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The impact of childhood neighborhood disadvantage on adult joblessness and income

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ABSTRACT

Research on residential inequality focuses heavily on adult economic outcomes as crucial components of the intergenerational transmission of poverty. Yet, empirical evidence on whether youth neighborhoods have a lasting impact on adult economic outcomes at the national level is scarce. Further, we know little about how youth neighborhood effects on adult economic outcomes manifest. This study uses 26 years (14 waves) of restricted panel data from the NLSY79 and the NLSY Children and Young Adults cohorts – data that have never been used to analyze long-term neighborhood effects – to examine whether youth neighborhood disadvantage impacts adult economic outcomes through sensitive years in childhood, teen socialization, duration effects, or cumulative effects. Sibling fixed effects models that net out unobserved effects of shared family characteristics suggest that youth neighborhood disadvantage increases joblessness and reduces income in adulthood. However, exposure across specific developmental stages of youth does not appear to act as a significant moderator while sustained exposure yields pernicious effects on adult economic outcomes. Moreover, these results are robust to alternative variable specifications and cousin fixed effects that net out potentially unobserved confounders, such as the inheritance of neighborhood disadvantage across three generations.

1. Introduction

As the catalyst of modern research on residential inequality, Wilson (1987, 1996) theorized that the reproduction of poverty largely operated through the social and economic dislocation that residents of disadvantaged neighborhoods endured throughout their lives (Massey, 2013). In the years since *The Truly Disadvantaged* (1987), research on the impact of concentrated socioeconomic disadvantage and the resultant social dislocation from the cultural and economic mainstream has proliferated (Harding, 2007; Sampson et al., 2002; Sharkey and Faber, 2014; Small and Newman, 2001). Although the neighborhood effects literature has illuminated how ecological context contributes to the intergenerational transmission of poverty, rarely has the direct impact of childhood neighborhoods on adult joblessness or income been the main focus. Instead, scholars have tended to focus on intermediate mechanisms such as educational outcomes, teen pregnancy, and health. I build on previous research and provide an analysis of the impact of childhood neighborhood disadvantage on adult joblessness and income that focuses on the timing of neighborhood effects across the life-course to better understand how socioeconomic context contributes to inequality across generations.

Neighborhood effects researchers have increasingly focused on a temporal dimension that stresses dynamic contextual change and effects that evolve and accumulate over time (Sharkey and Faber, 2014; Tienda, 1991). Thus, scholars have advanced the theoretical conceptualization of how neighborhood effects manifest to involve the timing of effects at certain ages of development and sustained exposure across the life-course (Sharkey and Faber, 2014). Therefore, two puzzles emerge: The first is in regards to whether youth

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neighborhood disadvantage affects adult economic outcomes; the second revolves around the idea of differentiating when (and how) neighborhoods matter.

The current study examines these puzzles of the neighborhood effects literature with more recent and complete adult economic outcome data at the national level, new and more complete control variables, and a focus on the timing of neighborhood effects that stresses both exposure at certain ages of development and sustained exposure. This study also attempts to at least partially address some of the daunting challenges to causality in contextual effects research (Durlauf, 2004; Hauser, 1970, 1974; Manski, 1993). For instance, unobserved family instability shared by siblings could impact both selection into neighborhoods and adult economic outcomes and could subsequently threaten estimates from observational data. The methods I employ partially address some of these issues. Further, the goal here is to establish whether there are any direct impacts and not to exhaustively examine mechanisms.

I use restricted geocoded data from the National Longitudinal Survey of Youth 1979 (NLSY79) cohort and data from the NLSY Children and Young Adults (NLSY:CYA) cohort, the latter of which has followed respondents (many of whom are siblings and cousins) from childhood to adulthood and allows for fixed effects (FE) analyses. The sibling FE models capitalize on neighborhood change that occurs from either 1) moves that occur between siblings or 2) staying and having the neighborhood change around siblings over time. The cousin FE models capitalize on variation in neighborhoods between cousins (i.e., children of NLSY79 sister pairs). These temporally sequenced data and FE methods allow me to address some, but certainly not all, of the potential problems of neighborhood effects estimation. These data have never been used before to study long-term neighborhood effects on adult economic outcomes and allow me to build on seminal work on residential inequality by illuminating when and how neighborhoods matter.

2. Childhood neighborhoods, timing of exposure, and adult economic outcomes

We may expect childhood neighborhood conditions to impact adult economic outcomes indirectly or directly through mediating processes such as education or experience in the labor force. For example, youth who are exposed to disadvantaged neighborhoods may experience fewer educational resources embedded within the community (e.g., high-quality curricula in schools, tutoring center, libraries) that could impact their educational attainment, which in turn may impact their labor market outcomes. Several prior studies demonstrate a link between neighborhood context and key outcomes measured in youth that can have ramifications for future well-being. For example, previous research has found that neighborhoods impact outcomes such as cognitive development (Alvarado, 2016a; Sampson et al., 2008; Sharkey and Elwert, 2011) and high school attainment (Aaronson, 1998; Harding, 2003; Wodtke et al., 2011, 2016). Furthermore, others demonstrate a link between neighborhood disadvantage and critical health outcomes such as youth obesity (Alvarado, 2016b) and teenage childbearing (Wodtke, 2013). Collectively, these findings suggest that neighborhood disadvantage may handicap the cognitive, educational, and health status of youth as they venture into adulthood. Still, despite a well-established theoretical conceptualization about the connection between childhood neighborhoods and adult economic outcomes, empirical tests of it are scarce.

Timing of exposure is another understudied topic in the area of childhood neighborhood effects on adult economic outcomes. As children age, their sense of an autonomous self grows in tandem with their physical, emotional, and social independence from their parents. The increase in autonomy from parents that is embedded in the process of transitioning from elementary school to middle school, for example, may therefore affect how neighborhood effects change over time (Aber et al., 1997). Scholars have often relied on such transition points that match well with key stages of the early life-course, such as educational time frames, to study heterogeneity in outcomes by developmental stage (Ellen and Turner, 1997; Pallas, 2000). Scholars studying heterogeneous neighborhood effects by age define childhood as approximately ages 10 and younger and adolescence as approximately 11 and older and use this cutoff as the approximate age when the gradual shift away from the family as the center of influence and toward neighborhood peers and institutions begins for many children (Aber et al., 1997; Vartanian and Buck, 2005). Following these scholars, I categorize the stages of youth as childhood (ages 0–10) and adolescence (ages 11–18) in assessing heterogeneous neighborhood effects by developmental stage.

Furthermore, rarely has previous research studied childhood neighborhood effects on adult economic outcomes with a focus on sustained exposure, despite the fact that many of the theories of neighborhood effects (e.g., social isolation, social disorganization, resource, or environmental) emphasize the compounding effects of consistent exposure to distressed neighborhoods over time (Jencks and Mayer, 1990). Therefore, this study focuses on four leading theories for how and when youth neighborhoods exert their effects on adult outcomes: (1) sensitive years; (2) teen socialization; (3) duration effects; and (4) cumulative effects. I now turn to a discussion of these four theories.

2.1. Sensitive years

The first theory, which I label sensitive years, revolves around the idea that there is an early period in development when children are most affected by ecological context which can have lasting effects on their well-being (Heckman, 2006; Wheaton and Clarke, 2003). In this model, children are able to absorb their neighborhood conditions directly through engagement with school-age peers or indirectly through their parents' experiences, for example. Also, the idea of neural plasticity suggests that the young child's mind is particularly malleable and susceptible to environmental inputs (Huttenlocher, 2002), suggesting that young children may be especially vulnerable to neighborhood disadvantage.

