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Poor Families, Poor Neighborhoods: How Family Poverty Intensifies the Impact of Concentrated Disadvantage on High School Graduation

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ABSTRACT

Theory suggests that the effects of disadvantaged neighborhoods on child educational outcomes may depend on a family's economic resources as well as the timing of neighborhood exposures during the course of child development. However, most previous research assumes that disadvantaged neighborhoods have the same effects on all children regardless of their family resources, and few prior studies specifically analyze the timing of exposure to different neighborhood conditions across the early life course. This study extends research on neighborhood effects by investigating how timing of exposure to disadvantaged neighborhoods during childhood and adolescence affects high school graduation and whether these effects vary across families with different economic resources. Results based on novel counterfactual methods for time-varying treatments and time-varying effect moderators indicate that exposure to disadvantaged neighborhoods, particularly during adolescence, has a strong negative effect on high school graduation, and that this deleterious effect is much more severe for children from poor families. The severe impact of spatially concentrated disadvantage on children from poor families suggests that ecological socialization models of neighborhood effects must account for the interactions between nested social contexts like the family environment and local neighborhood, as well as for the dynamic coevolution of these contexts over time.

INTRODUCTION

Since the publication of Wilson's (1987) influential treatise on urban poverty, researchers have worked to better understand the spatial dimensions of stratification processes, focusing especially on the impact of concentrated neighborhood disadvantage on educational attainment. Disadvantaged neighborhoods are thought to have a harmful impact on educational attainment because resident children are socially isolated from successful role models, lack access to institutional resources, are exposed to a variety of environmental health hazards, and must navigate heterogeneous subcultures with conflicting views about the utility of formal schooling (Anderson 1999; Brooks-Gunn, Duncan, and Aber 1997; Harding 2010; Jencks and Mayer 1990; Massey 2004; Massey and Denton 1993; Sampson 2001; Wilson 1987; Wilson 1996). Although earlier empirical evidence is mixed—some studies report no effect of neighborhood context on educational attainment (e.g., Ginther, Haveman, and Wolfe 2000) and others find only small effects (e.g., Aaronson 1998; Brooks-Gunn, Duncan, Klebanov, and Sealand 1993; Crane 1991; Harding 2003)—more recent research documents strong neighborhood effects on educational outcomes (Crowder and South 2010; Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011).

Few studies, however, investigate whether the impact of neighborhood context depends on, or is moderated by, other circumstances of a child's life. In particular, prior research typically assumes that the effects of neighborhood context are the same for all children, regardless of their families' economic resources (e.g., Ginther, Haveman, and Wolfe 2000; Harding 2003; Wodtke, Harding, and Elwert 2011). Yet several sociological theories strongly suggest that the socioeconomic position of the family should moderate the impact of neighborhood context. Neighborhood-effect moderation occurs when individual or family characteristics dampen or amplify the effect of some neighborhood treatment. For example, compound disadvantage theory contends that family poverty intensifies the harmful effects of neighborhood deprivation because children from poor families must rely more heavily on neighborhood networks, adults, and institutional resources than children from nonpoor families (Jencks and Mayer 1990; Wilson 1987; Wilson 1996). By contrast, relative deprivation theory posits that the effects of disadvantaged neighborhoods are less severe among children in poor families because they lack the personal resources needed to capitalize on social advantages available in more affluent neighborhoods (Crosnoe 2009; Jencks and Mayer 1990). Previous studies that consider only the marginal, or population average, effects of neighborhood context may obscure potentially divergent consequences of growing up in disadvantaged neighborhoods among different subgroups of children.

The consequences of living in disadvantaged neighborhoods also likely depend on the timing of exposure during the course of development. Theories about the impact of concentrated disadvantage on child development suggest that different neighborhood exposure trajectories may have different effects on child educational outcomes, where, for example, those perspectives emphasizing peer socialization mechanisms anticipate more pronounced effects of adolescent, rather than early childhood, exposure to neighborhood disadvantage. Recent research shows that it is critically important to account for duration of exposure to disadvantaged neighborhoods (Crowder and South 2010; Wodtke, Harding, and Elwert 2011), but prior studies do not examine how effects of neighborhood deprivation vary across different development periods. If neighborhood effects are different between childhood and adolescence, then previous studies provide an incomplete assessment of the developmental process through which neighborhoods impact individuals. Neighborhood context is not a static feature of a child's life: families move, and neighborhoods change, exposing many children to different neighborhood conditions throughout the course of development (Quillian 2003; Timberlake 2007).

This study investigates whether neighborhood effects are moderated by family economic resources and whether these effects depend on the timing of neighborhood exposures during different developmental stages. Specifically, it examines how exposure to disadvantaged neighborhoods during childhood versus adolescence affects the chances of high school graduation among different subgroups of children defined in terms of the resources available to their families over time. We focus on high school graduation because it is a critical educational transition and essentially a precondition for economic security as an adult (Rumberger 1987).

Analyses of neighborhood-effect moderation are complicated by a number of methodological difficulties. First, estimating neighborhood effects without regard for effect moderation is itself a difficult endeavor because families dynamically select into and out of neighborhoods on the basis of time-varying covariates. For example, parental marital status affects future neighborhood attainment; in turn, where the family lives affects subsequent marital events. In technical terms, parental marital status is a time-varying confounder affected by past treatment. Conventional regression adjustments for observed time-varying confounders affected by past neighborhood conditions cannot consistently estimate neighborhood effects (Wodtke, Harding, and Elwert 2011).

Second, estimating how neighborhood effects are moderated by family economic resources is even more complicated because the resources available to a family at a given point in time depend in part on where the family lived in the past. If, for example, we are interested in the effects of adolescent exposure to disadvantaged neighborhoods among subjects whose families are poor during this developmental stage, we must contend with the problem that neighborhood conditions during childhood in part create the subgroup of interest (poor families) during adolescence. Appropriate handling of endogenous effect moderators, such as family resources, presents an additional challenge above and beyond the problem of adjusting for time-varying confounders affected by prior neighborhood context. Methods designed to appropriately adjust for time-varying confounders in models for the marginal, or population average, effects of treatment—for example, marginal structural models estimated by inverse probability of treatment weighting (Wodtke, Harding, and Elwert 2011)—are not appropriate for analyzing effect moderation by endogenous time-varying covariates.

To overcome these problems, we use a two-stage regression-with-residuals estimator for structural nested mean models to assess the effects of different neighborhood exposure trajectories conditional on the evolving economic position of the family (Almirall, McCaffrey, Ramchand, and Murphy 2011; Almirall, Ten Have, and Murphy 2010; Robins 1994). Under assumptions defined below, these methods provide for unbiased estimation of the moderated effects of time-varying treatments when time-varying confounders and putative moderators are affected by past treatment.

This study advances research on the educational effects of concentrated neighborhood disadvantage by (1) delineating a counterfactual model for neighborhood effects that incorporates both time-varying treatments and effect moderators and (2) estimating the effects of exposure to disadvantaged neighborhoods during childhood versus adolescence for different subgroups of children defined in terms of their family resource history. We begin with a brief review of the theoretical mechanisms through which neighborhood disadvantage is thought to impact high school graduation. Next, we discuss several theories positing that the effects of neighborhood disadvantage depend on the economic position of the family and the developmental timing of exposure. Then, we outline the dynamic neighborhood selection process that complicates conventional regression analyses, present the structural nested mean model and its two-stage regression estimator, and, with data from the Panel Study of Income Dynamics, estimate the moderated effects of exposure to disadvantaged neighborhoods during childhood versus adolescence on high school graduation.

Results indicate that exposure to disadvantaged neighborhoods, particularly during adolescence, has a strong negative effect on the chances of high school graduation and that the deleterious effect of adolescent exposure to disadvantaged neighborhoods is much more severe among individuals whose families are poor during this developmental period. In other words, we find that the subgroup of individuals living in poor families during adolescence is especially vulnerable to the harmful effects of disadvantaged neighborhoods. We conclude that ecological socialization models must account for the interactions between nested social contexts like the family and neighborhood, as well as for the dynamic coevolution of these contexts over time.

NEIGHBORHOOD MECHANISMS

The mechanisms through which residence in disadvantaged neighborhoods is thought to influence educational attainment include social isolation, social disorganization, institutional resource deprivation, and environmental health hazards. Social isolation theories emphasize the absence of adult role models demonstrating the advantages of formal education (Wilson 1987; Wilson 1996) and the alternative, or heterogeneous, cultural messages about the value of schooling that children must navigate in impoverished communities (Anderson 1999; Harding 2007; Harding 2010; Harding 2011; Massey and Denton 1993). Social disorganization models contend that violent crime and a breakdown of collective trust in poor communities impact the emotional and behavioral development of children in ways that may interfere with progression through school (Harding 2009; Sampson 2001; Sampson, Morenoff, and Gannon-Rowley 2002). Institutional resource perspectives focus on the detrimental effects of low-quality schools and the limited services available to residents of disadvantaged neighborhoods (Brooks-Gunn, Duncan, and Aber 1997; Small and Newman 2001). Environmental models posit that the physical hazards to which children living in impoverished communities are disproportionately exposed, such as heavy air pollution and indoor allergens, have harmful effects on child health and disrupt educational progress (Earls and Carlson 2001; Kawachi and Berkman 2003).

NEIGHBORHOOD EFFECT MODERATION BY FAMILY RESOURCES

Children raised in families with different economic resources likely respond differently to the social milieu in which they are immersed. It remains unclear, however, whether children of resource-rich or resource-poor families are more sensitive to disadvantaged neighborhoods.

Competing social theories about neighborhood effect moderation—compound disadvantage theory and relative deprivation theory—suggest starkly different scenarios.

Compound disadvantage theory posits that the detrimental impact of exposure to poor neighborhoods is more severe for children who are also living in poor families (Jencks and Mayer 1990; Wilson 1987; Wilson 1996). Family poverty itself is known to harm children's educational attainment (Duncan and Brooks-Gunn 1997; Duncan, Yeung, Brooks-Gunn, and Smith 1998; Mayer 1997). Beyond this independent effect, family poverty is further thought to exacerbate the effects of neighborhood disadvantage for several reasons. First, the social networks of poor families are more often restricted to the local neighborhood than those of nonpoor families (Jencks and Mayer 1990). By virtue of the limited geographic scope of their social networks, children with poor parents may be more sensitive to the absence of successful role models and the presence of "ghetto-related" subcultures in the local neighborhood (Wilson 1996). Without parents or resident adults to signal that socioeconomic advancement is possible, children living in both poor families and disadvantaged neighborhoods may develop indelible fatalistic sentiments about their life chances.

Second, in order to acquire the cultural skills that facilitate advancement in the formal education system (Carter 2005), children with poor parents must rely more heavily on resident adults and neighborhood institutions. By contrast, children with economically advantaged parents can learn these skills at home and thus are less dependent on the local neighborhood. Thus, if the neighborhood lacks role models and institutions to instill the requisite cultural skills, then children in poor families will be most affected.

Finally, parents with greater economic resources may be able to "buy out" of the potentially harmful effects of institutional resource deprivation in disadvantaged neighborhoods. For example, nonpoor parents living in disadvantaged neighborhoods may be able to afford higher-quality childcare outside the neighborhood, enroll their children in private schools or other supplementary educational programs, and travel beyond the neighborhood to secure other goods and services that facilitate effective parenting. Children from poor families, on the other hand, are likely more dependent on the institutional resources, or lack thereof, within the neighborhood. For compound disadvantage theory, then, the negative educational effects of residence in more versus less disadvantaged neighborhoods are hypothesized to be especially severe for children in poor families and comparatively modest for children in nonpoor families.

In sharp contrast to compound disadvantage theory, relative deprivation theory, as it relates to neighborhood effect moderation, contends that the impact of neighborhood disadvantage is less severe for children in poor families than for children in nonpoor families because a variety of social processes prevent poor children from realizing the benefits associated with residence in more advantaged neighborhoods. Children in both poor and nonpoor families are thought to have lower educational outcomes in disadvantaged neighborhoods, but children in poor families are thought to benefit less than children in nonpoor families from residence in more advantaged neighborhoods.

Because poor families lack disposable income, they may not be able to capitalize on the availability of institutional resources in advantaged neighborhoods (Jencks and Mayer 1990). For example, living in a neighborhood with quality childcare, high-end grocery stores, and many recreational programs may be of little consequence to families that cannot afford these goods and services. In this situation, residence in a more affluent neighborhood, relative to residence in a disadvantaged neighborhood, may have little impact on children from poor families. By contrast, children in nonpoor families, who can realize the benefits of access to neighborhood resources, are expected to be more sensitive to their neighborhood context.

According to social psychological variants of relative deprivation theory, children evaluate themselves, and are evaluated by resident adults, relative to their neighborhood or school peers (Crosnoe 2009; Marsh 1987). Poor children living in more affluent neighborhoods, then, may suffer stigmatization or develop negative self-perceptions that interfere with their schooling. Nonpoor children in affluent neighborhoods do not suffer the harmful psychological and emotional effects of relative deprivation. Thus, when children from poor families live in affluent neighborhoods, they may encounter a unique set of psychosocial harms that attenuate the potential benefits of residence in more advantaged neighborhoods. The harmful psychological effects of relative deprivation do not befall children from nonpoor families, and they are more likely to prosper as a result of moving from a more to a less disadvantaged neighborhood.

Living in a more affluent neighborhood may also put children with poor parents at a competitive disadvantage for access to limited educational resources, such as college preparatory courses and attention from school staff (Crosnoe 2009; Jencks and Mayer 1990). Because nonpoor children tend to be better prepared for school and have parents who are better equipped to navigate the school system (Lareau 2000), they are more likely to secure these desired resources, while children from poor families are displaced into less rigorous courses and overlooked by instructors. If neighbors act as competitors for limited institutional resources (Jencks and Mayer 1990), children from poor families are at a decided disadvantage in in affluent neighborhoods. In this situation, living in less disadvantaged neighborhoods may not lead to improved educational outcomes for poor children.

