NEIGHBORHOODS, SCHOOLS, AND ACADEMIC ACHIEVEMENT: A FORMAL MEDIATION ANALYSIS OF CONTEXTUAL EFFECTS ON READING AND MATHEMATICS ABILITIES DURING ADOLESCENCE

Geoffrey T. Wodtke

University of Toronto

January 27, 2016

Corresponding Author

Geoffrey T. Wodtke, Department of Sociology, University of Toronto, 725 Spadina Avenue, Toronto, ON M5S 2J4, Canada. Email: geoffrey.wodtke@utoronto.ca

Acknowledgements

The author would like to thank Ken Wodtke, Felix Elwert, David Harding, John Myles, participants in the Toronto Inequality Workshop, and participants in the Harvard Quantitative Methods Workshop for comments on previous drafts of this study. This research was supported by a Connaught New Researcher Award from the University of Toronto.

Keywords

Neighborhoods, schools, academic achievement, poverty, mediation

ABSTRACT

Although evidence indicates that neighborhoods affect educational outcomes, there is relatively little research on the mechanisms thought to mediate these effects. This study investigates whether school poverty mediates the effect of neighborhood context on academic achievement during adolescence. Specifically, it uses longitudinal data from the PSID together with counterfactual methods to estimate the total, natural direct and indirect, and controlled direct effects of adolescent exposure to advantaged rather than disadvantaged neighborhoods on reading and mathematics abilities. Total effects estimated from regression models that control for childhood measures of achievement, neighborhood context, and school poverty indicate that exposure to an advantaged neighborhood during adolescence reduces subsequent exposure to school poverty and improves academic achievement. Estimates of natural direct and indirect effects, however, indicate that the total effect of adolescent neighborhood context is not significantly mediated by school poverty because the differences in school composition induced by moving to an advantaged neighborhood have only a minimal direct impact on achievement. Similarly, estimates of controlled direct effects indicate that adolescent neighborhood context would still significantly affect academic achievement even if schools were desegregated along socioeconomic lines. These findings are highly robust to hypothetical patterns of unobserved confounding and to alternative measures of school context, which suggests that neighborhood effects, at least during adolescence, are largely due to mediating factors unrelated to schools.

Why does living in an advantaged rather than disadvantaged neighborhood improve academic achievement? Although evidence from a variety of different study designs indicates that neighborhood context affects educational outcomes (Aaronson 1998, Chetty, Hendren and Katz 2015, Harding 2003, Rosenbaum 1995, Wodtke, Harding and Elwert 2011), few studies investigate the mechanisms thought to mediate these effects. Neighborhood effect mediation refers to the causal process whereby changes in neighborhood context lead to changes in an intermediate variable, known as a mediator, which in turn lead to changes in an outcome of interest. A frequent criticism of research on neighborhood effects is that the mediators of these effects remain obscured in a "black box" (Galster 2012, Jencks and Mayer 1990, Sampson, Morenoff and Gannon-Rowley 2002)—that is, "research findings…are too scant to draw any firm conclusions about the potential pathways through which neighborhood effects may be transmitted" (Leventhal and Brooks-Gunn 2000:322).

Mediation analyses are essential for testing and refining theories of neighborhood effects. For example, institutional resource theory contends that the quality of the schools to which children have access is an important mediator of neighborhood effects on educational outcomes (Arum 2000, Jencks and Mayer 1990, Johnson 2012, Leventhal and Brooks-Gunn 2000, Wilson 1987). According to this perspective, neighborhood context directly affects the socioeconomic composition of schools because school assignment rules are based on residential location. Neighborhood context is also thought to affect school quality because schools composed predominantly of students from poor families may have fewer high-quality teachers, lower funding levels, and frequent classroom disruptions (Harris 2010, Willms 2010). Thus, differences in school environments linked to differences in neighborhood contexts are expected to significantly affect academic achievement.

Mediation analyses are also essential for developing and evaluating policy interventions to mitigate the harmful effects of spatially concentrated poverty. For example, consider the Moving to Opportunity (MTO) demonstration program (Orr et al. 2003, Sanbonmatsu et al. 2011). The MTO program issued rent vouchers through a random lottery to residents of high-poverty public housing projects in order to help them move to new neighborhoods with lower poverty levels. Contrary to expectations, children in the MTO experimental group, despite moving to neighborhoods with lower poverty rates, did not perform better academically than children in the control group (Orr et al. 2003, Sanbonmatsu et al. 2011). Echoing institutional resource theory, one plausible explanation for these findings is that children in the experimental group did not end up attending schools that were any better on common indicators of quality than children in the control group (Ferryman et al. 2008, Sanbonmatsu et al. 2006). But without detailed knowledge of the mechanisms that mediate neighborhood effects on educational outcomes, it is difficult to determine the reasons for various housing policy successes and failures, and by extension, to design more effective interventions in the future.

Although it is commonly hypothesized that neighborhood effects are mediated by schools, no prior study provides a formal mediation analysis that appropriately decomposes the total effect of neighborhood context into an indirect component operating through the school environment and a direct component operating through alternative pathways. While there are several prior studies that consider the joint effects of neighborhoods and schools on educational outcomes, they are all limited by their reliance on measurements of neighborhood and school contexts taken simultaneously rather than sequentially over time (Ainsworth 2002, Card and Rothstein 2007, Carlson and Cowen 2014, Cook et al. 2002, Goldsmith 2009, Owens 2010, Rendón 2014). This limitation precludes a formal mediation analysis because the effect of neighborhood context on subsequent exposure to the putative mediator—school context—cannot be assessed, and thus the total effect of neighborhood context cannot be decomposed into direct and indirect components (VanderWeele 2015). As Cook et al. (2002:1303-4) astutely note, it is exceedingly difficult to evaluate whether "neighborhoods exercise their influence through their effects on schools" without sequential measurements because any assumption about "the simultaneity of multiple causal relations is surely an oversimplification of reality." Further complicating their interpretation, results from these prior studies are mixed, with some finding mainly neighborhood effects (Ainsworth 2002, Card and Rothstein 2007), some finding mainly school effects (Goldsmith 2009, Carlson and Cohen 2014, Cook et al. 2002), and others finding both (Owens 2010, Rendón 2014).

This study investigates whether school poverty mediates the effects of adolescent neighborhood context on academic achievement using counterfactual methods and longitudinal data that provide the requisite sequential measurements of the treatment, mediator, and outcome. It focuses on measures of reading and mathematics achievement because these outcomes are closely linked with other dimensions of social stratification among adults, such as educational attainment, income, and health (Auld and Sidhu 2005, Murnane and Levy 2006). It focuses on school poverty because prior research suggests that the socioeconomic composition of students is more closely related to child outcomes than any other school-level factor (Coleman et al. 1966), although alternative measures of school context are also considered in ancillary analyses. The focus on adolescence is driven by prior research indicating that neighborhood context is particularly consequential during this developmental stage (Author Forthcoming, Wodtke 2013).

More specifically, this study investigates the following four research questions. First, what is the effect of adolescent exposure to an advantaged rather than disadvantaged neighborhood on academic achievement? Second, what is effect of adolescent exposure to an advantaged rather than disadvantaged neighborhood on academic achievement if subjects are subsequently exposed to the level of school poverty they would have experienced living in a disadvantaged neighborhood? Third, what is the effect of adolescent exposure to the level of school poverty that subjects would experience living in an advantaged neighborhood rather than the level of school poverty that they would experience living in a disadvantaged neighborhood? And fourth, what is the effect of adolescent exposure to an advantaged rather than disadvantaged neighborhood on academic achievement if subjects are subsequently exposed to the school poverty level that would prevail across all schools if they were socioeconomically desegregated?

These questions respectively refer to what are termed total, natural direct, natural indirect, and controlled direct effects within the counterfactual framework for causal inference (VanderWeele 2015). To estimate these effects, I use sequential measurements of neighborhood context, school poverty, and academic achievement taken during childhood and adolescence from subjects in the Panel Study of Income Dynamics (PSID). With these data, I estimate the effects of neighborhood context on exposure to school poverty and academic achievement during adolescence, while controlling for prior measures of achievement, neighborhood context, and school poverty taken during childhood. Short of conducting several different randomized experiments, mediation analyses that control for prior levels of the treatment, mediator, and outcome to estimate the effects of future levels of the treatment and mediator on future levels of the outcome provide the strongest grounds for causal inference (Pearl 2000, VanderWeele 2015).

Nevertheless, mediation analyses still require strong assumptions about unobserved confounding that have not been assessed in contextual effects research. Specifically, unbiased estimation of the effects outlined previously requires the conventional assumption that the effect of neighborhood context on academic achievement is unconfounded as well as the assumptions that the effect of school poverty on academic achievement and the effect of neighborhood context on exposure to school poverty are both unconfounded. Thus, I additionally provide a formal sensitivity analysis that evaluates whether key findings are robust to potential biases that result from different violations of these assumptions in practice.

This study makes several contributions to theory and research on contextual effects. First, substantively, it provides evidence that the school environment is not a particularly important mediator of neighborhood effects on academic achievement during adolescence. Although total effect estimates indicate that adolescent exposure to an advantaged rather than disadvantaged neighborhood reduces subsequent exposure to school poverty and improves academic achievement, direct and indirect effect estimates indicate that the total effect on achievement is not significantly mediated by school poverty because the differences in school composition induced by moving to an advantaged neighborhood have only a minimal impact on reading and mathematics abilities. These findings are highly robust to hypothetical patterns of unobserved confounding and to alternative measures of school context. Second, theoretically, these results suggest that institutional resource theory, at least as it relates to the mediating role of schools during adolescence, is in need of reconsideration or refinement. Third, methodologically, this study introduces counterfactual methods for mediation analyses and for evaluating the assumptions on which these analyses are based.

In the sections that follow, I review prior research related to each of the pathways through which neighborhood context is hypothesized to effect educational outcomes, focusing on the link between neighborhoods and schools. Next, I define a set of total, direct, and indirect effects using the counterfactual framework and explain the assumptions needed to identify these effects from observed data. Finally, I estimate these effects using regression-based procedures applied to the PSID and assess their robustness with a formal sensitivity analysis.

NEIGHBORHOOD EFFECT MEDIATION BY SCHOOL POVERTY

Institutional resource theory highlights the mediating role of schools in transmitting neighborhood effects on educational outcomes (Arum 2000, Jencks and Mayer 1990, Johnson 2012, Leventhal and Brooks-Gunn 2000, Wilson 1987). According to this perspective, differences in neighborhood context lead to differences in the school environment to which children are exposed by virtue of their residential location, which in turn lead to differences in academic achievement.

Neighborhood context directly affects the socioeconomic composition of the schools to which children are exposed primarily because the public schooling options available to residents are, with some exceptions, geographically determined. In most U.S. districts, public schools have designated attendance areas that restrict enrollment to residents from a set of local neighborhoods (National Center for Education Statistics 2014a). These assignment rules engender an important connection between neighborhood and school contexts: changes in neighborhood composition due to residential mobility or turnover lead to changes in the pool of eligible students from which local schools draw their enrollment. This indicates that exposure to an advantaged rather than disadvantaged neighborhood will tend to reduce the number of poor students with whom a child attends school.

Although neighborhood context is directly linked with the socioeconomic composition of schools, this link is not absolute. For example, some public schools may serve attendance areas composed of different neighborhoods that vary in their socioeconomic composition. Moreover, charter schools and intra-district open enrollment policies provide many families with schooling options beyond their immediate residential area. About 50 percent of urban residents have at least some degree of school choice within their public school system, and of those offered at least some choice, about 50 percent elect to enroll in a school outside of their local attendance area (Carlson and Cowen 2014, National Center for Education Statistics 2014a). Families may also choose to send their children to private schools, which tend to enroll substantially fewer poor students than public schools because of the additional tuition costs. Private schools may represent a feasible option for higher income families living in disadvantaged neighborhoods or for low-income families with access to school vouchers or targeted scholarships. Thus, while moving to an advantaged rather than disadvantaged neighborhood is expected to reduce the level of school poverty to which children are exposed, it is not uncommon for children to attend schools that are much more, or much less, advantaged than their neighborhoods (Saporito and Sohoni 2007).

The differences in exposure to school poverty induced by neighborhood context are commonly hypothesized to have significant effects on academic achievement because school poverty is linked with a number of educational deficiencies that may hamper student learning (Battistich et al. 1995, Choi et al. 2008, Coleman et al. 1966, Hedges et al. 1994, Willms 1986). First, the socioeconomic composition of schools may affect the quality of the learning environment. Schools with a large proportion of low-income students tend to disproportionately enroll students with lower ability levels because family socioeconomic background is a strong predictor of cognitive development, and these schools typically have a slower pace of instruction and a less rigorous curriculum (Barr and Dreeben 1983, Willms 2010). Schools with a large proportion of high-ability students, by contrast, provide greater exposure to peers with more expansive vocabularies and advanced subject knowledge, which may diffuse through student networks, heighten teacher expectations, and establish an achievement-oriented normative environment (Kahlenberg 2001). In addition, because family socioeconomic background is also a strong predictor of behavioral problems, schools with a large proportion of low-income students tend to have more disorderly classrooms and greater absenteeism, which makes it difficult for teachers to maintain instructional continuity (Barr and Dreeben 1983, Kahlenberg 2001, Raudenbush, Jean and Art 2011).

Second, the socioeconomic composition of schools may affect the quality of instruction. Schools with a large proportion of low-income students suffer from high rates of teacher attrition, which makes it difficult for these schools to recruit, retain, and develop high-quality teachers (Borman and Dowling 2008, Boyd et al. 2005). As a result, students at high-poverty schools may often receive less effective instruction than students at low-poverty schools. In addition, compared to low-income parents, high-income parents are more vigilant of low-quality teachers and tend to closely monitor school personnel, which may put pressure on administrators to install more effective teachers at schools with a large proportion of students from high-income families. In general, high-income parents tend to be more engaged in their children's education than low-income parents and thus may provide financial, human, and social resources to schools that have spillover benefits for all students (Ho and Willms 1996, Kahlenberg 2001, Steinberg 1997).

Finally, the socioeconomic composition of schools may be linked to school financial resources because public education funding is in part determined by local property taxes. Specifically, about 45 percent of public school revenues comes from local governments, while an additional 45 percent comes from state governments and 10 percent comes from the federal government (National Center for Education Statistics 2015). Because advantaged neighborhoods have a wealthier local tax base than disadvantaged neighborhoods, low-poverty schools serving advantaged communities may be relatively better off financially, which would enable them to invest more in their personnel, infrastructure, and program offerings. State and especially federal funding, however, is often specifically targeted at high-poverty schools and thus may compensate for funding disparities linked to local tax revenues. For example, according to a study of school expenditures conducted by the U.S. Department of Education, 73 percent of high schools in the highest poverty quartile of their district spend *more* per student than the average school in the lowest poverty quartile (Heuer and Stullich 2011), indicating that the link between school poverty and school funding may be weaker than is often assumed in contextual effects research (e.g., Sampson, Sharkey and Raudenbush 2008, Wodtke et al. 2011).

Despite these inconsistencies, many prior studies suggest that exposure to a school with a higher proportion of low-income students has a negative effect on educational outcomes (Battistich et al. 1995, Choi et al. 2008, Coleman et al. 1966, Willms 1986, Willms 2010). For example, prior research documents negative associations between school-level poverty rates and academic expectations, aspirations, and test scores (Battistich et al. 1995, Willms 2010). Similarly, other studies report positive associations between school-wide averages of parental socioeconomic status and individual student achievement (Choi et al. 2008, Willms 1986). In most cases, these associations are attenuated but still persist after controlling for different dimensions of a student's family background. Moreover, in their seminal study of school effects, Coleman et al. (1966:325) find that "the social composition of the student body is more highly related to achievement…than is any other school factor," including different characteristics of the facilities, curriculum, and teachers.

