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Abstract

Although it is widely hypothesized that neighborhood effects are explained by differences in the schools to which children have access, few prior studies have investigated the explanatory role of school quality. In this study, we examine whether school quality mediates or interacts with the effects of neighborhood context on academic achievement. With data from the Early Childhood Longitudinal Study, we operationalize a school's quality as the difference between the school-year and summer learning rates among its 1_{st} grade students. We then decompose the total effect of neighborhood context on achievement at the end of 3_{rd}, 5_{th}, and 8_{th} grade into components due to mediation versus interaction, which we estimate using novel counterfactual methods. Results indicate that living in a disadvantaged neighborhood substantially reduces academic achievement. But contrary to expectations, we find no evidence that neighborhood effects are mediated by or interact with school quality. The school environment does not mediate the effects of neighborhood context because differences in the socioeconomic composition of neighborhoods are not, in fact, strongly linked with differences in school quality. The school environment also does not interact with neighborhood context because attending a high-quality school is similarly beneficial whether children reside in advantaged or disadvantaged neighborhoods.

Keywords: neighborhoods, schools, achievement, poverty, inequality, mediation, interaction

1. Introduction

Are neighborhood effects on academic achievement explained by differences in the quality of the schools to which resident children have access? Although a large volume of evidence indicates that neighborhood context affects academic achievement (Chetty et al. 2016; Harding 2003; Rosenbaum 1995; Wodtke et al. 2011, 2016), relatively little is known about the causal processes through which these effects may be transmitted. Indeed, a frequent criticism of research on concentrated poverty is that "the social mechanisms…accounting for neighborhood effects have remained largely a black box" (Sampson 2012:46).

Whether the effects of neighborhood context can be explained by differences in school quality depends on two causal processes: mediation and interaction. Effect mediation refers to the operation of a causal chain whereby differences in neighborhood context engender differences in access to higher versus lower quality schools, which in turn engender differences in academic achievement. Effect interaction, by contrast, refers to a causal process whereby the effects of school quality on academic achievement are dampened or amplified by residence in an advantaged versus disadvantaged neighborhood. Effect mediation may occur in the absence of effect interaction, effect interaction may occur in the absence of mediation, or both may occur together (VanderWeele 2015). In other words, neighborhood context may influence academic achievement not only by changing the school environment to which children are exposed but also by altering the effects of this environment on student learning.

It is widely hypothesized that neighborhood effects on academic achievement are *mediated* by differences in school quality. For example, according to institutional resource theory, children in disadvantaged neighborhoods are more likely to attend lower quality schools because schools composed predominantly of students from poor communities may have fewer

experienced teachers, a slower pace of instruction, and a social climate that does not prioritize college preparation (Arum 2000; Jencks and Mayer 1990; Johnson 2012; Leventhal and Brooks-Gunn 2000; Sanbonmatsu et al. 2006; Wilson 1987).

Neighborhood effects are also thought to *interact* with differences in school quality. For example, compound disadvantage theory suggests that the effects of attending a higher versus lower quality school may be more pronounced for children in disadvantaged neighborhoods because residents of these neighborhoods rely more heavily on local institutions than children from wealthier communities (Jencks and Mayer 1990; Wodtke et al. 2016). By contrast, relative deprivation theory suggests that the effects of school quality may be less pronounced when children live in disadvantaged neighborhoods because children from poor communities may not be able to capitalize on the instructional advantages available in higher quality schools (Crosnoe 2009; Jencks and Mayer 1990; Wodtke et al. 2016).

Several prior studies have investigated the joint effects of neighborhood and school contexts on educational outcomes, but their results are mixed. Some report mainly neighborhood effects (Ainsworth 2002; Card and Rothstein 2007; Wodtke and Parbst 2017); some report mainly school effects (Goldsmith 2009; Carlson and Cohen 2014; Cook et al. 2002); and others report both (Owens 2010; Rendón 2014). All of these prior studies, however, suffer from two important limitations. First, none properly evaluate the explanatory role of schools by decomposing the total effect of neighborhood context into components due to mediation versus interaction. Second, none accurately measure school quality, as prior studies rely almost exclusively on school-level measures that are, at best, noisy proxies for the quality of a school's learning environment.

