DURATION AND TIMING OF EXPOSURE TO NEIGHBORHOOD POVERTY AND THE RISK OF ADOLESCENT PARENTHOOD

Geoffrey Wodtke

University of Michigan

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Direct correspondence to Geoffrey Wodtke (wodtke@umich.edu), Population Studies Center, University of Michigan, 426 Thompson Street, Ann Arbor, MI 48106-1248. This research was supported by a University of Michigan Population Studies Center Traineeship (National Institute of Child Health and Human Development grant T32 HD007339) and a National Science Foundation Graduate Research Fellowship.

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ABSTRACT

Theory suggests that the impact of concentrated neighborhood poverty depends on both the duration and timing of exposure. Previous research, however, does not properly measure and analyze the sequence of neighborhood contexts to which children are exposed throughout the early life course. This study investigates the impact of different longitudinal patterns of exposure to disadvantaged neighborhoods on the risk of adolescent parenthood. To this end, I follow a cohort of children in the PSID from age 4 to 19, measuring neighborhood context and a rich set of covariates once per year, and use novel counterfactual methods for time-varying exposures that overcome critical limitations of conventional regression models when selection processes are dynamic. Results indicate that sustained exposure to high-poverty 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. In addition, results suggest that conventional regression models severely understate the full impact of long-term exposure to neighborhood poverty, regardless of whether neighborhood context is measured longitudinally or at a single point in time. These findings demonstrate the importance of a temporal framework for the study of neighborhood effects on adolescent sexual behavior.

DURATION AND TIMING OF EXPOSURE TO NEIGHBORHOOD POVERTY AND THE RISK OF ADOLESCENT PARENTHOOD

Growing up in impoverished neighborhoods is thought to precipitate a number of problematic behaviors during adolescence and early adulthood (Jencks and Mayer 1990; Wilson 1987; Wilson 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), adolescent parenthood is the focus of much research on the behavioral effects of neighborhood context. Although contemporary stratification theory holds that the social milieu in which children are embedded has strong effects on their sexual behavior and the consequences thereof, empirical research on this topic is conflicted. Some studies find that teens and young adults who reside in poor neighborhoods are significantly more likely to become parents compared to their peers living in more affluent areas (e.g., Harding 2003; South and Crowder 1999; Sucoff and Upchurch 1998), but other studies report no effect of neighborhood poverty on adolescent parenthood (e.g., Brooks-Gunn, Duncan, Klebanov, and Sealand 1993; Galster, Marcotte, Mandell, Wolman, and Augustine 2007; Ginther, Haveman, and Wolfe 2000; Thornberry, Smith, and Howard 1997). The impact of neighborhood context on adolescent sexual behavior thus remains a contested issue.

Nearly all previous studies of neighborhood effects on family formation, however, are limited by a set of problems related to the duration and timing of exposures to different neighborhood contexts. First, past research often relies on point-in-time measurements of neighborhood context (e.g., Brooks-Gunn, Duncan, Klebanov, and Sealand 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 severely understate the impact of neighborhood poverty on adolescent parenthood. Second, theories of child development and the life-course perspective suggest that the effects of neighborhood poverty also depend on the timing of exposure, but few studies investigate whether residence in poor neighborhoods during different developmental periods (e.g., 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 developmental 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 characteristics of the family environment that are time-varying, such as parental income and employment status. Some past studies treat these factors as confounders that require statistical adjustment (e.g., Ginther, Haveman, and Wolfe 2000; Harding 2003; South and Crowder 1999) while others view them as mediators for the effect of neighborhood context that must not be controlled (e.g., Brewster 1994; Brewster, Billy, and Grady 1993). In fact, both these 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 rely almost exclusively on regression-based adjustments for observed selection and therefore provide biased estimates of neighborhood effects on developmental outcomes, even when neighborhood context is measured longitudinally (e.g., Ginther, Haveman, and Wolfe 2000; South and Crowder 2010).

This study investigates the impact of neighborhood poverty on the risk of adolescent parenthood using a temporal, life-course framework. Specifically, it examines whether sustained exposure to neighborhood poverty is more consequential than transitory exposure to such environments and whether exposure to poor neighborhoods during childhood versus adolescence has heterogeneous effects on the chances of becoming a teen parent. 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 putative confounders repeatedly across time, and use inverse probability of treatment weighting, which properly adjusts for dynamic neighborhood selection, to estimate the time-dependent effects of neighborhood poverty. By using rich longitudinal data and novel methods that overcome critical limitations of conventional regression, the present study is uniquely positioned to elucidate the temporal dimensions of neighborhood effects.

Below, 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 high-poverty neighborhoods and argue that many of the 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 the method of 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 indicate that conventional regression models, which mishandle dynamic neighborhood selection, provide estimates that severely understate the total effect of long-term neighborhood disadvantage. Together, these findings demonstrate the importance of a temporal framework for neighborhood-effects research.

NEIGHBORHOOD EFFECTS: TEMPORAL AND LIFE-COURSE DIMENSIONS

The mechanisms through which poor neighborhoods are hypothesized to increase the risk of adolescent parenthood in contemporary theories of neighborhood effects include social isolation and alternative, or heterogeneous, local cultures (Anderson 1991; Harding 2010; Massey and Denton 1993; Wilson 1987; Wilson 1996), a breakdown of collective trust among resident adults (Sampson 2001; Sampson, Morenoff, and Gannon-Rowley 2002), high levels of violent crime

(Harding 2009; Harding 2010), and institutional resource deprivation (Brooks-Gunn, Duncan, and Aber 1997; Wilson 1987; Wilson 1996). 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 that 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 curbs 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 the concomitant social isolation are thought to engender alternative subcultures that encourage early sexual activity and adolescent childbearing (Anderson 1991; Massey and Denton 1993). For children in high-poverty neighborhoods, socialization within these subcultures may attenuate the perceived costs of becoming a parent during adolescence.

Implicit in social isolation and alternative subculture theories is that children must be exposed to these harmful social conditions for an extended period of time in order for neighborhoods to exert their hypothesized effects. Indeed, those families that cannot escape impoverished neighborhoods, the so-called "urban underclass," are the central focus of neighborhood-effects theorizing, and Wilson (2009:55) emphasizes the "cumulative cultural experiences" of those trapped in poor communities. For social and cultural isolation perspectives, which focus on socialization by peers and resident adults, it is important to account for duration of residence in poor neighborhoods because long-term exposure is likely necessary for children to sufficiently absorb the deviant local values. Children are also more likely to develop feelings of fatalism and hopelessness about their life chances if they are immersed in poor neighborhoods for most of their lives. By contrast, those who experience only short-term residence in high-poverty neighborhoods may be able to remain optimistic about the future and cling to mainstream cultural values learned elsewhere.

Social disorganization and institutional resource deprivation theories are also premised on long-term exposure to poor neighborhoods. Social disorganization 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 (Harding 2010; Sampson 2001; Sampson and Morenoff 1997). 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 of time, parents' attention may rarely be focused on preventing children from engaging in early or unsafe sexual activity, thereby elevating the cumulative risk of adolescent parenthood. According to resource deprivation theories, the ability of adults to provide adequate supervision for children is also affected by access to local services, such as recreation facilities, childcare centers, and after-school programs, which are lacking in poor neighborhoods. School quality is another important dimension of resource deprivation theories directly linked to the socioeconomic composition of neighborhoods. Children who attend schools with overcrowded classrooms, poor instructional resources, and dilapidated facilities may be less likely to develop positive educational and occupational aspirations. The longer children are exposed to such negative school environments, the more their aspirations are likely to be subdued, and consequently, the perceived costs of adolescent parenthood may decrease with the duration of time spent in poor neighborhoods.

