 voucher groups using the same specification as in column 8 of Table 3, with one observation per individual per year from 2008–2012 in which the individual is 24 or older. The solid line is a best fit line for the plotted estimates. The dashed lines show the 95 percent confidence interval for each of the estimates. Panel B replicates panel A using college quality as the dependent variable. The regression specification used to estimate the coefficients plotted in panel B is the same as that in column 6 of Table 4, with one observation per year when the child is between the ages of 18–20. We plot the coefficients on the experimental voucher group indicator in this figure; the corresponding estimates for the Section 8 voucher group are shown in online Appendix Figure 2. See notes to Tables 3 and 4 for definitions of household income and college quality.

negative for children moving in adolescence.[36] In both cases, we cannot reject the hypothesis that the relationship between age at move and the treatment effects is linear, although the age-specific estimates are not very precise because of the small sample sizes. There is little evidence of a "critical age" below which children must move to benefit from a better neighborhood. The roughly linear pattern of exposure effects in the MTO data matches the quasi-experimental findings of Chetty and Hendren (2015), who document a much more precisely estimated pattern of linear childhood exposure effects using a sample of five million families that moved across counties.[37]

IV. Reconciling the Findings with Previous MTO Research

In this section, we reconcile our new findings with prior research on MTO's impacts on the economic outcomes of adults and children. We first show that, consistent with prior work, exposure to better neighborhoods does not appear to improve adults' outcomes. We then explain why our findings of exposure effects for children were not detected in prior research. Finally, we evaluate whether our findings on the heterogeneous effects of the MTO treatments by age at move may be an artifact of multiple hypothesis testing given that prior research on MTO has tested for heterogeneity in several other dimensions as well.

A. *MTO Impacts on Adults' Economic Outcomes*

Previous research has found that the MTO treatments had little impact on adults' income and employment rates (Kling, Liebman, and Katz 2007; Sanbonmatsu et al. 2011). These prior studies used data from state unemployment insurance (UI) records through 2008 and survey data collected in 2008–2009. In Table 9, we reexamine the effects of MTO on adults' economic outcomes using the tax data. The tax data allow us to follow the MTO adults through 2012 and track individuals who move across state lines, who are missing from the state UI data of the original randomization sites.

Table 9 presents ITT estimates of MTO treatment impacts on the individual earnings, household income, and employment rates of MTO adults.[38] The specifications in columns 1–2 and 4–5 use one observation per year from 2008–2012 for each of the 4,215 adults in the linked MTO-tax data, while column 3 uses data only from 2012.

[36] Online Appendix Figure 2 plots the corresponding ITT estimates for the Section 8 voucher by age bin. We find qualitatively similar declining patterns for the impacts of the Section 8 voucher, although the estimates are attenuated in magnitude, consistent with our earlier findings.

[37] Chetty and Hendren's quasi-experimental estimates of exposure effects are identified on a sample consisting entirely of families who moved across counties, comparing the outcomes of children who move to different areas at different ages. Since everyone in Chetty and Hendren's sample moves a significant distance, their estimates net out any fixed disruption costs of moving across social environments. In contrast, here we compare families who move to a low-poverty area (who face disruption costs of relocating to a very different area) to families who largely remain in higher-poverty areas (who do not pay such disruption costs). Our estimates therefore include the disruption cost of moving to a different environment. This difference may explain why we find negative effects for children who move at older ages in the MTO data, whereas Chetty and Hendren estimate positive exposure effects at all ages.

[38] As in prior work, the "adult" whom we follow in the MTO data and link to the tax data is the household head at the point of RA, with preference given to the mother or other adult female guardian if present.

Table 9—Impacts of MTO on Adults' Income

	Individual earnings ($)				
	2008–2012 ITT (1)	ITT w/controls (2)	2012 ITT (3)	Employed (%) ITT (4)	Hhold. inc. ($) ITT (5)
Exp. versus control	−354.1	−168.7	−681.9	−0.946	−338.5
	(621.9)	(558.2)	(675.1)	(1.704)	(740.4)
Section 8 versus control	249.5	468.1	−99.67	−0.702	516.7
	(675.2)	(609.0)	(758.4)	(1.833)	(829.4)
Observations	21,075	21,075	4,215	21,075	21,075
Control group mean	14,381.0	14,381.0	13,700.6	54.9	17,951.5

Notes: All columns report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Standard errors, reported in parentheses, are clustered by family. The sample consists of all individuals in the linked MTO-tax dataset who were not classified as children at the point of random assignment. The number of individuals is 4,215 in all columns. The dependent variable in columns 1–3 is individual earnings. Column 1 includes one observation per individual per year from 2008–2012 in which the individual is 24 or older. Column 2 replicates column 1, controlling for the predetermined characteristics listed in online Appendix Table 1A. In column 3 we replicate column 1 using data from 2012 only. Columns 4 and 5 replicate column 1 using employment and household income as the dependent variables. See notes to Table 3 for definitions of all dependent variables.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Consistent with prior work, we find no effects of MTO treatments on any of the adults' economic outcomes. The point estimates tend to be slightly negative for the experimental group and slightly positive for the Section 8 group, but all of the estimates are small and are not significantly different from zero. For example, the experimental voucher ITT on mean individual earnings from 2008–2012 is −$354 (2.4 percent of the control group mean), with a standard error of $622. The corresponding TOT estimate on individual earnings is −$734, 5.1 percent of the control group mean and 4.7 percent of the control complier mean (online Appendix Table 9B, column 1). The upper bound of the 95 percent confidence interval for the TOT estimate is $1,795, 12 percent of the control group mean. This is far below the 31 percent increase in individual earnings for young children.

Exposure Effect Estimates.—Clampet-Lundquist and Massey (2008) show that the number of years an adult spends in a low-poverty area is correlated with their earnings and other economic outcomes. Their findings raise the possibility of time of exposure impacts for adults similar to what we documented above for children.[39] We test for such exposure effects in Figure 3 by estimating the effects of the MTO treatments on individual earnings by the number of years since random assignment. We group the data into two-year bins based on the number of years elapsed since RA and estimate ITT regression specifications using the data within each bin.

[39] Clampet-Lundquist and Massey's analysis does not directly identify causal exposure effects because it exploits cross-sectional variation across individuals (which may be confounded by omitted variables) rather than the experimental variation generated by the randomly assigned treatments.

