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The Significance of Duration Weighted Neighborhood Effects for Violent Behavior and Explanation of Ethnoracial Differences

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Abstract

Purpose Two important issues constrain the neighborhood effects literature. First, most prior research examining neighborhood effects on aggression and self-reported violence uses a point in time (i.e., cross-sectional) estimate of neighborhood disadvantage even though the duration of exposure to neighborhood disadvantage varies between families. Second, neighborhood effects may be understated due to over-controlling for family socioeconomic conditions. Both limitations suggest that prior research may be underestimating neighborhood effects, which impacts research on the invariance thesis and explanation of ethnoracial differences.

Methods The sample is drawn from the restricted use Future of Families and Child Wellbeing study. Data to measure youth's exposure to neighborhood disadvantage is drawn from birth through age 9, with dependent variables measured at age 15. We estimate marginal structural models (MSM) with inverse probability of treatment weights (IPTW.

Results The results support hypotheses, indicating that the duration weighted measure of neighborhood disadvantage is more strongly associated with aggression and self-reported violence than the point in time, and that it accounts for a larger share of the ethnoracial differences.

Conclusions The findings provide a clear image of the consequences of long-term exposure to neighborhood disadvantage for aggression and violence. They suggest that criminologists addressing neighborhood effects should attempt, when feasible, to document and model the duration of exposure to neighborhood disadvantage. They are also consistent with and add to a growing literature addressing MSM modeling with IPTW weights.

Keywords Aggression · Self-reported violence · Neighborhood disadvantage · Neighborhood effect · Social disorganization · Ethnoracial differences · Marginal structural modeling with IPTW · Duration weighted neighborhood disadvantage

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Introduction

The literature documenting neighborhood effects on aggression and self-reported violence is constrained by at least two issues. First, prior studies likely understate the magnitude of neighborhood effects because they employ a point in time estimate of neighborhood disadvantage. As such, those studies do not capture cumulative, longer-term consequences of exposure to neighborhood disadvantage. Analytically, this means that adolescents experiencing short-term exposure to neighborhood disadvantage are treated as conceptually equivalent to those who have been exposed across a larger segment of their lives, particularly during childhood. Previous research finds that neighborhood effects on health, sex risk, educational, and life course outcomes are more pronounced when neighborhood effects are weighted to reflect the duration of the exposure (Kravitz-Wirtz 2016a, b; Wodtke 2013; Wodtke et al. 2011). However, prior research has not addressed criminological outcomes such as self-reported violence or focused more specifically on aggression. Second, neighborhood effects research has characteristically controlled for family socioeconomic characteristics, especially family structure and income, for instance (see Baumer and South 2001; Brewster 1994; Browning et al. 2004; Cleveland and Gilson 2004; Kim 2010; Santelli et al. 2000). Wodtke et al. (2011) illustrate that these conventional regression models overcontrol for family SES and introduce collider–stratification bias (Elwert and Winship 2014; Greenland 2003). Wodtke et al. (2011) advocate utilizing marginal structural modeling (MSM) with inverse probability of treatment weights (IPTW), which corrects for the tendency to underestimate neighborhood effects.

Utilizing MSM with IPTW is also important when addressing ethnoracial differences in violent behavior. Shaw and McKay (1942) were among the first to document racial and ethnic differences in delinquency and to argue that they are a function of disadvantaged neighborhood conditions. Sampson and Wilson (1995) extend this logic and propose the racial invariance thesis, which hypothesizes that racial/ethnic disparities in violence reflect relative group-specific exposure to neighborhood structures, particularly concentrated disadvantage. Research addressing ethnoracial differences is built up on "point in time" (i.e., cross-sectional) measures of concentrated disadvantage, even though this approach makes the dubious assumption that each group's length of exposure to disadvantage is roughly equivalent. This is a critical limitation. As Sharkey and Elwert (2011) document, many African American youth reside in disadvantaged neighborhoods their entire life. Sometimes this pattern is inter-generational within families, and both are likely true to an extent for other groups of color including multiracial and Hispanic families. This suggests that the developing literature on duration weighted modeling may have substantial relevance for understanding ethnoracial differences in aggression and violence.

In this paper, we contribute to the literature on violence by utilizing MSM and IPTW methods to inform a comparison between duration weighted and point in time estimates of concentrated neighborhood disadvantage. Drawing data from the Future of Families and Child Well-being study, which provides a national sample of adolescents who have been followed since birth, we test the hypothesis that duration weighted disadvantage exerts a stronger effect on primary caregiver's reports of aggressive behavior and youth's self-reported violence than the point in time estimate, and that it explains a greater percent of ethnoracial differences in those outcomes. We find support for both hypotheses. The findings carve a clearer image of the consequences of longer-term exposure to neighborhood disadvantage for aggressive and violent behaviors, and the explanation of ethnoracial differences. In the next sections, we review foundational literature examining

neighborhood, duration weighted measurement, ethnoracial differences, and provide additional background on MSMs and IPTW.

Neighborhood Effects and Ethnoracial Disparities

The literature documenting neighborhood effects emerged in earnest in the mid-1980's (see Simcha-Fagan and Schwartz 1986) and early 1990's, and "has become something of a cottage industry in the social sciences" (Sampson et al. 2002: p 444). Although important methodological issues remain, researchers have focused inquiry on whether neighborhoods matter, rather than on the question of how they matter (Sharkey 2014). In particular, researchers have in general neglected the conceptual difference between episodic and long-term residence in disadvantaged neighborhoods (Sharkey and Elwert 2011).

The majority of research on ethnoracial disparities in violent behavior is framed within the social disorganization tradition. For instance, Shaw and McKay (1942) argue that high rates of delinquency among ethnoracial groups relative to Whites is a product of segregation and economic inequality (Sampson and Wilson 1995; Sampson et al. 2018; Wilson 1987, 1996, 2009), and is reflected by racialized economic disadvantage (Krivo et al. 2009; Peterson and Krivo 2010). As Graham (2018: 450–1) eloquently summarizes with respect to Black and White youth:

> "The typical Black adolescent in America lives in a very different type of neighborhood, attends a very different type of school, and is embedded in a very different type of social network, than her White counterpart. Almost a half century after the civil rights era, these differences remain poignant ... Minority children tend to grow up in different social environments than their White counterparts. This observation, while self-evident, is nevertheless important and, perhaps, too infrequently made."

