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Duration and Timing of Exposure to Neighborhood Poverty and the Risk of Adolescent Parenthood

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Abstract Theory suggests that the impact of neighborhood poverty depends on both the duration and timing of exposure. Previous research, however, has not properly analyzed the sequence of neighborhoods to which children are exposed throughout the early life course. This study investigates the effects of different longitudinal patterns of exposure to disadvantaged neighborhoods on the risk of adolescent parenthood. It follows a cohort of children in the PSID from age 4 to 19 and uses novel methods for time-varying exposures that overcome critical limitations of conventional regression when selection processes are dynamic. Results indicate that sustained exposure to poor neighborhoods substantially increases the risk of becoming a teen parent and that exposure to neighborhood poverty during adolescence may be more consequential than exposure earlier during childhood.

Keywords Neighborhood · Poverty · Adolescence · Parenthood · Childbearing

Introduction

Growing up in impoverished neighborhoods is thought to precipitate a number of problematic behaviors during adolescence (Jencks and Mayer 1990; Wilson 1987, 1996). Motivated by Wilson's (1987, 1996) forceful arguments about the impact of spatially concentrated poverty on family formation as well as widespread public concern over high teenage birth rates and the dire economic circumstances that frequently befall young parents and their children (Hayes 1987; Maynard 1996), much research on neighborhood effects has focused on adolescent parenthood. Although contemporary stratification theory holds that the social milieu in which

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children are embedded has strong effects on their sexual behavior and the consequences thereof, empirical research on this topic is conflicted. Some studies have found that teens who reside in poor neighborhoods are significantly more likely to become parents compared with their peers living in more-affluent areas (Harding 2003; South and Crowder 1999; Sucoff and Upchurch 1998), but other studies have reported no effect of neighborhood poverty on adolescent parenthood (Brooks-Gunn et al. 1993; Galster et al. 2007; Ginther et al. 2000; Thornberry et al. 1997).

Nearly all studies of neighborhood effects on family formation, however, have been limited by a set of problems related to the duration and timing of exposures to different neighborhood contexts. First, past research has often relied on point-in-time measurements of neighborhood context (e.g., Brooks-Gunn et al. 1993; Sucoff and Upchurch 1998), but the neighborhood environments to which children are exposed change over time (Quillian 2003; Timberlake 2007). Studies based on measurements of neighborhood context taken at a single age, or even averaged over several years, conflate the effect of recent exposure to neighborhood poverty with that of long-term neighborhood disadvantage. By failing to account for duration of exposure to different neighborhood environments throughout the early life course, previous studies may have understated the impact of neighborhood poverty on adolescent parenthood.

Second, research on social stratification and child development suggests that the effects of neighborhood poverty also depend on the timing of exposure (Darling and Steinberg 1997; Duncan et al. 1998), but few studies have investigated whether residence in poor neighborhoods during childhood versus adolescence has heterogeneous effects on later outcomes. To the extent that the impact of neighborhood poverty is lagged, cumulative, or heterogeneous during different stages of development, the results of previous empirical research may provide a misleading representation of the longitudinal process through which neighborhoods affect adolescent sexual behavior.

Consideration of exposure duration and timing reveals a third limitation of previous research: the improper conceptualization and analysis of neighborhood selection, where confusion emanates from time-varying characteristics of the family environment, such as parental income and employment status. Some studies have treated these factors as confounders that require statistical adjustment (Ginther et al. 2000; Harding 2003; South and Crowder 1999); other studies have viewed them as mediators for the effect of neighborhood context that must not be controlled (Brewster 1994; Brewster et al. 1993). In fact, both perspectives are correct, since selection into different neighborhoods is *dynamic* and depends in part on transitory characteristics of the family that are themselves affected by past neighborhood conditions. That is, time-varying family characteristics simultaneously mediate the effect of past exposures and confound the effect of future exposures to neighborhood poverty. This dynamic selection process poses considerable methodological difficulties. In particular, when time-varying confounders are affected by past neighborhood context, conventional regression adjustments for observed selection remove part of the total neighborhood effect that operates indirectly through the family environment. Previous empirical studies have relied almost exclusively on regression adjustments for observed selection and thus have likely provided biased estimates of neighborhood effects, even when neighborhood context was measured longitudinally (e.g., Ginther et al. 2000; South and Crowder 2010).

Building on prior research investigating the temporal dimensions of neighborhood effects (Sampson et al. 2008; Sharkey and Elwert 2011; Wodtke et al. 2011), this study examines the longitudinal impact of neighborhood poverty on adolescent parenthood. Specifically, it examines the differential effects of sustained versus transitory exposure to neighborhood poverty and of exposure during childhood versus adolescence. To this end, I follow a cohort of children in the Panel Study of Income Dynamics from age 4 to 19, measuring neighborhood context and an extensive set of covariates once per year, and use inverse probability of treatment (IPT) weighting, which properly adjusts for dynamic neighborhood selection, to estimate time-dependent effects of neighborhood poverty on adolescent parenthood.

In the next section, I begin with a discussion of the mechanisms through which poor neighborhoods are hypothesized to affect adolescent sexual behavior, focusing on the importance of both duration and timing of exposure. Next, I review research on the determinants of living in poor neighborhoods and argue that many factors linked to neighborhood selection are themselves affected by prior neighborhood conditions. Following this discussion, I explain the limitations of conventional regression when time-varying factors are simultaneously confounders and mediators for the effect of neighborhood poverty, describe how IPT weighting overcomes these problems, and compute several different estimates of neighborhood effects on adolescent parenthood. Results from this analysis indicate that sustained exposure to neighborhood poverty substantially increases the risk of becoming an adolescent parent, that exposure to poor neighborhoods during adolescence may have a greater effect than exposure earlier in childhood, and that the effect of neighborhood poverty is mediated by time-varying characteristics of the family environment. Furthermore, results suggest that conventional regression models provide estimates that understate the total effect of long-term neighborhood disadvantage, regardless of whether neighborhood context is measured longitudinally or at a single point in time.

Temporal Dimensions of Neighborhood Effects

The mechanisms through which poor neighborhoods are hypothesized to increase the risk of adolescent parenthood include social isolation and alternative, or heterogeneous, local cultures (Anderson 1991; Harding 2010; Wilson 1987), a breakdown of collective trust among resident adults (Sampson 2001), high levels of violent crime (Harding 2009), and institutional resource deprivation (Brooks-Gunn et al. 1997). These contextual factors are thought to shape adolescents' knowledge of the reproductive process, perceptions about access to contraception, expectations for the future course of their adult lives, and beliefs about the social, economic, and psychological costs associated with early parenthood.

The most extensive account of how concentrated neighborhood poverty affects family formation comes from Wilson (1987, 1996). Because of deindustrialization and the out-migration of middle-class families, children who grow up in poor neighborhoods are socially isolated from adult role models who have achieved a degree of economic and familial security through "mainstream" channels: formal education, employment, marriage, and delayed parenthood. The absence of successful role models and infrequent contact with stable two-parent families curb the educational and career aspirations of resident children and promotes the perception that adolescent parenthood is a normative life-course event. In addition, spatially concentrated poverty and concomitant social isolation are thought to engender alternative subcultures among peer groups that encourage early sexual activity and adolescent childbearing (Anderson 1991; Massey and Denton 1993). Implicit in social and cultural isolation theories is that children must be exposed to these harmful social conditions for an extended period for neighborhoods to exert their hypothesized effects. For example, long-term exposure to poor neighborhoods is likely necessary for children to sufficiently absorb the alternative local values from peers and resident adults. Children may also be more likely to develop feelings of fatalism and hopelessness about their life chances if they are exposed to poor neighborhoods for an extended rather than transitory period. Those who experience only short-term residence in high-poverty neighborhoods may be able to remain optimistic about the future and draw on "mainstream" values learned elsewhere.