However, there is not much research on the topic of whether childhood neighborhoods affect adult economic outcomes.[1] The

[1] Mixed evidence exists for adult neighborhood effects on adult economic outcomes. For instance, some research suggests positive adult neighborhood effects on employment and earnings (Casciano and Massey, 2012) while still other research has found null adult neighborhood impacts on short term economic outcomes (Katz et al., 2001).

strongest evidence guiding what we know about the impact of childhood neighborhood conditions on adult income comes from two studies (Chetty et al., 2016; Vartanian and Buck, 2005), both of which stress the salience of exposure in early childhood. First, Vartanian and Buck (2005) find that very early childhood neighborhood SES impacts income-to-needs ratios in adulthood (i.e, age 25 and older). Their analysis makes use of national data from the Panel Study of Income Dynamics (PSID) and sibling FE models for a group of respondents who were born before and up to 1976 and estimates impacts on the natural log of the respondents' average family income-to-needs ratio at age 25 and beyond. Further, their study examines fine age categories (e.g., 0–4, 5–8, 9–13, and 14–18) and finds the strongest neighborhood effects for those exposed at ages 0–4. In contrast, the current study uses NLSY:CYA data to examine the natural log of respondents' average household income at any age beyond 18 and uses wider age categories (i.e., 0–10 and 11–18) to examine heterogeneous effects by age of exposure. Furthermore, the variables that Vartanian and Buck (2005) use to create their neighborhood quality indices focus on the income profile of the neighborhood and do not account for factors such as unemployment, education levels, housing values, or the occupational makeup of neighborhood residents – all of which are included as components of neighborhood disadvantage in the current paper and arguably represent a more complete picture of the experience of living in disadvantaged neighborhoods.

Second, analyses that have taken advantage of randomly assigned housing vouchers have shed light on neighborhood effects on both income and employment. For example, Chetty et al. (2016) use income tax records for Moving to Opportunity (MTO) compliers to find a positive link between moving to a less-poor neighborhood in childhood and adult income. Their analysis focuses solely on moving into a less-poor neighborhood and also does not include many features of the neighborhood ecology. Their finding that moving to less poor neighborhoods increases adult income are primarily driven by children whose families moved out of high poverty neighborhoods before they were 13 years old. They argue that this effect is likely due to the accumulation of advantages associated with living in a nonpoor neighborhood for a significant number of years in childhood.[2] Although these scholars imply that duration effects or cumulative effects may explain their findings, they do not directly assess the role of sustained exposure.

In a study of the effect of randomly assigned housing vouchers on employment for low-income Latino and black public housing residents in Denver, CO, Galster and Santiago (2017) find a statistically significant positive impact for increasing exposure to neighbors of higher occupational prestige. Moreover, they find stronger occupational prestige neighborhood effects on adult employment if respondents moved before age 13. This study analyzes individual neighborhood characteristics, rather than a composite measure of disadvantage, which allows the authors to pinpoint occupational prestige as a salient neighborhood indicator. Yet, it is likely that their analysis understates the totality of how residents experience living in a disadvantaged neighborhood by separately examining the effects of individual neighborhood characteristics. Moreover, the reliability of the data for employment could be in question because the data was not collected directly from the respondents. Instead, caregivers acted as proxies and were asked retrospective questions about their children's employment history since they turned 18 during a single cross-sectional telephone survey in 2006–2008. Moreover, like Chetty et al. (2016), Galster and Santiago (2017) imply that duration or cumulative effects are at work, but they do not directly examine sustained exposure effects that span childhood and adulthood.

These previous studies have used disparate and specific neighborhood predictors such as income levels, occupational prestige, or simply the act of being offered a voucher to move to a less-poor neighborhood. In contrast, the current study uses a much more comprehensive composite scale that measures the impact of underlying neighborhood disadvantage rather than a specific trait or feature of a neighborhood's makeup. Moreover, the samples are largely different given that both Galster and Santiago (2017) and Chetty et al. (2016) rely on data from poor and largely Latino and black residents of mainly inner-city housing projects. While Vartanian and Buck (2005) use a national sample, the current study uses data from a more recent cohort of Americans and does not primarily examine differences in age of exposure in as fine of categories.

2.2. Teen socialization

The second theory, which I label teen socialization, posits that as children grow older, their interaction with neighborhood peers, role models, and institutional actors increases and that this should result in stronger neighborhood effects. Various scholars have elaborated on this idea (Aber et al., 1997; Ellen and Turner, 1997; Sharkey and Faber, 2014). For example, Ellen and Turner (1997) provide a rich and intuitive account as to why we might expect neighborhood effects to vary by developmental age. In essence, younger children may be less sensitive to neighborhood conditions because they are more shielded from the normative cultural values and behaviors of the residential ecology and also because they interact much less with neighborhood institutions compared to older children. In contrast, older adolescents may engage more often with institutions such as schools and police and may also become more attuned to peer-influence in the neighborhood, thereby increasing their sensitivity to ecological norms and values. Although there is relatively little evidence to support the notion that teen socialization explains how youth neighborhood conditions may impact adult economic outcomes, teen socialization may in fact explain how neighborhood conditions impact cognitive development (Alvarado, 2016a), youth obesity (Alvarado, 2016b), teen childbearing (Wodtke, 2013), and high school attainment (Wodtke et al., 2016).

[2] While partially addressing the critical methodological challenges associated with neighborhood selection (Durlauf, 2004; Hauser, 1970), the MTO findings for income in adulthood are constrained because the MTO sample is composed of only 4604 low-income families with children who resided in census tracts with at least 40% poverty rates in Baltimore, Boston, Chicago, Los Angeles, and New York from 1994 to 1998 (Sanbonmatsu et al., 2011). One must consider, however, that those compliers who moved to nonpoor neighborhoods before age 13 and stayed long enough to be included in the Chetty et al. (2016) study may have had unobserved individual- and/or family-level characteristics (e.g., parents' motivation and ambition for their children) that made them more likely to be successful later on in the labor market, for instance.

2.3. Duration of exposure and cumulative effects

The third and fourth theories, duration effects and cumulative effects, stress sustained exposure to socially-disconnected and resource-poor neighborhood contexts (DiPrete and Eirich, 2006). Children who consistently interact with social norms that eschew mainstream attitudes, engage in risky behaviors, and who are exposed to dilapidated housing, resource-poor schools, negative police encounters, and violence on a consistent basis in disadvantaged neighborhoods, may therefore experience an ecological scarring that follows them later in life (Blau and Duncan, 1967; Harding et al., 2010; Sharkey and Faber, 2014).

Empirically, there is growing evidence to demonstrate that sustained exposure to neighborhood disadvantage has negative consequences in adolescence (Alvarado, 2016a; Crowder and South, 2011; Sharkey and Elwert, 2011; Wodtke, 2013; Wodtke et al., 2011). However, no research to date has examined whether sustained neighborhood exposure can affect joblessness and income in adulthood.[3] Here, I define duration of exposure as the total length of exposure to neighborhood disadvantage no matter the respondent's age and I define cumulative exposure as the combination of exposure in both childhood and in adulthood. Relatedly, I examine how childhood exposure affects adult exposure.

2.4. Joblessness

Joblessness, in particular, is virtually absent from the neighborhood effects literature as a salient economic outcome despite its central role in the theoretical model put forth by Wilson (1987, 1996). This dearth of knowledge is important because joblessness not only reflects the human capital resources one brings to the labor market, but also because it can be an indication of economic failure that stays on one's record and can impact one's long-term economic prospects. Moreover, results for joblessness may differ from income because one may have an income without having a job through various legal and illegal activities (e.g., dividends, interest, and illicit activity). Joblessness, therefore, can be a more accurate indicator of one's long-term economic fortitude and also better represents a connection to the mainstream economy and social networks. As Wilson (1996, p. xiii) argued, joblessness can have spillover effects that not only adversely affects "individuals, families, and neighborhoods, but the social life of the city at large as well." Neighborhoods where people are poor *and* jobless can create an ecology of malaise where crime, family dissolution, welfare, and low levels of social organization are normative and can seep into the culture of the community.[4] It is the combination of neighborhood strife coupled with joblessness that lies at the heart of Wilson's argument regarding the social, economic, and cultural isolation of disadvantaged communities. While educational achievement, teenage pregnancy, and high school dropout, and even income are important symptoms of social disorganization in their own right, they are not as central as joblessness in explaining the reproduction of poverty in Wilson's model. Without knowing whether or not neighborhoods impact joblessness, we simply cannot fully understand how concentrated disadvantage reproduces inequality.