In addition, cultural adaptation and legal cynicism reinforce the use of aggression and violence as a means of conflict resolution in impoverished neighborhoods, which further places youth at risk (Anderson 1999; Heimer 1997; Stewart et al. 2006; Stewart and Simons 2006).

Our hypothesis is that the differences in social worlds experienced by White, Black, Multiracial, and Latinx youth are directly relevant for understanding ethnoracial differences in aggression and violence. Official (UCR) and self-reported data point to race differences, but the magnitude varies by source. Compared with non-Hispanic White adolescents, it is well documented that Black youth are over-represented in arrests for serious index crimes, particularly violent crimes (Hawkins et al. 2000), while Asian youth are under-represented. UCR data do not include the ethnicity of arrestees, thus they are less useful for investigating Hispanic involvement. Ethnoracial differences are less pronounced in self-report surveys for a variety of reasons, but Black-White difference is nevertheless evident, particularly in violence and more serious offenses (Morenoff 2005). Self-report studies also reveal that Hispanics are over-represented in violence relative to Whites (McNulty and Bellair 2003b). Adolescents of Asian descent are under-represented relative to Whites (McNulty and Bellair 2003b).

Findings from recent contextual studies indicate that ethnoracial disparities in violence are reduced once concentrated disadvantage is held constant (Bellair and McNulty 2005; Lauritsen and White 2001; McNulty and Bellair 2003a, b; Sampson et al. 2005), which

is consistent with the racial invariance thesis (Sampson et al. 2018). This has created ambiguity in the literature because concentrated disadvantage does not fully explain ethnoracial disparities in violence in many studies, which some argue contradicts the invariance thesis (Unnever 2018). This has led to calls for a distinct theory to address the unique causes of crime in poor, African American neighborhoods, and we presume other communities of color, such as a high incidence of police contact and experiences of discrimination (Unnever et al. 2016). Yet, this may be a false dichotomy, because point in time measurement of neighborhood disadvantage may not fully capture historic or more contemporary discrimination that is rooted in neighborhood structures (Sampson et al. 2018). Indeed, most studies employ point-in-time (i.e., cross-sectional) indicators of disadvantage, which may underestimate neighborhood effects relative to duration weighted measurement (see Carlson et al. 2022).

Duration Weighted Neighborhood Disadvantage and Ethnoracial Disparities

Ambiguous findings pertaining to the role of neighborhoods in producing ethnoracial disparities in violence do not account for the length of one's life spent residing in disadvantaged neighborhoods. Most importantly, we hypothesize that the failure to account for cumulative exposure to neighborhood disadvantage throughout childhood and adolescence may lead to underestimation of neighborhood effects. Some research assesses the critical distinction between point-in-time or cross-sectional measurement of neighborhood disadvantage and duration weighted or cumulative exposure to neighborhood disadvantage. One study suggests that point in time measurement is a reasonable proxy for duration weighted measurement (Kunz et al. 2003). Most studies (Kravitz-Wirtz 2016a, b; Wodtke 2013), however, document significant variation in youth exposure to neighborhood disadvantage over time (e.g., Timberlake 2007; Quillian 2003), and report that incorporating variation in exposure significantly improves prediction and explanation of self-rated health, smoking initiation, sex risk behavior, health inequalities, high school graduation, reading and math test scores, and adolescent parenting (Carlson et al. 2022; Hicks et al. 2018; Jackson and Mare 2007; Quillian 2003; Timberlake 2007; Wodtke et al. 2011). Although the aforementioned studies indicate support for duration weighted measurement of neighborhood disadvantage, there is very limited research that includes criminological outcomes. In particular, there are no studies to our knowledge focusing specifically on youth aggression and self-reported violence.

Disadvantaged neighborhoods have fewer institutional resources, such as quality schools, recreational activities, child care, medical facilities, and employment opportunities (Leventhal and Brooks-Gunn 2000). The dearth of institutional resources is likely to undermine children and adolescent's social bonds to prosocial institutions such as school (Hirschi 1969). Adolescents residing in disadvantaged neighborhoods often experience distinctive cultural milieus and role models (Akers 1998), and may observe economic marginality among residents that leads them to conclude that their own future opportunities are limited (Bellair and Roscigno 2000). Economic deprivation, blighted institutional resources, and limited educational or employment opportunities may weaken the constraints and opportunity costs that impede risky behavior (Borowsky et al. 2009; Edin and Kefalas 2005).

The distinction between point-in-time exposure and cumulative exposure is critical because the neighborhood mechanisms that produce delinquent behavior may be

more fundamental when experienced over a longer duration. As Sharkey (2014: p 567) poignantly describes:

> "It is natural to think that the residential environment surrounding children will have a greater influence on their lives if they are in the same environment over years or decades. Only recently, however, has the dimension of time entered into the empirical literature on neighborhood effects."

Sharkey (2014) further points out that in the context of Wilson (1987) and Massey and Denton's (1993) literature defining work, duration of exposure to disadvantaged residential environments is a critical dimension, yet that component is rarely addressed in most research. Due in part to discriminatory housing practices and residential segregation, African American, Multiracial, and Hispanic youth are more likely than Whites (and Asians) to experience generational exposure to concentrated neighborhood disadvantage (Lichter et al. 2016; Sharkey 2013; Timberlake 2007). Because a duration-weighted neighborhood disadvantage index captures adolescents that may have spent their entire lives exposed to the extremes of concentrated disadvantage, it is likely to yield better explanation of ethnoracial disparities.