A small number of empirical studies analyze how neighborhood effects on various outcomes are moderated by socioeconomic characteristics of the family, with inconsistent results. South and Crowder (1999) focus on family formation and find no significant moderation of neighborhood effects by family resources. By contrast, Wheaton and Clarke (2003) investigate neighborhood effects on mental health and find evidence that children from poor families are more vulnerable to disadvantaged neighborhoods than children from nonpoor families. Brooks-Gunn et al. (1993), as the only study of educational attainment that tests for neighborhood effect moderation, finds no evidence that the impact of neighborhood disadvantage varies by family economic resources. These studies provide important insights into neighborhood-effect moderation, but their results are limited because they do not properly account for the dynamic coevolution of neighborhood contexts and family resources over time. Families move between different neighborhood contexts (Quillian 2003; Timberlake 2007), and their economic resources change as parental income and household size fluctuate (Gottschalk, McLanahan, and Sandefur 1994). As we explain below, failure to account for the dynamic selection and feedback mechanisms by which neighborhood context and family resources influence each other, as well as inappropriate measurement of the timing and duration of exposure to different neighborhood contexts, can lead to bias.

DURATION AND TIMING OF NEIGHBORHOOD EXPOSURES

The theories outlined above all suggest that neighborhood effects on educational outcomes depend on the duration of exposure to neighborhood disadvantage. For social isolation models, where the detrimental impact of poor neighborhoods is hypothesized to operate through alternative cultural messages, a sustained exposure period is likely necessary for children to internalize the local norms, beliefs, and values. Similarly, exposure to disadvantaged neighborhoods for an extended period of time is expected to have a greater impact on educational progress if the primary neighborhood mechanisms involve school quality, institutional resource deprivation, or environmental health hazards. For example, children with transitory exposure to deficient instruction in school may be able to overcome temporary setbacks if they are enrolled in high-quality schools otherwise. By contrast, the learning deficits associated with substandard schools will likely compound with long-term exposure. Several studies attempt to assess the sensitivity of neighborhood effect estimates to duration of exposure (Crowder and South 2010; Jackson and Mare 2007; Wodtke, Harding, and Elwert 2011). The weight of the evidence suggests that long-term exposure to disadvantaged neighborhoods has a more severe impact on child outcomes than transitory exposure.

The consequences of living in disadvantaged neighborhoods likely depend not only on the duration but also on the developmental timing of exposure. Since school continuation decisions typically occur during late adolescence, residence in disadvantaged neighborhoods during this developmental stage may be especially consequential for educational attainment. Adolescence is also the period when the neighborhood becomes an important part of a child's social world (Darling and Steinberg 1997). If neighborhood effects operate primarily through peer socialization mechanisms, then adolescence is the stage at which the neighborhood would have an appreciable impact.

On the other hand, research on cognitive development and skill formation indicates that individuals are particularly sensitive to environmental deprivation early in childhood (Duncan, Yeung, Brooks-Gunn, and Smith 1998; Heckman 2006; Heckman and Krueger 2004). To the extent that later educational outcomes are affected by cognitive abilities formed during childhood, exposure to disadvantaged neighborhoods at a young age may affect school continuation decisions during adolescence. These divergent perspectives suggest that the educational effects of neighborhoods depend on exposure during a specific developmental period, but previous research has not evaluated these competing hypotheses.

NEIGHBORHOOD SELECTION AND FEEDBACK

From the moment children are born, they are, together with their parents, embedded in a neighborhood. And throughout the course of a child's development, their families often move, or the social composition of their neighborhood changes around them. Decisions to depart or stay in a particular neighborhood are determined by a variety of family characteristics, such as parental income and marital status, which also change over time. Furthermore, the same family characteristics that influence neighborhood choice are themselves influenced by the history of neighborhood conditions experienced by the family. This process of dynamic neighborhood selection and feedback, whereby characteristics of the family environment are simultaneously outcomes of prior neighborhood conditions and determinants of future neighborhood attainment, results in temporally variable patterns of exposure to different neighborhood contexts and family environments for children. This time-dependent process presents a difficult methodological problem for estimating how the effects of neighborhood disadvantage vary across groups: time-varying family covariates may be confounders for the effect of future exposures, mediators for the effect of past exposures, and potential effect moderators. To assess the effects of time-varying neighborhood conditions for subgroups of children defined in terms of family characteristics that are themselves time-varying, knowledge of the dynamic selection process is crucial.

Previous research highlights socioeconomic position, family structure, and race as important determinants of neighborhood attainment (Charles 2003; Sampson and Sharkey 2008; South and Crowder 1997a; South and Crowder 1997b; South and Crowder 1998a; South and Crowder 1998b; South and Deane 1993; Speare and Goldscheider 1987). Education, income, employment status, and homeownership are all closely linked to the social composition of the neighborhood in which a family resides, where those families who are more advantaged on these characteristics are much less likely to live in disadvantaged neighborhoods (Sampson and Sharkey 2008; South and Crowder 1997a; South and Crowder 1998a). In addition, parental marital status and family size are associated with neighborhood socioeconomic characteristics. Specifically, single parents and larger families are more likely than smaller and intact families to live in disadvantaged neighborhoods (Sampson and Sharkey 2008; South and Crowder 1998a; Speare and Goldscheider 1987). Past research also shows that spatial attainment is largely determined by race. Because of extensive discrimination at all levels of the residential sorting process, blacks are much more likely than whites to live in disadvantaged neighborhoods, regardless of group differences in education, income, or family structure (Massey and Denton 1993; Yinger 1995). Comparative studies of residential mobility show that black families, unlike their white counterparts, often struggle to convert personal resources into improved neighborhood conditions, indicating that neighborhood selection processes operate differently for blacks and whites (Iceland and Scopilliti 2008; South and Crowder 1998b; South and Deane 1993).

While there is considerable evidence that family structure and socioeconomic characteristics influence neighborhood attainment, theory and research also suggests that these covariates are themselves affected by neighborhood context (Fernandez and Su 2004; Wilson 1987; Wilson 1996). Wilson (1987) argued that adult residents of disadvantaged neighborhoods have more difficulty finding stable employment because of the paucity of jobs at appropriate skill levels in these areas (see also Fernandez and Su 2004). Living in a disadvantaged neighborhood also affects family structure, for example, by limiting the pool of potential spouses with sufficient income to support a family (Wilson 1987). Several studies suggest that exposure to disadvantaged neighborhoods leads to delayed marriage and increases the chances of non-marital fertility (South and Crowder 1999; South and Crowder 2010). Thus, time-varying family covariates may simultaneously confound, mediate, and, as outlined above, moderate the effects of disadvantaged neighborhoods.

METHODS

Data

To assess the impact of different longitudinal patterns of exposure to disadvantaged neighborhoods among subgroups of children defined by time-varying family resources, we use data from the Panel Study of Income Dynamics (PSID). The PSID is a longitudinal study of families that focuses on the dynamic aspects of economic and demographic behavior. It began in 1968 with a national sample of about 4,800 households. Then, from 1968 to 1997, the PSID interviewed household members annually; after 1997, interviews were conducted biennially. Families are matched to census tracts with the restricted-use PSID geocode file, which contains tract identifiers for 1968 through 2003, and data on the socioeconomic composition of census tracts come from the Geolytics Neighborhood Change Database (NCDB). The NCDB contains nation-wide tract-level data from the 1970-2000 U.S. Censuses with variables and tract boundaries defined consistently across time. Tract characteristics for intercensal years are imputed using linear interpolation. Longitudinal data from the PSID together with tract-level measures from the NCDB allow us to analyze trajectories of neighborhood conditions and putative effect moderators throughout the early life-course.

The analytic sample for this study includes the 6,135 subjects in the PSID who were age 2 at any time between 1968 and 1982. Using all available data for these subjects between age 2 and 17, measurements of neighborhood disadvantage and family-level covariates are constructed separately by developmental period, where the time index k is used to distinguish between measurements taken during childhood $(k = 1)$ versus adolescence $(k = 2)$. The outcome of interest, high school graduation, is measured at age 20.[1]

Treatment, Covariates, and Notation

Following Wodtke et al. (2011), principal component analysis is used to generate a composite measure of neighborhood disadvantage based on seven tract characteristics: poverty, unemployment, welfare receipt, female-headed households, education (percent of residents age 25 or older without a high school diploma, percent of residents age 25 or older with a college degree), and occupational structure (percent of residents age 25 or older in managerial or professional occupations). Census tracts are then divided into quintiles based on the national distribution of the composite disadvantage index. Treatment is an ordinal variable, A_k, coded 1 through 5 to record the neighborhood disadvantage quintile in which a subject resides. Lower values of A_k indicate that a neighborhood is less disadvantaged and higher values indicate greater disadvantage (see Appendix A for details). The childhood measurement of neighborhood disadvantage, A_1, is based on a subject's average tract disadvantage score over the four survey years from age 6 to 9. Neighborhood disadvantage during adolescence, A_2, is based on the average tract disadvantage score between age 14 and 17. Measuring neighborhood disadvantage with multi-wave averages simultaneously reduces measurement error and accounts for duration of exposure.[2]

The analysis adjusts for time-invariant and time-varying covariates. The vector of time-invariant covariates includes gender, race, birth year, mother's age and marital status at the time of childbirth, and the family head's highest level of education completed.[3] The vector of time-varying covariates includes the family income-to-needs ratio, the family head's marital and employment status, homeownership, residential mobility, and family size, all of which are measured at every wave in the PSID. At each survey wave, parental marital status is dummy-coded, 1 for married and 0 for not married; employment status is coded 1 for employed and 0 for not employed; residential mobility is coded 1 if the family moved in the previous year, and 0 otherwise; homeownership is expressed as a dummy that indicates whether the family owns the residence they occupy; and household size counts the number of people present in a subject's family at the time of the interview. The income-to-needs ratio is equal to a family's annual real income divided by the poverty threshold, which is indexed to family size. For ease of interpretation, the income-to-needs ratio is centered at the poverty line, so that this variable is greater than 0 for families with incomes that exceed poverty level and is less than 0 for families with sub-poverty incomes. In the results section, we use the descriptor "poor" for families at the poverty line (income-to-needs $= 0$), while "extremely poor" and "nonpoor" refer to families with resources equivalent to one-half the poverty line (income-to-needs $= -.5$) and three times the poverty line (income-to-needs $= 2$), respectively.

We construct separate multi-wave averages of all time-varying covariates during childhood and adolescence. The vector of time-varying covariates during childhood, L_1, is averaged over the survey waves in which a subject is age 2 to 5—the four waves immediately preceding measurement of childhood exposure to neighborhood disadvantage. To simplify notation, we include all time-invariant covariates measured at baseline in L_1. Similarly, the vector of time-varying covariates during adolescence, L_2, is averaged over the four survey waves in which a subject is age 10 to 13—the four waves preceding measurement of adolescent neighborhood disadvantage. These variables thus have the following temporal order:

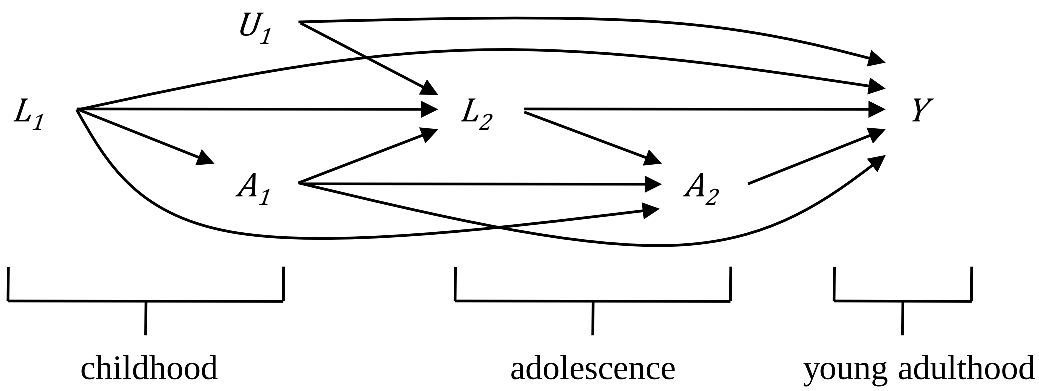
(L_1, A_1, L_2, A_2, Y), where Y is the outcome coded 1 if a subject graduated high school by age 20, and 0 otherwise. We use multiple imputation with 100 replications to fill in missing values for all covariates and the outcome (Royston 2005; Rubin 1987).[4,5]

Hypothesized Causal Relationships

Figure 1 presents a directed acyclic graph that describes the hypothesized causal relationships between neighborhood disadvantage, family covariates, unobserved factors, and the outcome, high school graduation. In directed acyclic graphs, nodes represent variables, arrows represent direct causal effects, and the absence of an arrow indicates the absence of a direct causal effect (Pearl 1995; Pearl 2000). In Figure 1, selection into different neighborhood contexts is affected by baseline covariates and prior time-varying covariates. Neighborhood context, in turn, affects future values of the time-varying covariates. The reciprocal relationship between neighborhood context and time-varying covariates at adjacent time periods defines the dynamic neighborhood selection and feedback process. Figure 1 also permits direct effects on high school graduation for exposure to neighborhood disadvantage at each developmental stage. In addition, neighborhood disadvantage during childhood has an indirect effect that operates through future family covariates. In departure from more restrictive conventional assumptions, we allow unobserved factors to directly affect time-varying covariates but not neighborhood exposure status.

Consistent with previous theory and research, this figure shows that time-varying characteristics of the family environment are simultaneously confounders for the effect of future exposure to neighborhood disadvantage and mediators for the effect of past exposure to neighborhood disadvantage. Theory also suggests that time-varying family covariates are effect moderators. Specifically, family economic resources are thought to temper or exacerbate the educational effects of exposure to disadvantaged neighborhoods. Figure 1 is consistent with neighborhood-effect moderation because the outcome in this graph depends on the hypothesized effect moderator (Elwert and Winship 2010; VanderWeele 2009; VanderWeele and Robins 2009).

Figure 1. Hypothesized causal relationships



Notes: A_k = neighborhood disadvantage, L_k = family economic resources and other time-varying covariates, U_k = unobserved factors and Y = high school graduation. L_1 includes time-invariant baseline covariates.

Counterfactual Models of Moderated Neighborhood Effects

The central aim of this analysis is to estimate how the causal effects of exposure to disadvantaged neighborhoods during childhood and adolescence are moderated by the evolving economic position of the family. In this section, we use the counterfactual framework of causal inference for time-varying treatments to formally define the moderated neighborhood effects of interest (Almirall, McCaffrey, Ramchand, and Murphy 2011; Almirall, Ten Have, and Murphy 2010; Holland 1986; Robins 1994; Robins 1999b; Rubin 1974). For expositional clarity, we treat L_1 and L_2 in this section as repeated measures of a single time-varying covariate, the family income-to-needs ratio; our empirical analyses below, however, incorporate vector-valued L_k.