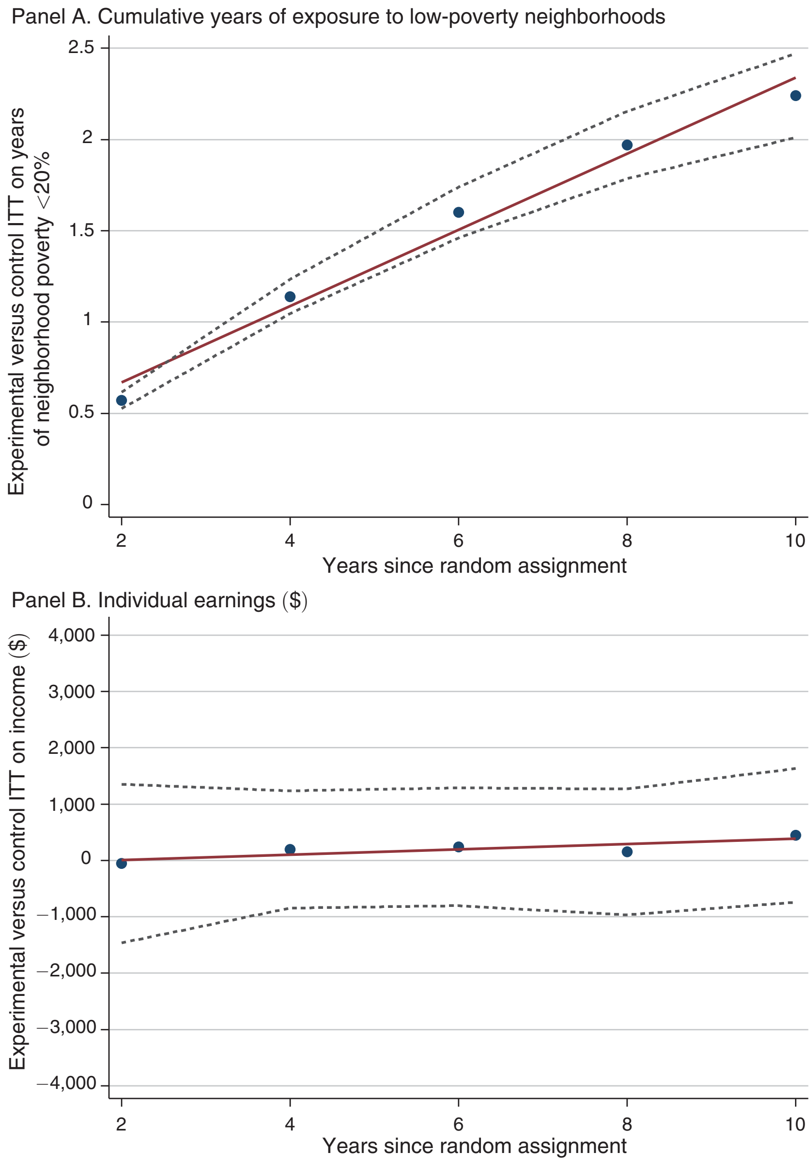

Figure 3. Impacts of Experimental Voucher on Adults by Years since Random Assignment

Notes: These figures plot ITT estimates of the impact of being assigned to the experimental voucher group by the number of years since random assignment (RA) for adults. Panel A plots impacts on the total number of years the individual lived in a census tract with a poverty rate of less than 20 percent since RA. To construct panel A, we first divide the data into two-year groups based on the number of years since RA (e.g., data in the first and second year after the calendar year of RA are assigned a value of 2). Using the data within each bin (with two observations per adult), we regress the total number of years in which the individual lived in a census tract with a poverty rate below 20 percent since RA on indicators for being assigned to the experimental and Section 8 voucher groups as well as randomization site indicators, following the standard ITT specification used for other outcomes. The solid line is a best fit line for the plotted estimates. Tract poverty rates were linearly interpolated using data from the 1990 and 2000 decennial censuses as well as the 2005–2009 American Community Survey. Panel B plots impacts on individual earnings, and is constructed using the same approach as in panel A. The regression specification used to estimate the coefficients plotted in panel B is analogous to that in column 1 of Table 9, with one observation per adult at age 24 or above for the relevant years in each bin. We plot the coefficients on the experimental voucher group indicator in this figure; the corresponding estimates for the Section 8 voucher group are shown in online Appendix Figure 3. See notes to Table 9 for the definition of individual earnings.

We first verify that the total time of exposure to low-poverty environments increases with time since RA for adults who were assigned to the experimental voucher group relative to the control group. Prior studies have observed that some MTO participants in the experimental group moved back to higher-poverty areas over time, while some families in the control group moved to lower-poverty areas over time (e.g., Clampet-Lundquist and Massey 2008). We assess the impacts of such subsequent moves in panel A of Figure 3. We regress the cumulative number of years that the adult lived in a census tract with a poverty rate below 20 percent since RA on the MTO treatment indicators. The figure plots the ITT effects of the experimental voucher on cumulative exposure to low-poverty areas versus the number of years since RA. It is clear that the total amount of exposure to low-poverty areas rises substantially over time in the experimental group relative to the control group despite the fact that some families moved again in subsequent years.

Panel B of Figure 3 shows ITT effects of the experimental voucher on adults' individual earnings by years since RA, estimated using regressions analogous to that in column 1 of Table 9. The estimated impact on adult earnings is consistently close to zero when measuring earnings in the one to ten years after RA, with no evidence of the increasing pattern that one would expect if time of exposure in adulthood has a causal effect. The results are very similar for the Section 8 treatment (online Appendix Figure 3). We conclude that exposure to improved neighborhood environments—at least for the range of moves generated by the MTO experiment—has little impact on adults' economic outcomes.[40]

Together with our findings in Section III, the results in Figure 3 show that it is the amount of exposure to better neighborhoods *during childhood* (rather than total lifetime exposure) that matters for long-term economic success. Moreover, these findings imply that the MTO treatment effects on children's outcomes do not arise from improvements in family income. Instead, they are likely to be driven by direct effects of neighborhood environments on the children or to be mediated by parental health and stress, which were improved by the MTO treatments (Ludwig et al. 2011, Ludwig et al. 2012).

B. *MTO Impacts on Children's Economic Outcomes*

The MTO final impacts evaluation (Sanbonmatsu et al. 2011) found no treatment effects on children's economic outcomes using data from state UI records in 2008 and survey data from 2008–2009. In Table 10, we reconcile our findings with these earlier results. As a reference, we begin in column 1 of Table 10 by replicating the specification in column 2 of Table 3, which shows that the MTO treatments had substantial positive effects on the individual earnings of younger children (those

[40] Quigley and Raphael (2008) argue that the moves induced by MTO did not change neighborhood environments by enough to offset the spatial disadvantages faced by low-skilled minority female household heads. Although larger neighborhood changes could have different effects, we note that MTO moves did change neighborhood environments quite substantially. TOT estimates show that adults who moved using an experimental voucher lived in lower-poverty areas for approximately five more years on average (as implied by Figure 3) and experienced an 18 percentage point reduction in neighborhood poverty (1.5 standard deviations in the US census tract poverty distribution) up to the point of the MTO final impacts evaluation, 10–15 years after RA (Ludwig et al. 2013).

Table 10—MTO Impacts on Children's Earnings: Comparison to MTO Final Impacts Evaluation

Dependent variable:	Individual earnings				Individual earnings measured age 16–21 ($)	
	Measured age $\geq$ 24 ($)					
Sample:	< Age 13 2008–2012 (1)	All children 2008–2012 (2)	< Age 13 2008 (3)	All children 2008 (4)	< Age 13 Up to 2012 (5)	All children 2008 (6)
Exp. versus control	1,624.0**	302.5	1,840.9	−236.6	−30.97	−286.3
	(662.4)	(578.2)	(1339.7)	(757.0)	(229.7)	(410.8)
Section 8 versus control	1,109.3	−44.06	2,860.1*	−213.7	197.4	190.0
	(676.1)	(621.5)	(1,486.1)	(799.9)	(176.6)	(351.8)
Observations	8,420	20,043	552	2,851	30,011	3,384
Control group mean	11,270.3	13,807.1	11,615.0	14,531.5	4,033.3	4,923.0

Notes: All columns report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Standard errors, reported in parentheses, are clustered by family. The dependent variable is individual earnings in all columns, defined in the notes to Table 3. Column 1 replicates the specification in column 2 of Table 3, panel A, and includes children below age 13 at age of random assignment. This specification includes one observation per individual per year from 2008–2012 in which the individual is 24 or older. Column 2 replicates column 1, pooling all children (irrespective of age at random assignment) in the sample. Columns 3 and 4 replicate columns 1 and 2, limiting the sample to data from the 2008 tax year, which was the last year of data available for the MTO final impacts evaluation (Sanbonmatsu et al. 2011). Columns 3 and 4 therefore only include children who were 24 or older in 2008. Column 5 replicates column 1, with one observation per year in which the child is between the ages of 16–21, using all years in which we observe individual earnings (1999–2012). Column 6 replicates column 4, restricting the sample to children between the ages of 16 and 21 in 2008, as in the MTO final impacts evaluation.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

under 13 at RA). The remaining columns present variants of this specification that highlight three reasons why our findings differ from prior results.

