INTRODUCTION

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).

It is commonly hypothesized that neighborhood effects are mediated by the school environment (Arum 2000, Ferryman et al. 2008, Jencks and Mayer 1990, Johnson 2012, Leventhal and Brooks-Gunn 2000, Sanbonmatsu et al. 2006, Sanbonmatsu et al. 2011, Wilson 1987). For example, neighborhood context directly affects the socioeconomic composition of schools because school assignment rules are based, at least in part, on residential location, and schools composed predominantly of students from poor families may have fewer resources, a lower quality of instruction, and a more disruptive learning environment (Harris 2010, Willms 2010). Thus, the differences in school composition linked to differences in neighborhood contexts are expected to significantly affect academic achievement.

Although theory and prior research suggest a potentially important mediating role for schools in transmitting the effects of neighborhoods on academic achievement, 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 several prior studies have considered the joint effects of neighborhoods and schools on educational outcomes – with some finding mainly neighborhood effects and others finding mainly school effects – they are all

academic achievement, 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.

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 important 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

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 (e.g., Battistich et al. 1995, Choi et al. 2008, Coleman et al. 1966, Halpern-Manners 2016, Kahlenberg 2001, Rumberger and Palardy 2005, Schellenberg 1999, Willms 1986, Willms 2010). For example, prior research documents significant negative associations between school-level poverty rates and academic expectations, aspirations, and test scores (Battistich et al. 1995, Halpern-Manners 2016, Rumberger and Palardy 2005, Schellenberg 1999, 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). 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

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 widely 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 effect 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 from a sample of 3,563 children in the PSID main panel who were between the ages of 0 and 12. It collected additional data from this sample at follow-up waves in 2002-2003 and 2007.

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. This measure is the most widely used indicator of student socioeconomic composition in research on school effects (e.g., Ainsworth 2002, Battistich et al. 1995, Halpern-Manners 2016, Lauen and Gaddis 2013). 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] In all multivariate analyses, the mediator is rescaled by its standard deviation but is not mean centered.

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, outcome measures 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

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 natural indirect 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 bottom rows of the lower panel in Table 4, indicate that the total effects of neighborhood context on academic achievement are not significantly mediated by the socioeconomic composition of schools. For

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 minor 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 modest in practical terms and is not statistically significant at conventional thresholds. 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., $\hat{\theta}_{11}(1.5) = -0.195(1.5) = -0.293$).[6] 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 large reduction in exposure to school poverty induced by moving from a more disadvantaged to a more advantaged neighborhood has only a small effect on academic achievement.

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.

Table 5 presents bias-adjusted point estimates and confidence intervals for the total effect of neighborhood context on applied problem scores.[7] 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 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.

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

Effect/type			gamma 1		gamma 2		gamma 3	
			est	ci	est	ci	est	ci
Total effect								
A⟶Y confnding	delta	1	.151	(.057, .246)	.141	(.047, .236)	.131	(.037, .226)
		2	.141	(.047, .236)	.121	(.027, .216)	.101	(.007, .196)
		3	.131	(.037, .226)	.101	(.007, .196)	.071	(–.023, .166)

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.

The sensitivity of natural 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}.[8] 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 upper panel of Table 6 present estimates of the natural 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 natural direct effect estimates remain statistically significant and substantively large except under extreme levels of unobserved treatment-outcome confounding.

The sensitivity of natural direct effects to unobserved mediator-outcome confounding is assessed using these 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 natural 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* a spurious association between the treatment and outcome through the mediator-outcome confounder (Elwert and Winship 2014). In this situation, the direction of the induced association is determined by the effect of the unobserved confounder on the mediator, by the effect of the treatment on the mediator, and by the effect of the unobserved confounder on the outcome. As documented previously, the effect of neighborhood advantage on 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 a variable have effects that operate in the same direction, conditioning on that variable 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 neighborhood must have parents who do value 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 natural 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.

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	gamma$_m$ 1 ci	gamma$_m$ 2 est	gamma$_m$ 2 ci	gamma$_m$ 3 est	gamma$_m$ 3 ci
Nat. direct effect								
A→Y confnding	delta$_m$	1	.137	(.042, .232)	.127	(.032, .222)	.117	(.022, .212)
		2	.127	(.032, .222)	.107	(.012, .202)	.087	(–.008, .182)
		3	.117	(.022, .212)	.087	(–.008, .182)	.057	(–.038, .152)
M→Y confnding	delta$_m$	–3	.177	(.082, .272)	.207	(.112, .302)	.237	(.142, .332)
		–2	.167	(.072, .262)	.187	(.092, .282)	.207	(.112, .302)
		–1	.157	(.062, .252)	.167	(.072, .262)	.177	(.082, .272)
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.

To assess the sensitivity of natural 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 upper panel of Table 6 present point estimates and confidence intervals for the natural direct effect that are adjusted for this type of confounding. These results indicate that estimates of the natural 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 natural direct effects. 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 natural direct effect, 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 conditional mean difference in exposure to school poverty associated with a unit change in a hypothetical treatment-mediator confounder. 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 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.