Let the potential outcome $Y(a_1, a_2)$ indicate whether a subject would have graduated high school had she been exposed to the sequence of neighborhood conditions (a_1, a_2) during childhood and adolescence, possibly contrary to fact. For example, $Y(1,1)$ is the subject's outcome had she been exposed to the least disadvantaged, first quintile of neighborhoods during childhood and adolescence, $Y(2,1)$ is the outcome had she been exposed to the second quintile of neighborhoods during childhood and the least disadvantaged, first quintile of neighborhoods during adolescence, and so on. Similarly, let $L_2(a_1)$ represent the family income-to-needs ratio the subject would have experienced during adolescence had she and her family been exposed to neighborhood conditions (a_1) during childhood. Note that $L_2(a_1)$ is itself a potential outcome. Because subjects are exposed to one of five levels of neighborhood disadvantage at two developmental periods, there are twenty-five potential education outcomes $\{Y(1,1), Y(2,1), \ldots, Y(4,5), Y(5,5)\}$ and five intermediate potential income-to-needs outcomes $\{L_2(1), L_2(2), \ldots, L_2(5)\}$. For each subject, we only observe the potential outcomes corresponding to the neighborhood contexts actually experienced; all other potential outcomes are unobserved, or counterfactual.

In the counterfactual framework, causal effects are defined as contrasts between potential outcomes. We define two sets of moderated neighborhood effects, one set for exposure during childhood and one set for exposure during adolescence. The first set of moderated neighborhood effects is defined as

$$u_1(L_1, a_1) = E(Y(a_1, 1) - Y(1,1)|L_1) = (a_1 - 1)(\beta_1 + \beta_2 L_1), \quad (1)$$

which gives the direct causal effect of childhood exposure to neighborhood disadvantage, holding adolescent neighborhood conditions constant. Specifically, it gives the average causal effect of exposure sequence $(a_1, 1)$ compared to sequence $(1,1)$ within levels of L_1. In words, $u_1(L_1, a_1)$ compares the probability of high school graduation had subjects been exposed to neighborhoods in quintile a_1 during childhood and neighborhoods in the least disadvantaged quintile during adolescence with the probability of high school graduation had subjects been continuously exposed to the least disadvantaged quintile of neighborhoods, separately for families with baseline income-to-needs given by L_1. For example, $u_1(L_1 = 0, a_1 = 5)$, is the causal effect of living in the most disadvantaged, fifth quintile of neighborhoods during childhood and then in the least disadvantage quintile of neighborhoods during adolescence rather than sustained exposure to neighborhoods in the least disadvantaged quintile of neighborhoods throughout childhood and adolescence among subjects whose families had poverty-level resources $L_1 = 0$ at baseline.

We use a linear parametric function, $(a_1 - 1)(\beta_1 + \beta_2 L_1)$, to summarize these effects: β_1 gives the average direct causal effect on high school graduation of childhood exposure to neighborhoods located in quintile a_1, rather than the less disadvantaged quintile $a_1 - 1$, among subjects in families with poverty-level resources during childhood, and β_2 increments this effect for subjects in families with incomes above or below the poverty line. If $\beta_2 = 0$, then the baseline income-to-needs ratio does not moderate the impact of exposure to disadvantaged neighborhoods during childhood.[6]

The second set of moderated neighborhood effects is defined as

$$u_2(L_2(a_1), a_2) = E\left(Y(a_1, a_2) - Y(a_1, 1) \middle| L_2(a_1)\right) = (a_2 - 1)\left(\beta_3 + \beta_4 L_2(a_1)\right), \quad (2)$$

which gives the causal effect of adolescent exposure to neighborhood disadvantage, holding childhood neighborhood conditions constant. Specifically, it gives the average causal effect of neighborhood exposure sequence (a_1, a_2) compared to sequence $(a_1, 1)$ within levels of $L_2(a_1)$. That is, $u_2(L_2(a_1), a_2)$ compares the probability of high school graduation had subjects been exposed to neighborhoods in quintile a_1 during childhood and then neighborhoods in quintile a_2 during adolescence with the probability of high school graduation had subjects been exposed to neighborhoods in quintile a_1 during childhood but then neighborhoods in the least disadvantaged quintile during adolescence, separately for families with income-to-needs given by $L_2(a_1)$. For example, $u_2(L_2(5) = 0, a_2 = 5)$ is the causal effect of living in the most disadvantaged quintile of neighborhoods during adolescence, rather than the least disadvantaged quintile, had subjects first been exposed to the most disadvantaged quintile of neighborhoods during childhood and lived in families that would have poverty-level incomes $L_2(5) = 0$ during adolescence under the specified childhood exposure.

The parametric function, $(a_2 - 1)\left(\beta_3 + \beta_4 L_2(a_1)\right)$, summarizes the average effects of adolescent exposure to different neighborhood conditions for subgroups of individuals defined in terms of their family's income-to-needs ratio measured during adolescence: β_3 gives the average causal effect on high school graduation of adolescent exposure to neighborhoods located in quintile a_2, rather than the less disadvantaged quintile $a_2 - 1$, holding neighborhood conditions during childhood constant, among subjects in families that would have poverty-level resources during adolescence under the fixed childhood exposure, and β_4 increments this effect for subjects in families that would have incomes above or below the poverty line at this development stage. As above, if $\beta_4 = 0$, then the family income-to-needs ratio does not moderate the impact of adolescent exposure to neighborhood disadvantage.[7]

The causal functions defined here describe how the effects of exposure to disadvantaged neighborhoods during childhood versus adolescence depend on the evolving economic resources of the family. By including cross-product terms for the family income-to-needs ratio and neighborhood context, these functions allow us to evaluate the compound disadvantage and relative deprivation theories. In addition, by evaluating moderated neighborhood effects within a longitudinal framework, we can examine whether individuals' sensitivity to different neighborhood conditions varies by developmental stage.

These causal functions involve conditional counterfactuals that are quite complex. For clarity, it can be helpful to explain $u_1(L_1, a_1)$ and $u_2(L_2(a_1), a_2)$ using the language and logic of sequential experiments. Consider a hypothetical experiment where, at baseline (childhood), the researcher would first measure the family resources of each subject in the study. Next, the researcher would randomly assign all subjects (and their families) to neighborhoods in different quintiles of the disadvantage distribution during childhood, and then later, during adolescence, assign all subjects to neighborhoods in the same disadvantage quintile. Finally, the researcher would observe at the end of follow-up whether or not each subject graduated high school. Comparing mean outcomes for subjects assigned to different neighborhood contexts during childhood, separately by their families' resources at baseline, would be an experimental estimate of $u_1(L_1, a_1)$, the childhood causal function.

The moderating role of family resources with respect to adolescent neighborhood disadvantage would be captured in a different hypothetical experiment. In this experiment, the researcher would first assign all subjects to live in the same quintile of neighborhoods during childhood and measure their families' resources only after this initial intervention. Then, the researcher would randomly assign subjects to neighborhoods in different quintiles of the disadvantage distribution during adolescence and measure high school graduation at the end of follow-up. Comparing mean outcomes for subjects assigned to different neighborhood contexts during adolescence, separately by family resources measured just prior to adolescent treatment assignment, would be an experimental estimate of $u_2(L_2(a_1), a_2)$, the adolescent causal function. Note that, rather than conducting two separate experiments to recover $u_1(L_1, a_1)$ and $u_2(L_2(a_1), a_2)$, one could also conduct a single sequentially randomized experiment in which subjects are randomly assigned to different neighborhood quintiles during both childhood and adolescence, and measurements of family resources are taken just prior to treatment assignment at each developmental stage. Such a sequentially randomized experiment is the canonic motivation for the structural nested mean model (SNMM), which is used to simultaneously estimate the causal functions.[8]

The SNMM is a particular decomposition of the conditional expectation of $Y(a_1, a_2)$ given $\left(L_1, L_2(a_1)\right)$ that includes the moderated neighborhood effects of interest, $u_1(L_1, a_1)$ and $u_2(L_2(a_1), a_2)$, as well as a set of "nuisance" functions, denoted by $\varepsilon_1(L_1)$ and $\varepsilon_2\left(L_1, a_1, L_2(a_1)\right)$, that capture the association of the moderator(s) with outcome (Almirall, Coffman, Yancy, and Murphy 2010; Almirall, McCaffrey, Ramchand, and Murphy 2011; Almirall, Ten Have, and Murphy 2010; Robins 1994; Robins 1999b). These nuisance functions do not identify causal effects, and they are not of direct substantive interest, but estimating moderated neighborhood effects requires their correct specification in the model.

Specifically, the SNMM is expressed as

$$E\big(Y(a_1,a_2)\big|L_1,L_2(a_1)\big)$$
$$= \beta_0 + \varepsilon_1(L_1) + u_1(L_1,a_1) + \varepsilon_2\big(L_1,a_1,L_2(a_1)\big) + u_2(L_2(a_1),a_2), \tag{3}$$

where $\beta_0 = E\big(Y(1,1)\big)$ is the mean of the potential outcomes under sustained exposure to the least disadvantaged quintile of neighborhoods, $\varepsilon_1(L_1) = E(Y(1,1)|L_1) - E\big(Y(1,1)\big)$ is the association between the family income-to-needs ratio and high school graduation had all subjects lived only in the least disadvantaged quintile of neighborhoods, and $\varepsilon_2\big(L_1,a_1,L_2(a_1)\big) = E\big(Y(a_1,1)|L_1,L_2(a_1)\big) - E(Y(a_1,1)|L_1)$ is the association between L_2 and high school graduation had subjects with characteristics (a_1,L_1) lived in the least disadvantaged quintile of neighborhoods during adolescence.[9]

An important property of $\varepsilon_1(L_1)$ and $\varepsilon_2\big(L_1,a_1,L_2(a_1)\big)$ is that their conditional expectation equals zero given the past (Almirall, McCaffrey, Ramchand, and Murphy 2011; Almirall, Ten Have, and Murphy 2010). Indeed, the central challenge to estimating the causal functions of the SNMM is to specify the nuisance functions in a way that preserves this zero conditional expectation property. To this end, $\varepsilon_1(L_1)$ is modeled with the function $\eta_1\big(L_1 - E(L_1)\big)$, and $\varepsilon_2\big(L_1,a_1,L_2(a_1)\big)$ is modeled with the function $\eta_2\big(L_2(a_1) - E(L_2(a_1)|L_1)\big)$. Note that the terms in parentheses associated the parameters η_1 and η_2 are like residuals for the childhood and adolescent measurements of the income-to-needs ratio. This property informs the two-stage estimation strategy explained below.

The causal effects defined in Equations 1 and 2 can be identified from observed data under the assumption of sequential ignorability of treatment assignment. Formally, this condition is expressed in two parts as $Y(a_1,a_2) \perp A_1|L_1$ and $Y(a_1,a_2) \perp A_2|L_1,A_1,L_2$, where $\perp$ denotes statistical independence. Substantively, this condition states that at each time period there exist no other variables that directly affect selection into different neighborhood contexts and the outcome, high school graduation, apart from prior measured covariates and prior neighborhood context, as previously illustrated in Figure 1. Sequential ignorability is met by design in experimental studies where treatment is randomly assigned at each time point. However, in observational studies, such as the present investigation, satisfying this assumption requires data on all the joint predictors of neighborhood disadvantage and high school graduation (we present a sensitivity analysis for this assumption below).

Limitations of Conventional Regression Models

Estimating how the causal effects of time-varying neighborhood conditions are moderated by a family's economic resources is difficult. Here, we briefly present an explanation for why traditional methods fail at this task. Consider the conventional linear probability model for the effects of exposure to disadvantaged neighborhoods during childhood and adolescence with a single time-varying effect moderator, the family income-to-needs ratio:

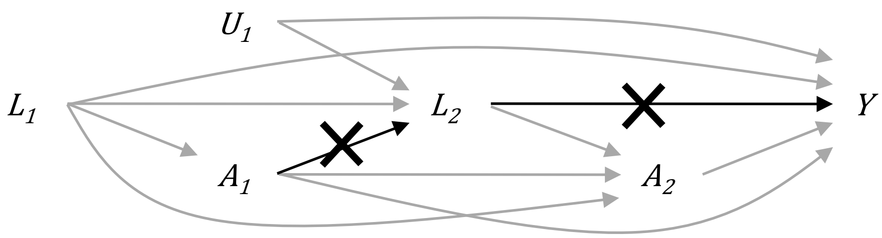
$$E(Y|L_1, A_1, L_2, A_2)$$
$$= \lambda_0 + \lambda_1 L_1 + (A_1 - 1)(\lambda_2 + \lambda_3 L_1) + \lambda_4 L_2 + (A_2 - 1)(\lambda_5 + \lambda_6 L_2). \quad (4)$$

Equation 4 includes "main effects" for neighborhood disadvantage and the income-to-needs ratio measured at each developmental period. The model also includes terms that allow the coefficients on neighborhood disadvantage to vary for families above or below the poverty line.

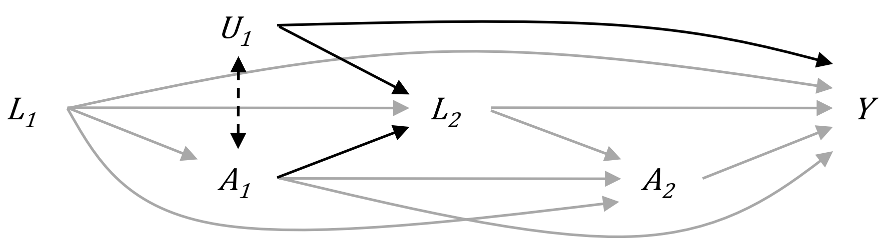
Unfortunately, this model yields biased estimates of moderated causal effects under the assumptions encoded in Figure 1. Because Equation 4 directly conditions on the adolescent income-to-needs ratio, L_2, which is affected by childhood exposure to neighborhood disadvantage, the parameters λ_2 and λ_3 do not represent the moderated causal effects of childhood exposure to neighborhood disadvantage. As depicted graphically in Figure 2, conditioning on L_2 removes the indirect effect of exposure to disadvantaged neighborhoods during childhood that is transmitted through the family income-to-needs ratio during adolescence and induces a noncausal association between neighborhood context and unobserved determinants of high school graduation (i.e. the error term of the outcome) (Elwert and Winship 2012; Greenland 2003; Pearl 1995; Pearl 2000; VanderWeele and Robins 2007).

