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 educational impact of disadvantaged neighborhoods depends on the evolving socioeconomic position of the family 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 properly measure and analyze the sequence of neighborhood conditions experienced by children throughout the early life course. This study extends research on neighborhood effects by investigating how different longitudinal patterns of exposure to disadvantaged neighborhoods during childhood and adolescence impact high school graduation and whether the effects of different exposure patterns are moderated by family poverty. Results based on novel counterfactual methods for time-varying treatments and effect moderators—the structural nested mean model and two-stage regression-with-residuals estimator—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 income inequality and income segregation are mutually reinforcing: income inequality begets income segregation, and income segregation facilitates the reproduction of poverty.

KEYWORDS

neighborhoods, high school graduation, effect moderation, structural nested mean models

Since the publication of Wilson's (1987) influential treatise on urban poverty, researchers have worked to better understand the spatial dimensions of stratification processes, where the impact of concentrated neighborhood disadvantage on educational attainment has been of particular interest. 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 prior empirical research is mixed, with some studies reporting no effects of neighborhood context on educational attainment (e.g., Ginther, Haveman, and Wolfe 2000) and others finding only small effects (e.g., Aaronson 1998; Brooks-Gunn, Duncan, Klebanov, and Sealand 1993; Crane 1991; Harding 2003), more recent research documents strong effects linked to the neighborhood environment (Crowder and South 2010; Wodtke, Harding, and Elwert 2011).

Few studies, however, investigate how the neighborhood environment interacts with other aspects of a child's social life. In particular, prior research typically assumes that the effects of neighborhood context are the same for all children, regardless of the economic resources at the disposal of their families (e.g., Ginther, Haveman, and Wolfe 2000; Harding 2003; Wodtke, Harding, and Elwert 2011). Yet several theories indicate that the socioeconomic position of the family should moderate the impact of neighborhood context. 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 local networks, adults, and institutional resources than children from nonpoor families (Jencks and Mayer 1990; Wilson 1987; Wilson 1996). By contrast, the relative deprivation perspective 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 nonpoor 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.

In addition to heterogeneity by family resources, the consequences of living in disadvantaged neighborhoods also likely depend on the timing of exposure during the course of development. The neighborhood environment is not a static feature of a child's life: families move and communities change, exposing many children to different neighborhood conditions throughout the course of development (Quillian 2003; Timberlake 2007). 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 heterogeneous effects of neighborhood deprivation during different development periods. If neighborhood effects are lagged or are different during childhood versus adolescence, then previous studies provide an incomplete assessment of the developmental process through which neighborhoods impact children.

This study investigates neighborhood-effect heterogeneity by family economic resources and child developmental stage. 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 heterogeneity are complicated by several difficult methodological problems. First, selection into different neighborhood contexts across time is partly based on characteristics of the family environment, such as parental income and family size, that are themselves time-varying and likely affected by prior neighborhood conditions. Because neighborhood selection processes are dynamic, conventional regression models cannot consistently estimate effects of disadvantaged neighborhoods (Wodtke, Harding, and Elwert 2011). Second, time-varying family characteristics not only confound and mediate the effect of disadvantage neighborhoods but they also define the subgroups of children under consideration. That is, the economic resources available to a child's family vary over time and are thought to simultaneously confound, mediate, and moderate the effect of neighborhood disadvantage on educational outcomes. Because family poverty is time-varying and affected by prior neighborhood conditions, inverse probability of treatment weighting—an estimation method for *marginal* effects used in recent studies to overcome the dynamic neighborhood selection problem outlined above (Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011)—is also unable to recover the *conditional, or moderated,* effects of interest in the present study.

To overcome these problems, we use the structural nested mean model and two-stage regression-with-residuals estimator to assess the effects of different neighborhood exposure trajectories conditional on the evolving economic position of the family (Almirall, Ten Have, and Murphy 2010; Almirall, McCaffrey, Ramchand, and Murphy 2011; 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 and adolescence for different subgroups of children defined in terms of their family poverty history. We begin with a brief review of the theoretical mechanisms through which neighborhood poverty is thought to impact high school graduation, focusing on the importance of different longitudinal exposure sequences. Next, we discuss several theories positing that the effects of neighborhood disadvantage depend on the economic position of the family and outline the dynamic neighborhood selection process that complicates conventional regression analyses. Then, we present the structural nested mean model and two-stage regression estimator and, with data from the Panel Study of Income Dynamics, estimate the moderated effects of exposure to disadvantaged neighborhoods 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 children whose families are poor during this developmental period. In other words, we find that the subgroup of children living in poor families during adolescence is especially vulnerable to the harmful effects of disadvantaged neighborhoods. We conclude that ecological socialization models of neighborhood effects must account for the interactions between nested social contexts like the family environment and local community, as well as for the dynamic coevolution of these contexts over time.

NEIGHBORHOOD EFFECT MODERATION

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; Massey and Denton 1993). For social disorganization models, 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, by contrast, 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). The health effects of neighborhood poverty are central to environmental models, which 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).

Children raised in families with different economic resources likely respond differently to the social milieu in which they are immersed, but it remains unclear which subgroups of children are most sensitive to the neighborhood environment. Competing theories about neighborhood effect moderation suggest starkly different effects for subgroups of children defined in terms of the resources available to their families. The two perspectives—compound disadvantage theory and the relative deprivation model—describe how children in poor versus non-poor families may be differentially affected by neighborhood conditions and propose conflicting hypotheses about which subgroup of children is most harmed by residence in disadvantaged neighborhoods.

The compound disadvantage perspective 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, Yeung, Brooks-Gunn, and Smith 1998; Duncan and Brooks-Gunn 1997; Mayer 1997). Beyond this independent effect, family poverty is further thought to exacerbate the effects of neighborhood poverty for several reasons. First, the social networks of poor families are more often restricted to the local neighborhood than those of non-poor 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 environment (Wilson 1996). Without parents or resident adults to signal that socioeconomic advancement is possible, children living in both poor families and poor 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 community. 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, non-poor parents living in disadvantaged neighborhoods may be able to afford higher-quality childcare outside the local community, 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 the compound disadvantage perspective, 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 the compound disadvantage model, 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 advantaged communities. Children in both poor and nonpoor families are thought have substandard educational outcomes in disadvantaged neighborhoods, but only the latter group benefits from residence in more affluent social contexts.

At a simple level, 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 community, relative to residence in a disadvantaged neighborhood, may have no appreciable impact on children from poor families. By contrast, children in non-poor families, who can realize the benefits of access to neighborhood resources, are expected to be more sensitive to the local environment.

According to social psychological variants of the relative deprivation perspective, 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 communities, then, may suffer stigmatization or develop negative self-perceptions that interfere with their schooling. Non-poor children in affluent neighborhoods do not suffer the harmful psychological and emotional effects of relative deprivation. Thus, when children from poor families live in more affluent communities, 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 non-poor families, and they are more likely to prosper as a result of moving from a more to a less disadvantaged community.

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 non-poor children tend to be better prepared for school and have parents who are better equipped to navigate the school system, 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 communities. In this situation, living in more versus less disadvantaged neighborhoods may not lead to improved educational outcomes for poor children.