First, if we had followed earlier MTO work in pooling younger and older MTO children, we also would have found no mean effects on earnings in adulthood, as shown in column 2 of Table 10. Such pooled estimates hide the positive MTO effects on younger children and negative effects on older children.

Second, we measure the earnings of children who were 24 years or older in 2012 in our data. If instead we had conducted our analysis in 2008—the time of the MTO final impacts evaluation—we would have found positive but very imprecisely estimated effects on earnings for children who were below age 13 at RA, as shown in column 3. This is because one would have had only 552 observations on earnings for children who were less than 13 at RA in 2008. If one had attempted to expand the sample by including all children in the analysis, as in column 4, one would have again obtained a point estimate close to zero.

Finally, partly because of these data limitations, prior analyses measured earnings at very early ages, between the ages of 16–21. Columns 5 and 6 show that we find no effects on earnings at these early ages in our data even when we focus on children who were less than 13 at RA. The earnings impacts of MTO emerge only after children complete education and begin to enter the labor market, as shown in Figure 1.

In sum, there is no inconsistency between our empirical findings and prior MTO evaluations. The childhood exposure effects we document here were not apparent in prior studies because they did not have adequate long-term data to observe the emergence of MTO's impacts on earnings and other outcomes in adulthood for children who moved at young ages.[41]

C. *Multiple Comparisons*

Previous research has searched for impacts of MTO in a wide range of subgroups: across the five treatment sites, for different races, and for each gender. Given the extensive subgroup analysis that has been conducted in the MTO data, one may be concerned that our findings of significant effects in certain age subgroups are an artifact of multiple hypothesis testing. Of course, examining many subgroups can generate p-values that appear to be individually statistically significant purely by chance.

To address this concern, we implement a set of parametric F-tests for the null hypothesis that there are no subgroup-specific treatment effects in the pooled data. These F-tests adjust for the over-rejection rate when analyzing any one subgroup separately by using a single joint test across all subgroups in the pooled sample. In panel A of Table 11, we test the hypothesis that there is no treatment effect for either young children (under 13) or older children (13–18). We regress a subset of the outcomes analyzed in Tables 3–6 above on the MTO treatment indicators (*Exp* and *S*8) interacted with an indicator for being below age 13 at RA (Below13), including site dummies as controls. We then test the hypothesis that the *Exp* and *Exp-Below*13 interaction effect are both 0 (row 1), the *S*8 and *S*8-*Below*13 interaction effect are both zero (row 2), and both sets of treatment effect estimates are 0 (row 3).

We reject the null of zero treatment effects in both age subgroups with $p < 0.05$ in most cases, especially for the experimental voucher group. For example, we reject the null hypothesis that the experimental voucher has no effect on individual earnings in either age subgroup with $p = 0.020$. For college quality, we reject the hypothesis that the experimental voucher has no effect in either subgroup with $p = 0.0006$ and reject the hypothesis that the experimental and Section 8 treatments have zero effects in all subgroups with $p = 0.0020$.

The tests in panel A consider the age-specific subgroups we focus on in this study, but not the other subgroups that have been analyzed in the broader literature. In panel B, we test the hypothesis that there is no treatment effect in any of the primary subgroups that have been studied to date in the MTO data: randomization sites, racial groups, gender, and age at RA. As in panel A, we regress outcomes on the MTO treatment indicators interacted with all of these subgroup indicators. We then test the hypothesis that the experimental indicator and all of its subgroup interactions are 0 (row 1), the Section 8 indicator and all of its interactions are 0 (row 2), and both sets of treatment effects are zero (row 3).

The tests in panel B have less power than those in panel A because they consider many more subgroups. Nevertheless, when we focus on the outcomes for which we

[41] The MTO final impacts evaluation (Sanbonmatsu et al. 2011) found no significant effects of the MTO treatments on educational outcomes or risky behaviors for children who moved at young ages when they were observed as adolescents. The positive treatment effects for younger children show up only when we look at their outcomes in adulthood. Hence, our findings differ from the conclusions of prior research on MTO both because we focus children who moved at young ages and because we analyze long-term impacts rather than intermediate outcomes. We discuss our findings in the context of prior research on MTO in greater detail in the conclusion.

Table 11—Multiple Comparisons: *F*-Tests for Subgroup Heterogeneity

	Individual earnings 2008–2012 ($) (1)	Household income 2008–2012 ($) (2)	College attendance 18–20 (%) (3)	College quality 18–20 ($) (4)	Married (%) (5)	Poverty share in zip 2008–2012 (%) (6)
Panel A. p-values for comparisons by age group						
Exp. versus control	0.0203	0.0034	0.0035	0.0006	0.0814	0.0265
Section 8 versus control	0.0864	0.0700	0.1517	0.0115	0.0197	0.0742
Exp. and Section 8 versus control	0.0646	0.0161	0.0218	0.0020	0.0434	0.0627
Panel B. p-values for comparisons by age, site, gender, and race groups						
Exp. versus control	0.1121	0.0086	0.0167	0.0210	0.2788	0.0170
Section 8 versus control	0.0718	0.1891	0.1995	0.0223	0.1329	0.0136
Exp. and Section 8 versus control	0.1802	0.0446	0.0328	0.0202	0.1987	0.0016

Notes: This table presents *p*-values for nonzero MTO treatment effects in subgroups for selected outcomes analyzed in Tables 3 to 6. In panel A, we regress the outcome on the MTO treatment indicators (*Exp* and *S*8) interacted with an indicator for being below age 13 at RA (Below13), including site dummies as controls, and clustering standard errors by family. We then run *F*-tests for the null hypothesis that the Exp and Exp-Below13 interaction effect are both 0 (row 1), the *S*8 and *S*8-Below13 interaction effect are both 0 (row 2), and both sets of treatment effect estimates are 0 (row 3). In panel B, we regress the outcomes on the MTO treatment indicators interacted with the following subgroup indicators: the five randomization sites, racial groups (black, Hispanic, and other), gender, and age at RA below 13. As in panel A, we then test the hypothesis that the experimental indicator and all of its subgroup interactions are 0 (row 1), the Section 8 indicator and all of its interactions are 0 (row 2), and both sets of treatment effects are 0 (row 3). See notes to Tables 3–6 for definitions of the outcome variables.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

have the most precise estimates in our baseline analysis—e.g., the college outcomes and household income—we reject the null of zero treatment effects in all subgroups with $p < 0.05$ both for the experimental versus control comparison in row 1 and the pooled comparison in row 3.