Causal inferences about natural direct and indirect effects are also based on the assumption that the models in Equations 2 and 3 are correctly specified. Part D of the Online Supplement reports results from a variety of different specifications for these regression models, including several that permit extensive nonlinearities and several others that include treatment and mediator interactions with baseline controls (e.g., with respondent race and gender). Results from these different specifications indicate that the reported estimates are highly robust.

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

Effect/type			kappa –3 est	kappa –3 ci	kappa –2 est	kappa –2 ci	kappa –1 est	kappa –1 ci
Nat. direct effect								
A→M confnding	delta	1	.147	(.053, .242)	.147	(.052, .242)	.147	(.052, .242)
		2	.148	(.053, .243)	.148	(.053, .242)	.147	(.052, .242)
		3	.148	(.053, .243)	.148	(.053, .243)	.147	(.053, .242)
Nat. indirect effect								
A→M confnding	delta	1	.016	(–.005, .036)	.017	(–.005, .038)	.018	(–.005, .040)
		2	.013	(–.005, .031)	.015	(–.005, .034)	.017	(–.005, .038)
		3	.010	(–.006, .025)	.013	(–.005, .031)	.016	(–.005, .036)

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.

Finally, in addition to unobserved confounding and model misspecification, measurement error in the mediator can also lead to bias in estimates of natural direct and indirect effects. This type of measurement error is particularly concerning because it would tend to inflate estimates of natural direct effects and deflate estimates of natural indirect effects, potentially obscuring an important mediating role for school context in the present study. Moreover, because this study measures the socioeconomic composition of schools with just a single indicator of student poverty rather than with a composite index based on multiple different socioeconomic characteristics – as is the case for neighborhood composition – it adopts a measure of school context that is arguably less reliable and more prone to error than its measure of neighborhood context. Part E of the Online Supplement assesses the sensitivity of natural direct and indirect effects to measurement error in the mediator. Results from this analysis indicate that estimates of the natural direct effect remain substantively large and statistically significant, while estimates of the natural indirect effect remain substantively small and statistically insignificant, even under extreme levels of measurement error in the mediator.

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 school poverty mediates the effects of neighborhood context on academic achievement during adolescence. Using appropriate sequential measurements of the treatment, mediator, and outcome together with counterfactual methods, it finds that adolescent exposure to an advantaged rather than disadvantaged neighborhood substantially 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 small impact on academic achievement, the socioeconomic composition of schools does not appear to be a very important mediator of neighborhood effects during adolescence. 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 school poverty during adolescence, and they suggest that neighborhood effects during this developmental period are primarily due to mediating factors not directly linked to the socioeconomic composition of schools, 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. In other words, overcoming the harmful effects of spatially concentrated poverty 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 and corroborating mediation analyses, however, the policy implications of the present study are preliminary and somewhat speculative.

The results of this analysis are also inconsistent, at least in part, with several prior studies that attempt to estimate the joint effects of neighborhood and school contexts on academic achievement (e.g., Cook et al. 2002, Carlson and Cowen 2014). 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. 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 appear to be substantively small and statistically insignificant during this developmental period.

There are a variety of possible explanations for these conflicting results, including differences in sampling design and in the measurement of key variables between studies, but two possible explanations stand out as particularly important. First, compared to prior studies, the present analysis estimates adolescent contextual effects while controlling for a much more extensive set of confounders, including baseline measures of the treatment, mediator, and outcome. Recent methodological research suggests that inferences about school effects are especially sensitive to the extent to which the study design controls for confounding: designs with less rigorous controls tend to yield larger estimates of school effects, while alternative designs with more rigorous controls yield smaller estimates that are often substantively trivial (Lauen and Gaddis 2013). Results from the present study resonate with these findings and with the results of other studies that report relatively small school effects after controlling for an extensive set of background characteristics (e.g., Ainsworth 2002, Coleman et al. 1966, Card and Rothstein 2007).

Second, compared to prior studies, the present analysis focuses on a later developmental period. Specifically, 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 contextual exposures during childhood. Although this aspect of the research design provides considerable protection against confounding bias, it is not without limitations.

Prior research suggests that neighborhood effects are more pronounced during adolescence (Wodtke 2013, Wodtke and Almirall 2015, Wodtke et al. 2016), while several other studies suggest that school effects may be strongest earlier during childhood (e.g., Heckman and Krueger 2004, Heckman 2006). If neighborhood and school effects predominate during different developmental periods, then the present study, by focusing only on adolescence, may obscure a more important mediating role for the school environment during childhood. Thus, results from this analysis should not be extrapolated to other developmental periods, and an important direction for future research will be to investigate neighborhood effect mediation throughout the entire early life course.

Another limitation of the present study is that it focuses on a single dimension of school context—the student poverty rate. 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. To address this limitation, I conducted an ancillary analysis 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 ancillary analysis provide no evidence that any of these school characteristics, taken individually or jointly, mediate the effects of neighborhood context during adolescence (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 both 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 from this study indicates that neighborhood effects on academic achievement during adolescence are primarily the result of mediating factors unrelated to the socioeconomic composition of schools. This suggests that unpacking the “black box” through which neighborhood effects are transmitted during adolescence will likely require a renewed focus on alternative pathways, such as those related to local subcultures, violent crime, or environmental health hazards, 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 total proportion of missing, and thus imputed, data in the analytic sample is 12 percent. 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 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.
6. 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.
7. 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.
8. This sensitivity analysis is based on 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.