2.5. The current study

This study contributes to the neighborhood effects literature in six ways: (1) it examines the impact of childhood neighborhood disadvantage on adult economic outcomes between ages 19 and 41; (2) it uses 26 years of longitudinal data at the national level; (3) it partially addresses unobserved confounding that operates at the level of the immediate family by using sibling FE models; (4) it incorporates the NLSY:CYA that adds new and more comprehensive variables; (5) it analyzes joblessness as a new outcome using national data; and (6) it examines duration and cumulative neighborhood effects on adult economic outcomes.

The basic research questions that this paper interrogates are:

(1) Does youth neighborhood disadvantage impact joblessness across adulthood?
(2) Does youth neighborhood disadvantage impact income across adulthood?
(3) If so, does developmental stage of exposure moderate these impacts?
(4) Are there duration effects and cumulative effects?

Based on previous literature I expect youth neighborhood disadvantage to increase joblessness and decrease income in adulthood. I also expect the age of exposure to neighborhood disadvantage in childhood to moderate long-term neighborhood effects, favoring childhood exposure. Lastly, I expect that neighborhood influence on adult economic outcomes can flow through duration effects and cumulative effects.

3. Methods

3.1. Data

I use 14 waves (1986–2012) of restricted tract-level data on two cohorts of respondents from the National Longitudinal Survey of Youth: the 1979 cohort of mothers (NLSY79) and their children (NLSY:CYA). The NLSY79 is an ongoing nationally representative

[3] However, see (Clampet-Lundquist and Massey, 2008) for a duration-weighted analysis of MTO data on adult economic self-sufficiency.
[4] Although, see Harding (2007) for a discussion of the heterogeneity embedded within disadvantaged neighborhoods.

multi-stage clustered sample of 12,686 young men and women who were 14–22 years old when they were first surveyed in 1979. The NLSY79 survey includes information on educational attainment, labor force behavior, income, health conditions, and marital and fertility histories in each wave. Moreover, the Bureau of Labor Statistics (BLS) has granted me access to the restricted Census tract identifiers of the NLSY79 respondents. The BLS requires a federal clearance to access these data and I must complete all analyses on-site in Washington, DC. I dropped male respondents from the NLSY79 because only female respondents who have ever had children are linked to the NLSY:CYA cohort. Despite dropping NLSY79 males, family background measures remain mostly intact because I include key household information that captures contributions from both parents.

The NLSY:CYA is composed of all children born to the female respondents from the NLSY79 and has essentially gone untouched by neighborhood effects researchers [for rare exceptions see Chase-Lansdale and Gordon (1996), Chase-Lansdale et al. (1997), and Alvarado (2016a, 2016b)].[5] Following developmental theory, I label youth as "children" when they are ages 0–10 and I label them as "adolescents" when they are ages 11–18.

The NLSY:CYA represents youth born to a nationally representative sample of women aged 21–28 on January 1, 1986. The BLS started collecting information biennially for all youth that were born (or would be born) to female NLSY79 respondents beginning in 1986 (Center for Human Resource Research 2009). That is, the BLS collected retroactive data for all youth who had been born prior to 1986 and collected data proactively for those born after 1986 so that data is available for every youth ever born to a female NLSY79 respondent from their birth through 2012. Administrators for the NLSY have improved precision and minimized measurement error over the years by cross-referencing survey responses using the longitudinal framework of the data. However, some respondent data are not available across the 14 waves because some children were either not living with the mother, the child was deceased, or the child simply did not answer the survey. Across all of the variables in all 14 waves, the variable with the most missingness was adult income for NLSY:CYA respondents in 2010 (17.05%). Following standard practice in the literature, I analyzed data after I imputed to correct for missingness using chained equations imputation procedures in Stata (Allison, 2002; von Hippel, 2007; Royston, 2005).

These data are unique in that they provide information on sibling pairs within families and cousins within extended families. That is, the BLS originally sampled siblings within the same household in the NLSY79 cohort. Therefore, all of the youth of any sister pairs from the NLSY79 are cousins and are captured in the NLSY:CYA. These longitudinal data provide key indicators on youth's cognitive, socioemotional, and physical development that when tied to the restricted tract-level identifiers belonging to the NLSY79 respondents provide rich data at three levels: the family (including the extended family), the youth, and the neighborhood. Most importantly, these data facilitate a dynamic long-term analysis of neighborhood effects across the life-course. One potential shortcoming of the data is the fact that some youth were born to young and socioeconomically disadvantaged mothers, especially in the early waves. However, in subsequent waves, the sample of mothers becomes increasingly representative of mothers across the full range of childbearing years (e.g., by 2012 over 95 percent of childbearing years have been covered by NLSY79 mothers). That is, the over-representation of young low-SES mothers decreases with each wave of the NLSY:CYA and any associated bias in estimates should diminish as well. Nevertheless, all models control for mother's age at child's birth.

By 2012, a total of 11,512 youth had been born to NLSY79 mothers, the oldest youth being 41 years old. In the first 1986 round, 2922 mothers reported having 5255 youth whose ages ranged from 0 to 23. In 1994, 3464 mothers reported having 6109 children under age 15 and 980 young adults older than 15. In 2012, there were 3190 mothers interviewed, 515 children under age 15 interviewed, and 5808 young adults interviewed (i.e., 6323 children were interviewed in 2012). As such, the size of the NLSY:CYA sample increases over time and depends on the number of youth born to female NLSY79 respondents. The number of interviewed youth in a given wave also depends on any wave-specific non-interviews. From 1986 to 2012 there had been 6106 white, 3191 black, and 2215 Latino (N = 11,512) youth who had been born to the NLSY79 mothers. Naturally, as subsequent waves have been collected, there has been a continuing shift in the age composition of the child sample from a predominately younger group to a more adult population over time. On average, 6788 children of NLSY79 mothers have been interviewed at each wave between 1986 and 2012. Thus, the NLSY:CYA is the best available data set to answer my research questions because it provides more extensive family demographic, developmental, and economic outcome data than the PSID or any other national longitudinal survey of youth in the U.S.

For this analysis I use data on youth of NLSY79 mothers between 1986 and 2012. Interviewed mothers in 2012 fell between the ages of 47 and 55, underscoring the fact that many NLSY79 women are reaching the end of their childbearing years. Also in 2012, 92 percent of the child sample is over the age of 15. The youngest members of the NLSY:CYA sample reside in middle class NLSY79 member households and were born to women at older ages, thereby minimizing any bias associated with young motherhood that was prevalent during the earliest NLSY79 waves. On average, NLSY79 women have had 2 children. Over 90% of the children and young adults interviewed in 2012 had been born to women age 20 and over. In fact, all children interviewed in 2012 were born to women over the age of 30 and only approximately 10% of interviewed young adults were born to adolescent mothers.

3.2. Neighborhood disadvantage

Although neighborhoods are difficult to conceptualize, measure, and analyze (Lee and Campbell, 1997), I followed previous neighborhood effects researchers by operationalizing neighborhoods using Census tracts (Brooks-Gunn et al., 1993; Vartanian and Buck, 2005; Wodtke et al., 2016). I used tract-level neighborhood data from the Geolytics Neighborhood

[5] Like the PSID Child Development Supplement before it, the NLSY:CYA is not nationally representative. Rather, the PSID CDS and the NLSY:CYA both represent the children of nationally representative samples of the U.S. population in 1968 and in 1979, respectively.

