Title
Sequential Neighborhood Effects: The Effect of Long-Term Exposure to Concentrated Disadvantage on Children’s Reading and Math Test Scores

Journal
Demography, 55(1)

ISSN
0070-3370

Authors
Hicks, Andrew L
Handcock, Mark S
Sastry, Narayan
et al.

Publication Date
2018-02-01

DOI
10.1007/s13524-017-0636-5

Peer reviewed

Sequential Neighborhood Effects: The Effect of Long-Term Exposure to Concentrated Disadvantage on Children's Reading and Math Test Scores

Andrew L. Hicks[1], Mark S. Handcock[2], Narayan Sastry[3], and Anne R. Pebley[4]

RUNNING HEAD: Sequential Neighborhood Effects on Children's Test Scores

Anne R. Pebley (corresponding author)
e-mail: pebley@ucla.edu

[1]Department of Health Care Policy, Harvard Medical School, USA

[2]California Center for Population Research and Department of Statistics, University of California Los Angeles, USA

[3]Population Studies Center and Survey Research Center Institute for Social Research, University of Michigan, USA

[4]California Center for Population Research and Fielding School of Public Health, University of California Los Angeles, USA

Abstract Prior research has suggested that children living in a disadvantaged neighborhood have lower achievement test scores, but these studies typically have not estimated causal effects that account for neighborhood choice. Recent studies used propensity score methods to account for the endogeneity of neighborhood exposures, comparing disadvantaged and nondisadvantaged neighborhoods. We develop an alternative propensity function approach in which cumulative neighborhood effects are modeled as a continuous treatment variable. This approach offers several advantages. We use our approach to examine the cumulative effects of neighborhood disadvantage on reading and math test scores in Los Angeles. Our substantive results indicate that recency of exposure to disadvantaged neighborhoods may be more important than average exposure for children's test scores. We conclude that studies of child development should consider both average cumulative neighborhood exposure and the timing of this exposure.

Keywords: Child Development, Neighborhoods, Residential Histories, Propensity Funtion Models

Introduction

Lower achievement in reading and math among children from poor, black, and Latino families plays a central role in perpetuating socioeconomic and racial/ethnic inequality across many domains in the United States. Early and continuing development of reading and math skills during childhood is essential for school success, a college education, well-paying and stable employment, and navigating through life (Duncan and Murnane 2011). There is growing consensus in the scientific literature that differences in family, learning, and social environments between disadvantaged and advantaged children play a central role in causing an academic achievement gap (Duncan and Murnane 2011; Heckman 2011; Roksa and Potter 2011; Shonkoff et al. 2012). For example, evidence indicates that children's and adolescents' brain development is negatively affected by exposure to violence and stress but positively affected by cognitive stimulation, opportunities for learning, and warm and supportive relationships with parents and others in their surroundings (Eiland and Romeo 2013; Fox et al. 2010; Walker et al. 2011).

Although research on the determinants of academic achievement has typically focused on family and school characteristics (Phillips 2011a,b), a sizable literature has also examined the effects of children's neighborhoods while taking family characteristics into account (Burdick-Will et al. 2011; Dupere et al. 2010; Jargowsky and El Komi 2011;Sampson 2011; Sastry and Pebley 2010; Sharkey and Faber 2014). This research suggests that residential segregation has a direct effect in creating a socioeconomic and racial/ethnic achievement gap by exposing disadvantaged children to neighborhoods with, for example, lower levels of safety and trust; fewer role models; greater violence, drug use, and other social problems; and poorer quality social institutions, such as schools and enrichment activities. These studies have largely focused on the effects of neighborhood concentrated disadvantage (Sampson et al. 1997), a composite indicator that summarizes several highly correlated neighborhood-level indicators of family or population characteristics, such as poverty, low income, welfare receipt, and presence of minority populations. Recent studies—including randomized experiments and observational studies—have produced mixed results, but they have generally supported the importance of neighborhood effects on children's achievement, particularly the effects of neighborhood concentrated disadvantage (Sastry 2012; Sharkey and Faber 2014).

A potentially important limitation of most studies in this literature is that they considered the effects of neighborhood disadvantage at a single point in time on children's concurrent or subsequent outcomes. However, children are exposed to their current neighborhood environments for different amounts of time and may have previously lived in neighborhoods with quite different characteristics. The attributes of a neighborhood can also change markedly over time. As Sampson et al. (2008) suggested, cognitive development is a cumulative process and likely to be influenced by the duration of exposure to neighborhood disadvantage rather than whether a child was exposed at a single point in time. For this reason, Jackson and Mare (2007), Kunz et al. (2003), and Crowder and South (2011) examined whether cumulative neighborhood quality measures from children's residential histories are more strongly associated with children's developmental outcomes than single point-in-time measures. In general, prior studies found that differences in the effects of the cumulative and single-point-in-time measures were relatively modest, although Crowder and South (2011) found stronger effects of neighborhood socioeconomic status on high school graduation when measured for all years of a child's life rather than at a single point in time. None of these studies, however, controlled for the endogeneity of neighborhood characteristics.

Neighborhood characteristics are likely to be endogenous because parents choose neighborhoods and also investments in their children. Not only do parents choose which type of neighborhood to live in initially, but they also choose whether to stay or move (and where to move) throughout their sons' and daughters' childhood. These subsequent choices are likely to be influenced by previous neighborhood choices and by the effects of these previous choices on the child's and family's time-varying characteristics (Harding et al. 2011; Wodtke et al. 2011). For example, parents who moved to a neighborhood because of low housing costs and a shorter commute to work, but then found the local school is not good for their child, may subsequently choose a neighborhood with a longer commute but better schools. Wodtke et al. (2011) labeled this complex problem "dynamic neighborhood selection."

To advance research on the effects of neighborhood and other contextual exposures on children's academic outcomes, studies must account for the endogenous selection processes that lead to different levels of cumulative exposure. More generally, many types of demographic and social research investigating the cumulative effects of individual histories (e.g., marriage, poverty, poor health, unemployment, contraceptive use) on subsequent outcomes face the same methodological problem. Two studies addressed endogenous neighborhood selection in studies of children's educational outcomes (Sampson et al. 2008; Wodtke et al. 2011) and showed that doing so is important. Both studies employed a propensity score technique for longitudinal data, known as a *marginal structural model with inverse probability of treatment weighting* (IPTW). As outlined later, however, this approach has several important disadvantages and limitations.

In this study, we consider the problem of modeling the effects of cumulative histories of neighborhood disadvantage on children's test scores while accounting for endogenous neighborhood selection. We make two key contributions. Our primary contribution is methodological: we present and apply a novel statistical approach that overcomes certain limitations and disadvantages of IPTW models used in recent research. Our alternative strategy is based on a propensity function approach in which cumulative neighborhood effects are modeled as a continuous treatment variable. To test our approach, we estimate models using data from the Los Angeles Family and Neighborhood Survey (L.A.FANS). We compare estimates using this new approach to those from the IPTW model proposed by Wodtke et al. (2011) and show that although the results are qualitatively similar, our approach has certain distinct strengths. A second contribution is to use these estimates to examine the effects of cumulative exposure to neighborhood disadvantage on children's test scores in Los Angeles. As Burdick-Will et al. (2011) noted, much of the evidence on the effects of neighborhood disadvantage comes from studies based on a single city: Chicago. Los Angeles provides a useful comparison for previous results. We also extend the literature on the cumulative effects of neighborhood disadvantage for children's development by examining two dimensions of exposure: (1) an average "treatment" effect of living in a disadvantaged neighborhood, which captures the cumulative experience of residence in disadvantaged neighborhoods; and (2) an effect of the recency of this treatment—that is, how recently a child was exposed to neighborhood disadvantage.

Neighborhood Effects on Children's Achievement

In the past three decades, scholars have proposed several theories about why exposure to disadvantaged residential neighborhoods may influence child and adolescent development. Recent reviews of this literature include Sampson (2011), Sastry (2012), Harding et al. (2011), and Sharkey and Faber (2014). The theories largely imply that for negative effects to occur, children must be exposed to disadvantaged neighborhood for an extended period (Wodtke 2013) and that the effects are persistent. We briefly summarize these ideas and then subsequently return to the role of cumulative exposure and recency.

Limited Neighborhood Resources

Neighborhood deprivation hypotheses suggest that disadvantaged neighborhoods are likely to offer poorer resources and weaker institutions for children and families (Peterson et al. 2000; Small et al. 2008; Wo 2016). For example, the quality of childcare and schools may be low, and after-school homework programs, playgroups, libraries, and recreational activities may be scarce or nonexistent. Parents in more-affluent neighborhoods typically demand more and better public resources and can afford to pay for private resources, if necessary. Poorer neighborhoods may also be worse off because the greater needs of residents may overtax the existing resources (Pebley and Sastry 2004).

Social Organization

Social organization theory posits that structural conditions in disadvantaged neighborhoods (e.g., poverty, high levels of unemployment and residential instability) lead to low levels of trust, shared norms, social capital, and willingness to take collective action to regulate behavior, such as truancy, violence, and gang activity (Coleman 1988; Sampson 2012; Sampson et al. 1999; Shaw and McKay 1942). Among other consequences, children may receive less reinforcement for school attendance and achievement as well as less censure for antisocial and potentially self-destructive behaviors.

Social Isolation

Several observers have argued that disadvantaged neighborhoods are more socially isolated than neighborhoods with higher socioeconomic status and political power: residents are less likely to have extralocal social ties that provide information and access to opportunities, services, outside ideas (Coleman 1988; Edin 1991; Massey and Denton 1993; Tigges et al. 1998). Thus, neighborhood residents may find it harder to solve problems and to obtain better resources for children and families. In addition, low-skilled adults who earn low wages and have a high frequency of unemployment are concentrated in disadvantaged neighborhoods, often at a distance from good jobs (Gobillon et al. 2007; Stoll and Covington 2012). These neighborhoods may, therefore, offer few successful adult role models and have weaker norms about the importance of school achievement (Coleman 1988; Sampson et al. 1999; Wilson 1987).

Violence and stress

Violence and stress have received considerable attention as potential factors impeding children’s development in disadvantaged neighborhoods through mechanisms such as poorer mental health (Aneshensel and Sucoff 1996; Wheaton and Clarke 2003), biological stress responses (McCoy et al. 2015), and more punitive parenting practices (Morrison Gutman et al. 2005). For example, Sharkey and colleagues have shown that acute exposure to violence can affect children’s attention, impulse control, and academic performance (Sharkey 2010; Sharkey et al. 2012).

Cumulative Exposure and Recency

In our analysis, we hypothesize that two aspects of exposure to neighborhood disadvantage are important for children's academic achievement: cumulative exposure and recency of exposure. As noted earlier, much of the literature on neighborhood disadvantage implies that children exposed to neighborhood disadvantage for substantial periods are more likely to experience negative effects. For example, the stress on children and their parents of a high-crime and violent neighborhood may have limited effects if they live there for a short time. Longer-term neighborhood residence or residence in a sequence of high-crime neighborhoods is likely to have a much more profound and cumulative effect. In our analysis, the cumulative exposure to neighborhood disadvantage is based on a mean expected treatment score, reflecting the degree of disadvantage in neighborhoods a child has lived in weighted by the duration spent in each neighborhood. Consistent with the literature, we expect that greater cumulative exposure to neighborhood disadvantage is associated with poorer test scores.