In this study, we investigate whether differences in schools explain the effect of neighborhood context on academic achievement using a more defensible measure of school quality together with novel decomposition methods. Specifically, with data from the Early Childhood Longitudinal Study - Kindergarten Class of 1998 (ECLS-K), we operationalize school quality as a school's contribution to student learning—that is, as the difference between a school's average learning rate among its 1_{st} grade students during the school year and the average learning rate among those same students during the previous summer. The difference between school-year versus summer learning rates captures the degree to which a school increases its students' learning above the rates that would prevail were its students not in school, thereby isolating the quality of a school's instructional regime from other factors (Downey et al. 2008, 2019; Raudenbush and Eschmann 2015). With this measure, we then decompose the total effect of neighborhood context on achievement measured later at the end of 3_{rd}, 5_{th}, and 8_{th} grade into components due to mediation versus interaction. We examine contextual effects on achievement through the end of 8_{th} grade to examine whether the influence of early exposures fade out over time (Bailey et al. 2017).

Estimating the effects of neighborhood context due to mediation versus interaction with school quality is complicated by the problem of exposure-induced confounding. Exposure-induced confounding occurs when a variable affected by the exposure of interest confounds the effect of a putative mediator on the outcome. It arises in this study because demographic characteristics of schools, such as their socioeconomic and racial composition, are strongly affected by neighborhood context and may in turn affect both the quality of a school's instructional regime and individual student achievement. It is problematic because consistently estimating direct, indirect, and interaction effects requires adjustment for these variables, but

conventional methods that do so naively are biased (VanderWeele 2015). To overcome this challenge, we use a novel counterfactual approach, termed regression-with-residuals (RWR), that can accurately evaluate mediation and interaction, even in the presence of exposure-induced confounders (Wodtke and Almirall 2017; Wodtke et al. 2020; Zhou and Wodtke 2019).

Results from this analysis suggest that exposure to a disadvantaged neighborhood during kindergarten has substantively large and statistically significant negative effects on both reading and mathematics achievement that persist through the end of 8th grade. Contrary to expectations, however, we find no evidence that these effects are mediated by or interact with school quality. The school environment does not appear to mediate the effects of neighborhood context because differences in the socioeconomic composition of local communities are not, in fact, strongly linked with differences in school quality. The school environment also does not appear to interact with neighborhood context because attending a high-quality school appears to have similar effects on achievement whether children reside in advantaged versus disadvantaged neighborhoods. An exhaustive set of sensitivity analyses indicates that these findings are robust to the presence of unobserved confounding and measurement error, to the use of many alternative model specifications, and to the use of alternative measures of school quality. This suggests that neighborhood effects on academic achievement are primarily explained by factors unrelated to the school environment. In other words, our results suggest that bad schools are generally not to blame for the poor academic performance of students in disadvantaged neighborhoods, and that improving the quality of schools in these communities may not be among the most effective policy interventions for mitigating neighborhood-based disparities in achievement.

2. Neighborhood Effect Mediation via School Quality

The mediators through which poor neighborhoods are hypothesized to affect academic achievement include social and cultural isolation (Anderson 1999; Wilson 1987), a breakdown of collective trust among residents and proximity to violent crime (Sampson 2001; Sharkey 2010), exposure to environmental health hazards (Crowder and Downey 2010; Rosenfeld et al. 2010), and institutional resource deprivation (Galster 2012; Jencks and Mayer 1990; Wilson 1987). Schools are one particularly important type of institutional resource, and differences in school quality are widely thought to explain neighborhood effects on academic achievement (e.g., Arum 2000; Ferryman et al. 2008; Galster 2012; Johnson 2012).