Figure 2. Problems with conventional regression models

A. Over-control of intermediate pathways



B. Collider-stratification bias



Notes: A_k = neighborhood disadvantage, L_k = family economic resources and other time-varying covariates, U_k = unobserved factors and Y = high school graduation. L_1 includes time-invariant baseline covariates.

With observational data in which time-varying moderators are affected by past levels of a time-varying treatment, conventional regression models provide biased estimates of moderated treatment effects *even if there is no unobserved confounding of treatment* (Robins 1987; Robins 1994; Robins 1999a). In other words, even with data from an optimal experiment that sequentially randomized exposure to disadvantaged neighborhoods, conventional regression models would fail to recover the moderated effects of neighborhood disadvantage if the moderating covariates of interest were time-varying and affected by past neighborhood conditions (Almirall, McCaffrey, Ramchand, and Murphy 2011; Almirall, Ten Have, and Murphy 2010; Robins 1994; Robins 1999b). Thus, alternative methods are needed to estimate moderated neighborhood effects in this study.[10]

Two-stage Regression-with-Residuals Estimation

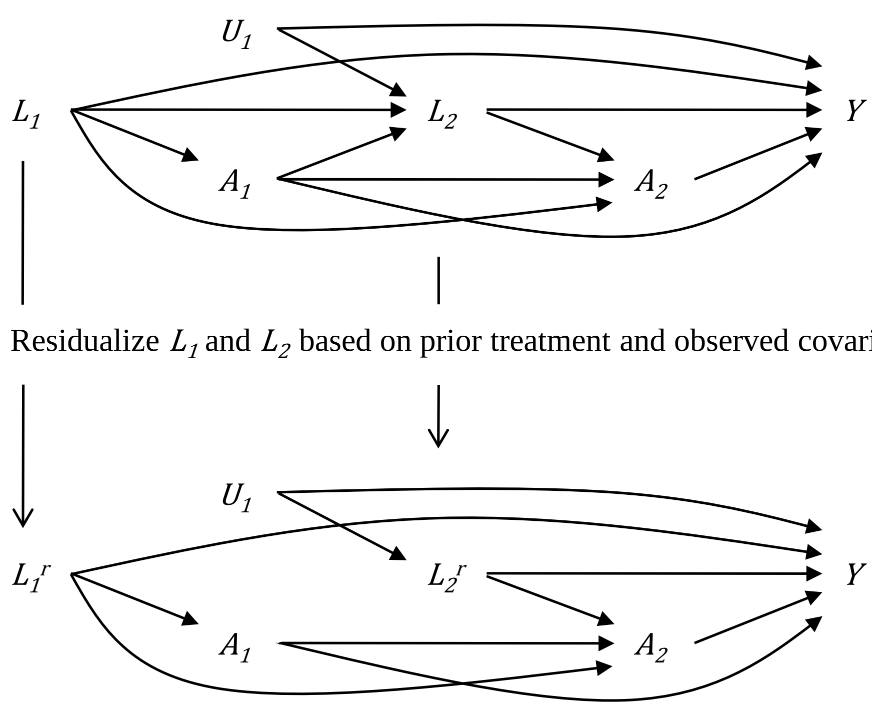
Almirall and colleagues (2011; 2010) provide a two-stage regression estimator for the SNMM that is motivated by the zero conditional expectation property of the nuisance functions discussed above. This approach is similar to estimating a conventional regression model, but it proceeds in two steps. In the first stage, all time-varying covariates are regressed on the observed past to obtain estimated residuals. For example, we regress the income-to-needs ratios in childhood and adolescence on prior neighborhood context and time-varying covariates in models with form $E(L_1) = \alpha_0$ and $E(L_2|L_1, A_1) = \gamma_0 + \gamma_1 L_1 + (A_1 - 1)(\gamma_2 + \gamma_3 L_1)$, and then we estimate the residuals as $L_1^r = L_1 - E(L_1)$ and $L_2^r = L_2 - E(L_2|L_1, A_1)$. In the second stage, the SNMM is estimated by regressing the observed outcome on neighborhood context and the residualized time-varying covariates in a model with form,

$$E(Y|L_1, A_1, L_2, A_2)$$
$$= \beta_0 + \eta_1 L_1^r + (A_1 - 1)(\beta_1 + \beta_2 L_1) + \eta_2 L_2^r + (A_2 - 1)(\beta_3 + \beta_4 L_2). \quad (5)$$

As described above, the beta coefficients quantify how the probability of high school graduation is expected to change with exposure to different neighborhood contexts during childhood versus adolescence, conditional on prior income-to-needs, and the eta coefficients capture the association between time-varying covariates and high school graduation. The only difference between Equation 5 and the conventional regression model in Equation 4 is that Equation 5 includes “main effects” for the residualized time-varying covariates obtained from the first-stage regressions rather than for the observed time-varying covariates. In other words, Equation 5 uses $\eta_1 L_1^r$ as the model for $\varepsilon_1(L_1)$ and $\eta_2 L_2^r$ as the model for $\varepsilon_2\big(L_1, a_1, L_2(a_1)\big)$, thereby satisfying the zero conditional expectation property of the nuisance functions in the SNMM.

Figure 3 shows a stylized graph describing how the relationship between treatment and future time-varying covariates changes after the latter are transformed into residuals. Residualizing the adolescent income-to-needs ratio purges it of its association with neighborhood disadvantage during childhood (i.e., no arrow from A_1 to L_2^r). Conditioning on the residualized income-to-needs ratio in the second-stage regression, then, does not "control away" the indirect effects of childhood exposure to disadvantaged neighborhoods that operate through this time-varying covariate nor does it induce an association between childhood exposure status and unobserved determinants of high school graduation. Thus, unlike conventional regression, the two-stage regression-with-residuals estimator does not incur the biases associated with time-varying covariates affected by prior treatment, and it provides unbiased estimates of moderated treatment effects under assumptions of no unobserved confounders and no model misspecification.

Figure 3. Consequences of residualizing time-varying covariates



Notes: A_k = neighborhood disadvantage, L_k = family economic resources and other time-varying covariates, U_k = unobserved factors and Y = high school graduation. L_1 includes time-invariant baseline covariates.

We compute two-stage regression estimates of the moderated effects of neighborhood disadvantage on high school graduation, focusing on a single time-varying effect moderator, the income-to-needs ratio, because theory suggests an important role for family economic resources in buffering or amplifying the effects of neighborhood context. Other covariates are treated as controls, entering only nuisance functions in the SNMM. Estimates are reported for the total population and also for black and nonblack subjects separately in order to investigate potential differences in the severity of neighborhood effects by race. Standard errors are estimated from 2,000 bootstrap samples (Efron and Tibshirani 1993).[11]

RESULTS

Sample Characteristics

Time-invariant covariates are summarized in Table 1, revealing considerable racial inequalities. Overall, 80 percent of the total sample graduated high school by age 20, but only 75 percent of blacks are high school graduates compared to 85 percent of nonblacks. Parents of black subjects are also much more disadvantaged than parents of nonblack subjects. For example, blacks are more likely than nonblacks to have been born to young, unmarried mothers, and black heads of household have much lower educational attainment than their nonblack counterparts.

Table 1. Time-invariant covariates

Variable	Total			Blacks		Nonblacks	
	% miss	mean	sd	mean	Sd	mean	sd
S - high school graduate	43.0	.80	(.40)	.75	(.44)	.85	(.36)
S - female	0.0	.48	(.50)	.49	(.50)	.48	(.50)
M - age at childbirth	23.4	24.79	(5.56)	23.78	(5.62)	25.70	(5.35)
M - married at childbirth	25.8	.71	(.45)	.50	(.50)	.90	(.30)
H - high school graduate	2.9	.24	(.43)	.25	(.43)	.24	(.43)
H - some college	2.9	.35	(.48)	.22	(.41)	.48	(.50)

Notes: Results are combined estimates from 100 multiple imputation datasets. S, M and H indicate subject, mother of subject, and household head, respectively.

Table 2 presents descriptive statistics for time-varying covariates, which further document sizeable racial disparities. Nonblacks are much more likely than blacks to live with heads of household that are married, employed, and who are homeowners. Nonblack families are also smaller and less mobile than black families, and nonblacks have substantially more economic resources at their disposal. Racial disparities in time-varying family covariates appear to widen over time. For example, black-nonblack differences in marital and employment status, as well as the income-to-needs ratio, increase between childhood and adolescence. Although the economic position of both black and nonblack families improves over time, the improvement is much greater for nonblacks, leading to growing racial disparities in material circumstances.

Table 2. Time-varying covariates

Variable	Total			Blacks		Nonblacks	
	% miss	mean	sd	mean	Sd	mean	sd
Childhood							
H - married	0.0	.73	(.40)	.58	(.45)	.87	(.29)
H - employed	0.0	.79	(.35)	.67	(.40)	.90	(.24)
FU - owns home	0.0	.46	(.45)	.30	(.41)	.61	(.44)
FU - size	0.0	4.85	(1.78)	5.23	(2.06)	4.51	(1.38)
FU - number of moves	13.1	1.15	(1.13)	1.20	(1.12)	1.11	(1.14)
FU - inc-to-needs ratio	0.0	.89	(1.22)	.35	(.92)	1.37	(1.26)
Adolescence							
H - married	23.8	.67	(.44)	.49	(.47)	.82	(.34)
H - employed	23.8	.78	(.37)	.65	(.42)	.89	(.25)
FU - owns home	23.8	.57	(.46)	.40	(.46)	.72	(.41)
FU - size	23.8	4.86	(1.57)	5.09	(1.83)	4.65	(1.25)
FU - number of moves	29.8	.76	(1.01)	.83	(1.03)	.69	(.98)
FU - inc-to-needs ratio	23.8	1.28	(1.66)	.55	(1.14)	1.95	(1.76)

Notes: Results are combined estimates from 100 multiple imputation datasets. H and FU indicate household head and family unit, respectively.

Neighborhood Conditions during Childhood and Adolescence

Table 3 describes exposure to different levels of neighborhood disadvantage during childhood and adolescence for blacks and nonblacks. The main diagonal cells show the extent of continuity in neighborhood conditions, while the off-diagonal cells describe upward and downward neighborhood mobility.

Table 3. Joint treatment distribution

n		Blacks					Nonblacks				
row		NH disadvantage quintile - adolescence					NH disadvantage quintile - adolescence				
cell		1	2	3	4	5	1	2	3	4	5
NH disadvantage quintile - childhood	1	38	11	6	8	5	358	49	23	15	3
		.56	.16	.09	.12	.07	.80	.11	.05	.03	.01
		.01	.00	.00	.00	.00	.11	.02	.01	.00	.00
	2	19	26	28	12	6	169	279	87	31	6
		.21	.29	.31	.13	.07	.30	.49	.15	.05	.01
		.01	.01	.01	.00	.00	.05	.09	.03	.01	.00
	3	20	37	62	39	38	48	245	356	107	34
		.10	.19	.32	.20	.19	.06	.31	.45	.14	.04
		.01	.01	.02	.01	.01	.01	.08	.11	.03	.01
	4	15	24	75	180	152	34	61	229	425	130
		.03	.05	.17	.40	.34	.04	.07	.26	.48	.15
		.01	.01	.03	.06	.05	.01	.02	.07	.13	.04
	5	14	33	76	239	1738	8	13	49	144	331
		.01	.02	.04	.11	.83	.01	.02	.09	.26	.61
		.00	.01	.03	.08	.60	.00	.00	.02	.04	.10

Notes: Results based on first imputation dataset.

Among blacks, 60 percent are exposed to the most disadvantaged quintile of American neighborhoods during both childhood and adolescence. While the majority of blacks grow up in highly disadvantaged neighborhoods, a nontrivial number live in less disadvantaged areas and some of these individuals are upwardly mobile. For example, among blacks living in third quintile neighborhoods during childhood, about 30 percent remain in these neighborhoods and another 30 percent move to even less disadvantaged neighborhoods during adolescence.

Downward neighborhood mobility is also common, however, with nearly 40 percent of blacks in third quintile neighborhoods during childhood moving to more disadvantaged neighborhoods in adolescence.

Compared to blacks, nonblacks grow up in much less disadvantaged neighborhoods. Only 10 percent of nonblacks live in the most disadvantaged, fifth quintile of neighborhoods throughout childhood and adolescence, and upward mobility from these areas is more common. About 11 percent of nonblacks live in the least disadvantaged, first quintile of neighborhoods throughout the early life course, and nearly 30 percent are continuously exposed to either first or second quintile neighborhoods. By contrast, only about 3 percent of blacks live in either first or second quintile neighborhoods during childhood and adolescence. Most nonblacks live in second through fourth quintile neighborhoods during childhood, and many transition upward to less disadvantaged neighborhoods in adolescence. The frequent mobility between different neighborhood contexts among both blacks and nonblacks underscores the importance of longitudinal measurement and dynamic modeling strategies in research on neighborhood effects.

Table 4 describes differences in exposure to neighborhood disadvantage during childhood and adolescence by prior family resources. The rows in this table define different levels of the income-to-needs ratio, where values below zero represent sub-poverty incomes and values greater than zero represent incomes above the poverty line. Family resources during childhood and adolescence are strongly related to neighborhood context, where those with higher income-to-needs ratios are much less likely to live in the most disadvantaged neighborhoods and much more likely to live in the least disadvantaged neighborhoods, compared to those with lower income-to-needs ratios, as expected.

Poor families, however, are not restricted to living in disadvantaged neighborhoods, and families of greater means are not bound to more advantaged communities. For example, Table 4 shows that 13 percent of families with income-to-needs ratios greater than two during childhood (i.e., with incomes more than three times the poverty line) are exposed to the most disadvantaged quintile of neighborhoods during the same developmental period. And among subjects in families with incomes at or just above the poverty line during childhood, 4 percent and 8 percent live in less disadvantaged first and second quintile neighborhoods, respectively. Even among extremely poor families with sub-poverty incomes, a nontrivial number live in less disadvantaged first and second quintile neighborhoods. Many individuals at all income-to-needs levels reside in middling, third quintile neighborhoods. The presence of families with different income-to-needs ratios in all neighborhood disadvantage quintiles enables us to estimate how neighborhood effects are moderated by family resources in childhood and adolescence.