As an alternative to the parametric *F*-test, we implement a nonparametric permutation test for subgroup heterogeneity using the *p*-values from our OLS regressions as critical values, as in Ding, Feller, and Miratrix (2015). We generate 5,000 "placebo" samples in which we randomly reassign treatment status to families within randomization sites. In each placebo sample, we estimate the experimental and Section 8 treatment effects for our 5 core outcomes (individual earnings, college attendance, college quality, marriage, and poverty share in zip) for the 12 primary subgroups analyzed to date (age above/below 13, male/female, 5 sites, and 3 racial groups). Finally, we calculate the fraction of placebo simulations in which there is a subgroup where the *p*-values for *all five* outcomes (for either the experimental or Section 8 group) fall below the corresponding true *p*-values for the experimental treatment estimates in the below-age-13 subgroup. Intuitively, this approach asks, "if one were to loop over the 12 subgroups and estimate treatment effects on the 5 outcomes, what is the chance that one would obtain a set of *p*-values below the actual estimates purely by chance in one of the subgroups?"

We find that fewer than 1 percent of the placebo replications produce a subgroup where the *p*-values for the five outcomes lie below the values we estimate. Hence, the permutation test yields an adjusted p-value for the null hypothesis that there is no treatment effect on any of the five outcomes of $p < 0.01$. The permutation test and parametric F-tests thus both indicate that the significant treatment effects we detect are unlikely to be an artifact of making multiple comparisons.

Finally, it is important to note that we did not reexplore the MTO data arbitrarily searching for subgroups that exhibit significant effects. Rather, motivated by the quasi-experimental evidence in Chetty and Hendren (2015), we returned to the MTO data with a specific hypothesis that we would find larger effects for younger children. The fact that the results align closely with this hypothesis further reduces the likelihood that they reflect statistical noise driven by multiple hypothesis testing.

V. Cost-Benefit Analysis and Policy Implications

In this section, we compare the costs and benefits of the MTO interventions and discuss the implications of our results for the design of affordable housing policies, on which the US federal government currently spends $46 billion per year (Collinson, Ellen, and Ludwig 2015). We focus on two policy questions. First, what are the costs and benefits of an MTO-type experimental voucher program that moves families with young children out of traditional project-based public housing into lower-poverty neighborhoods? Second, what are the benefits of expanding the existing Section 8 housing voucher program? We begin by calculating the benefits of the MTO experimental vouchers, focusing on the increased earnings for children who move when young. We then quantify the fiscal costs of the program and discuss the policy implications of these calculations. We caution that all of the calculations reported in this section should be treated as rough estimates because they rely on several strong assumptions, starting with the basic premise that the treatment effects estimated from the MTO experiment can be extrapolated to evaluate current policy interventions.

Earnings Benefits.—The MTO experimental treatment increased individual earnings in early adulthood for children whose families moved before they were age 13 by $3,477; 30.8 percent of the control group mean (Table 3, column 4). We translate this estimate into a predicted lifetime earnings impact by assuming that (i) this 30.8 percent increase in individual earnings remains constant over the life cycle; (ii) the life cycle profile of earnings for MTO participants follows the US population average; and (iii) the real wage growth rate is 0.5 percent, approximately the rate of wage growth in the United States over the past decade, and the discount rate is 3 percent, approximately the current 30-year Treasury bond rate.[42]

Under these assumptions, moving to a lower-poverty area when young (at age eight on average) using the experimental voucher increases total pretax lifetime earnings by $302,000.[43] The present value of this increase in lifetime earnings is

[42] As shown in Figure 1, the assumption of a constant 30.8 percent effect is conservative, as the estimated treatment effects increase steadily over the ages where we measure earnings.

[43] We estimate the average life cycle earnings profile by tabulating mean earnings by age for a random sample of the US population in 2012 from ages 26–65. We then apply a 0.5 percent wage growth rate and a 3 percent annual discount rate to this profile to obtain an undiscounted sum of lifetime earnings for the average American of $1.74 million and a PDV at age eight of $570,000. The younger children in the MTO control group earn 56 percent of the

\$99,000 at age eight. For a family with two young children at the point of the move, the MTO experimental treatment therefore has an estimated present value of approximately \$198,000 in terms of increased children's earnings.

Fiscal Costs.—Next, we turn to the fiscal cost of the MTO experimental intervention. In calculating this cost, it is important to recognize that the higher earnings of children who moved to low-poverty areas at young ages increases tax revenue, reducing the cost of the program to the government. We therefore begin by estimating the effects of the MTO treatments on income tax revenue, a fiscal externality that is also a key input for normative analysis (Hendren 2013).

We examine MTO treatment impacts on tax filing rates and federal tax payments in adulthood in Table 12. Column 1 shows ITT effects on tax filing rates when children are 24 or older. Among younger children (panel A), the experimental voucher treatment increases the fraction who file tax returns in their mid to late twenties by 5.7 pp, while the Section 8 treatment increases the filing rate by 4.8 pp. Column 2 reports ITT estimates on income taxes paid. The experimental ITT is \$184, while the Section 8 ITT is \$109. The corresponding TOT estimates, reported in column 3, show that children whose families moved using the experimental voucher when they were young pay an additional \$394 in income taxes per year in their mid-twenties.[44] Conversely, the MTO treatments reduce tax filing rates and tax payments by the older children, as expected given the negative effects of the treatments on older children's earnings.

We use the estimates in Table 12 to predict the total tax revenue impacts of the MTO experimental intervention on families with young children. The experimental ITT on tax payments of \$184 equals 1.63 percent of mean control group individual earnings. Under the same assumptions used to calculate the lifetime earnings gains above, this translates to an increase in lifetime tax revenue of \$5,200 in PDV at age eight. The TOT estimate of \$394 implies a PDV increase in tax payments of \$11,200 per child who moves to a lower-poverty area at a young age. If there are two young (below age 13) children per family on average, the increased federal tax payments would be worth \$22,400 in PDV per family moved.

Olsen (2009) estimates that the direct fiscal costs of housing voucher programs are similar to or slightly lower than the costs of project-based public housing.[45] Olsen's estimates imply that the main incremental cost of moving families out of public housing using an MTO-type voucher program would be the funding of counselors to help low-income families relocate. The mean MTO counseling costs were \$1,789 per family counseled (in 2012 dollars) or \$3,783 per family who took up a voucher (Goering et al. 1999, Table 4). This counseling cost of \$3,783 is far smaller than the tax revenue gain of \$22,400 for each family with two young children that is moved. Thus, an MTO-type experimental voucher policy that moves low-income

mean individual earnings in the US population at age 26. Therefore, the estimated impact on undiscounted lifetime earnings is $0.308 \times 0.56 \times \$1.74m = \$302{,}000$.

[44] Our measure of taxes paid does not include tax credits received. We find no significant treatment effects on Earned Income or Child Tax Credit amounts. This is consistent with the fact that most of the earnings increases induced by the treatments are on the intensive rather than extensive margin (Table 3), and many people move into the phase-out region for these credits as they earn more.

[45] The costs of public housing are debated because of disagreements about how one should account for the depreciation of housing projects.