Consider, for example, the Moving to Opportunity (MTO) field experiment, which found that children in an experimental group who received housing vouchers to move into low-poverty neighborhoods performed no better academically than children in a control group who did not receive housing assistance (Orr et al. 2003, Sanbonmatsu et al. 2011). Although the MTO experiment was limited in a variety of different ways (e.g., Clampet-Lundquist and Massey 2008; Sampson 2008), many observers have attempted to explain its findings by pointing out that children in the experimental group did not end up attending schools that scored higher on common indicators of quality compared with children in the control group (Dobbie and Fryer 2009; Ferryman et al. 2008, Sanbonmatsu et al. 2006). The small differences in school quality observed across MTO treatment groups prompted Dobbie and Fryer (2011:179) to conjecture that "a better community, as measured by the poverty rate, does not significantly raise test scores if school quality remains essentially unchanged."

Neighborhood context directly affects the socioeconomic composition of the schools to which children have access because, in most districts, school assignment rules are based on a

student's residential location. As a result, children in disadvantaged neighborhoods typically attend schools with a greater number of low-income students than children in advantaged neighborhoods. In total, about 70 percent of the variance in the socioeconomic composition of public schools can be explained by the socioeconomic composition of the catchment areas they serve, despite the proliferation of magnet schools, charter schools, and intra-district open enrollment policies (Saporito and Sohoni 2007).

Schools with a large proportion of low-income students are thought to provide a lower quality of instruction because they suffer from multiple educational deficiencies. First, schools with a large proportion of low-income students tend to disproportionately enroll children with lower ability levels and more behavioral problems. Consequently, these schools may have a slower pace of instruction, a less rigorous curriculum, and more disorderly classrooms (Kahlenberg 2001; Willms 2010). Second, schools with a large proportion of low-income students suffer from higher rates of teacher attrition, and they often have difficulty recruiting and retaining the most qualified teachers (Borman and Dowling 2008; Boyd et al. 2005). Finally, schools with many poor students may enroll fewer high-achieving children, who help to engender an academic climate that prioritizes creativity, scholastic excellence, and university preparation rather than obedience, discipline, and vocational preparation (Esposito 1999; Kahlenberg 2001).

Neighborhood context may also directly affect school quality, apart from its link with the socioeconomic composition of students. For example, schools serving poor communities may have fewer resources because school funding is determined in part by local property tax revenues and because low-income residents are ill-equipped to raise private funds or to provide in-kind benefits for their children's school (Heuer and Stullich 2011; Kahlenberg 2001; Steinberg 1997).

Schools located in disadvantaged neighborhoods may also experience additional difficulties recruiting and retaining high-quality teachers if, for example, criminal activity in the surrounding area prompts concerns about safety at work or in transit (Boyd et al. 2011). Similarly, violent crime in disadvantaged neighborhoods may also negatively influence a school's academic climate if it erodes interpersonal trust and promotes a more authoritarian disciplinary environment (Arum 2005; Devine 1996; Nolan 2011).

In sum, neighborhood context is widely thought to affect school quality both directly and indirectly through its link with the socioeconomic composition of students, and school quality is in turn expected to have a lasting influence on academic achievement. Few studies, however, have empirically assessed this hypothesized causal chain. Moreover, results from what limited research exists are disconfirming. For example, Wodtke and Parbst (2017) found that the indirect effects of neighborhood context on academic achievement, as mediated by the proportion of a school's students who are eligible for a free lunch, are substantively small and statistically insignificant, whereas the direct effects operating independently of school composition were large and significant at stringent thresholds. They also found little evidence of mediation via the racial composition of students, the teacher-pupil ratio, or per-pupil expenditures, among several other characteristics of schools and their staff. All of these measures, however, are noisy and arguably invalid proxies for school quality that may have obscured an important explanatory role for the school environment.

3. Effect Interaction between Neighborhood and School Contexts

Neighborhood context is also thought to interact with school quality. Different theoretical perspectives, however, yield divergent hypotheses about whether living in an advantaged versus

disadvantaged neighborhood intensifies or attenuates the effects of attending a higher versus lower quality school.