Table 4. Treatment distribution at childhood and adolescence by prior family poverty status

	n row cell	Childhood NH disadvantage quintile 1	 2	 3	 4	 5	Adolescence NH disadvantage quintile 1	 2	 3	 4	 5
Family income-to-needs ratio	>2	279 .28 .05	217 .22 .04	177 .18 .03	177 .18 .03	131 .13 .02	506 .31 .08	366 .22 .06	332 .20 .05	236 .14 .04	197 .12 .03
	(1,2]	119 .09 .02	216 .16 .04	324 .24 .05	344 .26 .06	345 .26 .06	132 .10 .02	225 .16 .04	309 .23 .05	321 .23 .05	385 .28 .06
	[0,1]	92 .04 .01	166 .08 .03	391 .18 .06	565 .26 .09	974 .45 .16	59 .03 .01	144 .08 .02	251 .15 .04	413 .24 .07	833 .49 .14
	<0	26 .02 .00	64 .04 .01	94 .06 .02	239 .15 .04	1195 .74 .19	26 .02 .00	43 .03 .01	99 .07 .02	230 .16 .04	1028 .72 .17

Notes: Results based on first imputation dataset. Income-to-needs ratio is centered around 1 such that values less than zero represent sub-poverty incomes, values equal to 0 represent poverty-level incomes, and values greater than 0 represent incomes above the poverty line.

Moderated Neighborhood Effects

Table 5 presents two-stage regression estimates for the SNMM causal function parameters. The first column contains results from the total sample. Estimates for the direct effect of childhood exposure to neighborhood disadvantage are highly imprecise and fail to reach conventional thresholds of statistical significance. The point estimates suggest a negligible direct impact of exposure to neighborhood disadvantage during childhood $(\hat{\beta}_1 = -.005, p = .700)$ and provide no evidence of effect moderation by prior levels of the family income-to-needs ratio $(\hat{\beta}_2 = .005, p = .235)$. For example, among individuals in poor families, even the most extreme treatment contrast—exposure to the most disadvantaged quintile of neighborhoods during childhood and then exposure to the least disadvantaged quintile of neighborhoods during adolescence, compared to sustained residence in the least disadvantaged quintile of neighborhoods—is estimated to reduce the probability of high school graduation by only 2 percentage points (i.e., $\hat{u}_1(L_1 = 0, a_1 = 5) = \hat{E}(Y(5,1) - Y(1,1)|L_1 = 0) = 4(\hat{\beta}_1 + \hat{\beta}_2 L_1) = 4(-.005 + .005(0)) = -.020$, where $L_1 = 0$ indicates that a subject's family is at the poverty line during childhood). Among individuals in families above or below the poverty line, the direct effects of neighborhood disadvantage during childhood are also modest. Thus, regardless of family resources, results indicate that childhood exposure to different neighborhood contexts has a minimal impact on high school graduation if adolescent neighborhood conditions are held constant.

Table 5. Effects of neighborhood disadvantage on high school graduation (two-stage estimates)

Model	Total			Blacks			Nonblacks		
	coef	se		coef	Se		coef	se	
Intercept	.888	(.021)	***	.916	(.044)	***	.877	(.019)	***
Childhood									
NH dadvg	–.005	(.012)		–.004	(.019)		–.006	(.015)	
NH dadvg x inc-to-needs	.005	(.004)		.005	(.008)		.005	(.005)	
Adolesence									
NH dadvg	–.042	(.010)	***	–.054	(.018)	**	–.026	(.013)	†
NH dadvg x inc-to-needs	.012	(.003)	***	.017	(.006)	**	.007	(.004)	†

Notes: Results are combined estimates from 100 multiple imputation datasets. Standard errors are based on 2000 bootstrap samples.
†p < 0.10, *p < 0.05, **p < 0.01, and ***p < 0.001 for two-sided tests of no effect.

Estimates for the effect of adolescent neighborhood context, by contrast, indicate that exposure to disadvantaged neighborhoods during this developmental period has a strong and statistically significant negative effect on high school graduation $(\hat{\beta}_3 = -.042, p < .001)$ and that this effect is substantially moderated by the family income-to-needs ratio $(\hat{\beta}_4 = .012, p < .001)$. Consistent with compound disadvantage theory, these estimates indicate that disadvantaged neighborhoods are especially harmful for individuals from poor families. For example, among individuals in families living at the poverty line during adolescence, exposure to the most disadvantaged quintile of neighborhoods, rather than the least disadvantaged quintile, is estimated to reduce the probability of high school graduation by about 17 percentage points. For individuals in families who are extremely poor during adolescence, exposure to the most disadvantaged quintile of neighborhoods, compared to the least disadvantaged quintile, is estimated to reduce the probability of high school graduation by nearly 20 percentage points (i.e., $\hat{u}_2(L_2(a_1) = -.5, a_2 = 5) = \hat{E}(Y(a_1, 5) - Y(a_1, 1)|L_2(a_1) = -.5) = 4(\hat{\beta}_3 + \hat{\beta}_4 L_2) = 4(-.042 + .012(-.5)) = -.192$, where $L_2 = -.5$ indicates that a subject's family has an income equal to one-half the poverty line during adolescence).

The effects of adolescent exposure to disadvantaged neighborhoods for nonpoor individuals, on the other hand, are much less severe. Among individuals from nonpoor families with incomes equivalent to three times the poverty line during adolescence, exposure to the most, compared to the least, disadvantaged quintile of neighborhoods during the same developmental period only reduces the probability of high school graduation by about 7 percentage points. In sum, these results indicate that an individual's chance of high school graduation is most sensitive to neighborhood context during adolescence, and that family poverty intensifies the negative effects of adolescent exposure to disadvantaged neighborhoods.

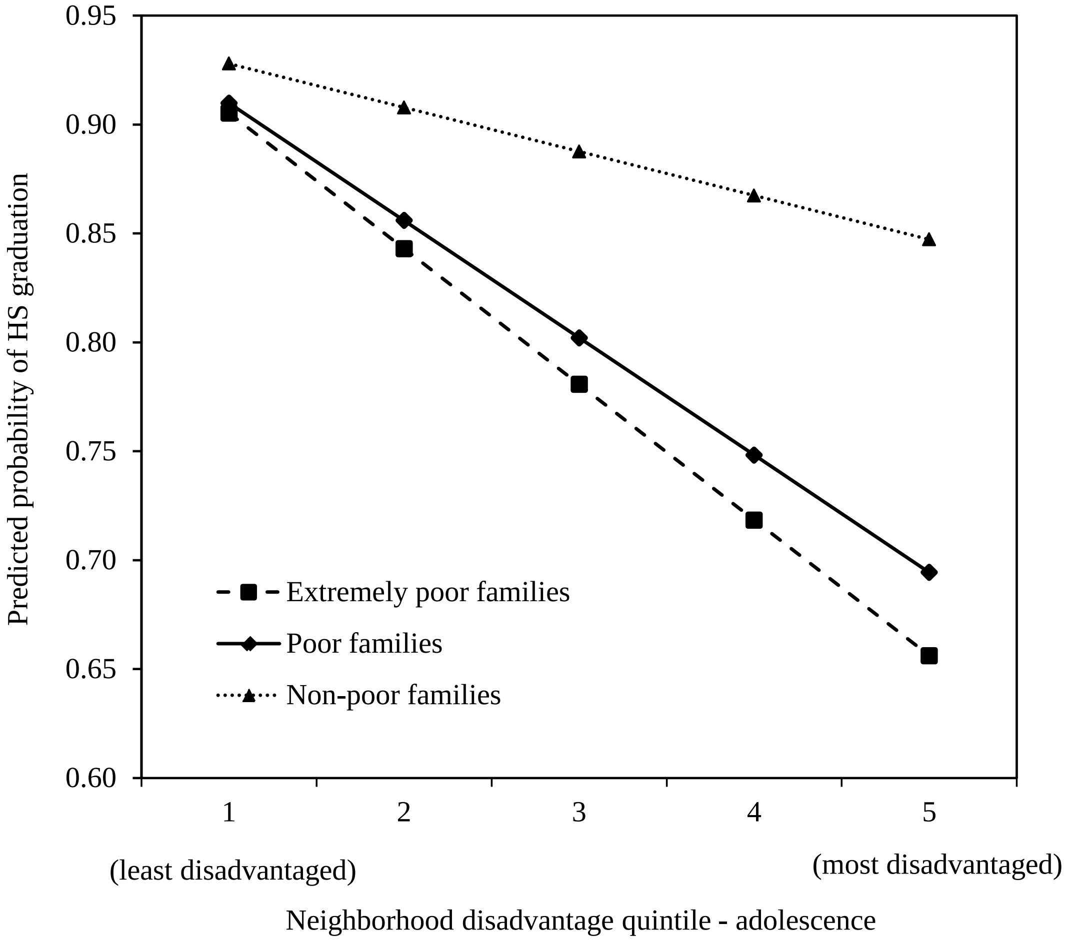
Separate effect estimates for blacks and nonblacks are reported in the second and third columns of Table 5. These estimates are comparable to those from the total sample, indicating that adolescent exposure to disadvantaged neighborhoods is highly consequential, while the impact of exposure earlier during childhood is minimal, and that effects are most severe for individuals living in poor families. Among blacks, exposure to the most disadvantage quintile of neighborhoods during adolescence, compared to the least disadvantaged quintile, is estimated to lower the probability of high school graduation by 25 percentage points for individuals whose families are extremely poor, by about 21 percentage points for individuals in families at the poverty line, and by only 8 percentage points for individuals in nonpoor families during adolescence.

Among nonblacks, estimates associated with adolescent neighborhood context are smaller and only marginally significant, but they too suggest harmful effects for disadvantaged neighborhoods during this developmental stage that are amplified by family resource deprivation. Specifically, adolescent exposure to the most disadvantaged quintile of neighborhoods, rather than the least disadvantaged quintile, is estimated to reduce the probability of high school graduation by about 10 percentage points for nonblacks in poor families and by about 5 percentage points for nonblacks in families with resources equivalent to three times the poverty line.

Figure 4 displays probabilities of high school graduation for blacks with different neighborhood and family resource histories computed from the SNMM estimates. The graph shows how the probability of high school graduation would be expected to change if black individuals that had lived in middle class, third quintile neighborhoods during childhood were later exposed to different levels of neighborhood disadvantage in adolescence. Estimates are plotted separately for subjects living in families that were extremely poor, poor, or nonpoor during both childhood and adolescence to demonstrate the substantial magnitude of effect moderation by the family income-to-needs ratio.

Results indicate that if blacks in both poor and extremely poor families had lived in third quintile neighborhoods during childhood and then moved to a neighborhood in the least disadvantaged quintile during adolescence, about 91 percent would have graduated high school by age 20. If, on the other hand, these same individuals had moved from third quintile neighborhoods in childhood to the most disadvantaged quintile of neighborhoods during adolescence, only an estimated 69 percent of poor children and 65 percent of extremely poor children would have graduated high school. For blacks living with nonpoor families, an estimated 93 percent would have graduated had they moved, between childhood and adolescence, from third quintile neighborhoods to neighborhoods in the least disadvantaged quintile. About 86 percent of nonpoor blacks would be expected to graduate had they instead moved to the most disadvantaged quintile of neighborhoods during adolescence.

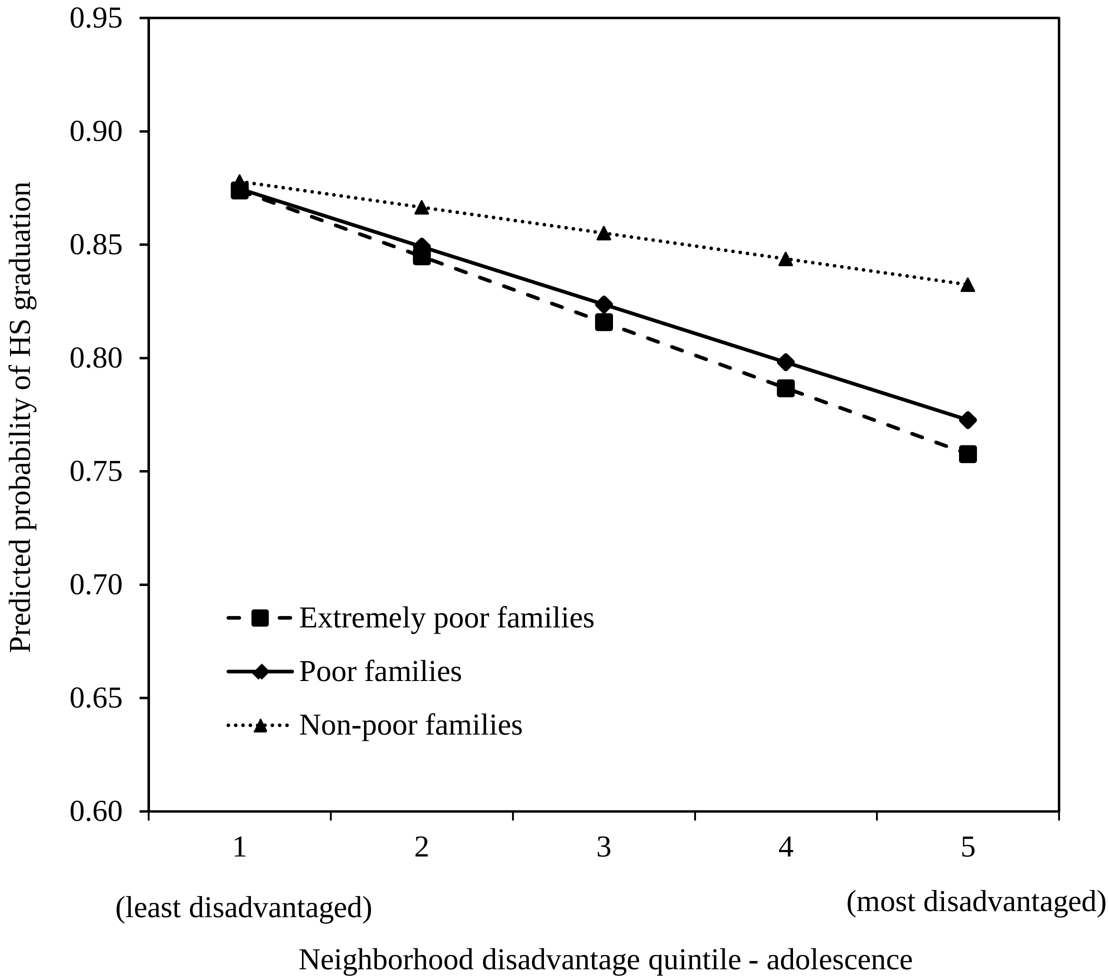
Figure 4. Predicted probability of high school graduation by adolescent exposure to neighborhood disadvantage and family poverty history, black respondents



Notes: Childhood treatment set to residence in a third quintile, or middle class, neighborhood.