Social disorganization theories are also premised on long-term exposure to poor neighborhoods. These models describe how collective distrust and high rates of violent crime in poor neighborhoods make it extremely difficult for adults to effectively parent their children (Browning et al. 2004, 2005; Harding 2010; Sampson 2001). For example, in neighborhoods where violence is widespread, parents are primarily concerned with keeping their children safe and devote less effort to monitoring romantic relationships (Harding 2010). If families reside in poor, violent neighborhoods for an extended period, parents' attention may rarely be focused on preventing children from engaging in early or unsafe sexual activity, thereby elevating the chances of adolescent parenthood. From a social disorganization perspective, the cumulative risk of becoming an adolescent parent increases with the amount of time children live in social environments lacking adequate supervision.

According to resource deprivation theories, the dearth of local services in disadvantaged neighborhoods also complicates effective parenting (Brooks-Gunn et al. 1997; Wilson 1987). With limited access to recreational facilities, childcare centers, and after-school programs, working parents in poor neighborhoods may often be forced to leave their children unsupervised. It is important to account for duration of exposure because the regular, long-term absence of adult supervision necessarily increases the cumulative risk of adolescent parenthood beyond that associated with temporary, short-term disruptions in parental supervision. School quality is another important dimension of resource deprivation directly linked to the socioeconomic composition of neighborhoods. Attending schools with overcrowded classrooms, poor instructional resources, and dilapidated facilities inhibits the development of positive educational and occupational aspirations. The longer children are exposed to such negative school environments, the more their aspirations are likely to be curbed; consequently, the perceived costs of adolescent parenthood may decrease with the duration of time spent in poor neighborhoods.

Research on social stratification and child development suggests that the effects of neighborhood poverty depend on not only the duration of exposure but also the timing of exposure during different developmental periods. Because adolescence is the stage at which a child's social world begins to incorporate the outside community (Darling and Steinberg 1997), living in poor neighborhoods during this period may have the greatest impact on the risk of teen parenthood, especially if neighborhood effects operate primarily through peer socialization mechanisms. Furthermore, children are not directly at risk of becoming parents until they reach adolescence, so exposure to poor neighborhoods prior to this developmental stage may be less consequential. On the other hand, research suggests that children are particularly sensitive to socioeconomic inputs during early childhood (Duncan et al. 1998; Heckman 2006). To the extent that cognitive abilities, academic achievements, and career aspirations are shaped by neighborhood conditions during childhood, early-life contextual exposures may affect the perceived costs of becoming a parent later in adolescence. Although extant theory and research do not provide a clear account of how neighborhood effects operate across the early life course, the developmental perspectives reviewed here suggest that different patterns of exposure to neighborhood poverty during childhood versus adolescence may have heterogeneous effects on the risk of teen parenthood.

Dynamic Neighborhood Selection

Consider a family whose primary earner is laid off from work. This event may precipitate movement to a new neighborhood with inexpensive housing, more low-income residents, and fewer quality employment opportunities within reasonable commuting distance. Because of the disadvantaged social conditions and inconvenient physical location of the neighborhood, adults in this family may have a difficult time finding new jobs. Long-term unemployment further reduces family income and depletes savings. With few economic resources to draw upon, the chances of this family relocating to a more-advantaged neighborhood become increasingly slim. This scenario demonstrates the process of dynamic neighborhood selection, whereby time-varying family characteristics, such as parental employment and income, influence where a family lives in the future but are also shaped by past neighborhood conditions.

Previous research indicates that socioeconomic characteristics, family structure, and race are important determinants of the neighborhood environment in which a family resides. Neighborhood attainment is linked to parental education, employment, income, public assistance receipt, and homeownership, such that more-affluent and better-educated parents are much less likely to live in poor neighborhoods (Sampson and Sharkey 2008; South and Crowder 1997). Parental marital status and family size also affect neighborhood attainment: the risk of moving to a poor neighborhood is especially high for children of parents who recently divorced (Sampson and Sharkey 2008; South and Crowder 1997; South and Deane 1993; Speare and Goldscheider 1987). In addition to family structure and socioeconomic characteristics, race is also closely related to neighborhood attainment. Because of widespread racial discrimination in the real estate industry and strong preferences among whites to live with same-race neighbors (Charles 2003; Yinger 1995), blacks are substantially more likely to live in poor neighborhoods, regardless of their personal economic resources (Iceland and Scopilliti 2008).

Although previous research demonstrates that a variety of socioeconomic and demographic characteristics affect neighborhood selection, evidence also indicates that some of these characteristics are in turn affected by neighborhood conditions. Residence in disadvantaged neighborhoods is thought to influence both the structure and economic foundations of the family (Wilson 1987, 1996). For example, the decline of manufacturing and suburbanization of employment have substantially reduced the number of jobs available to residents of poor urban neighborhoods; consequently, this population is more likely to experience long spells of unemployment and subpoverty incomes (Fernandez and Su 2004; Wilson 1987, 1996). Furthermore, the limited employment prospects in disadvantaged neighborhoods may lead to marital instability, delayed marriage, and increasing nonmarriage in these communities (South and Crowder 1999; Wilson 1987).

In sum, a number of time-varying family characteristics—parental employment, income, and family structure, in particular—affect future neighborhood selection and are themselves affected by past neighborhood contexts. Because these factors also influence the risk of adolescent parenthood (Duncan et al. 1998; McLanahan and Percheski 2008), they are simultaneously confounders for the effect of future exposures and mediators for the effect of past exposures to neighborhood poverty. Time-varying confounders affected by past levels of a time-varying treatment pose several difficult problems for conventional regression models. In the next section, I explain the limitations of conventional regression for estimating time-dependent neighborhood effects and describe novel methods designed specifically to resolve these problems.

Methods

Data

This study uses data from the Panel Study of Income Dynamics (PSID), a longitudinal survey that began in 1968 with a nationally representative sample of about 4,800 families. These families, together with new families formed by sample members over time, were interviewed annually from 1968 to 1997 and biennially thereafter. The analytic sample for this study consists of 8,757 respondents who at age 4 lived in a PSID family between 1968 and 1989. These respondents were followed from age 4 until they became a parent, turned 20 years old, dropped out of the PSID, or reached administrative end of follow-up (defined to be the 1997 wave of the PSID). Of the initial analytic sample, 6,242 respondents remained in the study until age 12, the beginning of the risk period for adolescent parenthood. Baseline is defined to be the PSID wave, indexed by $k \in \{0, 1, \ldots K\}$, in which a respondent is age 4. From baseline ($k = 0$) until the end of follow-up ($K = 15$), neighborhood context and a set of confounders are measured every year. The timing of adolescent parenthood for both male and female respondents is determined from the PSID childbirth history file, which uses retrospective reports to measure the date of childbirth events for all household members aged 12 to 44 at the time of the interview.[1,2]

[1] Childbearing data for males are likely of poorer quality than those for females. In general, evidence indicates that males are simply not as accurate as females in their fertility reporting (Nock 1998; Vere 2008).

[2] This study analyzes the risk of both marital and nonmarital adolescent parenthood. Parallel analyses of neighborhood effects on only nonmarital adolescent parenthood yield findings nearly identical to those reported here.