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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 natural indirect 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 indirect effects are close to zero and statistically insignificant. Furthermore, 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 individually (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 natural indirect 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 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	.079	.041	.113	.001	.144	<.001
	(.030)		(.032)		(.039)		(.033)		(.040)	
School BLK					–.029	.467			–.095	.007
					(.039)				(.035)	
Nhood x school					–.008	.680			–.033	.084
					(.018)				(.019)	
Tot. effect			.108	.024			.169	.001		
			(.048)				(.049)			
Nat. direct effect					.108	.025			.171	.001
					(.048)				(.049)	
Nat. indirect effect					.002	.596			.006	.347
					(.003)				(.006)	
Prop. mediated					.01				.03	

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. The treatment and outcome are standardized to have zero mean and unit variance, while the mediator is rescaled by its standard deviation only. 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	.023	.754	.122	<.001	.143	.048
	(.047)		(.031)		(.074)		(.032)		(.072)	
School TPR					.003	.881			–.013	.529
					(.022)				(.021)	
Nhood x school					.012	.456			–.006	.711
					(.017)				(.016)	
Tot. effect			.107	.021			.182	<.001		
			(.046)				(.048)			
Nat. direct effect					.109	.020			.180	<.001
					(.047)				(.048)	
Nat. indirect effect					–.003	.599			.003	.534
					(.005)				(.005)	
Prop. mediated					–.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. The treatment and outcome are standardized to have zero mean and unit variance, while the mediator is rescaled by its standard deviation only. 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	.127	.053	.121	<.001	.160	.008
	(.035)		(.031)		(.066)		(.032)		(.060)	
District EXP (log)					.019	.586			–.008	.814
					(.035)				(.033)	
Nhood x district					–.015	.316			–.010	.438
					(.015)				(.013)	
Tot. effect			.107	.021			.182	<.001		
			(.047)				(.048)			
Nat. direct effect					.111	.018			.186	<.001
					(.047)				(.049)	
Nat. indirect effect					.000	.924			–.001	.680
					(.002)				(.002)	
Prop. mediated					.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. The treatment and outcome are standardized to have zero mean and unit variance, while the mediator is rescaled by its standard deviation only. 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)

Variable/estimand	School GRDEG		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	.132	.118	.050	.368	.060	.690	.093	.062	.225	.036
	(.084)		(.055)		(.151)		(.049)		(.106)	
School GRDEG					.005	.908			–.019	.615
					(.046)				(.038)	
Nhood x school					–.004	.929			–.049	.140
					(.048)				(.033)	
Tot. effect			.075	.364			.139	.059		
			(.082)				(.074)			
Nat. direct effect					.075	.391			.159	.036
					(.087)				(.076)	
Nat. indirect effect					.000	.984			–.014	.271
					(.015)				(.013)	
Prop. mediated					.00				–.10	

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. The treatment and outcome are standardized to have zero mean and unit variance, while the mediator is rescaled by its standard deviation only. 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	–.056	.770	.093	.062	–.053	.767
	(.075)		(.055)		(.193)		(.049)		(.178)	
School TEXP					.047	.265			.033	.388
					(.042)				(.038)	
Nhood x school					.025	.566			.036	.393
					(.044)				(.042)	
Tot. effect			.075	.364			.139	.059		
			(.082)				(.074)			
Nat. direct effect					.073	.375			.142	.049
					(.083)				(.072)	
Nat. indirect effect					.006	.567			.005	.567
					(.010)				(.009)	
Prop. mediated					.08				.04	

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. The treatment and outcome are standardized to have zero mean and unit variance, while the mediator is rescaled by its standard deviation only. 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	.146	.597	.093	.062	.504	.083
	(.087)		(.055)		(.276)		(.049)		(.289)	
School TSAL					.098	.018			.065	.119
					(.041)				(.042)	
Nhood x school					–.014	.668			–.053	.142
					(.034)				(.036)	
Tot. effect			.075	.364			.139	.059		
			(.082)				(.074)			
Nat. direct effect					.052	.522			.150	.050
					(.082)				(.077)	
Nat. indirect effect					.027	.211			.005	.823
					(.022)				(.020)	
Prop. mediated					.36				.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. The treatment and outcome are standardized to have zero mean and unit variance, while the mediator is rescaled by its standard deviation only. 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 E.1. 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	(.054, .245)
0.8	.148	(.052, .243)
0.7	.146	(.050, .242)
0.6	.143	(.046, .240)
0.5	.140	(.042, .238)
Nat. indirect effect		
Phi		
0.9	.012	(-.004, .028)
0.8	.013	(-.005, .031)
0.7	.015	(-.005, .036)
0.6	.018	(-.006, .042)
0.5	.021	(-.007, .050)

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.