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Neighborhood Effects in Temporal Perspective

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Abstract

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

Keywords

neighborhoods; education; causality; marginal structural models

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

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

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

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

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

Building on previous work investigating the temporal dimensions of neighborhood effects (Crowder and South 2010; Jackson and Mare 2007; Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert Forthcoming), this study extends research on neighborhood context and child development by (1) measuring duration of exposure to disadvantaged neighborhoods throughout childhood and adolescence, (2) explicitly defining neighborhood effects within a counterfactual causal framework for time-varying exposures, and (3) using novel statistical methods of adjusting for dynamic neighborhood selection that overcome critical shortcomings of conventional regression. Specifically, we estimate the effects of sustained exposure to different levels of neighborhood disadvantage on high school graduation, a central dimension of social stratification (Rumberger 1987). Since educational attainment is one of the most extensively studied outcomes in neighborhood effects research (e.g., Aaronson 1997; Brooks-Gunn, Duncan, Klebanov, and Sealand 1993; Crane 1991; Crowder and South 2010; Ginther, Haveman, and Wolfe 2000; Harding 2003), we can compare our estimates, which take exposure duration and dynamic neighborhood selection into account, with the results of past analyses that neglect these issues.

We begin by reviewing the theoretical mechanisms through which long-term exposure to disadvantaged neighborhoods is thought to affect educational attainment. Next, we review past estimates of neighborhood effects, focusing on the limitations of static models and point-in-time measurements, and delineate the dynamic neighborhood selection process. Following this discussion, we specify counterfactual models for the longitudinal effects of neighborhood disadvantage on high school graduation, explain the challenges that dynamic selection processes pose for the estimation of duration-dependent neighborhood effects, and describe procedures to estimate these effects using inverse probability of treatment weights. Then, with data from the Panel Study of Income Dynamics (PSID), we follow a cohort of children from birth through early adulthood, measuring neighborhood context once per year, every year, for 17 years, and estimate the effects of sustained exposure to different levels of neighborhood disadvantage on high school graduation. The results of this analysis indicate that exposure to disadvantaged neighborhoods throughout the early life course has a severe negative impact on the chances of high school graduation for both black and nonblack children. These effects are considerably larger than estimates from prior research and appear to be mediated by time-varying characteristics of the family. In other words, our findings suggest that neighborhood effects on children operate in part through neighborhood effects on parents. We conclude that a temporal framework is essential for understanding the deleterious effects of disadvantaged neighborhoods on child development.

NEIGHBORHOOD MECHANISMS AND EXPOSURE DURATION

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

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

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

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

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

PREVIOUS ESTIMATES OF NEIGHBORHOOD EFFECTS

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

The most comprehensive neighborhood experiment to date is the Moving to Opportunity (MTO) study, which randomly assigned low-income residents of poor inner-city neighborhoods to receive Section 8 housing vouchers, enabling a subset of participants to move into more affluent suburban neighborhoods. Neighborhood-effect estimates from the MTO experiment are mixed. Early evaluations indicated that children who moved into low-poverty neighborhoods had significantly better developmental outcomes, including higher test scores, fewer behavioral problems, and better mental health (Ladd and Ludwig 1997; Ludwig, Duncan, and Hirschfield 2001). However, a second round of evaluations conducted 4–7 years after the intervention revealed few significant differences between treatment and control groups (Ludwig, Liebman, Kling, Duncan, Katz, Kessler, and Sanbonmatsu 2008).

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

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

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

DYNAMIC NEIGHBORHOOD SELECTION

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

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

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

Research on spatial attainment therefore shows that a variety of demographic and economic factors are important determinants of residence in different neighborhood environments. Conversely, theory and research also indicate that past neighborhood context in turn affects many of the same characteristics that influence future neighborhood selection. For example, spatial mismatch theories contend that poor neighborhoods are located far from areas with employment opportunities at appropriate skill levels, making it difficult for residents to acquire and maintain jobs (Fernandez and Su 2004; Wilson 1987; Wilson 1996). Furthermore, Wilson (1987) argues that the decline of manufacturing during the 1970s diminished the pool of marriageable men (i.e., employed men with income sufficient to support a family) in urban black neighborhoods, leading to delayed marriage and increasing non-marriage among this population. Thus, a number of family characteristics may simultaneously confound and mediate the effects of disadvantaged neighborhoods on child educational outcomes. That is, certain time-varying characteristics of the family, such as parental employment and marital status, affect both the educational attainment of children and the chances of living in different neighborhood environments and are in turn affected by prior neighborhood conditions. As we explain below, time-varying confounders that are affected by past neighborhood context pose unique methodological challenges for estimating neighborhood effects that cannot be addressed with conventional regression models.

METHODS

Data

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

The analytic sample for this study consists of the 4,154 children present at age 1 in PSID core FUs between 1968 and 1978. We gather information on these children for every year until age 20 or they are lost to follow-up.[1] 2,093 children—834 black and 1,259 nonblack—are continuously present in a responding PSID FU from age 1 to 17 and are present in, or reported on by, a PSID FU at age 20 (we describe methods used to adjust for sample

[1]A subject is lost to follow-up at wave *k* if their FU does not respond to the PSID at wave *k*. Subjects who leave the PSID at wave *k* but return to the study several years later are considered permanently lost to follow-up at wave *k*.

attrition below). The PSID wave, indexed by k, in which a child is age 1 defines the baseline time period ($k = 0$) when neighborhood context and a rich set of covariates are first measured. We then measure neighborhood context and all time-varying covariates once per year from age 2 to 17. Baseline neighborhood context is not used to estimate neighborhood effects but rather is absorbed into the vector of control variables measured at age 1. Thus, our study uses $K = 16$ post-baseline waves of follow-up—each of the waves in which children are between ages 2 and 17—to estimate the educational effects of neighborhood context. The outcome of interest in this study, high school graduation, is measured at age 20, defined to be the end of follow-up.[2]

Treatment

Measurements of neighborhood context come from the NCDB, which contains nation-wide tract-level data from the 1970–2000 U.S. Censuses with variables and tract boundaries defined consistently across time. Tract data for intercensal years are imputed using linear interpolation. We use principal component analysis to generate a composite score of neighborhood disadvantage based on seven tract characteristics: poverty, unemployment, welfare receipt, female-headed households, education (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).[3] We then divide census tracts into quintiles based on the national distribution of the composite disadvantage score and create a time-varying ordinal treatment variable, coded 1 through 5, that records the neighborhood quintile in which a child resides at each wave. The first quintile contains the least disadvantaged, and the fifth quintile contains the most disadvantaged census tracts. For example, in the average first quintile tract, less than 5% of residents are poor, only 2% receive welfare, and about 40% of adults are college graduates. By contrast, in the average fifth quintile tract, nearly 30% of residents are poor, about 19% receive welfare, and less than 50% of have graduated from high school (see Appendix A for details). In the analysis below, we use this ordinal wave-specific treatment variable to compute a measure of duration-weighted exposure to different levels of neighborhood disadvantage between ages 2 and 17.[4]