Measurements of neighborhood poverty, the exposure of interest in this study, are derived from the GeoLytics Neighborhood Change Database (NCDB) (GeoLytics 2003). The NCDB contains tract-level data from the 1970–2000 censuses with tract boundaries and measures defined consistently across time. Linear interpolation is used to impute tract characteristics for intercensal years. Although neighborhood disadvantage can be measured with a wide variety of indicators, I focus on the tract poverty rate because previous research suggests that it is closely related to the underlying social processes thought to be responsible for neighborhood effects (e.g., Cook et al. 1997) and because it has a straightforward interpretation, unlike multidimensional scales. However, the poverty rate is not a direct measure of proximate mechanisms, and it is highly collinear with other dimensions of the neighborhood environment, such as aggregate family structure and residential stability, which leads to some ambiguity in the interpretation of estimated neighborhood effects.

From these data, a time-varying, three-level ordinal treatment variable is defined at each wave based on the poverty rate of the census tract in which a child lived. Specifically, treatment is coded 1, 2, or 3 to indicate that a child lived in a low-poverty (<10 %), moderate-poverty (10-20 %), or a high-poverty (>20 %) neighborhood, respectively. In the analysis that follows, this ordinal treatment variable is used to generate duration-weighted measures of exposure to different levels of neighborhood poverty throughout childhood and adolescence. Although a variety of different thresholds have been used to define moderate- and high-poverty neighborhoods (e.g., Harding 2003; Jargowsky 1997; Wilson 1987), exploratory analyses indicated that measures based on the 10 % and 20 % cutoffs best capture the relationship between neighborhood poverty and adolescent parenthood.

The time-invariant baseline covariates in this study are gender, race, birth weight, mother's age and marital status at the time of a subject's birth, and the completed education of the family head.[3] Dummy variables are used to indicate female gender and low birth weight (<2500 g). Mother's age at the time of a respondent's birth is measured in years, and her marital status at this juncture is dummy coded as 1 for married and 0 for unmarried. Family head's education, measured at or just prior to baseline, is expressed as a series of dummy variables for less than high school, high school graduate, and at least some college. Race is dummy coded as 1 for black and 0 for nonblack. This study also adjusts for a set of time-varying covariates measured at each wave: the marital status, employment status, and work hours of the family head, as well as family income, family size, homeownership, residential mobility, and welfare receipt. The marital and employment status of the family head are coded as dummy variables. The average number of hours worked per week during the previous year is used to measure the family head's work hours. Family size is the number of people living in a subject's household; homeownership is coded 1 if a family owns the residence they occupy and 0 otherwise; welfare receipt is also expressed as a dummy variable indicating whether a family received income from Aid to Families with Dependent Children in the past year; and family income is the inflation-adjusted taxable income earned by all family members in the previous year. Residential mobility is defined as the cumulative number of times that a subject moved prior to

[3] The family head is the person with primary financial responsibility for the family and must be at least 16 years old, unless this person is female and lives with a husband, in which case the husband is designated as family head.

wave k. Missing treatment and covariate data are simulated by multiple imputation with five replications (Rubin 1987).[4]

Counterfactual Models for Neighborhood Effects on Adolescent Parenthood

This section draws on potential outcomes notation for time-varying treatments and failure-time outcomes to define the causal effects of neighborhood poverty on adolescent parenthood (Robins 1987). Let $A_k \in \{1, 2, 3\}$ be the ordinal treatment variable for exposure at wave k to a neighborhood with low, moderate, or high levels of poverty; and define $\overline{A}_k = (A_1, \ldots, A_k)$ as the sequence of exposures to different levels of neighborhood poverty through wave k (overbars are used to denote treatment or covariate history).[5] Let $\overline{a} = \overline{a}_K$ represent a particular treatment regime from one wave post-baseline through the end of follow-up, where a subject is said to follow the treatment regime $\overline{a}$ if s/he is exposed to the specified level of neighborhood poverty, a_k, at each wave prior to becoming an adolescent parent. Then, let S equal the observed time between baseline and the point at which a subject becomes a parent, and define $S(\overline{a})$ to be the potential time until parenthood had s/he, possibly contrary to fact, followed the treatment regime $\overline{a}$. For each subject, only the one failure time where $S(\overline{a}) = S$ is observed and the other $S(\overline{a})$ are counterfactuals. Three discrete-time logit models based on the potential failure times are considered in this analysis. For these models, the potential failure times are transformed into wave-specific failure indicators, $Y_k(\overline{a})$, equal to 1 if $k < S(\overline{a}) < k + 1$ and 0 otherwise. That is, $Y_k(\overline{a})$ indicates whether a subject would have become a parent during wave k had s/he experienced the history of neighborhood poverty $\overline{a}$.

To investigate the effects of long-term exposure to neighborhood poverty, the first logit model expresses the risk of adolescent parenthood as a function of the cumulative proportion of time that subjects live in low-, moderate-, and high-poverty neighborhoods. This model can be written as

$$\begin{aligned}\operatorname{logit}\left(P\left(Y_k(\overline{a}) = 1 \middle| k > 7, \overline{Y}_{k-1}(\overline{a}) = 0\right)\right) &= \beta_0(k) + \beta_1\left(\frac{\sum_{t=1}^{k-1} I(a_t = 2)}{k-1}\right) \\ &\quad + \beta_2\left(\frac{\sum_{t=1}^{k-1} I(a_t = 3)}{k-1}\right),\end{aligned} \tag{1}$$

where $P\left(Y_k(\overline{a}) = 1 \middle| k > 7, \overline{Y}_{k-1}(\overline{a}) = 0\right)$ is the probability of becoming an adolescent parent at wave $k > 7$ (i.e., at age 12 or later) had subjects followed the neighborhood exposure trajectory $\overline{a}$ through the prior wave, and $\beta_0(k)$ is the log odds of becoming a parent at wave k had subjects previously lived only in low-poverty neighborhoods. The functions $\sum_{t=1}^{k-1} I(a_t = 2)/(k-1)$ and $\sum_{t=1}^{k-1} I(a_t = 3)/(k-1)$ give the proportion

[4] Neighborhood effect estimates reported in this article are combined estimates from the five multiple imputation data sets. For simplicity, descriptive statistics are based on only the first imputed data set. A small number of subjects who remained in the PSID through adolescence but are missing childbirth history information are treated as though they left the study at age 11 and are incorporated into the adjustment for censoring.

[5] Neighborhood poverty at baseline, A_0, is not used to estimate causal effects because the covariate data needed to model selection into treatment at this time point are not available. Instead, this measure is treated as a confounder for the effects of later treatments and absorbed into the vector of baseline controls.

of time that subjects live in moderate- and high-poverty neighborhoods, respectively, from one wave post-baseline (i.e., age 5) through wave $k-1$, and the beta coefficients associated with these functions are log odds ratios. Specifically, $\exp(\beta_1)$ is the multiplicative effect on the odds of adolescent parenthood associated with sustained exposure to moderate-poverty neighborhoods. The multiplicative effect of sustained exposure to high-poverty neighborhoods is $\exp(\beta_2)$. Different weighted sums of the beta parameters give the effects of any other exposure trajectory.