Compound disadvantage theory contends that the experience of material deprivation in one social context exacerbates the harmful consequences of deprivation in other contexts (Jencks and Mayer 1990; Wodtke et al. 2016). This suggests that living in a disadvantaged neighborhood intensifies the harmful effects of attending a lower quality school, or equivalently, that it amplifies the benefits of attending a higher quality school. These effects may be more pronounced when children live in a poor neighborhood because the experience of material deprivation across multiple social contexts may engender an especially fatalistic outlook about one's life chances and the value of a formal education. Similarly, when attending a lower quality school, children from poor neighborhoods may become less resilient to the cognitive effects of violent crime or environmental health hazards if, for example, the school does not provide adequate coping, counseling, or health services. Children in poor neighborhoods may also rely more heavily on their local public schools to acquire important academic skills and develop their vocabulary, whereas children in advantaged neighborhoods may have more opportunities to acquire these skills elsewhere.

Relative deprivation theory, by contrast, suggests that living in a disadvantaged neighborhood may actually attenuate the harmful effects of attending a lower quality school, or equivalently, that it may dampen the positive effects of attending a higher quality school (Crosnoe 2009; Davis 1966; Jencks and Mayer 1990; Owens 2010). This is because children living in disadvantaged neighborhoods are thought to be poorly equipped to benefit from the resources and instruction provided at high-quality schools. For example, compared to students from advantaged neighborhoods, children from disadvantaged neighborhoods may not come as

well prepared for class and may begin school with fewer academic and social skills. Consequently, in higher rather than lower quality schools, they may struggle with the faster pace of instruction and the more demanding curriculum (Crosnoe 2009; Owens 2010), or they may have difficulty making friends and becoming socially integrated. Children from disadvantaged neighborhoods may also suffer stigmatization or develop negative self-perceptions when they attend higher rather than lower quality schools, where they are more likely to evaluate themselves, and to be evaluated by school staff, relative to their more advantaged peers.

Few prior studies have investigated interaction effects between neighborhood and school contexts, and among those that have, results are mixed. For example, in models of high school graduation and college attendance, Owens (2010) found that living in an advantaged neighborhood amplified the positive effects of attending a school with more advantaged students, whereas living a disadvantaged neighborhood muted these effects, consistent with relative deprivation theory. By contrast, Cook et al. (2002) and Wodtke and Parbst (2017) found that the effects of neighborhood and school contexts on several different measures of achievement were additive rather than multiplicative and thus provide little evidence of interaction.

4. School Quality and its Measurement

Although it is widely hypothesized that differences in school quality explain neighborhood effects, only a handful of prior studies have attempted to investigate this causal process, and their results do not consistently provide evidence of mediation or interaction. These mixed results, however, may be due to potentially severe limitations of measurement, as prior studies have relied on school-level measures that do not accurately reflect school quality.

In general terms, a school's quality can be conceptualized as the investment and consumption value of the education it provides to students (Ladd and Loeb 2013). Investment value here refers to benefits in the form of greater knowledge, more advanced abilities, higher earnings, and so forth, while consumption value refers to the immediate gratification that comes from attending school. Measuring a school's quality directly as the sum of its investment and consumption value is prohibitively difficult, as consumption benefits are often impossible to quantify and investment benefits often take years to realize. Consequently, all research must rely on proxies for school quality. But some proxies are better than others.

Prior research on the joint effects of neighborhood and school contexts has relied almost exclusively on proxies measuring *inputs* to schools that are thought to influence their quality. For example, Wodtke and Parbst (2017), among others (e.g., Card and Rothstein 2007; Dobbie and Fryer 2011; Goldsmith 2009; Owens 2010), focused mainly on measures of the socioeconomic and racial composition of students, per-pupil expenditures, and the teacher-pupil ratio. Demographic characteristics of the student body, however, are not strongly associated with the most important investment benefits of schooling (Coleman et al. 1966; Lauen and Gaddis 2013; Raudenbush 2004). Per-pupil spending also suffers from serious drawbacks as a measure of school quality. It does not account for cost differences across districts, for differences in how money is spent on tangible resources, or for how spending on one versus another resource differentially contributes to the quality of the school environment (Hanushek 2003). Similarly, the teacher-pupil ratio accounts only for the quantity, and not the quality, of but one school input. In general, any proxy based on school inputs is likely to be a poor measure of school quality because of the difficulty associated with capturing all relevant inputs and appropriately weighting their contributions to the educational benefits of interest.

