THE LEGACY OF DISADVANTAGE: MULTIGENERATIONAL NEIGHBORHOOD EFFECTS ON COGNITIVE ABILITY[1]

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Abstract

This study examines how the neighborhood environments experienced over multiple generations of a family influence children's cognitive ability. Building on recent research showing strong continuity in neighborhood environments across generations of family members, we argue for a revised perspective on "neighborhood effects" that considers the ways in which the neighborhood environment in one generation may have a lingering impact on the next generation. To specify such multigenerational effects is not simply a theoretical problem, but poses considerable methodological challenges. Instead of traditional regression techniques that may obscure multigenerational effects of neighborhood disadvantage, we utilize newly developed methods designed to generate unbiased treatment effects when treatments and confounders vary over time. The results confirm a powerful link between neighborhoods and cognitive ability that extends across generations. Being raised in a high-poverty neighborhood in one generation has a substantial negative effect on child cognitive ability in the next generation. A family's exposure to neighborhood poverty across two consecutive generations reduces child cognitive ability by more than half a standard deviation. A formal sensitivity analysis suggests that results are robust to unobserved selection bias.

Research on the relationship between neighborhoods and child development has frequently overlooked a crucial dimension of neighborhood stratification: that of *time*. Whereas much research on neighborhood effects implicitly treats the neighborhood environment as a static feature of a child's life and assumes that the neighborhood has instantaneous effects on children, a life course perspective on neighborhood inequality shifts attention toward continuity and change in the neighborhood environment over time and across generations, and considers the role that neighborhoods play in altering or structuring individuals' or families' trajectories.[2]

The significance of this shift in perspective is supported in recent research demonstrating the complex relationships between exposure to disadvantaged neighborhood environments and

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[2]A group of studies focuses on neighborhoods over the life course, including Quillian (2003); Briggs and Keys (2009), which examines spells of exposure to poor and non-poor neighborhoods over time; Kunz, Page, and Solon (2003); Jackson and Mare (2007), which examines the implications of measuring children's neighborhood characteristics over multiple years for neighborhood effects estimates; and Wodtke, Harding and Elwert (2010), which estimates neighborhood effects by tracking neighborhood deprivation annually from birth to age 17. A parallel literature has examined similar questions with regard to family income, including Duncan and Brooks-Gunn (1997), Wagmiller et al. (2006), and Wolfe et al. (1996).

child developmental outcomes, which suggests that the neighborhood may be most salient early in adolescence, and that the influence of the neighborhood environment may be lagged or cumulative (Wheaton and Clarke 2003). Most notably, a recent study of adolescent cognitive ability among youth in Chicago neighborhoods demonstrates that if children are raised in extremely disadvantaged neighborhood environments, the influence of their exposure to neighborhood disadvantage lingers even if they move on to a more diverse neighborhood (Sampson, Sharkey, and Raudenbush 2008).

But what if the child's caregivers were also raised in similarly disadvantaged environments? Is it possible that a parent's childhood neighborhood environment could have an influence that extends to the next generation? In this study we add to the recent line of research that has begun to incorporate time into the literature on neighborhood inequality, but we step further back in time than other studies and ask how the neighborhood environment, experienced over multiple generations of a family, influences children's cognitive ability.

Our focus on multigenerational disadvantage is motivated by recent research on the persistence of neighborhood economic status across generations, which demonstrates that neighborhood inequality that exists in one generation is commonly transmitted to the next. For instance, more than 70 percent of African-American children who grow up in the poorest quarter of American neighborhoods remain in the poorest quarter of neighborhoods as adults (Sharkey 2008). The persistence of neighborhood disadvantage across generations adds considerable complexity to the way researchers approach the relationship between neighborhoods and child development, as it forces one to consider direct and indirect pathways by which the neighborhood exposures in both the parent and child generations may influence children's trajectories. A child's own neighborhood may influence her cognitive ability through, for instance, the quality of her schooling experience or the influence of her peers. But there also may be pathways by which a parent's childhood neighborhood, experienced a generation earlier, continues to exert a lingering influence on her child's cognitive ability. It is plausible that the parent's childhood neighborhood may influence her own schooling experience, her experiences in the labor market, and even her mental health. All of these aspects of a parent's life may, in turn, influence the resources available to her for childrearing—including the quality of the home environment, the resources available for the child, and the neighborhood in which she raises her child.

Illuminating the relationships that link parents' and children's residential environments to children's cognitive outcomes is not only a theoretical problem, but also poses considerable methodological challenges. Virtually all previous observational studies of neighborhood effects use regression techniques (or some variant, such as propensity score matching), in which a set of family background measures are controlled. The same techniques are not appropriate for investigating multigenerational effects if these dimensions of family background are influenced by neighborhood conditions in the first generation. In essence, controlling for family background would block the indirect pathways by which first generation neighborhood characteristics may influence developmental outcomes a generation later, thus underestimating the importance of parents' neighborhoods for child outcomes.

The theoretical problem of multigenerational relationships thus becomes a methodological problem, one that arises in any scenario in which confounders are potentially endogenous to treatments experienced at an earlier time point—or in an earlier generation. Instead of conventional regression models, we draw on newly developed methods designed to generate unbiased treatment effects in such situations, under assumptions that we specify below. In a series of papers introducing marginal structural models and the method of inverse probability of treatment weighting, Robins and colleagues (Robins 1998; 1999a; Hernán,

Brumback, and Robins 2000; Robins, Hernán, and Brumback 2000) show that treatment effect bias can be addressed by fitting a model that weights each subject by the inverse of the predicted probability that the subject receives a given treatment at a given time point conditional on prior treatment history, and prior confounders (both time-varying and time-invariant). Here, we adapt this method for treatments received across generations in order to estimate multigenerational neighborhood effects on children's cognitive ability. We supplement the analysis with a novel type of formal sensitivity analysis (Robins 1999b; Brumback, Hernán, and Robins 2004) to test the robustness of our results to unobserved selection bias.

The focus on child cognitive ability is driven by the extensive literature linking this developmental measure with a wide range of adult outcomes, including educational attainment, adult economic status, and health (Auld and Sidhu 2005; Heckman 1995; Herrnstein and Murray 1994; Murnane and Levy 2006; Singh-Manoux et al. 2005). If the neighborhood environment influences early cognitive ability, this relationship may be a key to understanding social stratification across a number of domains.

Drawing on data from the Panel Study of Income Dynamics, our findings confirm a powerful link between neighborhoods and cognitive ability that extends across generations. We find that being raised in a high-poverty neighborhood in one generation has a substantial negative effect on child cognitive ability in the next generation. Multigenerational exposure to neighborhood poverty, compared to living in non-poor neighborhoods in each generation, is estimated to reduce children's cognitive ability by more than one half of a standard deviation.

THE LINK BETWEEN NEIGHBORHOODS AND COGNITIVE ABILITY

Previous literature

The evidence generated to date on the relationship between neighborhoods and cognitive ability comes from a group of observational studies and multiple residential mobility programs. Observational studies typically show significant associations between neighborhood socioeconomic composition and cognitive test scores, after controlling for various measures of family socio-economic status and demographic characteristics; although some studies find non-significant effects, and the strength of the relationship often is found to vary by age and to be substantively weak (Ainsworth 2002; Brooks-Gunn et al. 1993; Brooks-Gunn, Klebanov, and Duncan 1996; Caughy and O'Campo 2006; Chase-Lansdale and Gordon 1996; Chase-Lansdaleet al. 1997; Duncan, Brooks-Gunn, and Klebanov 1994; Duncan, Boisjoly, and Harris 2001; Klebanov et al. 1998; Kohen et al. 2002; Leventhal, Xue and Brooks-Gunn 2006; McCulloch and Joshi 2001; McCulloch 2006; Sampson et al. 2008).

One important problem with many of these studies is that they control for confounders that may be endogenous to neighborhood characteristics, such as family income or health. This approach has the effect of blocking the indirect pathways by which neighborhoods may influence developmental outcomes.[3] This problem becomes more severe when we consider pathways that extend across generations. Virtually all of the standard measures of family background that are controlled in regression analyses of "neighborhood effects" are potentially endogenous to neighborhood environments in the prior generation.

The primary alternative method of identifying neighborhood effects on cognitive ability is to exploit exogenous variation in families' neighborhoods arising from experimental or quasi-

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[3]The Sampson et al. (2008) study is an exception, finding strong lagged effects that persist years after children live in disadvantaged neighborhoods.

experimental residential mobility programs targeting low-income families, as in the Gautreaux program in Chicago (Rubinowitz and Rosenbaum 2000), the Moving to Opportunity (MTO) experiment (Goering and Feins 2003), and two recent natural experiments exploiting exogenous variation in neighborhood conditions due to the demolition of public housing (Jacob 2004) and the random assignment of public housing families to different neighborhoods (Ludwig et al. 2009). Because Gautreaux did not assess cognitive outcomes, we focus on results from the other three studies.

Results from these (quasi-) experimental studies are mixed. Analyses of the full MTO sample across five cities mostly do not detect statistically significant effects on cognitive outcomes (Sanbonmatsu et al. 2006), with the exception of a positive effect on the reading scores of African-Americans (Kling, Liebman, and Katz 2007). The limitations of the MTO experiment have been noted and debated in several recent articles (Clampet-Lundquist and Massey 2008; Ludwig et al. 2008; Sampson 2008; Sobel 2006). Jacob's (2004) study exploits exogenous variation in the timing of public housing demolitions in Chicago to estimate effects on standardized test scores, and does not find statistically significant effects among a sample composed primarily of African Americans. By contrast, Ludwig et al. (2009) exploits the random assignment of public housing recipients to the waitlist of Chicago's Housing Voucher program, and finds that changes in neighborhood conditions similar to those generated by MTO produce strong positive effects on standardized reading and math scores.

These studies provide the best evidence on how moves to new environments may impact children's cognitive ability. For the purposes of the current analysis, however, the designs of the various residential mobility studies and the treatments under study do not offer any information about the impact of long-term, or multigenerational, exposure to disadvantaged environments. For instance, MTO is designed to produce evidence on the effect of a point-in-time move to a new neighborhood, but is not designed to assess whether neighborhoods experienced at earlier points in time have a lingering influence on family members.

Our perspective considers the possibility that the neighborhood may affect an individual's educational attainment in one generation, in turn influencing the individual's occupational status and income as an adult, the quality of the home environment in which that individual raises his/her own child, and the developmental trajectory of that child. These indirect pathways are obscured in observational studies that control for a set of endogenous covariates such as education or the quality of the home environment, and they are impossible to assess with experimental data such as MTO. For this reason, interpreting estimates from MTO as "neighborhood effects" is valid only in a narrow sense that is inconsistent with a conception of the neighborhood as a long-term developmental context that offers unique resources, risks, opportunities and constraints that have the potential to alter the trajectories of families across generations.

Intergenerational pathways of influence

The theory underlying a multigenerational perspective argues that there are numerous possible pathways, observed and unobserved, by which the neighborhood environment in one generation may be linked with child cognitive ability in the next generation. In outlining this theory we emphasize that our goal in this analysis is not to produce evidence identifying the relative importance of each of these mechanisms. Elaborating the mechanisms underlying multigenerational effects is an important goal, but doing so introduces a number of methodological problems that would compromise the first order objective of the analysis, which is to test for the presence of multigenerational neighborhood effects and to identify the effect of exposure to neighborhood poverty over successive generations. This discussion

is designed to provide the theoretical basis for the study of multigenerational effects, but is not meant to set up testable hypotheses about specific mechanisms.

Evidence supporting a multigenerational perspective comes from two strands of research, the first assessing the relationship between neighborhoods and adult outcomes, the second assessing the effect of different aspects of the family environment on children's cognitive ability. The first strand of literature is enormous, and has been reviewed in several previous articles (Diez-Roux 2001; Ellen and Turner 2003; Jencks and Mayer 1990; MacIntyre and Ellaway 2003; Pickett and Pearl 2001; Sampson, Morenoff, and Gannon-Rowley 2002; Small and Newman 2001). However, few studies focus specifically on the relationship between childhood neighborhoods and adult outcomes, which is the most relevant relationship for the current analysis.