Table 12—Impacts of MTO on Federal Income Tax Payments

	Filing a tax return (%) 2008–2012 ITT (1)	Total income taxes paid ($) 2008–2012 ITT (2)	Total income taxes paid ($) 2008–2012 TOT (3)
Panel A. Children < age 13 at random assignment			
Exp. versus control	5.748***	183.9***	393.6***
	(2.055)	(62.80)	(134.1)
Section 8 versus control	4.789**	109.0**	169.1**
	(2.237)	(54.76)	(85.48)
Observations	8,420	8,420	8,420
Control group mean	59.3	447.5	447.5
Panel B. Children age 13–18 at random assignment			
Exp. versus control	−2.079	−175.9*	−441.6*
	(2.055)	(91.15)	(230.8)
Section 8 versus control	−2.248	−127.1	−230.1
	(2.249)	(95.52)	(173.2)
Observations	11,623	11,623	11,623
Control group mean	65.6	775.2	775.2

Notes: Columns 1 and 2 report ITT estimates from OLS regressions (weighted to adjust for differences in sampling probabilities across sites and over time) of an outcome on indicators for being assigned to the experimental voucher group and the Section 8 voucher group as well as randomization site indicators. Column 3 reports TOT estimates using a 2SLS specification, instrumenting for voucher take-up with the experimental and Section 8 assignment indicators. Standard errors, reported in parentheses, are clustered by family. Panel A restricts the sample to children below age 13 at random assignment; panel B includes children between age 13 and 18 at random assignment. The estimates in panels A and B are obtained from separate regressions. The dependent variable in column 1 is an indicator for filing a 1040 tax return. In columns 2 and 3, the dependent variable is total taxes paid, defined as the total tax field from form 1040 for filers and total taxes withheld on W-2 forms for non-filers. Columns 1 and 2 include one observation per individual per year from 2008–2012 in which the individual is 24 or older. Column 3 reports TOT estimates corresponding to the ITT estimates in column 3.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

families with young children out of high-poverty housing projects will most likely save the government money.

Policy Implications.—We now return to the policy questions posed at the beginning of this section. On the narrower question of comparing MTO-type experimental vouchers to project-based public housing, the data strongly suggest that vouchers targeted at families with young children are likely to yield net gains. Indeed, such a policy is likely to *reduce* government expenditure while increasing children's future earnings substantially. However, it is critical to target such vouchers effectively to obtain these benefits. First, targeting the vouchers so that families are required to move to low-poverty areas is important. The MTO experimental vouchers—which restricted families to move to low-poverty census tracts—improve children's outcomes much more than existing Section 8 vouchers that give families more flexibility in choosing where to live.[46] Second, it is critical to target the vouchers to

[46] An interesting question is why giving families greater choice in where to live appears to *reduce* long-term benefits for children. One possibility is that the neighborhoods chosen by families with unrestricted Section 8 vouchers have other amenities that families value more than their children's long-term outcomes. However, the

families with young children. As shown above, moving families with older children out of existing public housing projects not only has smaller benefits, but actually appears to be detrimental. The common practice of putting families on wait lists to receive a housing voucher may be particularly inefficient, as this effectively allows many families to move to better neighborhoods only when their children grow older.

We next consider the broader issue of offering Section 8 housing vouchers to more low-income families. The MTO experiment shows that moving families who started out in high-poverty public housing projects to lower-poverty areas has substantial long-term benefits for children. However, the marginal Section 8 voucher may not induce such a move; instead, recent evidence suggests that Section 8 housing vouchers are frequently used to rent better housing within the same neighborhood rather than move to better neighborhoods (Jacob, Kapustin, and Ludwig 2015). Consistent with the lack of impact of neighborhood environments, Jacob, Kapustin, and Ludwig (2015) find that obtaining a Section 8 voucher through a lottery in Chicago has little impact on children's long-term outcomes for families living in unsubsidized private housing. These results again suggest that one may need to carefully target housing voucher subsidies to have an impact on children's outcomes. Providing more Section 8 vouchers (or equivalent cash benefits) may have little effect on children's outcomes, but providing MTO-type restricted vouchers that require families to move to better (e.g., low-poverty) neighborhoods may be quite valuable.

Our simple calculations neglect many important factors that should be considered in a more comprehensive cost-benefit evaluation. First, our calculations do not account for reductions in transfer payments or gains from better outcomes in future generations. As discussed above, the MTO treatments reduce dependence on long-term transfer programs such as disability insurance (online Appendix Table 3c, column 4) and are likely to have persistent effects on subsequent generations (Section IIIE). Second, our calculations focus exclusively on the benefits in terms of children's earnings and thereby neglect other benefits, such as improved subjective well-being and health of adults (Ludwig et al. 2012) and reduced rates of crime (Kling, Ludwig, and Katz 2005).

Finally, our calculations ignore any spillover effects on prior residents of the neighborhoods where the MTO families moved. Although the MTO experiment itself yields no evidence on the magnitude of these spillovers, Chetty and Hendren's (2015) quasi-experimental estimates show that mixed-income areas produce better outcomes for children in low-income families while generating, if anything, slightly better outcomes for children in higher-income families as well. This finding suggests that policies which reduce concentrated poverty may not have detrimental spillover effects on higher-income households, but further work that directly estimates these spillover effects is required to measure the social benefits of MTO-type policies.

Section 8 voucher did not yield significantly greater benefits than the experimental voucher in terms of adults' earnings or their subjective well-being (Ludwig et al. 2012). Another possibility is that families make suboptimal neighborhood choices because of behavioral biases, so that restrictions in the choice set and nudges to encourage families to move to lower-poverty areas improve their own private welfare. See Chetty (2015) for further discussion of optimal policy and welfare analysis of neighborhood choice in behavioral models.

VI. Discussion and Conclusion

This paper has presented a new analysis of the impacts of the Moving to Opportunity experiment on children's long-term outcomes. We find robust evidence that children who moved to lower-poverty areas when they were young (below age 13) are more likely to attend college and have substantially higher incomes as adults. These children also live in better neighborhoods themselves as adults and are less likely to become single parents themselves, suggesting that some of the benefits of the initial MTO voucher treatment will persist into the following generation (the grandchildren of the parents who received the MTO vouchers). In contrast with the large gains for young children, moving to lower-poverty areas had negative effects on older youth. Finally, we replicate earlier findings that the moves induced by MTO had little impact on adults' economic outcomes.

Our findings show that a simple model featuring linear childhood exposure effects coupled with a fixed disruption cost of moving to a distinctly different social environment can reconcile some of the key findings and debates in the literature on neighborhood effects. First, our results suggest that a substantial fraction of the systematic variation in economic outcomes across areas documented in observational studies that attempt to control for selection effects (e.g., Brooks-Gunn et al. 1993; Cutler and Glaeser 1997; Ellen and Turner 1997; Sampson, Morenoff, and Gannon-Rowley 2002) can indeed be explained by causal effects of neighborhoods. Since many low-income individuals observed in a given area have grown up in that area since an early age, childhood exposure effects of the type documented here would generate significant differences in mean outcomes across areas in observational data. The fact that MTO had no impact on adults' outcomes (irrespective of exposure time to lower-poverty areas in adulthood) implies that neighborhood effects operate primarily through "developmental" effects during childhood (Sampson 2008) rather than contextual effects arising from spatial mismatch or other forces (Kain 1968, Wilson 1996).