An alternative and more accurate measurement strategy uses a school's *outputs* to assess its quality. The outputs most widely used to assess school quality are achievement test scores. Although test scores certainly do not capture all of the investment and consumption benefits of interest, their use is justified on the grounds that they reflect one particularly important benefit—that is, the acquisition of knowledge and abilities—that predicts many others, like higher earnings and better health in adulthood (Auld and Sidhu 2005; Ladd and Loeb 2013; Murnane and Levy 2006).

The central challenge associated with using outputs, like test scores, as a proxy for school quality is that it can be difficult to isolate a school's contribution to these outcomes from other aspects of students' lives (Ladd and Loeb 2013). Because children select into schools on the basis of many different factors that affect their outcomes, differences in student achievement across schools cannot simply be equated with differences in quality, as this would confound the contribution of the school environment with that of family and other influences on children (Downey et al. 2008, 2019; Raudenbush 2004; Raudenbush and Eschmann 2015). Thus, a defensible proxy for school quality must not only focus on outputs rather than inputs but also must correctly isolate a school's contribution to these outputs. This informs our measurement strategy below.

5. A Graphical Causal Model

Figure 1 presents a directed acyclic graph (DAG; Pearl 2009) that depicts a set of hypothesized causal relationships between neighborhood context, the school environment, and academic achievement. In this figure and henceforth, *A* denotes the socioeconomic composition of a child's neighborhood, *M* denotes the quality of a child's school, and *Y* denotes academic

achievement. There are also a set of potentially confounding variables measured at a baseline time period, which are collectively denoted by C, as well as a set of measures that capture the socioeconomic composition of a child's school, which are collectively denoted by Z.

As indicated in Figure 1, neighborhood context is hypothesized to have an indirect effect on academic achievement via school quality, which is represented by the $A \to M \to Y$ and $A \to Z \to M \to Y$ paths. In other words, school quality is hypothesized to mediate, at least in part, the effect of neighborhood context on achievement. Moreover, because A and M are both depicted to directly affect the outcome, Y, this figure is consistent with an interaction effect between neighborhood context and school quality.[1] Finally, this figure shows that the composition of a child's school, Z, may confound the effect of its quality on achievement, as indicated by the $M \leftarrow Z \to Y$ path. It also shows, via the $A \to Z$ path, that school composition is affected by neighborhood context. Thus, school composition is an exposure-induced confounder, which requires special adjustments when estimating the joint effects of neighborhood context and school quality on achievement. This informs our analytic strategy below.

6. Methods

6.1. Data

We use data from the ECLS-K linked to information from the U.S. Census and the National Center for Education Statistics (NCES).[2] The ECLS-K is a longitudinal study based on a national sample of schools and the children within them. It collected information on academic achievement, along with a wide range of other factors, in the fall and the spring of kindergarten (1998-99), the fall and spring of 1st grade (1999-2000), the spring of 3rd grade (2002), the spring of 5th grade (2004), and the spring of 8th grade (2007). By collecting data during the spring and

fall of both kindergarten and first grade, the ECLS-K allows for seasonal learning comparisons. The analytic sample for this study includes $n = 6,040$ children in $k = 310$ schools that were selected for participation in both the fall and spring assessments during kindergarten and 1st grade (all sample sizes are rounded to nearest ten in accordance with Department of Education disclosure risk guidelines).

6.2. Measures

The outcome of interest in this study is academic achievement. We measure achievement with item-response theory (IRT) theta scores on ECLS-K assessments of math and reading abilities. IRT theta scores on these assessments provide an equal-interval, vertically scaled measure of achievement that is capable of capturing student learning over time.[3] Both the math and reading assessments have desirable psychometric properties, including high reliability, high validity, and low differential item functioning (Pollock et al. 2005).

The exposure, or treatment, of interest is the socioeconomic composition of a child's census tract, which we use to approximate their neighborhood. To construct this measure, we match children in the ECLS-K to their census tracts using a restricted-access geocode file. Demographic information on census tracts comes from the GeoLytics Neighborhood Change Database, which contains tract-level data from the U.S. Census that have been harmonized over time (GeoLytics 2013).[4] With these data, we use principal components analysis to compute a composite index of neighborhood disadvantage based on the following tract characteristics: the poverty rate, the unemployment rate, the proportion of families receiving cash assistance, median household income, the proportion of households that are female-headed, aggregate levels of

education, and the occupational structure. This measure is standardized to have zero mean and unit variance, and it is scaled so that higher values represent more disadvantaged neighborhoods.