In addition to duration of exposure, the life-course perspective and research on child development suggest that neighborhood poverty may have heterogeneous effects that depend on 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 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 on cognitive development and skill formation suggests that children are particularly sensitive to environmental inputs during early childhood, where later attainments are closely linked to foundations formed earlier (Heckman 2006; Heckman and Krueger 2004). 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 does not provide a clear account of how neighborhood effects operate across the early life course, the diverse perspectives reviewed here all suggest that different longitudinal patterns of exposure to poor neighborhoods will have heterogeneous effects on the risk of adolescent 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 their new neighborhood, the adults in this family may have a difficult time finding new jobs. Then, as a result of long-term unemployment, parental incomes may be reduced and savings depleted. With few economic resources to draw upon, the chances that this family escapes their deteriorating neighborhood become increasingly slim. This hypothetical example demonstrates the process of dynamic neighborhood selection, whereby time-varying family characteristics, such as parental employment status 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, where more affluent and educated parents are much less likely to live in poor neighborhoods (Sampson and Sharkey 2008; South and Crowder 1997a; South and Crowder 1997b). 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 1997a; South and Deane 1993; Speare and Goldscheider 1987). In addition to family structure and socioeconomic characteristics, neighborhood attainment is closely related to race. Because of widespread racial discrimination in the real estate industry and strong preferences among whites to live with same-race neighbors (Charles 2003; Massey and Denton 1993; Yinger 1995), blacks are substantially more likely to live in poor neighborhoods, regardless of their personal economic resources (Iceland and Scopilliti 2008).

While previous research demonstrates that a variety of socioeconomic and demographic characteristics affect neighborhood selection, there is also evidence that some of these same factors are in turn affected by social conditions in the neighborhood. Residence in disadvantaged neighborhoods is thought to influence both the structure and economic foundations of the family (Wilson 1987; Wilson 1996; Wilson 2009). For example, the decline of manufacturing and suburbanization of employment have substantially reduced the number of jobs available to residents of poor urban neighborhoods, and consequently, this population is more likely to experience long spells of unemployment and sub-poverty incomes (Fernandez and Su 2004; Wilson 1987; Wilson 1996). Furthermore, the limited employment prospects in poor neighborhoods may lead to greater marital instability, delayed marriage, and increasing non-marriage in these communities (South and Crowder 1999; Wilson 1987).

In sum, there are a number of time-varying family characteristics—parental employment status, income, and family structure in particular—that affect future neighborhood selection and are themselves affected by past neighborhood contexts. Because these factors also influence the risk of adolescent parenthood (Duncan, Yeung, Brooks-Gunn, and Smith 1998; McLanahan and Percheski 2008; McLanahan and Sandefur 1994), they are simultaneously confounders for the effect of future exposures and mediators for the effect of past exposures to neighborhood poverty. Time-varying confounders that are affected by past levels of a time-varying treatment pose several difficult problems for conventional regression models. Below, 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). The PSID is a longitudinal survey that began in 1968 with a nationally representative sample of about 4,800 families in the U.S. These original 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 the 8,757 subjects present at age 4 in a PSID family between 1968 and 1989. These subjects are followed from age 4 until they become a parent, turn 20 years old, drop out of the PSID, or reach administrative end of follow-up (defined to be the 1997 wave of the PSID).[1] Of the initial analytic sample, 6,242 subjects remain in the study until age 12, the beginning of the risk period for adolescent parenthood (methods to adjust for potential nonrandom censoring are described below). The PSID wave, indexed by $k \in \{0,1,\dots,K\}$, in which a subject is age 4 defines baseline. Then, from baseline $(k = 0)$ until the end of follow-up $(K = 15)$, neighborhood context and a rich set of potential confounders are measured every year. The timing of adolescent parenthood for both male and female subjects is determined from the PSID childbirth history file.

[1] A subject whose family drops out of the PSID but later returns is considered permanently lost to follow-up at the wave when they initially leave the study.

Neighborhood poverty is the exposure of interest in this study, measurements of which come from the Geolytics Neighborhood Change Database (NCDB).[2] The NCDB contains tract-level data from the 1970-2000 U.S. censuses with tract boundaries and measures defined consistently across time. Linear interpolation is used to impute tract characteristics for intercensal years. From these data, a time-varying, three-level ordinal treatment variable is defined at each wave k 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% poverty), moderate-poverty (10-20% poverty), or a high-poverty neighborhood (>20% poverty), respectively. In the analysis below, this ordinal wave-specific treatment variable is used to generate duration-weighted measures of exposure to different levels of neighborhood poverty throughout childhood and adolescence.

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 grams). Mother's age at childbirth is measured in years and her marital status at this juncture is dummy coded, 1 for married and 0 for unmarried. For family head's education, the most recent measurement of this characteristic taken at or just prior to baseline is expressed as a series of dummies for "less than high school," "high school graduate," and "at least some college." Race is dummy coded, 1 for black and 0 for nonblack. This study also adjusts for an extensive set of time-varying covariates measured at each wave k. These include the marital

[2] Measurements of neighborhood poverty from the NCDB are appended to individual records using the PSID restricted-use geocode match files.

[3] Measurement limitations in the PSID inhibit accurate tracking of changes in parental education over time. This factor is therefore treated as time-invariant.

status, employment status, and work hours of the family head as well as family income, household size, homeownership, residential mobility, and receipt of Aid to Families with Dependent Children (AFDC). The marital and employment status of the family head are coded as dummies. The average number of hours worked per week during the previous year is used to measure the family head's work hours. Household size is the number of people living in a subject's residence; homeownership is coded 1 if a family owns the residence they occupy and 0 otherwise; AFDC receipt is also expressed as a dummy indicating whether a family received any AFDC income in the past year; and total family income, measured in real dollars, is the inflation-adjusted taxable income earned by all family members in the previous year. Residential mobility is defined to be 1 at each wave where a family reports a move during the previous year and 0 otherwise. Missing treatment and covariate data are simulated by multiple imputation with 5 replications (Royston 2005; 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; Robins, Hernan, and Brumback 2000). 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 $\bar{A}_k = (A_1, \ldots, A_k)$ to be the sequence of exposures to different levels of neighborhood

[4] The neighborhood effect estimates and standard errors reported below are combined estimates from the 5 multiple imputation datasets. For simplicity, descriptive statistics are based on only the first imputed dataset. A small number of subjects who remained in the PSID through adolescence but are missing birth history information are treated as though they left the study at age 12 and incorporated into the adjustment for censoring described below.

poverty through wave k (overbars are used to denote treatment or covariate history).[5] Let $\bar{a} = \bar{a}_K$ 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 $\bar{a}$ if s/he is exposed to the prescribed 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 $S(\bar{a})$ to be the potential time until parenthood had s/he, possibly contrary to fact, followed the treatment regime $\bar{a}$. For each subject, only the one failure time where $S(\bar{a}) = S$ is observed, and the other $S(\bar{a})$ are counterfactuals. Three discrete-time hazard models based on the potential failure times are considered below. For these models, the potential failure times, $S(\bar{a})$, are transformed into wave-specific failure indicators, $Y_k(\bar{a})$, equal to 1 if $k < S(\bar{a}) < k + 1$ and 0 otherwise. That is, $Y_k(\bar{a})$ indicates whether a subject would have become a parent during wave k had they experienced the history of neighborhood poverty $\bar{a}$.