A few empirical studies analyze how neighborhood effects are moderated by socioeconomic characteristics of the family. South and Crowder (1999), focusing on family formation, find no significant interaction between family resources and neighborhood context, but Wheaton and Clarke (2003), investigating neighborhood effects on mental health, provide evidence that children from poor families are more vulnerable to disadvantaged neighborhoods. Brooks-Gunn et al (1993), the only study of educational attainment that tests for neighborhood effect moderation, finds no interaction between neighborhood disadvantage and family economic resources. These studies provide important analyses of moderated neighborhood effects but their results are limited because they do not properly account for the dynamic nature of both neighborhood context and family resources. Families move between different neighborhood contexts (Quillian 2003; Timberlake 2007). They also move in and out of poverty as parental income and household size fluctuate over time (Gottschalk, McLanahan, and Sandefur 1994). As we explain below, it is critically important to account for both duration and timing of exposure to different neighborhood contexts, as well as the dynamic selection and feedback mechanisms that structure neighborhood effects on educational outcomes.

NEIGHBORHOOD EFFECTS: TEMPORAL AND DEVELOPMENTAL DIMENSIONS

The theories of neighborhood effects on child development outlined previously focus on a variety of different mechanisms, but they all suggest that both duration and timing of exposure to disadvantaged neighborhoods are important for educational outcomes. For social isolation models, where the detrimental impact of poor neighborhoods is hypothesized to operate through deviant 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 a more severe impact for long-term rather than transitory exposure to disadvantaged neighborhoods.

In addition to duration of exposure, the consequences of living in disadvantaged neighborhoods also likely depend on the timing of exposure during the course of development. Since school continuation decisions typically occur during late adolescence, residence in disadvantaged neighborhoods during this developmental stage may be the most consequential for educational attainment. Adolescence is also the period when the outside community 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 environment would have an appreciable impact.

On the other hand, research on cognitive development and skill formation indicates that children are particularly sensitive to environmental deprivation earlier 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 each 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 community 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 employment status, which also change over time. Furthermore, the same family characteristics that influence the type of neighborhood environment to which children are exposed 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 the effects of neighborhood poverty: time-varying family characteristics 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 poor 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 high-poverty 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 high-poverty 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 factors are themselves affected by the neighborhood environment (Fernandez and Su 2004; Wilson 1987; Wilson 1996). Wilson (1987) argued that adult residents of poor 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 poor 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 high-poverty 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 characteristics 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 children defined by time-varying family characteristics, 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 consists of the 6,135 children present in the PSID at age 2 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.

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 0 through 4 that records the neighborhood quintile in which a child resides, with lower values indicating a neighborhood is less disadvantaged and higher values representing more disadvantaged neighborhoods (see Appendix A for details). Specifically, the childhood measurement of neighborhood disadvantage, denoted by A_1, is based on a child's average tract disadvantage score over the four survey years from age 6 to 9. Neighborhood conditions during adolescence, denoted by A_2, is based on the average tract disadvantage score between age 14 and 17. Multi-wave averages of the neighborhood disadvantage index are used to minimize measurement error and account for duration of exposure.[1]

The vector of time-invariant covariates, represented by V, 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.[2] The time-varying covariates included in this analysis are the family head's marital and employment status, the family income-to-needs ratio, 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 child'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 around

1 (the poverty line) so that this variable is greater than 0 for families with incomes that exceed the poverty line and is less than 0 for families with sub-poverty incomes.

With these data, we construct multi-wave averages of time-varying factors during childhood and adolescence for use in the analysis of neighborhood effects. Specifically, let L_1 be the vector of time-varying covariates averaged over the survey waves in which a child is age 2 to 5—the four waves immediately preceding measurement of treatment during childhood. Similarly, L_2 is the vector of time-varying characteristics averaged over the four survey waves preceding measurement of treatment during adolescence, when a child is age 10 to 13. These data, then, have the following temporal structure; $(V, L_1, A_1, L_2, A_2, Y)$, where Y is the outcome coded 1 if a child graduated from high school by age 20, and 0 otherwise. Multiple imputation with 100 replications is used to fill in missing values for all variables (Royston 2005; Rubin 1987).[3]

Hypothesized Causal Relationships

Figure 1 presents a directed acyclic graph that describes hypothesized causal relationships between neighborhood disadvantage, family characteristics, 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 no causal effect (Pearl 1995; Pearl 2000). In Figure 1, selection into disadvantaged neighborhoods is affected by prior time-varying family characteristics, and residence in a disadvantaged neighborhood, in turn, affects future levels of these time-varying factors.[4] The reciprocal relationship between neighborhood context and time-varying family characteristics at adjacent time periods reflects the dynamic neighborhood selection and feedback process. Figure 1 also shows that exposure to neighborhood poverty at each developmental stage has a direct effect on high school graduation. In addition, exposure to neighborhood poverty during childhood has an indirect effect that operates through future family characteristics. In departure from more restrictive conventional assumptions, we permit 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 poverty and mediators for the effect of past exposure to neighborhood poverty. Theory also suggests that time-varying family characteristics are effect moderators. Specifically, family economic resources are thought to temper or exacerbate the educational effects of exposure to disadvantaged neighborhoods. Although not explicitly depicted, Figure 1 is consistent with treatment-effect moderation because the outcome in this graph depends on the hypothesized effect moderator (Elwert and Winship 2010; VanderWeele 2009; VanderWeele and Robins 2009). The central aim of this analysis is to estimate the effects of different patterns of exposure to disadvantaged neighborhoods during childhood and adolescence conditional on the evolving economic position of the family.

Counterfactual Models of Moderated Neighborhood Effects

In this section, we use the counterfactual framework and potential outcomes notation for time-varying treatments to define the moderated neighborhood effects of interest (Almirall, Ten Have, and Murphy 2010; Almirall, McCaffrey, Ramchand, and Murphy 2011; Holland 1986; Robins 1999a; Robins 1994; Rubin 1974). For notional simplicity, L_1 and L_2 are treated as repeated measures of a single time-varying covariate, the family income-to-needs ratio, but the methods discussed here are easily generalized for vector-valued L_k.

Let $Y(a_1, a_2)$ indicate whether a subject would have graduated from high school had she been exposed to the fixed sequence of neighborhood conditions (a_1, a_2) during childhood and adolescence, possibly contrary to fact. For example, $Y(0,0)$ is the subject's outcome had she been exposed to the least disadvantaged quintile of neighborhoods during childhood and adolescence, $Y(1,0)$ is the outcome had she been exposed to second quintile neighborhoods during childhood and the least disadvantaged 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. That is, the adolescent income-to-needs ratio, $L_2(a_1)$, is here defined as a potential outcome of neighborhood conditions during childhood, reflecting the dynamic selection and feedback process described above. Because subjects are exposed to one of five levels of neighborhood disadvantage at two developmental periods, there are twenty-five potential education outcomes $\{Y(0,0), Y(1,0), \dots, Y(3,4), Y(4,4)\}$ and five intermediate income-to-needs outcomes $\{L_2(0), L_2(1), \dots, L_2(4)\}$. For each subject, we only observe the outcomes where $Y = Y(a_1, a_2)$ and $L_2 = L_2(a_1)$. The other potential outcomes are thus counterfactual.