The recency of exposure may also be important because effects of earlier exposure may or may not fade over time. Sharkey (2010) provided evidence that effects do fade: he found that the effects of a local homicide in Chicago on African American children's vocabulary and reading test results were largest immediately after the event but diminished over time. Although exposure to neighborhood disadvantage may not be comparable with experiencing a single homicide, children who move to more-advantaged neighborhoods from a disadvantaged one plausibly are able to make up for their earlier experience in terms of improving academic skills. However, a large literature on the importance of early childhood neural and social development for later academic performance suggests that early exposure to certain types of disadvantage may have larger effects than exposure later in childhood (Johnson and Schoeni, 2011) .**[AU: Need**

citations for the preceding sentence? Teresa] Thus, the probable direction of the association between recency and children's test scores in this analysis is not clear.

Data and Methodological Challenges

Investigating the effects of neighborhood characteristics over time is challenging for three reasons. First, data requirements are substantial because information is needed on residential locations over a considerable period. Second, as described earlier, because parents' decisions about where to live reflect each family's choices and constraints, the characteristics of the neighborhoods in which children reside are determined endogenously (Duncan and Raudenbush 1999; Manski 1995). Third, the effects of children's exposure to disadvantaged neighborhoods are likely to be cumulative and lagged (Sampson et al. 2008). We discuss the methodological implications of each of these challenges as well as the data requirements and statistical options for addressing them.

Data Challenges: Measuring Exposure to Disadvantaged Neighborhoods

A challenge in examining the effects of cumulative neighborhood exposures on children's outcomes is the need for prospective longitudinal data that include frequent or continuous measures of residential location and moves, family processes, and children's outcomes. In practice, this is a very high bar, and only a handful of studies have been able to meet these requirements. Among these studies are the Project on Human Development in Chicago Neighborhoods (PHDCN), the Panel Study of Income Dynamics (PSID), and the Los Angeles Family and Neighborhood Survey (L.A.FANS).

Important first steps in analyzing the effects of exposure to disadvantaged neighborhoods on children's outcomes are to: (1) characterize children's residential histories and (2) understand the factors associated with residential moves. Several studies have demonstrated that children's exposure to disadvantaged neighborhoods varies considerably by characteristics, such as race and income. For example, African American children are much more likely than whites to spend long, uninterrupted periods in disadvantaged neighborhoods and are more likely to return to disadvantaged neighborhoods if their families succeed in moving out (Briggs and Keys 2009; Quillian 2003; Timberlake 2009). Residential moves are highly selective and more likely to involve white and upwardly socioeconomically mobile families (Geronimus et al. 2014; Pettit and McLanahan 2003). Socioeconomic, racial/ethnic, and other characteristics influencing children's residential histories may also affect their outcomes, thus highlighting the importance of considering selection into and out of neighborhoods in studies of the effects of disadvantaged neighborhoods on children's well-being.

Statistical Modeling Challenges

Accounting for the endogenous selection processes that determine children's exposure to a particular set of neighborhood characteristics has challenged researchers for a considerable time. There are no widely accepted solutions to this challenge, although many different statistical modeling approaches have been employed, each of which has a variety of data requirements along with specific strengths and limitations.

The most common approach for research examining neighborhood effects on children's outcomes is to attempt to control for all possible observed child and family characteristics that shape both neighborhood selection and children's outcomes (Sastry and Pebley 2010). This approach responds to the critique that the endogenous selection mechanism for neighborhood of residence is a problem insofar as it is the result of *unmeasured* characteristics. Many recent studies—including L.A.FANS—have, as a consequence, collected detailed information about child and family processes. These measures cover aspects of children's and parents' characteristics that were not previously measured well or at all, and include such indicators as cognitive assessments of parents themselves and more detailed indicators of family income and wealth. Incorporating such measures into studies of neighborhood effects on children's outcomes provides clearer and more convincing estimates of these effects.

Despite better and more comprehensive measurement of previously omitted child and family variables, unmeasured or unmeasurable characteristics remain an important concern. An ambitious attempt to address this issue directly used an experimental design to randomly assign families to neighborhoods through a lottery for a housing subsidy. The Moving to Opportunity (MTO) study uncovered mixed effects of the program, with small or no significant effects on test scores for children after four to seven years (Burdick-Will et al. 2011; Sanbonmatsu et al. 2006) or a range of other schooling outcomes in the final impact evaluation of the program (Gennetian et al. 2012). Among the numerous critiques of the MTO experiment, the most salient from our perspective is that MTO did not experimentally randomize neighborhood quality but rather reflected a specific intervention focused on receipt of a housing voucher and a particular type of move from a high-poverty to a low-poverty neighborhood (Clampet-Lundquist and Massey 2008; Sampson 2008). Furthermore, neighborhood changes occurred along with residential moves, making it difficult to disentangle the two effects.

The rest of the neighborhood effects literature has used a variety of statistical techniques to account for neighborhood endogeneity, including family fixed-effects models, instrumental variable approaches, and propensity score matching (Aaronson 1998; Alvarado 2016; Evans et al. 1992; Foster and McLanahan 1996; Harding 2003; Plotnick and Hoffman 1999; Solon et al. 2000). None of these approaches are wholly satisfactory; see Dietz (2002).

The newest statistical approach to be applied to studying neighborhood effects on children's outcomes is marginal structural models that use IPTW. Two recent studies have applied this approach, which draws on statistical techniques from epidemiology (Hernán et al. 2000; Robins et al 2000). Using longitudinal data from the PHCDN and from the PSID, respectively, Sampson et al. (2008) and Wodtke et al. (2011) employed marginal structural models based on trajectories of exposure among children to different neighborhood characteristics over time.

These models have a number of strengths. For instance, they directly address the issue of neighborhood selection as it unfolds over time, which creates different trajectories of exposure. However, these models also suffer from four limitations. First, like most other approaches based on propensity score techniques, they focus exclusively on accounting for observed child and family measures; hence, they are susceptible to the potential effects of unmeasured or unmeasurable factors that shape neighborhood exposures and children's outcomes.

Second, neighborhood characteristics are multidimensional, but these studies offer a circumscribed treatment of exposure. Although much of the literature has largely focused on neighborhood concentrated disadvantage, Sampson et al. (2008) further restricted attention to a dichotomous indicator of exposure to a neighborhood with high concentrated disadvantage. Wodtke et al. (2011) considered five ordinal treatments based on levels of exposure to neighborhood disadvantage in each year. Nonlinear effects cannot be examined with two categories of exposure, and Wodtke et al. (2011) included only a limited investigation of nonlinear effects of exposure. Although neighborhood exposures are difficult to conceptualize in terms of discrete categories, propensity score methods are generally confined to binary or ordinal treatment scenarios. However, Imai and van Dyk (2004) generalized Rosenbaum and Rubin's

(1983) propensity score by developing the *propensity function* to allow for continuous treatment regimes—an approach that we adopt in this study.

Third, Wodtke et al. (2011) did not examine the sequence or timing of exposure to neighborhood disadvantage. Sampson et al. (2008), on the other hand, examined specific sequences of exposure by examining just two types of neighborhood environments across three time points. Wodtke et al.'s (2011) long observation period of 17 years and use of five treatment categories made it difficult for them to use Sampson et al.'s (2008) approach to examining specific sequences of exposures because of their multiplicity. Wodtke et al. (2011) instead summarized exposure using a mean of each period's treatment category across all observation periods. Thus, children who spent the first year of the observation period in a disadvantaged neighborhood and those who spent the most recent year in that environment were classified as having equivalent exposure. In this study, we incorporate an indicator of the recency of exposure to neighborhood disadvantage

Finally, inverse probability of treatment (IPT) weights are susceptible to imprecise estimation, which can lead to unstable or large weights—an experience Wodtke et al. (2011) reported.

In this study, in order to benchmark our results, we replicate the marginal structural models with IPTW using L.A.FANS data following the approach used by Wodtke et al. (2011). This replication differs only in our use of L.A.FANS data with its shorter observation period of 6 years (rather than 17 years) and in the outcome measure (reading and math test scores, as opposed to high school graduation as in Wodtke et al.). We also propose and apply a new statistical approach to characterizing exposure to disadvantaged neighborhood environments that does not rely on arbitrary categories of neighborhood conditions, as has been the case in prior research. Rather, we allow exposure to disadvantaged neighborhoods to be continuous, which provides a more realistic characterization of exposures and greater potential scope for being able to discern their causal effects on children's achievement.

One implication of our novel approach is that it mitigates certain methodological limitations of the standard marginal structural modeling approach, which often result in large and unstable weights. Our approach also allows us to more naturally consider nonlinear effects of neighborhood exposures over time, which we extend further by using generalized additive modeling (Hastie and Tibshirani 1990). It conceptualizes neighborhood exposure as a function of time and allows us to take into account more than one dimension of exposure to neighborhood conditions. We consider the joint effects of six years of exposure and use dimensional reduction to summarize these effects as functions of the cumulative exposure and the recency of exposure. The new framework proposed can be used in similar studies where treatments are longitudinal and have complex effects.

Methods

Data

Data used in this analysis came from the two completed waves of the Los Angeles Family and Neighborhood Survey (L.A.FANS), a longitudinal study of households and neighborhoods in Los Angeles County, California. In Wave 1, a stratified, multistage, clustered random-sample survey of 3,100 households in 65 neighborhoods (defined as 1990 census tracts) in Los Angeles County was conducted between April 2000 and December 2001 (Sastry et al. 2006). In households with children (70 % of the sample), one child was chosen at random from all household members aged 17 and younger. If the child had siblings, one was chosen at random as a second sampled child. The sampled children's primary caregivers (PCG; usually the mother) were asked to complete questions about their own marital and work history, family income and homeownership, the family unit head's education, and the sampled child's health and residence history. The response rate the Wave 1 sample of children was 86 % (Sastry and Pebley 2003).

Wave 2 of L.A.FANS was fielded between August 2006 and December 2008. This wave followed all respondents from Wave 1 and added a sample of new entrants who moved into the 65 sampled neighborhoods between the two waves using similar rules as in Wave 1. In Wave 2, respondents completed a similar set of questionnaire instruments except that the duration of retrospective reports was expanded to obtain a complete residential history for all respondents between 2001 and 2006. The Wave 2 response rate for children in the panel sample was 63 %, and the response rate for children in the new entrant sample was 82 % (Peterson et al. 2011). The vast majority of noninterviewed cases in Wave 2 were refusals among cases that were located and contacted.

Our analysis sample comprises 611 children for the math score analysis and 616 children for the reading score analysis. To be included in our analysis sample, children had to (1) participate in Waves 1 and 2 of L.A.FANS (and hence were aged 3–11 years in Wave 1 and 9–17 years in Wave 2), (2) have been eligible for and completed a test in Wave 1, and (3) have coresided with their PCGs in Los Angeles County between waves to accurately assign neighborhood characteristics over the full study period. A total of 259 children for whom we did not have a residential history because they did not meet the PCG coresidence requirement were omitted from the analysis sample; we also dropped 185 children who did not complete a test in Wave 2 but completed other study components. We accounted for children who were lost to follow-up in Wave 2 through our use of the L.A.FANS panel weights. By design, the L.A.FANS panel weights do not apply to children who moved into or out of Los Angeles County between

Waves 1 and 2 because they are not members of the L.A.FANS in-person longitudinal sample (see Peterson et al. 2011). For these excluded children, we had data from at least one wave, which allowed us to assess how different they are from children in the sample. We found no evidence of systematic differences between these two groups in test scores or other characteristics.