Covariates

This study includes an extensive set of covariates to control for potential confounding of neighborhood effects on high school graduation. The time-invariant baseline covariates are race, gender, birth weight, mother's age at birth, mother's marital status at birth, and FU head's education.[5] Race is coded 1 for black and 0 for nonblack; gender is coded 1 for female and 0 for male; birth weight is expressed as a dummy variable equal to 1 if the child was <2500 grams at birth and 0 otherwise; mother's age at birth is measured in years; and a dummy variable is used to indicate whether the mother was married at the time of childbirth. FU head's education is expressed as series of dummies for "less than high school," "high

[2]Due to measurement limitations in the PSID, sample members who earned a general equivalency degree (GED) by age 20 are coded as high school graduates.

[3]PCA of the seven tract characteristics reveals a single component representing neighborhood disadvantage. The disadvantage score for each tract-year observation is equal to the first principal component from a pooled analysis using all tract-years between 1968 and 2000. Appendix A, Table A.1, reports component weights and correlations with each tract characteristic. Table A.2 provides descriptive statistics for census tracts in each quintile of the composite disadvantage distribution.

[4]Results from analyses using a binary measure of neighborhood disadvantage (not shown, available upon request) are similar to those based on the five-level ordinal measure. The ordinal treatment variable is preferred because it retains more information about neighborhood context, allows for more flexible contrasts between different exposure trajectories, and attenuates certain technical problems associated with dichotomization, such as loss of statistical power.

[5]The PSID does not measure parental education at regular intervals, which limits our ability to track changes over time. We therefore treat parental education as time-invariant and use measurements of this factor taken at baseline or, short of that, the most recent measurement prior to baseline.

school graduate," and "at least some college." The time-varying covariates in this analysis, measured at each wave k, include the marital status, employment status, and work hours of the FU head, as well as family size, homeownership, receipt of Aid to Families with Dependent Children (AFDC), total family income, and residential mobility. Marital status is coded 1 for married and 0 for unmarried; employment status is coded 1 for employed and 0 for not employed; and work hours is equal to the average number of hours worked per week during the preceding year. Family size is defined as the total number of people present in a child's family at wave k; homeownership is expressed as a dummy variable indicating whether the family owned the residence they occupied at the time of the interview; and AFDC receipt is coded 1 if a family received AFDC income during the past year and 0 otherwise. Total family income is measured as the sum of taxable income the family head, partner, and other FU members earned over the past year, inflated/deflated using the Consumer Price Index (CPI-U) to 1990 dollars. Residential mobility is defined as the total number of times a child moved prior to wave k. For all variables, multiple imputation is used to fill in missing values due to item-specific nonresponse.[6]

Counterfactual Models for Time-Varying Neighborhood Exposures

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

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

$$E(Y_{\overline{a}}-Y_{\overline{a}'})=E(Y_{\overline{a}})-E(Y_{\overline{a}'})=P(Y_{\overline{a}}=1)-P(Y_{\overline{a}'}=1), \qquad (1)$$

where $P(Y_{\bar{a}} = 1)$ is the probability of high school graduation had all children experienced the neighborhood exposure trajectory $\bar{a}$, and $\mathrm{P}(Y_{\bar{a}'} = 1)$ is the analogous probability of high school graduation if all children had experienced the exposure sequence $\bar{a}'$. Since the same individual cannot simultaneously be exposed to two different treatment trajectories, the

[6]Multiple imputation replaces missing data with $m > 1$ values that are simulated from an imputation model. Each of the m complete datasets are then analyzed separately, and the results are combined to produce estimates and standard errors that account for the uncertainty associated with missing information (Little and Rubin 2002; Rubin 1987). We use $m = 5$ datasets with simulated missing values from multiple imputation by chained equations (Royston 2005). Neighborhood effect estimates are based on the combined results from these 5 datasets; for simplicity, descriptive statistics are reported for only the first imputed dataset.

effects of interest are impossible to observe directly and must be estimated. In principle, the effects of neighborhood context could be analyzed non-parametrically by comparing expectations, as in (1), for all possible exposure trajectories. However, with a five-level ordinal treatment and $K = 16$ waves of follow-up, there are 5^{16}, or over 150 billion, possible exposure trajectories and the same number of potential outcomes. Thus, data limitations force the imposition of simplifying functional form assumptions about the exposure-outcome relationship.

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

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

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

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

$$Y_{\bar{a}} \perp A_k | \bar{L}_k, \bar{A}_{k-1}, \tag{3}$$

where $\bar{L}_k = (L_0,\ldots,L_k)$ represents observed covariate history up to wave k, $\bar{A}_{k-1}$ encodes treatment history through the prior wave, and $\perp$ denotes statistical independence. Substantively, this says that children with the same combination of observed covariate values do not systematically select into different neighborhood contexts based on factors predictive of the outcome. Condition (3) is satisfied if there are no unobserved covariates that affect both neighborhood exposure status and high school graduation (i.e., if there is no unobserved confounding of treatment).

Failure of Conventional Regression Estimators

To understand the limitations of conventional regression models for estimating the effects of time-varying neighborhood exposures, consider the simplified two-wave example depicted

[7]MSMs that relax the linearity assumption in model (2) by including quadratic and cubic terms for duration-weighted exposure to neighborhood disadvantage provide no evidence of nonlinearity in treatment-outcome relationship—none of the high-order polynomial terms in these models are statistically significant. In addition, we fit models that allow the effect of duration-weighted exposure to differ between childhood (age 2–11) and adolescence (age 12–17). There is no statistical evidence of effect heterogeneity by developmental stage.

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

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

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

Estimation Using Inverse Probability of Treatment Weights

Inverse probability of treatment (IPT) weighting is an alternative approach specifically developed to adjust for confounding by time-varying covariates (Robins 1999; Robins, Hernan, and Brumback 2000).[9] This method has important advantages over conventional

[8]The assumption of no unobserved confounding of treatment does not preclude the existence of unobserved factors that affect time-varying covariates and the outcome.