Figure 5 displays predicted probabilities of high school graduation for nonblacks by neighborhood and family resource history. These estimates indicate that had nonblack individuals in poor and in nonpoor families been exposed to third quintile neighborhoods during childhood and then later moved to the least disadvantaged, first quintile of neighborhoods during adolescence, about 87 percent of both groups would be expected to graduate from high school. If, on the other hand, these individuals had moved to neighborhoods in the most disadvantaged quintile during adolescence, only an estimated 77 percent of nonblacks in poor families and 83 percent of those in nonpoor families would have graduated high school.

Figure 5. Predicted probability of high school graduation by adolescent exposure to neighborhood disadvantage and family poverty history, nonblack respondents



Notes: Childhood treatment set to residence in a third quintile, or middle class, neighborhood.

Robustness Analyses

Under assumptions of no unobserved confounding (i.e., sequential ignorability) and no model misspecification, the estimates presented above can be interpreted as average causal effects of neighborhood context among different subgroups of children defined by their time-varying family resource history. These assumptions, although less stringent than those required for conventional regression, are strong, and their violation would invalidate our inferences about the moderated effects of disadvantaged neighborhoods. First, if either the causal or nuisance functions of the SNMM were incorrectly specified, then our neighborhood-effect estimates would be biased. Experimentation with a wide variety of specifications for both the causal and nuisance functions, however, indicates that the reported estimates are quite robust (see Appendix B).

Second, if there are unmeasured factors that simultaneously affect neighborhood selection and the probability of high school graduation, then our estimates are biased due to unobserved confounding of neighborhood context. The assumption of no unobserved confounding is not directly testable, but we measure and adjust for an extensive set of putative confounders to mitigate this problem as much as possible. Furthermore, we investigate the sensitivity of our effect estimates to hypothetical unobserved confounding. Results from this formal sensitivity analysis, summarized in Appendix C, indicate that the magnitude of unobserved confounding would have to be unreasonably large to alter our inferences about the effects of adolescent exposure to disadvantaged neighborhoods. For example, among individuals in poor families, we find that the effect of adolescent neighborhood disadvantage remains statistically significant even in the extreme situation where unobserved confounding is assumed to be twice as strong as the amount of confounding due to all observed covariates.

Finally, because some subjects drop out of the PSID before end of follow-up, a nontrivial number are missing covariate and outcome data. To account for the uncertainty associated with missing information, we report combined estimates from multiply imputed data. But in addition, we also compute estimates using multiple imputation with deletion (von Hippel 2007), single regression imputation (Longford 2005), and complete case analysis to investigate whether our results are sensitive to different methods of missing data adjustment. Results indicate that neighborhood effect estimates are stable under different procedures for handling missing data (see Appendix D).

DISCUSSION

Research on the spatial dimensions of social stratification is central to understanding the reproduction of poverty and persistent educational inequality in America. Although the educational consequences of disadvantaged neighborhoods are extensively studied, previous research does not investigate whether neighborhood effects on high school graduation are moderated by the evolving state of a child's family environment or depend on the timing of exposure to different neighborhood contexts during childhood versus adolescence. This study integrates spatial, temporal, and developmental perspectives of the stratification process by analyzing how the effects of different neighborhood contextual trajectories on high school graduation are moderated by families' economic resources in childhood and adolescence.

Using novel methods that properly account for dynamic neighborhood selection, this study indicates that exposure to concentrated disadvantage, particularly during adolescence, has a strong negative effect on the chances of high school graduation, and it reveals that the consequences of adolescent exposure to disadvantaged neighborhoods are much more severe for individuals whose families are also economically disadvantaged. By contrast, the effect of neighborhood disadvantage is less pronounced among adolescents whose families live well above poverty level. Neighborhood effects are thus moderated, time-dependent contextual determinants of high school graduation.

Neighborhoods are important to "ecological" socialization models that describe how interconnected social contexts influence child development (Brooks-Gunn, Duncan, Klebanov, and Sealand 1993). This study demonstrates that such models must account for interactions between nested contexts, like the family environment and neighborhood. Specifically, the evidence presented in this study is consistent with the compound disadvantage theory of neighborhood effect moderation, which contends that children in poor families are especially vulnerable to the harmful effects of living in disadvantaged neighborhoods. Results indicate that family resource deprivation greatly exacerbates the educational consequences of neighborhood deprivation. The "truly disadvantaged," in this sense, are children simultaneously embedded in impoverished families and impoverished neighborhoods, consistent with Wilson's (1987) seminal arguments about spatially concentrated poverty.

In addition to neighborhood effect moderation by family economic resources, this study shows that it is essential to account for the longitudinal sequence of neighborhood contexts experienced by individuals throughout the course of development. While previous research documents the importance of duration of exposure to different neighborhood conditions (Crowder and South 2010; Wodtke, Harding, and Elwert 2011), results presented here elaborate these findings by demonstrating that neighborhood effects on high school graduation also depend on the timing of exposure during childhood versus adolescence. Point estimates indicate that exposure at both developmental periods reduces the probability of high school graduation, but the effects of adolescent exposure are considerably larger and highly significant. These findings add to the growing body of research indicating that neighborhood effects should be studied within a longitudinal and developmental framework (Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011).

Investigating contextual effects within a temporal framework, however, requires new methods that overcome critical problems with conventional regression analyses. Because time-varying characteristics of the family environment are simultaneously mediators for the effect of past neighborhood conditions and confounders for effect of future neighborhoods, conventional regression estimators that condition on these covariates are biased due to over-control of intermediate pathways and collider stratification. Several recent studies use inverse probability of treatment weighting (Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011), a method that avoids the problems with conventional regression, to estimate *marginal, or population average,* effects of neighborhood context. But this approach is not designed for analyses of *conditional, or moderated,* neighborhood effects among subgroups of children defined by time-varying family covariates. We use the SNMM and two-stage regression-with-residuals estimator (Almirall, McCaffrey, Ramchand, and Murphy 2011; Almirall, Ten Have, and Murphy 2010) to analyze neighborhood effect moderation by family economic resources, a time-varying attribute that simultaneously confounds, mediates, and moderates effects of past and future neighborhood exposures. The two-stage estimator is unbiased for moderated neighborhood effects under a weaker set of assumptions than is required for conventional regression, and analyses of potential violations of these assumptions indicate that our results are robust. The methods used in this study can be easily adapted for time-varying subgroup analyses of other contextual effects, such as the impact of school or firm characteristics.

Although this study extends previous work on the temporal dimensions of neighborhood effects, it is not without limitations. First, because the requisite data is unavailable for our sample from the PSID, we are not able investigate the specific mechanisms, such as school quality or environmental health hazards, through which structural neighborhood characteristics impact children's education progress. An important task for future research is to conduct mediation analyses of neighborhood effects within an appropriate temporal framework. Second, this study only examines a single educational outcome, high school graduation, measured during early adulthood. Because of the timing of this particular school transition, our conclusions about the importance of adolescent versus childhood exposure to disadvantaged neighborhoods should not be extrapolated to other educational outcomes. Residence in disadvantaged neighborhoods earlier rather than later in life likely has different consequences depending on the educational outcome of interest. To better understand how neighborhood effects depend on timing of exposure, future research should examine a variety of outcomes related to school progression and achievement measured throughout the early life course.

These limitations notwithstanding, the present study provides important new evidence about temporal dependency and family resource moderation of neighborhood effects on high school graduation. With both income inequality and income segregation increasing in America

(Reardon and Bischoff 2011), the devastating impact of spatially concentrated disadvantage on high school graduation for children from poor families suggests that these broad social trends are mutually reinforcing: income inequality begets income segregation, and income segregation facilitates the reproduction of poverty.

END NOTES

1. Due to measurement limitations in the PSID, subjects with a general equivalency degree (GED) are coded as high school graduates. In addition, for subjects with item-specific missing data on educational attainment at age 20, we use measures of educational attainment taken at age 21, if available, to construct our indicator of high school graduation.

2. Classifying neighborhoods into quintiles of the composite disadvantage distribution results in some information loss about neighborhood context. Measurement error in treatment would be particularly concerning if it were linked to the moderating variable of interest, family poverty, because this might lead to inappropriate inferences about the degree of effect moderation. To investigate this issue, we also conduct analyses with the raw disadvantage index scores. Results (not reported) are very similar to those based on the quintile treatment definition. Because the quintile classification of neighborhoods greatly simplifies notation in the counterfactual model and facilitates a clean interpretation of effect estimates, we report results based on this treatment definition.

3. Parental education is treated as time-invariant because the PSID does not measure this factor at regular intervals, thereby limiting our ability to track changes over time. We use measurements of parental education taken when a child is age 2 or, if that is not available, the most recent measurement prior to age 2.

4. Some sample members leave the PSID before age 20 and thus are missing data for the outcome and covariates measured after their departure from the study. In addition to sample attrition, a small amount of data is missing due to item-specific nonresponse. Multiple imputation replaces missing data with $m > 1$ values that are simulated from an imputation model. Separate estimates are computed for each of the m complete datasets and then combined to account for the uncertainty associated with missing information. The combined estimates reported in this study are based on $m = 100$ datasets.

5. In addition to reducing measurement error and accounting for duration of exposure, we measure neighborhood context and time-varying covariates using adjacent four-year averages to make the investigation of moderated neighborhood effects analytically tractable. Because the PSID provides annual measures of neighborhood context and time-varying covariates, we could, at least in theory, analyze the effects of different neighborhood exposures at every age during childhood and adolescence, conditional on family resource history through each time point. However, with separate annual measures, analyzing moderated neighborhood effects would require estimating millions of parameters, which is impossible with extant data. Thus, we divide the early life course into two developmental stages and measure key variables at each stage in a way that ensures unambiguous temporal ordering of the treatment, moderator, and confounders.

6. When defining the moderated effects of neighborhood disadvantage during childhood, the analyst chooses the value to which adolescent treatment is set. We set adolescent treatment to residence in the least disadvantaged quintile of neighborhoods and define $u_1(L_1, a_1)$ as $E(Y(a_1, 1) - Y(1,1)|L_1)$ for two reasons. First, the resulting contrasts between childhood exposure sequences $\{(2,1), \ldots, (5,1)\}$ and sustained exposure to the least disadvantaged quintile of neighborhoods $(1,1)$ are of key theoretical interest, and second, this formulation of $u_1(L_1, a_1)$ simplifies parameterization and interpretation of the causal function for adolescent neighborhood context. Note that different childhood treatment contrasts with, for example, adolescent treatment set to residence in a third quintile neighborhood, can be obtained from more complex combinations of model parameters.

7. Models that permit the effect of adolescent neighborhood disadvantage to be moderated by and the childhood measurements of neighborhood context and the income-to-needs ratio are also considered in supplemental analyses (see Table B.1 in Appendix B). There is no evidence that the effect of later exposure to disadvantaged neighborhoods during adolescence is moderated by neighborhood context or the family income-to-needs ratio measured earlier during childhood. Thus, we focus on the more parsimonious parameterization of adolescent neighborhood effects in Equation 2.

8. At this juncture, readers may wonder about other estimands that appear to represent moderated neighborhood effects but are not investigated in this study. For example, consider the question, "what is the effect of continuously living in the most disadvantaged quintile of neighborhoods, rather than the least disadvantaged quintile, among subjects whose families stay poor throughout the study?" This question, translated into counterfactual notation, is expressed as $E\big(Y(5,5) - Y(1,1)|L_1 = 0, L_2 = 0\big)$. While this contrast may appear reasonable at first, it is not, in fact, a well-defined causal comparison: subjects whose families would stay poor had they continuously been exposed to the most disadvantaged neighborhoods and subjects whose families would stay poor had they been continuously exposed to the least disadvantaged neighborhoods are not the same set of individuals, and this comparison is therefore nonsensical because counterfactuals must compare the same subjects. Alternatively, consider the question, "what is the effect of continuously living in the most disadvantaged quintile of neighborhoods, rather than the least disadvantaged quintile, *among subjects whose families would stay poor regardless of treatment received*?" Translated into counterfactual notation, this question is expressed as $E\big(Y(5,5) - Y(1,1)|L_1 = 0, L_2(5) = L_2(1) = 0\big)$. This is a well-defined causal contrast, but it cannot be identified because it is impossible to determine which families would stay poor regardless of treatment. Furthermore, it is unclear whether this effect is even a substantively interesting quantity, since it involves an unobservable subpopulation for which one of the mechanisms thought to mediate neighborhood effects is inoperative. By contrast, $u_1(L_1, a_1)$ and $u_2(L_2(a_1), a_2)$ are well-defined, identified under defensible assumptions, and substantively interesting in that they directly evaluate theories about compound disadvantage, relative deprivation, and developmental timing of neighborhood exposures.

9. Equation 3 is a parametric linear probability SNMM. The decomposition of the conditional mean on which the SNMM is based does not hold in nonlinear models, such as logit or probit regressions. While nonparametric linear probability, logit, and probit models are basically equivalent, parametric linear probability models can be problematic because they allow fitted values outside the logical $[0,1]$ range. We find that this model provides a reasonable fit to the data without nonsensical predicted probabilities. In addition, models that relax the parametric restrictions in Equation 3 do not substantially alter the treatment-effect estimates of interest (see Appendix B).

10. In observational studies of neighborhood effects where time-varying confounders are affected by past neighborhood conditions, inverse probability of treatment (IPT) weighting can be used to estimate marginal, or population average, effects (Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011). However, this approach precludes conditioning on time-

varying covariates and thus does not permit analyses of moderated neighborhood effects when the moderator of interest varies across time, as in the present study. IPT weighting can be used to analyze effect moderation by baseline covariates only (Robins 1999b; Robins, Hernan, and Brumback 2000).

11. Bootstrapping is a method for estimating the variance of a sample statistic by resampling with replacement from the observed data. To compute bootstrap standard errors, we draw $b = 2{,}000$ random samples of equal size to the observed data, apply the two-stage estimation procedure to each sample, and store the results. Then, the standard deviation of the 2,000 separate estimates obtained from this procedure gives the bootstrap estimate of the standard error. Hypothesis testing and p-values are based on a standard normal approximation.