To examine how the effects of neighborhood poverty depend on the timing of exposure during the course of development, a second model allows different effects for cumulative exposure during childhood versus adolescence. This model has form

$$\operatorname{logit}\left(P\left(Y_k\left(\overline{a}\right)=1\middle|k>7,\overline{Y}_{k-1}\left(\overline{a}\right)=0\right)\right)=\theta_0\left(k\right)+\theta_1\left(\frac{\sum_{t=1}^{6}I\left(a_t=2\right)}{6}\right)$$
$$+\theta_2\left(\frac{\sum_{t=1}^{6}I\left(a_t=3\right)}{6}\right)+\theta_3\left(\frac{\sum_{t=7}^{k-1}I\left(a_t=2\right)}{k-7}\right)+\theta_4\left(\frac{\sum_{t=7}^{k-1}I\left(a_t=3\right)}{k-7}\right), \quad (2)$$

where $P\left(Y_k(\overline{a})=1\middle|k>7,\overline{Y}_{k-1}(\overline{a})=0\right)$ and $\theta_0(k)$ are defined as previously, but rather than two functions for cumulative exposure to neighborhood poverty, as in Eq. (1), four functions are used to specify, separately for childhood and adolescence, the proportion of time that subjects live in moderate- and high-poverty neighborhoods. For example, the function $\sum_{t=1}^{6} I(a_t=2)/6$ gives the proportion of time between one wave post-baseline (age 5) and wave $k=6$ (age 10) spent in moderate-poverty neighborhoods, and $\sum_{t=7}^{k-1} I(a_t=2)/(k-7)$ gives the proportion of time lived in moderate-poverty neighborhoods from wave $k=7$ (age 11) through wave $k-1\geq 7$. The two functions for exposure to high-poverty neighborhoods are defined analogously. The log odds ratios associated with cumulative exposure to moderate- and high-poverty neighborhoods during childhood are given by θ_1 and θ_2, respectively, while the second set of coefficients, θ_3 and θ_4, capture the effects of cumulative exposure to different levels of neighborhood poverty during adolescence.

In addition to models that account for duration and timing of exposure, I also consider, for comparative purposes, a naïve model that links the risk of adolescent parenthood to a point-in-time measure of neighborhood poverty. This model can be written as

$$\operatorname{logit}\left(P\left(Y_k(\overline{a})=1\middle|k>7,\overline{Y}_{k-1}(\overline{a})=0\right)\right)=\eta_0(k)+\eta_1 I(a_7=2)+\eta_2 I(a_7=3), \quad (3)$$

where a_7 is the neighborhood poverty level at age 11. Equation (3) is based on the measurement strategy used in most prior studies of neighborhood effects. It ignores duration and timing of exposure and thus imposes severe constraints on the counterfactual probabilities. Only if neighborhood poverty at age 11 is assumed to represent a subject's complete exposure history can the parameters η_1 and η_2 be interpreted as the effects of sustained exposure to moderate- and high-poverty neighborhoods.

Equations (1)–(3) are referred to as marginal structural models in the causal inference literature (Robins et al. 2000). Their parameters can be identified from observational data under the assumption of sequential ignorability of treatment assignment. This assumption is formally expressed as

$$S(\overline{a}) \perp A_k \big| \overline{A}_{k-1}, \overline{L}_k, \overline{Y}_k(\overline{a}) = 0, \tag{4}$$

where $\overline{L}_k = (L_0, L_1, \ldots, L_k)$ represents observed covariate history through wave k, and $\perp$ denotes statistical independence. In words, Eq. (4) states that neighborhood poverty at wave k, A_k, is independent of the potential outcomes, $S(\overline{a})$, given prior neighborhood exposures, covariate history, and survival through wave k. This assumption is satisfied in observational studies if no unobserved factors affect both selection into poor neighborhoods and the risk of becoming an adolescent parent—that is, if there is no unobserved confounding of neighborhood poverty. In the next section, I show that even when the assumption defined in Eq. (4) is true, conventional regression models for the effects of neighborhood poverty are biased if there are time-varying covariates that simultaneously confound and mediate these effects.

Limitations of Conventional Regression Models

Consider the set of relationships depicted in Fig. 1, which shows a three-wave snapshot of the causal process hypothesized in this study. Prior exposures to neighborhood poverty have both direct effects on the chances of becoming an adolescent parent and indirect effects that operate through time-varying family characteristics. Selection into different neighborhood contexts at each wave is affected by observed time-varying factors, with no unobserved confounding of neighborhood poverty. Thus, Fig. 1 assumes that neighborhood selection is sequentially ignorable conditional on the observed past. Note that unobserved determinants of adolescent parenthood may affect time-varying covariates but not selection into neighborhoods.

To estimate the effects of neighborhood poverty on the risk of adolescent parenthood, the conventional regression approach involves fitting to the observed data a discrete-time logit model that conditions on confounder history. This model has form

$$\operatorname{logit}\Big(P\big(Y_k = 1 \big| k > 7, \overline{Y}_{k-1} = 0, \overline{A}_{k-1}, \overline{L}_{k-1}\big)\Big) = \alpha_0(k) + u\big(\overline{A}_{k-1}\big) + \varepsilon\big(\overline{L}_{k-1}\big), \tag{5}$$

where $\alpha_0(k)$ are wave-specific intercept terms, $u\left(\overline{A}_{k-1}\right)$ is a linear parametric function of neighborhood exposure history through wave $k-1$, and $\varepsilon\left(\overline{L}_{k-1}\right)$ is some parameterization

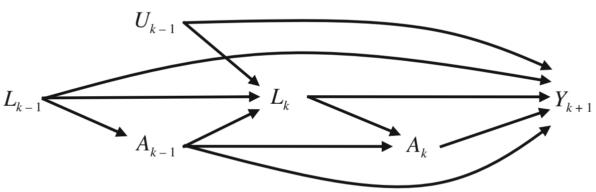

Fig. 1 Hypothesized causal relationships between neighborhood poverty, time-varying covariates, and adolescent parenthood. A_k = neighborhood poverty, L_k = observed time-varying covariates, U_k = unobserved factors, and Y_k = outcome

of confounder history. For example, to estimate the parameters in Eq. (1), $u(\overline{A}_{k-1})$ includes main effects for the proportion of time lived in moderate- and high-poverty neighborhoods through wave $k-1$, and the function $\varepsilon(\overline{L}_{k-1})$ typically includes main effects for the average of time-varying covariates from baseline through wave $k-1$ (e.g., South and Crowder 2010), although many different specifications are possible.

This modeling strategy has two problems when time-varying confounders in L_k are affected by prior exposure to neighborhood poverty. First, Fig. 2a shows that conditioning on time-varying covariates affected by past treatment "controls away" the indirect effects of treatment that operate through these factors. Second, because conditioning on the common effect of two variables induces an association between them, models that include time-varying confounders as regressors may introduce a nuisance association between past treatment and unobserved determinants of the outcome (Greenland 2003; Pearl 2000). This problem, known as collider-stratification bias, is depicted in Fig. 2b. Thus, even when there is no unobserved confounding of neighborhood poverty, conventional discrete-time logit models fail to recover the treatment effects of interest.

Inverse Probability of Treatment Weighting

The method of inverse probability of treatment (IPT) weighting overcomes the problems outlined in the previous section (Robins et al. 2000). It involves computing a set of weights that, when applied to the observed data, generate a pseudo-population in which treatment at each time period is independent of prior (observed) time-varying covariates. Then, to estimate the effects of neighborhood poverty on adolescent parenthood, conventional discrete-time logit models that do not condition on time-varying confounder history are fit to the weighted observations.