In the counterfactual framework, causal effects are defined as contrasts between different 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

$$u_1(L_1, a_1) = E(Y(a_1, 0) - Y(0,0)|L_1) = \beta_1 a_1 + \beta_2 L_1 a_1, \quad (1)$$

the average causal effect of neighborhood exposure sequence $(a_1, 0)$ relative to sequence $(0,0)$ within levels of L_1. In words, $u_1(L_1, a_1)$ compares the probability of high school graduation had subjects in families with resources given by L_1 been exposed to neighborhoods in quintile a_1 of the composite disadvantage distribution during childhood and neighborhoods in the least disadvantage quintile during adolescence versus had subjects within the given subgroup been continuously exposed to the least disadvantaged quintile of neighborhoods. We use a linear parametric function, $\beta_1 a_1 + \beta_2 L_1 a_1$, to summarize these effects: β_1 gives the average causal effect on high school graduation of childhood exposure to neighborhoods located in quintile a_1 of the composite disadvantage distribution, 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 children in families with incomes above or below the poverty line. If the coefficient on the interaction term, β_2, equals zero, then the family income-to-needs ratio does not moderate the impact of exposure to disadvantaged neighborhoods during childhood.[5]

The second set of causal effects is defined as

$$u_2(L_2(a_1), a_2) = E\big(Y(a_1, a_2) - Y(a_1, 0) \big| L_2(a_1)\big) = \beta_3 a_2 + \beta_4 L_2(a_1) a_2, \qquad (2)$$

the average causal effect of neighborhood exposure sequence (a_1, a_2) compared to sequence $(a_1, 0)$ within levels of $L_2(a_1)$. That is, $u_2(L_2(a_1), a_2)$ gives the effect of adolescent exposure to disadvantaged neighborhoods on the probability of high school graduation, holding neighborhood conditions during childhood constant, among families with different economic resource levels when the subject is a teenager. The parametric function, $\beta_3 a_2 + \beta_4 L_2(a_1) a_2$, returns the average effects of adolescent exposure to different neighborhood conditions for subgroups of children defined in terms of their family's income-to-needs ratio measured during adolescence. As above, if the interaction coefficient, β_4, equals zero, then the family income-to-needs ratio does not moderate the impact of adolescent exposure to neighborhood disadvantage.[6]

The causal functions defined here describe how the effects of exposure to disadvantaged neighborhoods during childhood versus adolescence depend on the evolving resources of the family. By including interaction terms between family poverty and neighborhood context, these functions allow us to evaluate the compound disadvantage and relative deprivation perspectives. In addition, by evaluating moderated neighborhood effects within a longitudinal framework, we can examine whether children's sensitivity to different neighborhood conditions varies by developmental stage.

The structural nested mean model (SNMM) directly links $u_1(L_1, a_1)$ and $u_2(L_2(a_1), a_2)$, the moderated neighborhood effects of interest, to the conditional mean of $Y(a_1, a_2)$ given $\big(L_1, L_2(a_1)\big)$, the time-varying effect moderators (Almirall, Coffman, Yancy, and Murphy 2010; Almirall, Ten Have, and Murphy 2010; Almirall, McCaffrey, Ramchand, and Murphy 2011; Robins 1999a; Robins 1994). The SNMM is expressed as

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

where $\beta_0 = E\big(Y(0,0)\big)$ is the mean of the potential outcomes under sustained exposure to the least disadvantaged quintile of neighborhoods, and $\varepsilon_1(L_1)$ and $\varepsilon_2\big(L_1, a_1, L_2(a_1)\big)$ are nuisance functions that capture the association of family poverty with the outcome.[7] Specifically, $\varepsilon_1(L_1) = E(Y(0,0)|L_1) - E\big(Y(0,0)\big)$ 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 $\varepsilon_2\big(L_1, a_1, L_2(a_1)\big) = E\big(Y(a_1, 0)|L_1, L_2(a_1)\big) - E(Y(a_1, 0)|L_1)$ 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. These functions capture both causal and non-causal relationships between family resources and the outcome and are called “nuisance” functions because they provide no information about the effects of neighborhood context. An important property of $\varepsilon_1(L_1)$ and $\varepsilon_2\big(L_1, a_1, L_2(a_1)\big)$ is that they have, by definition, zero conditional mean given the past, that is, $E\big(\varepsilon_1(L_1)\big) = E\big(\varepsilon_2\big(L_1, a_1, L_2(a_1)\big)\big|L_1\big) = 0$. To estimate $u_1(L_1, a_1)$ and $u_2(L_2(a_1), a_2)$, the causal functions of the SNMM, the central challenge is to properly model the nuisance functions associated with time-varying covariates.

The causal effects defined in Equations 1 and 2 above 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. Sequential ignorability is met by design in experimental studies where treatment is randomly assigned at each time point, but in observational studies, as with the present empirical investigation, satisfying this assumption requires data on all the joint predictors of neighborhood disadvantage and high school graduation.

Limitations of Conventional Regression Models

Consider the following 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:

$$E(Y|L_1, A_1, L_2, A_2) = \lambda_0 + \lambda_1 L_1 + \lambda_2 A_1 + \lambda_3 L_1 A_1 + \lambda_4 L_2 + \lambda_5 A_2 + \lambda_6 L_2 A_2. \tag{4}$$

Equation 4 includes "main effects" for neighborhood disadvantage and the income-to-needs ratio measured at each developmental period. The model also includes interaction terms between neighborhood exposure status and the income-to-needs ratio, which allow the effect of neighborhood disadvantage during childhood (or adolescence) to vary by family poverty status measured earlier in childhood (or adolescence).

The set of causal relationships depicted in Figure 1 pose several problems for this conventional modeling strategy. Because the family income-to-needs ratio in adolescence is affected by exposure to neighborhood disadvantage during childhood, the parameters associated with A_1 and the $L_1 A_1$ interaction term do not represent the moderated causal effects of childhood exposure to disadvantaged neighborhoods. The problem is that Equation 4 directly conditions on the income-to-needs ratio measured during adolescence. As depicted graphically in Figure 2, conditioning on this measurement of the family income-to-needs ratio (a) removes the indirect effect of exposure to disadvantaged neighborhoods during childhood that is transmitted through family poverty during adolescence and (b) introduces bias through an induced association between unobserved determinants of high school graduation and neighborhood context (Greenland 2003; Pearl 1995; Pearl 2000; VanderWeele and Robins 2007).

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 1999b). In other words, even with data from an ideal experimental study that sequentially randomized exposure to disadvantaged neighborhoods, conventional regression models would still fail to recover the moderated effects of neighborhood disadvantage if the moderating variables of interest are time-varying and affected by past neighborhood conditions (Almirall, Ten Have, and Murphy 2010; Almirall, McCaffrey, Ramchand, and Murphy 2011; Robins 1999a; Robins 1994). Thus, alternative methods are needed to estimate moderated neighborhood effects in this study.[8]

Two-stage Regression-with-Residuals Estimation

Almirall and colleagues (2010; 2011) provide a two-stage regression estimator for the SNMM that is motivated by the zero conditional mean property of the nuisance functions discussed above. This approach is very similar to estimating a conventional regression model, but it proceeds in two steps. First, time-varying covariates are regressed on the observed past to obtain estimated residuals. Specifically, the income-to-needs ratio, for example, is regressed at each time point on prior treatment and time-varying covariates in models with form $E(L_1) = \alpha_0$ and $E(L_2|L_1, A_1) = \gamma_0 + \gamma_1 L_1 + \gamma_2 A_1 + \gamma_3 L_1 A_1$. Based on these models, the residuals $L_1^r = L_1 - E(L_1)$ and $L_2^r = L_2 - E(L_2|L_1, A_1)$ are estimated. Second, with estimates of L_1^r and L_2^r from the first stage, the SNMM is estimated by fitting the following regression for the observed outcome,

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

In contrast to conventional regression models, this model includes “main effects” not for the observed time-varying factors themselves but for residualized time-varying covariates obtained from the first-stage regressions. Equation 5 shows that $\eta_1 L_1^r$ forms the model for $\varepsilon_1(L_1)$ and $\eta_2 L_2^r$ forms the model for $\varepsilon_2(L_1, a_1, L_2)$, both of which satisfy their zero conditional mean property in SNMM (i.e., $E(\eta_1 L_1^r) = 0$ and $E(\eta_2 L_2^r|L_1, A_1) = 0$ by design).