To investigate the effects of long-term exposure to neighborhood poverty, the first hazard 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

$$logit\big(P(Y_k(\bar{a}) = 1 | k > 7, \bar{Y}_{k-1}(\bar{a}) = 0)\big)$$

$$= \beta_0(k) + \beta_1 \left(\frac{\sum_{t=1}^{k-1} I(a_t = 2)}{k-1}\right) + \beta_2 \left(\frac{\sum_{t=1}^{k-1} I(a_t = 3)}{k-1}\right), \tag{1}$$

where $P(Y_k(\bar{a}) = 1 | k > 7, \bar{Y}_{k-1}(\bar{a}) = 0)$ is the (approximate) hazard of becoming an adolescent parent at wave $k > 7$ (i.e., at age 12 or later when the probability of parenthood is nonzero) had

[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. Rather, this measure is treated as a confounder for the effects of later treatments and absorbed into the vector of baseline control variables.

subjects followed the neighborhood exposure trajectory $\bar{a}$ through the prior wave, and $\beta_0(k)$ is the log baseline hazard 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 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 (approximate) log hazard rate ratios. Specifically, $\exp(\beta_1)$ is the multiplicative effect on the hazard 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 will 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, I specify a second model that allows different effects for cumulative exposure during childhood versus adolescence. This model has form

$$logit\left(P(Y_k(\bar{a}) = 1 | k > 7, \bar{Y}_{k-1}(\bar{a}) = 0)\right)$$

$$= \theta_0(k) + \theta_1\left(\frac{\sum_{t=1}^{6} I(a_t=2)}{6}\right) + \theta_2\left(\frac{\sum_{t=1}^{6} I(a_t=3)}{6}\right) + \theta_3\left(\frac{\sum_{t=7}^{k-1} I(a_t=2)}{k-7}\right) + \theta_4\left(\frac{\sum_{t=7}^{k-1} I(a_t=3)}{k-7}\right), \quad (2)$$

where $P(Y_k(\bar{a}) = 1 | k > 7, \bar{Y}_{k-1}(\bar{a}) = 0)$ and $\theta_0(k)$ are defined as above, but rather than two functions for cumulative exposure to neighborhood poverty, as in Model (1), there are now four functions that 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 $\sum_{t=7}^{k-1} I(a_t = 2)/(k-7)$ is 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 high-poverty neighborhoods are defined analogously.

The (approximate) log hazard rate 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

$$logit\big(P(Y_k(\bar{a}) = 1 | k > 7, \bar{Y}_{k-1}(\bar{a}) = 0)\big) = \eta_0(k) + \eta_1 I(a_7 = 2) + \eta_2 I(a_7 = 3) \qquad (3)$$

where a_7 is the neighborhood poverty level at age 11. Model (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 highly suspect constraints on the counterfactual hazards. 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.

Models (1-3) are referred to as marginal structural models in the causal inference literature (Hernan, Brumback, and Robins 2000; Robins, Hernan, and Brumback 2000). Their parameters can be identified from observational data under the assumption of sequential ignorability of treatment assignment (Robins 1999; Robins, Hernan, and Brumback 2000). This assumption is formally expressed as

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

where $\bar{L}_k = (L_0, L_1, \dots, L_k)$ represents observed covariate history through wave k and $\perp$ denotes statistical independence. In words, Equation (4) states that neighborhood poverty at wave k, A_k, is independent of the potential outcomes, $S(\bar{a})$, given prior neighborhood exposures, covariate history, and survival through wave k. This assumption is satisfied in observational studies if there are no unobserved factors that affect both selection into poor neighborhoods and the risk of becoming an adolescent parent, that is, if there is no unobserved confounding of neighborhood exposure status. In the next section, I show that even when the condition defined in (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 Figure 1, which shows a three-wave snapshot of the causal process hypothesized in this study. Prior exposures to neighborhood poverty have direct effects on the chances of becoming an adolescent parent but also indirect effects that operate through time-varying characteristics of the family. Selection into different neighborhood contexts at each wave is affected by observed time-varying factors, but there is no unobserved confounding of neighborhood poverty. Thus, Figure 1 shows 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

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

where $\alpha_0(k)$ are wave-specific intercept terms, $u(\bar{A}_{k-1})$ is a linear parametric function of neighborhood exposure history through wave $k-1$, and $\varepsilon(\bar{L}_{k-1})$ is some parameterization of confounder history. For example, to estimate the parameters in Model (1) above, $u(\bar{A}_{k-1})$

includes main effects for the proportion of time lived in moderate- and high-poverty neighborhoods through wave $k-1$, and the function $\varepsilon(\bar{L}_{k-1})$ typically has 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.

There are two problems with this modeling strategy when time-varying confounders in L_k are affected by prior exposure to neighborhood poverty. First, Figure 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 Figure 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 the previous section (Hernan, Brumback, and Robins 2000; Robins, Hernan, and Brumback 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.

The stabilized version of the IPT weight for the i^{th} subject at the k^{th} follow-up wave is given by

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

The denominator of the weight is the probability that subject i is exposed to their observed level of neighborhood poverty at a given wave conditional on their history of prior neighborhood exposures and time-varying covariates. The numerator, by contrast, is the conditional probability that a subject is exposed to their 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 post-baseline 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 (i.e., treatment has the appearance of sequential randomization). 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 (6) is estimated from an ordinal logistic regression model with form

$$logit\big(P(A_k > j | \bar{A}_{k-1}, \bar{L}_k)\big)$$
$$= \gamma_{0j}(k) + \gamma_1 A_{k-1} + \gamma_2 L_0 + \gamma_3 L_{k-1} + \gamma_4 L_k + \gamma_5 L_k L_{k-1} , \quad j = 1, \ldots, J-1 \quad (7)$$

where $\gamma_{0j}(k)$ is a wave-specific intercept term for the j^{th} cumulative logit. In Model (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 (including baseline treatment status), time-varying covariates measured at waves k and $k-1$, and cross-time interactions between selected time-varying factors. The conditional probability in the numerator of the stabilized IPT weight was estimated from a similar model that constrains the coefficients on post-baseline covariates to zero. These models are estimated separately for black and nonblack subjects 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 weight models are reported in Appendix A.

Below, I report regression-adjusted and IPT-weighted estimates of the causal parameters defined in Models (1-3) separately for blacks and nonblacks. The regression-adjusted estimates come from models similar to (5) that condition on treatment history, baseline covariates, and post-baseline measurements of time-varying factors averaged across time. The 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. Note that conditioning on baseline covariates does not incur the problems described in the previous section because these factors are by definition measured prior to treatment initiation. Huber-White robust standard errors are computed for the IPT-weighted estimates to account for serial correlation induced by the weighting (Robins, Hernan, and Brumback 2000; Robins, Rotnitzky, and Scharfstein 1999).[6]

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[6] The standard errors computed here are conservative because they do not take into account that the IPT weights are estimated (Robins, Rotnitzky, and Scharfstein 1999). This makes rejecting the null hypothesis of no treatment effect more difficult at stated thresholds for statistical significance.

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, Hernan, and Brumback 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, nevertheless, nontrivial. 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. The assumption of no unobserved confounding is not testable with observed data, but I attempt to mitigate this problem by adjusting for an extensive set of putative confounders. 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.[7] Third, IPT weighting requires the positivity condition that there be nonzero treatment probabilities across all levels and combinations of prior confounders (i.e., treatment status must not be a deterministic function of the past). This condition is satisfied in the present context, since neighborhood choice is not formally restricted on the basis of economic or demographic characteristics in the U.S.