Child Achievement Outcomes

Child reading and math achievement were assessed using two subtests of the Woodcock-Johnson Revised (WJ-R) Tests of Achievement, administered to children during the second wave of L.A.FANS. The Letter-Word Identification subtest assesses symbolic learning and reading identification skills. The Applied Problems test assesses mathematics reasoning. Raw scores were converted to standardized scores based on the child's age and national norms (McGrew et al. 1991). Standardizing scores by age allow outcomes to be compared across children of different ages. The standard scores have a population mean of 100 and a standard deviation of 15. The mean standardized scores for the math and reading subtests for children in this sample were 104.7 and 106.9, respectively, which were slightly higher than the national norms of 100 for each test. The sample standard deviations of 19.0 for reading and 20.6 for math were slightly higher than the national standard deviation of 15.

Cumulative Exposure to Neighborhood Concentrated Disadvantage

Our key independent variable of interest is children's cumulative long-term exposure to neighborhood concentrated disadvantage, a structural measure of several associated dimensions of neighborhood poverty, economic disadvantage, racial segregation, and family structure that has been widely used in previous research as a focal neighborhood characteristic (Sampson et al. 1997; Sastry 2012; Wilson 1987). Factor analysis was used to reduce the dimensionality of the multiple tract-level indicators of neighborhood disadvantage to a single variable. The specificindicators include the race and age composition of the population and the prevalence of female-headed households. We also included traditional indicators of economic disadvantage, comprising family-level indicators of poverty rates, public assistance receipt, and low income. We used these specific tract-level indicators in light of previous research characterizing neighborhood disadvantage that also found a disproportionate exposure to disadvantage among particular population segments based on the included demographic variables (Sampson et al. 1997). The factor analysis results are shown in Online Resource 1, Table S1.

We constructed an indicator of exposure to neighborhood concentrated disadvantage for each child using their residential histories and information from the census on characteristics of the tracts in which they resided. We first created a complete residential history for each child between 2001 and 2006 using data from Waves 1 and 2 of L.A.FANS. For each calendar year in this period, we identified the child's home census tract. Although most children remained in the same neighborhood (or type of neighborhood) over the study period, a substantial fraction moved to better or worse neighborhoods. During the six-year period, 41 % of children experienced at least one change in disadvantage quintile, 33 % had at least one change from a high quintile to a lower quintile, and 13 % experienced a change from a lower quintile to a higher one. Next, we used census data on tract characteristics to compute an annual score of neighborhood concentrated disadvantage for each tract in Los Angeles County for each year of the study period. Finally, we assigned the values of the concentrated disadvantage score by tract and year to each child based on his or her residential history.

Tract-level data came from the 2000 U.S. Census and from the 2006–2010 American Community Survey (ACS) five-year estimates. We estimated values for six individual tract measures for each year from 2001 to 2007 using linear interpolation between the observed values for 2000 and 2008 (the midpoint of the five-year ACS estimates) for all census tracts in Los Angeles County: (1) percentage of households headed by females with children; (2) percentage of families with income less than $25,000; (3) percentage of the population that is nonwhite/non-Asian/non–Pacific Islander; (4) percentage of individuals in poverty; (5) percentage of the population under age 18 years; and (6) percentage of households receiving public assistance. Following the approach of Sampson et al. (1997), we performed a factor analysis using annual data on the six neighborhood variables for all census tracts in Los Angeles County from 2000 to 2008 to generate a composite score of neighborhood concentrated disadvantage for each tract in each year. The resulting neighborhood concentrated disadvantage scores ranged from –1.15 to 4.45, with higher scores indicating higher levels of neighborhood disadvantage.

(place Table 1 about here)

Table 1 shows summary neighborhood characteristics by quintile of the concentrated disadvantage score across all tracts and years from 2000 to 2008. Moving from the first to the fifth quintile, each of the six neighborhood indicators of disadvantage increased monotonically. Across all measures, tracts in the first quintile are substantially better off than those in the fifth quintile. For instance, 6 % of the population was living in poverty for tracts in the first quintile compared with 34 % for tracts in the fifth quintile. Less than 1 % of households in the first quintile received welfare benefits compared with 13 % in the fifth quintile.

Covariates

Our analysis incorporated an extensive set of baseline and time-varying covariates to control for potential confounding by background individual and family characteristics. Table 2 includes a list of the full set of model covariates along with weighted and unweighted summary statistics.

(place Table 2 about here)

Individual-level baseline characteristics include children's age, sex, race/ethnicity, and birth weight. The weighted average age of children in the sample in 2001 was 7.2 years. The sample contains slightly more males (52 %). The majority of children (53 %) are Latinos, reflecting the demographic composition of Los Angeles County. Blacks account for 10 % of the sample. We combined whites and other smaller racial groups (Asians, Pacific Islanders, and Native Americans) into a third category, representing 37 % of the sample. Children with low birth weight (less than 5.5 pounds) account for 9 % of the sample.

Mother and family time-invariant characteristics include the mother's age and marital status at the child's birth, the mother's standardized test score from the WJ-R Passage Comprehension test of reading skills, the family unit head's educational attainment, and whether the family owned their home at Wave 1 of the survey. A majority of mothers (60 %) were married at time of childbirth, and the average age at childbirth was 28 years. Mothers had a mean score of 85 on the reading assessment, which is 1 standard deviation below the national average. Two-thirds of households were headed by a family member with at least a high school diploma (67 %). Most families (58 %) did not own a home at the time of the Wave 1 interview.

We also included time-varying family characteristics at annual intervals between 2001 and 2006; Table 2 shows summary statistics at baseline. These covariates include the mother's marital status, number of children, employment status, and work hours, as well as the log of the family unit's income and whether the family received any public assistance in that year. A majority (69 %) of PCGs remained married for all six years of the study. Less than one-half (48 %) remained employed throughout the study. On average, PCGs worked 23 hours per week throughout the course of the study and had 1.8 children. The median family income for this period was \$40,000, and 9 % of families received public assistance at some point between 2001 and 2006.

Statistical Methods

We used two approaches to investigate the effects of exposure to neighborhood disadvantage on children's reading and math test scores. First, we employed the propensity function approach developed by Imai and van Dyk (2004) for nonbinary treatment regimes, which allowed us to model the effects of neighborhood disadvantage as a continuous treatment variable. Second, we used marginal structural models with IPTW to examine the effects of exposure to neighborhood disadvantage, which allowed us to apply an approach that has previously been used in the neighborhood effects literature.[1]

Propensity Function Approach

Our first step in applying the propensity function approach of Imai and van Dyk (2004) was to specify a model for the propensity function. This function represents each child's probability of receiving the treatment of exposure to neighborhood disadvantage. Let $T_i = (T_{1i}, T_{2i}, \ldots, T_{6i})$ represent each child's neighborhood disadvantage at Years 1–6 (which correspond to years 2001–2006), and let $\mathbf{X}_i$ be the set of child- and family-level covariates for respondent i, measured at each year and at the baseline wave of the survey. The propensity function, $P(T_i|\mathbf{X})$, models the distribution of the vector of treatment variables, T_i, as a function of the covariates in $\mathbf{X}$. Following Imai and van Dyk (2004), we assume that $P(T_i|\mathbf{X})$ depends on $\mathbf{X}$ only through some function $\theta_\psi(\mathbf{X})$, where ψ is a parameter. Let $Y(t)$ be the potential outcomes for children's math or reading test scores for children exposed to treatment $t = (t_1, t_2, \ldots, t_6)$. Our main results are

[1] The R and Stata programs used to implement these procedures are provided in Online Resource 2.

based on estimating the expected outcome for each treatment exposure while controlling for the covariates in **X** using the following model:

$$E\left(Y(t)\middle|\mathbf{X}=x\right)=\int E\left(Y(t)\middle|\theta_{\psi}(x)=s,\mathbf{X}=x\right)p\left(\theta_{\psi}(\mathbf{X})=s\right)ds. \tag{1}$$

Based on the assumption of strong ignorability of the treatment assignment given the propensity function, we have

$$E\left(Y(t)\middle|\mathbf{X}=x\right)=\int E\left(Y(T)\middle|T=t,\theta_{\psi}(x)=s,\mathbf{X}=x\right)p\left(\theta_{\psi}(\mathbf{X})=s\right)ds. \tag{2}$$

To estimate Eq. (2), we needed to specify the terms within the integral. We modeled $E(Y(T)|T = t, \theta_{\psi}(x) = s, \mathbf{X} = x)$ as a smooth function of s and x, the propensity function statistic and the covariate values, respectively. Specifically, we used a generalized additive regression model (Hastie and Tibshirani 1990) based on a joint tensor-spline function of t and s that includes the covariates in **X**. To specify $\theta_{\psi}(\mathbf{X})$ and estimate ψ, we modeled the joint distribution of T_i as multivariate Gaussian with distribution expressed as

$$P\left(T_i\middle|\mathbf{X}\right)=P\left(T_{1i}\middle|\mathbf{X}\right)P\left(T_{2i}\middle|T_{1i},\mathbf{X}\right)P\left(T_{3i}\middle|T_{2i},\mathbf{X}\right)P\left(T_{4i}\middle|T_{3i},\mathbf{X}\right)P\left(T_{5i}\middle|T_{4i},\mathbf{X}\right)P\left(T_{6i}\middle|T_{5i},\mathbf{X}\right), \tag{3}$$

where $\mathbf{X} = (\mathbf{X}_1, \ldots, \mathbf{X}_n)$ is the combined matrix of covariates for the n respondents. Each component of the Eq. (3) was modeled as a separate Gaussian additive model.

Whereas Imai and van Dyk generated a single propensity function from a cross-sectional data set, we produced six sequential propensity functions that model a child's propensity for exposure to neighborhood disadvantage in each year of the observation period between Waves 1 and 2 of L.A.FANS. We used intervals of one year, allowing us to compare our methods to the IPTW model proposed by Wodtke et al. (2011).

To compute the expected value of the propensity function for each child at each year, we averaged across a large number, m, of simulated responses from each year's distribution. This technique allowed us to use expected values of the propensity function rather than the model's predicted values. Respondent *i*'s expected propensity for neighborhood disadvantage at each year was then averaged across the six years to obtain an overall mean expected treatment score, $\bar{T}_i$:

$$\bar{T}_i = E\left(T_i\right) = \frac{1}{6}\sum_{t=1}^{6}\frac{1}{m}\sum_{k=1}^{m} T_{itk}, \tag{4}$$

where T_{itk} is the kth simulated response of respondent i's neighborhood disadvantage distribution at year t = 1 – 6. We chose m = 10,000 simulated responses in our computations.

Because the overall mean expected treatment score provides a single summary measure of neighborhood exposure across multiple years (2001–2006), we looked for any time trend in the relationship among the neighborhood disadvantage scores within the six-year study period. We performed a principal components analysis of the six annual scores. As shown in Table 3, factor loadings of the first component, which accounted for 95 % of the total variance in the six expected annual neighborhood disadvantage scores, were roughly equal across the six years. This finding suggests an absence of differences in exposure to neighborhood disadvantage across the six years in terms of their contributions to the main component of the treatment score, and that the overall mean expected treatment score for person i over the six years, $\bar{T}_i$, could be used as an indicator of exposure to neighborhood disadvantage covering the entire six-year study period.