The bulk of evidence that has been generated from observational research indicates that neighborhood characteristics are associated in the expected direction with adult outcomes typically thought of as important dimensions of family background and social and economic status. For some outcomes, such as educational attainment, studies have shown strong impacts of neighborhood poverty that are robust to potential violations of the core assumptions underlying observational studies (Harding 2003). However, by and large the literature has not produced consistent evidence of strong neighborhood impacts on adult SES—some studies report null effects, the associations that are found are often weak, and most of this literature is subject to the standard critiques of research that attempts to make causal claims based on observational data (examples assessing neighborhood effects on measures of adult social and economic status include Aaronson 1997; Corcoran and Adams 1992; Corcoran et al. 1992; Datcher 1982; Page and Solon 2003; Plotnick and Hoffman 1999; Vartanian 1999; Vartanian and Buck 2005).

The literature from experimental and quasi-experimental residential mobility has produced similarly mixed results, and is subject to its own set of critiques and limitations. Research from the Gautreaux mobility program is most relevant to our analysis, as this program has followed children in original Gautreaux families and tracked their social outcomes as they move into adulthood. Much of the research from Gautreaux focuses attention on differences in outcomes among families that remained within the city and those that moved to the suburbs, and finds that children in Gautreaux families that moved to surburban neighborhoods had higher rates of high school completion, college attendance, and labor force participation in early adulthood (Kaufman and Rosenbaum 1992; Rubinowitz and Rosenbaum 2000).

The central problem with Gautreaux is that it was not a true experiment, and there is some evidence that variation in the neighborhood destinations of participants should not be considered exogenous (Votruba and Kling 2008). The Moving to Opportunity program is a carefully designed experiment, but has been running for a much shorter duration, and it is not yet possible to estimate the effects of a change in children's neighborhood environments on their outcomes in adulthood. So far, however, the results from MTO appear very different from Gautreaux. Several years after the program started, Kling et al. (2007) find that the effects of residential mobility on several children's outcomes appear to vary by gender, with girls showing positive effects across several developmental outcomes and boys showing null or negative effects.

While this literature has produced inconsistent results, the theory underlying the current analysis does not depend on evidence for any specific causal pathway between neighborhoods and any *single* outcome. Rather it rests on the assumption that the *sum* of potential pathways collectively may transmit appreciable disadvantage across generations. If

childhood neighborhoods affect any dimension of adult social or economic status, health, or family life, then disadvantages experienced during childhood in one generation may linger and affect cognitive ability in the next generation. The fact that several studies have found strong childhood neighborhood effects on specific aspects of adult attainments, such as educational attainment and mental health (Harding 2003; Kaufman and Rosenbaum 1992; Wheaton and Clarke 2003), lends credence to the hypothesis that neighborhood effects may extend across generations. The presence of neighborhood effects in studies examining various additional dimensions of adult life strengthens this hypothesis considerably, even if these studies produce inconsistent results.

For such indirect effects to exist, however, we must make the additional assumption that aspects of family background and the social environments in which children spend their childhoods have an influence on child cognitive development. This assumption taps into a longstanding debate on the malleability of cognitive ability (Heckman 1995; Herrnstein and Murray 1994; Jacoby and Glauberman 1995; Neisser et al. 1996). While there is little doubt that cognitive ability—whether conceived as intelligence, IQ, or simply performance on tests of cognitive skills—has a genetic component, there is also a virtual consensus that development is sensitive to the family, school, and social environment. Empirically, children's cognitive development has been linked with parents' education, alcohol use, mental health, social and economic status, parents' parenting practices and various aspects of the home environment (Guo and Harris 2000; Shonkoff and Phillips 2000). These same characteristics of parents also may affect the schooling experiences of children, which influence children's cognitive development (Alexander, Entwisle, and Olson 2007; Downey, von Hippel, and Broh 2004; Winship and Korenman 1997). Experimental evaluations of early interventions in the family and school environment provide further evidence in support of the claim that cognitive ability is malleable (Campbell and Ramey 1994; Campbell et al. 2002; Brooks-Gunn et al. 1994; Hill, Brooks-Gunn, and Waldfogel 2003; Gross, Spiker, and Haynes 1997; McCarton et al. 1997; Schweinhart and Weikart 1997; and Wasik, Bond, and Hindman 2006).

This review is not designed to be exhaustive, but rather to provide a sense of the multiple number of ways in which different aspects of the home or family environment may be linked with children's cognitive development. While it is not possible to observe all of these aspects of the child's environment in the PSID, the point is that they represent potential pathways linking parent's own childhood environments to their children's development, a generation later. The presence of numerous possible pathways, observable and unobservable, provides the theoretical basis for a multigenerational analysis.

DATA

To assess the multigenerational effects of neighborhood poverty on cognitive ability we draw on data from the Panel Study of Income Dynamics (PSID) (Hill and Morgan 1992). The PSID began with a nationally representative sample of roughly 5,000 families in 1968, and has followed the members of these families over time.[4] This feature of the data makes it possible to follow the trajectories of families across generations. We match families to their census tract of residence through the PSID restricted-use geocode file, which contains tract identifiers for sample families from 1968 through 2003.[5] Data on the economic composition of census tracts is obtained from the Neighborhood Change Database (NCDB) (GeoLytics

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[4]The original survey contained an oversample of low-income households, typically referred to as the Survey of Economic Opportunity component of the sample. See Brown (1996) for a discussion of the low-income oversample in the PSID. See Becketti et al. (1988) and Fitzgerald, Gottschalk and Moffit (1998a; 1998b) for analyses of attrition and representativeness.
[5]The geocode file does not include tract identifiers for survey year 1969.

2003) for Census years 1970, 1980, 1990 and 2000—tract characteristics in intercensal years are imputed using linear interpolation.

Finally, we utilize data on cognitive ability from the 2002 Child Development Supplement (CDS) (Hofferth et al. 1997; Mainieri 2004). The 2002 CDS is a follow-up survey of a sample of PSID parents with children age 5–18 who were originally assessed in the 1997 CDS at age 0–12. The CDS was designed to supplement the core PSID interview with information on child development and details about the home, school, and neighborhood environments, as well as familial and social relationships. We use data from the 2002 CDS in order to maximize the sample, as virtually all children were eligible for the cognitive ability assessments in 2002 (only 5 year-olds were not given the full verbal assessments). Because we are utilizing data covering multiple generations of a family, the file structure and the temporal sequence of the various measures used in the analysis are complex. In order to be included in our sample families must meet several criteria. First, children must be assessed in the 2002 CDS and have non-missing data on measures of cognitive ability. Eligibility for the 2002 CDS was based on eligibility for the original (1997) CDS, which was restricted to PSID sample families active in the survey who had children age 0–12 in 1997. The 1997 CDS sample comprised 3,563 children, and the 2002 CDS successfully re-contacted and interviewed 2,907 children (Mainieri 2004). Non-missing data from the cognitive assessments are available for 2,603 children.

Second, to measure treatment status for children, information on the census tract of residence must be available for children's families in at least one year among the three survey years prior to the 2002 CDS (survey years 1997, 1999, and 2001). Third, background characteristics from the child's family must be available in at least one year prior to the measurement of the treatment status—that is, prior to the 1997 survey. This information is used to predict selection into the treatment for children.6 Fourth, to measure the treatment in the parent's generation at least one parent must be observed, and information on the parent's census tract of residence must be available, during "childhood"; that is, in at least one year from the age of 15 to 17. Fifth, background characteristics from the parent's family must be available in at least one year prior to the age of 15. This information is used to predict selection into the treatment for parents.

The final sample comprises 1,556 parent-child pairs. Roughly 1,000 subjects with non-missing data from the cognitive assessments are not included in this sample because our sample selection criteria whittled down the number of cases with information in each generation. Virtually all of the lost cases are due to missing data in the parent's generation. Many cases are lost because parents have missing information on their childhood neighborhood characteristics, which is in part explained by the fact that not all U.S. areas had been assigned census tracts in the 1970s. Rural areas were less likely to be "tracted" in the 1970s, meaning our final sample is disproportionately urban, as is true for all studies of neighborhood effects using national data on census tracts from the 1980s or earlier. Among the final sample of 1,556 parent-child pairs, there are 730 African-American pairs, 792 white pairs, and 32 pairs of all other racial and ethnic groups. Our results are estimated among the children and grandchildren of the original PSID sample, which was a cross-

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6If the measures of parents' or children's family characteristics are missing, but the family meets all other criteria for selection into the sample, we use a regression imputation method developed by Royston (2004) to impute values for nonresponse. Treatment status and the outcome measures are not imputed in the main results. However, we report results using imputed values for the two dependent variables in Footnote 17. Several variables have extensive missing data primarily because they are based on questions that were only asked in early years of the PSID survey, or in the case of the *occupational status* measure because some household heads were unemployed for several years. To test whether these heavily imputed variables affect results we created an additional set of IPT weights that excluded variables in each generation with more than 10% of cases missing. Results (available upon request) were not sensitive to the exclusion of these variables.

section of the U.S. population in 1968. Therefore, by construction, our sample is not representative of the current U.S. population due to extensive immigration since the late 1960s.

Outcomes

The outcomes under study represent two dimensions of child and adolescent cognitive ability measured using the Woodcock-Johnson Psycho-Educational Battery-Revised (WJ-R) (Woodcock 1989): Broad Reading scores and Applied Problems scores. The Broad Reading score measures reading ability and combines results from two subscales, the Letter-Word assessment and the Passage Comprehension assessment. To measure ability in math, we use the Applied Problems score.[7] Raw results from each subtest are normalized to reflect the child's abilities relative to the national average for the child's age (Mainieri 2004). The Woodcock-Johnson assessments are well-established and are the same assessments used to measure cognitive ability in the Moving to Opportunity experiment (Leventhal and Brooks-Gunn 2004; Sonbonmatsu et al. 2006).

Treatment

The treatment is defined as living in a high-poverty neighborhood during childhood, and is measured for children in the three survey years prior to the CDS, and for their parents when they were age 15–17. Specifically, we define high-poverty neighborhoods as those where the poverty rate is at least 20%. While various cutoffs have been used to define high-poverty neighborhoods in the literature (e.g., see Harding 2003; Jargowsky 1997; and Quillian 1999), we chose this threshold because it allows for a pooled analysis of whites and African-Americans in the sample. Table 1 shows the percentage of whites and blacks in the treatment and control groups under various definitions of the treatment. Overall, 36 percent and 28 percent of respondents in the parent and child generation, respectively, grew up in neighborhoods where at least 20 percent of households were poor, and 20 percent of respondents came from families in which both generations lived in poor neighborhoods. Among African-American respondents, 70 percent in the parent generation, and 52 percent in the child generation lived in poor neighborhoods; 41 percent lived in poor neighborhoods in both generations. Among whites, 5 percent of the parent generation and 6 percent in the child generation lived in poor neighborhoods; only 2 percent of white respondents' families lived in poor neighborhoods in both generations. Using a more stringent standard for defining high-poverty neighborhoods (either 30% or 40% poverty) removes virtually all whites from the treatment group, whereas using a lower cutoff (10% poverty) includes almost all blacks in the treatment group.[8] For these reasons we settled on the 20% cutoff. However, mindful of severe racial discrepancies in exposure to high-poverty neighborhoods, we conduct an additional set of race-specific analyses in which the definition of the treatment is allowed to vary by race. In these analyses we use a 40% poverty threshold to define high-poverty neighborhoods for blacks, and a 10% threshold for whites.

Parental neighborhood poverty is measured as the average poverty rate in parents' census tracts of residence over the three survey years from age 15 to 17. Child neighborhood poverty is measured as the average poverty rate in children's neighborhoods in the three survey years prior to the 2002 CDS: survey years 1997, 1999, and 2001.[9] In each case we use a three-wave average in order to minimize measurement error in the treatment. While

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[7]A calculation assessment was administered in the 1997 CDS but was not administered in the 2002 CDS.
[8]Note that these proportions are unweighted and thus non-representative. Many African-Americans in the PSID were included in the low-income oversample, and thus the proportion of blacks in high-poverty neighborhoods in our sample is higher than the proportion nationally. We include Table 1 to document the sample sizes on which inferences are based.
[9]From 1968 through 1997, the PSID interviewed families on an annual basis, but since 1997 families have been interviewed every other year.

the two treatments were designed to capture neighborhood poverty during childhood in consecutive generations, there is considerable variation in the temporal gap between the measurement of parent and child treatment status because some parents have children at young ages and others have children much later in life. The average time elapsed between the two treatments is 16 years, with a standard deviation of 7 years.[10]

Control variables

We construct a wide range of measures to model selection into treatment status for parents and again for children. Because the content of the PSID survey instrument changed slightly over the course of the survey, the measures available differ somewhat across generations.