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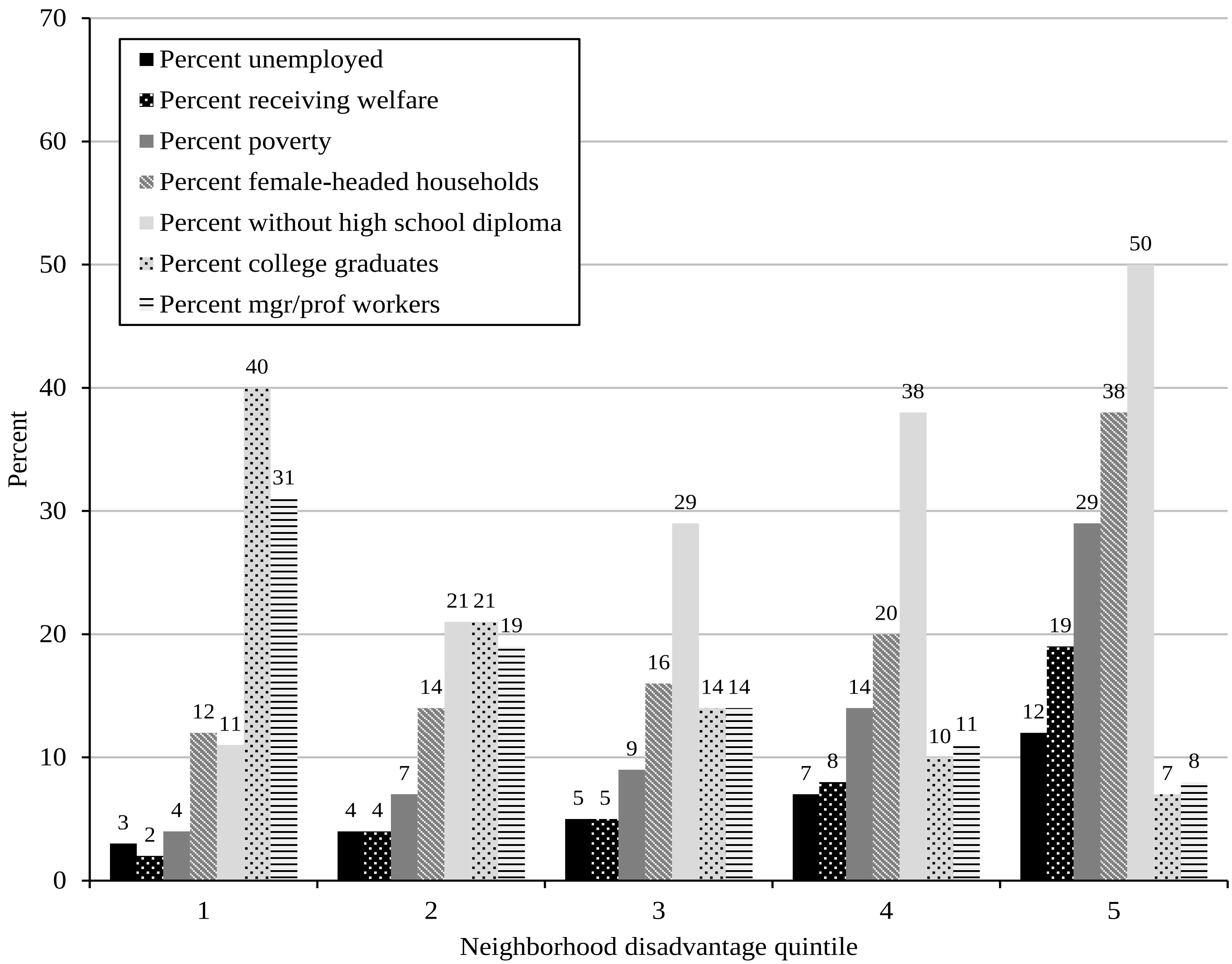
APPENDIX A. NEIGHBORHOOD DISADVANTAGE INDEX

Table A.1 Principal component weights and correlations

Variable	1st PC	
	Weight	Corr
Percent poverty	.408	.861
Percent unemployed	.371	.783
Percent receiving welfare	.412	.868
Percent female-headed households	.337	.711
Percent without high school diploma	.378	.798
Percent college graduates	–.348	–.735
Percent mgr/prof workers	–.385	–.812
Component variance	4.449	
Proportion total variance explained	.636	

Notes: Principal component analysis based on correlation matrix. Analysis includes all tract-year observations from the 1970 to 2000 U.S. censuses.

Figure A.1. Neighborhood socioeconomic characteristics by disadvantage index quintile



APPENDIX B. MODEL SPECIFICATION

This appendix investigates the sensitivity of our estimates to different specifications of the causal and nuisance functions of the SNMM. Table B.1 presents two-stage estimates for models that allow the effect of neighborhood disadvantage to vary across not only family economic resources but also all other family-level covariates as well as prior neighborhood context. Model A is the base model reported in the main text of the paper. Model B extends the base model by including a cross-product term between childhood and adolescent neighborhood context. This model provides no evidence that the effect of later exposure to disadvantaged neighborhoods is moderated by earlier neighborhood conditions. Models C and D allow the effect of neighborhood context to vary across a variety of different family-level covariates, including parental education, marital status, and employment status, in addition to family resources. These models do not reveal any significant interactions between neighborhood context and family covariates apart from the income-to-needs ratio, and point estimates of the focal moderated neighborhood effects are highly stable across the different specifications of the SNMM causal functions considered here. Thus, these analyses indicate that the causal functions of our base model are well specified.

Tables B.2 and B.3 present two-stage estimates of SNMMs that have the same causal functions but use many different specifications for the nuisance functions. In Table B.2, Model A is the base model reported in the main text of the paper. The nuisance functions in this model include "main effects" for time-invariant covariates, here denoted by V; time-varying family covariates measured during childhood, L_1; and time-varying family covariates measured during adolescence, L_2. Models B, C, and D use progressively more complex specifications for the nuisance functions, including all two-way interactions between different time-invariant covariates, between different time-varying covariates measured during childhood, and between time-varying covariates measured during adolescence. Table B.3 shows estimates based on nuisance functions with all two-way interactions between different time-varying covariates measured during childhood and adolescence, as well as cross-time interactions between these covariates. Estimates of the moderated neighborhood effects of interest are relatively invariant and remain highly significant across all different specifications for the SNMM nuisance functions.

Table B.1. Two-stage estimates with different specifications of SNMM causal functions

Model	A (base)			B			C			D		
	coef	se		coef	se		coef	se		coef	se	
Intercept	.888	(.021)	***	.892	(.025)	***	.880	(.024)	***	.882	(.024)	***
Childhood												
NH dadvg	–.005	(.012)		–.002	(.015)		.022	(.028)		.027	(.034)	
NH dadvg x inc-to-needs	.005	(.004)		.001	(.006)		.007	(.005)		.008	(.005)	
NH dadvg x H-less than HS							–.010	(.021)		–.007	(.021)	
NH dadvg x H-some college							–.016	(.019)		–.012	(.020)	
NH dadvg x H-married							.005	(.018)		.003	(.019)	
NH dadvg x H-employed							–.034	(.027)		–.034	(.026)	
NH dadvg x H-homeowner							.010	(.013)		.003	(.014)	
NH dadvg x family size										.004	(.004)	
NH dadvg x num. moves										–.003	(.005)	
Adolesence												
NH dadvg	–.042	(.010)	***	–.047	(.017)	**	–.044	(.023)	†	–.047	(.030)	
NH dadvg x inc-to-needs	.012	(.003)	***	.010	(.003)	**	.011	(.007)	**	.010	(.004)	**
NH dadvg x H-less than HS							.003	(.018)		.001	(.018)	
NH dadvg x H-some college							.005	(.017)		.004	(.017)	
NH dadvg x H-married							.002	(.013)		.009	(.014)	
NH dadvg x H-employed							–.008	(.020)		–.009	(.020)	
NH dadvg x H-homeowner							.009	(.012)		.010	(.012)	
NH dadvg x family size										–.005	(.004)	
NH dadvg x num. moves										.000	(.005)	
Chld inc-to-needs x Adl NH dadvg				.007	(.007)							
Chld x Adl NH dadvg				.000	(.004)							

Notes: Results are combined estimates from 100 multiple imputation datasets. Standard errors are based on 2000 bootstrap samples.
†$p < 0.10$, *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ for two-sided tests of no effect.

Table B.2. Two-stage estimates with different specifications of SNMM nuisance functions

Model	A (base)			B			C			D		
	coef	se		coef	se		coef	se		coef	se	
Intercept	.888	(.021)		.886	(.021)		.879	(.021)		.876	(.021)	
Childhood												
NH dadvg	–.005	(.012)		–.005	(.012)		.000	(.012)		–.005	(.012)	
NH dadvg x inc-to-needs	.005	(.004)		.006	(.004)		.002	(.004)		.005	(.004)	
Adolesence												
NH dadvg	–.042	(.010)	***	–.042	(.010)	***	–.041	(.010)	***	–.033	(.011)	**
NH dadvg x inc-to-needs	.012	(.003)	***	.013	(.003)	***	.012	(.003)	***	.007	(.003)	*
Description												
Num. of 2nd stage parameters	25			39			69			99		
Nuissance functions	main effects for V, L_1 and L_2			A + all two-way interactions btw elements of V			B + all two-way interactions btw V and L_1			C + all two-way interactions btw V and L_2		

Notes: Results are combined estimates from 100 multiple imputation datasets. Standard errors are based on 2000 bootstrap samples. For this analysis, V denotes time-invariant baseline characteristics. L_1 and L_2 denote time-varying factors measured during childhood and adolescence, respectively.
†$p < 0.10$, *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ for two-sided tests of no effect.

Table B.3. Two-stage estimates with different specifications of SNMM nuisance functions continued

Model	E coef	E se		F coef	F se		G coef	G se	
Intercept	.882	(.021)		.883	(.021)		.882	(.021)	
Childhood									
NH dadvg	–.001	(.012)		–.006	(.012)		–.006	(.012)	
NH dadvg x inc-to-needs	.002	(.004)		.005	(.005)		.005	(.005)	
Adolesence									
NH dadvg	–.041	(.010)	***	–.037	(.011)	***	–.035	(.011)	**
NH dadvg x inc-to-needs	.012	(.003)	***	.009	(.003)	**	.008	(.004)	*
Description									
Num. of 2nd stage parameters	40			55			91		
Nuissance functions	A + all two-way interactions btw elements of L_1			E + all two-way interactions btw elements of L_2			F + all two-way interactions btw L_1 and L_2		

Notes: Results are combined estimates from 100 multiple imputation datasets. Standard errors are based on 2000 bootstrap samples.
†$p < 0.10$, *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ for two-sided tests of no effect.

APPENDIX C. SENSITIVITY ANALYSIS

In this section, we implement a formal sensitivity analysis to test the robustness of our estimates to unobserved confounding, a violation of the sequential ignorability assumption. Unobserved confounding would occur if families select different neighborhood contexts on the basis of unmeasured factors that affect the chances of high school graduation. We consider unobserved confounding of the following type: children currently living in more disadvantaged neighborhoods may have lower graduation rates regardless of where they live, while children currently living in less disadvantaged neighborhoods may have higher graduation rates regardless of neighborhood context. This may occur because subjects living in disadvantaged neighborhoods, compared to those living in more affluent communities, have parents that are less "ambitious" or "skilled" when it comes to raising children or because they come from families with less accumulated wealth. Since we lack reasonable measures of parental skill or ambition, as well as family wealth, our neighborhood-effect estimates would be downwardly biased if these characteristics are in fact confounders, indicating a negative impact of concentrated disadvantage even if there is no such effect.

Following Sharkey and Elwert (2011), we implement a sensitivity analysis for time-varying neighborhood treatments that models bias due to unobserved confounding as a function of potential outcomes (Brumback, Hernan, Haneuse, and Robins 2004; Robins 1999a; Robins 1999b). With this approach, a selection function is used to summarize the relationship between observed and counterfactual outcomes and then to compute bias-adjusted effect estimates. If inferences about the negative effect of neighborhood disadvantage on high school graduation do not change across a range of substantively reasonable confounding scenarios, as defined by different values of the selection function, we conclude that our results are robust to unobserved confounding.

To illustrate the logic behind this type of sensitivity analysis, consider a point-in-time experiment that randomized a sample of families and their children to neighborhoods in each of the five quintiles of the disadvantage distribution. Table C.1 provides a cross-tabulation of the potential outcomes for this hypothetical experiment. The main diagonal cells give the observed proportion of high school graduates in neighborhood quintile a for subjects that were in fact assigned to a neighborhood in quintile $A = a$ of the composite disadvantage distribution. The off-diagonal cells in parentheses are unobserved, or counterfactual, graduation rates. For example, cell (EE) is the graduation rate in the most disadvantaged quintile of neighborhoods for individuals actually assigned to live in the least disadvantaged quintile of neighborhoods. In an optimal randomized experiment, the potential outcome for a given treatment is the same for members of all treatment groups, such that the observed and counterfactual graduation rates are equal within columns: $E = (F) = (G) = (H) = (I)$, $(J) = K = \cdots = (N)$, and so on. In other words, with perfect randomization, the observed mean potential outcome among subjects living in quintile a should equal the unobserved mean potential outcome of living in quintile a among subjects randomized to live in some other quintile a'. If the probability of high school graduation is a linear function of neighborhood disadvantage, a regression of the observed outcome, Y, on the treatment variable, A, would provide a valid estimate of the average causal effect of neighborhood disadvantage.

In this framework, unobserved confounding can be thought of as a departure from perfect randomization of neighborhood context. Specifically, bias due to unobserved confounding occurs if $E \neq (F) \neq \cdots \neq (I)$, $(J) \neq K \neq \cdots \neq (N)$, and so on. That is, if the observed mean outcome in one treatment group is not exchangeable with the counterfactual mean outcome in the other treatment groups, estimates are biased due to unobserved confounding. Based on this relationship between observed mean outcomes and counterfactual mean outcomes, unobserved confounding can be summarized by the following parsimonious selection function,

$$s(a, a') = E(Y(a)|A = a) - E(Y(a)|A = a'), \tag{6}$$

where, for example, $s(0,1) = E - (F)$. Different values of $s(a, a')$ correspond to varying types and degrees of unobserved confounding.

For the present analysis, we adopt one particular specification of the selection function: $s(a, a') = (a - a')\alpha$, where $\alpha \leq 0$ is a sensitivity parameter that specifies the magnitude of bias due to unobserved confounding. We use a linear model for unmeasured confounding because our empirical analysis of neighborhood effects is focused on estimating the parameters of a linear SNMM. In this model, $\alpha = 0$ implies no unobserved confounding of neighborhood context, and $\alpha < 0$ defines the type of confounding described previously: children currently living in more disadvantaged neighborhoods have lower graduation rates regardless of where they live, and children currently living in less disadvantaged neighborhoods have higher graduation rates regardless of neighborhood context. Note that this model constrains the magnitude of unobserved confounding to be the same across levels of observed covariates and moderators, that is, we assume a uniform unobserved selection process for all subgroups in the analysis.