An additional limitation of previous research is that time-varying factors, like family structure and socioeconomic status (SES), are treated as control variables that are unique relative to the neighborhood effect. However, time-varying confounders are in part outcomes of living in a disadvantaged residential context, and could bias estimates of neighborhood effects downward. This may introduce collider-stratification bias (Elwert and Winship 2014; Greenland 2003) due to joint associations with unobserved confounders that are themselves related to youth violence, and prior treatment to neighborhood disadvantage and violence (Elwert and Winship 2014; Greenland 2003; Wodtke et al. 2011). Time-varying factors may also mediate the association between neighborhood disadvantage and youth violence. Although including time-varying controls in regression models may produce downwardly biased estimates of neighborhood effects, excluding these variables may result in overestimation of neighborhood effects. Indeed, factors like family structure and SES also affect the length of youths' exposure to neighborhood disadvantage (for a review, see Carlson et al. 2014).

Data and Methods

We address these issues with data drawn from the Future of Families and Child Well-Being Study (FFCWS). FFCWS is a longitudinal cohort study of 4,898 children born between 1998 and 2000 in 20 large U.S. cities, with an oversample of nonmarital births. Interviews in the hospital with mothers and fathers (or primary caregivers) at the child's birth comprise the baseline, Wave 1 data. Follow ups are conducted when children reach age 1 (Wave 2), 3 (Wave 3), 5 (Wave 4), 9 (Wave 5), and 15 (Wave 6). Phone interviews with parents are conducted when the children are ages 1 (Wave 2), 3 (Wave 3), and 5 (Wave 4), and in-home assessments of children's home environments are also conducted at ages 3 (Wave 3), 5 (Wave 4), 9 (Wave 5), and 15 (Wave 6). US census tract data are matched to the FFCWS at each wave because they have been used extensively to estimate neighborhood-level effects in prior research (Coulton et al. 2001; Krieger et al. 2003). Indicators of neighborhood disadvantage are drawn from the 2000 US Census through Wave 5, and the 2010 US Census and the 2015 American Community Survey 5-year averages are used at Wave 6.

The analysis is restricted to youth with complete data through Wave 6 because that is the year we measure the dependent variables. The sample is further delimited to youth who were identified by their parents as multiracial, non-Hispanic Black, non-Hispanic White, non-Hispanic Asian, or Hispanic. The very small number of youth (i.e., 40) who did not fit those categories are dropped due to limited statistical power. Of the original 4,898 children whose mothers participated in the baseline interview, 3,404 completed the Wave 6 interview when the youth were 15 years old. Missing information on parents' income and work hours contributed the most to attrition across waves. Of the 3,404 youth who participated in the Wave 6 interview, 3,360 cases had complete information on parent/guardian reports of youths' aggressive behavior and 3,381 had complete information on youths' self-report of violent behavior. Missing information for independent and control variables at each wave was replaced using multiple imputation with chained imputations in Stata 15. We used Von Hippel (2020) program "how_many_imputations" in Stata to estimate the number of imputations needed for efficient estimation of standard errors. The program provides a recommended number of imputations such that standard errors for estimates do not change significantly with additional imputations. The results indicated that 10 imputed data sets were likely sufficient. As we describe in more detail below, missing data is addressed in part by employing a censoring weight that upweights cases with a greater likelihood of attrition.

Measures

Two dependent variables are analyzed in this study. The p*rimary caregiver report of youth aggressio*n is a summary scale of primary caregivers' responses to 11 items from the child behavior checklist (CBCL). The items reflect whether the child is aggressive in different contexts. The items were completed by the primary caregiver when the youth were fifteen years old. Examples of items include whether the child argues a lot, is cruel to or bullies other children, destroys things, and gets into fights with and/or physically attacks other children. Response options for each item include: 0 = not true; 1 = sometimes true; 2 = always true. The scale reflects the mean level of involvement across each item.

The second dependent variable is y*outh's self-report of violent behavio*r at age 15. Four items are included: getting into a serious physical fight, hurting someone badly enough to need medical care, using or threatening to use a weapon, and taking part in a group fight. Response categories range from never, 1 or 2 times, 3 or 4 times, or 5 or more times (coded 0 to 3, respectively). Self-reported violence is measured as the mean involvement across those items.

Ethnoracial identity is represented by a set of dummy variables. The variables distinguish *multiracial, non-Hispanic Black, Hispanic, non-Hispanic Asian youth from non-Hispanic White* youth. White youth comprise the reference category.

Neighborhood disadvantage is a composite index of seven indicators at the census-tract level, including (1) percentage of residents over age 16 who are unemployed, (2) percentage of households below the federal poverty threshold, (3) percentage of female-headed households with minor children, (4) percentage of adults over age 25 without a high school degree, (5) percentage of properties that are vacant, (6) percentage of residents who are non-White, and (7) median household income (reverse coded). Not all studies include percent non-White in the calculation of concentrated disadvantage. It is included here to capture the effect of historic discrimination, which Sampson (2012) argues is rooted in contemporary neighborhood structure. Principal component analysis identified a single

common factor among the indicators and a composite score for each.[1] Each indicator of disadvantage is then standardized (i.e., represented as z scores) using the national census-tract average and standard deviation and then combined.

We then divided census tracts into quintiles based on the national distribution of the composite neighborhood disadvantage measures to create an ordinal measure of *neighborhood disadvantage at age 9* that ranged from 1 = least disadvantaged to 5 = most disadvantaged. By creating an ordinal measure of neighborhood disadvantage we can more parsimoniously predict treatment and construct probability of treatment weights. The use of a continuous measure of disadvantage would likely necessitate the estimation of thousands of models for each disadvantage score. To construct *duration of exposure to neighborhood disadvantage through age 9*, we took the average of neighborhood disadvantage across the five waves up to and including wave 5 (age 9).