Nevertheless, several studies suggest that school effects may be rather small in practical terms. For example, despite their conclusion that school composition exhibits a relatively stronger association with achievement than other measures of the school environment, Coleman et al. (1966:325) also find that all of the school-level associations documented in their analysis are small in practical terms and conclude that “schools bring little influence to bear on a child's achievement that is independent of his background and general social context.” Consistent with these findings, estimates of school effects from study designs that provide more defensible grounds for causal inference, such as those that control for time-invariant unobserved characteristics of students or for baseline measures of the outcome, are typically much smaller than those estimated from studies with less extensive controls, and in some cases, their magnitude is substantively trivial (Lauen and Gaddis 2013).

Furthermore, there are even several prior studies that document positive, rather than negative, effects of school poverty on educational outcomes for certain groups of students (Attewell 2001, Crosnoe 2009, Davis 1966). According to relative deprivation theory, students evaluate themselves, and are evaluated by others, relative to their school peers. They may also compete with their school peers over limited educational resources, such as access to advanced placement courses. In this situation, attending school with mostly high-income students might actually depress academic self-esteem, curricular placement, and other educational outcomes because students—especially those from low-income families—are evaluated and must compete against peers who are, on average, better equipped to succeed in school. Consistent with this perspective, several prior studies find that low-income students who attend school with a greater proportion of high-ability or high-income peers tend to have lower academic achievement and more psychosocial problems (Attewell 2001, Crosnoe 2009, Davis 1966).

In sum, although the school environment is commonly hypothesized to be an important mechanism through which neighborhood effects operate, findings from prior studies that separately examine the different pathways that comprise this broader causal process are somewhat ambiguous, as are the mixed results from prior research on the joint effects of neighborhood and school contexts measured simultaneously (e.g., Ainsworth 2002, Carlson and Cowen 2014, Cook et al. 2002, Goldsmith 2009, Owens 2010, Rendón 2014). That there is a strong link between neighborhood context and the socioeconomic composition of schools seems beyond dispute, but it remains unclear whether the effects of exposure to school poverty on academic achievement are large enough to represent a substantively important mediating pathway through which the effects of neighborhood context are transmitted.

NEIGHBORHOOD EFFECTS VIA ALTERNATIVE PATHWAYS

In addition to the school environment, theories of neighborhood effects also highlight a number of other powerful mechanisms through which neighborhoods may influence educational outcomes. This suggests that the direct effects of neighborhood context operating through mediators other than school poverty are potentially substantial.

Social isolation theories of neighborhood effects posit that living in a disadvantaged neighborhood isolates resident children from influential peers and role models who value education, have ambitious educational aspirations, and discourage risky behaviors (Jencks and Mayer 1990, Wilson 1987). Infrequent contact with positive role models is thought to curb the aspirations of children living in disadvantaged neighborhoods and ultimately lead to disengagement from school. Furthermore, social isolation may facilitate the development of “ghetto-specific” (Wilson 1987), “oppositional” (Anderson 1999, Massey and Denton 1993), or “heterogeneous” (Harding 2010) cultures among neighborhood peer groups. These alternative subcultures are variously thought to devalue schooling and valorize risky behaviors, or to confuse and overwhelm children with a wide array of conflicting messages, all of which may lead to negative educational outcomes.

Social organization theories contend that disadvantaged neighborhoods engender lower levels of collective efficacy than more advantaged neighborhoods and that this, in turn, hinders academic progress (Sampson, Raudenbush and Earls 1997, Sampson 2001). Collective efficacy consists of “social cohesion among neighbors combined with a willingness to intervene on behalf of the common good” (Sampson et al. 1997:918). In disadvantaged neighborhoods with low levels of collective efficacy, residents may have difficulty realizing their common values and maintaining informal social control. As a result, youth in these neighborhoods may encounter fewer obstacles to engaging in potentially harmful behaviors. Social disorganization theory also contends that the breakdown of informal social control in disadvantaged neighborhoods leads to more violent crime, and exposure to violent crime is associated with a variety of cognitive, emotional, and behavioral problems (Sharkey 2010, Sharkey et al. 2012).

Environmental theories of neighborhood effects focus on the disparate health hazards encountered in advantaged versus disadvantaged neighborhoods. Because of the poor physical condition of disadvantaged neighborhoods together with their proximity to freeways and major industrial centers, residents of these neighborhoods are more likely to be exposed to pollutants, toxins, and allergens than residents of more advantaged neighborhoods (Crowder and Downey 2010, Ponce et al. 2005, Rosenfeld et al. 2010), which may lead to place-based educational disparities. For example, living in a disadvantaged neighborhood, which is more likely to have older and poorly maintained housing structures than an advantaged neighborhood, increases the risk of exposure to lead-based paint and lead-contaminated soil, and elevated blood lead levels are in turn a major risk factor for developmental problems that impede academic achievement (Lanphear, Weitzman and Eberly 1996, Tong, von Schirnding and Prapamontol 2000).

Finally, although institutional resource theory focuses largely on the mediating role of schools, it also suggests that several other local institutions are important for explaining neighborhood effects on academic achievement. For example, in addition to high-quality schools, advantaged neighborhoods are more likely than disadvantaged neighborhoods to have stable, accessible, and enriching childcare centers; grocery stores with healthy food options; and safe recreational facilities, all of which may promote positive educational outcomes for children (Bader et al. 2010, Johnson 2012, Weiss et al. 2011, Wilson 1987).

In sum, although the school environment is thought to be a particularly important mediator of neighborhood effects on academic achievement, there are several other potentially powerful pathways through which these effects may be transmitted, including the local culture, violent crime, environmental health hazards, and other institutional resources. Thus, theory and prior research additionally suggest a significant direct impact of neighborhood context on academic achievement that does not operate through the school environment.

METHODS

Data and Measures

To investigate whether neighborhood effects on academic achievement are mediated by the socioeconomic composition of schools, I use data from the PSID (Panel Study of Income Dynamics 2014). The PSID is a multicomponent longitudinal study that began in 1968 with a probability sample of about 4,800 households. From 1968 to 1997, the PSID main panel interviewed household members annually, and after 1997, interviews were conducted biennially. Detailed data on academic achievement in the PSID come from the Child Development Supplement (CDS). The CDS is a component of the PSID designed to track the dynamic process of human capital formation among children. The CDS first collected data in 1997 for a sample of 3,563 children in the PSID main panel who were between the ages of 0 and 12. It collected additional data for this sample at follow-up waves in 2002-2003 and in 2007.

Subjects in the CDS are matched to census tracts using the restricted-use geocode file, which contains tract identifiers for every wave of the PSID main panel. Data on the composition of census tracts come from the Geolytics Neighborhood Change Database (GeoLytics 2013). This database contains tract-level data from the 1970 to 2010 U.S. Censuses and from the 2006 to 2010 American Community Surveys, with tract characteristics and boundaries defined consistently over time.[1] Subjects in the CDS are also matched to schools using the restricted-use school file, which contains school identifiers for each wave of the CDS. Information about these schools comes from the NCES Common Core of Data (CCD) and Private School Universe Survey (PSS) (National Center for Education Statistics 2014b, National Center for Education Statistics 2014c). The CCD and PSS contain annual school-level measures of student and staff characteristics from all public and private schools in the U.S., respectively.

The analytic sample for this study includes the 2,208 children who were interviewed at the 1997 wave of the CDS when they were between the ages of 3 and 12. I focus on this subset of children because it is the group for which I can obtain measures of key variables during both childhood and adolescence. Using all available data from these subjects, I construct sequential measures of neighborhood context, school composition, and academic achievement, separately by developmental period. The time index t is used to distinguish between measures taken during childhood versus adolescence.

Treatment in this study, denoted by A_{it}, is the socioeconomic composition of a subject's neighborhood. I use principal components analysis to generate a composite measure of neighborhood composition based on seven tract characteristics: the poverty rate, the unemployment rate, median household income, the proportion of households that are female-headed, aggregate levels of education (the proportion of residents age 25 or older without a high school diploma and the proportion of residents age 25 or older with a college degree), and the occupational structure (the proportion of residents age 25 or older in managerial or professional occupations). This composite measure is scaled so that higher values represent more advantaged neighborhoods and lower values represent more disadvantaged neighborhoods. Part A of the Online Supplement describes the construction and properties of the treatment variable in detail.

The mediator in this study, denoted by M_{it}, is the socioeconomic composition of a subject's school. I measure the socioeconomic composition of schools using the proportion of students who are eligible for a free lunch through the National School Lunch Program. To qualify for a free lunch, a student's family must have an income at or below 130 percent of the federal poverty threshold. Thus, the proportion of students eligible for a free lunch approximates a school-level poverty rate. In addition, I also conduct ancillary analyses using several alternative measures of school context, including the racial composition of students, the teacher-pupil ratio, per-pupil expenditures, the average level of work experience among teachers, the average compensation level of teachers, and the proportion of teachers with an advanced degree. Part B of the Online Supplement presents results from these ancillary analyses, which are substantively similar to those based on school poverty.[2]

The outcome in this study, denoted by Y_{it}, is academic achievement. I measure two separate dimensions of academic achievement using the letter-word and applied problem tests from the Woodcock-Johnson Psycho-educational Battery–Revised (Woodcock and Johnson 1989), which assess reading and mathematics abilities, respectively. Normalized scores from each test reflect a subject's abilities relative to the national average for children of the same age. These tests are widely used in studies of contextual effects on academic achievement (e.g., Levanthal and Brooks-Gunn 2004, Sanbonmatsu et al. 2006, Sharkey and Elwert 2011), and they have excellent psychometric properties. For example, their test-retest reliabilities consistently exceed 0.90, and their correlations with alternative measures of achievement consistently exceed 0.70, indicating a high degree of criterion validity (LaForte, McGrew and Schrank 2014). In all multivariate analyses, measures of the treatment, mediator, and outcome are standardized to have zero mean and unit variance.

This study adjusts for an extensive set of covariates, denoted by C_{it}, to control for potential confounding of contextual effects on academic achievement. These include the race, gender, and age of the subject; the age and education level of the subject's primary caregiver; the marital and employment status of the family head; the net worth, income, homeowner status, and size of the subject's family; the regional location of the household; and the level of cognitive stimulation provided within the household. Race is coded 1 for black and 0 for nonblack. The nonblack category is composed predominantly of whites, but for parsimony, a small number of Hispanics and Asians are pooled with whites in this category because analyses based on more disaggregate measures of race yield nearly identical results. Gender is coded 1 for female and 0 for male. The age of both the subject and primary caregiver are measured in years, as is the education level of the primary caregiver. The marital and employment status of the family head are both dummy coded: 1 for married and 0 for unmarried, and 1 for employed and 0 for not employed. A family's net worth is equal to the value of all assets minus all debts. This measure is expressed in cube-root real dollars to adjust for its extreme positive skew while also accommodating negative values (i.e., net debtors). Family income is expressed as an "income-to-needs ratio" equal to the family's annual real income divided by the official poverty threshold. Homeownership status is expressed as a dummy variable indicating whether the family owns their residence. Family size is equal to the total number of people present in the household. Region is coded 1 for residence in a southern census division and 0 otherwise. Analyses based on more disaggregate measures of region yield very similar results. Finally, the level of household cognitive stimulation is measured using the Caldwell-Bradley HOME inventory (Caldwell and Bradley 1984). In all multivariate analyses, these covariates are centered at their sample mean.

Table 1 depicts the longitudinal measurement strategy used to ensure appropriate temporal ordering of the treatment, mediator, outcome, and covariates. Specifically, I first construct childhood measures of these variables using data collected in the both the PSID main panel and baseline waves of the CDS when subjects were 12 years old or younger. The "baseline" wave of the CDS here refers to the wave at which subjects were 8 to 12 years old. All childhood measures are indexed by the time subscript $t = 0$ and are used only as control variables to support the estimation of contextual effects during adolescence. Next, I construct adolescent measures of treatment using residential data collected as part of the PSID main panel surveys fielded in between the baseline wave and follow-up wave of the CDS. The "follow-up" wave of the CDS here refers to the wave fielded five years after baseline, when subjects were 13 to 17 years old. Finally, I construct adolescent measures of the mediator and outcome using data collected at the follow-up wave of the CDS. For notational simplicity, all adolescent measures are indexed by the time subscript $t = 1$, even though these measures of the treatment, mediator, and outcome are in fact sequentially ordered. Because residential information comes from a period at least two years before the outcome is measured and because school information refers to the academic year immediately preceding measurement of the outcome, these data have the following temporal structure: $\{A_{i0}, M_{i0}, Y_{i0}, C_{i0}, A_{i1}, M_{i1}, Y_{i1}\}$. Descriptive statistics for these variables are summarized in Table 2.

Figure 1 presents a directed acyclic graph that describes the hypothesized causal relationships outlined previously (Pearl 2000). The graph shows that adolescent exposure to different neighborhood contexts directly affects subsequent exposure to school poverty and also academic achievement. In addition, it shows that exposure to school poverty directly affects academic achievement, indicating that neighborhood effects during adolescence are mediated, at least in part, by the socioeconomic composition of schools.

Total, Direct, and Indirect Effects of Neighborhood Context

To evaluate whether neighborhood effects on academic achievement are mediated by school poverty, this study focuses on estimating total, natural direct and indirect, and controlled direct effects. In this section, I formally define these effects using potential outcomes and the counterfactual framework (Rubin 1974, VanderWeele 2015).

First, let a_1 indicate exposure to a specific level of neighborhood advantage during adolescence, and let $Y_{i1}(a_1)$ denote the potential outcome for academic achievement under exposure to these neighborhood conditions. More specifically, $Y_{i1}(a_1)$ is the achievement level of subject i had she previously been exposed to neighborhood conditions given by a_1. In the counterfactual framework, each subject is conceived to have a set potential outcomes corresponding to all possible values of treatment, and contrasts between potential outcomes associated with different values of treatment define causal effects. Thus, in this framework, the average total effect of neighborhood context is defined as $TE = E\big(Y_{i1}(a_1^*) - Y_{i1}(a_1)\big)$, which is the expected difference in academic achievement had subjects previously been exposed to the level of neighborhood advantage given by a_1^*, rather than a_1, during adolescence.

Next, let $M_{i1}(a_1)$ represent the adolescent level of school poverty to which subject i would subsequently be exposed under prior exposure to neighborhood conditions given by a_1. In addition, let $Y_{i1}(a_1) = Y_{i1}\big(a_1, M_{i1}(a_1)\big)$ denote the academic achievement level for subject i under adolescent exposure to a level of neighborhood advantage given by a_1 and, by extension, under subsequent exposure to the level of school poverty this subject would encounter as a result of prior residence in these neighborhood conditions. Using this expanded notation for the potential outcomes, the average total effect defined previously can be expressed as $E\big(Y_{i1}(a_1^*) - Y_{i1}(a_1)\big) = E\Big(Y_{i1}\big(a_1^*, M_{i1}(a_1^*)\big) - Y_{i1}\big(a_1, M_{i1}(a_1)\big)\Big)$ and then additively decomposed as follows: $E\Big(Y_{i1}\big(a_1^*, M_{i1}(a_1^*)\big) - Y_{i1}\big(a_1, M_{i1}(a_1)\big)\Big) = E\Big(Y_{i1}\big(a_1^*, M_{i1}(a_1)\big) - Y_{i1}\big(a_1, M_{i1}(a_1)\big)\Big) + E\Big(Y_{i1}\big(a_1^*, M_{i1}(a_1^*)\big) - Y_{i1}\big(a_1^*, M_{i1}(a_1)\big)\Big)$.