The mediator of interest is school quality, which we operationalize as the difference between a school's average learning rate among its 1st grade students during the school year and the average learning rate among those same students during the previous summer. This measure captures the degree to which a school increases its students' learning rates above those that would prevail were its students not in school under the following two assumptions: first, any effects of non-school factors on achievement must operate similarly during both the school year and the summer, and second, schools must not have sizeable "spillover" effects on summer learning. Although not beyond critique, these assumptions are generally consistent with the available evidence (Downey et al. 2008, 2019; von Hippel 2009). Thus, by isolating the impact of each school on its students' learning from potentially contaminating non-school factors, this measure reflects a school's quality much more accurately than other measures previously considered in the literature on contextual effects.

Following Downey et al. (2008, 2019), we estimate our measure of school quality from the ECLS-K using a multilevel model of test score growth in which scores on tests administered during kindergarten and 1st grade are nested within children who are in turn nested within schools. From this model, we predict the monthly learning rates of students in each school during first grade and during the previous summer, and then school quality is measured by taking the difference between them. We compute separate measures for reading and math achievement to allow for the possibility that a school's quality may differ depending on the subject matter. In all multivariate analyses, these measures are standardized to have zero mean and unit variance, and

14

they are scaled so that higher values represent higher quality schools. Technical details underlying this measurement strategy are presented in Part A of the Online Supplement.

The baseline confounders include both child and family characteristics. First, we measure and adjust for a child's gender, race, and birth weight. Gender is dummy coded, one for male and zero for female. Race is expressed as a series of dummy variables that capture whether a child identifies as white, black, Hispanic, Asian, or another race. Birth weight is also dummy coded, one if a child weighed less than 88 ounces at birth and zero otherwise.

Second, we adjust for the following family characteristic at baseline: a mother's age and marital status at the time of her child's birth, family income, parental education and employment status, the level of cognitive stimulation a child received at home, an indicator of parental involvement with their child's education, and maternal depressive symptoms. Maternal age is measured in years. Parental employment status is expressed as a series of dummy variables capturing whether each parent is "working at least 35 hours per week," "working less than 35 hours per week," or involved in some other arrangement. Family income is measured in dollars, which we transform using the natural log in all multivariate analyses. The highest level of education attained by either parent is expressed as a series of dummy variables for having "less than a high school diploma," "a high school diploma," "a vocational or technical degree," "some college education," "a bachelor's degree," or a "graduate degree." The level of cognitive stimulation provided in the household is measured using the HOME inventory (Caldwell and Bradley 1984). The level of parental involvement in their child's education is measured as a count of more than 20 different activities in which a parent may be engaged, such as attending parent-teacher association meetings or participating in extracurricular activities (Greenman et al. 2011). Maternal depressive symptoms are measured using an abbreviated version of the Center

for Epidemiological Studies - Depression Scale (CES-D; Radloff 1977). In all multivariate analyses, the baseline confounders are centered at their sample means.

Finally, we measure and adjust for the socioeconomic and racial composition of a child's school, which are potentially exposure-induced confounders. Specifically, we adjust for the percentage of students in a school who are eligible for a free lunch through the U.S. National School Lunch Program. This measure is an approximate school-level poverty rate, as a student's family must have an income at or below 130 percent of the federal poverty threshold in order to qualify for a free lunch. In addition, we also adjust for the percentage of students at a child's school who identify as nonwhite.

Analyses of mediation and interaction require sequential measurements of key variables (VanderWeele 2015). Figure 2 depicts the longitudinal measurement strategy we use to ensure appropriate temporal ordering of the confounders, exposure, mediator, and outcome. Specifically, we first measure the baseline confounders (C) at the fall of kindergarten, which include – in addition to the child and family demographics outlined previously – initial measures of achievement at both the child and school-levels. We then measure neighborhood context (A) the following spring.[5] Next, we construct measures of school composition (Z) and school quality (M) that cover 1st grade. Finally, we use measures of academic achievement (Y) taken during the spring of 3rd grade as our focal outcome. Thus, our data are sequentially ordered as follows: $\{C, A, Z, M, Y\}$. Part B of the Online Supplement presents results from parallel analyses of academic achievement measured later during 5th and 8th grade, which are very similar to those based on the 3rd grade assessments that we present here.