To explain ethnoracial disparities in youth aggression and violence, and to demonstrate the utility of measuring duration-weighted exposure to disadvantage, models alternately and collectively include a measure of *neighborhood disadvantage at age 9* (Wave 5) and *duration-weighted exposure to neighborhood disadvantage*. Because duration-weighted exposure is an average composite of exposures to neighborhood disadvantage, direct comparisons of effect magnitude can be made between proximal exposure to disadvantage at age 9 and long-term exposure as indicated by the duration-weighted measure. The means of the neighborhood disadvantage measures are statistically indistinguishable, yet readers should exercise some caution when comparing coefficients because small differences in the distributions could complicate the comparison. Although measuring neighborhood disadvantage up to age 9 establishes temporal ordering with respect to the outcomes, it may result in underestimation of neighborhood effects. However, a supplemental model (not shown) that substituted a measure of neighborhood disadvantage (i.e., point in time) at age 15 (Wave 6) did not produce substantively different results.

Control Variables

We control for a set of time-invariant and time-varying confounders in our models. Time-invariant baseline controls include *youth female* (1 = female), *mother's age at youth's birth* (in years), *mother U.S. born* (1 = yes), youth *born outside of marriage* (1 = yes), and *mother's education* (1 = less than high school, 2 = high school or equivalent, 3 = some college or technical degree, 4 = college or advanced degree). Five time-varying socioeconomic confounders were assessed at each wave and are included in the estimation of the marginal structural and IPTW models. *Family structure* is assessed with dummy variables distinguishing two-parent biological family (reference), single mother living with relatives or alone, mother–stepfather family, and other family arrangements. Two-parent biological families were defined as a child living with both biological parents, either cohabiting or married. We controlled for *household income* measured in dollars to assess time-varying aspects of family socioeconomic status (SES). *Parent work hours* is a continuous measure indicating the number of hours the parent worked; participants not in the labor force were coded 0. Father's work hours are used in limited cases when the child resides with the father

[1] Factor loadings for Neighborhood Disadvantage: proportion no high school degree .399; proportion in poverty = .450; proportion unemployed = .384; proportion of properties that are vacant .123; proportion of female-headed households .432; proportion of residents who are non-white .368; median household income .392.

only, and mother's work hours are used in all other cases. Last, *residential mobility* (1 = yes) between waves is included to capture whether respondents moved in between waves.

Analytic Strategy

Marginal structural models (MSM) (Fewell et al. 2004) are used to assess the role of exposure to neighborhood disadvantage in explaining youths' aggressive and violent behavior, including ethnoracial differences. We choose MSM for our analyses because MSM produces more valid estimates in situations when an explanatory time-varying variable – in this instance exposure to neighborhood disadvantage – may be reciprocally related to time-varying confounders – like family SES. MSM creates a pseudo-population where time-varying explanatory variables and time-varying confounders are unassociated with each other (Do et al. 2013). The models are run in two stages. In the first stage, we construct an inverse probability of treatment weight (IPTW) for youths' probability of exposure to their actual neighborhood disadvantage quintile at Wave k. This probability is calculated based on time-varying and time-invariant predictors of exposure to neighborhood disadvantage. In Stage 2, the IPTW is used to adjust regression estimates predicting aggression and violence.

To construct the IPTW, we used data from the first five waves of the FFCWS among youth in our analytic sample. The IPTW is presented in Eq. 1.

$$W(t) = \prod_{k=0}^{t} \frac{1}{f\left\{A(k)|\underline{A}(k-1), \underline{L}(k)\right\}} \tag{1}$$

f{*} is the conditional probability density function, $A(k)$ is the time-varying treatment at Wave k, $\underline{A}(k-1)$ is previous treatment history up to wave $(k-1)$, and $\underline{L}(k)$ represents the history of time-dependent confounders. Estimation of the IPTW includes information on youths' neighborhood quintile at baseline and at Wave $(k-1)$, time-invariant baseline covariates, and information on time-varying covariates at Wave k, Wave $(k-1)$, and baseline.

We calculate a stabilized IPTW to reduce variability and non-normality in the estimate. Stabilized weights have smaller variance, a mean of around 1, and are approximately normally distributed (Robins et al. 2000). Using notation from Fewell et al. (2004), we calculate the stabilized IPTW as follows:

$$SW(t) = \prod_{k=0}^{t} \frac{f\left\{A(k)|\underline{A}(k-1), L(0)\right\}}{f\left\{A(k)|\underline{A}(k-1), \underline{L}(k)\right\}} \tag{2}$$

The numerator is a constrained version of the predicted probability of treatment (i.e., exposure to actual neighborhood disadvantage quintile) that includes neighborhood treatment at Wave k, Wave $(k-1)$, and baseline measures of covariates only $L(0)$. It can be interpreted as the subject's probability of experiencing their treatment history up to Wave k given baseline values of controls (Do et al. 2013). The denominator is the probability of treatment at Wave k given time-varying covariates and baseline controls.

To partially account for the possibility of nonrandom sample attrition, we multiply the stabilized IPTW at each wave of observation by a censoring weight which estimates the inverse probability of dropping out of the study at any particular wave as predicted by

Table 1 Stabilized treatment and censure weights

			Percentiles			
	M	SD	1st	25th	75th	99th
Stabilized treatment weight (SW)	1.01	.24	.52	.88	1.07	1.85
Stabilized censure weight (CW)	1.00	.05	.92	.97	1.04	1.17
SW×CW	1.01	.25	.52	.89	1.09	1.92

Data are from the Future of Families and Child Well-Being Study, Waves 1 through 5

time-invariant and time-varying variables. Table 1 displays summary statistics for the stabilized IPTW and censoring weight.

We conduct our analysis in two steps. First, we establish the utility of the MSM by comparing estimates of the association of exposure to neighborhood disadvantage with caregivers' reports of youth aggression and youths' self-reported violence across three models – 1) unadjusted bivariate generalized linear model (GLM) regression (gamma distribution); 2) multivariate GLM regression (gamma) with time-invariant and time-varying controls, and 3) IPTW adjusted multivariate gamma GLM regression (gamma) with time-invariant controls. Analyses of aggressive and violent behaviors are conducted using GLM (gamma) given the continuous, positively skewed distribution of scores for aggressive and violent behavior and a high variance relative to the scale mean. Given that we assess the dependent variables at only one point in time (age 15), values for time-varying variables and the IPTW are calculated as the average value of these variables across waves. To establish temporal order, all time-varying variables, including neighborhood disadvantage and all confounders, as well as the IPTW are assessed only through Wave 5 (age 9).