Our results also are consistent with recent studies that document the importance of childhood exposure effects by studying immigrant assimilation (e.g., Bleakley and Chin 2004, Basu 2010, van den Berg et al. 2014) and families that move across counties within the United States (Chetty and Hendren 2015). In particular, the decline in MTO's treatment effects for children with age at RA coupled with the lack of an impact for adults matches Chetty and Hendren's (2015) finding that the gains from moving to better areas fall linearly with a child's age at move.

Our findings also complement studies in the child development literature that have documented robust correlations between years of exposure to high-poverty family environments and later outcomes (e.g., Duncan, Brooks-Gunn, and Klebanov 1994). Some studies in this literature argue that environmental conditions in the earliest years of childhood (e.g., before age five) have much larger long-term impacts than conditions in later years (e.g., Brooks-Gunn and Duncan 1997; Shonkoff and Phillips 2000; Heckman 2006). Because we only observe long-term outcomes for children who were four or older at random assignment, our results demonstrate that improvements in neighborhood environments continue to have large effects on children's long-term outcomes even after early childhood. Whether the impacts would be even larger at younger ages remains to be explored.

Although our findings help reconcile some key findings on neighborhood effects on outcomes in adulthood, other pieces of evidence remain to be explained. Most notably, MTO's treatment effects on children's short-term and medium-term outcomes are not fully aligned with the long-term impacts documented here (especially for boys) in three respects. First, the MTO treatments improved young children's short-term outcomes (e.g., reducing behavioral problems) in the years immediately following random assignment, but these gains largely faded away over the next decade (e.g., as measured by achievement on standardized tests).[47] Yet the positive effects of the MTO treatments re-emerge in adulthood, as measured by earnings and college attainment.[48] Second, MTO had more positive effects for girls than for boys for medium-term outcomes (Kling, Liebman, and Katz 2007; Ludwig et al. 2013), but we find no significant gender differences in MTO's effects on children's outcomes in adulthood. Third, we find somewhat negative long-run impacts on older youth (ages 13 to 18 at RA), but earlier work showed positive initial impacts in terms of lower crime and problem behaviors for these children in the first three years after RA.

Although further work remains in synthesizing the evidence that has been collected from the MTO experiment, the results of this study demonstrate that offering low-income families housing vouchers and assistance in moving to lower-poverty neighborhoods has substantial benefits for the families themselves and for taxpayers. It appears important to target such housing vouchers to families with young children—perhaps even at birth—to maximize the benefits. Our results provide less support for policies that seek to improve the economic outcomes of adults through residential relocation. More broadly, our findings suggest that efforts to integrate disadvantaged families into mixed-income communities are likely to reduce the persistence of poverty across generations.

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[47] See Katz, Kling, and Liebman (2001); Sanbonmatsu et al. (2006); Sanbonmatsu et al. (2011); and Sciandra et al. (2013). Jacob (2004) also finds a similar lack of impacts on school outcomes for moves out of public housing in Chicago triggered by the quasi-random timing of the demolition of housing projects.

[48] This pattern echoes the results of other studies of early childhood and school interventions such as Perry Preschool (Heckman et al. 2010), Head Start (Deming 2009), Project STAR (Chetty et al. 2011), and changes in teacher quality (Chetty, Friedman, and Rockoff 2014), all of which find a pattern of fade-out on intermediate outcomes and reemergence in adulthood. However, an important difference between the MTO intervention and the other interventions is that the improvement in neighborhoods induced by MTO was an ongoing treatment throughout childhood rather than a one-time treatment whose impacts might later fade away.

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Wilson, William Julius. 1996. *When Work Disappears: The World of the New Urban Poor*. New York: Random House.

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This article has been cited by:

1. Sharon Barnhardt, Erica Field, Rohini Pande. 2017. Moving to Opportunity or Isolation? Network Effects of a Randomized Housing Lottery in Urban India. *American Economic Journal: Applied Economics* **9**:1, 1-32. [Abstract] [View PDF article] [PDF with links]
2. Robert J. Sampson. 2016. The Characterological Imperative: On Heckman, Humphries, and Kautz's The Myth of Achievement Tests: The GED and the Role of Character in American Life. *Journal of Economic Literature* **54**:2, 493-513. [Abstract] [View PDF article] [PDF with links]

Appendix 2

University of Southern California
University Park Campus, MC 1059
Los Angeles, CA 90089-1059
(213) 821-5730
annowens@usc.edu
http://scholar.harvard.edu/aowens

ANN OWENS

EMPLOYMENT

2013- Assistant Professor of Sociology, University of Southern California
Courtesy Appointment, Spatial Sciences Institute
Faculty Affiliate: Sol Price Center for Social Innovation, Population Research Center, Children's Data Network

2012-2013 Postdoctoral Fellow, Center on Poverty and Inequality, Stanford University

EDUCATION

2012 Ph.D., Sociology and Social Policy, Harvard University

2009 A.M., Sociology, Harvard University

2004 A.B., Sociology, University of Chicago

PUBLICATIONS

Journal Articles

Owens, Ann, and Susan Clampet-Lundquist. 2017. "Housing Mobility and the Intergenerational Durability of Neighborhood Poverty." *Journal of Urban Affairs* 39(3):400-420.

Owens, Ann. 2017. "Racial Residential Segregation of School-Age Children and Adults: The Role of Schooling as a Segregating Force." *RSF: The Russell Sage Foundation Journal of the Social Sciences* 3(2): 63-80.

Owens, Ann. 2017. "How Do People-Based Housing Policies Affect People (and Place)?" *Housing Policy Debate*. 2(1): 266-281.

Owens, Ann, Sean F. Reardon, and Christopher Jencks. 2016. "Income Segregation between Schools and School Districts." *American Educational Research Journal* 53(4): 1159-1197.

Owens, Ann. 2016. "Inequality in Children's Contexts: Income Segregation of Households With and Without Children." *American Sociological Review* 81(3): 549-574.

Owens, Ann. 2016. "Assisted Housing and Neighborhood Poverty Dynamics, 1977 to 2008." *Urban Affairs Review* 52(3): 287-322.

Owens, Ann. 2015. “Housing Policy and Urban Inequality: Did the Transformation of Assisted Housing Reduce Poverty Concentration?” *Social Forces* 94(1): 325-348.

Owens, Ann. 2015. “Assisted Housing and Income Segregation between Neighborhoods in U.S. Metropolitan Areas.” *The Annals of the American Academy of Political and Social Science* 660(1): 98-116.

Reardon, Sean F. and Ann Owens. 2014. “60 Years After Brown: Trends and Consequences of School Segregation.” *Annual Review of Sociology* 40: 199-218.

Owens, Ann. 2013. “Perceptions of Disorder and Safety amidst the Transformation of Assisted Housing.” *Cityscape* 15(3): 77-103.

Owens, Ann. 2012. “Neighborhoods on the Rise: A Typology of Neighborhoods Experiencing Socioeconomic Ascent.” *City & Community* 11(4): 345-369.

Edin, Kathryn, Stefanie DeLuca, and Ann Owens. 2012. “Constrained Compliance: Solving the Puzzle of MTO’s Lease-Up Rate and Why Mobility Matters.” *Cityscape* 14(2): 181-194.

Owens, Ann. 2010. “Neighborhoods and Schools as Competing and Reinforcing Contexts for Educational Attainment.” *Sociology of Education* 83(4): 287-311.