[9]For other applications of MSMs and IPT weighting in the social sciences, see Barber, Murphy, and Verbitsky 2004; Sampson, Laub, and Wimer 2006; Hong and Raudenbush 2008; Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert 2010; and Sharkey and Sampson 2010.

regression models because it resolves the problems outlined in the previous section without making additional assumptions about the dynamic selection process. Intuitively, the method involves weighting observations to generate a pseudo-population in which treatment is no longer confounded by measured covariates. An unadjusted model for the observed outcome can then be fit to the weighted pseudo-population to obtain unbiased and consistent treatment-effect estimates under assumptions described below.

The IPT weight for the i^{th} child is given by

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

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

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

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

which are less variable than w_i and are centered around 1. Estimates based on the stabilized IPT weights have smaller variance and an approximately normal sampling distribution (Hernan, Brumback, and Robins 2002; Robins, Hernan, and Brumback 2000). Because confounders measured at baseline are included in both the numerator and denominator of the stabilized weight, the outcome model fit to the weighted pseudo-population must condition on these factors to obtain unbiased estimates of the desired treatment effects (note that regression-adjustments for covariates measured prior to treatment initiation do not suffer the limitations described in the previous section).

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

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

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

Sample Attrition

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

$$cw_i=\prod_{k=1}^{K}\frac{P(c_k=0|\overline{c}_{k-1}=0,\overline{A}_{k-1}=\overline{a}_{(k-1)i},L_0=l_0)}{P(c_k=0|\overline{c}_{k-1}=0,\overline{A}_{k-1}=\overline{a}_{(k-1)i},\overline{L}_k=\overline{l}_{ki})},\qquad (6)$$

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

[10]Estimating separate models by race is equivalent to fitting a pooled model that includes interactions between race and all other covariates.

[11]Robust standard errors such as those computed here are conservative (i.e., too large) because they do not account for the fact that the IPT weights are estimated (Robins, Rotnitzky, and Scharfstein 1999). Conservative standard errors make rejecting the null hypothesis of no treatment effect more difficult, and thus provide for more exacting tests at given levels of statistical significance.

RESULTS

Sample Characteristics

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

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

Longitudinal Neighborhood Exposure Patterns

Table 3 describes long-term exposure to different levels of neighborhood disadvantage throughout childhood, demonstrating the heterogeneity in neighborhood environments both across children and within children over time. The first panel in Table 3 presents descriptive statistics for our independent variable, duration-weighted exposure to neighborhood disadvantage. This measure is the average of ordinal wave-specific treatments from age 2 to 17, where higher values represent sustained exposure to more disadvantaged neighborhoods and lower values indicate long-term residence in less disadvantaged neighborhoods. Black and nonblack children had starkly different cumulative exposure patterns. About 65% of black children were exposed to a sequence of neighborhoods during childhood that were, on average, extremely disadvantaged. Less than 1% of blacks experienced long-term exposure to the least disadvantaged neighborhoods. By contrast, among nonblack children, only 8.74% spent the majority of their childhood in the most disadvantaged neighborhoods, and about 12% were continuously exposed to the least disadvantaged neighborhoods. Black children, therefore, were about seven times more likely than nonblack children to experience long-term residence in the most disadvantaged 20% of U.S. neighborhoods. The lower panel of Table 3 describes the number of moves between neighborhoods in different quintiles of the composite disadvantage index (i.e., the number of times a subject moved between levels of the ordinal treatment). Only 37.53% of blacks and 16.52% of nonblacks never moved between neighborhood quintiles throughout the early life course, whereas 29.86% and 44.24% of blacks and nonblacks, respectively, moved between different neighborhood contexts at least three times. These data thus reveal frequent neighborhood mobility.

Two substantive findings emerge from our descriptive analysis of longitudinal patterns of exposure to different neighborhood contexts. First, the racial disparity in long-term exposure to the most disadvantaged neighborhoods throughout childhood was more pronounced than the racial disparity in neighborhood environments at any single age. Second, for both blacks and nonblacks, neighborhood context appears to have been a fairly transient ecological setting, with many families moving between different neighborhood environments (see also Briggs and Keys 2009; Quillian 2003; Timberlake 2007). This dynamic neighborhood selection forcefully demonstrates the need for IPT-weighted estimation.

Weights

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

Neighborhood Effect Estimates

Table 5 shows unadjusted, conventional regression-adjusted and stabilized IPT-weighted estimates for the effect of duration-weighted exposure to different neighborhood contexts on the probability of high school graduation. The unadjusted logit estimates suggest that long-term exposure to disadvantaged neighborhoods was strongly and negatively related to high school graduation for both black ($\theta_1^{u}=-0.703$, $p < 0.001$) and nonblack ($\theta_1^{u}=-0.581$, $p <$ 0.001) children.[12] Specifically, the unadjusted point estimates indicate that long-term exposure to neighborhoods in quintile q of the composite disadvantage distribution rather than neighborhoods in the less disadvantaged quintile $q - 1$ was associated with about 50% lower odds of graduation for blacks (exp(−0.703) = 0.495) and about 45% lower odds for nonblacks (exp(−0.581) = 0.559). These estimates are biased, however, because they do not adjust for nonrandom selection into different neighborhood contexts.

The next set of results comes from conventional logit models that condition on duration-weighted exposure, all covariates measured at baseline, and time-varying covariates averaged over ages 2 to 17. These adjustments substantially reduce the estimated effect of long-term exposure to neighborhood disadvantage for both blacks ($\theta_1^{r}=-0.416$, $p = 0.034$) and nonblacks ($\theta_1^{r}=-0.212$, $p = 0.091$). The problem with these estimates is that the models from which they are derived include all time-varying covariates as regressors. Because neighborhood effects are likely mediated by time-varying characteristics of the family, these regression-adjusted estimates are biased due to over-control of indirect pathways and collider-stratification.