In the second step we use GLM regression (gamma) and the KHB decomposition method (Breen et al. 2013) to examine how exposure to neighborhood disadvantage mediates the association of ethnoracial identity with aggressive and violent behaviors. As described in the measures section, two measures of neighborhood disadvantage in our models – a point-in-time estimate of neighborhood disadvantage at age 9 and a duration-weighted measure of exposure to neighborhood disadvantage through age 9. We include both measures to compare their association with aggression and self-reported violence. Both variables are measured on the same scale, with similar means and standard errors that are statistically indistinguishable ($p > 0.05$). Thus, the coefficients are directly comparable, but with caution. In the first regression model, we examine the association of ethnoracial identity with youths' aggressive/violent behaviors. In Models 2 through 4, we add the two measures of neighborhood disadvantage separately and then jointly. In Model 5, we include background confounders of the association between neighborhood disadvantage and the outcomes.

Results

Descriptive Statistics

Table 2 presents descriptive statistics for the full sample in the first column and the ethnoracial subgroups in columns 2–6. Non-Hispanic Black children (47%) comprise

Table 2 Descriptive statistics

Variables	All ($N=3,381$)	Multiracial ($n=507$)	Non-Hispanic Black ($n=1,586$)	Non-Hispanic White ($n=563$)	Hispanic ($n=685$)	Non-Hispanic Asian ($n=40$)
	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)
Caregiver Report of Youth Aggressive Behaviors (Age 15)	.273 (.005)	.280 (.014)	.301 (.008)	.245 (.012)	.229 (.011)	.222* (.043)
Youth Self-Report of Violence (Age 15)	.158 (.006)	.140 (.014)	.212 (.009)	.073 (.010)	.123 (.011)	.081* (.040)
Neighborhood disadvantage						
Duration-weighted	3.102 (.023)	2.724 (.054)	3.736 (.027)	1.549 (.035)	3.264 (.043)	1.866* (.148)
Age 9	3.055 (.029)	2.630 (.070)	3.642 (.040)	1.607 (.047)	3.275 (.058)	1.848* (.219)
Female	.486	.483	.492	.483	.486	.300
Mother's Age at Birth	25.122 (.103)	24.586 (.249)	24.359 (.142)	27.933 (.278)	24.694 (.218)	29.950* (.959)
Mother U.S. born	.868	.888	.967	.972	.589	.100
Born Outside Marriage	.758	.777	.877	.425	.775	.200*
Mother's education (Baseline)						
Less than high school	.320	.291	.324	.135	.499	.075*
High school or equivalent	.316	.283	.377	.233	.275	.225
Some college; technical school	.252	.327	.247	.272	.199	.150
College or graduate degree	.111	.099	.051	.361	.028	.555
Family structure (Average)						
Two-parent biological	.302	.263	.162	.561	.416	.743*
Single mother family	.544	.586	.630	.350	.491	.225
Mother-stepfather	.152	.152	.204	.089	.090	.033
Other family type	.003	.000	.004	.000	.003	.000

Table 2 (continued)

Variables	All ($N=3,381$)	Multiracial ($n=507$)	Non-Hispanic Black ($n=1,586$)	Non-Hispanic White ($n=563$)	Hispanic ($n=685$)	Non-Hispanic Asian ($n=40$)
	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)	Mean/Prop (SE)
Household Income (Average)	41,276.08 (663.85)	42,986.81 (1593.42)	30,718.08 (579.43)	77,125.13 (2325.10)	32,128.72 (832.11)	90,290.64* (8008.07)
Parental Work Hours (Average)	25.724 (.199)	27.297 (.482)	26.760 (.294)	24.805 (.485)	22.886 (.444)	26.223* (1.872)
Residential Mobility (Average)	.441	.485	.472	.354	.417	.322*

* indicates significant ethnoracial differences at .05 level

the largest share of the sample, followed by Hispanics (20%), non-Hispanic Whites (17%), and those who identify as multiracial (15%). Non-Hispanic Asian youths are under-represented in the FFCWS with only 40 children in the sample. Consistent with the literature on violence, primary caregiver reports of aggression and the youths' self-reported violent behavior are relatively rare within the sample. The scale mean of 0.27 on the primary caregiver report of aggression indicates that about 27% of youth behaved aggressively. Among youth, the mean score for violent behavior (0.16) indicates that only 16% of the youth engaged in any of the violent behaviors, on average. There is significant variation in aggression and violent self-reports among ethnoracial subgroups. Youth who identify as multiracial and non-Hispanic Black evidence a significantly higher score on primary caregivers' reports of aggressive behavior compared to non-Hispanic White children. Hispanic and Asian youth are statistically similar to non-Hispanic Whites. Regarding youths' self-reports of violent behavior, multiracial, Black, and Hispanic youth report engaging in more violence than non-Hispanic White youth. Asian youth are statistically similar to non-Hispanic Whites.

On average, youths in the sample reside in contexts that are neither extremely disadvantaged nor advantaged, experiencing levels between those polar extremes (i.e., 3.102 out of 5). The neighborhood disadvantage means also vary based on the duration weighted or point in time (i.e., age 9). Non-Hispanic White and Asian youth are at the lowest risk of longer-term exposure to neighborhood disadvantage, and evidence lower levels of neighborhood disadvantage at age 9, compared to multiracial, Black, and Hispanic youth. Duration weighted exposure is especially pronounced for non-Hispanic Blacks and Hispanics.