Broege, Nora, Ann Owens, Anthony P. Graesch, Jeanne E. Arnold, and Barbara Schneider. 2007. “Calibrating Measures of Family Activities between Large- and Small-Scale Data Sets.” *Sociological Methodology* 37(1): 119-149.

Owens, Ann, and Barbara Schneider. 2005. “Self-Regulation and the Transition to Adulthood.” *Academic Exchange Quarterly* 9(4): 62-68.

Book Chapters, Briefs, and Reports

Owens, Ann. In preparation. “School Racial and Economic Segregation.” Textbook chapter in *Education & Society*, edited by T. Domina, B. Gibbs, L. Nunn, and A. Penner. University of California Press.

Owens, Ann. 2016. “Urban Revitalization in U.S. Cities and Neighborhoods, 1990 to 2010.” White paper commissioned by the 21st Century Cities Initiative, Johns Hopkins University.

Owens, Ann. 2013. “Have Reforms in U.S. Housing Assistance Reduced Neighborhood Poverty?” Scholars Strategy Network Key Findings Brief. Cambridge: MA: Harvard University.

Owens, Ann and Robert J. Sampson. 2013. “Community Well-being and the Great Recession.” *Pathways* Spring 2013: 3-7. Stanford, CA: Stanford Center on Poverty and Inequality.

Jencks, Christopher, Ann Owens, Tracey Shollenberger, and Queenie Zhu. 2010. “How has Rising Economic Inequality Affected Children's Educational Outcomes?” Conference Paper.

Tobin Project Conference on Democracy & Markets: Understanding the Effects of America's Economic Stratification.

Owens, Ann, and Gail Sunderman. 2006. "School Accountability under NCLB: Aid or Obstacle for Measuring Racial Equity?" Policy Brief. The Civil Rights Project at UCLA.

Owens, Ann. 2006. "Common Schools." *Blackwell Encyclopedia of Sociology*. Edited by G. Ritzer. Blackwell Publishing.

Owens, Ann. 2006. "Public Schools." *Blackwell Encyclopedia of Sociology*. Edited by G. Ritzer. Blackwell Publishing.

Schneider, Barbara, Sylvia Martinez, and Ann Owens. 2006. "Barriers to Educational Opportunities for Hispanics in the U.S." Pp. 179-227 in *Hispanics and the Future of America*, edited by M. Tienda. Washington, DC: National Academies Press.

Schneider, Barbara, Allison Atteberry, and Ann Owens. 2005. "Family Matters: Family Structure and Child Outcomes." Alabama Policy Institute.

FELLOWSHIPS AND AWARDS

2016-2017	National Academy of Education/Spencer Foundation Postdoctoral Fellow
2012-2013	Postdoctoral Fellow, Center on Poverty and Inequality, Stanford University
2012	Honorable Mention, David Lee Stevenson Award for the Best Graduate Student Paper, ASA Sociology of Education Section
2011-2012	Graduate Society Dissertation Completion Fellowship, Harvard University
2009-2010	Doctoral Fellowship, NSF Multidisciplinary Program in Inequality & Social Policy, Harvard University
2006-2010	Perry Family Graduate Fellowship, Harvard University
2007-2008	Harvard University Certificate of Distinction in Teaching
2005-2006	Harvard University Graduate Fellowship
2005, 2006	NSF Graduate Research Fellowship Program-Honorable Mention
2004	Student Employee of the Year Award, University of Chicago

RESEARCH FUNDING

External Research Grants

2017	"School Choice and Inequality in Los Angeles." Haynes Foundation Faculty Fellowship Award. $12,000.
2015	"Income Segregation and Student Achievement Gaps, 1996-2010." New Scholars Grant, Center on Poverty and Inequality, Stanford University. $10,000. (Co-PI: Kendra Bischoff, Cornell University)

“Economic Segregation of School Districts and the Economic Achievement Gap.” Spencer Foundation. $49,161.

“Economic Segregation of School Districts and the Income Achievement Gap.” Russell Sage Foundation. $34,823.

2014 “Understanding the Role of Contextual Effects in STEM Pursuit and Persistence: A Synthesis Approach.” National Science Foundation Discovery Research K-12. $250,000. (Co-PIs: Michael Gottfried, UCSB; Darryl Williams, Tufts)

2013 “Economic Segregation between Schools Districts: Trends and Correlates, 1990 to 2010.” New Scholars Grant, Center on Poverty and Inequality, Stanford University. $20,000.

2012 “Housing Mobility and the Intergenerational Transmission of Neighborhood Poverty.” Center for Poverty Research, University of California, Davis. $20,000 (Co-PI: Susan Clampet-Lundquist, St. Joseph’s University)

2010 Eli Ginzberg Award (“For a project involving solutions to major health and welfare problems in urban settings”), Horowitz Foundation for Social Policy, $5,000

University Research Grants

2014 “Impacts of the Transformation of Public Housing on Neighborhood Wellbeing.” Lusk Research Center for Real Estate Research Award, University of Southern California. $19,474.

“Hispanic Neighborhood Ascent across U.S. Cities.” Zumberge Fund Individual Grant, University of Southern California. $25,000.

“The *Gente-fication* of Los Angeles: Trends and Processes of Hispanic Neighborhood Ascent.” Population Research Center, University of Southern California. $10,000.

2010 Research Grant, Harvard University Real Estate Academic Initiative, $10,000

TEACHING

2013- Assistant Professor, USC
Sociology 150: Social Problems (Undergraduate)
Sociology 521: Social Statistics and Quantitative Methods I (Graduate)
Sociology 525: Approaches to Sociological Research (Graduate)
Sociology 664: Seminar in Advanced Research Methods (Graduate)

2010 Instructor, Harvard University

Sociology 99s: Senior Thesis Writers Seminar (Undergraduate)
Author, "A Guide to Writing a Senior Thesis in Sociology"

2007-2009 Teaching Fellow, Harvard University
Sociology 107: The American Family (Undergraduate)
Sociology 128: Models of Social Science Research (Undergraduate)
Sociology 171: Sociology of Crime and Punishment (Undergraduate)
Sociology 145: Urban Social Problems (Undergraduate)

PRESENTATIONS (* invited presentation)

"Unequal School Contexts over Two Decades: Moving beyond Race and Free Lunch Composition" (with Kendra Bischoff). American Sociological Association Annual Meeting. Montreal. 2017.

*"Income Segregation and Opportunity for All." American Sociological Association Annual Meeting. Montreal. 2017.

* "School Integration by Race & Class: A Movement Reborn?" (Panelist). Albert Shanker Institute. Washington, DC. 2017.

"The Consequences of Income Segregation between School Districts for Economic and Racial Achievement Gaps."

*Center for the Study of Wealth and Inequality Seminar Series, Columbia University. New York, NY. 2017.

Population Association of America Annual Meeting. Chicago, IL. 2017.

"Racial Residential Segregation of Children and Adults and the Role of Schooling as a Segregating Force." Association for Public Policy Analysis and Management Fall Research Conference. Washington, DC. 2016.

"Income Segregation and the Income Achievement Gap." Association for Public Policy Analysis and Management Fall Research Conference. Washington, DC. 2016.

*Witness (provided testimony). The Spirit of Brown: Steps Congress Must Take to Address Segregation and Improve Equity of Opportunity in K-12 Public Schools. U.S. House Committee on Education and the Workforce. Washington, DC. 2016.