(place Table 3 about here)

However, the principal components analysis also revealed a second component, which accounted for the majority of the remaining variance (3 % of the total variance). Factor loadings for the second component were positive for the most recent scores and negative for the earliest scores, with a nearly linear decline in the weights across the six years (see Fig. S1 in Online Resource 1). We interpret this component as capturing the effect of the timing of exposure to neighborhood disadvantage. This second component, which we call *recency*, was linear, and we incorporated it into our regression models by generating an expected recency score, R_i:

$$R_i = \frac{1}{21}\sum_{t=1}^{6} t\left(T_{it} - \bar{T}_i\right), \qquad (5)$$

where T_{it} is the expected treatment for person i at year t = 1 – 6, and $\bar{T}_i$ is the mean expected treatment. The difference between the year-specific treatment and the mean treatment across all years was computed and summed across the six years. We included a linear weight of t = 1 – 6 so that later years would be given more weight than earlier years. The resulting expected recency measure ranges from –0.45 to 0.41, with positive scores indicating higher levels of neighborhood disadvantage in later years of the analysis (when compared with the child's overall levels of disadvantage) and negative scores indicating lower levels of disadvantage in later years (compared with overall individual disadvantage). Based on these results, we modeled children's exposure to neighborhood disadvantage as a function of their mean expected treatment score and recency score: $\theta_\psi(\mathbf{X}) = (\bar{T}, R)$.

The density and scatter plots of mean expected treatment versus recency are displayed in Fig. 1. Recency scores have a mean of –0.04, indicating that the average child experiences lower levels of neighborhood disadvantage in later years of the study. Although mean expected treatment and recency scores are negatively correlated, the average recency score for children with the lowest levels of neighborhood disadvantage is approximately 0, suggesting that children in more-advantaged neighborhoods have lower mobility across the disadvantage scale and tend to stay in high-advantage neighborhoods throughout the course of the study. Likewise, children experiencing higher overall expected levels of neighborhood disadvantage have, on average, negative recency scores, indicating higher disadvantage levels earlier in the analysis period rather than later. This pattern suggests that neighborhood conditions are improving among children exposed to neighborhood disadvantage as they age, either through residential mobility or neighborhood improvements.

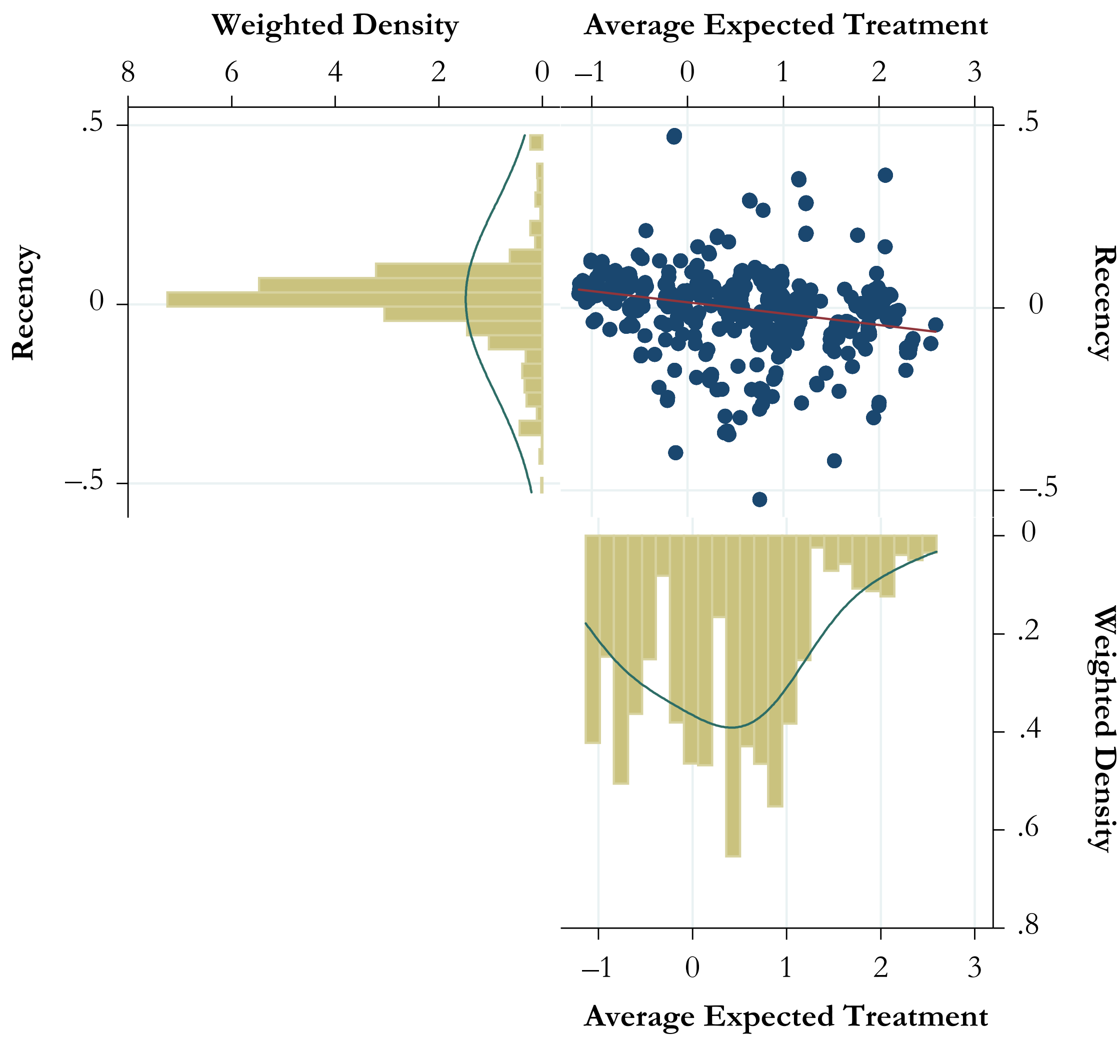

Fig. 1 Density and scatter plots for average expected treatment versus recency of treatment

We imputed missing values of covariates describing family, marital, employment, and income characteristics using other household data from the same year, creating 25 imputed data sets. All the estimates that we present were combined from results using the 25 imputed data sets, and standard errors were adjusted for variability between imputations. Finally, the estimated standard errors accounted for clustering of observations by family using the robust variance estimation.

Inverse Probability of Treatment Weighted Approach

To apply the IPTW approach, we first constructed a measure of treatment, which in this study is children's exposure to neighborhood disadvantage. Using year-specific quintiles of neighborhood disadvantage coded from 1 (lowest quintile of neighborhood disadvantage) to 5 (highest), we constructed a measure of duration-weighted exposure for each child as the mean across the six study years of the neighborhood disadvantage quintile in which a child resided at each year.

Conventional regression models adjust for neighborhood selection by controlling for time-varying individual and family characteristics that may be associated with neighborhood choice. However, these models may overcontrol the indirect pathways because these individual and family characteristics are also associated with child outcomes, which can lead to underestimating the true long-term effects of neighborhood disadvantage. The goal of the IPTW approach is to create a pseudo-population in which the treatment variable is no longer confounded by measured covariates. Wodtke et al. (2011) suggested weighting observations by the inverse of the probability that a child was exposed to their actual neighborhood quintile in each year conditional on past treatments and confounders. The IPT weight for the ith child is given by

$$w_i = \prod_{j=1}^{J} \frac{1}{P\left(T_{(j)} = t_{ij} \middle| \overline{T}_{(j-1)} = \bar{t}_{i(j-1)}, \overline{L}_j = \bar{l}_{ij}\right)}, \tag{6}$$

where $T_{(j)}$ represents neighborhood exposure status at the jth wave since start of follow-up, t_{ij} represents child i's neighborhood exposure at wave j, and $\bar{l}_{ij}$ represents child i's previous confounders since wave j. We constructed the weights using an ordered logistic regression model for each year that adjusts for confounding by time-varying covariates. We chose the covariates to align as closely as possible to those used by Wodtke et.al. (2011) so that we could compare our models, weights, and results with theirs. An example of a model used to construct weights for one of the six years is shown in Table S2 in Online Resource 1. By weighting regression models with these overall IPT weights, we balanced treatment assignments by giving more weight to children with underrepresented covariate histories.

(place Table 4 about here)

Applying this technique to data from L.A.FANS resulted in IPT weights that were hugely inflated, as shown in Table 4. More than one-quarter of the weights have values larger than 12, and the largest 5 % of weights have values well above 1,000. When these weights are applied to regression models for children's test scores, parameter estimates become wildly unstable, with most of the model's power being derived from a small number of observations with very large weights. Wodtke et al. (2011) found similar results when constructing IPT weights for their analysis and suggested stabilizing the weights by including baseline confounders in the numerator of the weighting equation:

$$sw_i = \prod_{j=1}^{J} \frac{P\left(T_{(j)} = t_{ij} \middle| \overline{T}_{(j-1)} = \bar{t}_{(j-1)i}, \overline{L}_0 = \bar{l}_0\right)}{P\left(T_{(j)} = t_{ij} \middle| \overline{T}_{(j-1)} = \bar{t}_{(j-1)i}, \overline{L}_i = \bar{l}_{ij}\right)}. \tag{7}$$

Because stabilized weights now contain baseline confounders in both the numerator and the denominator, the outcome regression model must also condition on these confounders. We implemented the stabilized IPT weights using L.A.FANS data and, following Wodtke et al., we further refined the weighting procedure by multiplying the stabilized weights with panel weights provided by L.A.FANS to account for the survey's sampling design and attrition. Summary statistics for the component and final weights are shown in Table 4.

Results

We present two sets of results for the effects of neighborhood disadvantage on children's math and reading scores in Los Angeles using data from Waves 1 and 2 of L.A.FANS. The first set of results is based on our new propensity function modeling approach, and the second set uses IPTW. The IPTW approach has been used in other recent studies and hence provides a useful comparison for the results of our novel approach.

(place Table 5 about here)

In Table 5 and Fig. 2, we present results from our propensity function models for children's math and reading scores. The table includes the full set of parameter estimates for all covariates in four different models: two for math scores and two for reading scores. The first model for each outcome examines the effects of mean exposure to neighborhood disadvantage, and the second model shows the effects of recency of exposure to neighborhood disadvantage. We do not present results for models that include the simultaneous effects of overall mean treatment and recency scores, although such a model could be estimated in principle. However, this model is not statistically identifiable because of the high correlations between the expected overall mean treatment and expected recency scores (r = .96).

For all four models, the effects of neighborhood disadvantage were estimated as an interaction with *expected* average exposure and recency, and all three variables (i.e., expected average exposure, expected recency, and either observed recency of exposure or observed mean exposure) were specified as flexible, smoothed tensor-spline functions. The bottom panel of Table 5 provides information about the smoothed functions—namely, the number of effective degrees of freedom, reference degrees of freedom, and an F test statistic for these functions. For all four models, the two sets of tensor spline functions have F test values indicating statistically significant contributions to model fit. The effective and reference degrees of freedom measures provide indicators of model complexity, although the large values for the reference degrees of freedom are not an indicator of overfitting (see Janson et al. 2015); note also that the noninteger values are due to the partial penalization.

In Fig. 2, we present the substantive effects on math and reading test scores of average exposure to neighborhood disadvantage and the recency of exposure to neighborhood disadvantage. In each panel, the solid line represents the conditional expectation of each test score, and the dashed lines show the 95 % pointwise confidence bounds for the expected test score. The figure displays the estimated dose-response functions, which are interpreted as the average treatment effect given the level of treatment received. Panel a shows no effect of average exposure to neighborhood disadvantage on children's math scores, and panel c reveals a suggestive but nonsignificant negative effect of average exposure to neighborhood disadvantage on children's reading scores.