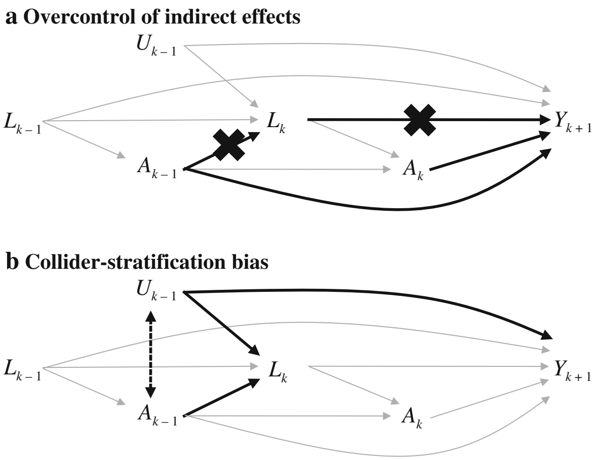

Fig. 2 Consequences of conditioning on time-varying confounders affected by past neighborhood context. A_k = neighborhood poverty, L_k = observed time-varying covariates, U_k = unobserved factors, and Y_k = outcome

The stabilized version of the IPT weight for the ith respondent at the kth follow-up wave is given by

$$sw_{ik} = \prod_{t=1}^{k-1} \frac{P\left(A_t = a_{it} \middle| \overline{A}_{t-1} = \overline{a}_{i(t-1)}, L_0 = l_0\right)}{P\left(A_t = a_{it} \middle| \overline{A}_{t-1} = \overline{a}_{i(t-1)}, \overline{L}_t = \overline{l}_{it}\right)}. \quad (6)$$

The denominator of the weight is the probability that respondent i is exposed to his/her observed level of neighborhood poverty at a given wave conditional on prior neighborhood exposures and time-varying covariates. The numerator, by contrast, is the conditional probability that a respondent is exposed to his/her observed level of neighborhood poverty at each time period given neighborhood exposure history and covariates measured only at baseline. The stabilized IPT weight varies around 1 based on the degree to which neighborhood selection is influenced by time-varying factors. By weighting each person-wave observation by sw_{ik}, treatment assignment at each wave is balanced across prior levels of observed time-varying covariates.

Figure 3 presents a stylized graph that illustrates the effect of IPT weighting: after the observed data are weighted by sw_{ik}, exposure to neighborhood poverty at each wave is independent of prior time-varying confounders. Conditioning on time-varying confounders, then, is no longer necessary, and conventional methods can be used with the weighted observations to estimate the neighborhood effects of interest.

The true stabilized IPT weights are unknown, but they can be estimated from data. For the three-level ordinal treatment, the denominator in Eq. (6) is estimated from an ordinal logistic regression model with form

$$\begin{aligned}\text{logit}\left(P\left(A_k > j \middle| \overline{A}_{k-1}, \overline{L}_k\right)\right) = \gamma_{0j}(k) + \gamma_1 A_{k-1} + \gamma_2 L_0 \\ + \gamma_3 L_{k-1} + \gamma_4 L_k, \qquad j = 1, \ldots, J-1\end{aligned} \quad (7)$$

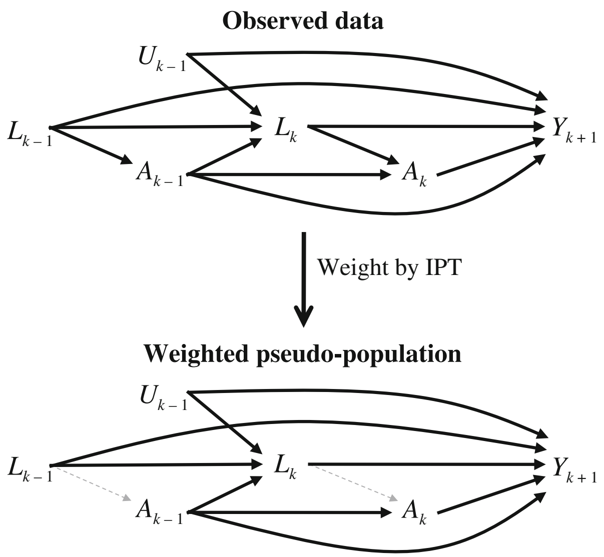

Fig. 3 Stylized graph illustrating the effect of weighting by the inverse probability of treatment on the joint distribution of neighborhood poverty, time-varying covariates, and the outcome. A_k = neighborhood poverty, L_k = observed time-varying covariates, U_k = unobserved factors, and Y_k = outcome

where $\gamma_{0j}(k)$ is a wave-specific intercept term for the jth cumulative logit. In Eq. (7), the level of neighborhood poverty to which a subject is exposed at each wave k is a function of neighborhood poverty at wave $k-1$, covariates measured at baseline, and time-varying covariates measured at waves k and $k-1$. The conditional probability in the numerator of the stabilized IPT weight is estimated from a similar model that constrains the coefficients on post-baseline covariates to zero. These models are estimated separately for black and nonblack respondents because prior research suggests that neighborhood selection processes differ by race (Charles 2003; Massey and Denton 1993; South and Deane 1993). Coefficient estimates from the treatment models and descriptive statistics for the treatment weights are reported in Online Resource 1 in Parts A and B, respectively.

In this article, regression-adjusted and IPT-weighted estimates of the parameters defined in Eqs. (1) and (2) are reported separately for blacks and nonblacks. Separate estimates for males and females are not reported because statistical tests provide no evidence that the effects of neighborhood poverty differ by gender. Regression-adjusted estimates come from models that condition on treatment history, baseline covariates, and post-baseline measurements of time-varying factors averaged across time. IPT-weighted estimates are computed by fitting discrete-time logit models to the weighted pseudo-population that condition on treatment history and covariates measured at baseline only. Equation (3) parameters are estimated from conventional discrete-time logit models that condition on treatment status at age 11, time-varying covariates measured concurrently with treatment, and baseline factors. Robust standard errors are computed for the IPT-weighted estimates to account for serial correlation induced by the weighting (Robins et al. 2000).

The IPT-weighted estimator is unbiased and consistent under the assumptions of no unmeasured confounders, no model misspecification, and positivity (Cole and Hernan 2008; Robins et al. 2000). Conventional regression estimators require these same assumptions and more. Specifically, they require the additional assumption that time-varying confounders are not affected by prior treatment. This assumption is almost certainly violated in observational studies of neighborhood effects. Although IPT weighting overcomes critical limitations of conventional regression modeling, the requisite assumptions for this method are not trivial.

First, if there are unobserved covariates that are risk factors for becoming an adolescent parent and for living in poor neighborhoods, then the IPT-weighted estimator is biased. I attempt to mitigate this problem by adjusting for a set of confounders that includes the most powerful joint predictors of neighborhood attainment and teen sexual behavior. Unobserved confounding, however, is still a possibility, given that families may select different neighborhood contexts on the basis of unmeasured factors that affect the risk of adolescent parenthood. For example, respondents who live in poor neighborhoods may be at greater risk of adolescent parenthood regardless of where they reside because their parents are simply less ambitious, skilled, or intelligent when it comes to encouraging safe sexual activity. Because measures of these parental psychological factors are unavailable in the PSID, the IPT-weighted estimator would be upwardly biased if these characteristics are in fact confounders, indicating a higher risk of adolescent parenthood resulting from exposure to neighborhood poverty even when there is no such effect.