*"How Neighborhoods Affect the Lives of Low-Income Families." Opportunity Everywhere: Affirmatively Furthering Fair Housing to Transform Communities. Open Communities Alliance and University of Connecticut Law School. Hartford, CT. 2016.

*"Trends in Segregation." Education Writers Association National Seminar. Boston, MA. 2016.

"Racial Hierarchy and Racial Transition of Ascending Neighborhoods in the U.S." (with Jennifer Candipan).

Urban Affairs Association Annual Conference. San Diego, CA. 2016.
American Sociological Association Annual Meeting. Seattle, WA. 2016.

*"Federal Assisted Housing Programs and Poverty Concentration." Government Accountability Office Rental Housing Conference, Washington, DC. 2016.

"How Do People-Based Housing Policies Affect People (and Place)?" Association for Public Policy Analysis and Management Fall Research Conference. Miami, FL. 2015.

"How do School Composition and Sorting between Schools Shape Student Achievement? Disentangling Complexities." American Sociological Association Annual Meeting. Chicago, IL. 2015.

"Assisted Housing and Intergenerational Income Transmission: Exploring the Geography of Unequal Opportunity" (with Deirdre Bloome).
Population Association of America Annual Meeting. San Diego, CA. 2015.
American Sociological Association Annual Meeting. Seattle, WA. 2016.

*"Economic Segregation of School Districts and Neighborhoods." UCLA California Center for Population Research. Los Angeles, CA. 2015.

"Childhood Neighborhood Inequality and Adult Educational Attainment: A Multi-Cohort Longitudinal Study, 1995 to 2012" (with Robert J. Sampson). Association for Public Policy Analysis & Management Annual Meeting. Albuquerque, NM. 2014.

"Assisted Housing and Economic Segregation within and between Neighborhoods in U.S. Metropolitan Areas." Penn State Stratification Conference: Residential Inequality in American Neighborhoods and Communities. State College, PA. 2014.

"Housing Mobility and the Intergenerational Transmission of Neighborhood Poverty" (with Susan Clampet-Lundquist).
Population Association of America Annual Meeting. San Diego, CA. 2015.
American Sociological Association Annual Meeting. San Francisco, CA. 2014.

"Inequality in Children's Contexts: Economic Segregation between School Districts, 1990 to 2010."
American Sociological Association Annual Meeting. San Francisco, CA. 2014.
*Colloquium on the Law, Economics, and Politics of Urban Affairs, New York University. New York, NY. 2014.
Sociology of Education Association Annual Meeting. Pacific Grove, CA. 2014.
* Population, Society, and Inequality Seminar, UC-Irvine. Irvine, CA. 2014.

"Subsidized Housing and the Concentration of Poverty in the U.S." International Sociological Association World Congress. Yokohama, Japan. 2014.

*"Assisted Housing and the Deconcentration of Poverty, 1977 to 2008." UCLA Lewis Center for Regional Policy Studies. Los Angeles, CA. 2014.

"The Impact of Neighborhood Disadvantage on Educational Outcomes: A Cohort Study of Chicago" (with Robert J. Sampson). American Educational Research Association Annual Meeting. Philadelphia, PA. 2014.

"Assisted Housing and the Concentration of Black and White Poverty." Urban Affairs Association Annual Meeting. San Francisco, CA. 2013.

"Trends in Economic School Segregation, 1970 to 2009" (with Sean F. Reardon and Christopher Jencks).

*Income, Inequality, and Educational Success: New Evidence about Socioeconomic Status and Educational Outcomes Conference at Stanford University. 2012.
Choosing Homes, Choosing Schools Mini-conference at the Eastern Sociological Society Annual Meeting. New York, NY. 2012.

"The Changing Geography of Subsidized Housing: Implications for Urban Poverty."

American Sociological Association Annual Meeting. Las Vegas, NV. 2011.
Population Association of America Annual Meeting. Washington, DC. 2011.

"Hidden Costs: How has the Policy Shift to Tenant-Based Housing Subsidies Affected the Economic Well-Being of Urban Neighborhoods?" Urban Affairs Association Annual Meeting. New Orleans, LA. 2011.

"How Do Neighborhoods Matter for Mental Health? Neighborhood Contexts and Mobility as Mechanisms." American Sociological Association Annual Meeting. Atlanta, GA, 2010.

"A Typology of Gentrification." Eastern Sociological Society Annual Meeting. Boston, MA. 2010

"Neighborhood Variation in Maternal Mental Health." Eastern Sociological Society Annual Meeting. Baltimore, MD. 2009.

"Neighborhoods and Schools as Contexts for Academic Achievement."

*Texas Schools Project. UT-Dallas. Dallas, TX. 2009.
Eastern Sociological Society Annual Meeting. New York, NY. 2008.
American Educational Research Association Annual Meeting. New York, NY. 2008.
American Sociological Association Annual Meeting. Boston, MA. 2008.

*"Accumulating Knowledge through Journals" (with Barbara Schneider). National Research Council and the National Academy of Sciences. Washington, DC. 2003.

PROFESSIONAL MEMBERSHIPS

American Sociological Association (Sections: Sociology of Education; Community and Urban Sociology; Inequality, Poverty, and Mobility); Association for Public Policy Analysis & Management; Population Association of America; American Educational Research Association; Urban Affairs Association; Scholars Strategy Network

SERVICE

To the Profession

2016-	Social Science Advisory Board, Poverty & Race Research Action Council
2017-2019	ASA Inequality, Poverty, and Mobility Section: Secretary/Treasurer
2016-2018	ASA Community and Urban Sociology Section: Publications Committee
2015-2016	ASA Sociology of Education Section: Nominations Committee
2014-2017	ASA Section on Inequality, Poverty, and Mobility: Student Outreach Committee
Occasional	Grant Reviewer: Russell Sage Foundation, National Science Foundation Conference Reviewer, Organizer, and Service: AERA, PAA, SREE, ASA Manuscript Reviewer: *American Educational Research Journal, American Journal of Sociology, American Sociological Review, City & Community, Demography, Journal of Urban Affairs, Social Problems, Social Forces, Social Science Research, Sociology of Education, Urban Affairs Review* (and others)

To the Department/University

University of Southern California

2013-	Department of Sociology Committees: Graduate Admissions, Merit Review, Colloquium, Website, Space and IT, Quantitative Methods Spatial Sciences Institute: Merit Review Committee Faculty Advisor, USC Chapter of Habitat for Humanity Grant Reviewer: Zumberge Individual Fund Award, Dornsife College

PAST RESEARCH AFFILIATIONS

2008-2012	Research Assistant to Robert J. Sampson, Department of Sociology, Harvard University
2005-2012	Research Assistant to Christopher Jencks, Harvard Kennedy School
2005-2012	Graduate Student Affiliate, Harvard Institute for Quantitative Social Science
2007	Research Intern, Abt Associates, Inc., Cambridge, MA
2007	Visiting Student, Center for the Analysis of Social Exclusion, London School of Economics
2005-2006	Research Assistant, Harvard Civil Rights Project, Harvard University
2004-2005	Project Administrator, Alfred P. Sloan Center on Parents, Children & Work at the University of Chicago
2002-2004	Research Assistant, Alfred P. Sloan Center on Parents, Children & Work at the University of Chicago