Change database to measure neighborhood disadvantage and I linked these data with NLSY data using the restricted NLSY79 geocode associated with mothers' residence location at each wave. Geolytics provides data on tract socioeconomic characteristics for 1980, 1990, and 2000 using the Census long form, normalized to 2000 tract boundaries. I also used the 5-year (2005–2009) American Communities Survey (ACS) data that contains these tract level socioeconomic indicators, normalized to 2000 tract boundaries. Following previous literature (Sampson et al., 2008; Sharkey and Elwert, 2011), I linearly-interpolated neighborhood characteristics to fill-in neighborhood data for every NLSY wave between 1986 and 2010 using four waves of Census data (1980, 1990, 2000, and 2009). I extended the interpolated trajectory between 2000 and 2009 to capture 2012 neighborhood conditions. The NLSY:CYA is ideally structured for a sibling FE model because in addition to multiple births per mother, siblings are often widely spaced in age (3.5 years apart on average), allowing for variation in neighborhood conditions between siblings over time. In addition, cousins provide additional variation and they extend the analysis to singleton children of NLSY79 mothers. That is, families with just one child are also included in the cousin FE analyses here.

I followed previous research and operationalized my main explanatory variable, neighborhood disadvantage, by first creating a scale so as to more accurately capture a multidimensional underlying contextual disadvantage (Harding, 2009; Shih et al., 2012). This scale is the mean of the following seven standardized variables measured at each wave when the child was between 0 and 18 and was presumably living with the mother: percent of residents at or below 100% of the poverty threshold as defined by the U.S. Census Bureau, the percent of residents who are jobless, the percent of residents out of the labor force, the percent who have at least a Bachelor's degree (reverse coded), the percent of managers and professionals in the neighborhood (reverse coded), median income (reverse coded), and the median housing value (reverse coded). The Cronbach's α was 0.88 for the neighborhood disadvantage scale. If a child was measured for 9 waves (i.e., once every two years) between age 0 and 18, then s/he would have 9 separate neighborhood disadvantage scale scores corresponding to the neighborhood conditions at each wave.

Second, I created a mean value for the continuous neighborhood disadvantage scale score for each respondent across the following three age ranges: (1) 0–18; (2) 0–10; and (3) 11–18. Each respondent then had a single mean neighborhood scale score value at each wave between ages 0 and 18, a single mean value at each wave between ages 0–10, and a single scale value between ages 11–18 that corresponded to the neighborhoods they lived in during those three separate age periods. Third, I assigned respondents to a neighborhood quintile based on the mean of their scale in each the three age ranges where the least disadvantaged neighborhood type is coded as 1 and the most disadvantaged neighborhood type is coded as 5.[6]

Finally, to create my dichotomous main predictor for neighborhood disadvantage, I coded respondents who had a neighborhood disadvantage quintile value of 4 or 5 as 1 and those who had a quintile value of 1, 2, or 3 as 0 in each of the three age ranges separately. That is, the models compare children who on average grew up in the top 40 percent of disadvantaged neighborhood contexts with those who on average grew up in the bottom 60 percent. I chose the 4th quintile as the cutoff because it provides sufficient sample size to run analyses and because it allows for focus on neighborhoods that can claim to be on the most disadvantaged side of average (i.e., appreciably above the 50th percentile of disadvantage). Additionally, this cutoff places less strain on the models to represent reality that could result from more extreme comparisons of siblings' neighborhood contexts (e.g., creating a dummy for quintile 5 vs. quintile1). That is, I am able to run models that, in addition to those rare and extreme pairings, compare siblings who have more proximate exposure to neighborhood disadvantage (e.g., quintile 4 vs. quintile 3). I also provide results for the continuous (and less severe) measure of neighborhood disadvantage using the mean of the scale scores (step 2 above).

3.3. Outcomes

I analyze the following outcomes in adulthood: (1) ever jobless and (2) mean net household income (logged). That is, I use measurements of joblessness and income taken from every wave in adulthood. The adult ages for NLSY CYA respondents spans ages 19 to 41 between 1986 and 2012. Joblessness refers to not currently working (and not in school or in the military). I follow previous research in this area and use household income instead of personal income (Chetty et al., 2016).

3.4. Individual-level covariates

I adjust for a diverse array of time-varying and time-invariant social and demographic control variables that BLS measured at each of the 14 waves spanning childhood and adulthood, many of which have never been included in models of neighborhood effects but may predict neighborhood selection in childhood and economic outcomes in adulthood. These new variables that the NLSY provides are mother's AFQT score, mother's age at childbirth, criminal justice contact, using hard drugs in adulthood (e.g., cocaine, crack, LSD), work limitations, poor health status, and neighborhood disadvantage in adulthood.[7] Scholars often criticize measures of

[6] These quintiles are relative to the NLSY:CYA sample.

[7] The AFQT is an intelligence exam that accounts for a share of inherited cognitive ability that can affect children's outcomes across the life-course, including educational and economic outcomes (Hauser, 2010). The AFQT represents respondents' scores calculated from the Armed Services Vocational Aptitude Battery (ASVAB) tests that NLSY79 respondents took in 1980 that was administered by BLS. The ASVAB measures knowledge and skill in general science, arithmetic reasoning, word knowledge, paragraph comprehension, numerical operations, coding speed, auto and shop information, mathematics knowledge, mechanical comprehension, and electronics information.

Table 1
Descriptive statistics for analytical sample of NLSY79 mothers & NLSY:CYA youth (N = 10,921).
Source: NLSY 1979–2012; NLSY CYA 1986–2012

	Mean	Standard deviation	Time varying
Mother & household characteristics			
In poverty	0.25	0.44	✓
Weeks unemployed	3.70	16.28	✓
Number of children in household	1.35	1.24	✓
Household income (logged 2012 dollars)	10.63	1.07	✓
Age at birth of child	24.97	5.94	✓
Single parent	0.46	0.50	✓
Parental education: less than high school degree	0.16	0.37	✓
Parental education: high school degree	0.45	0.50	✓
Parental education: some college	0.19	0.39	✓
Parental education: Bachelor's or higher	0.21	0.40	✓
Foreign born (0 = born in USA; 1 = not born in USA)	0.07	0.25	
Armed Forces Qualifying Test	39843.75	28959.46	
Child characteristics			
White	0.52	0.50	
Black	0.27	0.44	
Latino	0.15	0.36	
Female	0.49	0.50	
Ever been convicted of a crime	0.06	0.23	✓
Ever incarcerated	0.06	0.24	✓
Behavior Problems Index score	104.02	15.58	✓
Ever used hard drugs	0.07	0.25	✓
Ever had a work limitation	0.04	0.19	✓
Ever moved during childhood	0.43	0.14	
Ever in poor health	0.05	0.21	✓
Highest year of schooling	11.40	2.59	✓
Adult neighborhood disadvantage	0.75	0.43	✓
Neighborhood characteristics			
Neighborhood disadvantage (1 = most advantaged; 5 = most disadvantaged)			
Ages 0–18	3.12	1.18	
Ages 0 - 10	3.28	1.20	
Ages 11–18	2.93	1.20	
Outcomes			
Mean family income (logged 2012 dollars)	9.79	1.27	
Ever unemployed	0.37	0.49	

neighborhood conditions that use Census geographies. Fortunately, the BLS provides subjective assessments of the neighborhood characteristics that NLSY:CYA respondents lived in as adults that allow the respondent to define what s/he means by a "neighborhood" without the somewhat arbitrary definitions from the government. The adult neighborhood disadvantage scale is a composite of NLSY:CYA respondents' concern that the following conditions are a problem in the neighborhoods where they reside as adults in each wave: 1) people do not respect rules/laws; 2) crime and violence; 3) abandoned/run-down buildings; 4) parents do not supervise their children; 5) people do not care/keep to themselves; and 6) people cannot find jobs. Table 1 provides a full summary of means and standard deviations for all of the variables in this analysis.

All of the mother specific variables are limited to when the child was between 0 and 18 years of age. Lastly, in models that are estimating the effect of neighborhood disadvantage experienced during adolescence, I control for whether or not the respondent experienced neighborhood disadvantage in childhood.