[7] Alternative treatment model specifications include, for example, models with additional nonlinear terms for continuous time-varying factors, interactions between covariates measured at adjacent waves, and interactions between time and selected covariates.

Censoring

Subjects who leave the study before they become parents or reach the end of follow-up are said to be 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. The stabilized censoring weight for subject i at wave k is given by

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

where C_k is equal to 1 if a subject is censored at wave k and 0 otherwise (Robins, Hernan, and Brumback 2000). Pooled logistic regression models are used to estimate the stabilized censoring weights (results not shown). Then, to adjust for potential nonrandom censoring with respect to observed covariates, the IPT-weighted estimates are computed using the product of the treatment and censoring weights ($sw_{ik} \times cw_{ik}$) at each follow-up wave.

RESULTS

Sample Characteristics

Table 1 presents descriptive statistics for time-invariant covariates, revealing substantial racial disparities on the majority of measured characteristics. For example, 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 to 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 to racial differences, Table 2 also shows considerable change over time in several family characteristics for both blacks and nonblacks. For example, at age 4, only 32.67% of blacks live in families that own their residence, but by age 12, 44.89% live with families that are homeowners. Similarly, from age 4 to 12, nonblacks become more likely to live with families that own a home. Table 3 provides statistics that describe the risk of adolescent parenthood by age and race. In general, the risk of becoming an adolescent parent is substantially higher for blacks compared to nonblacks. At age 16, for example, the estimated probability of adolescent parenthood is about 0.05 for blacks and 0.01 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 describes neighborhood exposure trajectories from age 5 to 12 separately by race. Specifically, it contains sequence index plots, which use stacked line segments and differential coloring to show how subjects move between levels of neighborhood poverty across time (Brzinsky-Fay, Kohler, and Luniak 2006; Scherer 2001). Each subject is represented by one horizontal line segment, and temporal changes in neighborhood exposure status are indicated with changes of color. These plots reveal extreme racial disparities in long-term exposure to neighborhood poverty, where blacks and nonblacks follow virtually opposite treatment trajectories. The wide, lightly-shaded region at the top of the plot for nonblacks indicates that the modal treatment trajectory for this group is sustained exposure to low-poverty neighborhoods. The narrow, darkly-shaded region at the bottom of this plot shows that only a small number of nonblacks experience sustained exposure to high-poverty neighborhoods from age 5 to 12. The plot for blacks, by contrast, shows that the modal treatment trajectory is sustained exposure to high-poverty neighborhoods, and that very few blacks are continuously exposed to neighborhoods with low levels of poverty. Figure 4 also shows the extent of neighborhood mobility over time, as indicated by color changes to the horizontal line segments. For example, the regions that change from lighter to darker shades show that many sample members move from neighborhoods with lower poverty rates to neighborhoods with higher poverty rates. The plots also show some upward neighborhood mobility for both blacks and nonblacks where line segments change from darker to lighter shades over time.

Treatment Weights

Table 4 presents descriptive statistics for the stabilized IPT weights. The weights were computed from flexible ordinal logitistic regression models for the probability of exposure to different levels of neighborhood poverty at each follow-up wave. As an indicator of treatment endogeneity, the stabilized weights vary around 1 based on the degree to which neighborhood selection is affected by prior time-varying characteristics. The estimated weights have desirable properties for both the black and nonblack subsamples: at each time point, observed means are close to 1, and the weights are not highly variable. The small variance of the stabilized weights suggests that observed time-varying factors have a modest impact on future neighborhood attainment, net of covariates measured at baseline and prior exposure status.[8]

Neighborhood Effects on Adolescent Parenthood

The first panel of Table 5 contains regression-adjusted and IPT-weighted estimates of the parameters defined in Model (1), which describe how the risk of adolescent parenthood changes with the cumulative proportion of time spent in moderate- and high-poverty neighborhoods. The regression-adjusted estimates control for observed confounding of neighborhood exposure status

[8] To improve efficiency and avoid disproportionate influence from a small number of outliers, final weights are truncated at the 1st and 99th percentiles (Cole and Hernan 2008).

by conditioning on covariates measured at baseline and cross-time averages of time-varying covariates. These estimates indicate that exposure to neighborhood poverty has only moderate and marginally significant effects on the risk of adolescent parenthood among both blacks and nonblacks. For blacks, compared to continuous residence in low-poverty neighborhoods, the regression-adjusted estimates indicate that sustained exposure to moderate-poverty neighborhoods increases the risk of adolescent parenthood by 57% ($\exp(0.451) = 1.569$), and sustained exposure to high-poverty neighborhoods is estimated to increase the risk of becoming an adolescent parent by 54% ($\exp(0.432) = 1.540$). Among nonblacks, the regression-adjusted estimates for Model (1) indicate that sustained exposure to moderate-poverty neighborhoods increases the risk of adolescent parenthood by 47% compared to continuous exposure to low-poverty neighborhoods ($\exp(0.385) = 1.469$); sustained exposure to high-poverty neighborhoods is estimated to increase the risk of adolescent parenthood by about 80% ($\exp(0.586) = 1.797$).

Regression-adjusted estimators for the effects of time-varying treatments suffer from several known biases which may cause them to systematically understate the effects of neighborhood poverty on adolescent parenthood. Specifically, if observed time-varying confounders, such as family income and parental marital status, are affected by prior exposure to neighborhood poverty, then regression-adjusted estimates suffer from over-control of intermediate pathways and collider stratification bias. IPT weighting overcomes these problems without making additional, unrealistic assumptions about the neighborhood selection process and is therefore a superior approach to estimating the effects of cumulative exposure to neighborhood poverty.

IPT-weighted estimates of the log hazard ratios in Model (1) indicate that neighborhood poverty has substantial and statistically significant effects on the risk of adolescent parenthood for both blacks and nonblacks. For blacks, compared to continuous residence in low-poverty neighborhoods, the IPT-weighted estimates indicate that sustained exposure to moderate-poverty neighborhoods increases the risk of becoming an adolescent parent by about 70% ($\exp(0.539) = 1.714$); sustained exposure to high-poverty neighborhoods has a similar effect, increasing the risk of adolescent parenthood by about 74% compared to continuous residence in low-poverty neighborhoods ($\exp(0.551) = 1.735$). These IPT-weighted estimates are roughly 20% larger than corresponding regression-adjusted estimates for blacks. Among nonblacks, the IPT-weighted estimates indicate that sustained exposure to moderate-poverty neighborhoods increases the risk of adolescent childbearing by about 60% compared to extended residence in low-poverty neighborhoods ($\exp(0.476) = 1.609$), and sustained exposure to high-poverty neighborhoods is estimated to more than double the risk of early parenthood ($\exp(0.825) = 2.282$). IPT-weighted estimates for the effects of cumulative exposure to moderate- and high-poverty neighborhoods are 20% and 40% larger, respectively, than corresponding regression-adjusted estimates for nonblacks. These differences suggest that conventional regression estimators severely understate the effects of neighborhood poverty on adolescent parenthood.

The second panel in Table 5 reports estimates from models that allow for heterogeneous effects of cumulative exposure to neighborhood poverty during childhood versus adolescence. The IPT-weighted estimates have large standard errors, indicating that the available data are sufficient only to imprecisely estimate separate neighborhood effects by developmental stage. Nevertheless, these results provide at least some evidence of effect heterogeneity, where exposure during adolescence appears to be more consequential than exposure during childhood.