The first term in this decomposition represents the average natural direct effect, $NDE = E\Big(Y_{i1}\big(a_1^*, M_{i1}(a_1)\big) - Y_{i1}\big(a_1, M_{i1}(a_1)\big)\Big)$, which is the expected difference in academic achievement under adolescent exposure to the level of neighborhood advantage given by a_1^*, rather than a_1, if each subject were subsequently exposed to the level of school poverty they would have experienced under neighborhood conditions given by a_1. For example, with $a_1^* > a_1$, the average natural direct effect represents the expected difference in academic achievement linked to residence in a more advantaged neighborhood, rather than a more disadvantaged neighborhood, if each subject were subsequently exposed to the level of school poverty they would have experienced by virtue of residing in the more disadvantaged neighborhood.

The second term in the decomposition represents the average natural indirect effect, $NIE = E\left(Y_{i1}\left(a_1^*, M_{i1}(a_1^*)\right) - Y_{i1}\left(a_1^*, M_{i1}(a_1)\right)\right)$, which is the expected difference in academic achievement under exposure to a level of neighborhood advantage given by a_1^* if each subject were subsequently exposed to the level of school poverty they would have experienced under exposure to neighborhood conditions given by a_1^*, rather than a_1, during adolescence. For example, with $a_1^* > a_1$, the average natural indirect effect represents the expected difference in academic achievement resulting from exposure to the level of school poverty that each subject would have experienced had they lived in a more advantaged neighborhood rather than the level of school poverty that they would have experienced had they lived in a more disadvantaged neighborhood.

The natural indirect effect measures the effect of neighborhood context on academic achievement operating only through subsequent exposure to school poverty, while the natural direct effect measures the effect of neighborhood context operating through all pathways other than school poverty. For the natural direct effect, this is accomplished by fixing the mediator to the level it would have "naturally" been for each subject under the reference level of treatment, which deactivates the component of the total effect mediated via the socioeconomic composition of schools. For the natural indirect effect, this is accomplished by holding treatment fixed for each subject, which deactivates all direct pathways, and then comparing outcomes across the differences in the mediator that would have occurred under exposure to different treatments.

Finally, let $Y_{i1}(a_1, m_1)$ denote the academic achievement level for subject i had she lived in a neighborhood with a level of socioeconomic advantage given by a_1 and had she attended a school with a level of poverty given by m_1 during adolescence. In this notation, the average controlled direct effect is defined as $CDE(m_1) = E\big(Y_{i1}(a_1^*, m_1) - Y_{i1}(a_1, m_1)\big)$, which is the expected difference in academic achievement had subjects been exposed to the level of neighborhood advantage given by a_1^*, rather than a_1, and had they all been exposed to schools with the same level of poverty, m_1. For example, with $a_1^* > a_1$ and m_1 equal to its average value in the population, the controlled direct effect can be interpreted as the difference in academic achievement linked to residence in a more advantaged neighborhood, rather than a more disadvantaged neighborhood, if schools were desegregated by income.

In contrast to natural direct effects, where the mediator is set to whatever value it would have been for each subject under the reference level of treatment, controlled direct effects set the mediator to the same level for every subject regardless of treatment. This subtle distinction has several important implications. First, it implies that there cannot be a "controlled indirect effect" because setting the mediator to a specific value for all subjects precludes a change in treatment from inducing changes in the mediator. By extension, decomposition of the total effect into direct and indirect components cannot be accomplished with the controlled direct effect except under special conditions discussed below. Second, this distinction implies that controlled direct effects have a prescriptive interpretation, and that natural direct and indirect effects have a descriptive interpretation. Controlled direct effects are prescriptive in that they measure the effect of treatment after prescribing a specific intervention on the mediator for all subjects, whereas natural direct and indirect effects are descriptive in that they describe the causal process by which differences in neighborhood conditions bring about differences in academic achievement (Pearl 2000, VanderWeele 2009).

Despite these important distinctions, there are certain conditions when natural and controlled direct effects are equivalent. In particular, they are equivalent when there is no interaction between the effects of treatment and the mediator on the outcome. In this situation, the controlled direct effect of treatment is constant across all values of the mediator, which implies that $E\big(Y_{i1}(a_1^*, m_1) - Y_{i1}(a_1, m_1)\big) = E\left(Y_{i1}\big(a_1^*, M_{i1}(a_1)\big) - Y_{i1}\big(a_1, M_{i1}(a_1)\big)\right)$ and that the total effect can be decomposed into a controlled direct effect and a natural indirect effect.

Regression Estimation

In this section, I outline regression-based estimation procedures for the causal effects defined previously and explain the assumptions on which these procedures are based. For notational simplicity, childhood measures of the treatment, mediator, and outcome (i.e., A_{i0}, M_{i0}, Y_{i0}) are here subsumed into C_{i0}, the vector of baseline controls.

The average total effect can be computed from the following observed data regression model:

$$E(Y_{i1}|C_{i0}, A_{i1}) = \beta_0 + \beta_{10}C_{i0} + \beta_{11}A_{i1}, \tag{1}$$

where $TE = \beta_{11}(a_1^* - a_1)$ under the assumption that there is no unobserved treatment-outcome confounding and under the assumption that model for $E(Y_{i1}|C_{i0}, A_{i1})$ is correctly specified (VanderWeele 2015).[3] Note that these assumptions only support a causal interpretation of the coefficient associated with adolescent treatment, β_{11}. The confounding assumption in this context implies that there must not be any unobserved variables that jointly affect selection into different neighborhoods and academic achievement during adolescence. If this assumption is violated, then the average total effect is not identified. The modeling assumption in this context implies that the effect of neighborhood advantage on academic achievement during adolescence must be linear; that this effect must not vary systematically across levels of the baseline controls; and that the partial associations between academic achievement and the baseline controls must be linear and additive.

The average natural direct and indirect effects can be computed from the following set of observed data regression models:

$$E(M_{i1}|C_{i0}, A_{i1}) = \theta_0 + \theta_{10}C_{i0} + \theta_{11}A_{i1} \text{ and} \tag{2}$$

$$E(Y_{i1}|C_{i0}, A_{i1}, M_{i1}) = \lambda_0 + \lambda_{10}C_{i0} + \lambda_{11}A_{i1} + M_{i1}(\lambda_{21} + \lambda_{31}A_{i1}), \tag{3}$$

where $NDE = (\lambda_{11} + \lambda_{31}\theta_0 + \lambda_{31}\theta_{11}a_1)(a_1^* - a_1)$ and $NIE = (\lambda_{21}\theta_{11} + \lambda_{31}\theta_{11}a_1^*)(a_1^* - a_1)$ under the assumption that Equations 2 and 3 are both correctly specified and under the assumption that there is no unobserved treatment-outcome confounding, no unobserved mediator-outcome confounding, no unobserved treatment-mediator confounding, and no treatment-induced mediator-outcome confounding (VanderWeele 2015).[4] Note that these assumptions only support a causal interpretation for the coefficients associated with the adolescent treatment and mediator (i.e., θ_{11}, λ_{11}, λ_{21}, λ_{31}).

The confounding assumption in this context is rather complex. It implies that, during adolescence, there must not be any unobserved variables that jointly affect neighborhood context and academic achievement; that jointly affect school poverty and academic achievement; or that jointly affect neighborhood context and school poverty. In addition, this assumption also specifies that there must not be any treatment-induced mediator-outcome confounding. In other words, there must not be any variables—observed or unobserved—that jointly affect school poverty and academic achievement, and that are affected by neighborhood context. Figure 2 depicts each of these patterns of confounding using directed acyclic graphs. If any of these patterns are present in practice, then natural direct and indirect effects are not identified.

The assumption of a correctly specified model for $E(M_{i1}|C_{i0}, A_{i1})$ implies that the effect of neighborhood context on subsequent exposure to school poverty must be linear and must not vary across the baseline controls. It also implies that the partial associations between adolescent exposure to school poverty and baseline controls must be linear and additive. The assumption of a correctly specified model for $E(Y_{i1}|C_{i0}, A_{i1}, M_{i1})$ implies that the effect of neighborhood context on academic achievement must be linear within levels of school poverty; that the effect of school poverty on academic achievement must be linear within levels of neighborhood context; that the effects of neighborhood and school contexts must interact only with each other and that a change in neighborhood composition must increment the effect of school poverty by a constant amount; and finally, that the partial associations between academic achievement and baseline controls must be linear and additive.

The average controlled direct effect can be computed from the regression model in Equation 3. Moreover, this can be accomplished under a weaker set of assumptions than is required for natural direct and indirect effects. Specifically, $CDE(m_1) = (\lambda_{11} + \lambda_{31}m_1)(a_1^* - a_1)$ under the assumption that Equation 3 is correctly specified and under the assumption that there is no unobserved treatment-outcome or mediator-outcome confounding (VanderWeele 2015).[5] In contrast to natural direct and indirect effects, estimation of controlled direct effects does not require assumptions about unobserved treatment-mediator confounding or about treatment-induced mediator-outcome confounding, nor does it require a model for adolescent exposure to school poverty.

I estimate Equations 1 to 3 by ordinary least squares and then use them to construct point estimates for the total, natural direct and indirect, and controlled direct effects of neighborhood context on academic achievement during adolescence. Under the assumptions outlined previously, this estimation procedure is unbiased. Because several studies comparing observational with experimental estimates of contextual effects indicate that analyses controlling for prior measures of the outcome largely mitigate potential biases due to violations of these assumptions in practice (e.g., Chetty, Friedman and Rockoff 2014, Deming 2014), this study, which controls not only for prior measures of the outcome but also for prior measures of the treatment, mediator, and an extensive set of covariates, provides strong grounds for causal inference. Nevertheless, I also investigate the sensitivity of effect estimates to hypothetical violations of the confounding and modeling assumptions on which they are based.

In the results section below, I focus on total, natural direct, and natural indirect effects that contrast adolescent exposure to neighborhoods at the 80^{th} percentile of the national treatment distribution with exposure to neighborhoods at the 20^{th} percentile. The contrast between the 80^{th} versus the 20^{th} percentile returns the effects of living in an advantaged neighborhood with low poverty and unemployment, few female-headed households, and many highly educated adults versus living in a disadvantaged neighborhood with high poverty and unemployment, many female-headed households, and few well-educated adults. Similarly, for the controlled direct effect, I focus on contrasts between advantaged neighborhoods at the 80^{th} percentile and disadvantaged neighborhoods at the 20^{th} percentile of the national treatment distribution while setting the level of school poverty to its national average, which approximates the level that would prevail if schools were desegregated by income.

Standard errors for these effect estimates are computed from 500 bootstrap samples (Efron and Tibshirani 1993).[6] And to adjust for the uncertainty associated with missing data, I combine estimates across 100 complete datasets with missing values for all variables simulated via multiple imputation (Royston 2005, Rubin 1987).[7] In addition, because the PSID oversampled low-income families, weights may be required to generate representative estimates. Part C of the Online Supplement presents results based on the weighted sample. In the main text, I focus on unweighted descriptive statistics in order to document that the mediation analyses do not heavily rely on out-of-sample extrapolation, and I also present unweighted estimates from the mediation analyses because formal design tests indicate that the regression models described previously control for all relevant aspects of the sample design. In this situation, unweighted estimates are preferred because they are both representative and more efficient (Pfeffermann 1993, Winship and Radbill 1994).

RESULTS

Neighborhood and School Exposures during Adolescence

Table 3 describes the joint distribution of the treatment and mediator during adolescence. Specifically, it shows a cross tabulation of school poverty with neighborhood advantage, where both variables are grouped by quintile. Overall, this table confirms a strong correspondence between neighborhood and school composition. For example, among subjects in the most advantaged fifth quintile of neighborhoods, 68 percent attend schools in the first quintile of the school poverty distribution. Similarly, among subjects in the most disadvantaged first quintile of neighborhoods, 65 percent attend schools in either the fourth or fifth quintiles of the school poverty distribution. Despite this strong correspondence, Table 3 also documents that it is not uncommon for children in disadvantaged neighborhoods to attend lower poverty schools and for children in advantaged neighborhoods to attend higher poverty schools. For example, among subjects in the most disadvantaged first quintile of neighborhoods, 8 percent attend schools in the first quintile and 10 percent attend schools in the second quintile of the school poverty distribution. And among subjects in the most advantaged fifth quintile of neighborhoods, about 12 percent attend schools in the third, fourth, and fifth quintiles of the school poverty distribution. Furthermore, among residents of "middle class" neighborhoods in the second, third, and fourth quintiles of the national treatment distribution, there are a nontrivial number of subjects attending schools across the entire range of the school poverty distribution. In sum, Table 3 confirms a strong association between the socioeconomic composition of the neighborhoods and schools to which subjects are exposed during adolescence, but it also indicates that most combinations of neighborhood and school environments are well-represented in the analytic sample.

Total, Direct, and Indirect Effects of Adolescent Neighborhood Context

Table 4 presents results from the mediation analysis of neighborhood effects. Specifically, the upper panel of Table 4 presents estimates of the causal parameters in Equations 1 to 3, while the lower panel presents estimates of the total, natural direct and indirect, and controlled direct effects outlined previously. Total effect estimates, which are presented in the first row of the lower panel in Table 4, suggest that exposure to different neighborhood contexts has a modest impact on reading achievement and a large impact on mathematics achievement during adolescence. Specifically, the estimated total effect of neighborhood context on letter-word scores indicates that adolescent exposure to an advantaged neighborhood at the 80th percentile of the national treatment distribution, rather than a disadvantaged neighborhood at the 20th percentile, increases reading achievement by just under one-tenth of a standard deviation (i.e., $\widehat{TE}^{LW} = 0.079$). This effect is modest in substantive terms and fails to reach conventional significance thresholds. The estimated total effect of neighborhood context on applied problem scores, by contrast, is substantively large and statistically significant at the $\alpha = 0.001$ level. It indicates that adolescent exposure to an advantaged neighborhood at the 80th percentile of the national treatment distribution, rather than a disadvantaged neighborhood at the 20th percentile, increases mathematics achievement by about one-sixth of a standard deviation (i.e., $\widehat{TE}^{AP} = 0.161$). This effect is comparable in magnitude to the cognitive gains associated with one additional year of schooling (Winship and Korenman 1997).

Natural direct and indirect effect estimates, which are presented in the middle rows of the lower panel in Table 4, indicate that the total effects of neighborhood context on academic achievement are not mediated to any significant degree by the socioeconomic composition of schools. For example, the estimated natural direct effect of neighborhood context on applied problem scores indicates that if subjects were exposed to an advantaged neighborhood at the 80th percentile of the national treatment distribution, rather than a disadvantaged neighborhood at the 20th percentile, and then were subsequently exposed to the level of school poverty they would have experienced in the disadvantaged neighborhood, their mathematics abilities would still increase by about one-seventh of a standard deviation (i.e., $\widehat{NDE}^{AP} = 0.147$). This effect is substantively large, statistically significant at the $\alpha = 0.01$ level, and nearly equivalent to the total effect discussed previously. In other words, even with the pathway that operates via school poverty fully deactivated, differences in neighborhood context still exert a strong influence on academic achievement during adolescence.

By extension, the estimated natural indirect effect of neighborhood context on applied problem scores indicates that if subjects were first exposed to an advantaged neighborhood at the 80th percentile of the national treatment distribution and then were subsequently exposed to the level of school poverty they would experience under these neighborhood conditions, rather than the level they would experience under exposure to a disadvantaged neighborhood at the 20th percentile of the national treatment distribution, their mathematics abilities would only increase by about one-fiftieth of a standard deviation (i.e., $\widehat{NIE}^{AP} = 0.019$). This effect is not significant at conventional thresholds and is negligible in substantive terms. A similar pattern of natural direct and indirect effects are observed for letter-word scores (i.e., $\widehat{NDE}^{LW} = 0.071$, $\widehat{NIE}^{LW} = 0.008$), although the point estimates are smaller and neither are significant at conventional thresholds.