6.3. Estimands

To investigate whether school quality explains the effect of neighborhood context on academic achievement, we decompose a measure for the overall impact of living in a disadvantaged neighborhood into components due to mediation versus interaction, which is accomplished using potential outcomes notation and the counterfactual framework (Rubin 1974; VanderWeele 2014; VanderWeele et al. 2014). Let Y_a denote a child's achievement level in 3rd grade had she previously been exposed to the level of neighborhood disadvantage given by a during kindergarten, possibly contrary to fact. Similarly, let M_a denote the quality of a child's school during 1st grade under prior exposure to the level of neighborhood disadvantage given by a. Finally, let $M_{a|C}^R$ denote a level of school quality randomly selected from its population distribution under neighborhood exposure status a conditional on baseline covariates C.

Given this notation, consider the following estimand:

$$RATE = E\left(Y_{a^* M_{a^*|C}^R} - Y_{a M_{a|C}^R}\right),$$

which is similar to an average total effect except that it is defined in terms of both a contrast between neighborhood contexts and a randomized intervention on school quality. Specifically, when $a^* > a$, this effect gives the expected difference in achievement if children were exposed to a more versus less disadvantaged neighborhood, with school quality randomly selected from its distribution under each of these alternative exposures. It is therefore referred to as a "randomized intervention analogue" of the average total effect (VanderWeele et al. 2014).

The $RATE$ can be decomposed into direct and indirect components as follows:

$$RATE = E\left(Y_{a^* M_{a|C}^R} - Y_{a M_{a|C}^R}\right) + E\left(Y_{a^* M_{a^*|C}^R} - Y_{a^* M_{a|C}^R}\right) = RNDE + RNIE.$$

The first term in this decomposition, $RNDE = E\left(Y_{a^*M^R_{a|C}} - Y_{aM^R_{a|C}}\right)$, is a randomized intervention analogue of a natural direct effect. In words, the $RNDE$ is the expected difference in achievement under exposure to a more versus less disadvantaged neighborhood if children were subsequently exposed to a level of school quality randomly selected from its distribution among those in less disadvantaged neighborhoods. It captures the effect of neighborhood context on achievement that is not due to mediation via school quality.

The second term in this decomposition, $RNIE = E\left(Y_{a^*M^R_{a^*|C}} - Y_{a^*M^R_{a|C}}\right)$, is a randomized intervention analogue of a natural indirect effect. It represents the expected difference in achievement if children were first exposed to a disadvantaged neighborhood and then were subsequently exposed to a level of school quality randomly selected from its distribution among those in disadvantaged neighborhoods rather than from its distribution among those in more advantaged neighborhoods. The $RNIE$ captures the effect of neighborhood context on achievement that is due to mediation via school quality.

The $RNDE$ can be further decomposed into a controlled direct effect and an interaction effect occurring in the absence of mediation:

$$RNDE = E(Y_{a^*m} - Y_{am}) + \left\{E\left(Y_{a^*M^R_{a|C}} - Y_{aM^R_{a|C}}\right) - E(Y_{a^*m} - Y_{am})\right\}$$

$$= CDE + RINT_{\text{ref}}.$$

The first term in this expression, $CDE = E(Y_{a^*m} - Y_{am})$, is the controlled direct effect. It represents the expected difference in achievement if children were exposed to a more versus less disadvantaged neighborhood and then were all exposed to schools of the same quality m.