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 risk of becoming an adolescent parent while 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 from Model (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 than exposure earlier during childhood.

The lower panel of Table 5 reports effect estimates based on point-in-time measurements of neighborhood poverty taken at age 11. The estimates from conventional regression models with point-in-time measures of neighborhood context approximate the analytic strategy employed in most previous studies of neighborhood effects. As expected, these estimates are substantially smaller than IPT-weighted estimates based on longitudinal measurement of neighborhood context. For example, among blacks, IPT-weighted estimates for the effects of cumulative exposure to neighborhood poverty (Model 1) are more than twice as large as the regression-adjusted estimates based on point-in-time measures of neighborhood poverty (Model 3). Among nonblacks, the 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.

Figure 5 displays estimated age-specific hazard rates of becoming an adolescent parent by selected neighborhood exposure histories. The hazard rates are predicted from Model (1) using the IPT-weighted estimates. They show how the age-specific risk profile for adolescent parenthood would be expected to change if the target population were to experience different trajectories of exposure to neighborhood poverty. According to these estimates, if blacks were continuously exposed to low-poverty neighborhoods, the rate of adolescent parenthood at ages 15, 17 and 19, for example, would be about 0.01, 0.03, and 0.05, respectively. If blacks were continuously exposed to moderate- or high-poverty neighborhoods, by contrast, the rate of adolescent parenthood would be about 0.02, 0.06, and 0.09 for the same ages mentioned above. For nonblacks, estimates indicate that the rate of adolescent parenthood would not exceed 0.02 at any age if this population were to experience sustained exposure to low-poverty neighborhoods. On the other hand, if nonblacks were continuously exposed to high-poverty neighborhoods, the rate of adolescent parenthood would exceed 0.02 by age 17 and climb to about 0.05 by age 19.

DISCUSSION

The effect of growing up in disadvantaged neighborhoods on adolescent parenthood is central to understanding poverty and the reproduction of inequality over time, since having a child during adolescence often precipitates a life of economic hardship for both teen parents and their children. Past research on this issue, however, neglects duration and timing of exposure to poor neighborhoods and does not properly address the dynamic selection processes that define 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, where many studies suggest only a minimal influence for neighborhood context on adolescent parenthood (e.g., Brooks-Gunn, Duncan, Klebanov, and Sealand 1993; Galster et al. 2007; Ginther, Haveman, and Wolfe 2000; Thornberry, Smith, and Howard 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, of course, are not without limitations, but they allow for unbiased and consistent estimation of neighborhood effects under assumptions that are weaker than those required for conventional regression analyses.

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 considerably larger than estimates based on point-in-time measurements of neighborhood context. These differences suggest that it is critically important to account for longitudinal exposures to neighborhood poverty. This study also reveals the importance of dynamic selection and feedback mechanisms, which define how neighborhood poverty impacts sexual behavior during adolescence. The different estimates obtained by means of IPT weighting versus conventional regression indicate that the effects of neighborhood poverty operate indirectly through measured time-varying characteristics of family, such as parental employment, income, and marital status. This finding challenges widely accepted regression-based strategies of adjusting for observed selection in research on neighborhood effects and complicates the neat 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 (see also Sharkey and Elwert 2010). Thus, previous studies that do not measure neighborhood context across time and use conventional regression methods likely provide estimates that seriously understate the impact of neighborhood poverty.

The evidence presented here demonstrates that a temporal, life-course perspective 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 the effects of different longitudinal patterns of exposure 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 (Jencks and Mayer 1990; Wilson 1987; Wilson 1996), with research on neighborhood attainment and mobility (Sampson and Sharkey 2008; South and Crowder 1997a), and with life-course theories of human development (Elder 1998). To advance research on the processes through which poverty is generated and maintained, integration of ecological and temporal perspectives on spatial stratification is essential.

While this study addresses the lack of research on the effects neighborhood poverty using a temporal framework, it nevertheless suffers from several limitations. First, this study focuses on a single outcome, adolescent parenthood, which represents the final stage in a series of decisions about engaging in sexual intercourse, using contraception, and carrying a pregnancy to term. Investigating how neighborhood context influences the proximate determinants of fertility will 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 the time-dependent effects of neighborhood poverty. Thus, additional data are needed to better understand the temporal dimensions of neighborhood effects. Future studies might 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 (Axinn, Pearce, and Ghimire 1999) to record past residential locations. Finally, future research should investigate the specific mechanisms, such as social isolation, collective disorganization, and resource deprivation, thought to transmit the effects of concentrated neighborhood poverty to adolescent outcomes, since this type of mediation analysis is beyond the scope of the present study.

The impact of sustained exposure to impoverished neighborhoods on adolescent parenthood reported in this study captures the deleterious effects of growing up in communities that have been structurally neglected for decades. While the present study cannot speak to the efficacy of specific policy interventions, which must be evaluated on their own terms, it seems clear that a long-term commitment to neighborhood improvement is necessary to resolve the problems identified here. If future generations of children are to progress through the early-life course unencumbered by concentrated neighborhood poverty, lasting structural changes are needed.

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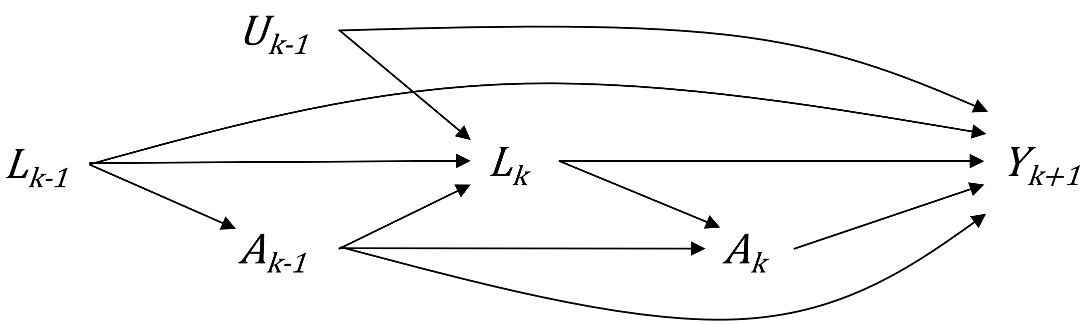
FIGURES

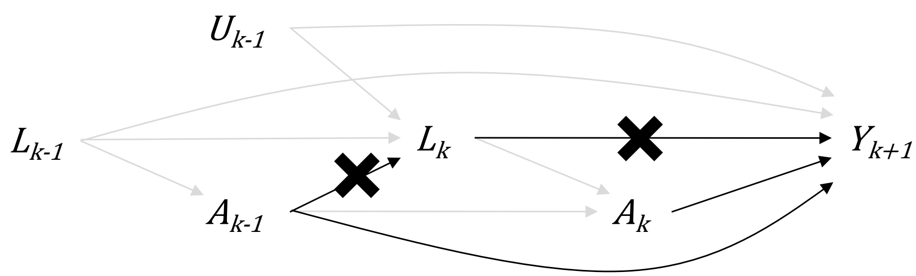
Figure 1. Causal relationships between neighborhood poverty, family characteristics, and adolescent parenthood



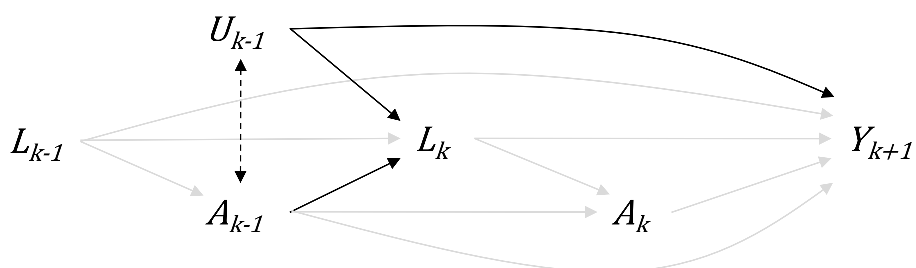
Notes: A_k = neighborhood poverty, L_k = observed time-varying covariates, U_k = unobserved factors, Y_k = outcome.