The stabilized IPT-weighted estimates come from logit models for high school graduation fit to the weighted pseudo-population in which neighborhood exposure status at each wave is independent of prior time-varying covariates. The regression-adjusted and stabilized IPT-weighted estimates differ only in the way they adjust for confounding by time-varying factors. Under the assumptions of no unmeasured confounders, no model misspecification, and positivity, IPT weighting—but not conventional regression—provides unbiased

12Superscripts on the theta parameters are used to distinguish the estimand defined in Model (2) from the different realized estimates reported in this section.

estimates of average causal effects. The stabilized IPT-weighted estimates indicate that living in a more disadvantaged neighborhood throughout childhood had a substantial negative effect on the chances of high school graduation among blacks ($\theta_1^w = -0.525$, p = 0.006) and nonblacks ($\theta_1^w = -0.274$, p = 0.033). For blacks, sustained exposure to the most disadvantaged quintile, of U.S. neighborhoods between ages 2 and 17 compared to residence, on average, in 3rd quintile neighborhoods reduced the odds of high school graduation by about 65% ($\exp((5-3) \times (-0.525)) = 0.349$). For nonblacks, these estimates indicate that long-term exposure to the most disadvantaged neighborhoods reduced the odds of high school graduation by about 40% compared to residence, on average, in 3rd quintile neighborhoods ($\exp((5-3) \times (-0.274)) = 0.578$). Compared to growing up in the least disadvantaged quintile of neighborhoods, sustained exposure to the most disadvantaged neighborhoods reduced the odds of high school graduation by nearly 90% for blacks ($\exp((5-1) \times (-0.525)) = 0.122$) and by about 70% for nonblacks ($\exp((5-1) \times (-0.274)) = 0.334$). The stabilized IPT-weighted estimates for the effect of duration-weighted exposure to neighborhood disadvantage on high school graduation are substantially larger than corresponding estimates from conventional regression models.[13]

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

IPT weighting allows for improved adjustment of observed confounding when risk factors for selection into different neighborhood contexts are also intermediate variables. Even though IPT-weighted estimation avoids the problems associated with conditioning on observed time-varying confounders, selection bias may still occur if there are unobserved factors that simultaneously impact decisions about where to live and the chances that a child graduates from high school. The assumption of no unobserved confounding is not testable with the observed data, but we address this challenge by adjusting for an extensive set of observed covariates. A second threat to the validity of causal inferences based on IPT-weighted estimates is the possibility that the treatment models are misspecified. Extensive experimentation with different specifications, however, indicates that our effect estimates are remarkably robust—estimated neighborhood effects hardly change across 14 different treatment model specifications and remain statistically significant (see Appendix C, Table C.1). Related to correct model specification, IPT weighting also requires that there be a positive probability of treatment for every level and combination of prior confounders. Since the U.S. does not formally restrict neighborhood choice based on economic or demographic characteristics, there is no reason to expect zero treatment probabilities in subgroups of children defined by their confounder history, except for the inherent limitations of sampling.

[13]Conventional logit models for high school graduation that use point-in-time measurements of neighborhood exposure status (at age 14) yield regression-adjusted estimates that are even smaller than those reported here (results not shown, available upon request). These models replicate the analytic strategy most often used in prior research and thus provide further evidence that past studies severely underestimated the effect of sustained exposure to neighborhood disadvantage.

Descriptive analyses of the empirical treatment distribution indicate that all exposure categories occur with positive probability across levels of several key confounders (see Appendix C, Tables C.2 and C.3).

DISCUSSION

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

Our results indicate that sustained exposure to disadvantaged neighborhoods—characterized by high poverty, unemployment, and welfare receipt, many female-headed households, and few well-educated adults—throughout the entire childhood life course has a devastating impact on the chances of graduating from high school. The effect estimates presented in this study suggest a more substantial influence for neighborhood context than estimates reported in prior research (e.g., Brooks-Gunn, Duncan, Klebanov, and Sealand 1993; Crane 1991; Ginther, Haveman, and Wolfe 2000; Harding 2003). For example, Harding (2003) reports propensity score matching estimates from the PSID indicating that exposure to high-poverty neighborhoods (>20% poverty) during adolescence, compared to living in low-poverty neighborhoods (<10% poverty), reduces the odds of high school graduation by about 50% for both blacks and nonblacks. Brooks-Gunn, Duncan, Klebanov, and Sealand (1993), also using PSID data, provide regression-adjusted estimates based on point-in-time measures of neighborhood context that suggest that moving from a less disadvantaged neighborhood (5% with incomes <$10,000, 60% with incomes >$30,000) to a more disadvantaged neighborhood (30% with incomes <$10,000, 10% with incomes >$30,000) is associated with only 4% lower odds of high school graduation for blacks and 30% lower odds for nonblacks. By contrast, similar comparisons based on the IPT-weighted estimates reported above indicate that *sustained* exposure to disadvantaged neighborhoods is associated with about an 80% decrease in the odds of high school graduation for blacks ($\exp((5-2) \times (-0.525)) = 0.207$) and close to a 60% decrease for nonblacks ($\exp((5-2) \times (-0.274)) = 0.439$).

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

The evidence presented in this study suggests that a temporal framework is crucial for understanding neighborhood effects on the educational outcomes of children. Most past research focused on the contemporaneous effects of neighborhood disadvantage and did not capture the cumulative impact of growing up in America's most disadvantaged communities. This cumulative effect is more consistent with Wilson's (1987) foundational arguments regarding the consequences of spatially concentrated poverty, which motivate nearly all recent studies of neighborhood effects. Our results demonstrate the importance of the neighborhood environment throughout childhood for one important educational outcome, high school graduation. These findings resonate with evidence from several studies suggesting that residence in disadvantaged neighborhoods may impact the cognitive development of children years or even generations later (Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert Forthcoming). This emerging body of research indicates that the developmental impact of neighborhoods should be studied as a longitudinal process (Crowder and South 2010; Quillian 2003; Sampson, Sharkey, and Raudenbush 2008; Sharkey and Elwert Forthcoming).

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

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

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

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APPENDIX A: NEIGHBORHOOD DISADVANTAGE INDEX

Table A.1

Component weights and correlations from principal component analysis (PCA) of neighborhood characteristics, U.S. Census data 1970–2000

Variable	1st PC	
	Weight	Corr
Percent poverty	0.408	0.861
Percent unemployed	0.371	0.783
Percent receiving welfare	0.412	0.868
Percent female-headed households	0.337	0.711
Percent without high school diploma	0.378	0.798
Percent college graduates	−0.348	−0.735
Percent mgr/prof workers	−0.385	−0.812
Component variance	4.449	
Proportion total variance explained	0.636	

Notes: (1) PCA is based on the correlation matrix; (2) analysis includes all tract-year observations between 1970 and 2000.