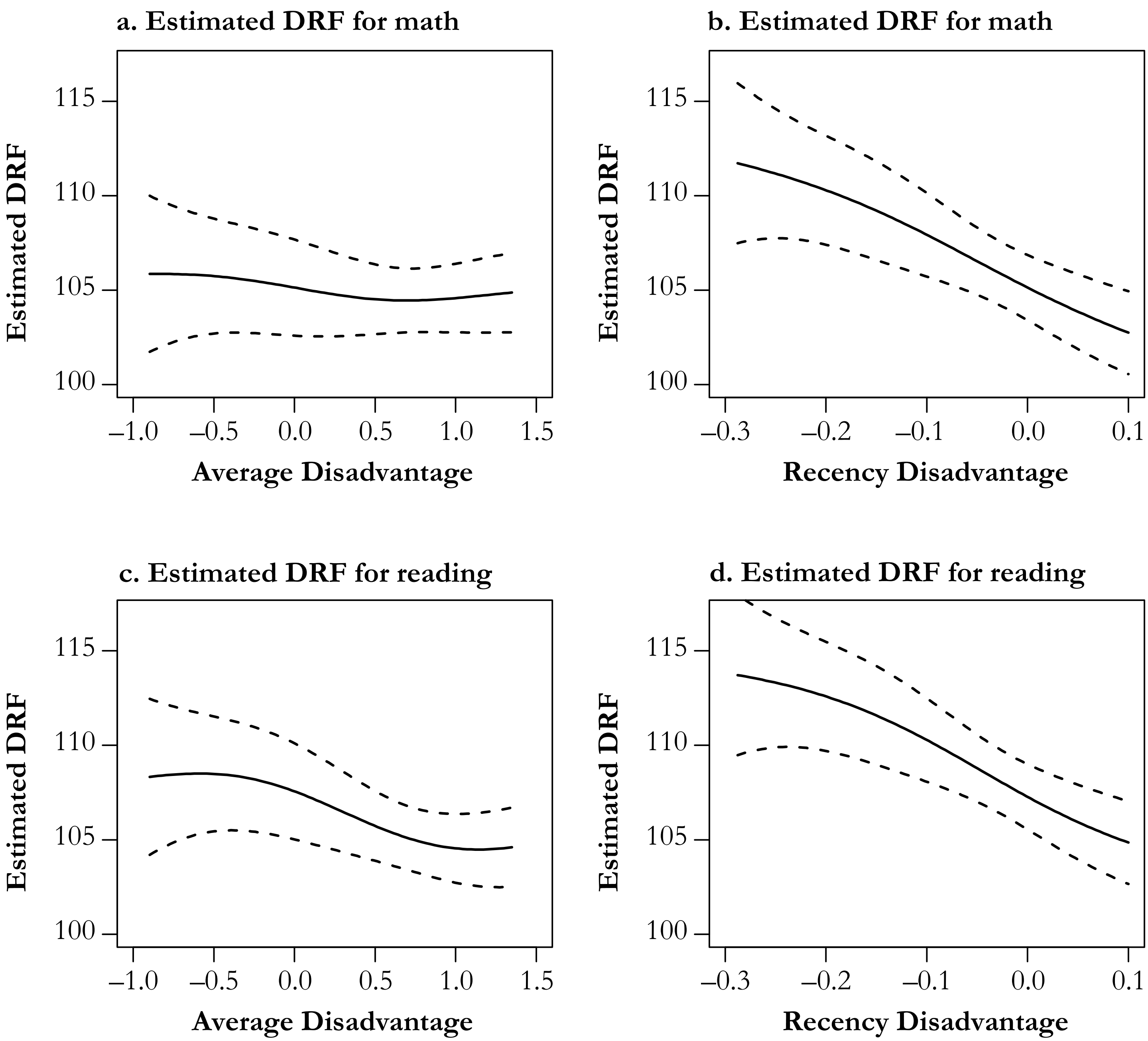

Fig. 2 Estimated effects of average expected neighborhood disadvantage and average expected recency of exposure to neighborhood disadvantage on reading and math scores. DRF = dose-response function

Panels b and d of Fig. 2 show significant negative effects for recency of exposure to neighborhood disadvantage on children's scores for both math (panel b) and reading (panel d). Although the effects of recency of exposure to neighborhood disadvantage on both outcome variables are estimated using flexible nonlinear specifications, the relationships appear nearly linear. The slopes of the effects of recency of exposure are similar for reading and for math test scores, indicating that a 1 standard deviation (0.094) increase in recency of exposure to neighborhood disadvantage is associated with a decrease of 2.6 points in math scores and 2.7 in reading scores—in both cases, approximately one-fifth of a standard deviation. Thus, we find no effects of average neighborhood disadvantage on children's math or reading test scores but a significant negative effect of recency of exposure to neighborhood disadvantage on both math and reading test scores. Note that the absence of a statistically significant effect of average neighborhood disadvantage is due to the absence of an effect (i.e., a nearly flat slope for the estimated relationship), particularly for math, rather than due to an apparent relationship but a lack of statistical precision in estimating this effect. Our results suggest that exposure to a high level of neighborhood disadvantage is a modest predictor of lower math and reading test scores—unless this exposure occurs in the recent past. These results are perhaps not surprising given that the expected average treatment adjusts for neighborhood exposure at Wave 1, and they are highly correlated (r = .995). As a result, the causal effect of exposure to neighborhood disadvantage is difficult to identify statistically, which also explains the large confidence regions.

Table 5 shows regression-adjusted estimates for the effects on children's math and reading test scores for the remaining variables in the models (other than mean exposure to neighborhood disadvantage and recency of exposure). We focus our discussion on results for which there is a consistent pattern across models. The results suggest that older children perform worse on both standardized tests, based on the significant and negative coefficients for the child's age. This result could reflect period effects or stronger negative effects of exposure to disadvantaged during later adolescence. Boys have higher test scores than girls for math but lower scores for reading, although these differences are statistically significant only for those models that examine the effects of recency. Children whose race is white or other have significantly higher reading scores compared with Latinos or blacks, although the result is statistically significant only in the models examining the effects of recency. Children with older mothers have significantly higher reading and math scores, although again the result is statistically significant only for the models examining the effects of recency. Mothers' reading test scores have a statistically significant positive association with test scores for both outcomes. However, the effects of mothers' reading scores are twice as large for children's reading scores as for math scores, presumably because of the correspondence between the test topic and the former outcome. The family head's level of educational attainment is also associated with significantly higher reading and math test scores for children.

(place Table 6 about here)

Next we turn to results from the models that use the IPTW approach, which are presented in Table 6. To compare these models with the preceding results and the findings from Wodtke et al. (2011), we focus initially on a pair of model specifications for each outcome that consider separately the effects of observed duration-weighted exposure and observed recency. We find negative effects of observed duration-weighted exposure on both math and reading test scores for children, although the effect on reading scores is statistically significant only at the .10 level. For math scores, these results indicate that a 1 standard deviation increase in observed duration-weighted exposure (1.262) is associated with a decrease of 4.36 points, or approximately one-third of a standard deviation. For reading scores alone, we find a negative effect of observed recency with a 1 standard deviation increase in (0.143) associated with a decrease of 3.09 points, or approximately one-fifth of a standard deviation.

The results for other covariates from the IPTW models are broadly similar to those based on the propensity function approach. Older children have lower test scores, with the estimated effects again twice as large for math compared with reading. Boys have substantially lower test scores than girls for reading but have similar scores for math. Several variables have effects on scores only for models that examine the effects of observed recency, including mother's age at the child's birth, mother's reading test score, and family head's educational attainment. Recall that we control for these variables because they are included in the calculation of the stabilized weights rather than as a focus for our analysis.

(place Table 7 about here)

Finally, we conducted a sensitivity analysis of our results for the IPTW models by investigating the joint effects of observed duration-weighted exposure and observed recency and examined nonlinear effects by estimating models stratified by neighborhood income. These results are presented in Table 7. This analysis was motivated by the possibility that the effects are qualitatively different by poverty status of the neighborhoods. If so, we may be able to identify them even with the reduced sample sizes and variation within strata. We stratified the models based on the same stratification design that was used for L.A.FANS sampling plan, which divided tracts in Los Angeles County into three strata classified as very poor, poor, and nonpoor (Sastry et al. 2006). Column 1 in Table 7 shows results from the full sample. The results for Models 1 and 2 replicate the findings from Table 6, and the results for Model 3 show the joint effects of observed duration-weighted exposure and observed recency. The results for Model 3 reveal that the effects of observed duration-weighted exposure and observed recency do not change when we also control for the other variable; rather, each variable appears to have an effect on children's reading and math scores independent of the other variable.

When estimating the models separately by sampling strata, we find the effects of neighborhood disadvantage exposure to vary qualitatively by poverty stratum (Table 7, columns 2–4). For both outcomes (math and reading), and across all three model specifications (observed duration-weighted exposure alone, observed recency alone, and both variables jointly), substantively large and statistically significant effects generally emerge only in models for the nonpoor stratum alone (column 4). The models of math scores show no statistically significant effects in models for the very poor and poor strata, and even the magnitudes of the (nonsignificant) estimated parameters are small. However, for the nonpoor stratum, we find statistically significant negative effects of both observed duration-weighted exposure and observed recency, whether estimated separately or jointly. For the models of reading scores, the estimated parameters across the three strata are generally similar, although the precision of the estimates varies greatly. We find statistically significant negative effects of observed duration-weighted exposure but only in the nonpoor stratum, and we find significant negative effects of observed recency but only in the poor stratum.

Comparing the results from the two modeling approaches, the IPTW models show significant negative effects of observed duration-weighted exposure to neighborhood disadvantage on children's math scores and marginally significant negative effects on reading scores. Results from the propensity function approach also suggest a small albeit nonsignificant, negative effect of average exposure on reading scores but no similar effect on math scores. We find a consistent negative linear effect of recency of exposure to neighborhood disadvantage on children's reading scores. Evidence of the effect of recency on math scores is less clear: the stratified IPTW models suggest a nonlinear relationship, with a strong negative effect only in the nonpoor stratum, whereas the propensity function approach suggests a negative linear effect that operates across all three strata. However, results from the stratified IPTW models were imprecisely estimated in the poor and very poor strata, and the apparent inconsistency in the findings across the two modeling approaches may be due to small sample sizes in the two smaller strata (the very poor and poor strata). We suspect that effect sizes in our underlying propensity models used to generate the IPT weights may also vary by stratum and conclude that these models must also be stratified into three separate models based on tract poverty status. However, because of small sample sizes, separate propensity models by poverty status were not identifiable under these conditions. In these circumstances, the propensity function technique approach provides an integrated approach that is attractive because it allows us to model the effects of neighborhood exposure and recency as continuous functions and eliminates the need to arbitrarily split the neighborhood disadvantage score into quintiles to fit an ordered logistic propensity function.

Discussion

A challenging problem in research on the effects of neighborhood disadvantage on children's development is how to estimate the causal effects of neighborhood exposure in the context of families being able to choose where they live based on the characteristics of these neighborhoods and their children's outcomes. In this article, we present a novel approach to estimating these effects while taking endogeneity into account. We compared our approach with a similar model recently proposed by Wodtke et al. (2011) that uses a marginal structural modeling technique with IPTW. The Wodtke et al. (2011) reweighting strategy aims to rebalance individuals in the sample across treatment categories so that treatment is not confounded by observed covariates. We identified several significant shortcomings of this strategy: (1) the creation of arbitrary treatment categories, rather than a continuous treatment, which would reflect the more realistic nature of exposure to neighborhood disadvantage; (2) the difficulty of examining nonlinear effects, which is compounded by the difficulty in estimating the underlying weights using an approach that is consistent with the nonlinear effects; and (3) the difficulty of estimating the effects of a treatment that includes more than one dimension.