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. When the adolescent measurement of the income-to-needs ratio, for example, is residualized with respect to past treatment, this covariate is purged of its association with exposure to disadvantaged neighborhoods 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 "partial out" the indirect effects of childhood exposure to disadvantaged neighborhoods that operate through this time-varying factor 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.

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. The other time-varying covariates, as well as time-invariant characteristics, are treated as control variables. That is, these factors only enter nuisance functions in the SNMM and not the causal functions. Estimates are reported for the total population and also for black and nonblack children 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).[9]

RESULTS

Sample Characteristics

Time-invariant sample characteristics 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 black children are high school graduates compared to 85 percent of nonblack children. Parents of black children are also much more disadvantaged than parents of nonblack children. For example, black sample members 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 2 presents descriptive statistics for time-varying sample characteristics, which further document sizeable racial disparities. Nonblack sample members 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 characteristics also 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 magnitude of this increase is much greater for nonblacks, leading to growing racial disparities in material circumstances.

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.

Among black children, 60 percent are exposed to the most disadvantaged quintile of American neighborhoods during both childhood and adolescence, and few blacks living in fifth quintile neighborhoods during childhood escape to less disadvantaged conditions later in adolescence. While the majority of black children grow up in highly disadvantaged neighborhoods, a nontrivial number live in less disadvantaged areas and some of these children 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 black children in third quintile neighborhoods during childhood moving to more disadvantaged neighborhoods later in adolescence.

Compared to black children, 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 nonblack children 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 4 percent of black children live in either first or second quintile neighborhoods during childhood and adolescence. Most nonblacks live in middling, second through fourth quintile neighborhoods during childhood, with many transitioning upward to less disadvantaged neighborhoods in adolescence. The frequent mobility between different neighborhood contexts among both black and nonblack sample members 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 poverty status. The rows in this table define different levels of the family income-to-needs ratio, where values below zero represent sub-poverty incomes and values greater than zero represent incomes above the poverty line. Family poverty during childhood and adolescence is intimately 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 destined to live in disadvantaged neighborhoods, and similarly, 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 children 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 children at all family income levels reside in middling, third quintile neighborhoods. The central aim of the present study is to estimate the effects of exposure to different neighborhood contexts during childhood versus adolescence among families with different levels of economic resources.

Moderated Neighborhood Effects

Table 5 presents two-stage regression estimates for the SNMM causal function parameters. Coefficient estimates describe how the probability of high school graduation is expected to change with exposure to different neighborhood contexts during childhood versus adolescence, conditional on prior family poverty status. Specifically, in the childhood causal function, the main effect of neighborhood disadvantage gives the expected difference in graduation probabilities had subjects living with poor families been exposed during childhood to neighborhoods in quintile a_1 of the composite disadvantage distribution, rather than the less disadvantaged quintile $a_1 - 1$, and then exposed during adolescence to neighborhoods in the least disadvantaged quintile. The interaction term in the childhood causal function increments this effect for subjects whose families are above or below the poverty line during childhood. In the adolescent causal function, the main effect of neighborhood disadvantage gives the expected difference in graduation probabilities had children living with poor families been exposed during adolescence to neighborhoods in quintile a_2, rather than the less disadvantaged quintile $a_2 - 1$, holding neighborhood context during childhood constant. The interaction term describes how this effect is moderated by adolescent family poverty status.

The first columns of Table 5 contain results from the total sample of children, and the upper panels summarize the effects of childhood exposure to neighborhood disadvantage. Point estimates associated with neighborhood context during childhood are in the direction hypothesized by compound disadvantage theory but are highly imprecise and fail to reach conventional thresholds of statistical significance. In general, results suggest a negligible impact for childhood exposure to neighborhood disadvantage $\left(\hat{\beta}_1 = -.005, p = .700\right)$ and provide no evidence of effect moderation by prior family poverty status $\left(\hat{\beta}_2 = .005, p = .235\right)$. For example, among children in poor families, even the most extreme treatment contrast—exposure to highly disadvantaged, fifth quintile neighborhoods during childhood and then exposure to neighborhoods in the least disadvantaged quintile 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.[10] Among children in families above or below the poverty line, the effects of neighborhood disadvantage during childhood are also modest. Thus, results indicate that childhood exposure to different neighborhood contexts has a minimal impact on high school graduation, regardless of family poverty status.

Estimates for the effect of adolescent neighborhood context, by contrast, indicate that exposure to disadvantaged neighborhoods during this developmental period has a significant negative effect on high school graduation $\left(\hat{\beta}_3 = -.042, p < .001\right)$ and that this effect is moderated by family poverty status $\left(\hat{\beta}_4 = .012, p < .001\right)$. These estimates, summarized for the total sample in the lower left section of Table 5, indicate that disadvantaged neighborhoods are especially harmful for children from poor families, consistent with the compound disadvantage perspective. For example, among children 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 children in families who are extremely poor during adolescence—those living at one-half the poverty line—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.

The effects of adolescent exposure to disadvantaged neighborhoods for non-poor children, on the other hand, are much less severe. Among children from non-poor families with resources 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 children are most sensitive to the neighborhood environment during adolescence, at least when considering educational transitions in early adulthood, and that family poverty intensifies the negative effects of adolescent exposure to disadvantaged neighborhoods.

Separate effect estimates for black and nonblack children are reported in the middle and right-hand columns of Table 5. These estimates are comparable to those from the total sample, indicating that adolescent exposure to disadvantaged neighborhoods is more consequential than exposure earlier during childhood and that effects are most severe for children 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 children whose families are extremely poor, by about 21 percentage points for children in families at the poverty line, and by only 8 percentage points for children in non-poor 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 nonblack children in poor families and by about 5 percentage points for nonblack children in families with resources equivalent to three times the poverty line.

Figure 4 displays estimated probabilities of high school graduation, computed from the two-stage regression estimates, for black children with different neighborhood and family resource histories. The graph describes how the probability of high school graduation would be expected to change if black children were to live in middling, third quintile neighborhoods during childhood but then were later exposed to different neighborhood contexts in adolescence. Estimates are plotted separately for children living in families that were extremely poor, poor, or non-poor during both childhood and adolescence to demonstrate the substantial magnitude of effect moderation by family economic resources.

Results indicate that if black children 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 children 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 black children living with non-poor 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 non-poor black children would be expected to graduate had they instead moved to the most disadvantaged quintile of neighborhoods during adolescence.

Figure 5 displays predicted probabilities of high school graduation for nonblack children by neighborhood and family poverty history. These estimates indicate that had nonblack children 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 children had moved to neighborhoods in the most disadvantaged quintile during adolescence, only an estimated 77 percent of nonblack children in poor families and 83 percent of children in non-poor families would have graduated high school.

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 are incorrectly specified, then our neighborhood-effect estimates are 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 so 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.

Finally, because some sample members drop out of the PSID before age 20, a nontrivial number of respondents 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 then deletion (von Hippel 2007), single regression imputation (Longford 2005), and complete case analysis in order 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. This study integrates spatial, temporal, and developmental perspectives of the stratification process, analyzing effects of different neighborhood contextual trajectories on high school graduation among subgroups of children defined by the economic resources available to their families throughout the early life course. 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.