Table A.2

Neighborhood (NH) characteristics by disadvantage index quintiles, U.S. Census data 1970–2000

Variable	NH Disadvantage Index									
	1st Quintile		2nd Quintile		3rd Quintile		4th Quintile		5th Quintile	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Percent poverty	4.30	3.32	6.44	3.96	9.14	4.49	13.68	5.29	28.31	11.65
Percent unemployed	3.16	1.33	4.27	1.70	5.30	2.12	6.91	2.72	12.12	6.23
Percent receiving welfare	2.31	1.40	3.71	1.87	5.21	2.41	8.01	3.26	18.98	9.73
Percent female-headed households	11.70	6.64	14.22	7.56	16.05	8.57	20.20	10.15	37.89	17.99
Percent without high school diploma	10.55	5.46	20.54	7.48	29.02	9.93	37.99	12.25	50.53	14.17
Percent college graduates	39.74	13.00	20.83	7.96	13.69	6.69	10.05	5.86	6.93	4.86
Percent mgr/prof workers	30.67	6.83	19.47	4.34	14.16	3.88	10.98	3.55	7.60	3.38

APPENDIX B: TREATMENT MODELS

Table B.1

Models of neighborhood (NH) exposure status

Covariate	Blacks (person-years=20953)				Nonblacks (person-years=28084)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-invariant characteristics								
Gender								
Male	ref	ref	ref	ref	ref	ref	ref	ref
Female	−0.030	(0.050)	−0.039	(0.051)	0.044	(0.029)	0.042	(0.029)
Birth weight								
>=5.5 lbs	ref	ref	ref	ref	ref	ref	ref	ref
<5.5 lbs	−0.067	(0.090)	−0.068	(0.091)	0.060	(0.072)	0.084	(0.072)
Mother's marital status at birth								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	−0.239	(0.070)	−0.202	(0.069)	−0.129	(0.065)	−0.083	(0.066)
Mother's age at birth (years)	0.019	(0.006)	0.017	(0.006)	−0.010	(0.004)	−0.012	(0.004)
Year born								
1968–1969	ref	ref	ref	ref	ref	ref	ref	ref
1970–1972	−0.134	(0.076)	−0.144	(0.077)	0.012	(0.048)	0.023	(0.048)
1973–1975	−0.080	(0.083)	−0.117	(0.085)	0.026	(0.047)	0.020	(0.046)
1976–1978	−0.153	(0.076)	−0.194	(0.077)	0.026	(0.046)	0.015	(0.047)
Time								
wave 2–6	ref	ref	ref	ref	ref	ref	ref	ref
wave 7–11	−0.128	(0.042)	−0.060	(0.046)	−0.003	(0.023)	0.042	(0.027)
wave 12–17	−0.205	(0.042)	−0.067	(0.054)	−0.064	(0.023)	0.019	(0.030)

Table B.2

Models of neighborhood (NH) exposure status continued

Covariate	Blacks (person-years=20953)				Nonblacks (person-years=28084)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-dependent characteristics measured at baseline (k=0)								
Ordinal NH disadvantage	0.129	(0.030)	0.141	(0.028)	0.149	(0.022)	0.146	(0.022)
FU head’s education								
Less than high school	0.184	(0.068)	0.145	(0.070)	0.248	(0.041)	0.185	(0.041)
High school graduate	0.122	(0.066)	0.120	(0.066)	0.154	(0.035)	0.120	(0.036)
At least some college	ref	ref	ref	ref	ref	ref	ref	ref
FU head’s marital status								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	0.139	(0.078)	0.214	(0.081)	0.246	(0.079)	0.248	(0.081)
FU head’s employment status								
Unemployed	ref	ref	ref	ref	ref	ref	ref	ref
Employed	−0.047	(0.094)	−0.039	(0.098)	0.003	(0.065)	0.032	(0.065)
Home ownership								
Do not own home	ref	ref	ref	ref	ref	ref	ref	ref
Own home	−0.106	(0.054)	−0.066	(0.060)	−0.077	(0.033)	−0.080	(0.035)
Public assistance receipt in past year								
Did not receive AFDC	ref	ref	ref	ref	ref	ref	ref	ref
Received AFDC	0.403	(0.094)	0.330	(0.096)	−0.046	(0.089)	−0.073	(0.090)
FU income in past year (log $)	−0.005	(0.044)	0.051	(0.046)	−0.163	(0.056)	−0.093	(0.047)
FU head’s work hours in past year (hrs)	−0.005	(0.002)	−0.003	(0.002)	−0.003	(0.001)	−0.003	(0.001)
FU size	0.001	(0.010)	−0.013	(0.012)	0.074	(0.012)	0.066	(0.015)

Table B.3

Models of neighborhood (NH) exposure status continued

Variable	Blacks (person-years=20953)				Nonblacks (person-years=28084)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-dependent characteristics measured at wave k−1								
Ordinal NH disadvantage	2.193	(0.084)	2.150	(0.086)	2.113	(0.050	2.096	(0.050)
FU head’s marital status								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	-	-	−0.147	(0.127)	-	-	0.067	(0.116)
FU head’s employment status								

Variable	Blacks (person-years=20953)				Nonblacks (person-years=28084)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Unemployed	ref	ref	ref	ref	ref	ref	ref	ref
Employed	-	-	0.074	(0.100)	-	-	−0.113	(0.103)
Public assistance receipt in past year								
Did not receive AFDC	ref	ref	ref	ref	ref	ref	ref	ref
Received AFDC	-	-	0.106	(0.080)	-	-	−0.073	(0.105)
Home ownership								
Do not own home	ref	ref	ref	ref	ref	ref	ref	ref
Own home	-	-	0.103	(0.087)	-	-	0.044	(0.086)
FU income in past year (log $)	-	-	0.013	(0.041)	-	-	−0.032	(0.024)
FU head's work hours in past year (hrs)	-	-	−0.001	(0.002)	-	-	0.002	(0.001)
FU size	-	-	−0.029	(0.028)	-	-	−0.012	(0.034)

Table B.4

Models of neighborhood (NH) exposure status continued

Variable	Blacks (person-years=20953)				Nonblacks (person-years=28084)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-dependent characteristics measured at wave k								
FU head's marital status								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	-	-	−0.324	(0.133)	-	-	0.134	(0.136)
FU head's employment status								
Unemployed	ref	ref	ref	ref	ref	ref	ref	ref
Employed	-	-	−0.046	(0.109)	-	-	−0.367	(0.097)
Public assistance receipt in past year								
Did not receive AFDC	ref	ref	ref	ref	ref	ref	ref	ref
Received AFDC	-	-	0.141	(0.083)	-	-	0.066	(0.105)
Home ownership								
Do not own home	ref	ref	ref	ref	ref	ref	ref	ref
Own home	-	-	−0.254	(0.096)	-	-	−0.116	(0.086)
FU income in past year (log $)	-	-	−0.149	(0.047)	-	-	−0.165	(0.031)
FU head's work hours in past year (hrs)	-	-	−0.002	(0.002)	-	-	−0.002	(0.001)
FU size	-	-	0.026	(0.029)	-	-	0.018	(0.033)
Cumulative residential moves	-	-	−0.056	(0.012)	-	-	−0.023	(0.008)
Interactions								
FU head's marital status (wave k x k−1)	-	-	0.375	(0.182)	-	-	−0.125	(0.142)

Variable	Blacks (person-years=20953)				Nonblacks (person-years=28084)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
FU head's employment status (wave k x k −1)	-	-	−0.104	(0.121)	-	-	0.270	(0.108)

Notes: (1) models 1 and 2 are ordinal logistic regressions for the numerator and denominator of the stabilized IPT weight; (2) analyses are based on all person-years contributed by children present at age 1 in a PSID core family unit between 1968–1978; (3) coefficients and standard errors are combined estimates from 5 multiple imputation datasets.