Our approach addresses each of these three shortcomings. Our model is based on the propensity function approach developed by Imai and van Dyk (2004) that obviates the need to construct arbitrary categories from a continuous distribution. We also adopted a generalized additive modeling approach (Hastie and Tibshirani 1990) to allow for the estimation of flexible, nonlinear effects of treatment. Finally, the specification of a multidimensional interactive effect between the propensity function and the treatment variables of interest allowed us to examine the effects of two (or more) continuous treatment variables, although in our application, sample size limitations prevented us from fully implementing this last feature.

With the growing availability of longitudinal and retrospective life history data, researchers are increasingly able to study the effects of cumulative experiences and statuses over the life course on classic demographic behavior such as fertility, marriage, mortality, health, and migration, as well as a wide range of other outcomes. As in the case of the effects of cumulative neighborhood disadvantage on children’s academic achievement, these studies face complex problems of endogeneity because each experience or status (like each neighborhood) is a choice based on past experiences. Approaches such as the one we present in this article, and others like it, are important tools to model these complex processes.

We used the statistical model developed in this article to estimate the effects of two aspects of neighborhood disadvantage on children's test scores: (1) average exposure during the previous six years to neighborhood disadvantage and (2) recency of exposure to disadvantage. Based on previous research, we expected that greater average exposure would be associated with poorer test scores. However, we had no *a priori* expectation about the direction of the association between recency of exposure and test scores.

Contrary to expectation, we found no relationship between average exposure to neighborhood disadvantage and math scores. In the case of reading scores, we found a negative association with average exposure, but the results were not statistically significant. The direction of the effect for reading test scores was consistent with the findings of Sampson et al. (2008) for African American children in Chicago and some previous studies of test scores (Burdick-Will et al. 2011), but because our results were not statistically significant, this result may have been due to chance. The reason for lack of significance may be our relatively small sample size, although Sampson et al.'s sample size of 772 children is not much larger than ours. Comparing our results with those of Wodtke et al. is more difficult because they looked at a very different outcome: high school completion. It is plausible that test scores for reading, which is so closely tied to other types of language use, are more vulnerable to neighborhood disadvantage if disadvantaged neighborhoods and families living in them emphasize speaking and reading to children less than those in more-advantaged neighborhoods.

We found that children who are more recently exposed to neighborhood disadvantage had significantly lower scores on reading and math tests compared with those who were exposed further in the past. Furthermore, the effects were nearly linear for both reading and math and were of similar magnitude, with a 1 standard deviation increase in recency associated with a 0.2

standard deviation decrease in test scores. These results are consistent with a review of a wide range of both experimental and observational studies by Burdick-Will et al. (2011) which concluded that moving to a different neighborhood is associated with improved children's test scores and that this association even affects children who have been long term residents of seriously disadvantaged neighborhoods

The situation here is subtly different than traditional settings with binary treatments. The causal effect of the bivariate and continuous treatment (i.e., recency and average exposure) is a function of both the reference level of the bivariate treatment and the comparison level of the bivariate treatment. To say that average exposure has no causal effect means that the causal effect is not a function of either the reference or comparison levels of average exposure. Because we did not have the information in the data to accurately estimate the causal effect of this quadvariate, we cannot conclude that either recency or average exposure does not have a causal effect. However, if we knew that average exposure was not a causal factor, Fig. 2 tells us that recency is. If recency is not a causal factor, then there is little evidence that average exposure is a causal factor. Our results suggest that recency may be a causal factor and may be more significant to children's test score than average exposure to neighborhood disadvantage. Perhaps recency and average exposure to disadvantage both have causal effects. However, there is little evidence that average exposure alone is a causal factor and recency is not. These findings imply a possible significant fadeout for the negative effects of early exposure to disadvantaged neighborhoods if children subsequently move to better neighborhoods—an optimistic result. However, there could also be a fadeout of positive benefits for children exposed to advantaged neighborhoods if they move to more disadvantaged neighborhoods subsequently. Additional research is needed to confirm and understand this result.

The results of our propensity function model for recency were very similar to those of the IPTW model for reading scores but not for math scores. For math scores, recency had a strong negative effect only in the nonpoor stratum. Results from the IPTW models suggest the presence of nonlinear effects, but these effects could not be estimated consistently because we were unable to estimate stratified models to generate the IPT weights.

Finally, two aspects of children's exposure to neighborhood disadvantage in L.A.FANS are worth highlighting. First, the average recency score was –0.05, which suggests that children in Los Angeles were exposed to less neighborhood disadvantage over time between 2000 and 2007. At the same time, child age at baseline was statistically significantly and negatively associated with test scores for both math and reading. Together, these two findings suggest the involvement of a family life cycle process in which families are able to move to better neighborhoods over time, and their younger children benefit most from better neighborhoods in terms of better achievement outcomes. Second, the distribution of recency was truncated near 0 and was at its lowest values when average expected treatment was near its minimum value. This result indicates that few families were moving to more-disadvantaged neighborhoods and that the few moves to disadvantaged neighborhoods that did occur were largely among families already exposed to such neighborhoods. This pattern of moves may again reflect life cycle effects in families' economic status and the selection processes involved in the endogeneity of residential choice as well as the general improvement over time in neighborhood characteristics.

A few limitations to this study can be identified. The sample was limited to children who resided in Los Angeles County and participated in both waves of L.A.FANS. As in any longitudinal survey, children who left Los Angeles after the first wave or who did not participate in the second wave for other reasons may have differed from those who stayed, and this difference could have altered the results compared with the ideal case in which all children in Wave 1 participated in Wave 2 (although there is no evidence to suggest this is the case). The L.A.FANS data also have relatively modest sample sizes, making estimation of certain relationships challenging. In particular, identifying the effect of average exposure to neighborhood disadvantage on children's test scores was difficult, although this situation was also a consequence of the high correlation with the overall mean expected treatment score from the propensity function. The high correlation between the overall mean treatment and recency scores also meant that we could not model the simultaneous effects of these two variables, and larger sample sizes may have ameliorated this problem. A separate limitation is that the propensity score techniques that we used consider only observed characteristics of children and families. Despite established approaches to examining the sensitivity of results to the effects of unobserved or unmeasured factors (e.g., Brumback et al. 2004), applying these techniques was beyond the scope of the current study. Finally, we used residential locations as indicators of exposure to concentrated disadvantage at the neighborhood level: a potential improvement for the future would be to use indicators of actual contextual exposures based on location of schools that children attend, activity patterns that reflect where children spend their time (Jones and Pebley 2014), and the locations of where peer group members, friends, and relatives reside. Few studies collect such data, although L.A.FANS is a notable exception.

Acknowledgements: This research began when all authors were affiliated with the California Center for Population Research (CCPR) at UCLA. We greatly appreciate support from CCPR under NICHD center grant P2C-HD041022 and from NICHD grant R25HD076814. It is also partially supported by the National Science Foundation (NSF, SES-1357619, IIS-1546259) and

NICHD grant, R21HD075714. We are also grateful for assistance from Christine Peterson at RAND and Rachel Veerman at UCLA.

References

Aaronson, D. (1998). Using sibling data to estimate the impact of neighborhoods on children's educational outcomes. *Journal of Human Resources, 33,* 915–946.

Alvarado, S. (2016). Delayed disadvantage: Neighborhood context and child development. *Social Forces, 94,* 1847–1877.

Aneshensel, C. S., & Sucoff, C. A. (1996). The neighborhood context of adolescent mental health. *Journal of Health and Social Behavior, 37,* 293–310.

Briggs, X. de Souza, & Keys, B. J. (2009). Has exposure to poor neighbourhoods changed in America? Race, risk and housing locations in two decades. *Urban Studies, 46,* 429–458.

Brumback, B. A., Hernán, M. A., Haneuse, S. J., & Robins, J. M. (2004). Sensitivity analyses for unmeasured confounding assuming a marginal structural model for repeated measures. *Statistics in Medicine, 23,* 749–767.

Burdick-Will, J., Ludwig, J., Raudenbush, S., Sampson, R. J., Sanbonmatsu, L., & Sharkey, P. (2011). Converging evidence for neighborhood effects on children's test scores: An experimental, quasi-experimental, and observational comparison. In G. J. Duncan & R. J. Murnane (Eds.), *Whither opportunity? Rising inequality, schools, and children's life chances* (pp. 255–276). New York, NY: Russell Sage Foundation.

Clampet-Lundquist, S., & Massey, D. S. (2008). Neighborhood effects on economic self-sufficiency: A reconsideration of the moving to opportunity experiment. *American Journal of Sociology, 114,* 107–143.

Coleman, J. S. (1988). Social capital in the creation of human capital. *American Journal of Sociology, 94,* S95–S120.

Crowder, K., & South, S. J. (2011). Spatial and temporal dimensions of neighborhood effects on high school graduation. *Social Science Research, 40,* 87–106.

Dietz, R. D. (2002). The estimation of neighborhood effects in the social sciences: An interdisciplinary approach. *Social Science Research, 31*, 539–575.

Duncan, G. J., & Murnane, R. J. (Eds.). (2011). *Whither opportunity? Rising inequality, schools, and children's life chances*. New York, NY: Russell Sage Foundation.

Duncan, G. J., & Raudenbush, S. W. (1999). Assessing the effects of context in studies of child and youth development. *Educational Psychologist, 34,* 29–41.

Dupere, V., Leventhal, T., Crosnoe, R., & Dion, E. (2010). Understanding the positive role of neighborhood socioeconomic advantage in achievement: The contribution of the home, child care, and school environments. *Developmental Psychology, 46,* 1227–1244.

Edin, K. (1991). Surviving the welfare system: How AFDC recipients make ends meet in Chicago. *Social Problems, 38,* 462–474.

Eiland, L., & Romeo, R. (2013). Stress and the developing adolescent brain. *Neuroscience, 249,* 162–171.

Evans, W. N., Oates, W. E., & Schwab, R. M. (1992). Measuring peer group effects: A study of teenage behavior. *Journal of Political Economy, 100,* 966–991.

Foster, E. M., & McLanahan, S. (1996). An illustration of the use of instrumental variables: Do neighborhood conditions affect a young person's chance of finishing high school? *Psychological Methods, 1,* 249–260.

Fox, S. E., Levitt, P., & Nelson III, C. A. (2010). How the timing and quality of early experiences influence the development of brain architecture. *Child Development, 81,* 28–40.

Gennetian, L. A., Sanbonmatsu, L., Katz, L. F., Kling, J. R., Sciandra, M., Ludwig, J., . . . Kessler, R. C. (2012). The long-term effects of moving to opportunity on youth outcomes. *Cityscape, 14,* 137–167.

Geronimus, A., Bound, J., & Ro, A. (2014). Residential mobility across local areas in the United States and the geographic distribution of the healthy population. *Demography, 51,* 777–809.

Gobillon, L., Selod, H., & Zenou, Y. (2007). The mechanisms of spatial mismatch. *Urban Studies, 44,* 2401–2427.

Harding, D. J. (2003). Counterfactual models of neighborhood effects: The effect of neighborhood poverty on dropping out and teenage pregnancy. *American Journal of Sociology, 109,* 676–719.