The sibling FE model capitalizes on variation between subjects on these covariates. Therefore, I standardized the within-person variation across the 14 waves of the NLSY by calculating the means of continuous covariates (e.g., mother's household income) over time within a given respondent and by maximizing dichotomous variables within a given respondent to indicate whether s/he ever experienced being convicted of a crime in adulthood, for instance. I convert years of schooling to the highest year ever reported. In this way, the values of these variables are constant within persons but vary between siblings. All of the neighborhood conditions are limited to childhood waves.

3.5. Estimation strategy

The analysis proceeds in three steps: (1) bivariate pooled logistic and OLS models; (2) fully adjusted pooled logistic and OLS models; and (3) fully adjusted logistic and OLS FE models. The bivariate approach allows for an understanding of the total direct effects of childhood neighborhoods absent overcontrolling. However, it comes up short on strong inference. The strength of the logistic and OLS approaches is that they include all respondents, regardless of whether they had siblings, and they do not necessitate

variation between siblings in neighborhood conditions or the outcomes. They also provide a base of comparison that could indicate whether strenuous longitudinal data are required to estimate effects (as they are in the FE approach). The weakness of traditional OLS, however, lies in the inability to control for unobserved factors that remain stable across siblings that could affect the selection of neighborhoods and the outcomes and lead to biased estimates. Potential unobserved variables include parental ability, parental ambition, and parental expectations (Aaronson, 1998). Although no panacea for the issue of selection bias, the FE models improve upon the logistic and OLS models by addressing unobserved "hidden" confounding by using siblings as controls for unobserved family processes and events (Allison, 2009). However, one potential weakness is that children must have at least one sibling to be included in the sibling FE models, possibly introducing some selection bias because of cultural and class sorting. Fortunately, 60% of NLSY79 mothers with children have at least two children (28% have three or more and 10% have four or more). Not only are there many family units with multiple births but also there are many family units where two or more children are widely spaced in age (mean of 3.5 years apart), thus expanding the possibility for variation in neighborhood conditions between siblings. Specifically, the FE model capitalizes on variation in neighborhood conditions that result from 1) older and younger siblings experiencing different neighborhood conditions due to moving and 2) neighborhood conditions changing around separate siblings over time. In these data, 57 percent of the sample never moved and only experienced neighborhood change exogenously because their neighborhoods changed around them over time.

For ease of interpretation, I present the general form of the fixed-effect model using a continuous outcome (logged household adult income):

$$Y_{if} - Y_{*f} = (\alpha_f - \alpha_f) + \beta_1(FF_f - FF_{*f}) + \beta_2(FITV_{if} - FITV_{*f}) + \gamma(N_{if} - N_{*f}) + (\mu_{if} - \mu_{*f})$$

Where i denotes the individual youth and f denotes the family; FF is the vector of fixed family characteristics; $FITV$ is the vector of family and individual time varying characteristics; N is the neighborhood variable; μ is the error term; β_1, β_2, and γ are the coefficients for the invariant family characteristics, the family and individual time varying characteristics, and the neighborhood characteristics, respectively. Finally, α is the intercept. In the sibling FE model, the constant and the fixed family characteristics drop out of the equation and the term $(FITV_{if} - FITV_{*f})$ and the other subtraction of the $*f$ demonstrate that the models subtract the overall mean values for families from the values of the individual youth for both the independent and the dependent variables. Therefore, the FE model is estimated by regressing the differences in adult income over the age of 19 on the differences in the observed family, neighborhood, and control variables.

Fixed effects models have been an important empirical estimation strategy in studies of neighborhood effects in economics (see Aaronson, 1998; Plotnick and Hoffman, 1999; Vartanian and Buck, 2005; Vartanian and Houser, 2010), but have rarely been used in sociology (Alvarado, 2016a, 2016b). A weakness of the FE approach is that it cannot account for any unobserved characteristics that differ between siblings. These include ambition and ability. Further, FE models do not control for varying parental characteristics such as parents' psychological and emotional states, which could vary over time and impact siblings differently, thereby potentially influencing neighborhood choice and children's outcomes.

In a robustness check that addresses the weaknesses of only including families with at least two children, mandating variation in neighborhoods and outcomes between siblings, and the inability to control for unobserved confounders that vary between siblings, I run cousin FE models that compare the children from sisters to each another. Fortunately, the NLSY79 survey collected data from family members of the same household in 1979, and therefore followed all sisters and their children over time. Furthermore, the cousin FE models control for unobserved confounders at the level of the grandparent. That is, the cousin models account for any unobserved legacies of disadvantage (Sampson et al., 2008) that are passed from the parents of the NLSY79 respondents down to their grandchildren. These factors could potentially bias estimates from sibling FE models given that neighborhood context follows an intergenerational process of inheritance (Sharkey, 2008, 2013).

4. Results

4.1. Neighborhood impacts on adult joblessness and income

Based upon previous literature, I expected neighborhood disadvantage to have a positive impact on adult joblessness and a negative impact on adult income. I ran models separately for exposure to neighborhood disadvantage between ages 0–18, 0–10, and 11–18 and I conducted post hoc tests for differences in means (see equation (4) in Paternoster et al., 1998). Further, I reported all results in beta coefficients and I rounded sample sizes (in accordance with BLS guidelines for analyses of restricted NLSY data). I begin with unadjusted models to assess the total impact of childhood neighborhood disadvantage.

Table 2 displays the unadjusted impact of neighborhood disadvantage on the likelihood of experiencing joblessness in adulthood and on adult income when examining exposure between the ages of 0–18, 0–10, and 11–18. Neighborhood disadvantage increased the likelihood of joblessness in adulthood regardless of age. For example, experiencing neighborhood disadvantage between the ages of 0–18 increased in the odds of ever being jobless by 120 percent ($e^{0.79} = 2.20$). The effect for childhood exposure is a 97 percent increase in the odds ($e^{0.68} = 1.97$) and the effect for adolescent exposure is a 200 percent increase in the odds ($e^{1.10} = 3.00$). Moreover, the difference between childhood (0–10) and adolescent (11–18) exposure was statistically significant, suggesting that adolescent exposure may be stronger than childhood exposure. Table 2 also displays the unadjusted impact of neighborhood disadvantage on income during adulthood. Here, neighborhood disadvantage had a negative impact on income. However, the bivariate difference

Table 2
Unadjusted associations between childhood neighborhood disadvantage and adult joblessness & adult income (NLSY:CYA 1986–2012).

	Adult joblessness			Adult income		
	Exposure age			Exposure age		
	0–18	0–10	11–18	0–18	0–10	11–18
Neighborhood disadvantage	0.79***	0.68***	1.10***	−0.23***	−0.21***	−0.25***
	(0.07)	(0.07)	(0.09)	(0.01)	(0.02)	(0.02)
Constant	–	–	–	10.24***	10.24***	10.22***
	–	–	–	(0.01)	(0.01)	(0.01)
Observations	9420	9140	9420	9400	9130	9410

Robust standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

Table 3
Childhood neighborhood disadvantage and adult joblessness (NLSY:CYA 1986–2012).

	Observed adjustments				Observed adjustments & sibling fixed effects			
	Exposure age				Exposure age			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0–18	0–10	11–18	11–18	0–18	0–10	11–18	11–18
Neighborhood disadvantage	0.50***	0.42***	0.72***	0.58***	0.56***	0.41**	0.69***	0.59**
	(0.08)	(0.08)	(0.10)	(0.12)	(0.17)	(0.16)	(0.20)	(0.21)
Constant	3.55***	3.72***	3.46***	3.35***	–	–	–	–
	(0.76)	(0.76)	(0.76)	(0.77)	–	–	–	–
LR chi square	856.24	797.88	868.58	823.75	241.50	226.80	243.01	235.01
McFadden's R2	0.10	0.10	0.10	0.10	0.12	0.11	0.12	0.12
Observations	8610	8370	8610	8370	2840	2760	2840	2760

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's highest year of schooling, child's history with hard drugs, neighborhood disadvantage in adulthood, child's incarceration history, and poor health. Models 4 & 8 control for early childhood exposure to neighborhood disadvantage.
Coefficients are shown.
Robust bootstrapped standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

between ages of exposure was not statistically significant, suggesting no difference by age of exposure. Based on the unadjusted models, it appears that neighborhood disadvantage indeed impacted adult joblessness and income and that adolescent exposure was more salient than childhood exposure, but only for joblessness. Next, I turn to more rigorous models that attempt to explain these results by including adjustments for observed and unobserved potential confounders.