APPENDIX C: MODEL SPECIFICATION AND POSITIVITY CHECKS

Table C.1

Stabilized IPT-weighted estimates by different specifications of the treatment model

Model	Description	Blacks (N=834)				Nonblacks (N=1259)			
		Weights		Effect estimates		Weights		Effect estimates	
		Mean	SD	Coef	SE	Mean	SD	Coef	SE
A	reported treatment model	1.04	0.61	−0.525	(0.190)	1.00	0.37	−0.274	(0.128)
B	(A) − birth weight, female	1.04	0.61	−0.537	(0.192)	1.00	0.37	−0.268	(0.128)
C	(B) − marital and employment interactions	1.04	0.61	−0.522	(0.192)	1.00	0.36	−0.284	(0.129)
D	(C) − covariates at wave k−1	1.04	0.61	−0.513	(0.190)	1.00	0.35	−0.280	(0.128)
E	(A) + income x family size interaction	1.03	0.61	−0.507	(0.193)	1.00	0.38	−0.288	(0.128)
F	(E) + income x homeowner interaction	1.03	0.61	−0.512	(0.195)	1.00	0.39	−0.282	(0.129)
G	(A) + income x time interaction	1.04	0.64	−0.536	(0.191)	1.00	0.39	−0.284	(0.128)
H	(G) + marital status x time interaction	1.04	0.64	−0.535	(0.192)	1.00	0.39	−0.279	(0.127)
I	(A) + quad. terms for income, family size	1.04	0.65	−0.540	(0.203)	1.00	0.42	−0.294	(0.130)
J	(I) + quad. terms for work hours, cum moves	1.04	0.65	−0.566	(0.202)	1.00	0.44	−0.282	(0.131)
K	(J) + income interaction (wave k x k−1)	1.04	0.65	−0.562	(0.201)	1.00	0.43	−0.279	(0.130)
L	(K) + family size interaction (wave k x k−1)	1.04	0.67	−0.579	(0.201)	1.00	0.43	−0.280	(0.131)
M	(L) + AFDC receipt interaction (wave k x k−1)	1.04	0.66	−0.588	(0.202)	1.01	0.44	−0.280	(0.131)
N	(M) + homeowner interaction (wave k x k−1)	1.05	0.70	−0.523	(0.209)	1.01	0.45	−0.280	(0.130)

Notes: (1) analyses based on children who were not lost to follow-up before age 20; (2) weights truncated at 1st and 99th percentiles; (3) coefficients and standard errors are combined estimates from 5 multiple imputation datasets.

Table C.2

Treatment distribution by selected covariates

Family Income	Homeownership	Marital Status	NH disadvantage quintiles				
			1	2	3	4	5
0–15K	Not homeowner	Unmarried	0.03	0.03	0.05	0.13	0.77
		Married	0.02	0.05	0.07	0.17	0.69
	Homeowner	Unmarried	0.02	0.05	0.07	0.18	0.68
		Married	0.05	0.07	0.15	0.26	0.48
15K–30K	Not homeowner	Unmarried	0.07	0.08	0.11	0.15	0.59
		Married	0.03	0.09	0.13	0.24	0.51
	Homeowner	Unmarried	0.04	0.08	0.11	0.19	0.57
		Married	0.06	0.09	0.19	0.30	0.36
30K–45K	Not homeowner	Unmarried	0.13	0.10	0.16	0.17	0.44
		Married	0.07	0.13	0.17	0.26	0.38
	Homeowner	Unmarried	0.07	0.15	0.21	0.20	0.37
		Married	0.09	0.15	0.25	0.26	0.24
45K–60K	Not homeowner	Unmarried	0.19	0.15	0.16	0.18	0.32
		Married	0.12	0.20	0.18	0.20	0.29
	Homeowner	Unmarried	0.18	0.26	0.23	0.14	0.18
		Married	0.16	0.22	0.23	0.20	0.19
60K+	Not homeowner	Unmarried	0.09	0.23	0.28	0.12	0.28
		Married	0.20	0.24	0.20	0.16	0.19
	Homeowner	Unmarried	0.40	0.21	0.10	0.07	0.22
		Married	0.38	0.22	0.16	0.14	0.10

Notes: (1) analyses are based on all person-year observations contributed by children present at age 1 in a PSID core family unit between 1968–1978; (2) cells contain the row proportion of person-years exposed to different quintiles of the neighborhood disadvantage index.

Table C.3

Treatment distribution by selected covariates continued

Cumulative moves	Education	Employment status	NH disadvantage quintiles				
			1	2	3	4	5
0	Less than HS	Unemployed	0.01	0.02	0.05	0.14	0.77
		Employed	0.04	0.05	0.14	0.23	0.55
	HS graduate	Unemployed	0.01	0.04	0.09	0.10	0.76
		Employed	0.05	0.12	0.21	0.30	0.32
	Some college	Unemployed	0.11	0.14	0.16	0.25	0.34
		Employed	0.22	0.22	0.20	0.19	0.17
1	Less than HS	Unemployed	0.02	0.05	0.06	0.11	0.75
		Employed	0.04	0.07	0.13	0.26	0.51
	HS graduate	Unemployed	0.02	0.07	0.16	0.17	0.58
		Employed	0.09	0.15	0.21	0.24	0.32
	Some college	Unemployed	0.09	0.11	0.13	0.21	0.47

Cumulative moves	Education	Employment status	NH disadvantage quintiles				
			1	2	3	4	5
		Employed	0.25	0.20	0.18	0.19	0.18
2	Less than HS	Unemployed	0.02	0.03	0.04	0.14	0.78
		Employed	0.03	0.05	0.14	0.24	0.53
	HS graduate	Unemployed	0.03	0.07	0.08	0.15	0.67
		Employed	0.08	0.14	0.20	0.24	0.34
	Some college	Unemployed	0.08	0.08	0.09	0.19	0.56
		Employed	0.24	0.22	0.23	0.16	0.15
3+	Less than HS	Unemployed	0.02	0.03	0.06	0.19	0.70
		Employed	0.04	0.10	0.14	0.24	0.47
	HS graduate	Unemployed	0.02	0.06	0.09	0.19	0.64
		Employed	0.13	0.15	0.18	0.22	0.32
	Some college	Unemployed	0.06	0.09	0.09	0.18	0.59
		Employed	0.20	0.19	0.24	0.19	0.19

Notes: (1) analyses are based on all person-year observations contributed by children present at age 1 in a PSID core family unit between 1968–1978; (2) cells contain the row proportion of person-years exposed to different quintiles of the neighborhood disadvantage index.