In addition to estimates of natural direct and indirect effects, Table 4 also reports a measure of the "proportion mediated," which is equal to the ratio of the natural indirect effect to the total effect (VanderWeele 2015). This measure captures the degree to which the pathway through school poverty can explain the effects of neighborhood context on academic achievement. The estimated proportion of the total neighborhood effect mediated by school poverty is only 10 percent for letter-word scores and only 12 percent for applied problem scores, indicating that the socioeconomic composition of schools is not a particularly important pathway through which neighborhoods affect academic achievement during adolescence.

Estimates of the causal parameters in Equation 3 illuminate why school poverty is not a particularly important mediator of neighborhood effects. Specifically, these estimates indicate that the socioeconomic composition of schools plays only a minimal mediating role primarily because school poverty does not have a very large effect on academic achievement during adolescence. For example, according to these estimates, a one standard deviation increase in the level of school poverty to which subjects are exposed during adolescence is linked to a decrease in applied problem scores of only about one-twentieth of a standard deviation, given that subjects were previously exposed to a neighborhood at the mean of the national treatment distribution (i.e., $\hat{\lambda}_{21}^{AP} = -0.044$). This effect is not statistically significant at conventional thresholds, and it is only about half as large as the analogous neighborhood effect. A similar pattern holds in the model for letter-words scores.

Results from Equation 2, by contrast, indicate that the effect of neighborhood context on subsequent exposure to school poverty is substantively large and highly significant. Specifically, the point estimate of the causal parameter in this equation indicates that adolescent exposure to an advantaged neighborhood at the 80th percentile of the national treatment distribution, rather than a disadvantaged neighborhood at the 20th percentile, would reduce subsequent exposure to school poverty by nearly one-third of a standard deviation (i.e., $\hat{\theta}_{11}(1.5) = -0.195(1.5) = -0.293$).[8] To put this effect in context, a one-third standard deviation reduction in the school poverty rate is equal to about 10 percentage points. Thus, the parameter estimates from Equations 2 and 3 together indicate that neighborhood effects are not mediated to a significant degree by the socioeconomic composition of schools because even the substantively large reduction in exposure to school poverty induced by moving from a more disadvantaged to a more advantaged neighborhood has only a minimal effect on academic achievement.

Finally, the controlled direct effect estimates, which are presented in the bottom rows of the lower panel in Table 4, indicate that changes in neighborhood context during adolescence would still effect significant changes in academic achievement even if all subjects were exposed to schools with poverty levels equivalent to the national average. For example, the estimated controlled direct effect on applied problem scores indicates that if subjects were exposed to an advantaged neighborhood at the 80th percentile of the national treatment distribution, rather than a disadvantaged neighborhood at the 20th percentile, and then were subsequently exposed to the average level of school poverty in the U.S., their mathematics abilities would still increase by about one-seventh of a standard deviation (i.e., $\widehat{CDE}^{AP} = 0.153$). This effect is statistically significant at the $\alpha = 0.01$ level, and it is comparable to both the total and natural direct effects discussed previously.

The last row in Table 4 reports a measure of the "proportion eliminated," which is equal to the difference between the total effect and controlled direct effect divided by the total effect (VanderWeele 2015). This measure captures the degree to which neighborhood effects can be mitigated via some school-level intervention that would desegregate students by income. The estimated proportion of the total neighborhood effect that would be eliminated by such an intervention is only 9 percent for letter-word scores and only 5 percent for applied problem scores, which suggests that the effects of neighborhood context cannot be overcome by equalizing the socioeconomic composition of schools.

Sensitivity Analyses

The estimates presented previously only have a causal interpretation under a number of strong assumptions about unobserved confounding and correct model specification. This section investigates the sensitivity of results to potential violations of these assumptions. The sensitivity of the total effect to unobserved treatment-outcome confounding is assessed by computing a bias term and then subtracting it from the point estimate and both limits of the confidence interval. The bias term in this context is $B = \gamma\delta$, where $\gamma = E(Y_{i1}|U_{i0} = 1, C_{i0}, A_{i1}) - E(Y_{i1}|U_{i0} = 0, C_{i0}, A_{i1})$ is the mean difference in academic achievement associated with a unit change in a hypothetical treatment-outcome confounder, U_{i0}, conditional on the observed treatment and baseline controls, and $\delta = E(U_{i0}|C_{i0}, A_{i1} = a_1^*) - E(U_{i0}|C_{i0}, A_{i1} = a_1)$ is the mean difference in the hypothetical confounder for those exposed to neighborhood conditions given by a_1^*, rather than a_1, conditional on baseline controls (VanderWeele 2015). If inferences about the total effect are invariant across a range of substantively reasonable values for γ and δ, this suggests that they are robust to unobserved confounding.[9]

The upper panel of Table 5 presents bias-adjusted point estimates and confidence intervals for the total effect of neighborhood context on applied problem scores.[10] In this analysis, the hypothetical treatment-outcome confounder is assumed to have a positive association with exposure to neighborhood advantage (i.e., $\delta > 0$) and a positive partial effect on academic achievement (i.e., $\gamma > 0$). An example of such a confounder might be parental skill—that is, skilled parents may be more likely to live in advantaged neighborhoods and to promote the academic achievement of their children. To facilitate interpretation of the sensitivity parameters, the values of γ are scaled to equal multiples of the conditional mean difference in academic achievement associated with a one standard deviation increase in parental education. Similarly, the values of δ are scaled to equal multiples of the conditional mean difference in parental education associated with living in an advantaged neighborhood at the 80th percentile of the national treatment distribution rather than a disadvantaged neighborhood at the 20th percentile. Results indicate that the estimated total effect of neighborhood context on applied problem scores is highly robust to unobserved treatment-outcome confounding. Specifically, the bias-adjusted estimates remain substantively large and statistically significant except under the most extreme scenarios where treatment-outcome confounding is two or three times as large as that due to parental education. Given that parental education is perhaps the most powerful joint predictor of academic achievement and neighborhood attainment, this level of treatment-outcome confounding is unlikely.

The sensitivity of controlled direct effects to unobserved treatment-outcome confounding is assessed using the same procedures described previously except the mean differences that compose the bias term are now also made conditional on the mediator, M_{i1}. That is, $B = \gamma_m \delta_m$, where $\gamma_m = E(Y_{i1}|U_{i0} = 1, C_{i0}, A_{i1}, M_{i1}) - E(Y_{i1}|U_{i0} = 0, C_{i0}, A_{i1}, M_{i1},)$ is the mean difference in academic achievement associated with a unit change in the hypothetical treatment-outcome confounder conditional on the observed treatment, mediator, and baseline controls, and $\delta_m = E(U_{i0}|C_{i0}, A_{i1} = a_1^*, M_{i1}) - E(U_{i0}|C_{i0}, A_{i1} = a_1, M_{i1})$ is the mean difference in the hypothetical confounder for those exposed to neighborhood conditions given by a_1^*, rather than a_1, conditional on the mediator and baseline controls (VanderWeele 2015). The first rows in the lower panel of Table 5 present estimates of the controlled direct effect on applied problem scores that are adjusted for unobserved treatment-outcome confounding. As before, the values of γ_m and δ_m are scaled to be multiples of the analogous mean differences associated with parental education. Results indicate that controlled direct effect estimates remain significant and substantively large except under extreme levels of unobserved treatment-outcome confounding.

The sensitivity of controlled direct effects to unobserved mediator-outcome confounding is assessed using these exact same procedures but with U_{i0} now re-conceptualized as a mediator-outcome confounder, rather than a treatment-outcome confounder, which has important implications for the specification of δ_m. Unobserved mediator-outcome confounding is problematic in analyses of controlled direct effects because conditioning on the mediator would lead to collider-stratification bias—that is, setting the level of the mediator to some fixed value would *induce* an association between treatment and the mediator-outcome confounder even though these two variables may be unconditionally independent. In this situation, the direction of the induced association is determined by the effect of the unobserved confounder on the mediator and by the effect of the treatment on the mediator. As documented previously, the effect of neighborhood advantage on subsequent exposure to school poverty is negative, and given that U_{i0} is assumed to have a positive effect on academic achievement, the only plausible assumption about its effect on the mediator, school poverty, is that this effect is also negative. In other words, U_{i0} is assumed to be an unobserved variable that reduces exposure to school poverty and increases academic achievement. An example of such a confounder might be the educational values of parents, where those who highly value formal education may be more likely to ensure their children attend low-poverty schools and to promote academic achievement at home.

When the common causes of an outcome have effects that operate in the same direction, conditioning on that outcome induces a negative association between its common causes. To better appreciate this pattern in the present context, consider the following highly exaggerated example: suppose that subjects only attend a low-poverty school if either they live in an advantaged neighborhood or they have parents that highly value formal education. In this contrived situation, subjects attending a low-poverty school and living in an advantaged neighborhood must have parents who do not value formal education, while subjects attending a low-poverty school and living in a disadvantaged neighborhoods must have parents who do value formal education. Thus, among subjects attending low-poverty schools, there is a perfect inverse association between neighborhood advantage and the educational values of parents. The association between educational values and neighborhood context induced by conditioning on school poverty would tend to suppress the positive controlled direct effect of neighborhood advantage on academic achievement because this effect would be based on a comparison of subjects in advantaged neighborhoods who have parents that do not value education with subjects in disadvantaged neighborhoods who have parents that do value education.

To assess the sensitivity of controlled direct effects to unobserved mediator-outcome confounding, I therefore use the negation of δ_m in the computation for the bias term, which reflects the assumed inverse association between treatment and the hypothetical unobserved confounder. The bottom rows in the lower panel of Table 5 present point estimates and confidence intervals for the controlled direct effect that are adjusted for this type of confounding. These results indicate that estimates of the controlled direct effect are highly robust to plausible patterns of mediator-outcome confounding. In fact, this type of confounding works to suppress, rather than inflate, estimates of controlled direct effects.

The sensitivity of natural direct effects to unobserved treatment-outcome and mediator-outcome confounding can be assessed using the same procedures for controlled direct effects under the assumption that there is no interaction between the effects of treatment and the mediator on the outcome. Because all treatment-mediator interactions in models of academic achievement are substantively small and not statistically significant, this assumption appears reasonable in the present analysis. The upper panel of Table 6 presents bias-adjusted estimates for the natural direct effect of neighborhood context on applied problem scores. These results indicate that estimates of the natural direct effect are also highly robust to treatment-outcome and mediator-outcome confounding. The lower panel of Table 6 presents bias-adjusted estimates for the natural indirect effect, which is only sensitive to unobserved mediator-outcome confounding. These estimates are obtained by computing the same bias term used to assess mediator-outcome confounding for the controlled and natural direct effects, but this term is then added, rather than subtracted, to compute a bias-adjusted estimate of the natural indirect effect (VanderWeele 2015). Results indicate that natural indirect effect estimates are indistinguishable from zero under modest levels of mediator-outcome confounding.

The sensitivity of natural direct and indirect effects to unobserved treatment-mediator confounding is assessed by first computing bias-adjusted estimates of θ_{11}, the effect of neighborhood advantage on subsequent exposure to school poverty, and then substituting these estimates in equations for the natural direct and indirect effects. The bias term for θ_{11} is given by $B = \kappa\delta$, where δ is defined exactly as before and $\kappa = E(M_{i1}|U_{i0} = 1, C_{i0}, A_{i1}) - E(M_{i1}|U_{i0} = 0, C_{i0}, A_{i1})$ is the mean difference in exposure to school poverty associated with a unit change in a hypothetical treatment-mediator confounder, conditional on the observed treatment and baseline controls. Table 7 presents bias-adjusted estimates for the natural direct and indirect effects of neighborhood context on applied problem scores. In this analysis, the treatment-mediator confounder is assumed to have a negative partial effect on subsequent exposure to school poverty (i.e., $\kappa < 0$), where the specific values of κ are scaled to equal multiples of the conditional mean difference in exposure to school poverty associated with a one standard deviation increase in parental education. These results indicate that estimates of natural direct and indirect effects are also highly robust to potential treatment-mediator confounding.

In addition to unobserved confounding, measurement error in the mediator can also lead to bias in estimates of direct and indirect effects. This is concerning because measurement error in the mediator tends to inflate estimates of natural direct effects and deflate estimates of natural indirect effects, which could potentially obscure an important mediating role for schools in the present study. Furthermore, this study measures the socioeconomic composition of schools with just a single indicator of student poverty, but it measures the socioeconomic composition of neighborhoods with a composite index based on multiple different characteristics of residents. Thus, this study adopts a measure of school context that is arguably less reliable and more prone to error than its measure of neighborhood context, which amplifies concerns about bias due to measurement error in the mediator.

Under the assumption of no treatment-mediator interaction, the sensitivity of natural direct and indirect effects to random measurement error can be assessed by computing bias-adjusted estimates of λ_{11} and λ_{21}, the coefficients associated with neighborhood advantage and school poverty in models of academic achievement, and then substituting these estimates in equations for the natural direct and indirect effects. Specifically, bias-adjusted estimates of these parameters are given by $\lambda_{11}^{adj} = \lambda_{11} - \lambda_{21}\theta_{11}\left(\frac{1}{\phi} - 1\right)$ and $\lambda_{21}^{adj} = \lambda_{21}\frac{1}{\phi}$, where ϕ is the proportion of variance in the mismeasured mediator explained by the true mediator (VanderWeele 2015). Table 8 presents bias-adjusted estimates for natural direct and indirect effects on applied problem scores under different values of ϕ, where lower values indicate greater measurement error.[11] The bias-adjusted estimates of the natural direct effect remain substantively large and statistically significant, while the bias-adjusted estimates of the natural indirect effect remain substantively small and statistically insignificant, even under extreme levels of measurement error in the mediator.

Finally, causal inferences about direct and indirect effects are also based on the assumption that the functional form of Equations 2 and 3 is correctly specified. Part D of the Online Supplement reports results from a variety of specifications for these regression equations, including several that permit extensive nonlinearities and several others that include treatment and mediator interactions with baseline controls. Results from these different specifications indicate that the reported estimates are highly robust. In sum, the central conclusion of this analysis—that the socioeconomic composition of schools is not a particularly important mediator of neighborhood effects on academic achievement during adolescence—withstands many different violations of the confounding, modeling, and measurement assumptions on which it is based.

DISCUSSION

Although the educational effects of neighborhood context are extensively studied, there is relatively little research on the mechanisms commonly hypothesized to mediate these effects. This study investigates whether the socioeconomic composition of schools mediates the effects of neighborhood context on academic achievement during adolescence. Using appropriate sequential measurements of the treatment, mediator, outcome, and controls together with counterfactual methods, it finds that adolescent exposure to an advantaged rather than disadvantaged neighborhood reduces subsequent exposure to school poverty and improves academic achievement; however, because the differences in school poverty induced via changes in neighborhood context have only a minimal direct impact on academic achievement, the socioeconomic composition of schools does not appear to be a very important mediator of neighborhood effects during adolescence. By extension, this study also finds that the effect of neighborhood context on academic achievement cannot be mitigated by an intervention that would equalize the socioeconomic composition of students across schools. An extensive battery of sensitivity analyses indicates that these results are highly robust to potential violations of the assumptions on which they are based.

Taken together, these findings are difficult to reconcile with institutional resource theory, at least as it relates to the mediating role of schools in transmitting neighborhood effects on academic achievement during adolescence. Although results indicate that moving from a disadvantaged neighborhood to an advantaged neighborhood would substantially reduce subsequent exposure to school poverty, the differences in school composition induced by this type of neighborhood mobility would have only a minimal direct impact on academic achievement. This suggests that neighborhood effects during adolescence are primarily due to mediating factors not directly linked to school composition, such as neighborhood subcultures, collective efficacy, violent crime, or environmental hazards.