In some cases, experimental, quasi-experimental, or instrumental variable research designs that require less stringent assumptions about neighborhood selection can be used to overcome the problem of unobserved confounding in neighborhood effects research (e.g., Evans et al. 1992; Goering and Feins 2003; Rosenbaum and Popkin 1991). However, for studies of *time-dependent* neighborhood effects, the high costs and logistical complexity associated with these methods likely preclude their implementation. Random assignment of subjects into different multiyear neighborhood exposure trajectories, rather than experimental or quasi-experimental manipulation of point-in-time contextual treatments, is prohibitively difficult. Absent some form of experimental control over neighborhood context across time, IPT weighting of observational data is the most defensible approach to estimating longitudinal neighborhood effects. Nevertheless, potential violations of the ignorability assumption on which this method is based necessitate caution when interpreting results.

Second, IPT-weighted estimation is biased if the models for selection into treatment are incorrectly specified. Experimentation with different treatment models, however, indicates that neighborhood-effect estimates are relatively invariant across a variety of specifications (see Online Resource 1, Part C, for details). Third, IPT weighting requires the positivity assumption that nonzero treatment probabilities exist across all levels and combinations of prior confounders. This assumption is reasonable in the present context, given that neighborhood choice is not formally restricted on the basis of economic or demographic characteristics, and descriptive analyses indicate that treatment occurs with positive probability across the support of several key confounders.

Censoring

Subjects who leave the study before they become parents or reach the end of follow-up are censored. Censoring can be problematic if subjects with certain characteristics are more likely to drop out of the study than others, so weights are used to adjust for potential nonrandom censoring based on observed covariates (Robins et al. 2000). The stabilized censoring weight for respondent i at wave k is given by

$$cw_{ik} = \prod_{t=1}^{k-1} \frac{P\left(C_t = 0 \middle| \overline{C}_{t-1} = 0, \overline{A}_{t-1} = \overline{a}_{i(t-1)}, L_0 = l_0\right)}{P\left(C_t = 0 \middle| \overline{C}_{t-1} = 0, \overline{A}_{t-1} = \overline{a}_{i(t-1)}, \overline{L}_t = \overline{l}_{it}\right)}, \tag{8}$$

where C_k is equal to 1 if a subject is censored at wave k and 0 otherwise. Pooled logistic regression models are used to estimate the probabilities in the weight (results not shown), and IPT-weighted estimates are computed using the product of the treatment and censoring weights.

Results

Sample Characteristics

Descriptive statistics for time-invariant covariates presented in Table 1 reveal substantial racial disparities on the majority of measured characteristics. For example,

Table 1 Descriptive statistics for time-invariant covariates in analyses of neighborhood effects on adolescent parenthood, Panel Study of Income Dynamics[a]

Variable	Blacks (N = 2,669)	Nonblacks (N = 3,573)
Gender (%)		
Male	50.13	50.32
Female	49.87	49.68
Birth Weight (%)		
≥5.5 lbs	91.08	94.57
<5.5 lbs	8.92	5.43
Mother's Marital Status at Birth (%)		
Unmarried	48.48	7.30
Married	51.52	92.70
Family Head's Education (%)		
Less than high school	43.42	21.80
High school graduate	29.30	24.71
At least some college	27.28	53.49
Mother's Age at Birth (mean)	24.74	26.51

[a] Statistics reported for subjects who were not lost to follow-up before age 12.

blacks are more likely than nonblacks to have young unmarried mothers and to come from families with low levels of parental education.

Racial differences are also pronounced in Table 2, which contains descriptive statistics for time-varying covariates. Compared with nonblacks, blacks are more likely to live in a family that receives AFDC benefits, does not own a home, and has lower income. In addition, these statistics show considerable change over time in several family characteristics for both blacks and nonblacks. For example, at age 4, only about 33 % of blacks live in families that own their residence; by age 12, about 45 % live with families that are homeowners. Similarly, from age 4 to 12, nonblacks become more likely to live in families that own a home.

Table 3 describes the risk of adolescent parenthood by age and race. The probability of becoming a teen parent is substantially higher for blacks than for nonblacks. At age 16, for example, the estimated probability of adolescent parenthood is about .05 for blacks and .01 for nonblacks. Overall, 511 blacks and 247 nonblacks (or about 19 % and 7 %, respectively) become adolescent parents.

Trajectories of Exposure to Neighborhood Poverty

Figure 4 illustrates neighborhood exposure trajectories from age 5 to 12 separately by race. Specifically, it presents sequence index plots, which use stacked line segments and differential shading to show how subjects move between levels of neighborhood poverty across time (Brzinsky-Fay et al. 2006). Each subject is represented by one horizontal line segment, and temporal changes in neighborhood exposure status are indicated with grayscale variations. These plots reveal extreme racial disparities in

Table 2 Descriptive statistics for time-varying covariates in analyses of neighborhood effects on adolescent parenthood, Panel Study of Income Dynamics[a]

Variable	Blacks (N = 2,669)		Nonblacks (N = 3,573)	
	Age 4	Age 12	Age 4	Age 12
Family Head's Marital Status (%)				
Unmarried	41.06	48.71	9.57	14.50
Married	58.94	51.29	90.43	85.50
Family Head's Employment Status (%)				
Unemployed	33.01	33.08	8.40	9.71
Employed	66.99	66.92	91.60	90.29
Welfare Receipt (%)				
Did not receive AFDC	76.47	78.16	95.27	95.83
Received AFDC	23.53	21.84	4.73	4.17
Homeownership (%)				
Did not own home	67.33	55.11	32.69	21.75
Owned home	32.67	44.89	67.31	78.25
Family Income in $1000s (mean)	15.70	17.13	30.13	37.10
Family Head's Work Hours (mean)	27.58	27.30	41.46	40.26
Family Size (mean)	5.32	5.10	4.63	4.69
Cumulative Residential Moves (mean)	0.28	1.67	0.24	1.32

[a] Statistics reported for subjects who were not lost to follow-up before age 12.

long-term exposure to neighborhood poverty, in which blacks and nonblacks typically follow opposite treatment trajectories. The wide, light gray region at the top of the plot for nonblacks indicates that their modal treatment trajectory is sustained exposure to low-poverty neighborhoods; about 40 % experience long-term residence in neighborhoods of this type. The narrow, dark gray region at the bottom of

Table 3 Risk of adolescent parenthood by age (wave) and race, Panel Study of Income Dynamics[a]

Age (wave)	Blacks				Nonblacks			
	n	Y_k	C_k	$P(Y_k)$	n	Y_k	C_k	$P(Y_k)$
12 (k = 8)	2,669	5	190	.002	3,573	0	252	.000
13 (k = 9)	2,474	6	193	.002	3,321	2	251	.001
14 (k = 10)	2,275	22	179	.010	3,068	1	225	.000
15 (k = 11)	2,074	47	164	.023	2,842	15	226	.005
16 (k = 12)	1,863	90	159	.048	2,601	26	263	.010
17 (k = 13)	1,614	99	155	.061	2,312	49	216	.021
18 (k = 14)	1,360	126	143	.093	2,047	77	186	.038
19 (k = 15)	1,091	116	975	.106	1,784	77	1,707	.043

[a] Y_k is an indicator for adolescent parenthood, C_k represents censored observations, and $P(Y_k)$ is the estimated probability of becoming an adolescent parent at wave k.

this plot shows that only a small number of nonblacks—about 5 %—experience sustained exposure to high-poverty neighborhoods from age 5 to 12. The plot for blacks, by contrast, shows that about 40 % experience sustained exposure to high-poverty neighborhoods and that only about 6 % are continuously exposed to low-poverty neighborhoods.