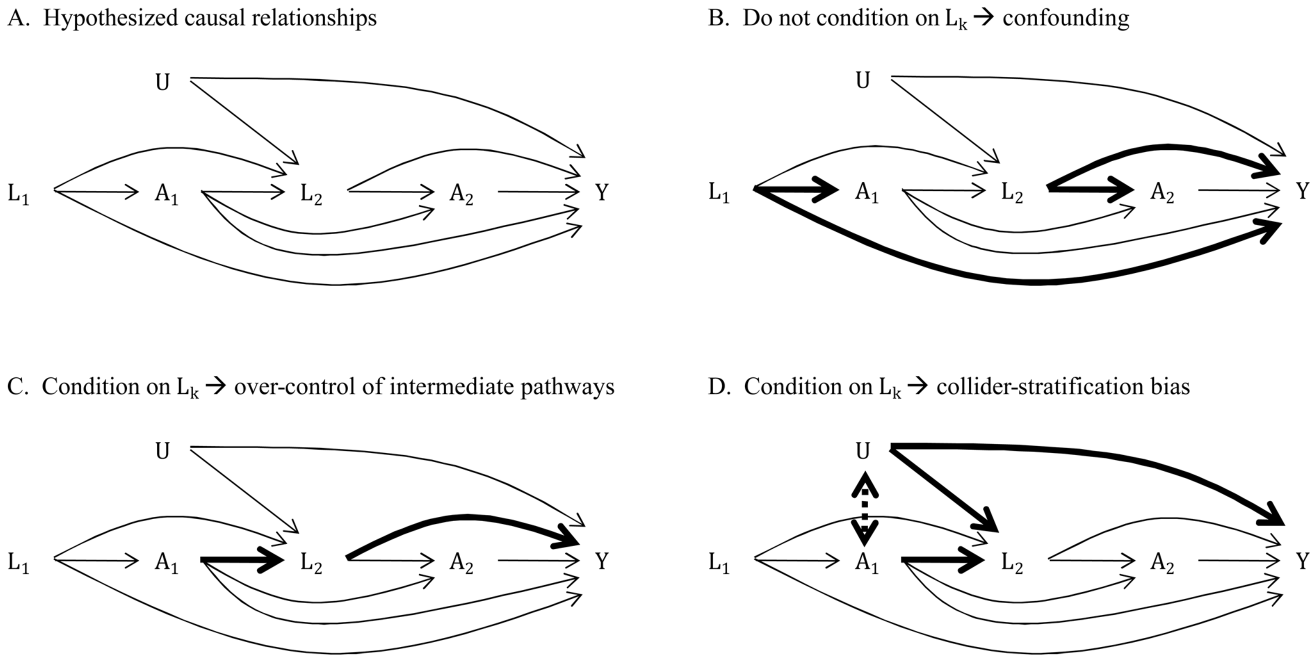

Figure 1.

Causal graphs for exposure to disadvantaged neighborhoods with waves of follow-up

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

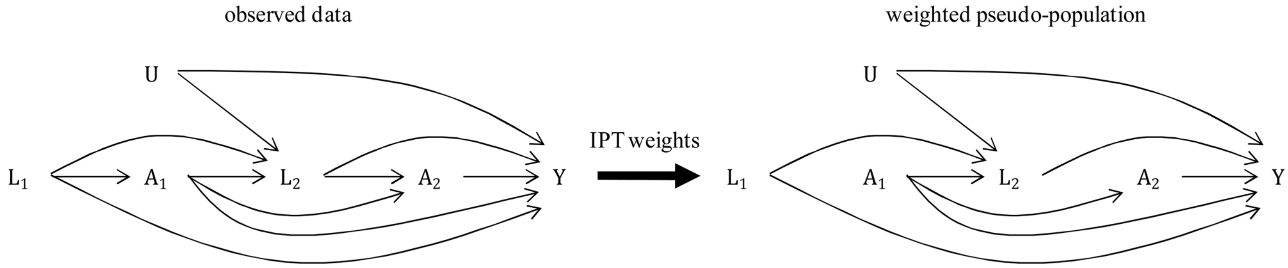

Figure 2.

Effect of weighting by the inverse probability of treatment (IPT)