Using novel methods that properly account for dynamic neighborhood selection, this study shows 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 children whose families are also economically disadvantaged during this developmental period. By contrast, for adolescent children whose families are well above poverty level, the effect of neighborhood disadvantage is less pronounced. Neighborhood effects are thus heterogeneous, 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 perspective on neighborhood effect moderation, which contends that children in poor families are especially vulnerable to the harmful effects of living in disadvantaged neighborhoods. That is, family resource deprivation, results indicate, greatly exacerbates the educational consequences of neighborhood resource 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 sequences of neighborhood contexts experienced by children 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 factors 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 characteristics. We use the SNMM and two-stage regression-with-residuals estimator (Almirall, Ten Have, and Murphy 2010; Almirall, McCaffrey, Ramchand, and Murphy 2011) 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 the temporal dependency and subgroup heterogeneity 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. To overcome the problems identified in this study, a long-term commitment to reducing both economic inequality and its geographic concentration is critical.

NOTES

1. 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.

2. 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.

3. 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.

4. Selection into high-poverty neighborhoods is also affected by the time-invariant factors, V, which are hereafter subsumed into L_1 for visual and notational simplicity.

5. 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 $E(Y(a_1, 0) - Y(0,0)|L_1)$ for two reasons. First, the resulting contrasts between childhood exposure sequences $\{(1,0), \dots, (4,0)\}$ and sustained exposure to the least disadvantaged quintile of neighborhoods $(0,0)$ 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 the full structural models' parameters.

6. Models with interactions between childhood and adolescent exposure to neighborhood disadvantage and between the childhood measurement of the income-to-needs ratio and adolescent neighborhood disadvantage 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 treatment effects in Equation 2.

7. 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).

8. 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 1999a; Robins, Hernan, and Brumback 2000).

9. 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.

10. This effect estimate is based on the following calculation, $\hat{E}(Y(4,0) - Y(0,0) | L_1 = 0) = 4(\hat{\beta}_1 + \hat{\beta}_2 L_1) = 4(-.005 + .005(0)) = -.020$, where $L_1 = 0$ indicates that a subject's family is at the poverty line during childhood. Subsequent estimates reported in text are computed with $L_k = -.5$ for families at one-half the poverty line and with $L_k = 2$ for families three times above poverty level. Throughout this section, we use the descriptor "poor" for families at the poverty line, while "extremely poor" and "nonpoor" refer to families with resources equivalent to one-half and three times the poverty line, respectively.

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TABLES

Table 1. Time-invariant sample characteristics

Variable	Total			Blacks		Nonblacks	
	% miss	mean	sd	mean	sd	mean	sd
R - high school graduate	43.0	.80	(.40)	.75	(.44)	.85	(.36)
R - 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. R, M and H indicate respondent, mother of respondent and household head, respectively.

Table 2. Time-varying sample characteristics

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. R, M and H indicate respondent, mother of respondent and household head, respectively.

Table 3. Joint treatment distribution

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

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

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

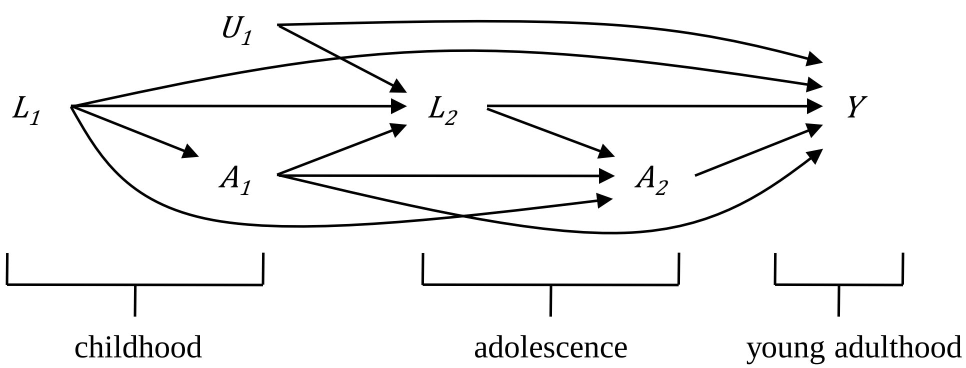
Table 5. Effects of neighborhood disadvantage on high school graduation (two-stage estimates)

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.

FIGURES

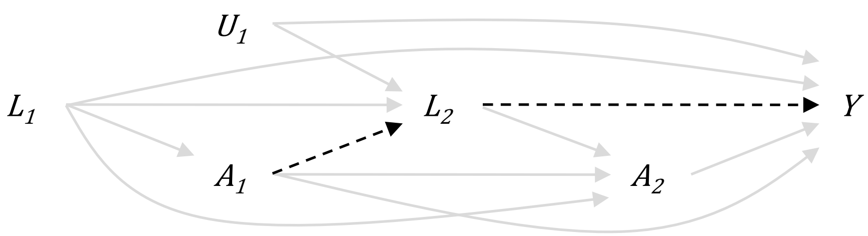
Figure 1. Hypothesized causal relationships



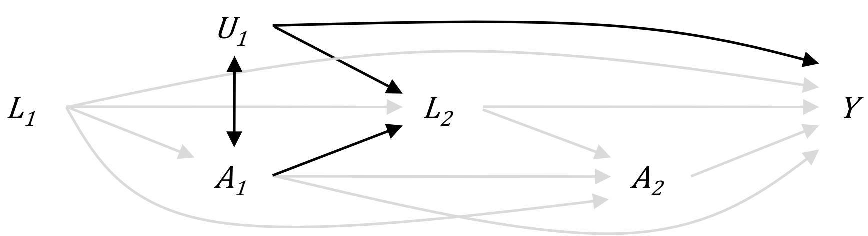
Notes: A_k = neighborhood dadvg, L_k = family economic resources,
U_k = unobserved factors and Y = high school graduation.