Table 3 summarizes coefficient results from logit models that estimated the impact of neighborhood disadvantage on adult joblessness after adding full covariate adjustments. Models 4 and 8 demonstrate associations for exposure between ages 11–18 after adjusting for childhood exposure.

The first four models included observed adjustments and suggest that (1) neighborhood disadvantage increased the likelihood of experiencing joblessness in adulthood, (2) the unadjusted impact of neighborhood disadvantage is partially explained by family background, child and adult development, and adult neighborhood conditions, and (3) that the timing of exposure to neighborhood disadvantage did not matter (i.e., no statistically significant difference in coefficients). In particular, comparing childhood exposure (Model 2) with adolescent exposure that adjusted for childhood exposure (Model 4) reveals statistically indistinguishable associations based on formula (4) in Paternoster et al. (1998). For example, the results suggest that exposure in childhood (Model 2) increased the odds of joblessness in adulthood by 52 percent ($e^{0.42} = 1.52$). Meanwhile, after controlling for childhood exposure (Model 4), the results suggest that exposure to neighborhood disadvantage in adolescence increased the odds of joblessness in adulthood by 79 percent ($e^{0.58} = 1.79$).

Next, I adjusted for family level unobserved confounders. The sibling FE models (Models 5–8) demonstrate similar results. That is, (1) neighborhood disadvantage throughout childhood and adolescence increased the likelihood of experiencing joblessness in adulthood and (2) the neighborhood impact is statistically equivalent between exposure during childhood and exposure during adolescence (i.e., no difference between Models 6 and 8). For example, the results suggest that exposure in childhood (Model 6) increased the odds of joblessness in adulthood by 51 percent ($e^{0.41} = 1.51$) and that exposure in adolescence (Model 8) increased the odds by 80 percent ($e^{0.59} = 1.80$). The differences in sample sizes between traditional and FE models suggest that many siblings may

Table 4
Childhood neighborhood disadvantage and adult income (NLSY:CYA 1986–2012).

	Observed adjustments				Observed adjustments & sibling fixed effects			
	Exposure age				Exposure age			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0–18	0–10	11–18	11–18	0–18	0–10	11–18	11–18
Neighborhood disadvantage	−0.09***	−0.08***	−0.11***	−0.08***	−0.10***	−0.08**	−0.10***	−0.08**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	8.89***	8.77***	8.90***	8.82***	11.00***	11.04***	11.02***	11.06***
	(0.21)	(0.21)	(0.21)	(0.21)	(0.67)	(0.72)	(0.67)	(0.72)
F	13.77	46.59	48.82	44.96	7.57	6.93	7.60	6.86
Rho	–	–	–	–	0.49	0.49	0.49	0.49
Observations	9400	8360	8600	8360	8600	8360	8600	8360

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's Behavioral Problem Index score, child's conviction history, child's highest year of schooling, child's history with hard drugs, child's work limitations due to health, neighborhood disadvantage in adulthood, and unemployment in adulthood. Models 4 & 8 control for early childhood exposure to neighborhood disadvantage.
Coefficients are shown.
Robust standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

have had similar experiences either with the types of neighborhoods they grew up in or with adult joblessness, and therefore the FE model only had a fraction of sample members from whom to extract variation in neighborhood conditions and adult joblessness to estimate coefficients.

Table 4 summarizes coefficient results from OLS models that estimate the impact of neighborhood disadvantage on logged adult household income after adjusting for observed covariates and after including sibling FE. The results suggest that observed potential confounders failed to explain away the unadjusted impact of neighborhood disadvantage on adult income. After adjusting for observed covariates (i.e., in Models 1–4), neighborhood disadvantage continued to have a negative impact on adult income. Furthermore, timing of exposure did not seem to matter according to post hoc tests of significance (Paternoster et al., 1998).

The sibling FE models rendered a similar picture. After adjusting for observed and unobserved confounders, the sibling FE models revealed that childhood and adolescent exposure to neighborhood disadvantage (Models 6 and 8) yielded statistically similar negative impacts on adult income. These results confirmed that neighborhood disadvantage had a negative impact on adult income, but that the impact was not moderated by age of exposure. The sample sizes between traditional and FE models were similar, however, when examining adult income. That is likely due to the fact that most siblings experienced differences in adult income, therefore allowing the FE models to include almost all respondents in the models. In reference to timing of exposure, neither sensitive years nor teen socialization appear to drive neighborhood effects on adult economic outcomes.

4.2. Sustained exposure to neighborhood disadvantage

In considering a temporal dimension to neighborhood effects, theory posits that in addition to sensitive years and teen socialization, duration effects and cumulative effects may explain when and how neighborhoods matter (DiPrete and Eirich, 2006; Harding et al., 2010; Sharkey and Faber, 2014). No study, however, has explicitly examined whether sustained exposure to youth neighborhood context affects adult economic outcomes.

Table 5 shows results for sibling FE models that estimate the effect of sustained neighborhood disadvantage on adult joblessness. The results suggest that duration of exposure to neighborhood disadvantage increased the odds of joblessness by 99 percent ($e^{0.69} = 1.99$). Similarly, being exposed to neighborhood disadvantage both in youth and in adulthood increased the odds of joblessness by 125 percent ($e^{0.81} = 2.25$). Table 6 shows results for sibling FE models that estimate the effect of sustained exposure to neighborhood disadvantage on adult income. The results suggest that duration of exposure decreased income in adulthood and that cumulative exposure had a marginally significant ($t = -1.80$) negative impact on income.

Moreover, I also found that youth (especially adolescent) exposure to neighborhood disadvantage increased the odds of adult exposure to neighborhood disadvantage (see Appendix A). Taken together, these results suggest that, (1) individuals are likely to remain in disadvantaged neighborhoods in adulthood if they grew up in similar neighborhoods during youth and (2) that the accumulation of time that one is exposed to neighborhood disadvantage negatively affects adult economic outcomes.

4.3. The legacy of disadvantage

Scholars have recently demonstrated that the rigid residential mobility structure of the U.S. has produced an inheritance of neighborhood disadvantage that may impact families over generations (Galster and Sharkey, 2017; Sampson, 2012; Sharkey, 2008,

Table 5
Sibling fixed effects results for duration of exposure and cumulative exposure to neighborhood disadvantage: Adult joblessness (NLSY:CYA 1986–2012).

	Observed adjustments & sibling fixed effects	
	(1)	(2)
	Duration of exposure	Cumulative exposure
Neighborhood disadvantage	0.69**	0.81*
	(0.24)	(0.39)
LR chi square	238.88	56.22
McFadden's R2	0.12	0.10
Observations	2840	840

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's highest year of schooling, child's history with hard drugs, neighborhood disadvantage in adulthood, child's incarceration history, and poor health.
Coefficients are shown.
Robust bootstrapped standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

Table 6
Sibling fixed effects results for duration of exposure and cumulative exposure to neighborhood disadvantage: Adult income (NLSY:CYA 1986–2012).