Figure 4 also shows the extent of neighborhood mobility over time, as indicated by grayscale variation in the horizontal line segments. The regions that change from

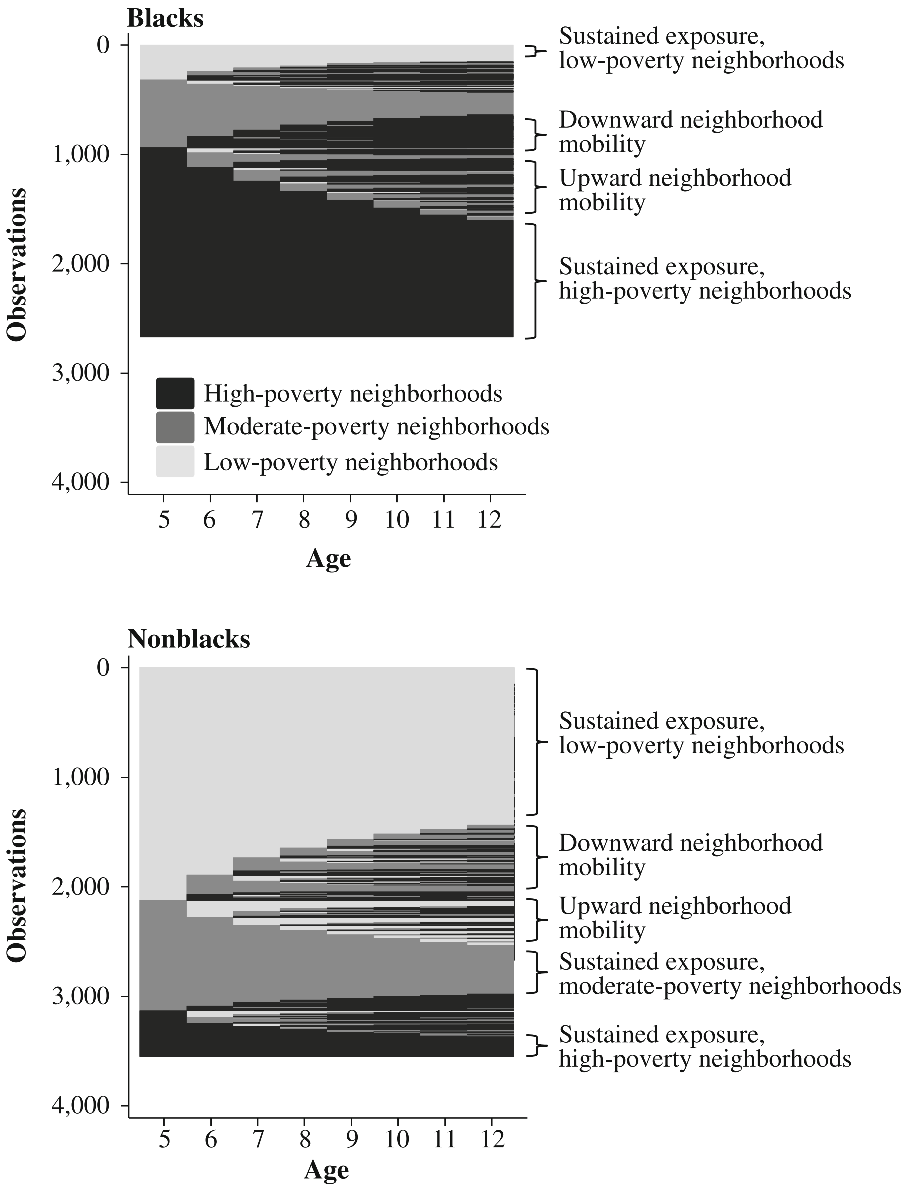

Fig. 4 Sequence index plots displaying trajectories of exposure to neighborhood poverty, Panel Study of Income Dynamics

lighter to darker shades of gray show that many sample members move from lower- to higher-poverty neighborhoods. The plots also show some upward neighborhood mobility for both blacks and nonblacks where line segments change from darker to lighter shades over time. Taken together, about 48 % of blacks and about 42 % of nonblacks experience at least one change in the level of neighborhood poverty to which they are exposed between age 5 and 12. About 25 % of both groups move between treatment levels two or more times during the early life course.

Neighborhood Effects on Adolescent Parenthood

The first panel of Table 4 contains regression-adjusted and IPT-weighted estimates of the parameters defined in Eq. (1), which describe how the probability of adolescent parenthood changes with the cumulative proportion of time spent in moderate- and high-poverty neighborhoods relative to low-poverty neighborhoods. The regression-adjusted estimates control for observed confounding of neighborhood exposure status by conditioning on covariates measured at baseline and cross-time averages of time-varying covariates. These estimates indicate that exposure to neighborhood poverty has marginally significant effects on adolescent parenthood among both blacks and nonblacks. For blacks, the regression-adjusted estimates indicate that the odds of adolescent parenthood increase by about 60 % with sustained exposure to either moderate-poverty (exp(0.467) = 1.595) or high-poverty (exp(0.455) = 1.576) neighborhoods, compared with continuous residence in low-poverty neighborhoods. Among nonblacks, the regression-adjusted estimates for Eq. (1) indicate that sustained exposure to moderate-poverty neighborhoods increases the odds of adolescent parenthood by 47 % compared to continuous exposure to low-poverty neighborhoods; sustained exposure to high-poverty neighborhoods is estimated to increase the odds of adolescent parenthood by about 80 %.

IPT-weighted estimates of the log odds ratios in Eq. (1) indicate that neighborhood poverty has substantial and statistically significant effects on adolescent parenthood for both blacks and nonblacks. For blacks, the IPT-weighted estimates indicate that the odds of adolescent parenthood increase by about 75 % with sustained exposure to moderate-poverty neighborhoods and by about 80 % with sustained exposure to high-poverty neighborhoods, compared with continuous residence in low-poverty neighborhoods. For nonblacks, the IPT-weighted estimates indicate that sustained exposure to moderate-poverty neighborhoods increases the odds of adolescent parenthood by about 60 % compared with extended residence in low-poverty neighborhoods, and sustained exposure to high-poverty neighborhoods is estimated to more than double the odds of early parenthood. Among nonblacks, IPT-weighted estimates for the effects of cumulative exposure to moderate- and high-poverty neighborhoods are about 20 % and 40 % larger, respectively, than corresponding regression-adjusted estimates. For blacks, the IPT-weighted estimates are more than 20 % larger than corresponding regression-adjusted estimates. These differences suggest that conventional regression estimators understate the effects of neighborhood poverty on adolescent parenthood.