Figure 2. Consequences of conditioning on time-varying family characteristics affected by past neighborhood poverty

A. Over-control of indirect effects



B. Collider-stratification bias



Notes: A_k = neighborhood poverty, L_k = observed time-varying covariates, U_k = unobserved factors, Y_k = outcome.

Figure 3. Stylized graph illustrating the effect of weighting by the inverse-probability-of-treatment

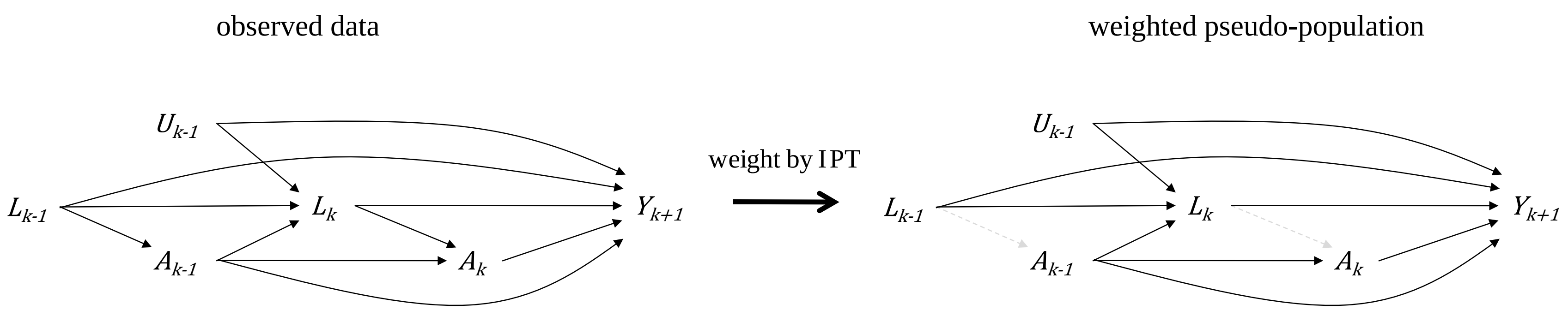
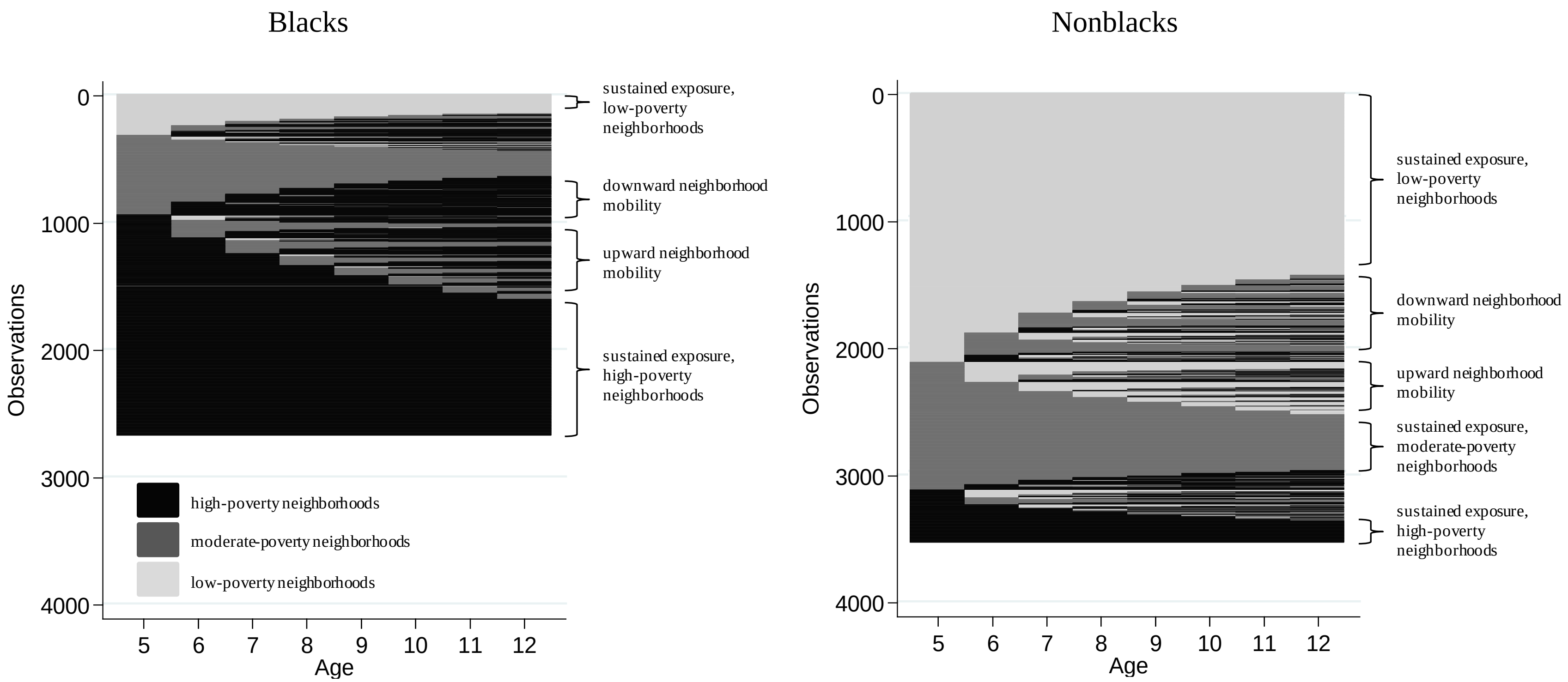
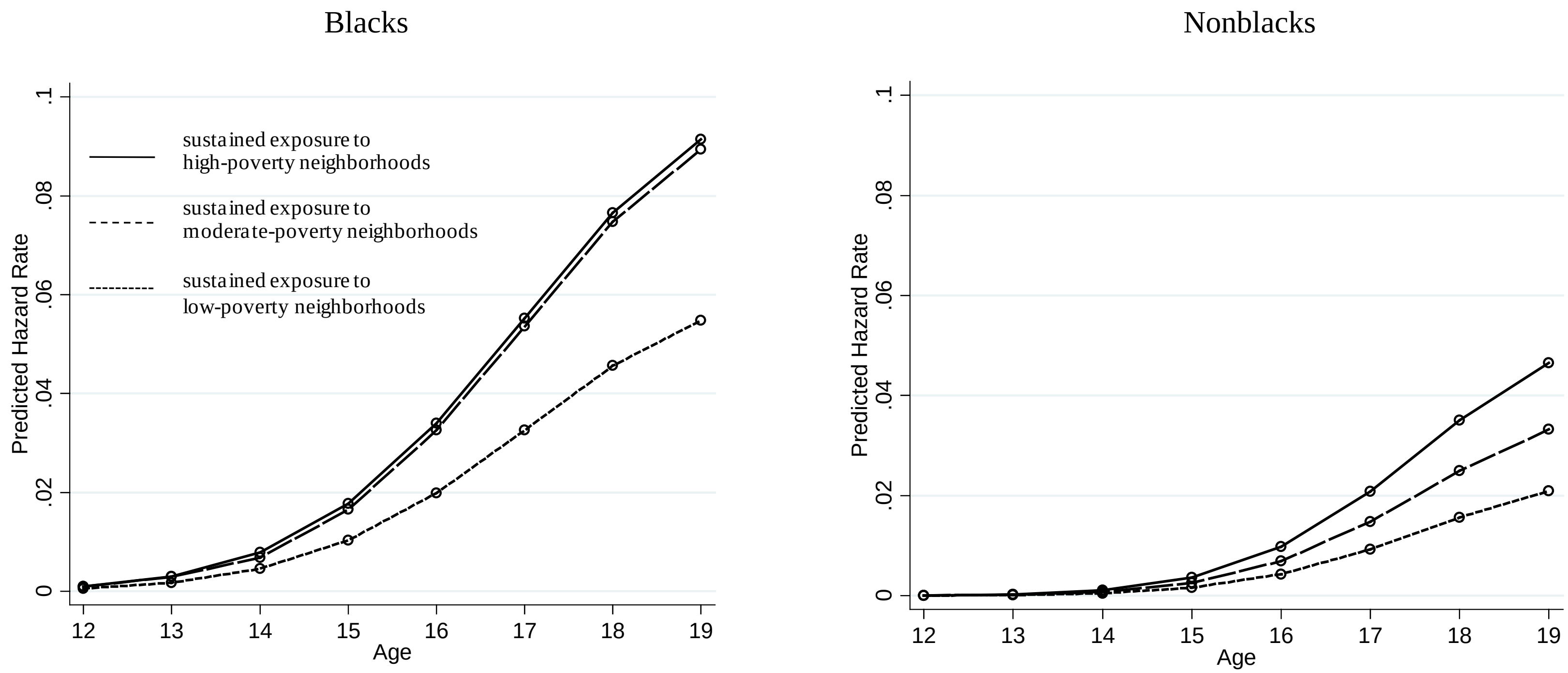