Based on this selection function, a bias-adjusted estimate for the average treatment effect in the point-in-time context can be obtained from the following calculations. First, we compute the proportion of subjects in each neighborhood quintile, denoted by $P(A)$ for $A = 1,2, \ldots ,5$. Second, we subtract the bias term, $B = \sum_{A'=1}^{A'=5} (A - A')\alpha\, P(A')$, from the observed outcome, Y, to obtain a bias-adjusted outcome $Y^c = Y - B$. Finally, we estimate a bias-adjusted treatment effect by regressing the corrected outcome, Y^c, on the treatment variable, A. By selecting a range of plausible values for the sensitivity parameter, α, and estimating bias-adjusted effects for each of those values, we are able to assess the robustness of our results to different degrees of unobserved confounding.

For the present analysis where treatment is time-varying, separate selection functions, $s_k(a_k, a'_k)$, are used to model unobserved confounding in childhood ($k = 1$) and adolescence ($k = 2$). The formula for the bias term is modified to account for the total bias accumulated across developmental periods, $B = \sum_{k=1}^{k=2} \sum_{A'_k=1}^{A'_k=5} (A_k - A'_k)\alpha_k\, P(A'_k)$, and then the adjusted outcomes are computed as above. To incorporate effect moderation and controls for observed confounders, we simply refit the SNMM using the adjusted outcomes, and this yields bias-adjusted estimates for the impact of neighborhood disadvantage on high school graduation. Following Sharkey and Elwert (2011), the sensitivity parameter, α_k, is calibrated such that a one unit change corresponds to the amount of bias eliminated from our main effect estimates in the childhood and adolescent causal functions after adjusting for all observed confounders. The sensitivity of neighborhood effect estimates is thus interpreted in terms of multiples of observed confounding bias.

Figures C.1 and C.2 display the results from this sensitivity analysis for the effects of childhood and adolescent exposure to neighborhood disadvantage, respectively. In both figures, separate bias-adjusted effect estimates are presented for children in poor and in nonpoor families. The value of the sensitivity parameter, α, is plotted on the horizontal axis. A value of $\alpha = 0$ indicates no unobserved confounding and simply reproduces the estimates reported in Table 5. For $\alpha = -1$, unobserved factors are assumed to confound the effect of neighborhood disadvantage on high school graduation to the same extent as all observed covariates already controlled for in the regression, including race, parental education, marital status, employment status, family structure, and so on. Because we adjust for a large and relevant set of observed confounders, we judge values of $\alpha < -1$ to be implausible unobserved confounding scenarios.

The results of this sensitivity analysis indicate that our estimates and main substantive conclusions are robust to unobserved confounding: across a wide range of values for α, we conclude that the direct effect of exposure to disadvantaged neighborhoods during childhood is small and not statistically significant for both poor and nonpoor children, while the effect of adolescent neighborhood disadvantage is severe and remains statistically significant under a moderate degree of unobserved confounding ($\alpha > -.5$) for nonpoor children and under a high degree of unobserved confounding ($\alpha > -1$) for poor children. Even in the most extreme situation where unobserved confounding is assumed to be twice as strong as that already controlled for through observed covariates ($\alpha = -2$), the negative effect of neighborhood disadvantage on high school graduation among children in poor families remains substantively large and statistically significant. Thus, based on the results in Figures C.1 and C.2, we conclude that the neighborhood effect estimates presented in Table 5 are highly robust to unobserved confounding.

Table C.1. Potential outcomes from hypothetical neighborhood experiment

Observed	Mean Potential Outcome, $E(Y(a)\|A)$				
Treatment	$E(Y(1)\|A)$	$E(Y(2)\|A)$	$E(Y(3)\|A)$	$E(Y(4)\|A)$	$E(Y(5)\|A)$
$A = 1$	E	(J)	(O)	(T)	(EE)
$A = 2$	(F)	K	(P)	(U)	(FF)
$A = 3$	(G)	(L)	Q	(V)	(GG)
$A = 4$	(H)	(M)	(R)	W	(HH)
$A = 5$	(I)	(N)	(S)	(X)	II

Figure C.1. Sensitivity of direct effect estimates for childhood neighborhood disadvantage to hypothetical unobserved confounding

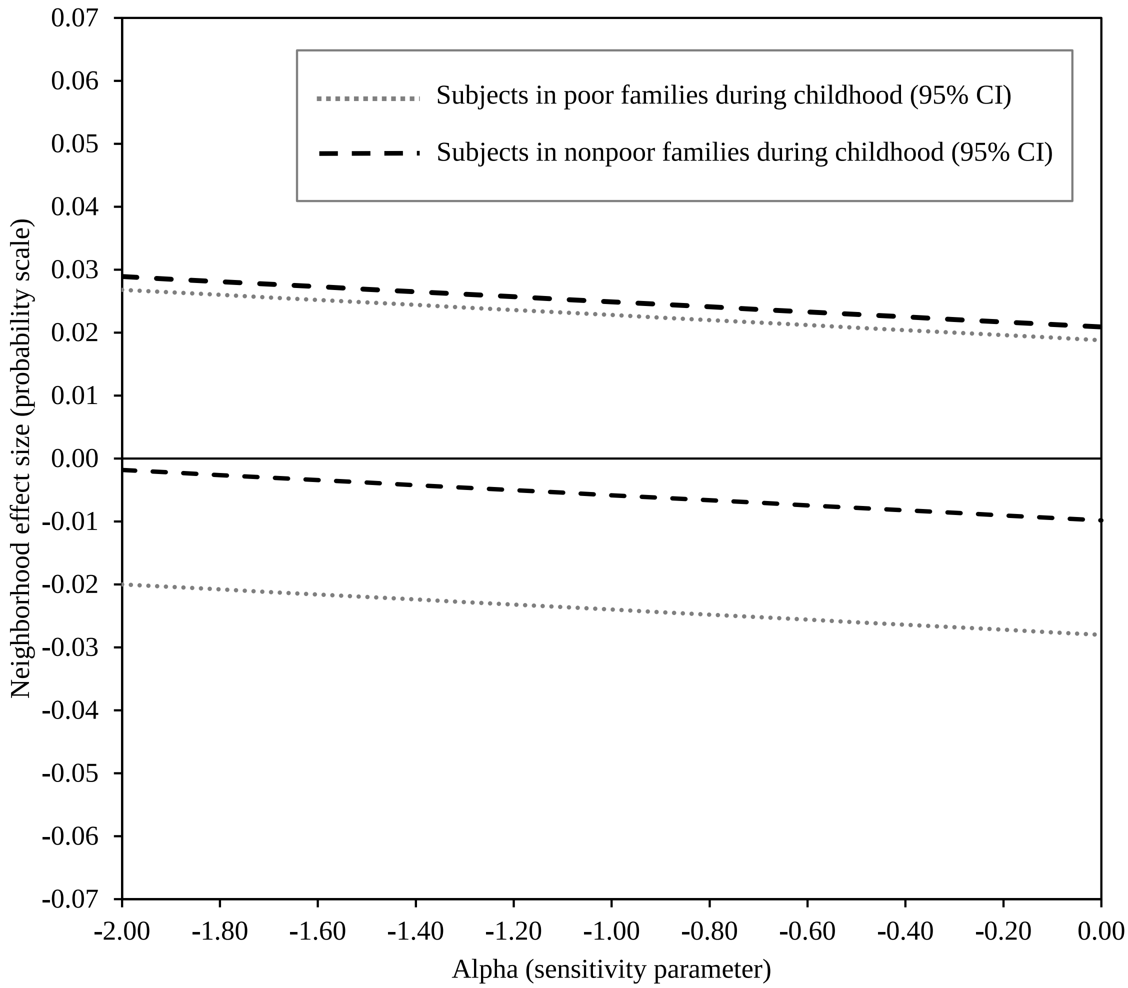
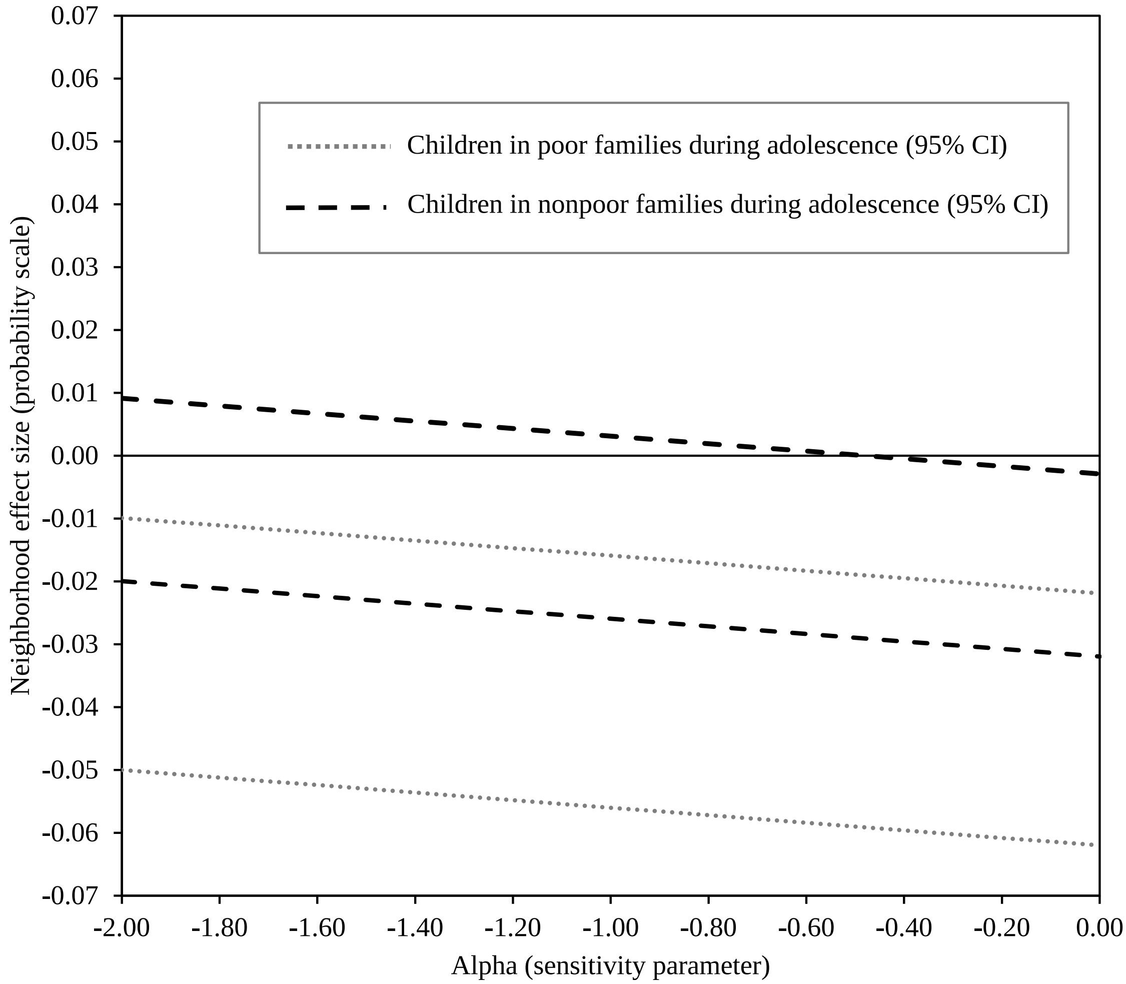


Figure C.2. Sensitivity of effect estimates for adolescent neighborhood disadvantage to hypothetical unobserved confounding



APPENDIX D. MISSING DATA ADJUSTMENTS

The analyses in this study suffer from a nontrivial amount missing data, summarized in Tables 1 and 2 of the main text, due primarily to attrition from the PSID. This appendix investigates whether results change considerably using different methods of adjustment for missing data. The first column of Table D.1 shows the combined estimates reported in the main text from 100 multiple imputation (MI) samples. Under the assumption that data are "missing at random" (MAR), specifically, that conditional on observed covariates, the mechanism governing missingness does not depend on unobserved factors that also affect the variable in question, combined effect estimates and standard errors based on MI are unbiased and valid, respectively. The second column contains combined estimates based on multiple imputation with deletion (MID), a procedure where all missing data are multiply imputed but cases with missing values for the outcome variable are deleted prior to estimation (von Hippel 2007). This approach offers greater statistical efficiency than MI but requires more stringent assumptions about the missing data mechanism. The third column contains estimates based on single regression imputation (SI) for which missing values are replaced with the conditional sample mean. Under the MAR assumption, this procedure yields unbiased effect estimates but understates their variance. Finally, the last column presents estimates from a complete case analysis (CC) that simply deletes all observations with any missing data. This procedure is unbiased only if data are "missing completely at random" (MCAR), that is, only if the mechanism governing missingness does not depend on observed or unobserved factors that affect the variable in question. Combined estimates based on conventional MI are virtually identical to those obtained from MID and are similar to those from SI and CC. We report MI estimates because this approach requires the weakest assumptions for unbiased estimation and valid inference.

Table D.1. Two-stage estimates under different methods of adjusting for missing data/sample attrition

Model	MI (base)			MID			SI			CC		
	coef	se		coef	se		coef	se		coef	se	
Intercept	.888	(.021)	***	.906	(.018)	***	.896	(.014)	***	.915	(.019)	***
Childhood												
NH dadvg	–.005	(.012)		–.008	(.012)		.006	(.008)		–.004	(.013)	
NH dadvg x inc-to-needs	.005	(.004)		.007	(.004)		.001	(.003)		.007	(.005)	
Adolesence												
NH dadvg	–.042	(.010)	***	–.040	(.010)	***	–.055	(.007)	***	–.051	(.011)	***
NH dadvg x inc-to-needs	.012	(.003)	***	.011	(.003)	***	.016	(.002)	***	.014	(.004)	***
Description												
Num. of observations	6135			3500			6135			2626		
Num. of replications	100			100			1			0		

Notes: MI = multiple imputation, MID = multiple imputation then deletion, SI = single imputation, and CC = complete case analysis. Standard errors are based on 2000 bootstrap samples.

†$p < 0.10$, *$p < 0.05$, **$p < 0.01$, and ***$p < 0.001$ for two-sided tests of no effect.

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