Figure 2. Problems with conventional regression models

A. Over-control of intermediate pathways

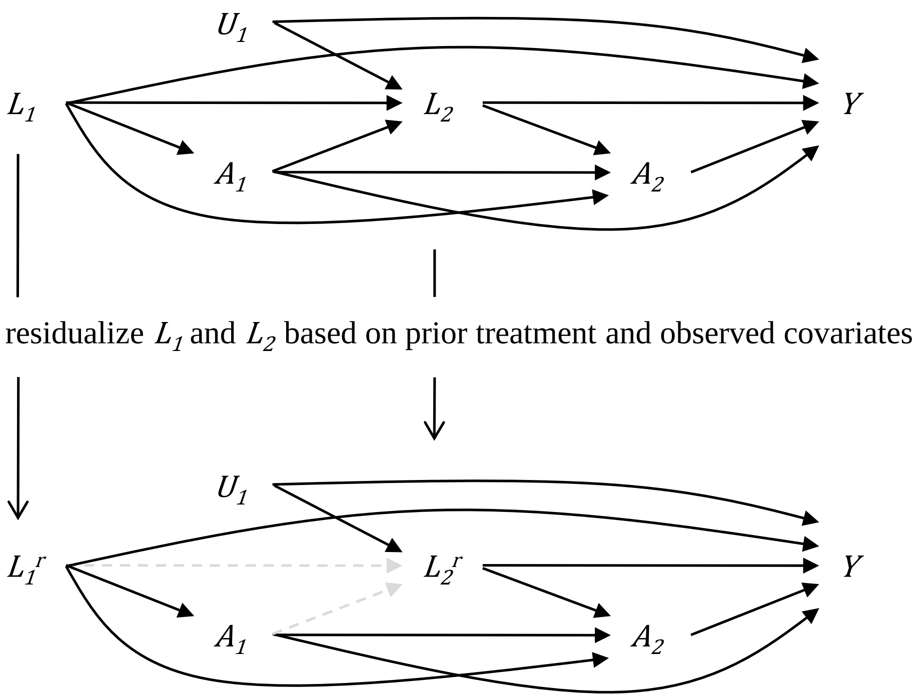


B. Collider-stratification bias



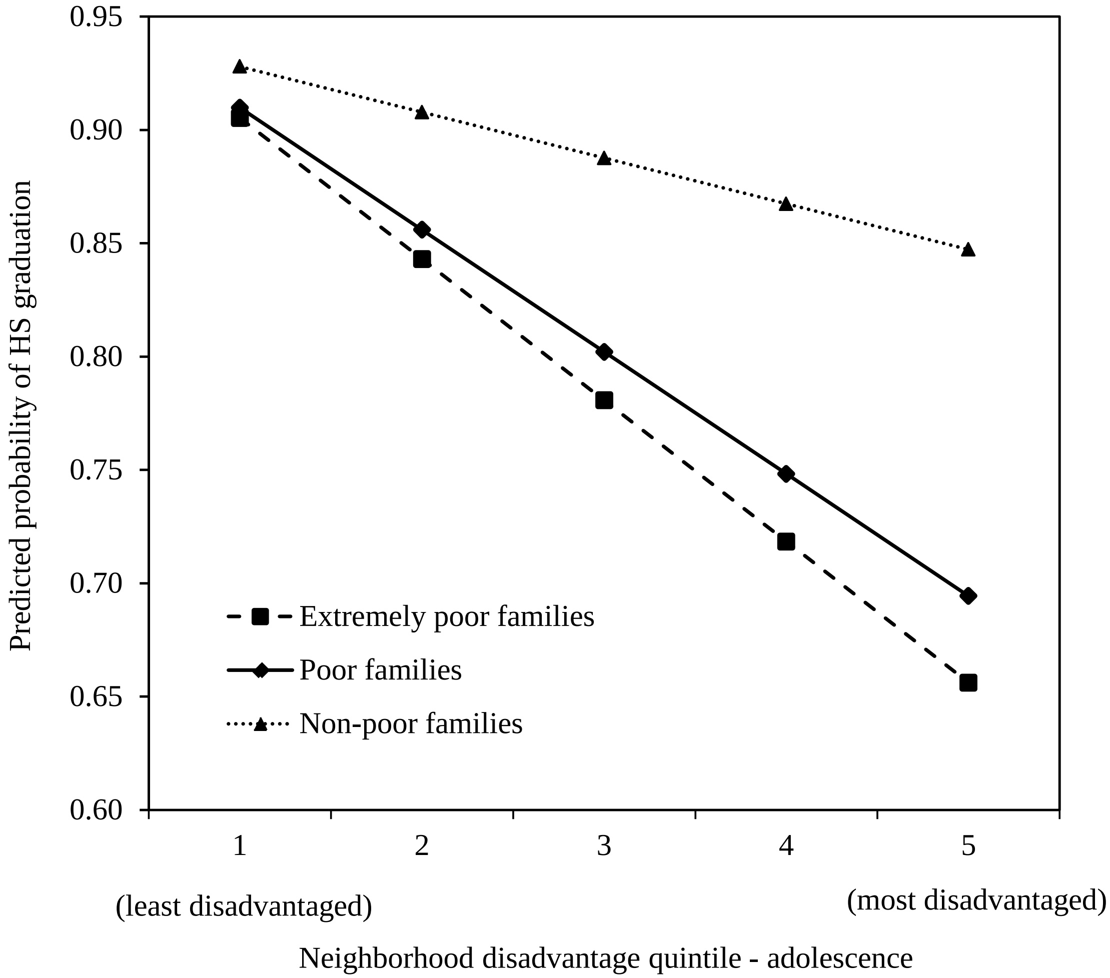
Notes: A_k = neighborhood dadvg, L_k = family economic resources, U_k = unobserved factors and Y = high school graduation.

Figure 3. Effects of residualizing time-varying covariates



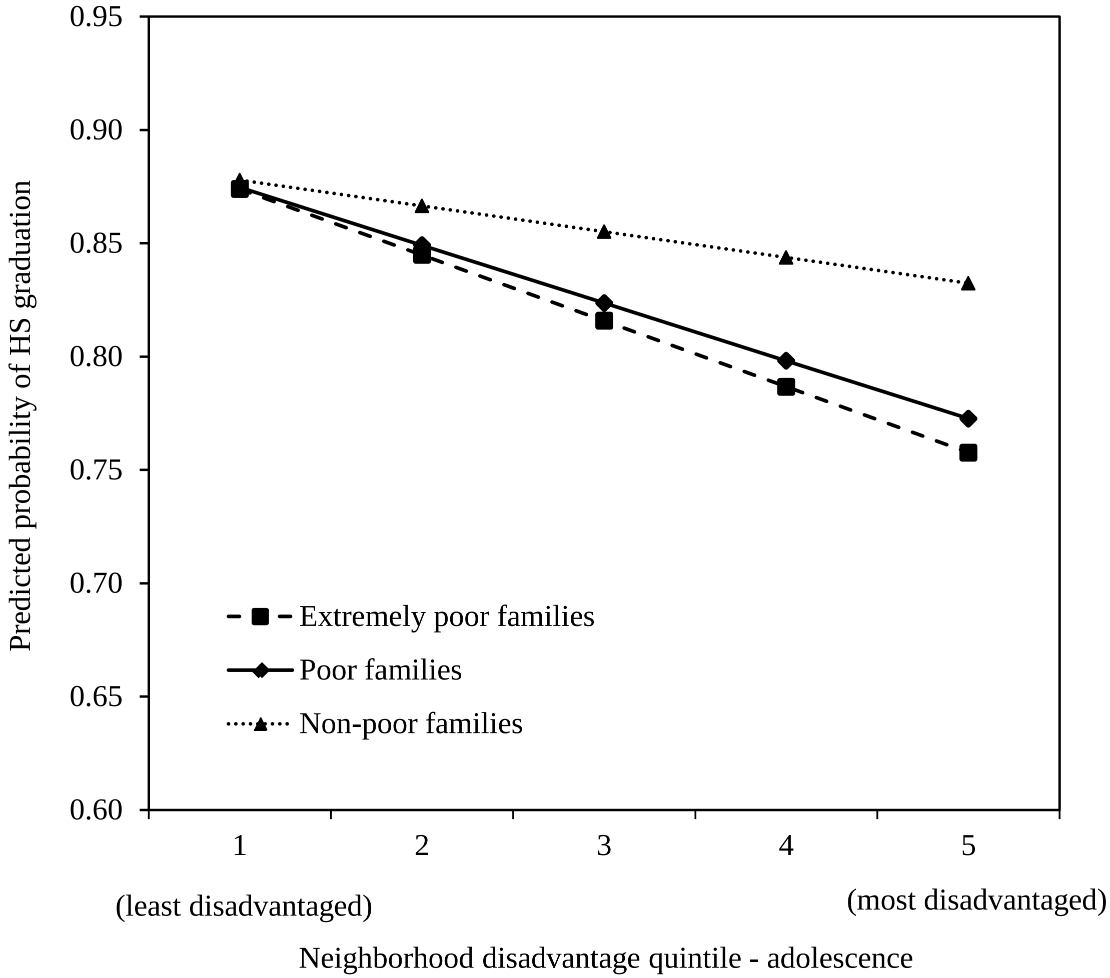
Notes: A_k = neighborhood dadvg, L_k = family economic resources,
U_k = unobserved factors and Y = high school graduation.