A potentially important policy implication of these findings is that interventions designed to reduce the socioeconomic segregation of students across schools may not significantly attenuate the educational effects of socioeconomic segregation across neighborhoods. While this type of school-level intervention can certainly be motivated on alternative grounds and implemented to achieve alternative goals, this study suggests that policies designed to mitigate the effects of neighborhood segregation on academic achievement may not be very effective if they focus primarily on the local school environment. In other words, overcoming the effects of socioeconomic segregation across neighborhoods may require place-based, rather than school-based, interventions that focus primarily on local neighborhood environments, such as targeted investments in infrastructure and housing, community policing, and small-scale residential mobility programs (Sharkey 2013). Without additional, corroborating mediation analyses that focus on other educational outcomes and other developmental periods, however, the policy implications of the present study remain preliminary and somewhat speculative.

The results of this analysis are also inconsistent, at least in part, with several prior studies that attempt to estimate joint effects of neighborhood and school conditions on academic achievement. For example, Cook et al. (2002:1305) report that "neighborhood coefficients were regularly smaller than the other context coefficients and were not even systematically reliable in models that included other contexts," such as schools. Similarly, Carlson and Cowen (2014:48) find that “for one-year test score gains, the school a student attends is more important than the neighborhood in which the student resides” and that “neighborhood disadvantage can be more than offset by attendance at a high-quality school.” By contrast, this study suggests that neighborhood effects are substantively large, statistically significant, and highly reliable during adolescence and that they cannot be explained in terms of school effects, which are substantively small, statistically insignificant, and generally unreliable during this developmental period.

There are a variety of possible explanations for these differences. For example, the studies mentioned previously use data representative of single cities rather than the national population; adopt different measures of neighborhood context, school context, and academic achievement; and rely on concurrent rather than sequential measures of these variables, making it difficult to determine causal priority. But two possible explanations for these differences stand out as particularly important. First, compared to these prior studies, the present analysis estimates adolescent contextual effects while controlling for a more extensive set of putative confounders, including baseline measures of the treatment, mediator, and outcome. Research suggests that inferences about school effects are extremely sensitive to the extent to which the study design controls for confounding (Lauen and Gaddis 2013). Specifically, findings indicate that study designs with less rigorous controls yield large estimates of school effects, while alternative designs with more rigorous controls yield small estimates that are substantively trivial. Results from the present study resonate with these findings, and in ancillary analyses not reported here, I also find that estimated school effects become larger and statistically significant in models that include a less extensive set of controls.

Second, these prior studies focus on an earlier developmental period than is considered in the present analysis. Research suggests that neighborhood effects are more pronounced during adolescence when individuals become especially sensitive to local peers (Author Forthcoming, Wodtke 2013), while several other studies suggest that school effects may be strongest earlier during childhood when individuals are especially sensitive to instruction (Heckman and Krueger 2004, Heckman 2006). Thus, neighborhood and school effects may operate during different developmental periods, and the purportedly weaker effects of neighborhood context during childhood may be mediated entirely by the school environment to which children are exposed at this stage of development.

This study focuses squarely on point-in-time effects of neighborhood and school contexts measured during adolescence, and it does not attempt to estimate total, direct, or indirect effects of time-varying contextual exposures throughout childhood and adolescence. Although this aspect of the research design provides considerable protection against confounding bias, it is not without limitations. In particular, given that contextual effects likely depend on the timing of exposure during the course of development, the results from this analysis cannot be extrapolated to other developmental periods. An important direction for future research will be to investigate neighborhood effect mediation during both childhood and adolescence using time-varying contextual measures. Unfortunately, the development of tractable estimation procedures for mediation analyses in the time-varying setting is still in its infancy (VanderWeele 2015).

Another limitation of the present study is that it focuses on a single dimension of school context—the socioeconomic composition of students. Although prior research on school effects indicates that this dimension tends to exhibit the strongest association with student achievement, it remains possible that other measures of the school environment play a more important mediating role in transmitting neighborhood effects. To address this limitation, I conducted ancillary analyses with a variety of different school-level measures, including the racial composition of students, the teacher-pupil ratio, per-pupil expenditures, and several aggregate measures of teacher human capital. Results from this analysis provide no evidence that any of these school characteristics, taken individually or jointly, mediate the effects of neighborhood context (see Part B of the Online Supplement for details). Nevertheless, future research should investigate the mediating role of school characteristics that are not considered in this study and that may be more closely linked with neighborhood context and academic achievement, such as the school social climate or in-school violence (e.g., Burdick-Will 2013).

Finally, this study is limited by its narrow focus on achievement test scores. Although test scores are correlated with a variety of other important outcomes, such as high school graduation and college attendance, it remains possible that these other outcomes are more sensitive to adolescent differences in school environments, and by extension, that schools play a more important role in mediating the effects of neighborhood context on these other outcomes. For example, there is considerable evidence that differences in school resources, teacher characteristics, and student composition have large effects on college attendance and criminal behavior (Deming 2011; Deming et al. 2014). Future research should investigate whether school characteristics mediate neighborhood effects on other developmental outcomes that are also important determinants of economic, social, and physical well-being.

These limitations notwithstanding, the weight of the evidence indicates that neighborhood effects on academic achievement during adolescence are primarily the result of mediating factors unrelated to the school environment. This suggests that unpacking the "black box" through which neighborhood effects are transmitted will likely require a renewed focus on alternative pathways, such as those related to local subcultures or violent crime (Harding 2009, Harding 2011), among a variety of other possibilities. Although this study fails to confirm an important role for one commonly hypothesized pathway, it directs the focus of future research toward alternative pathways and introduces powerful counterfactual methods with which they can be evaluated.

ENDNOTES

1. For intercensal years, tract characteristics are imputed using linear interpolation.

2. I do not employ a composite measure of school composition similar to that used for neighborhood composition because the different school-level measures outlined here are only weakly correlated with one another, which means that any composite measure will have low reliability (see Appendix B for details).

3. The expression for the average total effect comes from $E(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1^*) - E(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1) = (\beta_0 + \beta_{11}a_1^*) - (\beta_0 + \beta_{11}a_1) = \beta_{11}(a_1^* - a_1)$.

4. The expression for the average natural direct effect comes from $E\big(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1^*, M_{i1} = E(M_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1)\big) - E\big(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1, M_{i1} = E(M_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1)\big) = \big(\lambda_0 + \lambda_{11}a_1^* + (\theta_0 + \theta_{11}a_1)(\lambda_{21} + \lambda_{31}a_1^*)\big) - \big(\lambda_0 + \lambda_{11}a_1 + (\theta_0 + \theta_{11}a_1)(\lambda_{21} + \lambda_{31}a_1)\big) = (\lambda_{11} + \lambda_{31}\theta_0 + \lambda_{31}\theta_{11}a_1)(a_1^* - a_1)$. Similarly, the expression for the average natural indirect effect comes from $E\big(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1^*, M_{i1} = E(M_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1^*)\big) - E\big(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1^*, M_{i1} = E(M_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1)\big) = \big(\lambda_0 + \lambda_{11}a_1^* + (\theta_0 + \theta_{11}a_1^*)(\lambda_{21} + \lambda_{31}a_1^*)\big) - \big(\lambda_0 + \lambda_{11}a_1^* + (\theta_0 + \theta_{11}a_1)(\lambda_{21} + \lambda_{31}a_1^*)\big) = (\lambda_{21}\theta_{11} + \lambda_{31}\theta_{11}a_1^*)(a_1^* - a_1)$.

5. The expression for the controlled direct effect comes from $E(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1^*, M_{i1} = m_1) - E(Y_{i1}|C_{i0} = E(C_{i0}), A_{i1} = a_1, M_{i1} = m_1) = \big(\lambda_0 + \lambda_{11}a_1^* + m_1(\lambda_{21} + \lambda_{31}a_1^*)\big) - \big(\lambda_0 + \lambda_{11}a_1 + m_1(\lambda_{21} + \lambda_{31}a_1)\big) = (\lambda_{11} + \lambda_{31}m_1)(a_1^* - a_1)$.

6. Specifically, I use the cluster bootstrap, which resamples at the level of the primary sampling unit rather than the individual observations, to adjust for the clustering of children within families in the PSID.

7. Missing values in the PSID are primarily due to sample attrition and, to a lesser degree, item-specific nonresponse. In addition, because the PSS does not include information on free lunch eligibility, subjects attending private schools, who compose between 6 to 9 percent of the analytic sample at each wave, are also missing data on the mediator of interest. For this group, I use measures of school racial composition, which are included in the PSS, along with all other variables outlined in the data and measures section to impute school poverty rates.

8. The contrast between neighborhoods at the 80th versus the 20th percentile of the national treatment distribution is roughly equivalent to a one and one-half standard deviation difference on the composite measure of neighborhood advantage.

9. Note that this approach to sensitivity analysis assumes that the partial effect of U_{i1} on Y_{i1} does not vary across C_{i0} or A_{i1}.

10. I focus on results for applied problem scores throughout the sensitivity analysis because this is the measure of academic achievement for which there is evidence of a significant neighborhood effect.

11. To put these values in perspective, the proportion of variation in the single measure of neighborhood poverty explained by the composite measure of neighborhood advantage is about 0.80 in the analytic sample used in this study. Thus, values of $\phi \ll 0.80$ for the socioeconomic composition of schools seem implausible.

REFERENCES

Aaronson, Daniel. 1998. "Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes." *Journal of Human Resources* 33(4):915-46.

Ainsworth, James W. 2002. "Why Does It Take a Village? The Mediation of Neighborhood Effects on Educational Achievement." *Social Forces* 81:117-52.

Anderson, Elijah. 1999. *Code of the Street: Decency, Violence, and the Moral Life of the Inner City*. New York: Norton.

Arum, Richard. 2000. "Schools and Communities: Ecological and Institutional Dimensions." *Annual Review of Sociology* 26:395-418.

Attewell, Paul. 2001. "The Winner-Take-All High School: Organizational Adaptations to Educational Stratification." *Sociology of Education* 74:267-95.

Auld, M. Christopher and Nirmal Sidhu. 2005. "Schooling, Cognitive Ability and Health." *Health Economics* 14:1019-34.

Bader, Michael D. M., Marnie Purciel, Paulette Yuosefzadeh and Kathryn M Neckerman. 2010. "Disparities in Neighborhood Food Environments: Implications of Measurement Strategies." *Economic Geography* 86:409-30.

Barr, Rebecca and Robert Dreeben. 1983. *How Schools Work*. Chicago: University of Chicago Press.

Battistich, Victor, Daniel Solomon, Dong-il Kim, Marilyn Watson and Eric Schaps. 1995. "Schools as Communities, Poverty Levels of Student Populations, and Students' Attitudes, Motives, and Performance: A Multilevel Analysis." *American Educational Research Journal* 32:627-58.

Borman, Geoffrey D. and N. Maritza Dowling. 2008. "Teacher Attrition and Retention: A Meta-Analytic and Narrative Review of the Research." *Review of Educational Research* 78:367-409.

Boyd, Donald, Hamilton Lankford, Susanna Loeb and James Wyckoff. 2005. "Explaining the Short Careers of High-Achieving Teachers in Schools with Low-Performing Students." *The American Economic Review* 95:166-71.

Brooks-Gunn, Jeanne, Greg J. Duncan, Pamela K. Klebanov and Naomi Sealand. 1993. "Do Neighborhoods Influence Child and Adolescent Development?". *American Journal of Sociology* 99(2):353-95.

Burdick-Will, Julia. 2013. "School Violent Crime and Academic Achievement in Chicago." *Sociology of Education* 86:10-34.

Caldwell, Bettye M and Robert H Bradley. 1984. *Home Observation for Measurement of the Environment (Home) - Revised Edition*. Little Rock, AR: University of Arkansas.

Carlson, Deven and Josua M. Cowen. 2014. "Student Neighborhoods, Schools, and Test Score Growth: Evidence from Milwaukee, Wisconsin." *Sociology of Education* 88:38-55.

Card, David and Jesse Rothstein. 2007. "Racial Segregation and the Black-White Test Score Gap." *Journal of Public Economics* 91:2158-84.

Chetty, Raj, John N Friedman and Jonah E Rockoff. 2014. "Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates." *American Economic Review* 104:2593-632.

Chetty, Raj, Nathaniel Hendren and Lawrence Katz. 2015. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Project." *http://scholar.harvard.edu/files/hendren/files/mto_paper.pdf*.

Choi, Kate H, R. Kelly Raley, Chandra Muller and Catherine Riegle-Crumb. 2008. "Class Composition: Socioeconomic Characteristics of Coursemates and College Enrollment." *Social Science Quarterly* 89:846-66.

Coleman, James S., Ernest Q. Campbell, Carol J. Hobson, James McPartland, Alexander M. Mood, Frederic D. Weinfeld and Robert L. York. 1966. *Equality of Educational Opportunity*. Washington, DC: U.S. Department of Health, Education, and Welfare.

Cook, Thomas D, Melissa R Herman, Meredith Phillips and Richard A Settersten. 2002. "Some Ways in Which Neighborhoods, Nuclear Families, Friendship Groups, and Schools Jointly Affect Changes in Early Adolescent Development." *Child Development* 73:1283-309.

Crosnoe, Robert. 2009. "Low-Income Students and the Socioeconomic Composition of Public High Schools." *American Sociological Review* 74(5):709-30.

Crowder, K. and L. Downey. 2010. "Interneighborhood Migration, Race, and Environmental Hazards: Modeling Microlevel Processes of Environmental Inequality." *American Journal of Sociology* 115(4):1110-49.

Davis, James A. 1966. "Campus as a Frog Pond: An Application of the Theory of Relative Deprivation to Career Decisions of College Men." *American Journal of Sociology* 72:17-31.

Deming, David J. 2011. "Better Schools, Less Crime?" *The Quarterly Journal of Economics* 126:2063-115.

Deming, David J., Justine S. Hastings, Thomas J. Kane, and Douglas O. Staiger. 2014. “School Choice, School Quality, and Postsecondary Attainment.” *American Economic Review* 104:991-1013.

Deming, David J. 2014. "Using School Choice Lotteries to Test Measures of School Effectiveness (Working Paper No. W19803)." *National Bureau of Economic Research*.

Efron, Bradley and Robert J. Tibshirani. 1993. *An Introduction to the Bootstrap*. New York: Chapman and Hall.

Ferryman, Kadija S., Xavier de Souza Briggs, Susan J. Popkin and Maria Rendon. 2008. "Do Better Neighborhoods for Mto Families Mean Better Schools?". *Metropolitan and Housing Communities* 3:1-12.

Galster, George C. 2012. "The Mechanism(S) of Neighbourhood Effects: Theory, Evidence, and Policy Implications." Pp. 23-56 in *Neighbourhood Effects Research: New Perspectives*, edited by M. van Ham, D. Manley, N. Bailey, L. Simpson and D. Maclennan. New York: Springer.

GeoLytics, Inc. 2013. *Neighborhood Change Database, 1970-2010 Tract Data*. New Brunswick, NJ: GeoLytics.

Goldsmith, Pat R. 2009. "Schools or Neighborhoods or Both? Race and Ethnic Segregation and Educational Attainment." *Social Forces* 87:1913-42.

Harding, David J. 2003. "Counterfactual Models of Neighborhood Effects: The Effect of Neighborhood Poverty on Dropping out and Teenage Pregnancy." *American Journal of Sociology* 109(3):676-719.

Harding, David J. 2009. "Collateral Consequences of Violence in Disadvantaged Neighborhoods." *Social Forces* 88(2):757-84.

Harding, David J. 2010. *Living the Drama: Community, Conflict, and Culture among Inner-City Boys*. Chicago: University of Chicago Press.