	Observed adjustments & sibling fixed effects	
	(1)	(2)
	Duration of exposure	Cumulative exposure
Neighborhood disadvantage	−0.13**	−0.13†
	(0.04)	(0.07)
Constant	11.06***	10.00***
	(0.67)	(1.32)
F	7.49	3.91
Observations	8600	4170

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's Behavioral Problem Index score, child's conviction history, child's highest year of schooling, child's history with hard drugs, child's work limitations due to health, neighborhood disadvantage in adulthood, and unemployment in adulthood.
Coefficients are shown.
Robust standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

2013; Sharkey and Elwert, 2011). The disadvantages associated with the home and neighborhood conditions shared by the mothers, many of whom are sisters (i.e., NLSY79 female respondents), when they were growing up are potentially passed down to connect grandparents (i.e., the parents of the NLSY:79 respondents) to grandchildren (i.e., the NLSY:CYA respondents). Therefore, addressing inequality that originates upstream in the family structure and is passed down from grandparents, to parents, and eventually to grandchildren is an additional contribution here. Furthermore, the sibling FE model cannot address unobserved characteristics that differ between siblings, is limited to families with more than one child, and relies on variation in neighborhood conditions and economic outcomes between siblings. The cousin FE model, by contrast, does away with each of these potential weaknesses of the sibling FE model while netting out the impact of inequality rooted among grandparents.

Appendix B demonstrates results from cousin FE models that rely on variation between children of NLSY79 sisters (i.e., cousins) and include all observed adjustments. The results demonstrate that indeed, growing up in a disadvantaged neighborhood positively impacted joblessness and negatively impacted income in adulthood, net of observed characteristics and unobserved conditions associated with disadvantage at the level of the extended family that are shared between cousins. Despite only reaching marginal statistical significance ($z = -1.70$), the coefficient for neighborhood disadvantage for adult income was in the expected negative direction. These findings reinforce the conclusion that neighborhood conditions in childhood may impact adult economic outcomes.

4.4. Continuous specification for neighborhood disadvantage

A fair question to ask at this point is whether an alternative, less severe, specification of neighborhood disadvantage may yield

different results. A continuous specification of neighborhood disadvantage, for example, would have the added benefit of including families where siblings' variation in neighborhood conditions was smaller than the variation that the current specification demands. Appendices C and D summarize the results for a continuous specification of neighborhood disadvantage.

Examining the sibling FE results in Appendix C yields a similar conclusion as the dichotomous specification in regards to adult joblessness in Table 3. Here, the continuous specification of neighborhood disadvantage yields positive impacts for exposure to neighborhood disadvantage on adult joblessness, regardless of age of exposure. Additionally, in accordance with the previous dichotomous specification, the continuous specification yielded statistically indistinguishable impacts for timing of exposure (Model 2 [0.63; SE = 0.22] and Model 4 [0.60; SE = 0.23]).

The sibling FE results in Appendix D for adult income demonstrate that the continuous specification of the neighborhood disadvantage variable also yielded a similar conclusion as the dichotomous specification in Table 4. Further, Appendix D also demonstrates that there is no statistical difference in the magnitude of the negative impact of neighborhood disadvantage between childhood (Model 2 [-0.15; SE = 0.04]) and adolescent (Model 4 [-0.09; SE = 0.04]) exposure.

4.5. Continuous specification of joblessness

To reduce any bias due to constrained variation in siblings' employment status, I analyzed neighborhood effects on the proportion of the adult survey years that the respondents experienced employment. Appendix E shows sibling FE results for the continuous specification of proportion of years of employment. As expected, and in accordance with the dichotomous specification of joblessness, the results of the continuous specification suggest that neighborhood disadvantage reduced employment. Moreover, also in accord with the dichotomous specification, there is no statistically significant difference between childhood (Model 2 [-0.04; SE = 0.01]) and adolescent (Model 4 [-0.06; SE = 0.02]) exposure.

5. Conclusion

This paper builds off and contributes to the literature on the long-term impacts of neighborhood disadvantage by analyzing if neighborhoods matter across the life-course for adult economic outcomes and if the timing of exposure matters. Using restricted national data for a contemporary sample of U.S. adults from the NLSY:CYA that is relatively new to the literature, this paper estimates childhood neighborhood effects on joblessness and income in adulthood at the national level and by examining whether effects operate through sensitive years in childhood, teen socialization, duration effects, or cumulative effects. Sibling fixed effects models suggest that childhood neighborhood disadvantage negatively impacts economic well-being in adulthood. The findings also justify Wilson's (Wilson, 1987, 1996) focus on joblessness as a key feature of the intergenerational transmission of poverty. Moreover, the findings suggest that the timing of exposure matters – primarily through sustained exposure – in explaining how and when neighborhood effects manifest across time.

Specifically, I find that (1) youth exposure to neighborhood disadvantage increases the odds of adult joblessness and decreases adult income (which answers my first two research questions in the affirmative); (2) age of exposure yield statistically equivalent effects (which answers my third research question in the negative); and (3) sustained exposure to neighborhood disadvantage throughout youth and into adulthood yields negative effects (which answers my fourth research question in the affirmative). The finding that youth neighborhood effects on adult SES operate through sustained exposure aligns with sociological explanations of cumulative disadvantage (Blau and Duncan, 1967; DiPrete and Eirich, 2006; Harding et al., 2010; Sharkey and Faber, 2014). This paper therefore directly addresses the theoretical concern about the impact that concentrated disadvantage has on economic outcomes across generations, specifically joblessness that Wilson (1987, 1996) emphasized but has rarely been empirically tested.

The first theory of neighborhood effects, sensitive years, suggests that early childhood is a more salient period of exposure than adolescence. Yet, the statistically equivalent effects by age of exposure do not conform to recent analyses on outcomes such as children's test scores (Sharkey et al., 2014), adult mental health (Wheaton and Clarke, 2003), adult employment (Galster and Santiago, 2017), and adult income (Chetty et al., 2016; Vartanian and Buck, 2005) – all of which support the theory of sensitive years. However, methodological differences may contribute to differences in findings for age of exposure between previous studies of adult economic outcomes and mine. For instance, Vartanian and Buck (2005) used narrower age categories and a narrow definition of neighborhood disadvantage that mostly represented income context, an older cohort of respondents, and income-to-needs ratio for respondents over age 25 as the outcome. Chetty et al. (2016) used samples of acutely poor residents in the handful of MTO cities and limited income to the late 20s (i.e., 20–28). Nevertheless, my findings align with the MTO finding that youth neighborhood context impacts adult earnings. Galster and Santiago (2017) used a sample of Latino and black housing project residents in Denver, used narrow definitions of neighborhood disadvantage, and measured employment through the retrospective recollection of proxy respondents. However, my national-level findings generally conform to recent findings from Denver on employment (Galster and Santiago, 2017).

The findings here do not lend support for the second theory, teen socialization, either. From a child development perspective, theory would expect that adolescent exposure would be most salient (Aber et al., 1997; Ellen and Turner, 1997). Perhaps a way to help explain the discrepancy between developmental theory and my findings is through the idea of neural plasticity and the malleability of the brain to environmental inputs in early childhood (Huttenlocher, 2002). That is, the anatomy, physiology, and function of the brain in early childhood may be especially favorable toward an increased capacity for absorbing neighborhood influences either directly or indirectly that cannot be reproduced to the same extent or with the same ease in adolescence or in adulthood (Shonkoff and Phillips, 2000). Indeed, the idea that early environmental inputs can have lasting consequences forms part of the

justification for many interventions such as Head Start and other early-childhood programs aimed at increasing educational and economic outcomes later in life (Heckman, 2006; Heckman and Masterov, 2007). In particular, neighborhood disadvantage may yield strong impacts in early childhood if children's expectations, routines, and behaviors become aligned at such a young age with the pervasive structural conditions of rampant joblessness in the manner that Wilson (1987) predicted. That is, the combination of spatial mismatch to mainstream work and a dearth of social networks embedded in mainstream economic activity may lead to a disconnect from mainstream routines and cultural capital in disadvantaged communities that imprint on children's brains very early on either by way of their parents, early schooling environments, neighborhood adult role models, or interaction with other institutions such as the criminal justice system.[8]

In contrast to the first two theories, the third and fourth theories, duration effects and cumulative effects, appear to facilitate youth neighborhood effects on adult economic outcomes. The findings here demonstrate that the length of exposure to disadvantaged neighborhoods negatively affects adult economic outcomes, youth exposure to disadvantaged neighborhoods positively predicts adult exposure, and the combination of youth and adult exposure negatively affects adult economic outcomes. These findings align with previous research that has examined youth sustained neighborhood effects on youth outcomes (Alvarado, 2016a; Crowder and South, 2011; Wodtke, 2013; Wodtke et al., 2011). Moreover, this study contributes the first analysis of the effect of sustained exposure to neighborhood disadvantage on adult economic outcomes, which Chetty et al. (2016) and Galster and Santiago (2017) assumed explained their findings (but did not directly examine). From a policy perspective, minimizing sustained exposure is likely to maximize economic success in adulthood.