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. 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.

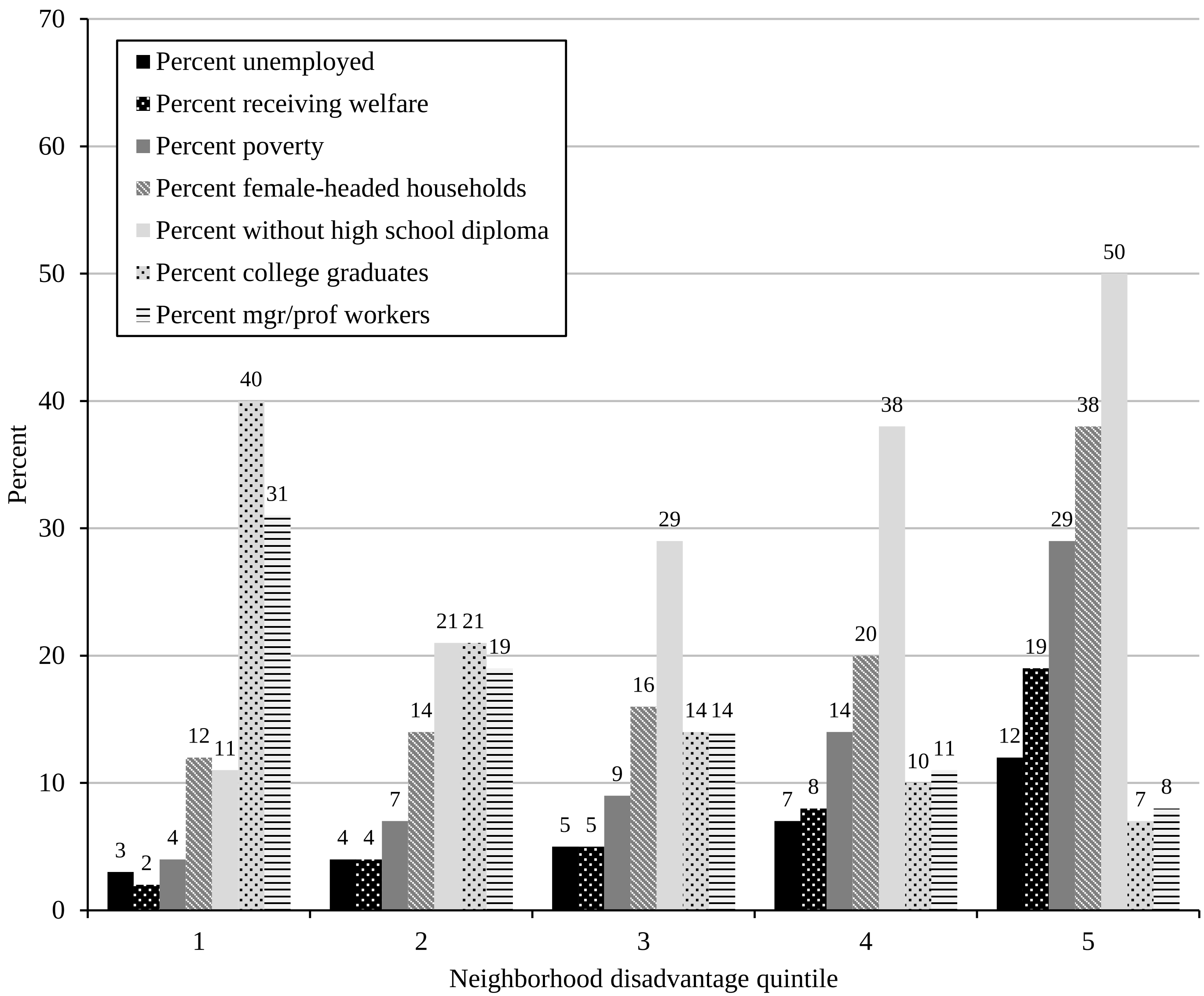
APPENDIX A. NEIGHBORHOOD DISADVANTAGE INDEX

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 an interaction between childhood and adolescent exposure to disadvantaged neighborhoods. 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 characteristics, 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 characteristics 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 supplemental 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 baseline factors, V; time-varying family characteristics measured during childhood, L_1; and time-varying family characteristics 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 baseline time-invariant characteristics and between baseline characteristics and time-varying factors measured during childhood and adolescence. Table B.3 shows estimates of SNMMs based on nuisance functions with all two-way interactions between different time-varying characteristics measured during childhood and adolescence, as well as cross-time interactions between these factors. 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.
†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 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, then we conclude that our results are robust to unobserved confounding.

To illustrate the logic behind this type of sensitivity analysis, consider a cross-sectional 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, $E = (F) = (G) = (H) = (I)$, $(J) = K = \cdots = (N)$, and so on, such that the observed and counterfactual graduation rates are equal within columns. In other words, with perfect randomization the observed mean potential outcome for subjects living in quintile a equals the unobserved mean potential outcome of subjects randomized to a neighborhood in some other quintile a', and vice versa. 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 is 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-corrected estimate for the average treatment effect can be obtained from the following calculations. First, we compute the proportion of subjects in each neighborhood quintile, denoted by $P(A)$ for $A = 0,1, \ldots, 4$. Second, we subtract the bias term, $B = \sum_{A'=0}^{A'=4} (A - A')\alpha\, P(A')$, from the observed outcome, Y, to obtain a corrected 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-corrected 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, $B = \sum_{k=1}^{k=2} \sum_{A'_k=0}^{A'_k=4} (A_k - A'_k)\alpha_k \, P(A'_k)$, and then the corrected outcomes are computed as above. To incorporate effect moderation and controls for observed confounders, we simply refit the SNMM using the corrected outcomes, and this yields bias-adjusted estimates for the impact of neighborhood disadvantage on high school graduation. 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 non-poor 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 point 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 factors 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 theoretically important 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 effect of exposure to disadvantaged neighborhoods during childhood is small and not statistically significant for both poor and non-poor children, while the effect of adolescent neighborhood disadvantage is severe and remains statistically significant under a moderate degree of unobserved confounding ($\alpha > -.5$) for non-poor children and under 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 variables ($\alpha > -2$), the negative effect of neighborhood disadvantage on high school graduation among children in poor families remains substantively large and highly 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 unobserved confounding.

Table C.1. Potential outcomes from hypothetical neighborhood experiment

Observed	Mean Potential Outcome, $E(Y(a)\|A)$				
Treatment	$E(Y(0)\|A)$	$E(Y(1)\|A)$	$E(Y(2)\|A)$	$E(Y(3)\|A)$	$E(Y(4)\|A)$
$A = 0$	E	(J)	(O)	(T)	(EE)
$A = 1$	(F)	K	(P)	(U)	(FF)
$A = 2$	(G)	(L)	Q	(V)	(GG)
$A = 3$	(H)	(M)	(R)	W	(HH)
$A = 4$	(I)	(N)	(S)	(X)	II