Harding, D. J., Gennetian, L., Winship, C., Sanbonmatsu, L., & Kling, J. R. (2011). Unpacking neighborhood influences on education outcomes: Setting the stage for future research. In G. J. Duncan & R. J. Murnane (Eds.), *Whither opportunity? Rising inequality, schools, and children's life chances* (pp. 277–298). New York, NY: Russell Sage Foundation.

Hastie, T. J., & Tibshirani, R. J. (1990). *Generalized additive models*. Boca Raton, FL: CRC Press.

Heckman, J. J. (2011). The economics of inequality: The value of early childhood education. *American Educator, 35*(1), 31–35.

Hernán, M. Á., Brumback, B., & Robins, J. M. (2000). Marginal structural models to estimate the causal effect of zidovudine on the survival of HIV-positive men. *Epidemiology, 11,* 561–570.

Imai, K., & van Dyk, D. A. (2004). Causal inference with general treatment regimes: Generalizing the propensity score. *Journal of the American Statistical Association, 99,* 854–866.

Jackson, M. I., & Mare, R. D. (2007). Cross-sectional and longitudinal measurements of neighborhood experience and their effects on children. *Social Science Research, 36,* 590–610.

Janson, L., Fithian, W., & Hastie, T. J. (2015). Effective degrees of freedom: A flawed metaphor. *Biometrika, 102,* 479–485.

Jargowsky, P. A., & El Komi, M. (2011). Before or after the bell? School context and neighborhood effects on student achievement. In H. B. Newburger, E. L. Birch, & S. M. Wachter (Eds.), *Neighborhood and life chances: How place matters in modern America* (pp. 50–72). Philadelphia: University of Pennsylvania Press.

Jones, M., & Pebley, A. R. (2014). Redefining neighborhoods using common destinations: Social characteristics of activity spaces and home census tracts compared. *Demography, 51*, 727–752.

Kunz, J., Page, M. E., & Solon, G. (2003). Are point-in-time measures of neighborhood characteristics useful proxies for children’s long-run neighborhood environment? *Economics Letters, 79*, 231–237.

Manski, C. F. (1995). *Identification problems in the social sciences*. Cambridge, MA: Harvard University Press.

Massey, D. S., & Denton, N. A. (1993). *American apartheid: Segregation and the making of the underclass*. Cambridge, MA: Harvard University Press.

McCoy, D. C., Raver, C. C., & Sharkey, P. (2015). Children's cognitive performance and selective attention following recent community violence. *Journal of Health and Social Behavior, 56,* 19–36.

McGrew, K. S., Werder, J. K., & Woodcock, R. W. (1991). *WJ-R technical manual: A reference on theory and current research*. Chicago, IL: Riverside.

Morrison Gutman, L., McLoyd, V. C., & Tokoyawa, T. (2005). Financial strain, neighborhood stress, parenting behaviors, and adolescent adjustment in urban African American families. *Journal of Research on Adolescence, 15,* 425–449.

Pebley, A. R., & Sastry, N. (2004). Neighborhoods, poverty and children's well-being. In K. Neckerman (Ed.), *Social dimensions of inequality* (pp. 119–146). New York, NY: Russell Sage Foundation.

Peterson, C. E., Pebley, A. R., Sastry, N., Yuhas, K., Ghosh-Dastidar, B., Hass, A. C., . . . Bitler, M. P. (2011). *The Los Angeles Family and Neighborhood Survey, Wave 2. User's guide and codebook* (Working paper WR-240/20-LAFANS). Retrieved from www.rand.org/content/dam/rand/pubs/working_papers/2012/RAND_WR240.20.pdf

Peterson, R. D., Krivo, L. J., & Harris, M. A. (2000). Disadvantage and neighborhood violent crime: Do local institutions matter? *Journal of Research in Crime and Delinquency, 37,* 31–63.

Pettit, B., & McLanahan, S. (2003). Residential mobility and children's social capital: Evidence from an experiment. *Social Science Quarterly, 84,* 632–649.

Phillips, M. (2011a). Ethnic and social class disparities in academic skills. In L. M. Stulberg & S. L. Weinberg (Eds.), *Diversity in American higher education: Toward a more comprehensive approach* (pp. 7–24). London, UK: Routledge.

Phillips, M. (2011b). Parenting, time use, and disparities in academic outcomes. In G. J. Duncan & R. J. Murnane (Eds.), *Whither opportunity? Rising inequality, schools, and children's life chances* (pp. 207–228). New York, NY: Russell Sage Foundation.

Plotnick, R. D., & Hoffman, S. D. (1999). The effect of neighborhood characteristics on young adult outcomes: Alternative estimates. *Social Science Quarterly, 80,* 1–18.

Quillian, L. (2003). How long are exposures to poor neighborhoods? The long-term dynamics of entry and exit from poor neighborhoods. *Population Research and Policy Review, 22,* 221–249.

Robins, J. M., Hernan, M. A., & Brumback, B. (2000). Marginal structural models and causal inference in epidemiology. *Epidemiology, 11,* 550–560.

Roksa, J., & Potter, D. (2011). Parenting and academic achievement: Intergenerational transmission of educational advantage. *Sociology of Education, 84,* 299–321.

Rosenbaum, P. R., & Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika, 70,* 41–55.

Sampson, R., Raudenbush, S., & Earls, F. (1997). Neighborhoods and violent crime: A multilevel study of collective efficacy. *Science, 277,* 918–924.

Sampson, R. J. (2008). Moving to inequality: Neighborhood effects and experiments meet social structure. *American Journal of Sociology, 114,* 189–231.

Sampson, R. J. (2011). Neighborhood effects, causal mechanisms, and the social structure of the city. In P. Demeulenaere (Ed.), *Analytical sociology and social mechanisms* (pp. 227–250). Cambridge, UK: Cambridge University Press.

Sampson, R. J. (2012). *Great American city: Chicago and the enduring neighborhood effect*. Chicago, IL: University of Chicago Press.

Sampson, R. J., Morenoff, J. D., & Felton, E. (1999). Beyond social capital: Spatial dynamics of collective efficacy for children. *American Sociological Review, 64,* 633–660.

Sampson, R. J., Sharkey, P., & Raudenbush, S. W. (2008). Durable effects of concentrated disadvantage on verbal ability among African-American children. *Proceedings of the National Academy of Sciences, 105,* 845–852.

Sanbonmatsu, L., Kling, J. R., Duncan, G. J., & Brooks-Gunn, J. (2006). Neighborhoods and academic achievement: Results from the moving to opportunity experiment. *Journal of Human Resources, 41,* 649–691.

Sastry, N. (2012). Neighborhood effects on children's achievement: A review of recent research. In V. Maholmes & R. B. King (Eds.), *Oxford handbook on child development and poverty*. New York, NY: Oxford University Press.

Sastry, N., Ghosh-Dastidar, B., Adams, J., & Pebley, A. R. (2006). The design of a multilevel survey of children, families, and communities: The Los Angeles Family and Neighborhood Survey. *Social Science Research, 35,* 1000–1024.

Sastry, N., & Pebley, A. R. (2003). *Non-response in the Los Angeles Family and Neighborhood Survey* (Labor and Population Working Paper No. DRU-2400/7-LAFANS). Retrieved from www.rand.org/content/dam/rand/pubs/drafts/2006/DRU2400.7.pdf

Sastry, N., & Pebley, A. R. (2010). Family and neighborhood sources of socioeconomic inequality in children's achievement. *Demography, 47,* 777–800.

Sharkey, P. (2010). The acute effect of local homicides on children's cognitive performance. *Proceedings of the National Academy of Sciences, 107,* 11733–11738.

Sharkey, P., & Faber, J. W. (2014). Where, when, why, and for whom do residential contexts matter? Moving away from the dichotomous understanding of neighborhood effects. *Annual Review of Sociology, 40,* 559–579.

Sharkey, P. T., Tirado-Strayer, N., Papachristos, A. V., & Raver, C. C. (2012). The effect of local violence on children's attention and impulse control. *American Journal of Public Health, 102,* 2287–2293.

Shaw, C. R., & McKay, H. D. (1942). *Juvenile delinquency and urban areas*. Chicago, IL: University of Chicago Press.

Shonkoff, J. P., Garner, A. S., Committee on the Psychosocial Aspects of Child and Family Health, Committee on Early Childhood, Adoption, and Dependent Care, & Section on Developmental and Behavioral Pediatrics. (2012). The lifelong effects of early childhood adversity and toxic stress. *Pediatrics, 129*(1), e232–e246. doi:10.1542/peds.2011-2663

Small, M. L., Jacobs, E. M., & Massengill, R. P. (2008). Why organizational ties matter for neighborhood effects: Resource access through childcare centers. *Social Forces, 87,* 387–414.

Solon, G., Page, M. E., & Duncan, G. J. (2000). Correlations between neighboring children in their subsequent educational attainment. *Review of Economics and Statistics, 82,* 383–392.

Stoll, M. A., & Covington, K. (2012). Explaining racial/ethnic gaps in spatial mismatch in the US: The primacy of racial segregation. *Urban Studies, 49,* 2501–2521.

Tigges, L. M., Browne, I., & Green, G. P. (1998). Social isolation of the urban poor. *Sociological Quarterly, 39*, 53–77.

Timberlake, J. M. (2009). Effects of household and neighborhood characteristics on children's exposure to neighborhood poverty and affluence. *Social Science Research, 38,* 458–476.

Walker, S. P., Wachs, T. D., Grantham-McGregor, S., Black, M. M., Nelson, C. A., Huffman, S. L., . . . Richter, L. (2011). Inequality in early childhood: Risk and protective factors for early child development. *Lancet, 378,* 1325–1338.

Wheaton, B., & Clarke, P. (2003). Space meets time: Integrating temporal and contextual influences on mental health in early adulthood. *American Sociological Review, 68,* 680–706.

Wilson, W. J. (1987). *The truly disadvantaged*. Chicago, IL: University of Chicago Press.

Wo, J. C. (2016). Community context of crime: A longitudinal examination of the effects of local institutions on neighborhood crime. *Crime & Delinquency, 62,* 1286–1312.

Wodtke, G. T. (2013). Duration and timing of exposure to neighborhood poverty and the risk of adolescent parenthood. *Demography, 50*, 1765–1788.

Wodtke, G. T., Harding, D. J., & Elwert, F. (2011). Neighborhood effects in temporal perspective. *American Sociological Review, 76*, 713–736.