The second panel in Table 4 reports estimates from models that allow for different effects of cumulative exposure to neighborhood poverty during childhood versus adolescence. The IPT-weighted estimates have large standard errors, indicating that

Table 4 Effects of neighborhood poverty on the risk of adolescent parenthood, Panel Study of Income Dynamics[a]

	Blacks (person-years = 15,420)				Nonblacks (person-years = 21,548)			
	Regression-Adjusted		IPT-Weighted		Regression-Adjusted		IPT-Weighted	
Model	LOR	SE	LOR	SE	LOR	SE	LOR	SE
Model 1								
Cumulative exposure								
Low-poverty neighborhood	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Moderate-poverty neighborhood	0.467	0.300	0.569	0.320†	0.383	0.269	0.460	0.265†
High-poverty neighborhood	0.455	0.261†	0.601	0.266*	0.571	0.306†	0.829	0.297**
Model 2								
Cumulative exposure, childhood								
Low-poverty neighborhood	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Moderate-poverty neighborhood	0.167	0.329	0.103	0.355	0.129	0.345	0.249	0.364
High-poverty neighborhood	0.152	0.319	0.212	0.339	−0.258	0.449	−0.138	0.478
Cumulative exposure, adolescence								
Low-poverty neighborhood	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Moderate-poverty neighborhood	0.279	0.260	0.422	0.283	0.224	0.263	0.189	0.266
High-poverty neighborhood	0.284	0.269	0.365	0.293	0.670	0.337*	0.790	0.341*
Model 3								
Point exposure (age 11)								
Low-poverty neighborhood	Ref.	Ref.	—	—	Ref.	Ref.	—	—
Moderate-poverty neighborhood	0.213	0.186	—	—	0.167	0.193	—	—
High-poverty neighborhood	0.191	0.178	—	—	0.549	0.218*	—	—

[a]Log odds ratios (LOR) and standard errors (SE) are combined estimates from five multiple imputation data sets.

†$p < .10$; *$p < .05$; **$p < .01$ (two-sided tests of no effect)

the available data allow only imprecise estimates of neighborhood effects by developmental stage, and Wald tests (not reported) show that observed differences in the effects of exposure to neighborhood poverty during childhood versus adolescence are not statistically significant at conventional thresholds. Nevertheless, the point estimates are at least suggestive of timing differences.

For example, among nonblacks, the IPT-weighted estimates indicate that cumulative exposure to high-poverty neighborhoods during adolescence has a significant positive effect on the probability of becoming an adolescent parent. The estimated effects of childhood exposure to neighborhood poverty, by contrast, are much smaller and not statistically significant. Similarly, for blacks, the IPT-weighted estimates for cumulative exposure to neighborhood poverty during adolescence are considerably larger than those for exposure during childhood, although none of these estimates are statistically significant. Thus, while point estimates for Eq. (2) should be interpreted with caution given their high variability, these results suggest that exposure to neighborhood poverty during adolescence may have a more notable effect on the risk of becoming a teen parent than exposure earlier during childhood.

The lower panel of Table 4 reports regression-adjusted estimates based on point-in-time measures of neighborhood poverty taken at age 11. As expected, these estimates are smaller than IPT-weighted estimates based on longitudinal measures of neighborhood poverty. For blacks, they are about a third the size of IPT-weighted estimates for the effects of cumulative exposure to neighborhood poverty. Among nonblacks, regression-adjusted estimates with point-in-time measures are also much smaller than estimates obtained via IPT weighting and longitudinal measurement of neighborhood poverty. These differences underscore the importance of accounting for both longitudinal exposure trajectories and dynamic neighborhood selection.

Discussion

The effect of growing up in disadvantaged neighborhoods on adolescent parenthood is central to understanding poverty and the reproduction of inequality over time. Past research on this issue, however, has neglected duration and timing of exposure to poor neighborhoods and has not properly addressed the dynamic selection process that defines how children come to live in different neighborhood environments throughout the early life course. This inattention to longitudinal exposure patterns and dynamic neighborhood selection may underlie the mixed results of previous research, wherein a number of studies have suggested only a minimal influence for neighborhood context on adolescent parenthood (Brooks-Gunn et al. 1993; Galster et al. 2007; Ginther et al. 2000; Thornberry et al. 1997).

The present study investigates how the impact of neighborhood poverty on adolescent parenthood depends on the duration and timing exposure. It measures neighborhood context once per year from early childhood through late adolescence and uses novel methods that properly adjust for dynamic neighborhood selection on observed covariates. Unlike conventional methods, the IPT weighting approach employed here does not remove the indirect effects of neighborhood poverty that operate through time-varying characteristics of the family and is therefore capable of estimating the total effects of different longitudinal exposure patterns. These methods are not without limitations, but they allow for unbiased and consistent estimation of neighborhood effects under assumptions that are less stringent than those required for conventional regression.

The results of this study indicate that long-term exposure to poor neighborhoods substantially increases the risk of adolescent parenthood, and that exposure to neighborhood poverty during adolescence may be more consequential than exposure earlier during childhood. Estimates for the effect of sustained exposure to poor neighborhoods are also considerably larger than estimates based on point-in-time measurements of neighborhood context, and the different estimates obtained from IPT weighting versus conventional regression indicate that effects of neighborhood poverty operate indirectly through measured time-varying characteristics of family, such as parental employment, income, and marital status. Taken together, these findings demonstrate that it is critically important to account for longitudinal exposures to neighborhood poverty and for the dynamic selection and feedback mechanisms that structure how neighborhood poverty affects sexual behavior during adolescence. Studies that rely on static measures of neighborhood context and conventional regression methods risk understating the full impact of neighborhood poverty. In addition, these results complicate the conceptual separation of neighborhood and family effects on child development in ecological socialization theories (e.g., Leventhal and Brooks-Gunn 2000; Small and Newman 2001). Neighborhood effects are mediated by family effects, and vice versa (Sampson et al. 2008; Sharkey and Elwert 2011; Wodtke et al. 2011).

The evidence presented here demonstrates that a temporal framework is essential for understanding neighborhood effects. Many families move between different neighborhood environments or remain in communities whose social composition changes over time, raising important questions about effects of different longitudinal exposures to neighborhood poverty. In contrast to previous research, the time-dependent effects of neighborhood poverty reported in this study are more consistent with core theories that motivate research on the consequences of spatially concentrated poverty (Wilson 1987, 1996), with research on neighborhood attainment and mobility (Sampson and Sharkey 2008), and with developmental perspectives on the reproduction of inequality (Duncan et al. 1998). To advance research on the processes through which poverty is generated and maintained, a more complete integration of ecological and temporal perspectives on spatial stratification is needed.

While this study addresses the lack of research on the effects of neighborhood poverty within a temporal framework, it nevertheless suffers from several limitations. First, it focuses on a single outcome, adolescent parenthood, which represents the final stage in a series of decisions about engaging in sexual intercourse, using contraceptives, and carrying a pregnancy to term. Investigating how neighborhood context influences the proximate determinants of fertility would provide further insight into the social processes through which neighborhood effects operate.

Second, although the PSID is arguably the most comprehensive source of longitudinal information on neighborhood context, this study still lacks the data needed to *precisely* estimate time-dependent effects of neighborhood poverty. Additional data must be collected to better understand the temporal dimensions of neighborhood effects. Future studies should experiment with new procedures to gather information on neighborhood exposure histories and prior time-varying confounders that are not as costly and difficult as following a cohort of children for more than 15 years. For example, large cross-sectional surveys might consider adapting retrospective life history calendars to record past residential locations (Axinn et al. 1999).

Finally, this study does not investigate the specific mechanisms—such as social isolation, collective disorganization, and resource deprivation—thought to transmit the effects of concentrated neighborhood poverty to adolescent parenthood. Although such an analysis is beyond the scope of the present study, it is nevertheless critically important for understanding the social processes through which the local environment shapes the sexual behavior of individuals during the early life course.

The impact of sustained exposure to neighborhood poverty on adolescent parenthood identified in this study suggests that neighborhood-effects research is essential to understanding the reproduction of inequality. As social scientists increasingly grapple with the role of time in spatial stratification, future studies using new and more flexible methods may find contextual effects on other outcomes to be even more important than previously documented. In order to advance knowledge of the causes and consequences of inequality, rigorous integration of social theory and quantitative empirical practice regarding the temporal dimensions of social context is crucial.

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