In the parent generation, the measures refer to characteristics of parents' families during the parent's childhood but prior to the measurement of treatment status. That is, all measures represent family characteristics averaged, or aggregated, over the years when the parent is age 1 to 14. Except where noted, measures refer to characteristics of the family unit as a whole (e.g., family income) or of the individual identified as the head of the household, or the "grandparent" (e.g., years of schooling).

Disability is a dichotomous measure and indicates the presence of a self-reported disability that ever limits the amount or type of work held by the household head in the family. *Welfare receipt* is a dichotomous indicator for whether the household head, or his/her spouse, ever reports receiving any income from programs typically referred to as "welfare," including ADC, AFDC, or TANF. *Vocabulary score* represents the number of questions answered correctly by the household head in a simple 13 item vocabulary assessment conducted in only a single year of the survey (1972). Because it was asked in a single year, the measure is missing for a large portion of grandparents (see footnote 5). We include it because it is the only assessment of grandparents' cognitive ability available and because of its obvious relevance to the subject matter.

Family income measures total income from all family members, and is adjusted to represent year 2000 dollars. *Occupational status* is the average status of the main jobs held by the household head. Based on research showing that measures of average educational attainment within occupational groups more effectively capture stratification in the occupational structure than composite measures such as the socio-economic index (Hauser and Warren 1997), we use a transformation of the percentage of individuals in an occupational grouping with at least one year of college education as our measure of occupational status. This measure is missing in years in which the household head was not working.[11] *Educational attainment* is measured as the household head's total years of schooling. *Annual hours worked* represents the estimated hours worked by the household head on all jobs in the year prior to the interview, and is coded as missing in years in which the individual is not working. *Home ownership* represents the proportion of years during the parent's childhood in which the family owned their home. *Ever married* is a dichotomous measure indicting if the individual reports being married at any time over the observation period. We also include measures for the gender and age of the household head (age is centered around 40) and the number of children in the child's family.

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[10]For 8 cases the measurement period for the parent and child treatments overlap; eliminating these cases from the analyses has no effect on the results. We also conducted the analysis after eliminating all parents who gave birth during the measurement of the treatment (i.e. younger than age 18), with no change in results.
[11]We also tested the core models with a more traditional measure of the socio-economic index (Stevens and Featherman 1981), and found that the results are not sensitive to the measure used.

Lastly, we include three additional measures that tap into the household head's outlook and/ or attitude toward the future, all of which are based on survey items asked only from 1968 through 1972. Parental *efficacy* measures household head's "sense of personal effectiveness, and a propensity to expect one's plans to work out" (Morgan et al. 1974, p. 417). Duncan and Liker (1983) demonstrate that the measure of efficacy is significantly associated with individual earnings, providing evidence of construct validity. *Aspiration/ambition* measures heads' "attitudes and attempts to improve economic well-being" (Morgan et al. 1974, p. 415), and consists of several items describing respondents' expressed desire to advance in economic status. The *horizon index* measures heads' self-reported behavior "indicating a propensity to plan ahead" (Morgan et al. 1974, p. 419). This measure includes items measuring the respondent's ideas about his/her own employment, savings, and family plans, but also his/her plans for children's education. All three "attitude" measures have been found to be associated with neighborhood attainment in previous research (Sharkey 2008).

In the child generation, control measures refer to average characteristics of the child's parents or families over the years when the parent is at least 18 years old and is a household head or the spouse of a household head, but prior to the measurement of the child's treatment status—that is, prior to survey year 1997. Several measures are constructed in the same way as in the first generation, including: *disability, family income, welfare receipt, occupational status, home ownership*, and *annual hours worked*. Other measures are constructed analogously but represent characteristics of the child's PSID sample parent, regardless of whether this parent is the household head.[12] These include: *educational attainment*, the measure of marital status (*ever married*), the age of the parent and the gender of the parent. We also include measures of the child's age in 2001 and the child's gender.

Due to changes in the survey, measures of *vocabulary score, efficacy, aspirations/ambition*, and *time horizons* are not available as control variables in the child generation. However, a dichotomous indicator for whether the parent is the *first child* in his/her family is included, along with a measure of the household head's self-reported *health*. This measure represents the household head's response to the question: "Would you say your health is excellent, very good, good, fair, or poor?" The corresponding scale ranges from 1 to 5, with lower values reflecting better self-reported health.

METHODS

This study uses an expanded counterfactual framework of causality to conceptualize and estimate causal effects from time-varying multigenerational treatments, employing marginal structural models and a new method of sensitivity analysis for selection bias.

Defining multigenerational effects of neighborhood disadvantage

The primary objective of our study is estimating the joint, multigenerational effect of parent and child exposure to neighborhood poverty on child cognitive ability in order to capture the effect of enduring disadvantage. Formally, let A_i be the observed cognitive ability of child i, and define the potential outcome A_i^N as the cognitive ability that would be observed if i's family had experienced the multigenerational neighborhood regime $N=\{N_P, N_O\}$ consisting of the ordered pair of consecutive neighborhood environments in the parent generation, N_P, and child generation, N_O. The contrast of potential outcomes $\partial_i = A_i^N - A_i^{N'}$ defines i's individual level causal effect of experiencing the multigenerational residential history N rather than some other residential history N'. Averaging across all i gives the average causal

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[12]Only one of a child's caregivers is an original sample member—if there is another caregiver in the household he/she has joined the sample after becoming the spouse/partner of an original sample member.

effect. In each generation, we classify neighborhoods as either poor or non-poor, yielding four possible multigenerational regimes, $N \in \{(poor, poor), (poor, non-poor), (non-poor, poor), (non-poor, non-poor)\}$ and six pairwise multigenerational causal contrasts between regimes. The methods introduced below can estimate all six contrasts, although we primarily focus on two: (1) $E[A_i^{N=\{poor, poor\}}] - E[A_i^{N=\{nonpoor, non-poor\}}]$, which defines the average causal effect if both parents and children had grown up in poor rather than non-poor neighborhoods, and which we term the *joint, or multigenerational causal effect*; and (2) $E[A_i^{N=\{poor, fix\}}] - E[A_i^{N=\{non-poor, fix\}}]$, which defines the average causal effect if parents had grown up in a poor rather than a non-poor neighborhood and children had grown up in a neighborhood of fixed type (either poor or non-poor), which we term the *direct causal effect* of parents' exposure on child cognitive ability. We note that this direct effect captures the portion of parental exposure that does not operate through influencing children's own neighborhood of residence but may operate via parents' education, income, parenting style, or other characteristics of the parents or the home environment that is influenced by parents' childhood neighborhood context.

Multigenerational effects differ from intergenerational effects. Intergenerational neighborhood effects capture the overall effect of parental neighborhood conditions on child's cognitive ability regardless of the pathway of influence, $E[A_i^{N_p=poor}] - E[A_i^{N_p=non-poor}]$. Multigenerational effects, by contrast, capture the effect of placing both parents and children in particular neighborhood environments, $E[A_i^{N=\{poor, poor\}}] - E[A_i^{N=\{non-poor, non-poor\}}]$. The distinction between intergenerational and multigenerational effects explains why multigenerational neighborhood effects cannot be estimated as the sum of parents' intergenerational neighborhood effect and children's own neighborhood main effect—that is, why, in general, $E[A_i^{N=\{poor, poor\}}] - E[A_i^{N=\{non-poor, non-poor\}}] \neq \{E[A_i^{N_p=poor}] - E[A_i^{N_p=non-poor}]\} + \{E[A_i^{N_o=poor}] - E[A_i^{N_o=non-poor}]\}$. One may be tempted, for example, first to estimate a regression or propensity score model for the effect of parent's neighborhood on child outcomes (the intergenerational effect), and then to add to this estimate a separate regression or propensity score estimate for the effect of child's own neighborhood on child outcomes. Summing these two effects, however, would amount to inappropriately counting the effect of child's neighborhood twice: if the parent's neighborhood of origin influences where the child grows up, then part of parent's intergenerational effect will operate by activating child's own neighborhood effect (via predisposing the child to live in, say, a poor rather than in a non-poor neighborhood). Double-counting child's own neighborhood effect by summing two generation-specific main effects would overstate the strength of multigenerational neighborhood effects, and must hence be avoided.[13] A seemingly more promising strategy to resolve the problem of estimating multigenerational neighborhood effects would be first to estimate the direct effect of parent's neighborhood on child outcomes using conventional regression models while holding child's neighborhood constant and then adding this direct effect to a conventional estimate of the child's own neighborhood effect. Alternatively, one might propose to estimate a regression model for the joint effect of parent and child neighborhoods by regressing child outcomes on parent neighborhood and child neighborhood. Neither of these strategies will produce unbiased results, however, if there is confounding in each generation (VanderWeele 2009). We elaborate on this point in the next section, before introducing methods that allow us to estimate unbiased multigenerational effects.

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[13]More precisely, summing the two main effects would overcount child's own neighborhood effect as weighted by the strength of the intergenerational inheritance of neighborhood disadvantage. Note, furthermore, that even if parents' neighborhood did not affect child outcomes by influencing the child's place of residence, summing two main effects would still yield a biased estimate if there were an interaction effect between child and parent neighborhoods.

Two endogeneity problems in the estimation of multigenerational causal effects

The causal effect of multigenerational neighborhood poverty on child's cognitive ability, A, can be identified from observational data if neighborhood of residence in each generation, N_P and N_O, is statistically independent of the potential outcomes, A^N, given observed covariates and previous treatments:

$$A^N \perp N_P \big| C_P \quad \text{and} \tag{1.a}$$

$$A^N \perp N_O \big| C_P, C_O, N_P, \tag{1.b}$$

where C_P and C_O refer to the observed covariates that influence the selection of parents' and children's neighborhood of residence, respectively, and the symbol $\perp$ denotes statistical independence. Conditions 1.a and 1.b encode the assumption of no unobserved confounders, collectively known as sequential ignorability or unconfoundedness of treatment assignment (Robins 1986; 1999b). Substantively, these assumptions state that individuals with the same combination of observed covariate values do not preferentially select into poor or non-poor neighborhoods. We relax this assumption below and test the robustness of our results to unobserved selection bias in a formal sensitivity analysis.

Even under conditions of sequential unconfoundedness, as previewed above, however, traditional regression (as well as conventional propensity score) methods are ill suited to recover the multigenerational effect of neighborhood disadvantage because neighborhood disadvantage in the second generation is endogenous to neighborhood disadvantage in the first generation in two distinct ways. Figure 1 illustrates these two endogeneity problems. The figure shows a directed acyclic graph (Pearl 1995, 2000) representing the causal relationships between neighborhood poverty, child cognitive outcomes, and other variables. All arrows between the temporally ordered (vectors of) variables represent direct causal effects, and the absence of an arrow indicates the absence of a direct causal effect. The variables in Figure 1 are defined as before, with the addition of U representing unmeasured variables causing both C_O and A. For expositional purposes, we assume that Figure 1 contains all variables in the system. We note that treatments in Figure 1 are sequentially ignorable in the sense of conditions 1.a and 1.b because there are no arrows from any unobserved variables into N_P or N_O. The potential outcomes thus are conditionally independent of treatment status in each generation given observed temporally prior variables, and the multigenerational effect of N on A is *identifiable* from the observed data.