Table 1 Neighborhood characteristics by quintile of concentrated disadvantage score for Los Angeles County census tracts, 2000 to 2008

	1st Quintile		2nd Quintile		3rd Quintile		4th Quintile		5th Quintile	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Percentage Female-Headed Households	2.46	1.70	5.17	2.01	7.76	3.08	11.31	3.86	16.29	6.14
Percentage Families With Income <$25K	5.68	4.30	11.80	4.80	18.89	6.12	29.46	7.35	46.53	11.36
Percentage Population Nonwhite Race	18.57	12.26	38.10	17.52	51.57	15.73	59.80	15.24	69.04	12.53
Percentage Population in Poverty	5.59	3.56	7.77	3.62	12.65	4.77	20.78	7.26	34.00	9.39
Percentage Population Age <18 Years	15.77	8.66	21.74	5.41	24.71	5.67	28.92	5.90	33.68	6.57
Percentage Households Receiving Welfare	0.96	0.87	2.02	1.15	4.02	1.79	7.12	2.79	12.94	5.53
Concentrated Disadvantage Score	–1.12	0.26	–0.66	0.11	–0.19	0.16	0.47	0.23	1.47	0.55

Table 2 Means (with standard deviations in parentheses) for L.A.FANS children in analysis sample

Variable	Unweighted (1)	Weighted (2)
Expected Average Exposure	0.480	0.272
	(0.974)	(0.859)
Expected Recency	–0.048	–0.039
	(0.086)	(0.094)
Observed Duration-Weighted Exposure	3.684	3.498
	(1.338)	(1.262)
Observed Recency	–0.039	–0.040
	(0.130)	(0.143)
Child's Age at Baseline (years)	7.255	7.229
	(2.522)	(2.514)
Child Sex Is Male (ref. = female)	0.513	0.517
	(0.500)	(0.500)
Child's Race: Latino	0.643	0.531
	(0.480)	(0.499)
Child's Race: Black	0.068	0.104
	(0.252)	(0.306)
Child's Race: White or Other	0.289	0.365
	(0.454)	(0.482)
Child Low Birth Weight (ref. = normal birth weight)	0.063	0.093
	(0.244)	(0.291)
Mother's Age at Child's Birth	28.159	28.229
	(6.500)	(6.019)
Mother Was Married at Child's Birth (ref. = not married)	0.588	0.600
	(0.493)	(0.49)
Mother's Reading Achievement Score	84.633	85.021
	(19.011)	(18.947)
Family Head Is High School Graduate (ref. = not high school graduate)	0.614	0.673
	(0.487)	(0.469)
Family Owns Home (ref. = does not own)	0.385	0.425
	(0.487)	(0.495)
Baseline: Mother Was Married (ref. = not married)	0.700	0.710
	(0.459)	(0.454)
Baseline: Mother's Number of Children	1.740	1.854
	(1.001)	(1.032)
Baseline: Mother Was Employed (ref. = not employed)	0.667	0.675
	(0.472)	(0.469)
Baseline: Mother's Hours of Work	24.154	25.029
	(18.923)	(19.076)
Baseline: Receiving Public Assistance (ref. = no receipt)	0.054	0.049
	(0.225)	(0.217)
Baseline: Family Income ($)	17,102	17,890
	(29.666)	(32.656)

Table 3 Component weights and variances from principal component (PC) analysis of neighborhood disadvantage scores between 2001 and 2006

Year of Disadvantage Score	1st PC Weight	2nd PC Weight
2001	0.40	–0.51
2002	0.41	–0.39
2003	0.41	–0.14
2004	0.41	0.01
2005	0.41	0.39
2006	0.40	0.65
Proportion of Total Variance Explained	.95	.03

Table 4. IPT Weights, stabilized IPT weights, attrition weights, stabilized IPT-attrition weights

			Percentiles			
Variable	Mean	SD	1st	25th	75th	99th
IPT Weight	1.75e+08	4.52e+09	1.10	1.58	12.14	467,295.9
Stabilized IPT Weight	1.04	1.16	0.13	0.85	1.04	5.26
Attrition Weights	0.88	0.80	0.07	0.38	1.11	4.10
Stabilized IPT Weight × Attrition Weight	0.97	2.21	0.20	0.32	1.02	5.38

Table 5 Regression model results for effects of neighborhood disadvantage mean exposure and recency of exposure on reading and math scores

	Math		Reading	
Variable	Exposure	Recency	Exposure	Recency
Child's Age at Baseline	–1.419***	–1.382***	–0.797**	–0.798***
	(0.278)	(0.142)	(0.307)	(0.151)
Child Is Male (ref. = female)	1.333	1.628*	–1.877	–1.593*
	(1.419)	(0.720)	(1.586)	(0.794)
Child's Race: Black (ref. = Latino)	–0.972	–0.722	0.117	0.058
	(2.236)	(1.173)	(2.482)	(1.220)
Child's Race: White or Other (ref. = Latino)	–1.188	–0.743	5.310	5.350**
	(3.313)	(1.708)	(3.663)	(1.818)
Child Low Birth Weight (ref. = normal birth weight)	0.719	0.457	2.757	2.623
	(2.897)	(1.446)	(3.230)	(1.560)
Mother's Age at Child's Birth	0.152	0.148*	0.274	0.273***
	(0.126)	(0.070)	(0.140)	(0.072)
Mother Married at Child's Birth (ref. = not married)	–0.615	–0.258	0.385	0.467
	(2.166)	(1.409)	(2.226)	(1.485)
Mother's Reading Achievement Score	0.099*	0.099***	0.185**	0.184***
	(0.050)	(0.027)	(0.056)	(0.030)
Family Head is High School Graduate (ref. = not high school graduate)	3.171†	3.320***	8.142***	8.106***
	(1.858)	(0.961)	(2.072)	(1.016)
Family Owns Home (ref. = does not own)	0.182	–0.204	–1.602	–1.710
	(1.594)	(0.815)	(1.781)	(0.902)
Baseline: Mother Was Married (ref. = not married)	2.467	2.213*	–0.064	–0.294
	(1.989)	(1.079)	(2.168)	(1.161)
Baseline: Mother's Number of Children	–0.186	0.010	0.578	0.702
	(0.835)	(0.451)	(0.919)	(0.460)
Baseline: Mother Was Employed (ref. = not employed)	0.232	0.221	–0.072	0.141
	(1.532)	(0.928)	(1.642)	(0.896)
Baseline: Mother's Hours of Work	0.060	0.054	–0.014	–0.034
	(0.055)	(0.031)	(0.059)	(0.030)
Baseline: Receiving Public Assistance (ref. = no receipt)	2.744	1.988	0.315	–0.059
	(4.166)	(2.458)	(4.486)	(2.225)
Baseline: Family Income (log $)	–0.078	–0.080	0.037	–0.016
	(0.263)	(0.215)	(0.296)	(0.235)
Constant	97.045***	96.167***	86.146***	86.643***
	(7.114)	(4.268)	(8.008)	(4.380)
Smoothed Expected Average Exposure and Recency				
Effective degrees of freedom	6.397	6.277	6.058	5.897
Reference degrees of freedom	7.246	7.105	6.870	6.712
F test	3.578***	9.293***	2.526*	7.060***
Smoothed Effects of Exposure and of Expected Average Exposure and Recency				
Effective degrees of freedom	2.660		3.324	
Reference degrees of freedom	96.000		97.000	
F test	188.382***		106.533***	
Smoothed Effects of Recency and of Expected Average Exposure and Recency				
Effective degrees of freedom		3.419		3.545
Reference degrees of freedom		97.000		97.000
F test		751.359***		1.444***
Number of Observations	611	611	616	616
R^2	.190	.195	.172	.177

Note: Standard errors are shown in parentheses.
†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 6 IPTW regression model results for effects of duration-weighted exposure and recency of exposure to neighborhood disadvantage on math and reading scores

	Math		Reading	
Variable	Exposure	Recency	Exposure	Recency
Observed Duration-Weighted Exposure	–3.456**	—	–2.486†	—
	(1.019)		(1.315)	
Observed Recency	—	–11.463	—	–21.592**
		(7.879)		(7.344)
Child's Age at Baseline	–1.647***	–1.635***	–0.732†	–0.732†
	(0.372)	(0.393)	(0.384)	(0.381)
Child Is Male (ref. = female)	–0.212	–0.047	–4.744*	–4.458*
	(1.708)	(1.733)	(1.924)	(1.919)
Child's Race: Black (ref. = Latino)	3.274	2.163	9.014	8.371
	(3.582)	(4.121)	(5.332)	(5.584)
Child's Race: White or Other (ref. = Latino)	3.205	0.291	2.550	0.050
	(2.997)	(2.943)	(3.146)	(3.153)
Child Low Birth Weight (ref. = normal birth weight)	1.196	1.406	–1.099	–1.104
	(3.198)	(3.736)	(3.226)	(3.092)
Mother's Age at Child's Birth	0.223	0.367*	0.375†	0.497*
	(0.155)	(0.159)	(0.193)	(0.192)
Mother Married at Child's Birth (ref. = not married)	1.233	1.094	0.936	0.819
	(2.773)	(2.951)	(2.965)	(3.115)
Mother's Reading Achievement Score	0.113	0.142*	0.135†	0.154*
	(0.060)	(0.057)	(0.070)	(0.067)
Family Head Is High School Graduate (ref. = not high school graduate)	4.546	6.272**	7.018**	8.463**
	(2.205)	(2.211)	(2.636)	(2.460)
Family Owns Home (ref. = does not own)	–1.810	–0.482	–2.975	–1.467
	(2.081)	(2.038)	(2.222)	(2.187)
Baseline: Mother Was Married (ref. = not married)	2.907	2.949	–0.533	–0.434
	(2.961)	(3.071)	(3.124)	(3.235)
Baseline: Mother's Number of Children	0.142	0.411	–0.528	–0.339
	(0.912)	(0.963)	(1.122)	(1.142)
Baseline: Mother Was Employed (ref. = not employed)	0.924	0.238	0.335	–0.169
	(2.511)	(2.516)	(3.366)	(3.309)
Baseline: Mother's Hours of Work	–0.033	–0.011	–0.066	–0.044
	(0.074)	(0.078)	(0.091)	(0.092)
Baseline: Receiving Public Assistance (ref. = no receipt)	3.640	1.129	–1.425	–2.485
	(3.404)	(3.606)	(3.596)	(4.242)
Baseline: Family Income (log $)	0.318	0.4331	–0.113	–0.031
	(0.283)	(0.258)	(0.305)	(0.278)
Constant	102.318***	81.795***	97.892***	82.080***
	(7.717)	(8.736)	(13.023)	(9.386)
Number of Observations	611	611	616	616
F Test	6.19***	4.90***	6.10***	5.55***

Note: Standard errors are shown in parentheses.
$^{\dagger}p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 7 IPTW regression model results for effects of duration-weighted exposure and recency of exposure to neighborhood disadvantage on math and reading test scores for the full sample (column 1) and stratified by poverty status (columns 2–4)

Variable	Full Sample (1)	Very Poor (2)	Poor (3)	Not Poor (4)
Math Scores				
Model 1				
Observed duration-weighted exposure	–3.456***	0.678	–0.717	–4.767**
	(1.019)	(4.229)	(1.447)	(1.666)
Model 2				
Observed recency	–11.463	2.057	–7.941	–45.976**
	(7.879)	(11.481)	(8.927)	(16.856)
Model 3				
Observed duration-weighted exposure	–3.466**	0.326	–0.542	–4.789**
	(1.007)	(4.799)	(1.442)	(1.655)
Observed recency	–11.685†	1.410	–7.653	–46.186**
	(7.152)	(12.574)	(8.943)	(16.013)
Number of observations	611	208	193	210
Reading Scores				
Model 1				
Observed duration-weighted exposure	–2.486†	–6.197	–1.963	–5.718**
	(1.315)	(7.648)	(1.715)	(1.950)
Model 2				
Observed recency	–21.592**	–28.552	-22.326*	–26.973
	(7.344)	(20.846)	(8.861)	(19.257)
Model 3				
Observed duration-weighted exposure	–2.511†	2.355	–1.516	–5.733**
	(1.315)	(7.838)	(1.740)	(2.015)
Observed recency	–21.792**	–33.493	–21.577†	–27.338
	(7.051)	(20.566)	(8.882)	(17.683)
Number of observations	616	209	197	210

Notes: All models also control for child's age at baseline, child's sex, child's race, child's low birth weight, mother's age at child's birth, mother's marital status at child's birth, mother's reading achievement score, family head's education, family homeownership, baseline mother's marital status, mother's number of children, mother's employment, mother's hours of work, family receipt of public assistance, and family income. Standard errors are shown in parentheses.

†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$