Youths' biological sex is roughly evenly split with females comprising about 49% of the sample, and there are slightly more males than females in each ethnoracial group except Asian. Non-Hispanic White and Asian children are significantly less likely to have been born to younger mothers, or to have been born outside of marriage, compared to the other ethnoracial groups. Most mothers were born in the U.S. The exception is Hispanic mothers, 40% of whom are foreign born. Roughly 76% of the sample is born outside of marriage, reflecting the sampling design of the FFCWS. Educational attainment among the mothers is somewhat constrained, with just under a third of mothers attaining less than high school and roughly the same proportion finishing high school. About 25% have attained some college with about 1 in 10 completing a college or graduate degree. Non-Hispanic White and Asian mothers are most likely to be college graduates. At the other extreme, multiracial (29%), Black (32%), and especially the mothers of Hispanic children (50%) are significantly more likely than White mothers (13%) to have less than a high school degree.

Over half of the families in the data are led by single mothers, just under one third are two-parent biological, and 15% are mother-stepfather, with a small fraction of other family types. Among the ethnoracial subgroups, non-Hispanic White and Asian children are significantly more likely to live in two biological parent families, whereas multiracial (59%), Black (63%), and Hispanic children (49%) are significantly more likely to be in single parent households compared to their White and Asian counterparts.

The typical household earns just over $41,000, and mothers work about 25 h per week on average. There is marked variation among the subgroups, with Asian and White households earning over twice the income relative to non-Hispanic Black and Hispanic, with multi-racial households earning somewhat, but not substantially higher wages relative to non-Hispanic Black and Hispanic households. Parent's work hours do not vary markedly across subgroups, although Hispanic mothers work the fewest number of hours. Multiracial and non-Hispanic Black youth are most likely to have moved between waves, followed by Hispanics, non-Hispanic Whites and Asians.

Table 3 Effects of duration-weighted exposure to neighborhood disadvantage at age 9 on parents' reports of youth aggression and self-reported violence

Model	Primary caregiver report of aggressive behaviors ($n = 3{,}360$)	Youth self-report of violent behaviors ($n = 3{,}381$)
Bivariate GLS (gamma) regression	.085*** (.015)	.350*** (.032)
Multivariate GLS regression (gamma) with time-invariant and time-varying controls	.028 (.021)	.171*** (.046)
Stabilized IPTW	.042* (.020)	.209*** (.038)

Multivariate Models

Table 3 illustrates the consequences of failing to properly account for reciprocity in the relationships of time-varying confounders with neighborhood disadvantage. In a bivariate GLM (gamma) model, the duration weighted measure of neighborhood disadvantage exerts a positive and significant effect on aggressive behavior, but the effect dissipates and is not significant in the multivariate GLM (gamma) regression when time-invariant and time-varying variables are included as statistical controls. This indicates that when the IPTW weight is not utilized, the effect of duration weighted disadvantage is underestimated. However, once the stabilized IPTW is incorporated into the model, the duration weighted effect is positive and significant, indicating the importance of the length of time exposed to disadvantage and its role in shaping aggressive behavior by age 15. A similar pattern is observed when the lens is shifted towards youth's self-reported violence, although the effect of duration weighted exposure to neighborhood disadvantage remains significant without invoking the stabilized IPTW.

Primary Caregiver Reports of Youth Aggression

Table 4 regresses primary caregiver reports of aggression (age 15) on ethnoracial group and neighborhood disadvantage. Model 1 highlights the baseline levels of aggressive behavior by ethnoracial identity. It indicates that Black children exhibit significantly more aggressive behavior compared to their non-Hispanic White counterparts. Multiracial, Hispanic, and Asian youth evidence statistically similar levels of involvement relative to non-Hispanic Whites.

Model 2 incorporates duration-weighted exposure to neighborhood disadvantage, which has the expected positive, significant effect on adolescent aggression. Importantly, Model 2 indicates that the disparity in aggressive behavior between White and Black adolescents is reduced to non-significance, reflecting the relatively longer-term exposure of the latter to neighborhood disadvantage compared to White children (see Table 2). The Hispanic-White disparity is suppressed in Model 1, as Model 2 reveals significantly lower levels of aggression among Hispanic youth (relative to White) when duration weighted exposure is controlled. In Model 3, we incorporate the point-in-time measure of neighborhood disadvantage, which exerts a significant, positive effect on aggressive behavior. It is also

Table 4 GLM gamma regression of parents'/guardians' reports of youth aggression at age 15 on ethnoracial status and neighborhood disadvantage ($N = 3{,}360$)

	Model 1 B (se)	Model 2 B (se)	Model 3 B (se)	Model 4 B (se)	Model 5 B (se)
Race-ethnicity (ref = Non-Hispanic White)					
Multiracial	.135 (.071)	.046 (.073)	.101 (.073)	.046 (.073)	-.001 (.074)
Non-Hispanic Black	.206*** (.057)	.037 (.078)	.142* (.064)	.037 (.070)	-.039 (.071)
Hispanic	-.072 (.068)	-.198* (.074)	-.124 (.072)	-.197** (.075)	-.187* (.079)
Non-Hispanic Asian	-.119 (.198)	-.153 (.197)	-.134 (.199)	-.152 (.197)	.119 (.193)
Duration-weighted exposure to neighborhood disadvantage through age 9		.078*** (.019)		.093** (.029)	.039* (.020)
Neighborhood disadvantage – age 9			.032* (.014)	-.016 (.022)	
Female					-.095* (.040)
Mom age at birth					-.015*** (.004)
Youth born outside marriage					.118* (.058)
Mother's education (baseline)					-.074** (.024)
Mother is U.S. born					.266*** (.073)

*** $p < .001$; ** $p < .01$; * $p < .05$; all models are weighted using IPTW

noteworthy that the point-in-time measure does not fully explain the heightened aggression among Black adolescents relative to White adolescents.

In Model 4, the duration weighted disadvantage measure is contrasted directly with the point in time measure. The results reveal that the duration weighted measure retains significance, while the point in time measure is zero. Variance inflation factor (VIF) estimates using OLS regression reveal little evidence of multicollinearity between these measures of disadvantage (VIF = 3.4). This indicates that the duration weighted measure, which reflects longer term exposure to disadvantaged contexts, has greater explanatory power than the point in time estimate and that point in time estimates are not proxies for measures of duration of exposure to disadvantage. Note as well that the difference in coefficients between the duration-weighted and point-in-time measures in Model 4 are statistically different ($p < 0.01$), lending additional evidence that duration weighted exposure better captures the realities of the consequences of longer term exposure to neighborhood disadvantage relative to cross-sectional measures.