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

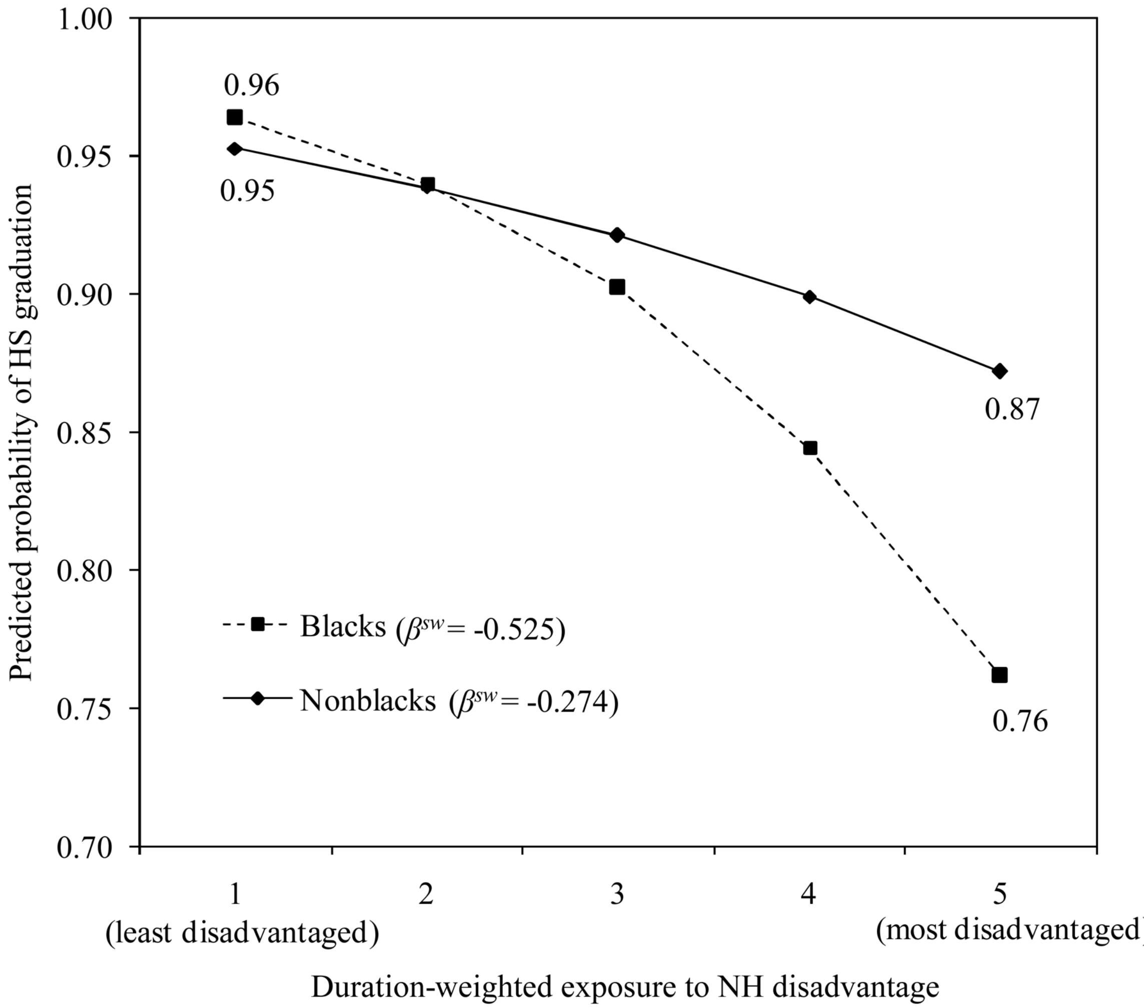

Figure 3.
Predicted probability of high school graduation by neighborhood (NH) exposure history

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Table 1

Time-invariant sample characteristics

Variable	Blacks (N=834)	Nonblacks (N=1259)
High school graduation, %		
Did not graduate high school	23.38	11.60
Graduated high school	76.62	88.40
Gender, %		
Male	52.04	50.99
Female	47.96	49.01
Birth weight, %		
>= 5.5 lbs	90.77	94.52
< 5.5 lbs	9.23	5.48
Mother’s marital status at birth, %		
Unmarried	41.97	5.56
Married	58.03	94.44
FU head’s education, %		
Less than high school	55.28	22.72
High school graduate	26.26	26.29
At least some college	18.46	50.99
Mother’s age at birth, mean	24.34	26.12

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

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Table 2

Time-dependent sample characteristics

Variable	Blacks (N=834)			Nonblacks (N=1259)		
	Age 1	Age 10	Age 17	Age 1	Age 10	Age 17
NH disadvantage index, %						
1st quintile	3.48	3.60	3.48	13.34	19.14	20.65
2nd quintile	3.24	3.72	6.00	19.46	18.67	21.84
3rd quintile	5.28	5.88	7.79	26.13	23.27	22.48
4th quintile	14.87	18.11	18.47	26.13	23.99	21.13
5th quintile	73.14	68.71	64.27	14.93	14.93	13.90
FU head’s marital status, %						
Unmarried	33.93	44.84	52.04	5.88	11.36	15.09
Married	66.07	55.16	47.96	94.12	88.64	84.91
FU head’s employment status, %						
Unemployed	27.22	32.61	33.09	8.10	8.02	9.69
Employed	72.78	67.39	66.91	91.90	91.98	90.31
Public assistance receipt, %						
Did not receive AFDC	81.06	75.66	82.37	96.27	96.19	97.93
Received AFDC	18.94	24.34	17.63	3.73	3.81	2.07
Homeownership, %						
Do not own home	69.66	53.48	50.12	40.19	22.32	20.73
Own home	30.34	46.52	49.88	59.81	77.68	79.27
FU income in $1000s, mean	19.68	25.04	27.45	32.59	46.65	57.50
FU head’s work hours, mean	30.08	26.82	27.51	42.65	40.84	40.68
FU size, mean	5.75	5.32	4.81	4.22	4.69	4.33
Cum. residential moves, mean	0.32	2.48	3.64	0.32	2.16	3.02

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

Table 3

Exposure to neighborhood (NH) disadvantage between age 2–17

Variable	Blacks (N=834)	Nonblacks (N=1259)
Duration-weighted exp. to NH disadvantage, %		
1.0–1.4 (least disadvantaged NHs)	0.84	12.31
1.5–2.4	2.64	20.57
2.5–3.4	6.24	30.26
3.5–4.4	24.82	28.12
4.5–5.0 (most disadvantaged NHs)	65.47	8.74
Num. of moves between exp. levels, %		
0	37.53	16.52
1	12.83	22.40
2	19.78	16.84
3+	29.86	44.24

Notes: (1) statistics reported for children not lost to follow-up before age 20 (first imputation dataset); (2) NH disadvantage quintiles are based on the distribution of the NH disadvantage index across all U.S. census tracts between 1970–2000.

Table 4

Stabilized treatment and attrition weights

Weight	Mean	SD	Percentiles			
			1st	25th	75th	99th
Blacks (N=834)						
Stabilized treatment weight (SW)	1.03	0.58	0.27	0.73	1.18	4.62
Stabilized attrition weight (CW)	1.00	0.12	0.76	0.92	1.06	1.44
SW × CW	1.04	0.61	0.26	0.71	1.18	4.80
Nonblacks (N=1259)						
Stabilized treatment weight (SW)	1.00	0.31	0.32	0.84	1.10	2.54
Stabilized attrition weight (CW)	1.00	0.14	0.71	0.93	1.03	1.70
SW × CW	1.00	0.37	0.32	0.81	1.10	3.00

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

Table 5

Effects of duration-weighted exposure to neighborhood disadvantage on high school graduation (log odds ratios)

Model	Blacks (N=834)		Nonblacks (N=1259)	
	Coef	SE	Coef	SE
Unadjusted	−0.703	(0.170) ***	−0.581	(0.109) ***
Regression-adjusted	−0.416	(0.196) *	−0.212	(0.125)
Stabilized IPT-weighted	−0.525	(0.190) **	−0.274	(0.128) *

Notes: (1) analyses based on children not lost to follow-up before age 20; (2) coefficients and standard errors are combined estimates from 5 multiple imputation datasets; (3) *p<0.05, **p<0.01, and ***p<0.001 for two-sided tests of no effect.