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

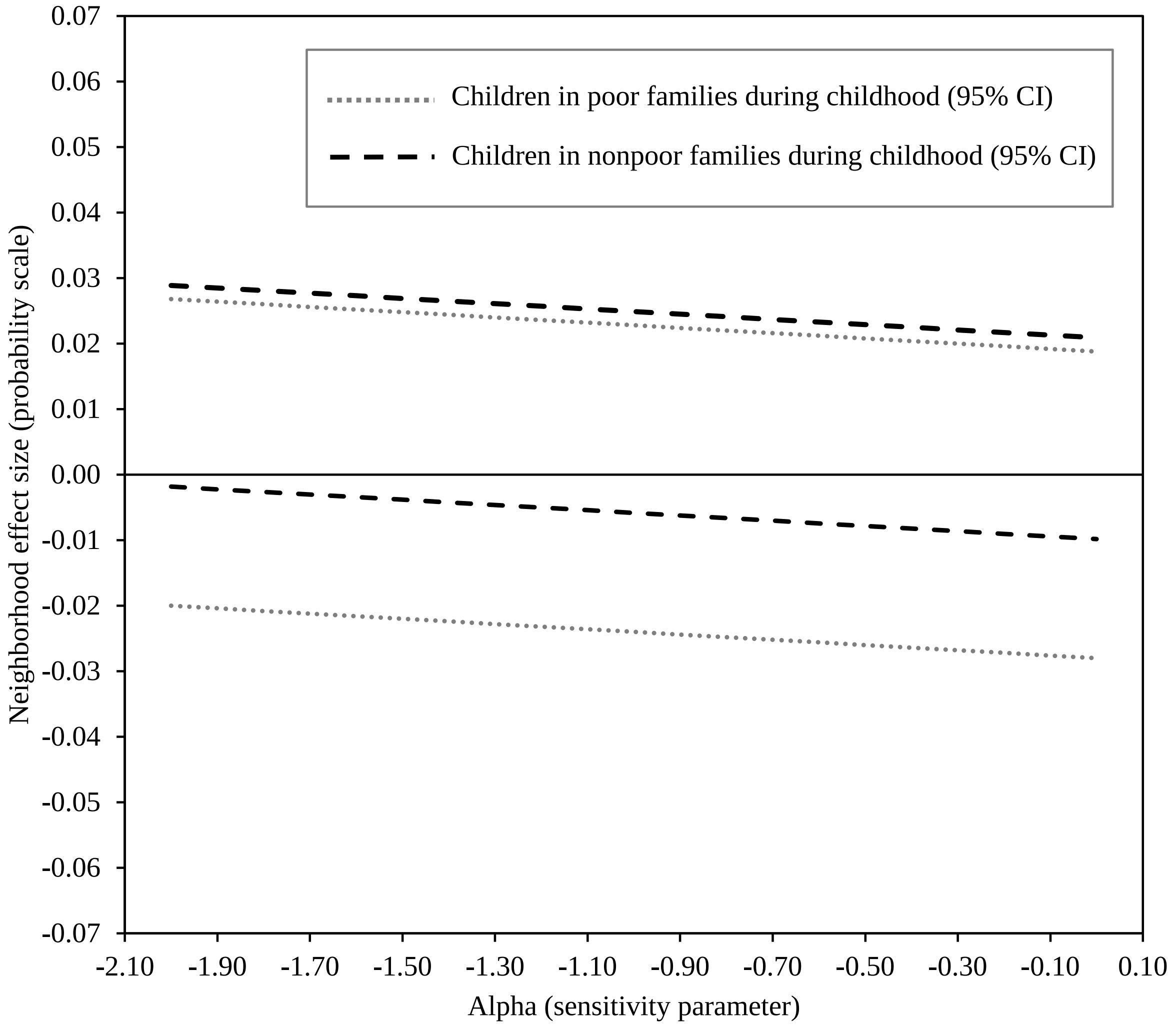
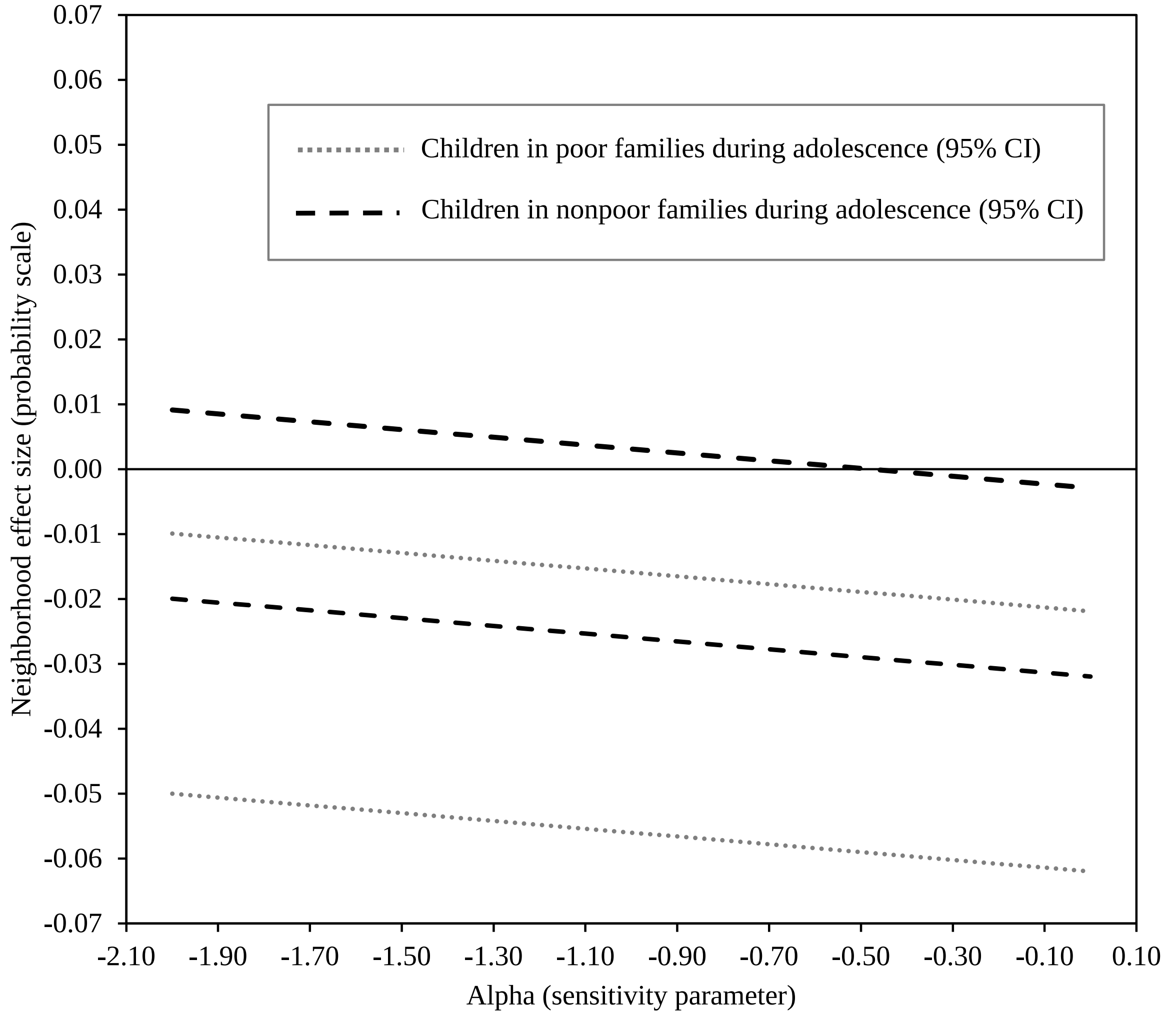


Figure C.2. Sensitivity of effect estimates for adolescent neighborhood disadvantage to hypothetical unobserved confounding



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 respondent 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 two-stage 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, combined effect estimates and standard errors based on MI are unbiased and valid, respectively. The second column contains combined estimates based on multiple imputation then 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. 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 weaker assumptions than MID and CC, and provides for valid inferences, unlike SI.

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.