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

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

Keywords

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

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

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

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

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

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

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

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

NEIGHBORHOOD MECHANISMS AND EXPOSURE DURATION

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

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

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

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

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

PREVIOUS ESTIMATES OF NEIGHBORHOOD EFFECTS

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

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

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

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

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

DYNAMIC NEIGHBORHOOD SELECTION

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

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

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

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

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

METHODS

Data

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

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

Treatment

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

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

Covariates

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

Counterfactual Models for Time-Varying Neighborhood Exposures

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

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

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

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

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

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

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

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

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

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

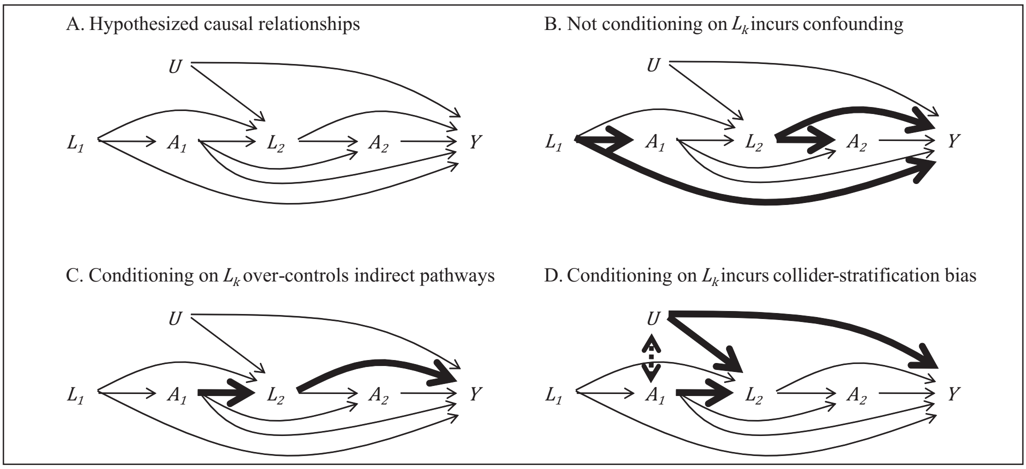

Figure 1. Causal Graphs for Exposure to Disadvantaged Neighborhoods with Two Waves of Follow-up

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

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

Failure of Conventional Regression Estimators

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

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

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

Estimation Using Inverse Probability of Treatment Weights

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

The IPT weight for the *i*th child is given by

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

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

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

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

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

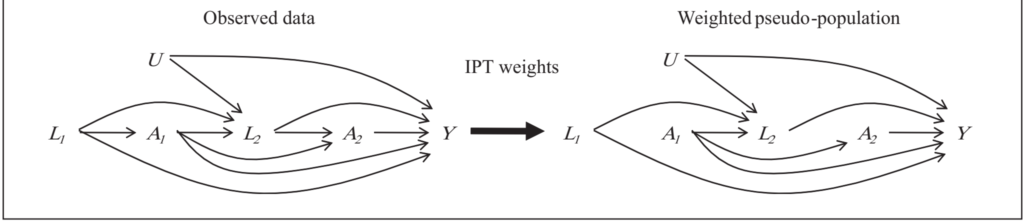

Figure 2. Causal Graphs for the Effect of Weighting by the Inverse Probability of Treatment (IPT)

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

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

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

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

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

Sample Attrition

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

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

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

RESULTS

Sample Characteristics

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

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

Longitudinal Neighborhood Exposure Patterns

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

Table 1. Time-Invariant Sample Characteristics

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

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

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

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

Table 2. Time-Dependent Sample Characteristics

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

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

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

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

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

Table 4. Stabilized Treatment and Attrition Weights

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

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

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

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

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

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

Weights

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

Neighborhood Effect Estimates

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

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

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

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

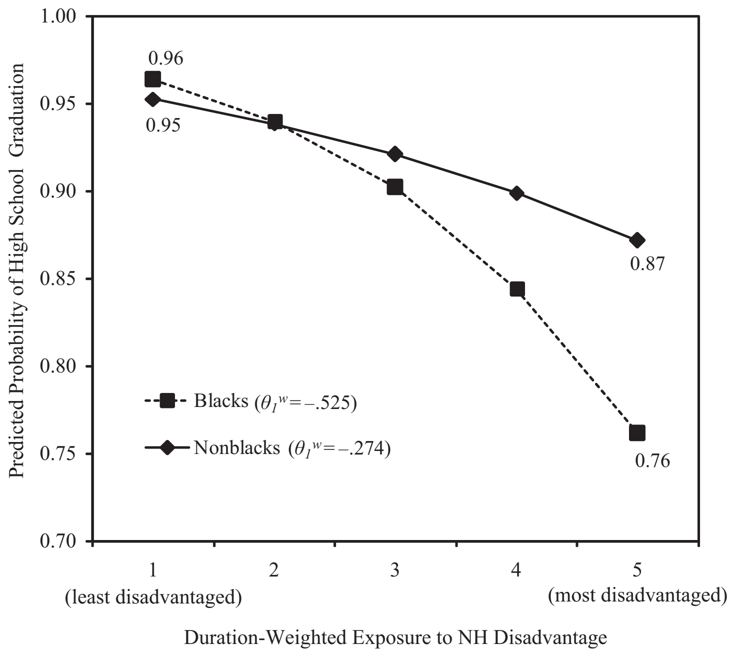

Figure 3. Predicted Probability of High School Graduation by Neighborhood Exposure History
Note: NH = Neighborhood

they had grown up in the most disadvantaged neighborhoods.

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

DISCUSSION

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

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

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

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

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

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

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

Authors' Note

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

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Notes

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

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

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

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