Harding, David J. 2011. "Rethinking the Cultural Context of Schooling Decisions in Disadvantaged Neighborhoods: From Deviant Subculture to Cultural Heterogeneity." *Sociology of Education* 84:322-39.

Harris, Douglas N. 2010. "How Do School Peers Influence Student Educational Outcomes? Theory and Evidence from Economics and Other Social Sciences." *Teachers College Record* 112:1163-97.

Heckman, James J. and Alan B. Krueger. 2004. *Inequality in America: What Role for Human Capital Policies?* Boston, MA: The MIT Press.

Heckman, James J. 2006. "Skill Formation and the Economics of Investing in Disadvantaged Children." *Science* 312:1900-1.

Hedges, Larry V, Richard Laine and Rob Greenwald. 1994. "Does Money Matter? A Meta-Analysis of Studies of the Effects of Differential School Inputs on Student Outcomes." *Education Researcher* 23:5-14.

Heuer, Ruth and Stephanie Stullich. 2011. "Comparability of State and Local Expenditures among Schools within Districts: A Report from the Study of School-Level Expenditures." Vol. Washington, DC: U.S. Department of Education.

Ho, Esther S and Douglas Willms. 1996. "Effects of Parental Involvement on Eighth-Grade Achievement." *Sociology of Education* 69:126-41.

Jencks, Christopher and Susan E. Mayer. 1990. "The Social Consequences of Growing up in a Poor Neighborhood." Pp. 111-86 in *Inner-City Poverty in the United States*, edited by L. E. Lynn and M. G. H. McGreary. Washington, D.C.: National Academy Press.

Johnson, Odis Jr. 2012. "A Systematic Review of Neighborhood and Institutional Relationships Related to Education." *Education and Urban Society* 44:477-511.

Kahlenberg, Richard D. 2001. *All Together Now: Creating Middle Class Schools through Public School Choice*. Washington, DC: Brookings.

LaForte, Erica M, Kevin S McGrew and Fredrick A Schrank. 2014. *Wj Iv Technical Abstract (Woodcock-Johnson Iv Assessment Service Bulletin No. 2).* Rolling Meadows, IL: Riverside.

Lanphear, Bruce P, Michael Weitzman and Shirley Eberly. 1996. "Racial Differences in Urban Children's Environmental Exposures to Lead." *American Journal of Public Health* 86:1460-63.

Lauen, Douglas Lee and S. Michael Gaddis. 2013. "Exposure to Classroom Poverty and Test Score Achievement: Contextual Effects of Selection?". *American Journal of Sociology* 118:943-79.

Levanthal, Tama and Jeanne Brooks-Gunn. 2004. "A Randomized Study of Neighborhood Effects on Low-Income Children's Educational Outcomes." *Developmental Psychology* 40:488–507.

Leventhal, T. and J. Brooks-Gunn. 2000. "The Neighborhoods They Live In: The Effects of Neighborhood Residence on Child and Adolescent Outcomes." *Psychological Bulletin* 126(2):309-37.

Massey, Douglas S. and Nancy A. Denton. 1993. *American Apartheid: Segregation and the Making of the Underclass*. Cambridge, MA: Harvard University Press.

Murnane, Richard J and Frank Levy. 2006. *Teaching the New Basic Skills: Principles for Educating Children to Thrive in a Changing Economy*. New York: Free Press.

National Center for Education Statistics. 2014a, "State Support for School Choice and Other Options, Tables 4.2-4.4". Retrieved August 17, 2015 (http://nces.ed.gov/programs/statereform/sss.asp).

National Center for Education Statistics. 2014b. "Common Core of Data School- and District-Level Datasets." edited by U. S. D. o. Education. Washington, DC.

National Center for Education Statistics. 2014c. "Private School Universe Survey Datasets." edited by U. S. D. o. Education. Washington, DC.

National Center for Education Statistics. 2015. "Revenues and Expenditures for Public Elementary and Secondary School Districts: School Year 2011-12." Vol. Washington, DC: U.S. Department of Education.

Orr, Larry, Judith D Feins, Robin Jacob, Erik Beecroft, Lisa Sanbonmatsu, Lawrence Katz, Jeffrey B Liebman and Jeffrey R Kling. 2003. *Moving to Opportunity: Interim Impacts Evaluation*. Washington, DC: U.S. Department of Housing and Urban Development.

Owens, Ann. 2010. "Neighborhoods and Schools as Competing and Reinforcing Contexts for Educational Attainment." *Sociology of Education* 83:287-311.

Panel Study of Income Dynamics. 2014. "Public- and Restricted-Use Datasets." edited by I. f. S. R. Survey Research Center, University of Michigan. Ann Arbor, MI

Pearl, Judea. 2000. *Causality: Models, Reasoning, and Inference*. Cambridge: Cambridge University Press.

Pfeffermann, Danny. 1993. "The Role of Sampling Weights When Modeling Survey Data." *International Statistical Review* 61:317-37.

Ponce, N. A., K. J. Hoggatt, M. Wilhelm and B. Ritz. 2005. "Preterm Birth: The Interaction of Traffic-Related Air Pollution with Economic Hardship in Los Angeles Neighborhoods." *American Journal of Epidemiology* 162(2):140-48.

Raudenbush, Stephen W, Marshall Jean and Emily Art. 2011. "Year-to-Year and Cumulative Impacts of Attending a High Mobility Elementary Schoolon Children's Mathematics Achievement in Chicago, 1995 to 2005." Pp. 359-76 in *Whither Opportunity?*, edited by G. J. Duncan and R. J. Murnane. New York: Russell Sage.

Reardon, Sean F. and Kendra Bischoff. 2011. "Income Inequality and Income Segregation." *American Journal of Sociology* 116(4):1092-153.

Rendon, Maria G. 2014. "Drop Out and ‘Disconnected’ Young Adults: Examining the Impact of Neighborhood and School Contexts." *The Urban Review* 46:169-96.

Rosenbaum, James E. 1995. "Changing the Geography of Opporuntity by Expanding Residential Choice: Lessons from the Gautreaux Program." *Housing Policy Debate* 6:231-69.

Rosenfeld, L., R. Rudd, G. L. Chew, K. Emmons and D. Acevedo-Garcia. 2010. "Are Neighborhood-Level Characteristics Associated with Indoor Allergens in the Household?". *Journal of Asthma* 47(1):66-75.

Royston, Patrick. 2005. "Multiple Imputation of Missing Values: Update." *The Stata Journal* 5(2):1-14.

Rubin, Donald B. 1974. "Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies." *Journal of Educational Psychology* 66(5):688-701.

Rubin, Donald B. 1987. *Multiple Imputation for Nonresponse in Surveys*. New York: J. Wiley & Sons.

Sampson, Robert J, Stephen W Raudenbush and Felton Earls. 1997. "Neighbourhoods and Violent Crime: A Multilevel Study of Collective Efficacy." *Science* 277:918-24.

Sampson, Robert J. 2001. "How Do Communities Undergird or Undermine Human Development? Relevant Contexts and Social Mechanisms." Pp. 3-30 in *Does It Take a Village? Community Effects on Children, Adolescents, and Families*, edited by A. Booth and N. Crouter. Mahwah, N.J.: Erlbaum.

Sampson, Robert J., Jeffrey D. Morenoff and Thomas Gannon-Rowley. 2002. "Assessing "Neighborhood Effects": Social Processes and New Directions in Research." *Annual Review of Sociology* 28:443-78.

Sampson, Robert J., Patrick Sharkey and Stephen W. Raudenbush. 2008. "Durable Effects of Concentrated Disadvantage on Verbal Ability among African-American Children." *Proceedings of the National Academy of Sciences* 105(3):845-52.

Sanbonmatsu, Lisa, Jeffrey R Kling, Greg J Duncan and Jeanne Brooks-Gunn. 2006. "Neighborhoods and Academic Achievement: Results from the Moving to Opportunity Experiment." *Journal of Human Resources* 41:649-91.

Sanbonmatsu, Lisa, Jens Ludwig, Lawrence Katz, Lisa A Gennetian, Greg J Duncan, Ronald C Kessler, Emma Adam, Thomas W McDade and Stacy T Lindau. 2011. *Moving to Opportunity for Fair Housing Demonstration Program: Final Impacts Evaluation*. Washington, DC: U.S. Department of Housing and Urban Development.

Saporito, Salvatore and Deenesh Sohoni. 2007. "Maping Educational Inequality: Concentrations of Poverty among Poor and Minority Students in Public Schools." *Social Forces* 85:1227-53.

Sharkey, Patrick and Felix Elwert. 2011. "The Legacy of Disadvantage: Multigenerational Neighborhood Effects on Cognitive Ability." *American Journal of Sociology* 116:1934-81.

Sharkey, Patrick. 2013. *Stuck in Place: Urban Neighborhoods and the End of Progress toward Racial Equality*. Chicago, IL: University of Chicago Press.

Sharkey, Patrick T. 2010. "The Acute Effect of Local Homocides on Children's Cognitive Performance." *Proceedings of the National Academy of Sciences* 107:11733-8.

Sharkey, Patrick T., Nicole Tirado-Strayer, Andrew V Papachristos and Cybele Raver. 2012. "The Effect of Local Violence of Children's Attention and Impulse Control." *American Journal of Public Health* 102:2287-93.

Steinberg, Laurence. 1997. *Beyond the Classroom: Why School Reform Has Failed and What Parents Need to Do*. New York: Simon & Schuster.

Tong, Shilu, Yasmin E von Schirnding and Tippawan Prapamontol. 2000. "Environmental Lead Exposure: A Public Health Problem of Global Dimensions." *Bulletin of the World Health Organization* 78:1068-73.

VanderWeele, Tyler J. 2009. "Marginal Structural Models for the Estimation of Direct and Indirect Effects." *Epidemiology* 20:18-26.

VanderWeele, Tyler J. 2015. *Explanation in Causal Inference*. New York: Oxford University Press.

Weiss, Christopher C., Marnie Purciel, Michael D. M. Bader, James W Quinn, Gina Lovasi, Kathryn M Neckerman and Andrew G Rundle. 2011. "Reconsidering Access: Park Facilities and Neighborhood Disamenties in New York City." *Journal of Urban Health* 88:297-310.

Willms, J. Douglas. 1986. "Social Class Segregation and Its Relationship to Pupils' Examination Results in Scotland." *American Sociological Review* 51:224-41.

Willms, J. Douglas. 2010. "School Composition and Contextual Effects on Student Outcomes." *Teachers College Record* 112:1008-38.

Wilson, William J. 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*. Chicago: University of Chicago Press.

Wilson, William J. 1996. *When Work Disappears: The World of the New Urban Poor*. New York: Vintage Books.

Winship, Christopher and Larry Radbill. 1994. "Sampling Weights and Regression Analysis." *Sociological Methods & Research* 23:230-57.

Winship, Christopher and Sanders Korenman. 1997. "Does Staying in School Make You Smarter? The Effect of Education on Iq in the Bell Curve." Pp. 215-34 in *Intelligence,* *Genes, and Success: Scientists Respond to the Bell Curve,* edited by B. Devlin, S. E. Fienberg, D. P. Resnick and K. Roeder. New York: Springer.

Wodtke, Geoffrey T., David J. Harding and Felix Elwert. 2011. "Neighborhood Effects in Temporal Perspective: The Impact of Long-Term Exposure to Concentrated Disadvantage on High School Graduation." *American Sociological Review* 76:713-36.

Wodtke, Geoffrey T. 2013. "Duration and Timing of Exposure to Neighborhood Poverty and the Risk of Adolescent Parenthood." *Demography* 50:1765-88.

Woodcock, Richard W and M. Bonner Johnson. 1989. *Tests of Achievement, Standard Battery (Form B)*. Chicago, IL: Riverside.

TABLES

Table 1. Longitudinal measurement strategy

	Time					
	1995	1997	1999	2001-03	2005	2007
Main survey	PSID95	PSID97	PSID99	PSID01/03	PSID05	PSID07
CDS survey	-	CDS97	-	CDS02	-	CDS07
Analytic sample						
8-12 year olds at CDS97	A_0	M_0, Y_0, C_0	A_1	M_1, Y_1	-	-
Age	6-10	8-12	10-14	13-17	-	-
3-7 year olds at CDS97	-	-	A_0	M_0, Y_0, C_0	A_1	M_1, Y_1
Age	-	-	6-10	8-12	11-15	13-17

Table 2. Sample characteristics

Variables	Mean	SD
Childhood measures (baseline controls)		
Prior treatment and mediator		
Neighborhood advantage index	–0.81	2.31
School poverty rate	.40	.30
Prior test scores		
Letter-word test score	104.02	18.62
Applied problem test score	105.71	16.74
Subject characteristics		
Black	.43	.49
Female	.49	.50
Age at baseline	10.00	1.41
Family characteristics		
PCG age at baseline	37.65	7.08
PCG education	12.84	2.44
Wealth (cube-root real dollars)	29.40	29.35
Income-to-needs ratio	3.01	2.48
Southern residence	.46	.50
Household cognitive stim. score	10.22	2.03
Family size	4.26	1.33
Family owns home	.65	.48
Head is married	.64	.48
Head is employed	.83	.38
Adolescent measures		
Focal treatment and mediator		
Neighborhood advantage index	–0.53	2.35
School poverty rate	.32	.27
Focal test scores		
Letter-word test score	100.41	19.30
Applied problem test score	100.17	15.61

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations.

Table 3. Joint treatment-mediator distribution during adolescence

n row cell		School poverty quintile					
		1	2	3	4	5	Total
Neighborhood advantage quintile	1	54 .08 .02	69 .10 .03	123 .17 .06	224 .32 .10	234 .33 .11	704
	2	72 .16 .03	104 .24 .05	101 .23 .05	92 .21 .04	69 .16 .03	438
	3	101 .29 .05	115 .33 .05	69 .20 .03	36 .10 .02	31 .09 .01	352
	4	148 .46 .07	79 .25 .04	46 .14 .02	33 .10 .02	15 .05 .01	321
	5	266 .68 .12	77 .20 .03	26 .07 .01	17 .04 .01	6 .01 .00	392
	Total	641	444	366	403	355	2208

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations.

Table 4. Total, direct, and indirect effects of neighborhood context on academic achievement during adolescence

Variable/estimand	School poverty		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	–.195	<.001	.053	.100	.048	.140	.107	.001	.100	.002
	(.052)		(.032)		(.032)		(.032)		(.032)	
School poverty					–.026	.336			–.044	.076
					(.027)				(.025)	
Nhood x school					–.001	.959			–.019	.310
					(.019)				(.019)	
Tot. effect			.079	.092			.161	.001		
			(.047)				(.049)			
Nat. direct effect					.071	.129			.147	.003
					(.047)				(.050)	
Nat. indirect effect					.008	.504			.019	.122
					(.012)				(.012)	
Prop. mediated					.10				.12	
Ctrl. direct effect					.071	.131			.153	.002
					(.048)				(.050)	
Prop. eliminated					.09				.05	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Models control for prior measures of the treatment, mediator, and outcome, as well as other characteristics of the subject and his/her family. The treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Table 5. Sensitivity of total and controlled direct effects on applied problem scores to hypothetical patterns of treatment-outcome and mediator-outcome confounding

Effect/type			gamma 1		gamma 2		gamma 3	
			est	ci	est	ci	est	ci
Total effect								
A→Y confnding	delta	1	.151	(.054, .248)	.141	(.044, .238)	.131	(.034, .228)
		2	.141	(.044, .238)	.121	(.024, .218)	.101	(.004, .198)
		3	.131	(.034, .228)	.101	(.004, .198)	.071	(–.026, .168)
Ctrl. direct effect								
A→Y confnding	$delta_m$	1	.143	(.045, .241)	.133	(.035, .231)	.123	(.025, .221)
		2	.133	(.035, .231)	.113	(.015, .211)	.093	(–.005, .191)
		3	.123	(.025, .221)	.093	(–.005, .191)	.063	(–.035, .161)
M→Y confnding	$delta_m$	–3	.183	(.085, .281)	.213	(.115, .311)	.243	(.145, .341)
		–2	.173	(.075, .271)	.193	(.095, .291)	.213	(.115, .311)
		–1	.163	(.065, .261)	.173	(.075, .271)	.183	(.085, .281)

Notes: Gamma represents the conditional mean difference in the outcome associated with a unit difference in the unobserved confounder. Delta represents the conditional mean difference in the unobserved confounder associated with a unit difference in treatment.