Figure 4. Sequence index plots of treatment trajectories from wave $k = 1$ (age 5) to wave $k = 8$ (age 12)



Notes: plots are based on subjects who remained in the study until wave $k = 8$ (age 12).

Figure 5. Predicted hazard of becoming an adolescent parent by neighborhood exposure history



Notes: hazards were estimated with baseline covariates set to their race-specific mean.

TABLES

Table 1. Time-invariant sample characteristics

Variable	% Missing	Blacks (N=2669)	Nonblacks (N=3573)
Gender, %			
Male	0.00	50.13	50.32
Female		49.87	49.68
Birthweight, %			
>= 5.5 lbs	9.52	91.08	94.57
< 5.5 lbs		8.92	5.43
Mother's marital status at birth, %			
Unmarried	6.62	48.48	7.30
Married		51.52	92.70
FU head's education, %			
Less than high school		43.42	21.80
High school graduate	2.03	29.30	24.71
At least some college		27.28	53.49
Mother's age at birth, mean	4.44	24.74	26.51

Notes: statistics reported for subjects who were not lost to follow-up before age 12.

Table 2. Time-varying sample characteristics

Variable	% Missing	Blacks (N=2669)		Nonblacks (N=3573)	
		Age 4	Age 12	Age 4	Age 12
FU head's marital status, %					
Unmarried	0.00	41.06	48.71	9.57	14.50
Married		58.94	51.29	90.43	85.50
FU head's employment status, %					
Unemployed	0.00	33.01	33.08	8.40	9.71
Employed		66.99	66.92	91.60	90.29
Public assistance receipt, %					
Did not receive AFDC	0.01	76.47	78.16	95.27	95.83
Received AFDC		23.53	21.84	4.73	4.17
Homeownership, %					
Do not own home	0.00	67.33	55.11	32.69	21.75
Own home		32.67	44.89	67.31	78.25
FU income in $1000s, mean	0.00	15.70	17.13	30.13	37.10
FU head's work hours, mean	0.00	27.58	27.30	41.46	40.26
FU size, mean	0.00	5.32	5.10	4.63	4.69
FU head's age, mean	0.00	33.58	39.64	32.39	40.04
Num. residential moves, mean	0.42	0.00	1.73	0.00	1.36

Notes: statistics reported for subjects who were not lost to follow-up before age 12.

Table 3. Hazard of adolescent parenthood by age and race

Age (wave)	Blacks				Nonblacks			
	n	Y_k	C_k	$P(Y_k=1)$	n	Y_k	C_k	$P(Y_k=1)$
12 (k=8)	2669	5	190	0.002	3573	0	252	0.000
13 (k=9)	2474	6	193	0.002	3321	2	251	0.001
14 (k=10)	2275	22	179	0.010	3068	1	225	0.000
15 (k=11)	2074	47	164	0.023	2842	15	226	0.005
16 (k=12)	1863	90	159	0.048	2601	26	263	0.010
17 (k=13)	1614	99	155	0.061	2312	49	216	0.021
18 (k=14)	1360	126	143	0.093	2047	77	186	0.038
19 (k=15)	1091	116	975	0.106	1784	77	1707	0.043

Table 4. Stabilized IPT weights by age (wave) and race

Age (wave)	Blacks				Nonblacks			
	Mean	**SD**	**Q(1)**	**Q(99)**	**Mean**	**SD**	**Q(1)**	**Q(99)**
12 (k=8)	1.01	0.34	0.38	2.19	0.99	0.20	0.50	1.53
13 (k=9)	1.01	0.37	0.38	2.23	0.99	0.21	0.49	1.62
14 (k=10)	1.02	0.52	0.38	2.32	0.99	0.23	0.48	1.68
15 (k=11)	1.03	0.90	0.34	2.37	0.99	0.24	0.47	1.71
16 (k=12)	1.01	0.38	0.33	2.49	1.00	0.26	0.45	1.86
17 (k=13)	1.01	0.40	0.33	2.71	1.00	0.30	0.44	2.02
18 (k=14)	1.01	0.41	0.34	2.68	1.00	0.33	0.43	2.10
19 (k=15)	1.02	0.47	0.32	2.67	1.01	0.43	0.41	2.26

Notes: Q(1) and Q(99) are the 1st and 99th percentiles, respectively.