Simply conditioning on C_P and C_O in a conventional regression model, however, will not provide an unbiased *estimate* for the multigenerational effect of N on A. To see why, consider how the confounding variables C_O should best be handled. On one hand, C_O must be controlled to avoid bias from confounding, as C_O causes both N_O and A. On the other hand, conditioning on C_O creates two endogeneity problems. First note that C_O is on the causal pathway from N_P to A. Controlling for C_O may thus "control away" part of the effect of parental neighborhood poverty on the child's cognitive outcome and consequently produce bias. Second, even if N_P had no direct or indirect causal effect on A (i.e. if there are no pathways to be "controlled away"), conditioning on C_O will induce a non-causal association between N_P and A if the unobserved variable U is present—conditioning on C_O will induce an association between N_P and U (as conditioning on the common effect of two variables invariably does [Pearl 1995]), and thus between N_P and A, thus inducing endogenous selection bias which would make it impossible to reject the causal Null hypothesis of no direct causal effect of N_P on A even if the Null hypothesis were true (Elwert and Winship 2008).14 In sum, the analyst is thus obliged to simultaneously

condition on C_O to control for confounding of N_O, and not to condition on C_O to avoid controlling away part of the effect of N_P and inducing endogenous selection bias. Conventional regression models, however, cannot simultaneously control and not control for the same variable. Thus, more powerful methods are required for estimating multigenerational neighborhood effects.

Estimating Multigenerational Neighborhood Effects using Marginal Structural Models

We use marginal structural models (MSM) with inverse probability of treatment (IPT) weighting to estimate the multigenerational effects of neighborhood disadvantage on children's cognitive outcomes. MSM are well-suited for the task because they are more powerful than conventional regression models in at least two senses: first, they are designed to resolve the two endogeneity problems of time-varying (here, multigenerational) exposure discussed above (Robins 1998; 1999a; 1999b; Robins et al. 2000; Hernán et al. 2000); second, they can do so making *fewer* assumptions than traditional regression models. MSM are two-step models. First, we estimate a logistic model of childhood residence in a poor neighborhood separately for each generation, G, as a function of baseline and time-varying confounders, C_G, influencing each generation's neighborhood poverty,

$$P(N_G)/(1 - P(N_G)) = \exp[\alpha_G + C_G \beta_G], \text{ for } G \in \{P, O\},\tag{2}$$

where C_O includes C_P and N_P to permit the possibility that factors influencing parent's childhood neighborhood of residence may extend their reach to also influence the child's neighborhood of residence.

From (2), we predict each family's probability of residing in the type of neighborhood that it did indeed reside in (actual treatment status) separately for each generation. The product of these two probabilities gives the probability, W, of the multigenerational residential history experienced by each family,

$$W = P(N_P|C_P) * P(N_O|N_P, C_P, C_O).\tag{3}$$

We then weight each case by the inverse of the probability of its family's residential history, W^{-1}. Weighting creates a pseudo-population in which the values of all variables included in the weights are balanced in expectation, such that treatment status in each generation is no longer confounded in the observables (Robins 1999b). Figure 2 illustrates the weighting process graphically by removing from Figure 1 all arrows into N_P and N_O illustrating that the structure of the reweighted data corresponds to the data structure of an experiment in which both N_P and N_O are randomized—controlling for C_O (or C_P) is no longer necessary. The expected values of all potential outcomes in the weighted pseudo-population, meanwhile, are the same as in the original population. Thus, simple conditioning on each generation's neighborhood poverty in the weighted pseudo-population recovers the desired potential outcomes—$E[A_N] = E_{weighted}[A| N_P, N_O]$—and conventional statistical models can be used to analyze the weighted data,

$$E_{weighted}[A|N_P, N_O] = \alpha + N_P \beta_1 + N_O \beta_2.\tag{4}$$

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[14]Note that U is not a confounder of the causal effect of NO on A once CO is controlled. Its existence is therefore perfectly compatible with the assumption of sequential unconfoundedness, 1.a and 1.b. And yet, it will lead to bias in a conventional regression analysis that controls for CO because of endogenous selection. Most conventional regression analyses simply ignore U at their peril.

Equation (4) gives a marginal structural model for the multigenerational effect of neighborhood disadvantage on child cognitive ability. The model is "marginal" because it recovers the marginal (as opposed to conditional) mean of the potential outcome distribution, and it is "structural" because its coefficients represent causal effects (rather than associations) if sequential unconfoundedness holds (Robins 1999b). The model intercept, α, estimates the child's mean cognitive ability if all parents and children had grown up in advantaged neighborhoods, and the sum of $\alpha + \beta_1 + \beta_2$ estimates the child's mean cognitive ability if all parents and children had grown up in disadvantaged neighborhoods. The sum of the two slopes $\beta_1 + \beta_2$ thus gives the *joint causal effect* of multigenerational neighborhood disadvantage on child's cognitive outcomes. The coefficient β_1 by itself estimates the *direct causal effect* of parental exposure to neighborhood poverty holding child's neighborhood type constant. The coefficient β_2 by itself estimates the causal effect of child's exposure to neighborhood poverty holding parents' neighborhood type constant.

MSM make fewer assumptions than corresponding conventional regression models. Like conventional regression, MSM assume that all variables jointly affecting treatments and outcome are measured, but unlike conventional regression, MSM can accommodate the existence of unobserved variables that may jointly affect C_P, C_O, and A (such as U in Figures 1 and 2) without inducing endogenous selection bias. Furthermore, since MSM need not control for observed confounders on the causal pathway in the outcomes equation (which are already accounted for in the weights), MSM do not "control away" part of the effect of interest.

One disadvantage of MSM is that weighting increases the standard errors of the parameter estimates. To increase efficiency, we use so-called stabilized weights (Robins et al. 2000),

$$SW = [P(N_P) * P(N_O | N_P)] * [P(N_P | C_P) * P(N_O | N_P, C_P, C_O)]^{-1}. \quad (5)$$

in lieu of the unstabilized weights of equation (3). We compute sandwich standard errors to account for the weighting (Robins et al. 2000) and to adjust for the clustering of siblings within families. We first estimate weights and MSMs for all respondents, and then again separately for blacks and whites in order to recover group-specific multigenerational treatment effect of neighborhood disadvantage.

Sensitivity Analysis

We test the robustness of our results to unobserved selection bias – violation of assumptions 1.a and 1.b – by implementing a formal sensitivity analysis. Unobserved selection bias would occur if families sort, or are sorted, into poor and non-poor neighborhoods on the basis of factors that affect child cognitive ability but that are not included in the weights equation. We distinguish two sociologically plausible selection scenarios. First, under *adverse selection*, the children currently living in poor neighborhoods would have lower cognitive ability regardless of where they live compared to the children currently living in non-poor neighborhoods, e.g. because the children currently placed in poor neighborhoods may come from families that read less to their children. *Adverse selection* would bias the estimated effect of living in a poor neighborhood downward, indicating a detrimental effect of neighborhood poverty on child cognitive ability even if no such effect exists. Second, under *positive self-selection*, parents may choose the place of residence that best benefits their children. For example, parents who believe that their child would benefit from organized extracurricular activities may choose to live in a non-poor neighborhood in which these activities are regularly available, whereas parents who believe that their children would benefit most from peer initiated outdoor play may choose a poor neighborhood in which children customarily structure their own leisure time. *Positive self-selection* (a

"parents-know-best" model) captures the classic formulation of selection bias where social actors make advantageous choices on the basis of information not available to the data analysts (Heckman 1979; Winship and Mare 1992). Although adverse selection and positive self-selection draw on very different behavioral models, both share in common that bias will arise if residential choice is a direct or indirect function of children's cognitive ability, i.e. their potential outcomes.

We implement a new type of sensitivity analysis for time-varying treatments that recognizes the advantages of modeling selection bias as a function of potential outcomes (Robins 1999a; 1999b). The key idea is to summarize the relationship between observed and counterfactual potential outcomes with a parsimonious selection function, and then to compute bias-adjusted causal estimates across the domain of the function. If the conclusions of the study do not change across a substantively reasonable range of values for the selection function, one concludes that the results are robust to selection bias. We then build on previous work (Brumback et al. 2004) to facilitate the substantive interpretation of the sensitivity analysis.

We illustrate the logic of our sensitivity analysis for a simple single-generation randomized experiment of neighborhood allocation. For such a hypothetical experiment, Figure 3 shows the cross tabulation of potential outcomes for individuals residing in poor neighborhoods, N=1, and non-poor neighborhoods, N=0, respectively. Cells E and H give the *observed* mean cognitive ability in a poor neighborhood for individuals living in a poor neighborhood, and the mean cognitive ability in a non-poor neighborhood for individuals living in a non-poor neighborhood, respectively. Cells F and G are *unobserved*, counterfactual, mean abilities: F gives the mean cognitive ability in a non-poor neighborhood for individuals actually living in a poor neighborhood; and G gives the mean cognitive ability in a poor neighborhood for individuals actually living in a non-poor neighborhood. In a perfect randomized experiment E=G and F=H, such that the observed mean potential outcome of people randomized to living in a poor neighborhood stands in for the unobserved mean potential outcome in a poor neighborhood of people actually living in a non-poor neighborhood, and vice versa. Thus, in a perfect randomized experiment, the difference in observed outcomes, E-H, would provide a valid estimate of the average causal effect of neighborhood poverty on child cognitive ability.

Departures from perfect randomization, by contrast, define selection bias. Selection bias results if E≠G, or F≠H, or both (Morgan and Winship 2007), i.e. if at least one observed mean outcome in one group is not representative of the unobserved mean counterfactual outcome in the other group. Selection can thus be described by a selection function (Robins 1999b),

$$c(n) = E[A^n | N=n] - E[A^n | N=1-n], \quad N \in \{0, 1\}, \tag{6}$$

where $c(0)$=H-F represents the mean baseline difference, and $c(1)$=E-G represents the mean treated-outcome difference between treatment and control groups. For any given value of $c(n)$, the table of observed and counterfactual outcomes is fully determined. Since the analyst knows the proportion of cases in the treatment group, $P(1)$, and the proportion of cases in the control group, $P(0)$, a bias-corrected estimate for the average causal effect can be computed immediately. Specifically, we first correct the observed outcomes, A, for the bias term $c(N)*P(1-N)$,

$$A_{corr} = A - c(N)*P(1-N), \tag{7}$$

and then compute a bias-corrected point estimate and standard errors from the corrected outcomes, A_{corr}.

$$E[\partial_i]=E[A_{corr}|N=1] - E[A_{corr}|N=0]. \tag{8}$$

The sensitivity analysis is completed by choosing a range of plausible values for $c(n)$ and computing the bias-corrected causal estimates for those values.

Note that our sensitivity analysis captures the *totality* of possible selection bias from *any* source, and *any* number of omitted variables, in one simple selection function, $c(n)$. This is an important advantage over other forms of sensitivity analysis that may be more familiar to sociologists (e.g. Rosenbaum and Rubin 1983, Harding 2003) but that only consider confounding due to a single omitted variable. Addressing the totality of selection makes for a far more rigorous safeguard against unobserved selection bias than do more traditional approaches to sensitivity analysis.

This basic logic generalizes to models of time-varying treatments with observed covariates, such as our MSM (Brumback et al. 2004). To accommodate time-varying treatments, the selection function $c(n)$ is generalized such that separate selection functions, $c_G(n)$ describe selection in each generation G, and formula (7) is modified to purge the observed outcomes of the bias that has accumulated across generations

$$A^{corr}=A - \sum_G C_G(N_G)P(1 - N_G). \tag{9}$$

To incorporate controls for observed covariates, the mean difference in (8) is simply replaced by the corresponding MSM.

Following Brumback et al. (2004), we constrain the selection functions for the two potential outcomes to be of the same absolute value, $|c_G(1_G)|= |c_G(0_G)|$, and explore two general specifications of the selection function, $c_G(n_G)=\alpha_G(2n_G-1)$ and $c_G(N_G)=\alpha_G$, where α_G is the generation-specific sensitivity parameter to be varied. Since purely statistical decision rules are neither available nor desirable, the interpretation of a formal sensitivity analysis needs to be judged against social theory and empirical subject matter knowledge. We argue that another central advantage of this approach to sensitivity analysis is that it enables the straightforward derivation of the behavioral implications of various selection functions across different regions of their sensitivity parameters. These implications are best gleaned by returning to Figure 3. Specifically, we note that the first selection function $c_G(n_G)=\alpha_G(2n_G-1)$ implies *adverse selection* into poor neighborhoods for negative values of α_G. (Positive values of α_G, by contrast, imply adverse selection into non-poor neighborhoods, which appears implausible.) The second selection function, $c_G(N_G)=\alpha_G$, by contrast, never implies adverse selection, but implies *positive self-selection* for sufficiently large (and in our specific application, positive[15]) values of α_G, $E[A|N_G=0] - E[A|N_G=1] \geq \alpha_G$, such that families currently living in poor and non-poor neighborhoods on average benefit from their current location compared to living in a neighborhood of the opposite type. Outside of the domain of positive self-selection, $E[A|N_G=1] - E[A|N_G=0] \leq \alpha_G$, the second selection function implies that children in non-poor neighborhoods, but not children in poor neighborhoods causally benefit from their current place of residence. This we also

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[15]This is easily confirmed by manipulating the variables in Figure 3 given the empirically true condition that the mean observed outcome in poor neighborhoods is smaller than the mean observed outcome in non-poor neighborhoods.

consider sociologically plausible. Hence, we believe that the most plausible range of α_G is $E[A|N_G=1] - E[A|N_G=0] \leq \alpha_G$, which in our specific case implies mildly negative to increasingly positive values for α_G. To facilitate interpretation, we calibrate α_G such that a unit change in α_G corresponds to the amount of observed confounding previously eliminated by adjusting for all observed confounders in the MSM. Results are then reported in terms of sensitivity to multiples of observed selection.