Table 6. Sensitivity of natural direct and indirect effects on applied problem scores to hypothetical patterns of treatment-outcome and mediator-outcome confounding

Effect/type			$gamma_m$ = 1 est	1 ci	2 est	2 ci	3 est	3 ci
Nat. direct effect								
A→Y confnding	$delta_m$	1	.137	(.040, .234)	.127	(.030, .224)	.117	(.020, .214)
		2	.127	(.030, .224)	.107	(.010, .204)	.087	(–.010, .184)
		3	.117	(.020, .214)	.087	(–.010, .184)	.057	(–.040, .154)
M→Y confnding	$delta_m$	–3	.177	(.080, .274)	.207	(.110, .304)	.237	(.140, .334)
		–2	.167	(.070, .264)	.187	(.090, .284)	.207	(.110, .304)
		–1	.157	(.060, .254)	.167	(.070, .264)	.177	(.080, .274)
Nat. indirect effect								
M→Y confnding	$delta_m$	–3	–.011	(–.035, .012)	–.041	(–.065, –.018)	–.071	(–.095, –.048)
		–2	–.001	(–.025, .022)	–.021	(–.045, .002)	–.041	(–.065, –.018)
		–1	.009	(–.015, .032)	–.001	(–.025, .022)	–.011	(–.035, .012)

Notes: Gamma represents the conditional mean difference in the outcome associated with a unit difference in the unobserved confounder. Delta represents the conditional mean difference in the unobserved confounder associated with a unit difference in treatment.

Table 7. Sensitivity of natural direct and indirect effects on applied problem scores to hypothetical patterns of treatment-mediator confounding

Effect/type			kappa					
			–3		–2		–1	
			est	ci	est	ci	est	ci
Nat. direct effect								
A→M confnding	delta	1	.147	(.050, .244)	.147	(.050, .244)	.147	(.050, .244)
		2	.148	(.051, .245)	.148	(.050, .245)	.147	(.050, .244)
		3	.148	(.051, .245)	.148	(.051, .245)	.147	(.050, .244)
Nat. indirect effect								
A→M confnding	delta	1	.016	(–.005, .037)	.017	(–.005, .038)	.018	(–.005, .040)
		2	.013	(–.005, .031)	.015	(–.005, .035)	.017	(–.005, .038)
		3	.010	(–.006, .026)	.013	(–.005, .031)	.016	(–.005, .037)

Notes: Kappa represents the conditional mean difference in the mediator associated with a unit difference in the unobserved confounder. Delta represents the conditional mean difference in the unobserved confounder associated with a unit difference in treatment.

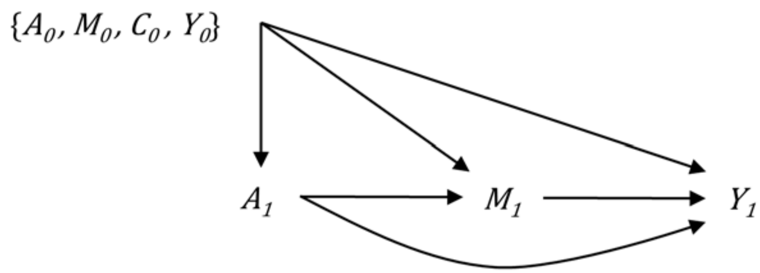
Table 8. Sensitivity of natural direct and indirect effects on applied problem scores to measurement error in the mediator

Effect	est	ci
Nat. direct effect		
Phi		
0.9	.149	(.052, .247)
0.8	.148	(.050, .245)
0.7	.146	(.048, .244)
0.6	.143	(.045, .242)
Nat. indirect effect		
Phi		
0.9	.012	(-.004, .028)
0.8	.013	(-.005, .032)
0.7	.015	(-.006, .036)
0.6	.018	(-.006, .042)

Notes: Phi represents the proportion of variance in the mismeasured mediator explained by the true mediator. These estimates are based on models of academic achievement that exclude the treatment-mediator interaction term.

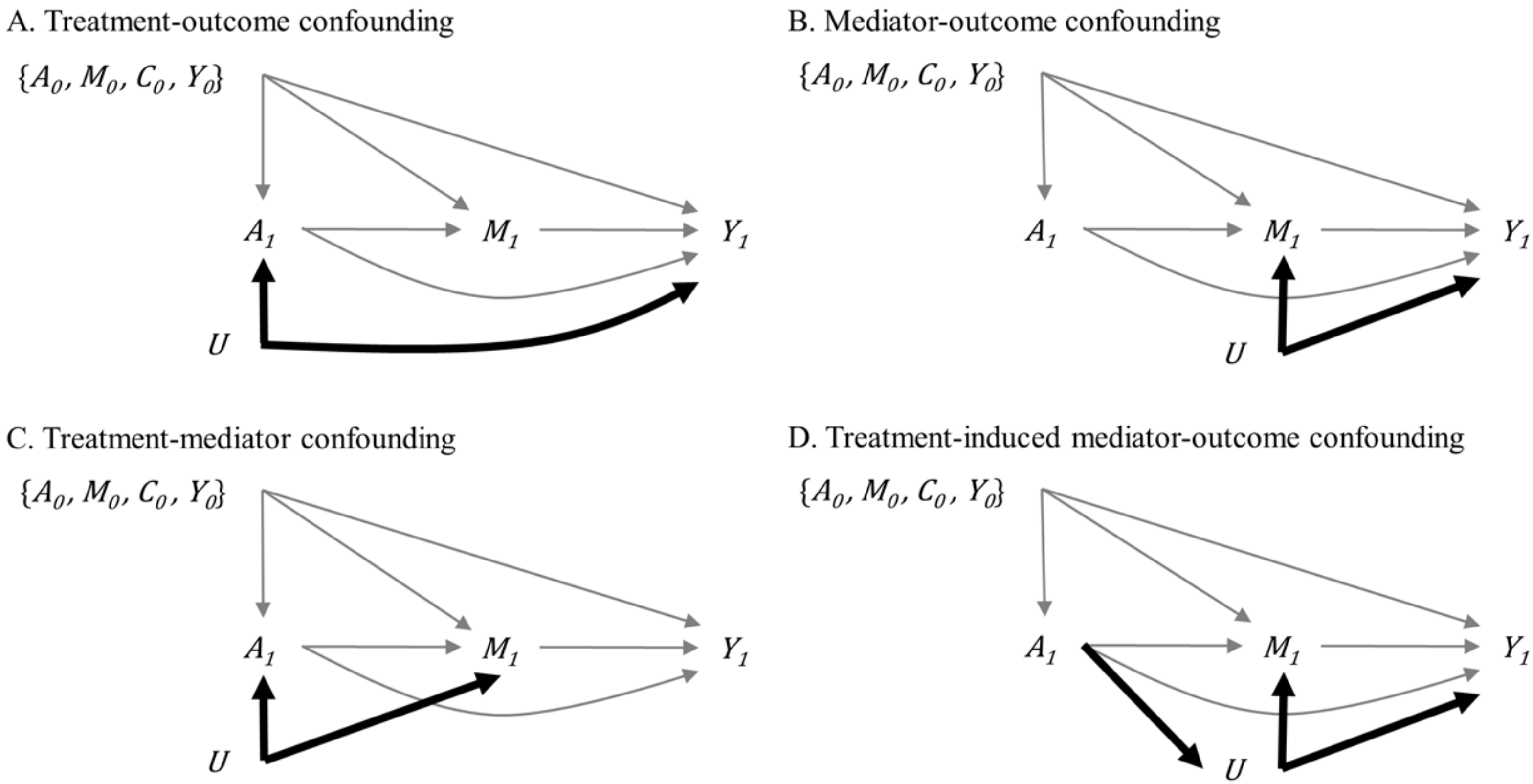
FIGURES

Figure 1. Directed acyclic graph depicting the hypothesized causal relationships between neighborhood context, school poverty, and academic achievement



Notes: A_t = neighborhood advantage, M_t = school poverty, C_t = covariates, and Y_t = academic achievement.

Figure 2. Directed acyclic graphs depicting patterns of unobserved confounding that would lead to bias in mediation analyses of neighborhood effects



Notes: A_t = neighborhood advantage, M_t = school poverty, C_t = covariates, Y_t = academic achievement, and U = a hypothetical unobserved covariate.

ONLINE SUPPLEMENT

Part A: The Composite Measure of Neighborhood Advantage

This section describes the composite measure of neighborhood advantage. Table A.1 presents bivariate correlations between the different neighborhood characteristics used to generate this composite measure: the poverty rate, the unemployment rate, median household income, the proportion of households that are female-headed, the proportion of residents age 25 or older without a high school diploma, the proportion of residents age 25 or older with a college degree, and the proportion of residents age 25 or older in managerial or professional occupations. All of these characteristics are highly correlated, with absolute values of the bivariate correlations consistently exceeding 0.50.

Table A.2 presents results from a principal components analysis (PCA) of these data. PCA is a dimension reduction technique that converts a high-dimensional set of correlated variables into a low-dimensional set of linearly uncorrelated “principal components” under the constraint that each successive component accounts for as much variability in the data as possible. Specifically, principal components are weighted linear combinations of the input variables, with weights given by an eigen decomposition of the correlation matrix. Table A.2 shows the weights used to construct the first principal component as well as the proportion of the total variance explained by this component. The first principal component is essentially a simple average of the different neighborhood characteristics with “disadvantaged” characteristics (e.g., the poverty rate) receiving positive weight and “advantaged” characteristics (e.g., the proportion of residents age 25 or older with a college degree) receiving negative weight. It accounts for 65 percent of the total variation in the data.

The composite measure of neighborhood advantage used in all mediation analyses is equal to the negation of this first principal component. Negating the component simply ensures that higher values are associated with more advantaged neighborhoods and that lower values are associated with more disadvantaged neighborhoods. Table A.3 presents descriptive statistics for each neighborhood characteristic, separately by quintiles of this composite measure. In the first quintile of neighborhoods, which are highly disadvantaged, about 30 percent of households are below the poverty line; 13 percent of resident adults are unemployed; and nearly 40 percent of resident adults have not earned a high school diploma. By contrast, in the fifth quintile of neighborhoods, which are highly advantaged, only 4 percent of households are below the poverty line; 4 percent of resident adults are unemployed; and just 6 percent of resident adults have not earned a high school diploma.

Table A.1. Correlation matrix for neighborhood socioeconomic characteristics

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Percent mgr/prof workers	1.00						
(2) Median income	.73	1.00					
(3) Percent college graduates	.93	.72	1.00				
(4) Percent without high school diploma	–.73	–.63	–.71	1.00			
(5) Percent female-headed households	–.41	–.54	–.39	.46	1.00		
(6) Percent in poverty	–.52	–.65	–.44	.69	.70	1.00	
(7) Percent unemployed	–.44	–.46	–.41	.54	.62	.72	1.00

Notes: Results based on all U.S. census tract-years pooled across 1995 to 2007.

Table A.2. Weights from principal component analysis (PCA) of neighborhood socioeconomic characteristics

Variables	1st PC Weight
Percent mgr/prof workers	–.400
Median income	–.394
Percent college graduates	–.386
Percent without high school diploma	.398
Percent female-headed households	.334
Percent in poverty	.388
Percent unemployed	.341
Component variance	4.568
Proportion total variance explained	.653

Notes: Results based on all U.S. census tract-years pooled across 1995 to 2007. PCA is based on the correlation matrix.

Table A.3. Neighborhood socioeconomic characteristics by advantage index quintiles

Variable	Neighborhood advantage index				
	1st quintile	2nd quintile	3rd quintile	4th quintile	5th quintile
	Mean	Mean	Mean	Mean	Mean
Percent mgr/prof workers	.18	.24	.29	.37	.52
Median income ($1,000)	25.94	34.90	41.98	52.36	77.69
Percent college graduates	.09	.14	.19	.29	.50
Percent without high school diploma	.38	.24	.17	.12	.06
Percent female-headed households	.43	.28	.22	.19	.13
Percent in poverty	.30	.16	.10	.07	.04
Percent unemployed	.13	.08	.06	.05	.04

Notes: Results based on all U.S. census tract-years pooled across 1995 to 2007.

Part B: Parallel Analyses with Alternative Measures of School Context

This section reports estimates from parallel analyses based on alternative measures of school context. Table B.1 presents bivariate correlations between the composite measure of neighborhood advantage, school poverty, and then several other measures of school context obtained from the CCD and PSS, including the percentage of a school's student body who identify as black, the school's teacher-pupil ratio, and the log of the school district's per-pupil expenditures. Several patterns are evident in these data. First, aside from the school poverty rate and school racial composition, none of the other school characteristics are very highly correlated with the composite measure of neighborhood advantage. For example, the bivariate correlation between neighborhood advantage and school poverty is –0.56, but the bivariate correlation between neighborhood advantage and per-pupil expenditures is only 0.12. The weak associations between neighborhood context and alternative measures of school context at the bivariate level suggest that these school characteristics are unlikely to be very important mediators of neighborhood effects.

Second, aside from the school poverty rate and school racial composition, the pairwise correlations between school characteristics in Table B.1 are also not very strong. While the correlation between the school poverty rate and the percentage of a school's student body who identify as black is 0.54, none of the other pairwise correlations between school characteristics exceed 0.25, and several are close to zero. The weak associations between these alternative measures of school context preclude the construction of a composite measure of school advantage similar to the composite measure of neighborhood advantage described previously. Any composite measure based on weakly correlated input characteristics will have low reliability and will not account for a sufficient proportion of variance in the multivariate distribution.

Tables B.2, B.3, and B.4 present total, natural direct and indirect, and controlled direct effect estimates based on measures of school racial composition, the teacher-pupil ratio, and per-pupil expenditures, respectively. None of the effect estimates provide any indication that these alternative measures of school context mediate neighborhood effects on academic achievement. Across all of these analyses, the estimated direct effects are substantively large, statistically significant, and comparable to the total effect of neighborhood context, while the estimated natural indirect effects are close to zero and statistically insignificant. Furthermore, controlled direct effects estimated from models that jointly control for school poverty, school racial composition, the teacher-pupil ratio, and per-pupil expenditures also provide little evidence that these school characteristics mediate neighborhood effects when considered simultaneously rather than separately (results not shown, available upon request).

Table B.5 presents bivariate correlations between the composite measure of neighborhood advantage, school poverty, and then several other measures of school context obtained from the 2007 NCES Teacher Compensation Survey (TCS), including the percentage of teachers with graduate degrees, the average number of years of work experience among teachers, and the average base salary of teachers. The TCS is a relatively new pilot survey that was only conducted in 16 participating states. The participating states include Arizona, Colorado, Florida, Idaho, Iowa, Kansas, Kentucky, Louisiana, Maine, Minnesota, Mississippi, Missouri, Nebraska, Oklahoma, South Carolina, and Texas. Thus, school-level measures from the TCS can only be matched to the subset of respondents who were 13 to 17 years old at the 2007 wave of the CDS and who were living in one of these states. Although this subsample includes just $n = 247$ subjects, analyses based on the TCS can still shed some light on whether teacher human capital is an important mediator of neighborhood effects during adolescence.