Model 5 includes the remaining time-invariant control variables along with duration weighted exposure to neighborhood disadvantage. The latter retains a significant, positive effect and the Black effect remains zero. We note, however, consistent with recent research, that Hispanic youth evidence a lower risk of aggressive behavior than White youth, an effect that is suppressed by duration-weighted exposure to neighborhood disadvantage.

Table 5 GLM gamma regression of youths' self-reports of violent behavior at age 15 on ethnoracial status and neighborhood disadvantage ($N = 3{,}381$)

	Model 1 B (se)	Model 2 B (se)	Model 3 B (se)	Model 4 B (se)	Model 5 B (se)
Race-ethnicity (ref = Non-Hispanic White)					
Multiracial	.652*** (.168)	.364* (.177)	.553* (.174)	.358* (.177)	.429* (.190)
Non-Hispanic Black	1.075*** (.143)	.530** (.162)	.868*** (.152)	.528** (.162)	.529** (.169)
Hispanic	.521** (.163)	.119 (.173)	.356* (.171)	.122 (.173)	.208 (.186)
Non-Hispanic Asian	.041 (.503)	-.329 (.496)	-.157 (.516)	-.312 (.491)	-.145 (.520)
Duration-weighted exposure to neighborhood disadvantage through age 9		.265*** (.037)		.305*** (.056)	.207*** (.044)
Neighborhood disadvantage – age 9			.110*** (.028)	-.042 (.042)	
Female					-.717*** (.044)
Mom age at birth					-.022* (.009)
Youth born outside marriage					.518*** (.131)
Mother's education (baseline)					-.262*** (.054)
Mother is U.S. born					.374* (.175)

*** $p < .001$; ** $p < .01$; * $p < .05$; all models are weighted using IPTW

Control variables have expected effects on aggression, but do not explain away the effect of duration weighted disadvantage on aggressive behavior. Females are less involved than males. Greater maternal educational attainment also exerts a constraint. In contrast, youth born outside of marriage, to younger mothers, and to U.S. born mothers are more likely to engage in aggressive behavior at age 15. Overall, the findings indicate support for the hypothesis that longer-term exposure to concentrated disadvantage is more consequential for Black and Hispanic youth (relative to White) than is point in time exposure.

Youth Self-reports of Violent Behavior

Table 5 presents GLM (Gamma) regressions of youth self-reports of violent behavior on ethnoracial identity and neighborhood disadvantage. Model 1 shows that Multiracial, Black, and Hispanic adolescents evidence significantly more involvement in violence compared to non-Hispanic white youth. Asian youth are statistically similar to their white counterparts. Model 2 incorporates duration weighted exposure to neighborhood disadvantage, which has a significant effect ($p < 0.001$) on adolescent self-reported violence. The Hispanic-White disparity in violence involvement is reduced in magnitude and to non-significance, whereas the Multiracial and Black coefficients have been reduced substantially but remain significant.

Model 3 in Table 5 incorporates point-in-time neighborhood disadvantage at age 9, which has a significant, positive effect on violent behavior. Ethnoracial disparities in violent behavior compared to Whites remain significant. Compared to Model 1 the Multiracial, Black, and Hispanic coefficients are reduced but not to the extent evidenced in Model 2, which included duration weighted exposure to neighborhood disadvantage. In Model 4, the duration weighted disadvantage measure is contrasted directly with the point in time measure. The latter is zero and not significant, whereas duration weighted exposure retains a significant positive effect on violence. This indicates that prolonged exposure to neighborhood disadvantage has greater explanatory power. Importantly, the effect of duration-weighted exposure is significantly larger than the point-in-time coefficient in Table 5, Model 4 ($p < 0.001$), which again suggests that duration-weighted exposure better captures the consequences of longer term exposure to neighborhood disadvantage relative to point-in-time measures.

Model 5 includes the time-invariant control variables along with duration weighted exposure to neighborhood disadvantage. The Hispanic-White disparity in violence remains zero, whereas the magnitude of the coefficients for Multiracial and Black youth have been reduced substantially (compared to Model 1), although they remain significant. Control variables have significant effects in theoretically expected directions that mirror the findings in Table 4.

KHB Decomposition

Table 6 presents the results from the KHB decomposition analyses. The KHB method decomposes the total effect of a variable into direct and indirect effects (Karlson et al. 2012), including both discrete and continuous variables, providing a test of mediation. The KHB method was not designed specifically to decompose effects for GLM models and so estimates should be interpreted with some caution. The decomposition compares the coefficients of the ethnoracial variables between models that alternately include the duration weighted measure of disadvantage (Model 5 of Tables 4 and 5 are used as input). The difference between the coefficient of the ethnoracial variables in the two models reveals the portion of the total effect that is mediated by duration-weighted exposure to neighborhood disadvantage.

With respect to caregiver reports of aggression, findings indicate that about 70% of the total Multiracial-White difference, 87% of the total Black-White difference, and about 217%[2] of the Hispanic-White difference in aggression is explained by duration-weighted exposure to neighborhood disadvantage. With respect to youth self-reports of violence, about 45% of the total Multiracial-White difference, 52% of the Black-White difference, and about 80% of the Hispanic-White difference is attributable to long-term exposure to neighborhood disadvantage. The results highlight the consequences of prolonged exposure to neighborhood disadvantage for aggression and violence outcomes, especially during childhood.

[2] Duration weighted exposure to neighborhood disadvantage explains more than 100% of the total Hispanic-White difference since lower levels of aggression among Hispanic youth relative to White youth is suppressed via exclusion of disadvantage from models.