Results

Sample Characteristics

Table 2 displays sample characteristics across generations for all variables used to model treatment status in the parent and the child generations, respectively, by residential history. Not surprisingly, families that lived in non-poor neighborhoods in both generations (Column 2) were also advantaged in several other respects compared to families in which either parents or children (or both) grew up in a poor neighborhood (Columns 3–5). Parents and grandparents in families of multigenerational advantage were considerably more likely to be married, in better health, have more schooling, higher income, and greater occupational status, among other factors. As potentially confounding factors for the relationship between neighborhood of residence and child cognitive outcomes, the analysis needs to account for these differences across treatment groups. A comparison between white and African-American sample members demonstrates considerable racial differences, where the average African-American respondent appears disadvantaged to the average white respondent on the majority of observed measures (not shown). The replication within each race group of differences between treatment groups previously detected for the entire sample indicates that race-specific analyses should account for the same predictors of treatment status.

Weight construction

Stabilized inverse probability of treatment weights (Equation 5) are designed to capture selection into parents' and children's neighborhoods. They are estimated from flexible logistic regression models containing all predictors of neighborhood status listed in Table 2 as well as numerous interactions between predictors and race. Experimentation revealed the weights to be remarkably stable across numerous regression specifications.[16] Table 3 shows descriptive statistics for the final stabilized weights (Equation 5). The stabilized weights and their generation-specific components are well behaved in the omnibus model for all respondents (Table 3, Panel 1), and again in separate models for black and white respondents (Panels 2–4). The observed means of the final weights and their components are close to 1, as they should be in expectation. The weights are skewed to the left but center quite closely about the mean (standard deviations not exceeding 2.25). Comparing the range of the weights in the overall sample to the range in the African-American and white sub-samples, we note that the within-race weight-range is considerably smaller than in the overall sample. Since the stabilized weights measure the degree of exogeneity of treatment assignment with respect to observed covariates, this indicates that neighborhood poverty is comparatively less endogenous within each race group than in the overall sample, which documents that race itself is a potent determinant of residential environment. To prevent disproportionate influence from a small number of outlying cases, we drop 9 cases with extreme final weights (>14) from the analysis of the overall sample.

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[16] The logistic models predicting parents' and children's neighborhood poverty are ancillary and serve no purpose beyond predicting treatment status. Appendix Table A displays the coefficients from which the generation-specific weights were derived. While noting that these coefficients do (and need) not have a causal interpretation, we observe that race is the strongest predictor of residence in a poor neighborhood in both generations. The intergenerational transmission of residential context (Sharkey 2008) is reflected in the large and statistically significant association between parent neighborhood poverty and child neighborhood poverty.

Regression and Marginal Structural Models

Table 4 shows estimates for the causal effects of multigenerational neighborhood poverty on children's cognitive ability from IPT-weighted marginal structural models and compares them to unadjusted and conventional regression-adjusted estimates.

Prior to accounting for non-random selection into neighborhoods, neighborhood poverty in each generation is strongly and negatively associated with children's broad reading scores. Column 1 shows that parent's childhood neighborhood poverty is associated with about half a standard deviation decrease in child's broad reading scores ($b_1 = -8.83$ points, p-value<0.01), and child's own neighborhood poverty is associated roughly with another third of a standard deviation decrease ($b_2 = -5.98$ points, p-value<0.01). Column 2 shows results from conventional regression models that adjust for neighborhood selection by including as regressors all observed potentially confounding factors in the parent and child generations. These regression adjustments substantially alter the association between neighborhood poverty and child broad reading scores, reducing the apparent direct effect of parental neighborhood poverty by two thirds to −2.85 points (p<0.05), and the apparent effect of child's own neighborhood poverty to −1.73 points (p=0.12). However, these specifications include all of the parents' covariates, including parent educational attainment, income, and so forth. If the influence of parents' childhood neighborhoods is mediated by these or other aspects of parents' adult lives, then the regression estimates will "control away" all of these indirect pathways of influence. For this reason, conventional regression models lack a causal interpretation.

Column 3 presents estimates from a marginal structural model, which accounts for observed selection into treatment status in both generations through IPT weighting. Conditional on the assumption of sequential unconfoundedness, these estimates can be interpreted as causal effects. The direct causal effect of parental neighborhood poverty on its own – while fixing child neighborhood poverty – reduces child broad reading scores by a third of a standard deviation ($b_1 = -5.07$ points, p<0.05). The causal effect of child's own neighborhood poverty – while fixing parental treatment status – reduces child's broad reading scores by more than one fourth of a standard deviation ($b_2 = -4.20$ points, p<.05). The multigenerational effect of coming from a family residing in poor neighborhoods in two successive generations compared to a family living in non-poor neighborhoods is a reduction of $b_1 + b_2 = -9.27$ points in child's broad reading scores.[17] This multigenerational effect of neighborhood disadvantage on reading scores is substantively large (more than half a standard deviation in broad readings scores) and statistically significant at the $\alpha = 0.01$ percent level. Results for applied problem solving scores are similar to results for broad reading scores. The unadjusted association between multigenerational poverty and applied problem scores is large and statistically significant (Column 4); −9.68 points (p-value<0.01) for parental neighborhood poverty, and −5.66 points (p-value<0.01) for child neighborhood poverty, respectively. Adjusting for selection into neighborhoods using conventional regression models reduces these associations substantially (Column 5). Yet neither of these two conventional models has a causal interpretation. Estimates from the marginal structural model with IPT weighting indicate that the direct causal effect of parent's neighborhood poverty on child applied problem scores – while fixing child neighborhood poverty – is −5.97 points, or more than one third of a standard deviation, and statistically significant (p-value<l0.01). The estimated causal effect of child's own neighborhood poverty – while fixing parent's neighborhood status – is also negative, but substantively smaller (−2.39

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[17]We also tested an interaction between parent and child neighborhood poverty. Consistent with the hypothesis of cumulative effects, the coefficient on the outcome is negative. Adding the interaction term does not change the estimate for the joint multigenerational effect, but – by relaxing the assumption of constant effects –unhelpfully increases standard errors, impeding substantive interpretation (results available upon request).

points) and not statistically significant. The joint causal effect of multigenerational exposure to neighborhood poverty is substantively large and statistically significant, reducing child's applied problem scores by 8.36 points, more than half a standard deviation (p-value<0.01). [18]

We note that the causal point estimates for parental neighborhood poverty exceed those for child neighborhood poverty for broad reading and for applied problem scores. Not surprisingly, standard regression estimates, which control for variables on the causal pathway between parental neighborhood poverty and the outcome, substantially underestimate the effect of parental neighborhood poverty (and hence also of multigenerational neighborhood poverty) on children's cognitive outcomes.[19]

One limitation of the analytic design is that we include children from a wide age range in order to estimate multigenerational effects more precisely. This decision may obscure differences in the developmental timing of neighborhood effects. To partially address this issue, we re-estimated the core specifications (columns 3 and 6 from Table 4) using more narrow age ranges comprising children age 5–9, 10–14, and 15–18 respectively. While individual coefficients are less precise, we find only minor substantive differences in the effect of multigenerational exposure to neighborhood poverty. The effect of multigenerational exposure on broad reading scores is −5.89 (p<.01) for 5–9 year-olds, −12.76 (p<.01) for 10–14 year-olds, and −11.48 (p<.05) for 15–18 year-olds. The effect on applied problem scores is −5.43 (p<.05) for 5–9 year-olds, −12.42 (p<.01) for 10–14 year-olds, and −8.33 (p<.01) for 15–18 year-olds (details available upon request).

Table 5 shows causal estimates from race-specific MSM for different definitions of neighborhood poverty. Unfortunately, large standard errors due to smaller samples hinder interpretability. Among African-Americans, the overall pattern of estimates agrees with the results for the entire sample at the 20 percent neighborhood poverty definition. The direct causal effect of parental neighborhood poverty – while fixing child's neighborhood poverty – reduces broad reading scores and applied problem scores by around one third of a standard deviation (b_1=−5.96 points, p-value<0.01; and b_2=−4.47 points, p-value<0.01, respectively). Child's neighborhood poverty – while fixing parental neighborhood poverty – has minimal estimated effects, which fail to reach statistical significance. The multigenerational effect of living in a neighborhood with at least 20 percent poor households across two successive generations is substantively large and highly statistically significant, reducing broad reading scores by b_1+b_2=−6.26 points (p-value<0.01), and broad reading scores by b_1+b_2=−5.84 points (p- value<0.01).

Under a more stringent definition of neighborhood poverty that includes only neighborhoods in which 40 percent of households are poor, we find that the negative multigenerational causal effect of neighborhood poverty for African Americans increases to b_1+b_2=−13.11 points for broad readings scores (p-value<0.01), and b_1+b_2=−8.59 points for applied

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[18]Results reported in Table 4 are based on models in which missing values on the dependent variables are not imputed. Results are nearly identical when we impute missing values on the dependent variables: the estimated multigenerational effect of neighborhood poverty is −9.22 for the broad reading score, and −8.25 for the applied problems score. Missing values on the covariates are always imputed.

[19]As a partial robustness check of our results, we also estimated the effect of child neighborhood poverty on child test scores using propensity score analysis (matching the five nearest neighbors on the common support within a caliper of 0.5 on the estimated propensity score with replacement, [Leuven and Sianesi, 2003]). In this matching analysis, child-neighborhood poverty is estimated to reduce broad reading scores by 3.31 points, and applied problem scores by 1.77 points. Research suggests that propensity score matching for point (i.e. single-generation) treatments is more robust to selection-model misspecifications than IPT weighting (Hong, in press). The similarity of propensity score and IPTW estimates for single-generation effects thus increases faith in the stability of our multigenerational IPTW estimates. (For methodological reasons not detailed here, one would expect the estimates to be close, but not identical.)

problem scores (p-value<0.01). Both of these estimates have wide confidence intervals, however.

Results for white respondents at the 20 percent neighborhood poverty threshold are erratic and estimated imprecisely, showing no multigenerational effect on either broad reading or applied problems scores. Using the 10 percent poverty threshold, the multigenerational effect of exposure to neighborhood poverty on broad reading scores is statistically significant, and is equal to almost a third of a standard deviation ($b_1+b_2=-4.60$, p<.05). Overall, the large standard errors in the white sample suggest that the data contain too little information to warrant substantive interpretation.