The supplemental analyses for alternative continuous specifications of neighborhood disadvantage and of joblessness reaffirm the main results that youth disadvantage negatively affects adult economic outcomes. Further, the finding that age of exposure does not make a statistically meaningful difference in effects sizes is also present in these supplemental analyses. Regardless of the variation in the specification of the main variables, the results of this study consistently demonstrate that neighborhood disadvantage across youth has a positive impact on joblessness and a negative impact on income across adulthood.

Here, I use the most recent national data for adult respondents available. Further, my models for adolescent exposure control for childhood exposure in addition to controlling for other new variables such as mother's AFQT, adult neighborhood disadvantage, non-cognitive skills, criminal justice contact, drug use, among others. The sibling FE modeling framework is, of course, no panacea for the challenge of selection bias that affects all analyses of observational data and contextual effects (Durlauf, 2004; Hauser, 1970; Sampson et al., 2002). I do not claim to have established purely causal effects. The sibling FE approach cannot account for time varying unobserved confounders that differ between siblings and may create selection bias due to differences between families with single and multiple children. However, the sibling FE model does go beyond traditional regression in accounting for unobserved confounders that are time invariant within families. Therefore, I supplement my analyses with cousin FE models (Appendix B) to include youth with no siblings and to address unobserved confounders such as grandparents' neighborhood disadvantage. The results from the cousin FE align with those of the sibling FE models; youth neighborhood disadvantage increases joblessness and decreases income in adulthood.

This paper, while addressing some theoretical and methodological challenges to conceptualizing and estimating neighborhood impacts, is not without its limitations. One limitation is the time horizon of the outcomes. For example, income and earnings continue to accrue beyond age 41 and I therefore cannot account for neighborhood impacts on economic outcomes across the entire span of respondents' careers. Analyzing neighborhood impacts on economic outcomes later in life is currently inadvisable with the NLSY:CYA, however, given the sample selection of older respondents in the currently available waves. Future NLSY:CYA waves will allow for the more trustworthy analysis of neighborhood impacts on income beyond age 40 because these respondents will have been born to older and more middle-class mothers. Further, I am unable to assess the impact of schools. However, recent research finds that school context may have weaker effects compared to neighborhoods (Ainsworth, 2002; Card and Rothstein, 2007; Owens, 2010; Wodtke and Parbst, 2017).

There are myriad alternative approaches to studying the research questions I posed and much more research is necessary to address these questions more thoroughly. However, I believe that the results evince a more nuanced understanding of long-term neighborhood effects. Further, these findings confirm Wilson (1987, 1996) concern regarding the impact of concentrated disadvantage on economic outcomes, joblessness in particular, and how economic outcomes partially depend on the contours of the structural inequality that stratifies the residential landscape of the United States.

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[8] I should note, however, that the evidence is mixed with some recent analyses have shown that schools do not appear to have much explanatory power for educational outcomes once one accounts for neighborhoods and other unobserved factors (Lauen and Gaddis, 2013; Owens, 2010; Wodtke and Parbst, 2017) while still others do find school effects that are independent of neighborhoods (Carlson and Cowen, 2015).

Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.ssresearch.2017.10.004.

Appendix A. Sibling fixed effects results for childhood neighborhood disadvantage and adult neighborhood disadvantage (NLSY:CYA 1986–2012)

	Observed adjustments & sibling fixed effects		
	Exposure age		
	(1)	(2)	(3)
	0–18	0–10	11–18
Neighborhood disadvantage	0.30†	0.30†	0.20
	(0.16)	(0.16)	(0.17)
LR chi square	429.49	406.22	427.35
Observations	3200	3120	3200

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's highest year of schooling, child's history with hard drugs, neighborhood disadvantage in adulthood, child's incarceration history, and poor health.
Coefficients are shown.
Robust bootstrapped standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

Appendix B. Cousin fixed effects models for the impact of neighborhood disadvantage on adult joblessness and income (NLSY:CYA 1986–2012)

	Observed adjustments & cousin fixed effects	
	(1)	(2)
	Adult unemployment	Adult income
Neighborhood disadvantage	2.75***	−0.07†
	(0.77)	(0.04)
Constant	–	9.34***
	–	(0.37)
Observations	390	8640

Notes: All models are fully adjusted.
Coefficients are shown.
Robust bootstrapped standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

Appendix C. Neighborhood disadvantage (continuous) and adult joblessness (NLSY:CYA 1986–2012)

	Observed adjustments & sibling fixed effects			
	Exposure age			
	(1)	(2)	(3)	(4)
	0–18	0–10	11–18	11–18
Neighborhood disadvantage (continuous)	0.99***	0.63**	0.75***	0.60**
	(0.26)	(0.22)	(0.21)	(0.23)
LR chi square	245.05	228.56	243.81	235.64
McFadden's R2	0.12	0.12	0.12	0.12
Observations	2840	2760	2840	2760

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's highest year of schooling, child's history with hard drugs, neighborhood disadvantage in adulthood, child's incarceration history, and poor health. Model 4 controls for early childhood exposure to neighborhood disadvantage.
Coefficients are shown.
Robust bootstrapped standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

Appendix D. Neighborhood disadvantage (continuous) and adult income (NLSY:CYA 1986–2012)

	Observed adjustments & sibling fixed effects			
	Exposure age			
	(1)	(2)	(3)	(4)
	0–18	0–10	11–18	11–18
Neighborhood disadvantage (continuous)	−0.21***	−0.15***	−0.15***	−0.09*
	(0.04)	(0.04)	(0.04)	(0.04)
Constant	11.00***	11.05***	11.01***	11.06***
	(0.67)	(0.72)	(0.67)	(0.72)
F	8.46	7.46	8.04	7.30
Rho	0.48	0.49	0.49	0.49
Observations	8600	8360	8600	8360

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's Behavioral Problem Index score, child's conviction history, child's highest year of schooling, child's history with hard drugs, child's work limitations due to health, neighborhood disadvantage in adulthood, and unemployment in adulthood. Models 4 controls for early childhood exposure to neighborhood disadvantage.
Coefficients are shown.
Robust standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

Appendix E. Sibling fixed effects results for neighborhood disadvantage and the proportion of adult years of employment (NLSY:CYA 1986–2012)

	Observed adjustments & sibling fixed effects			
	Exposure age			
	(1)	(2)	(3)	(4)
	0–18	0–10	11–18	11–18
Neighborhood disadvantage	−0.05***	−0.04**	−0.06***	−0.06***
	(0.01)	(0.01)	(0.01)	(0.02)
Constant	0.78**	0.71***	0.79**	0.73*
	(0.32)	(0.35)	(0.32)	(0.35)
F	14.41	13.85	14.93	13.92
Rho	0.45	0.45	0.45	0.46
Observations	8610	8370	8610	8370

Notes: All models control for mother's poverty, mother' weeks unemployed, number of children in the household, mother's logged household income, single parent, mother's education, mother's foreign born status, mother's AFQT score, mother's age at child's birth, race/ethnicity, child's sex, whether or not the household moved since previous wave, child's highest year of schooling, child's history with hard drugs, neighborhood disadvantage in adulthood, child's incarceration history, and poor health. Model 4 controls for early childhood exposure to neighborhood disadvantage.
Coefficients are shown.
Robust bootstrapped standard errors (in parentheses) are corrected for clustering.
†p < 0.10; *p < 0.05; **p < 0.01; ***p < 0.001.

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