Table 5. Log hazard rate ratios for the effect of neighborhood (NH) poverty on adolescent parenthood

Model	Blacks (person-years=15420)						Nonblacks (person-years=21548)					
	Reg. adjusted			IPT-weighted			Reg. adjusted			IPT-weighted		
Model 1, coef (se)												
cum. exposure (age 5 to prior wave)												
low-poverty NH	ref	ref		ref	ref		ref	ref		ref	ref	
moderate-poverty NH	0.451	(0.298)		0.539	(0.324)	†	0.385	(0.270)		0.476	(0.271)	†
high-poverty NH	0.432	(0.259)	†	0.551	(0.266)	*	0.586	(0.306)	†	0.825	(0.292)	**
Model 2, coef (se)												
cum. exposure (age 5 to 10)												
low-poverty NH	ref	ref		ref	ref		ref	ref		ref	ref	
moderate-poverty NH	0.143	(0.330)		0.115	(0.370)		0.125	(0.346)		0.230	(0.380)	
high-poverty NH	0.132	(0.320)		0.231	(0.342)		-0.250	(0.449)		-0.127	(0.469)	
cum. exposure (age 11 to prior wave)												
low-poverty NH	ref	ref		ref	ref		ref	ref		ref	ref	
moderate-poverty NH	0.284	(0.260)		0.397	(0.288)		0.228	(0.264)		0.219	(0.274)	
high-poverty NH	0.281	(0.271)		0.314	(0.293)		0.676	(0.337)	*	0.780	(0.346)	*
Model 3, coef (se)												
point-in-time exposure (age 11)												
low-poverty NH	ref	ref		ref	ref		ref	ref		ref	ref	
moderate-poverty NH	0.211	(0.185)		0.242	(0.210)		0.116	(0.193)		0.143	(0.201)	
high-poverty NH	0.167	(0.178)		0.274	(0.195)		0.459	(0.224)	*	0.573	(0.219)	**

Notes: (1) coefficients and standard errors are combined estimates from 5 multiple imputation datasets; (2) †p<0.10, *p<0.05, **p<0.01, and ***p<0.001 for two-sided tests of no effect.

APPENDIX A: NEIGHBORHOOD SELECTION MODELS

Table A.1 Neighborhood (NH) selection models

Covariate	Blacks (person-years=38964)				Nonblacks (person-years=50614)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-invariant characteristics								
Gender								
Male	ref	ref	ref	ref	ref	ref	ref	ref
Female	-0.051	0.031	-0.042	0.031	0.036	0.032	0.041	0.032
Birth weight								
>=5.5 lbs	ref	ref	ref	ref	ref	ref	ref	ref
<5.5 lbs	-0.042	0.054	-0.057	0.054	0.032	0.067	0.034	0.068
Mother's marital status at birth								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	-0.066	0.038	-0.104	0.038	-0.061	0.056	-0.112	0.055
Mother's age at birth (years)	0.005	0.004	0.006	0.003	-0.008	0.003	-0.007	0.003
Year born								
Year-1968	0.003	0.007	0.005	0.007	0.027	0.006	0.027	0.007
Year-1968 squared	0.000	0.000	-0.001	0.000	-0.002	0.000	-0.002	0.000

Table A.2 NH selection models continued

Covariate	Blacks (person-years=38964)				Nonblacks (person-years=50614)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-varying characteristics measured at baseline								
NH exposure status								
Low-poverty NH	ref	ref	ref	ref	ref	ref	ref	ref
Moderate-poverty NH	0.236	0.057	0.236	0.058	0.394	0.034	0.393	0.033
High-poverty NH	0.500	0.060	0.477	0.060	0.704	0.059	0.700	0.059
FU head's education								
Less than high school	0.151	0.042	0.190	0.042	0.243	0.044	0.330	0.044
High school graduate	0.055	0.045	0.089	0.044	0.104	0.037	0.166	0.036
At least some college	ref	ref	ref	ref	ref	ref	ref	ref
FU head's marital status								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	0.101	0.052	0.016	0.046	0.079	0.065	0.087	0.059
FU head's employment status								
Unemployed	ref	ref	ref	ref	ref	ref	ref	ref
Employed	0.057	0.056	0.015	0.055	0.071	0.063	0.010	0.062
Home ownership								
Do not own home	ref	ref	ref	ref	ref	ref	ref	ref
Own home	0.114	0.044	0.056	0.039	-0.027	0.040	-0.072	0.038
Public assistance receipt in past year								
Did not receive AFDC	ref	ref	ref	ref	ref	ref	ref	ref
Received AFDC	0.055	0.055	0.107	0.051	0.072	0.073	0.106	0.070
Moved in past year								
No	ref	ref	ref	ref	ref	ref	ref	ref
Yes	-0.037	0.038	-0.038	0.037	-0.001	0.036	-0.004	0.035
FU income in past year (log $)	-0.123	0.028	-0.198	0.029	-0.075	0.023	-0.125	0.028
FU head's work hours in past year (hrs)	0.001	0.001	0.000	0.001	0.001	0.001	0.001	0.001
FU size	0.000	0.010	0.015	0.008	0.049	0.015	0.037	0.012

Table A.3 NH selection models continued

Variable	Blacks (person-years=38964)				Nonblacks (person-years=50614)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-varying characteristics measured at wave k-1								
NH exposure status								
Low-poverty NH	ref	ref	ref	ref	ref	ref	ref	ref
Moderate-poverty NH	3.082	0.085	3.114	0.083	3.605	0.056	3.636	0.055
High-poverty NH	6.527	0.116	6.585	0.114	6.823	0.117	6.851	0.116
FU head's marital status								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	0.087	0.090	-	-	-0.024	0.083	-	-
FU head's employment status								
Unemployed	ref	ref	ref	ref	ref	ref	ref	ref
Employed	0.039	0.058	-	-	0.071	0.066	-	-
Public assistance receipt in past year								
Did not receive AFDC	ref	ref	ref	ref	ref	ref	ref	ref
Received AFDC	-0.033	0.068	-	-	-0.146	0.098	-	-
Home ownership								
Do not own home	ref	ref	ref	ref	ref	ref	ref	ref
Own home	0.158	0.075	-	-	0.056	0.077	-	-
Moved in past year								
No	ref	ref	ref	ref	ref	ref	ref	ref
Yes	0.034	0.039	-	-	0.043	0.042	-	-
FU income in past year (log $)	-0.038	0.029	-	-	-0.043	0.021	-	-
FU head's work hours in past year (hrs)	0.000	0.001	-	-	0.002	0.001	-	-
FU size	-0.004	0.025	-	-	-0.019	0.031	-	-

Table A.4 NH selection models continued

Variable	Blacks (person-years=38964)				Nonblacks (person-years=50614)			
	Model 1		Model 2		Model 1		Model 2	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Time-varying characteristics measured at wave k								
FU head's marital status								
Unmarried	ref	ref	ref	ref	ref	ref	ref	ref
Married	-0.196	0.088	-	-	0.075	0.091	-	-
FU head's employment status								
Unemployed	ref	ref	ref	ref	ref	ref	ref	ref
Employed	-0.020	0.059	-	-	-0.195	0.061	-	-
Public assistance receipt in past year								
Did not receive AFDC	ref	ref	ref	ref	ref	ref	ref	ref
Received AFDC	0.138	0.069	-	-	0.127	0.102	-	-
Home ownership								
Do not own home	ref	ref	ref	ref	ref	ref	ref	ref
Own home	-0.333	0.079	-	-	-0.161	0.078	-	-
Moved in past year								
No	ref	ref	ref	ref	ref	ref	ref	ref
Yes	-0.472	-0.045	-	-	-0.175	0.053	-	-
FU income in past year (log $)	-0.144	0.031	-	-	-0.155	0.019	-	-
FU head's work hours in past year (hrs)	-0.002	0.002	-	-	-0.002	0.001	-	-
FU size	0.012	0.025	-	-	-0.005	0.030	-	-
Age (wave)								
age	-0.016	0.018	-0.012	0.017	0.015	0.019	0.004	0.019
age squared	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.001

Notes: (1) models are based on all person-year observations contributed by subjects who were present at age 4 in a PSID core family unit (FU) between 1968 and 1989; (2) coefficients and standard errors are combined estimates from 5 multiple imputation datasets; (3) model 1 and model 2 are used to estimate the denominator and numerator of the stabilized IPT, respectively.