Sensitivity Analysis

Figures 4 and 5 graphically display the results of the formal sensitivity analysis for selection bias for reading scores and applied problem scores, respectively. The strength of unobserved selection, α, for which the sensitivity of results is assessed, is displayed in terms of multiples of observed and already-accounted-for selection (measured as the difference in regression coefficients in the unadjusted models versus the IPT-weighted MSMs). A value of $\alpha=0$ assumes the absence of unobserved selection bias and simply replicates the point estimates previously reported in Tables 4 and 5. A value of $\alpha=1$ assumes that the effect of neighborhood poverty is as confounded in unobserved selection factors as it is confounded in observed factors that are already controlled for in the weights equation, i.e. that the omitted variables are collectively as important for sorting individuals into neighborhoods as education, income, status, race, marital status, age, and all other observed variables combined. Given the large set of observed predictors of neighborhood selection used in this study, we believe that values of $\alpha>|1|$ indicate extreme unobserved selection. The figures display sensitivity analyses for all parameters in the MSM. Dotted lines represent the estimated direct causal effects of parents' neighborhood poverty on child outcomes, β_1, across different values of α. Dashed lines represent estimated causal effects of child's own neighborhood poverty on child outcomes, β_2. Solid lines represent the multigenerational joint effect of placing both parents and children in poor neighborhoods, $\beta_1+\beta_2$. Bold segments of these lines represent results that are statistically significant at p<0.05, and thin segments represent results that are not statistically distinguishable from zero. Finally, we implement separate sensitivity analyses for the two behavioral scenarios potentially underlying the selection process discussed above. Specifically, Panels A of Figures 4 and 5 assume adverse selection; and, given $\alpha<0$, specifically adverse selection into *poor* neighborhoods; Panels B assume positive self-selection ("parents-know-best") into respondents' observed neighborhood, where we judge most plausible values of α greater than about -1, as explained above.

Figures 4 and 5 indicate that our previously presented results are quite robust to possible unobserved selection bias. This is especially true for results pertaining to multigenerational joint effects, and it holds both for reading scores and applied problem scores. Assuming adverse selection into poor neighborhoods ($\alpha<0$, Panels A), the multigenerational causal effects of placing both parents and children into poor neighborhoods remain negative and statistically significant if unobserved selection is no greater than about ¾ of the selection already controlled for through observed control variables. Assuming positive self-selection (starting around ($\alpha>-1$, Panels B), i.e. assuming that parents may choose neighborhoods that best benefit their children, we find that the estimated average effect of placing both parents and children into poor neighborhoods is negative and statistically significant across all values of α here considered sociologically plausible.[20] As long as unobserved selection does not exceed observed selection, the results presented in Tables 4 and 5 are thus robust both under adverse selection and positive self-selection, and the results are robust even to extreme unobserved selection under the assumption of positive self-selection.

DISCUSSION

This paper responds to growing evidence that neighborhood inequality cannot be fully captured at a single point in a child's life, or even in a single generation in a family's history. In this sense the analysis builds on recent research showing that a large majority of African-American families living in today's most disadvantaged residential areas are the same families that occupied the most disadvantaged neighborhoods in the 1970s, suggesting that neighborhood inequality should be conceptualized and studied as a multigenerational process (Sharkey 2008).

This observation complicates theoretical perspectives and empirical approaches to understanding the impact of neighborhoods on individuals. The evidence presented here suggests that a multigenerational perspective is crucial to understanding the relationship between neighborhood environments and cognitive ability. A family's exposure to neighborhood poverty over two consecutive generations is found to reduce the average child's cognitive ability by more than half a standard deviation. Further, we find strong evidence that a parent's childhood neighborhood environment influences her children's cognitive ability, a generation later. This finding is consistent with the idea that the parent's own childhood environment may influence the parent's child through its impact on the parent's educational attainment, occupational choices, income, marriage partner, and mental health. Through these and any number of additional pathways, it is plausible that the effect of parents' neighborhood environments on parents' adult outcomes may linger on to impact the next generation. We stop short of assessing the role of specific mechanisms in mediating the effect of parent's childhood neighborhoods on their children's cognitive ability because of the methodological problems inherent in mediation analysis (Sobel 2008). The goal of this paper is to establish the existence of multigenerational causal effects of neighborhood poverty in the first place, and hence suggests the investigation of the relative importance of specific causative mechanisms is an important goal for future research.

Direct comparison to previous research is complicated because most previous studies examine the effects of more severely concentrated poverty. Our race-specific analyses using more severe thresholds to define high-poverty neighborhoods are more directly comparable to the treatment effects under study in Moving to Opportunity (Kling et al. 2007; Sanbonmatsu et al. 2006), two natural experiments in Chicago (Jacob 2004; Ludwig et al. 2009) and in the Sampson et al. (2008) study in Chicago. Among African-Americans, we find large effects of extreme poverty in the child's environment on both broad reading scores and on applied problems scores, although the effects have wide confidence intervals. The magnitude of our estimated effect on reading scores is similar to estimates from Sampson et al.'s (2008) study of verbal ability among a sample of African Americans in Chicago, and is somewhat larger than estimated effects from the Kling et al. (2007) sub-analysis of African Americans in MTO and the Ludwig et al. (2009) analysis of African Americans in Chicago. Estimated effects on applied problems scores are comparable to the estimates on math scores from Ludwig et al. (2009), and are larger than those found in studies from MTO and from Jacob (2004), all of which report null effects on math/applied problems assessments.

Before discussing the implications of these findings, we must acknowledge several limitations. Most importantly, causal inference about multigenerational neighborhood effects from observational data necessarily relies on strong assumptions about the absence of

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[20]Brumback et al. (2004) note that $c_G(n)=\alpha_G$ (which we interpret as positive self-selection for $\alpha>1$) implies effect heterogeneity across treatment and control groups. The negative average causal effect of neighborhood poverty on child cognitive outcomes is thus partially owed to the greater prevalence of non-poor-neighborhood living in the population.

unobserved selection bias, specifically the assumption of sequential ignorability. This includes the assumption that respondents select into neighborhoods only on the basis of factors observed by the analyst, or factors strongly correlated with these observed factors. Our empirical strategy addresses this limitation in two ways. First, we estimate marginal structural models that rely on assumptions of sequential unconfoundedness that are *weaker* than the assumptions necessary in the corresponding conventional regression models. Second, we perform a novel formal sensitivity analysis for the totality of unobserved confounding that explores two broad selection scenarios and find that the estimates for the multigenerational joint effect of neighborhood poverty reported in this study are substantially robust to quite strong violations of the unconfoundedness assumption.

A second set of limitations is that the structure of the PSID data forced us to make decisions in the analysis design that are less than ideal. First, parental neighborhood environment is necessarily measured for only one parent—the parent that was in the PSID sample during childhood. Second, although this is the first study to assess the multigenerational nature of neighborhood effects, we must follow previous work in measuring the neighborhood environment within each generation over a relatively short period (here, three years). This may lead our estimates to understate the true effects of sustained neighborhood disadvantage, as Wodtke, Harding, and Elwert (2010) demonstrate in an analysis of neighborhood effects using detailed year-by-year residential trajectories in the single-generation context. Third, because parents give birth at different ages, there is substantial variation in the duration of the gap between measurement of neighborhood conditions in each generation. Finally, in an effort to retain as many cases as possible, our specifications include children from a wide age range. Retaining most children assessed in the CDS allows us to estimate multigenerational neighborhood effects more precisely and to conduct race-specific analyses, but it compromises our ability to make any claims about the developmental timing of neighborhood effects. However, supplementary analyses reported above indicate that the multigenerational joint effects of neighborhood poverty are substantial even within narrower age ranges.

With these limitations in mind, we believe that the notion of multigenerational neighborhood effects points to a revised, broader, conceptualization of how the neighborhood environment influences cognitive ability, and furthermore suggests a revised theoretical and empirical perspective on the influence of social contexts on child development. We argue that this revised perspective should inform interpretations of experimental and quasi-experimental research assessing the impact of neighborhood change arising from residential mobility, as well as observational research on social contexts and child development.

First consider the experimental and quasi-experimental evidence available from residential mobility programs, including the Gautreaux program in Chicago, the Moving to Opportunity experiment, and other similar programs (Briggs 1997). In all such programs, participants (typically low-income families living in public housing) are provided the chance to move to less disadvantaged environments, frequently in the same city or within the metropolitan area. Research based on these programs exploits exogenous variation in the destinations of participants in the programs to estimate how a change in the neighborhood environment impacts child and adult social outcomes. While this type of study provides sound evidence on the causal effect of *contemporary* neighborhood exposure due to a change in the neighborhood environment arising from a residential move, by design these studies do not capture the *lagged or cumulative* effects of previous neighborhood environments.

This focus on contemporary neighborhood circumstances has been questioned in recent research on youth in Chicago, which shows that the impact of living in severely disadvantaged neighborhoods continues to be felt years later (Sampson et al. 2008). The

challenge is strengthened considerably when one considers the possibility of generation-lagged effects or cumulative, multigenerational effects. A change in a family's neighborhood may bring about an abrupt and radical change in the social environment surrounding children, but this change may be a short-term departure from a familial history of life in disadvantaged environments. The shift in context may improve the opportunities available to adults and children, the child's peers and school environment, and the parent's mental health, but it may not undo the lingering influence of the parent's childhood environment. In short, a temporary change of scenery may not disrupt the effects of a family history of disadvantage.

This assessment should not be taken as a critique of the residential mobility literature, but as a lens with which to interpret it. Evaluations of residential mobility programs provide powerful evidence for policy-makers interested in designing programs to move families into areas that may improve adults' mental health or children's life chances. But these programs tell us little about the cumulative disadvantages facing a family living in America's poorest neighborhoods over long periods of time, unless the residential move creates a lasting change in the neighborhood environment that persists over multiple generations. The Moving to Opportunity program did not produce this type of change in families' environments. The initial drops in neighborhood poverty among families in the experimental group have faded quickly, due to moves back to high-poverty neighborhoods and rising poverty in the destination neighborhoods of experimental group families (Clampet-Lundquist and Massey 2008; Kling et al. 2007).21 If the most powerful effects of neighborhoods stem from exposure in prior generations, as our evidence indicates, it is perhaps not surprising that research from mobility programs has produced inconsistent and relatively small impacts.

Next consider the extensive literature on neighborhood effects based on observational data. The most common analytic approach in this literature involves estimating neighborhood effects while controlling for a set of family background measures. A common claim made in reviews of these studies is that the family environment is more important for child development than the neighborhood environment (Ellen and Turner 1997; Leventhal and Brooks-Gunn 2000). A multigenerational perspective suggests that such a conclusion is misleading. Aspects of family background that are linked with child developmental outcomes, such as parental income or education, may be endogenous to neighborhood conditions in the prior generation. Parents' educational attainment, economic position, and health are better thought of as partial outcomes of their own earlier residential circumstances. In this sense, individuals and families inseparably *embody* neighborhood histories, and it is therefore a mistake to think of the family and the neighborhood as competing developmental contexts. Our multigenerational perspective thus amplifies and acts upon recent calls to revisit the classics and question the neat separation of individuals and contexts. Writes Entwistle et al. (2007, p. 1498): "The literature has become preoccupied with whether contextual effects exist given a competition between individual and neighborhood effects. Blau's (1960) essential insight, that contextual effects operate through, and in concert with, individual effects, is little in evidence."22

Our theory and our results indicate that the family and the neighborhood environments are closely intertwined, combining to influence the developmental trajectories of individuals in ways that extend across generations. As we have shown, a multigenerational perspective is

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21By contrast, there is some evidence that the changes in the neighborhood environment brought about by the Gautreaux intervention have persisted over time (Keels et al. 2005). For this reason Gautreaux is likely the best future source of quasi-experimental evidence on multigenerational neighborhood effects. However, as noted earlier, Gautreaux was not a true social experiment and there is evidence that the destinations of participants were not entirely exogenous (Votruba and Kling 2008).

22Two examples of research that reflect or support this insight can be found in: Klebanov, et al. (1998); and Wilson (1991).

essential to understanding inequality in cognitive ability. Our findings support other studies showing a link between the neighborhood environment and children's cognitive ability, but we extend this literature by calling attention to the history of social environments occupied by family members over generations. This approach reflects the broader implication of this paper, which is that to understand inequality, in cognitive ability and in other developmental domains, it is not sufficient to focus on a single point in a child's life, or even a single generation of a family. Instead, we must understand the history of disadvantages experienced over generations of family members. This approach recognizes the complex ways in which lives are linked across generations (Elder, Johnson and Crosnoe 2003), so that disadvantages or advantages experienced in one generation may linger and add to the disadvantages or advantages experienced by the next. Uncovering the ways in which disadvantages compound over time is central to developing a more complete understanding of the maintenance and reproduction of inequality.