Table 6 KHB method decomposition of indirect effects of race-ethnicity on aggression and violence attributable to duration-weighted exposure to neighborhood disadvantage

	Parent/Guardian report of youth aggression—age 15 ($N=3,360$)		Youth report of violent behavior – age 15 ($N=3,381$)	
	B (se)	% Of total effect that is indirect	B (se)	% Of total effect that is indirect
Race-Ethnicity (Ref = Non-Hispanic White)				
Multiracial				
Indirect Effect	.095*** (.026)	69.76%	.306*** (.063)	45.42%
Non-Hispanic Black				
Indirect Effect	.178*** (.042)	87.03%	.582*** (.089)	52.36%
Hispanic				
Indirect Effect	.140*** (.034)	216.65%	.455*** (.076)	79.79%
Non-Hispanic Asian				
Indirect Effect	.025 (.016)	18.68%	.082 (.051)	23.79%

Data are from the Future of Families and Child Well-Being Study (FFCWS), Waves 1–6; All models include controls for female, mom age at birth, youth born outside marriage, mother's education, and mother is U.S. born; All models are weighted using IPTW

* $p<.05$, ** $p<.01$, *** $p<.001$

Discussion and Conclusion

This study addresses a series of issues that are critical for the neighborhood effect literature as it pertains to studies of aggression and self-reported violence. First, and perhaps most importantly, the results support a growing body of literature documenting the importance of duration of exposure to neighborhood disadvantage (Kravitz-Wirtz 2016a, b; Wodtke 2013; Wodtke et al. 2011). Most studies do not differentiate between individuals or families that are intermittently exposed to socioeconomically disadvantaged neighborhoods from those that experience a longer duration of exposure, and some studies have suggested that there is no need to make this distinction (Kunz et al. 2003). Our findings support the hypothesis that duration weighted measurement of disadvantage exerts a larger effect on adolescent aggressive and violent behavior than the cross-sectional (i.e., point in time) estimate. When both measures are included in the same model, only the duration weighted measure retains significance.

The distinction intersects directly with multilevel racial invariance research, particularly studies that address ethnoracial differences in aggression and violence (e.g., McNulty and Bellair, 2003b). Shaw and McKay (1942) argued that the neighborhood contexts experienced by poor Blacks, in particular, are qualitatively distinct from those of predominantly White neighborhoods. This logic is the basis of the racial invariance thesis, wherein ethnoracial differences in delinquency, particularly violence, are the result of group-specific exposure to neighborhood structures, particularly concentrated disadvantage (Sampson and Wilson 1995). We find, as have others,

that Black and Hispanic families are more likely to be exposed to disadvantage and experience greater socioeconomic inequality relative to White families. Findings reveal that prolonged exposure to neighborhood disadvantage explains the Black-White disparity in aggression and that Hispanics are at a significantly lower risk of aggressive behavior relative to Whites (Table 4). Regarding youth self-reported violence, duration weighted exposure explains the Hispanic-White disparity in violence, and substantially mediates Multiracial and Black effects (Tables 5 and 6). The analysis indicates that duration weighted exposure to neighborhood disadvantage explains a larger share of the association with aggression and violence than does the point in time measure. We conclude that point in time estimation is not a proxy for duration weighted exposure despite the fact that those measures are typically correlated.

We suggest that analyses of neighborhood effects should be wary of this distinction in future research. It will also be important to replicate these findings and to go beyond with analysis of delinquency scales that tap into serious misbehavior that is likely to draw official attention from schools and the criminal justice system, including more serious forms of violence for which ethnoracial disparities are especially pronounced. In addition, multilevel models addressing neighborhood effects on individual-level disparities in violence are needed to revisit the racial invariance thesis using duration weighted exposure measures. Our results provide additional evidence that longer term exposure to concentrated neighborhood disadvantage is quite detrimental to families and children, and that it is directly associated with aggression and violence.

Beyond the issue of the importance of duration weighted disadvantage, the results for the primary caregiver reports of aggression indicate that conventional regression models often overcontrol for individual-level socioeconomic characteristics (e.g., family SES), leading researchers to underestimate neighborhood effects (Elwert and Winship 2014; Greenland 2003). As Wodtke et al. (2011) advocate, we use marginal structural modeling (MSM) with inverse probability of treatment weights (IPTW) to correct for the tendency to underestimate neighborhood effects.

We recognize the difficulty of addressing these issues in prior research. Cross-sectional datasets lack the detailed migration information needed to correctly characterize the duration of exposure to neighborhood disadvantage, but longitudinal data sets offer the opportunity to document these exposures. The issue can be rectified by geocoding previous addresses retrospectively. Yet, even with that information, it is unlikely that most longitudinal data sets contain the residential addresses of previous generations and thus cannot fully address the generational issues raised by Sharkey and Elwert (2011). The results presented here do not include generational data, but even without it, we are still able to pick up and document strong duration weighted effects on youth aggression and violent behavior.

The analysis has limitations, in particular the gaps between waves in the FFCWS prevent us from forming a more accurate estimate of long-term exposure to neighborhood disadvantage. For example, wave 1 and wave 2 are conducted at birth and at age 1, but subsequent follow-ups do not occur until age 3 and then again at age 5. The final wave we utilize for the disadvantage measures takes place when children are 9 years old. Although respondents are asked whether they changed residences between the follow-ups, there is no available residential geocode from which tract-level census data could be merged for the intervening years. This could be improved in future research with data sets that have annual geocoded residential information. Beyond the issue of measuring youth's exposure to disadvantage is the issue of measuring parent's residential patterns. This latter point addresses

intergenerational patterns of exposure, which are likely to vary significantly between and importantly within ethnoracial groups.

Overall, we think that the findings open up new possibilities to investigate neighborhood effects in criminology. There are several subareas in which neighborhood effects are prominent, including the delinquency, gang, recidivism, and conflict literatures, among others. Neighborhoods clearly matter, but so does measurement. In closing, we second Sharkey's (2013; 2014) insight that researcher's should refocus attention away from asking whether neighborhoods matter for families and youth outcomes, and towards investigation of how they matter, especially duration of exposure.

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