The bivariate correlations in Table B.5 indicate that the association between teacher human capital and neighborhood advantage is fairly weak. For example, the bivariate correlation between neighborhood advantage and the percentage of teachers with graduate degrees is just 0.13. The strongest of these correlations is between the composite measure of neighborhood advantage and average teacher base salary, which registers at only 0.21. By comparison, the bivariate correlation between neighborhood advantage and school poverty in this subsample is –0.68. As before, the rather weak bivariate associations between neighborhood context and aggregate measures of teacher human capital suggest that these alternative school characteristics are unlikely to be especially important mediators of neighborhood effects. Moreover, the correlations between different aggregate measures of teacher human capital, which range from 0.13 to 0.32, are also insufficiently strong to support the construction of a composite measure of school advantage.

Tables B.6, B.7, and B.8 present total, natural direct and indirect, and controlled direct effect estimates based on the percentage of teachers with graduate degrees, average teacher work experience, and average teacher base salary, respectively. Because these analyses are based on a substantially smaller sample than those presented in the main text, the regression models from which effect estimates are computed must be simplified considerably. Rather than adjust for all of the baseline controls outlined in the main text, these models control only for race and prior measures of academic achievement. This approach accommodates the relatively small number of respondents who can be matched to the TCS while still providing some protection against confounding bias. The effect estimates in Tables B.6 to B.8 provide little evidence that any of these alternative school-level measures mediate neighborhood effects on academic achievement during adolescence. Although all of these estimates are relatively imprecise owing to the small sample size, the estimated direct effects on applied problem scores are generally large, marginally significant, and comparable to the total effect, while the estimated natural indirect effects are close to zero and do not even approach conventional significance thresholds. These results are highly consistent with those presented in the main text.

Table B.1. Correlation matrix for neighborhood advantage and alternative measures of school context from the NCES Common Core of Data measured during adolescence

Variables	(1)	(2)	(3)	(4)	(5)
(1) Neighborhood advantage index	1.00				
(2) School poverty	–.56	1.00			
(3) School percent black	–.48	.54	1.00		
(4) School teacher-pupil ratio	–.02	–.02	–.13	1.00	
(5) District per-pupil expenditures (log)	.12	–.05	.08	–.23	1.00

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations.

Table B.2. Effects of neighborhood context during adolescence as mediated by school racial composition (BLK)

Variable/estimand	School BLK		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	–.030	.312	.072	.025	.072	.025	.113	.001	.114	.001
	(.030)		(.032)		(.032)		(.033)		(.033)	
School BLK					–.029	.467			–.095	.007
					(.039)				(.035)	
Nhood x school					–.008	.680			–.033	.084
					(.018)				(.019)	
Tot. effect			.108	.021			.169	.001		
			(.047)				(.050)			
Nat. direct effect					.108	.022			.171	.001
					(.047)				(.051)	
Nat. indirect effect					.002	.491			.006	.357
					(.003)				(.006)	
Prop. mediated					.01				.03	
Ctrl. direct effect					.108	.022			.171	.001
					(.047)				(.051)	
Prop. eliminated					.00				–.01	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Table B.3. Effects of neighborhood context during adolescence as mediated by the school teacher-pupil ratio (TPR)

Variable/estimand	School TPR		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	–.115	.013	.072	.022	.072	.022	.122	<.001	.120	<.001
	(.047)		(.031)		(.031)		(.032)		(.032)	
School TPR					.003	.881			–.013	.529
					(.022)				(.021)	
Nhood x school					.012	.456			–.006	.711
					(.017)				(.016)	
Tot. effect			.107	.019			.182	<.001		
			(.046)				(.050)			
Nat. direct effect					.109	.018			.180	<.001
					(.046)				(.050)	
Nat. indirect effect					–.003	.611			.003	.544
					(.005)				(.005)	
Prop. mediated					–.03				.02	
Ctrl. direct effect					.107	.019			.180	<.001
					(.046)				(.050)	
Prop. eliminated					.00				.01	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Table B.4. Effects of neighborhood context during adolescence as mediated by district per-pupil expenditures (EXP)

Variable/estimand	District EXP (log)		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	.035	.315	.072	.022	.074	.019	.121	<.001	.124	<.001
	(.035)		(.031)		(.031)		(.032)		(.032)	
District EXP (log)					.019	.586			–.008	.814
					(.035)				(.033)	
Nhood x district					–.015	.316			–.010	.438
					(.015)				(.013)	
Tot. effect			.107	.019			.182	<.001		
			(.046)				(.049)			
Nat. direct effect					.111	.016			.186	<.001
					(.046)				(.050)	
Nat. indirect effect					.000	.931			–.001	.714
					(.003)				(.003)	
Prop. mediated					.00				–.01	
Ctrl. direct effect					.110	.017			.185	<.001
					(.046)				(.050)	
Prop. eliminated					–.03				–.02	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Table B.5. Correlation matrix for neighborhood advantage and alternative measures of school context from the TCS measured during adolescence

Variables	(1)	(2)	(3)	(4)	(5)
(1) Neighborhood advantage index	1.00				
(2) School poverty	–.68	1.00			
(3) Percent of teachers with grad. degrees	.13	–.09	1.00		
(4) Average years of teacher experience	.13	–.18	.32	1.00	
(5) Average teacher base salary	.21	–.25	.18	.13	1.00

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 7 and who could be matched to a school in the 2007 TCS when they were between age 13 and 17. Results are combined estimates from 100 imputations.

Table B.6. Effects of neighborhood context during adolescence as mediated by the proportion of teachers with a graduate degree (GRDEG)

	School GRDEG		Letter-word scores				Applied problem scores			
Variable/estimand	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	.132	.118	.050	.368	.050	.390	.093	.062	.102	.042
	(.084)		(.055)		(.058)		(.049)		(.050)	
School GRDEG					.005	.908			–.019	.615
					(.046)				(.038)	
Nhood x school					–.004	.929			–.049	.140
					(.048)				(.033)	
Tot. effect			.075	.390			.139	.054		
			(.087)				(.072)			
Nat. direct effect					.075	.415			.159	.034
					(.092)				(.075)	
Nat. indirect effect					.000	.986			–.014	.317
					(.019)				(.014)	
Prop. mediated					.00				–.10	
Ctrl. direct effect					.074	.409			.154	.037
					(.090)				(.074)	
Prop. eliminated					.00				–.11	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 7 and who could be matched to a school in the 2007 TCS. Results are combined estimates from 100 imputations. Models control for race and prior measures of the outcome only. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Table B.7. Effects of neighborhood context during adolescence as mediated by school average teacher experience (TEXP)

Variable/estimand	School TEXP		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	.054	.470	.050	.368	.049	.375	.093	.062	.095	.051
	(.075)		(.055)		(.056)		(.049)		(.049)	
School TEXP					.047	.265			.033	.388
					(.042)				(.038)	
Nhood x school					.025	.566			.036	.393
					(.044)				(.042)	
Tot. effect			.075	.390			.139	.054		
			(.087)				(.072)			
Nat. direct effect					.073	.400			.142	.046
					(.087)				(.071)	
Nat. indirect effect					.006	.670			.005	.647
					(.013)				(.011)	
Prop. mediated					.08				.04	
Ctrl. direct effect					.074	.398			.143	.044
					(.088)				(.071)	
Prop. eliminated					.01				–.03	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 7 and who could be matched to a school in the 2007 TCS. Results are combined estimates from 100 imputations. Models control for race and prior measures of the outcome only. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Table B.8. Effects of neighborhood context during adolescence as mediated by school average teacher salary (TSAL)

Variable/estimand	School TSAL		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	.219	.012	.050	.368	.033	.540	.093	.062	.094	.065
	(.087)		(.055)		(.054)		(.049)		(.051)	
School TSAL					.098	.018			.065	.119
					(.041)				(.042)	
Nhood x school					–.014	.668			–.053	.142
					(.034)				(.036)	
Tot. effect			.075	.390			.139	.054		
			(.087)				(.072)			
Nat. direct effect					.052	.551			.150	.048
					(.088)				(.076)	
Nat. indirect effect					.027	.236			.005	.831
					(.023)				(.021)	
Prop. mediated					.36				.03	
Ctrl. direct effect					.050	.565			.141	.061
					(.087)				(.075)	
Prop. eliminated					.33				–.02	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 7 and who could be matched to a school in the 2007 TCS. Results are combined estimates from 100 imputations. Models control for race and prior measures of the outcome only. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.

Part C: Weighted Estimates

This section reports estimates that are weighted to adjust for the oversampling of low-income families in the PSID and also for nonrandom attrition. Tables C.1 to C.2 report weighted descriptive statistics analogous to those reported in the main text that approximate population distributions for the target cohort of children. Table C.3 reports weighted estimates of the causal parameters in Equations 1 to 3. These estimates are very similar to the unweighted estimates reported in the main text, which suggests that the regression models sufficiently control for all relevant aspects of the sample design without the use of weights. Standard errors for the weighted estimates are larger than those for the unweighted estimates, which reflects the inefficiency of additionally using weights to adjust for features of the survey design for which the regression model already adjusts directly (Winship and Radbill 1994). Table C.3 also contains results from "design ignorability tests" that evaluate the null hypothesis that the weighted and unweighted estimators converge in probability (Pfeffermann 1993). These tests are performed by conducting conventional heteroscedasticity-robust F-tests to evaluate the joint significance of interaction terms between the covariates and the weights in an unweighted regression model. P-values from these tests show that the null hypothesis is not rejected in any of these models at conventional significance thresholds, indicating that the weights can be safely ignored in the mediation analysis.

Table C.1. Weighted sample characteristics

Variables	Mean	SD
Childhood measures		
Prior treatment and mediator		
Neighborhood advantage score	–.38	2.25
School poverty rate	.35	.29
Prior test scores		
Letter-word test score	106.93	19.04
Applied problem test score	108.70	16.83
Subject characteristics		
Black	.18	.38
Female	.50	.50
Age at baseline	10.05	1.42
Family characteristics		
PCG age at baseline	38.10	6.56
PCG education	12.95	2.73
Wealth (cube-root real dollars)	34.44	30.46
Income-to-needs ratio	3.33	2.63
Southern residence	.34	.48
Household cognitive stim. score	10.41	2.02
Family size	4.41	1.31
Family owns home	.72	.45
Head is married	.73	.44
Head is employed	.86	.35
Adolescent measures		
Focal treatment and mediator		
Neighborhood advantage score	–.10	2.30
School poverty rate	.28	.25
Focal test scores		
Letter-word test score	103.62	19.43
Applied problem test score	103.39	15.83

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations and weighted to adjust for the PSID-CDS complex sampling design.

Table C.2. Weighted joint treatment-mediator distribution during adolescence

n row cell		School poverty quintile					
		1	2	3	4	5	Total
Neighborhood advantage quintile	1	41 .08 .02	51 .10 .02	89 .18 .04	175 .35 .08	149 .30 .07	504
	2	81 .18 .04	116 .26 .05	113 .26 .05	74 .17 .03	56 .13 .03	440
	3	130 .31 .06	141 .34 .06	73 .18 .03	40 .10 .02	31 .08 .01	415
	4	171 .48 .08	91 .25 .04	50 .14 .02	32 .09 .01	13 .04 .01	357
	5	348 .71 .16	91 .19 .04	30 .06 .01	18 .04 .01	6 .01 .00	493
	Total	770	490	354	339	255	2208

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations and weighted to adjust for the PSID-CDS complex sampling design.

Table C.3. Weighted estimates of causal parameters in models of exposure to school poverty, letter-word scores, and applied problem scores

Variable/estimand	School Poverty		Letter-word scores				Applied problem scores			
	Eq. 2		Eq. 1		Eq. 3		Eq. 1		Eq. 3	
	est	pval	est	pval	est	pval	est	pval	est	pval
Nhood advantage	–.203	.002	.073	.129	.067	.169	.134	.005	.123	.010
	(.064)		(.048)		(.049)		(.047)		(.048)	
School poverty					–.032	.353			–.045	.187
					(.034)				(.034)	
Nhood x school					.005	.838			–.018	.510
					(.026)				(.027)	
Design ignorability test		.654		.993		.961		.658		.922

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Treatment, mediator, and outcome are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect. The design ignorability tests evaluate the null hypothesis that the weighted and unweighted estimators converge in probability.

Part D: Regression Model Specification Checks

Regression estimation of direct and indirect effects requires correctly specified models for $E(M_{i1}|C_{i0}, A_{i1})$ and $E(Y_{i1}|C_{i0}, A_{i1}, M_{i1})$. This section explores several more flexible specifications for these models than are considered in the main text. Throughout this section, I use the term "causal function" to refer to those components of the model that involve the effects of treatment or the mediator during adolescence, and the term "nuisance function" to refer to those components of the model that involve only the baseline controls. Note that unbiased regression estimation of direct and indirect effects requires that both the causal and nuisance functions in these models are correctly specified.

Table D.1 presents parameter estimates from models with more flexible causal functions that allow the effects of treatment and the mediator to vary across levels of several other control variables in the model, including race, gender, and family income. In general, there is little evidence of effect heterogeneity across levels of these controls. The coefficients attached to the race, gender, and income interactions with treatment and the mediator are all substantively small and do not reach conventional significance thresholds. Table D.2 presents estimates for the causal parameters of interest from models with more flexible nuisance functions that include cubic polynomials for all continuous control variables or that include all possible two-way interactions between control variables. Estimates of the causal parameters from these more flexible models are nearly identical to those reported in the main text. Together, these ancillary results suggest that key findings from the mediation analysis are robust to alternative specifications of the regression models on which they are based.

Table D.1. Causal function specification checks

Variable	Applied problem scores						School poverty					
	Model A			Model B			Model C			Model D		
	est	se	pval	est	se	pval	est	se	pval	est	se	pval
(A) Nhood advantage	.103	(.034)	.003	.098	(.034)	.004	–.189	(.052)	<.001	–.192	(.052)	<.001
(B) School poverty	–.042	(.026)	.111	–.047	(.026)	.070						
(C) Nhood x school	–.018	(.023)	.435	–.025	(.023)	.267						
black x (A)	–.028	(.052)	.587				–.052	(.053)	.321			
black x (B)	–.013	(.047)	.783									
black x (C)	.002	(.046)	.962									
female x (A)	–.020	(.039)	.607				–.030	(.040)	.459			
female x (B)	–.014	(.042)	.741									
female x (C)	.027	(.035)	.452									
family income x (A)				–.017	(.023)	.464				.030	(.020)	.120
family income x (B)				–.006	(.028)	.840						
family income x (C)				.000	(.018)	.993						

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Treatment, mediator, and outcomes are standardized to have zero mean and unit variance. P-values are from two-sided z-tests of no effect.

Table D.2. Nuisance function specification checks

Variable	Applied problem scores				School poverty			
	Model E		Model F		Model G		Model H	
	est	pval	est	pval	est	pval	est	pval
Nhood advantage	.097	.003	.093	.006	–.202	<.001	–.218	<.001
	(.033)		(.034)		(.052)		(.053)	
School poverty	–.042	.090	–.044	.087				
	(.025)		(.026)					
Nhood x school	–.017	.422	–.019	.413				
	(.021)		(.023)					
Nuisance function description	base model + cubic poly-nomials		base model + two-way interactions		base model + cubic poly-nomials		base model + two-way interactions	

Notes: Sample includes respondents who were interviewed at the 1997 wave of the CDS between age 3 and 12. Results are combined estimates from 100 imputations. Treatment, mediator, and outcomes are standardized to have zero mean and unit variance. Standard errors are reported in parentheses. P-values are from two-sided z-tests of no effect.