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APPENDIX

Table A

Logit models for selection into treatment (neighborhood poverty >=20%)

	Generation 1	Generation 2
Grandparent characteristics		
Treatment: Neighborhood poverty	-	19 25 ***
Race: 1= African-American	0.00 ***	0.00
Disability	1.25	0.75 *
Welfare receipt	0.34 *	0.50
Vocabulary score (0 to 13)	0.85 *	1.00
Married	1.39	1.25
Occupational status	1.00	1.00 **
Number of children	1.00	1.03
Income (log)	0.00 **	0.02
Education (yrs schooling)	1.25	1.72 **
Age (centered around age 40)	1.04 ***	0.99
Own home	0.16 ***	0.77
Annual hours worked (log)	0.85	1.09
Efficacy scale	0.92	1.01
Aspirations scale	0.91	1.11
Time horizons scale	0.95	1.00
Gender: 1=male	0.65 *	0.77
Race×vocab score	1.08	1.05
Race×education	1.08	0.83
Race×occupation	1.00	1.00
Race×income	3.06 **	3.35 **
Race×home ownership	4 24 ***	1.89
Race×welfare	1.91	3.25
Income (log) squared	1.46 *	1.14
Education (yrs schooling) squared	0.99	0.98 ***
Parent characteristics		
First child	-	1.00

	Generation 1	Generation 2
Disability	-	0.91
Welfare receipt	-	0.33 **
Self-reported health	-	1.07
Married	-	0.89
Occupational status	-	1.00
Number of children	-	1 41 ***
Income (log)	-	0.21
Education (yrs schooling)	-	0.77
Age (centered around age 40)	-	1.05 *
Own home	-	0.25 **
Annual hours worked (log)	-	1.15
Gender: 1=male	-	1.27
Race×gen 1 treatment	-	0.13 ***
Race×education	-	0.94
Race×occupation	-	1.00
Race×income	-	0.91
Race×home ownership	-	1.65
Race×welfare	-	4 49 **
Income (log) squared	-	1.05
Education (yrs schooling) squared	-	1.01
Child characteristics		
Gender: 1=male	-	1.00
Age as of 2001	-	0.98

Notes: Figures in columns showing model results represent odds ratios, standard errors not shown.

*** significant at p<.01;

** significant at p<.05;

* significant at p<. 10

[1] These measures represent characteristics of the household head in the parent's childhood family.

[2] These measures represent characteristics of the household head in the child's family.
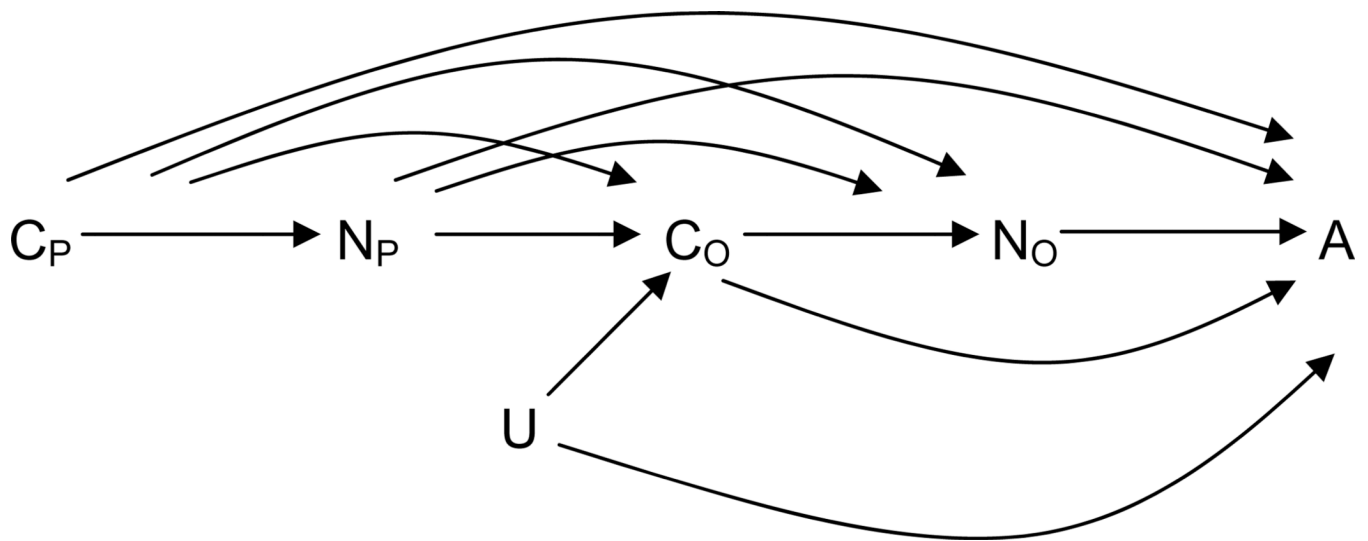
Figure 1.
Directed acyclic graph displaying possible direct and indirect causal pathways linking
neighborhood exposure (N) and confounding variables (C), which determine neighborhood
of residence in the parent (P) and child (O) generations, to child cognitive outcomes (A).
The vector U represents unobserved factors.
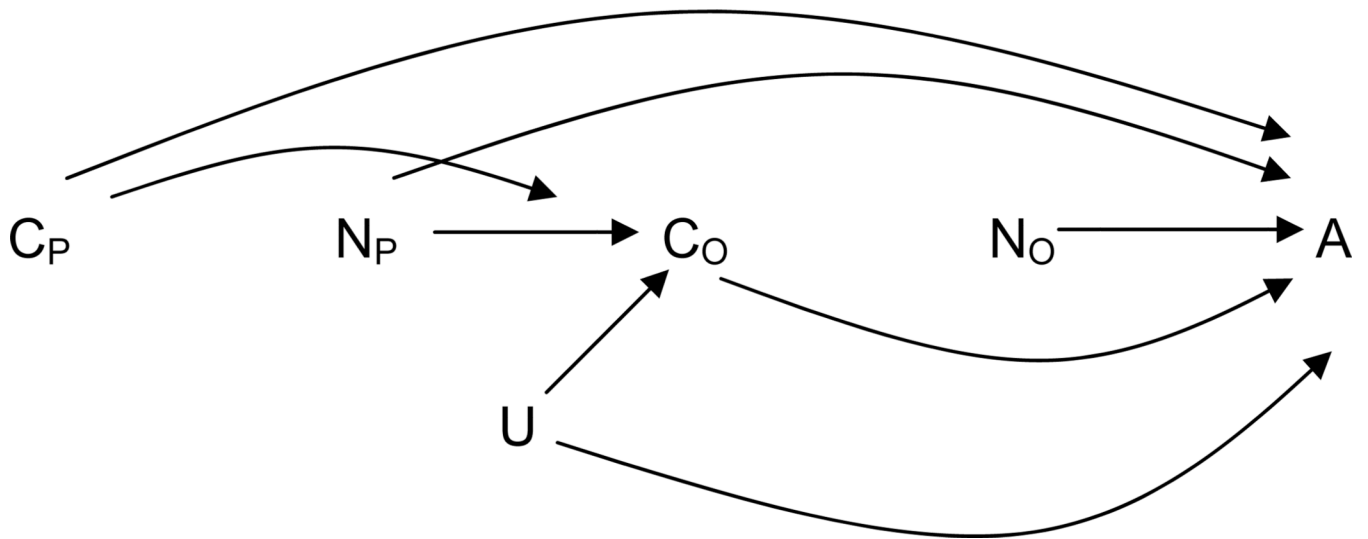
Figure 2.
Directed acyclic graph representing the same data structure as Figure 1 reweighted with IPT weights to remove the association between neighborhood of residence and confounding variables while keeping causal pathways between neighborhood of residence and child cognitive outcome unchanged.

	$E[A^{N=1} \mid N=n]$	$E[A^{N=0} \mid N=n]$
N=1	E	(F)
N=0	(G)	H

Figure 3.
Observed (E, H) and unobserved (F, G) mean potential outcomes by actual treatment status (N). N=1 indicates residence in a poor neighborhood and N=0 indicates residence in a non-poor neighborhood.

Reading: Adverse Selection

Reading: Self-Selection

Figure 4.
Sensitivity analyses for the robustness of neighborhood effects on child reading scores to unobserved selection bias of various strengths (α). Panel A assumes adverse selection into poor neighborhoods ($\alph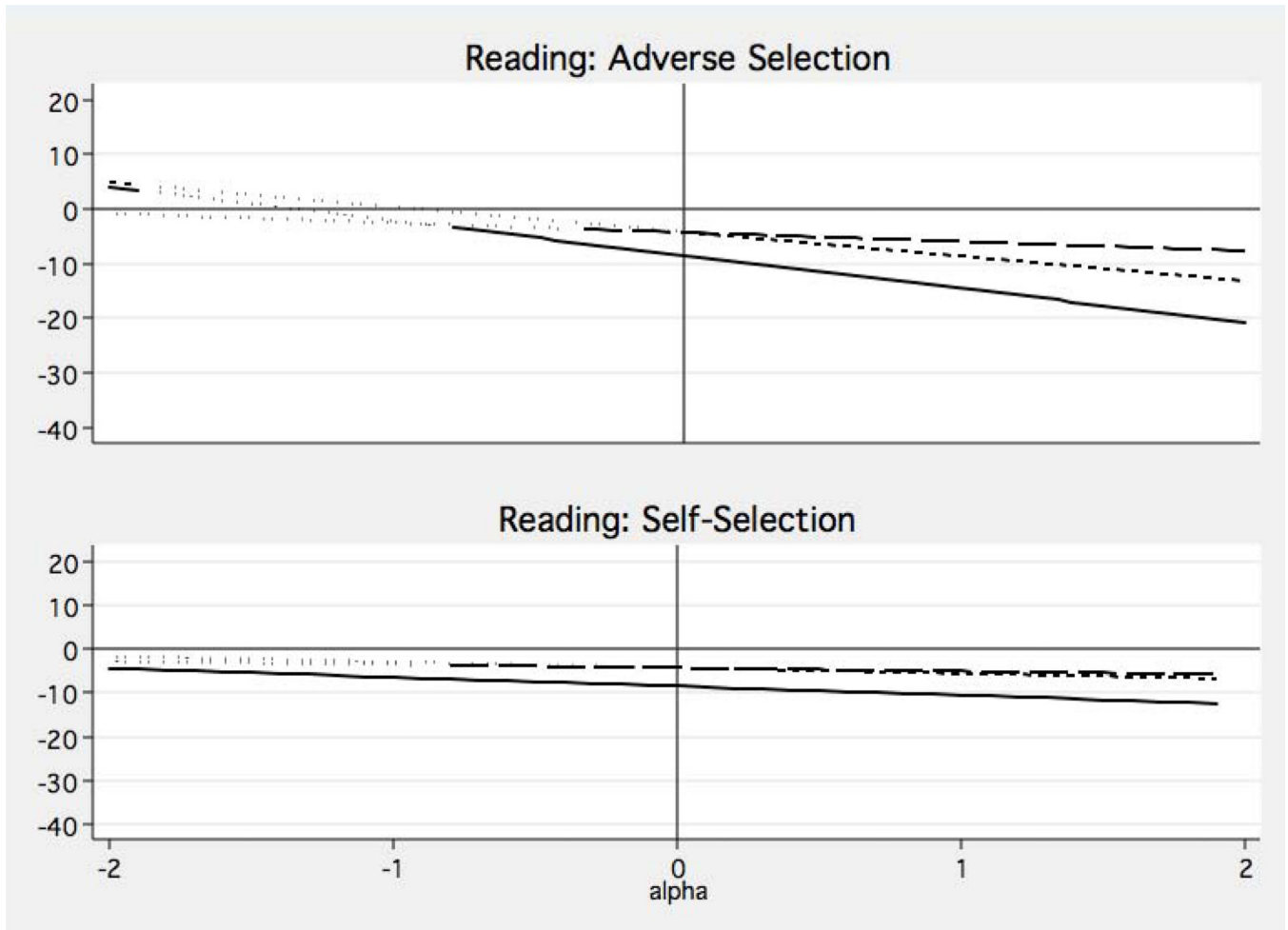a<0$) and adverse selection into non-poor neighborhoods ($\alpha>0$). Panel B assumes positive self-selection into each respondent's observed neighborhood type for approximately $\alpha>-1$. Dotted lines give point estimates for the direct causal effect of parent's neighborhood poverty. Dashed lines give point estimates for the causal effect of child's own neighborhood poverty. Solid lines give point estimates for the joint effect of parent's and child's multigenerational neighborhood poverty. Bold line segments contain point estimates that are statistically significant at $p<0.05$. Thin line segments contain point estimates that are not statistically significant. Combined sample of black and white families. Neighborhood poverty >20%.
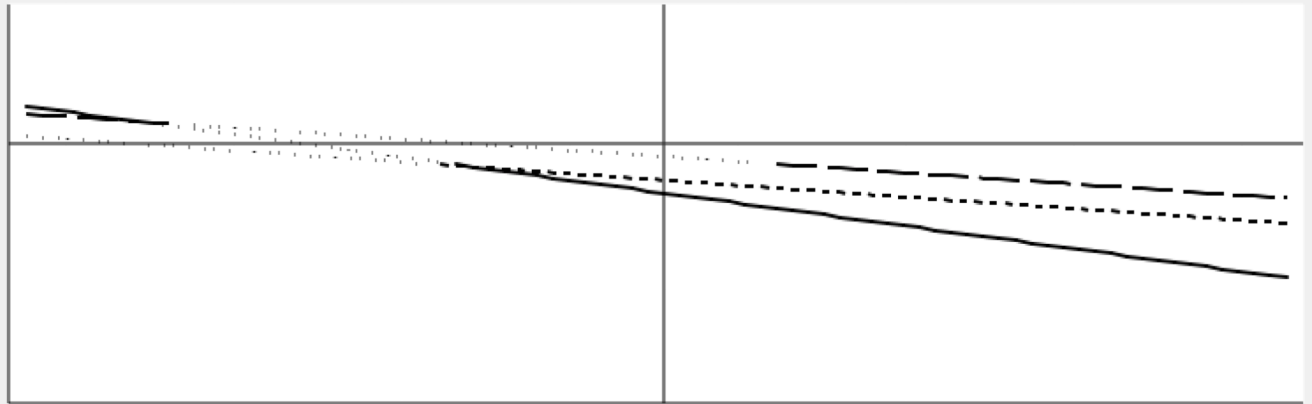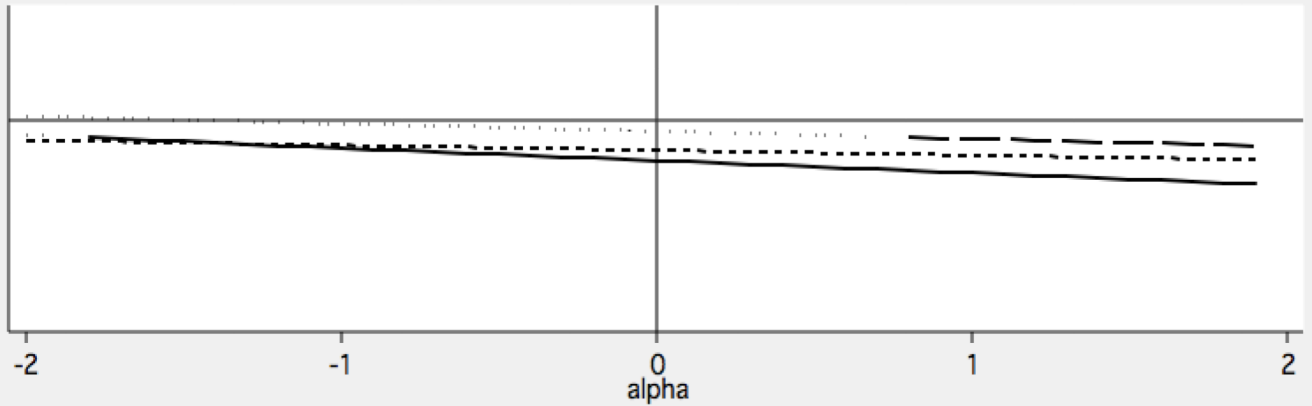
Figure 5.
Sensitivity analyses for the robustness of neighborhood effects on child applied problem scores to unobserved selection bias of various strengths (α). Panel A assumes adverse selection into poor neighborhoods ($\alpha<0$) and adverse selection into non-poor neighborhoods ($\alpha>0$). Panel B assumes positive self-selection into each respondent's observed neighborhood type for approximately $\alpha>-1$. Dotted lines give point estimates for the direct causal effect of parent's neighborhood poverty. Dashed lines give point estimates for the causal effect of child's own neighborhood poverty. Solid lines give point estimates for the joint effect of parent's and child's multigenerational neighborhood poverty. Bold line segments contain point estimates that are statistically significant at $p<0.05$. Thin line segments contain point estimates that are not statistically significant. Combined sample of black and white families. Neighborhood poverty >20%.

Table 1

Percentage of parent/child pairs exposed to neighborhood poverty under three different treatment definitions, by generation and race

	Treatment definition:		
Group	**≥10% poor**	**≥20% poor**	**≥40% poor**
All[a] (N=1556):			
Parent	57%	36%	8%
Child	55%	28%	4%
Both	45%	20%	2%
African-Americans (N=730):			
Parent	92%	70%	18%
Child	84%	52%	7%
Both	78%	41%	3%
Whites (N=792):			
Parent	23%	5%	0%
Child	28%	6%	1%
Both	14%	2%	0%

Note: Includes 34 parent-child pairs of other races.

Table 2

Sample means/fractions for the predictors of treatment status (≥20% poor) in each generation by treatment regime and race

	All	Parent not poor Child not poor	Parent not poor Child poor	Parent poor Child not poor	Parent poor Child poor
Grandparent characteristics predicting neighborhood poverty parent generation[1]					
African-American	0.47	0.15	0.69	0.90	0.93
Disability	0.34	0.28	0.26	0.46	0.44
Welfare receipt	0.22	0.08	0.29	0.34	0.47
Vocabulary score (0 to 13)	9.15	9.90	9.12	7.99	7.98
Married	0.69	0.81	0.67	0.53	0.48
Occupational status	−148.20	−101.94	−174.43	−213.46	−216.19
Number of children	3.56	3.18	3.77	3.87	4.32
Income (log)	10.64	10.98	10.53	10.18	10.11
Education (yrs schooling)	12.02	13.05	11.03	10.68	10.58
Age (centered around age 40)	−1.89	−1.13	−3.88	−2.16	−3.06
Own home	0.59	0.75	0.51	0.41	0.30
Annual hours worked (log)	7.43	7.66	7.57	7.20	6.94
Efficacy scale	3.50	3.84	3.45	3.09	2.88
Aspirations scale	3.49	3.39	3.77	3.48	3.69
Time horizons scale	5.06	5.30	4.96	4.68	4.74
Grandparent male	0.74	0.87	0.72	0.59	0.50
Parent (and child) characteristics predicting neighborhood poverty in child generation[2]					
First child	0.30	0.32	0.32	0.28	0.25
Disability	0.29	0.25	0.31	0.35	0.35
Welfare receipt	0.25	0.10	0.38	0.30	0.56
Self-reported health	2.22	2.03	2.30	2.42	2.56
Married	0.32	0.40	0.29	0.30	0.14
Occupational status	−132.57	−93.90	−169.46	−171.01	−196.19
Number of children	1.43	1.17	1.77	1.57	1.90
Income (log)	10.37	10.72	9.99	10.20	9.65
Education (yrs schooling)	13.34	13.88	12.71	13.04	12.33
Age (centered around age 40)	−12.06	−11.94	−12.91	−11.77	−12.27

	All	Parent not poor Child not poor	Parent not poor Child poor	Parent poor Child not poor	Parent poor Child poor
Own home	0.38	0.49	0.20	0.33	0.17
Annual hours worked (log)	7.35	7.59	7.09	7.24	6.86
Parent male	0.36	0.40	0.31	0.32	0.29
Child characteristics					
Child	0.51	0.49	0.55	0.54	0.51
Age	10.65	10.52	10.32	10.63	11.15
N	1,556	877	118	243	318

[1] These measures represent characteristics of the household head in the parent's childhood family.

[2] These measures represent characteristics of the parent or household head in the child's family (see text).

Table 3

Stabilized inverse probability of treatment weights

	Median	Mean	Std. dev.	Minimum	Maximum
All respondents (Treatment is ≥20% poor)					
Generation-specific components					
Generation 1	0.65	1.02	2.25	0.36	49.75
Generation 2	0.90	1.00	0.61	0.13	7.73
Final Weights	0.59	0.98	1.98	0.16	40.99
African-Americans (treatment is ≥20% poor)					
Generation-specific components					
Generation 1	0.90	1.00	0.43	0.38	4.68
Generation 2	0.83	1.00	0.56	0.43	4.43
Final Weights	0.79	1.00	0.69	0.24	5.49
African-Americans (treatment is ≥40% poor)					
Generation-specific components					
Generation 1	0.93	1.03	0.81	0.23	13.46
Generation 2	0.96	0.99	0.3	0.09	5.00
Final Weights	0.91	1.02	0.84	0.08	11.49
Whites (treatment is ≥20% poor)					
Generation-specific components					
Generation 1	0.97	1.00	0.37	0.07	9.45
Generation 2	0.97	1.01	0.50	0.10	10.81
Final Weights	0.95	1.01	0.60	0.05	12.55
Whites (treatment is ≥10% poor)					
Generation-specific components					
Generation 1	0.88	1.01	0.61	0.25	6.26
Generation 2	0.90	1.00	0.57	0.20	7.83
Final Weights	0.81	1.02	1.28	0.14	29.28

Note: Table includes 9 outlying cases with weights > 14, which are omitted from the marginal structural models presented in Table 4.

Table 4

Estimated effects of multigenerational exposure to neighborhood poverty on children's cognitive ability

| | Neighborhood poverty: >=20% poor | | | | | |
| | Broad reading score | | | Applied problems score | | |
	Unadjusted estimates	Regression adjusted	Marginal struct. model	Unadjusted estimates	Regression adjusted	Marginal struct. model
Parent neighborhood poverty only, β_1	−8.83 ***	−2.85 **	−5.07 **	−9.68 ***	−2.40 **	−5.97 ***
	(1.16)	(1.21)	(2.38)	(0.97)	(1.12)	(1.85)
Child neighborhood poverty only, β_2	−5.98 ***	−1.73	−4.20 **	−5.66 ***	−0.84	−2.39
	(1.23)	(1.10)	(2.00)	(1.00)	(1.02)	(1.79)
Multigenerational exposure, $\beta_1 + \beta_2$	-	-	−9.27 ***	-	-	−8.36 ***
	-	-	(1.68)	-	-	(1.69)

Notes:

***
significant at p<.01;

**
significant at p<.05;

*
significant at p<.10

Standard errors account for clustering at the family level.

Table 5

IPT weighted estimates for multigenerational exposure to different levels of neighborhood poverty on children's cognitive ability, by race

	≥10% Poor		≥20% Poor		≥40% Poor	
	Broad Reading	Applied Problems	Broad Reading	Applied Problems	Broad Reading	Applied Problems
African-Americans						
Parent Neighborhood Poverty, β_1	-	-	−5.96***	−4.74***	−5.13*	−2.91
	-	-	(2.07)	(1.69)	(2.87)	(2.00)
Child Neighborhood Poverty, β_2	-	-	−0.30	−1.09	−7.99*	−5.68*
	-	-	(1.87)	(1.45)	(4.30)	(2.97)
Multigenerational exposure, $\beta_1+\beta_2$	-	-	−6.26***	−5.84***	−13.11***	−8.59***
	-	-	(2.10)	(1.59)	(4.48)	(2.58)
Whites						
Parent Neighborhood Poverty, β_1	−2.12	−3.62*	6.76**	1.52	-	-
	(2.24)	(1.85)	(3.27)	(3.89)	-	-
Child Neighborhood Poverty, β_2	−2.98	−0.98	−8.07*	0.18	-	-
	(2.04)	(1.59)	(4.37)	(3.01)	-	-
Multigenerational exposure, $\beta_1+\beta_2$	−5.13	−4.60**	−1.31	1.70	-	-
	(3.13)	(2.15)	(4.31)	(3.84)	-	-

Notes:

***
significant at p<.01

**
significant at p<.05;

*
significant at p<. 10

Estimated causal effects from IPT weighted marginal structural models, standard errors in parentheses. Standard errors account for clustering at the family level.