The second term, $RINT_{\text{ref}} = \left\{E\left(Y_{a^*M^R_{a|C}} - Y_{aM^R_{a|C}}\right) - E(Y_{a^*m} - Y_{am})\right\}$, is a reference interaction effect, which captures the component of the overall effect due to an interaction

between neighborhood context and school quality that occurs absent any mediation. Specifically, it describes how the direct effect of living in a more versus less disadvantaged neighborhood differs depending on whether children are exposed to a level of school quality randomly selected from its distribution among those in less disadvantaged neighborhoods, $M_{a|C}^R$, as opposed to some fixed level, m. Because interactions are symmetrical, the $RINT_{\text{ref}}$ also describes how the effect of attending a school with quality $M_{a|C}^R$ versus m differs depending on whether children live in more versus less disadvantaged neighborhoods. It captures the component of the overall effect due to interaction in the absence of mediation because it may be nonzero even if neighborhood context does not affect school quality.

Similarly, the $RNIE$ can be further decomposed into an effect purely due to mediation and an effect due to interaction occurring together with mediation:

$$RNIE = E\left(Y_{aM_{a^*|C}^R} - Y_{aM_{a|C}^R}\right) + \left\{ E\left(Y_{a^*M_{a^*|C}^R} - Y_{aM_{a^*|C}^R}\right) - E\left(Y_{a^*M_{a|C}^R} - Y_{aM_{a|C}^R}\right) \right\}$$

$$= RPIE + RINT_{\text{med}}.$$

The first term in this expression, $RPIE = E\left(Y_{aM_{a^*|C}^R} - Y_{aM_{a|C}^R}\right)$, is a randomized intervention analogue of a pure indirect effect. It represents the component of the overall effect due only to mediation via school quality and not interaction.

The second term, $RINT_{\text{med}} = E\left(Y_{a^*M_{a^*|C}^R} - Y_{aM_{a^*|C}^R}\right) - E\left(Y_{a^*M_{a|C}^R} - Y_{aM_{a|C}^R}\right)$, is a mediated interaction effect. It captures the component of the overall effect due to interaction between neighborhood context and school quality that occurs jointly with mediation. Specifically, it describes how the effect of living in a more versus less disadvantaged neighborhood differs depending on whether children are exposed to a level of school quality randomly selected from its distribution among those in more disadvantaged neighborhoods rather

than among those in less disadvantaged neighborhoods. Symmetrically, the $RINT_\text{med}$ also describes how the effect of exposure to a level of school quality randomly selected from its distribution among those in more disadvantaged neighborhoods, rather than among those in less disadvantaged neighborhoods, differs depending on the neighborhood environment in which a child lives. It captures the component of the overall effect due to interaction and mediation operating together because, in the absence of mediation, the distributions of $M_{a^*|C}^R$ and $M_{a|C}^R$ would be identical and thus the $RINT_\text{med}$ would necessarily equal zero.

To summarize, combining the expressions outlined previously yields the following additive decomposition:

$$RATE = RNDE + RNIE = CDE + RINT_\text{ref} + RPIE + RINT_\text{med},$$

where the CDE captures the effect of neighborhood context due to neither mediation nor interaction; the $RINT_\text{ref}$ captures the effect due to interaction but not mediation; the $RPIE$ captures the effect due to mediation but not interaction; and the $RINT_\text{med}$ captures the effect due to both mediation and interaction operating jointly.

We focus on a decomposition defined in terms of randomized interventions on school quality because its components can be identified under more defensible assumptions than those required of other effect decompositions. In particular, unlike the components of alternative decompositions (VanderWeele 2014; VanderWeele et al. 2014), all of the effects outlined previously can be identified in the presence of exposure-induced confounders. Nevertheless, identifying and estimating randomized intervention analogues of direct, indirect, and interaction effects still requires strong assumptions, as we explain in detail below.

6.4. Identification

The effects outlined previously can be identified from observed data under a set of so-called "ignorability" assumptions (VanderWeele 2014; VanderWeele et al. 2014), which are formally expressed as follows:

$$Y_{am} \perp A|C; Y_{am} \perp M|C,A,Z; \text{ and } M_a \perp A|C.$$

In this notation, $\perp$ denotes statistical independence. Thus, the first of these assumptions states that the potential outcomes of the exposure and mediator, Y_{am}, must be independent of the observed exposure conditional on the baseline confounders. The second assumption states that the same potential outcomes must also be independent of the observed mediator conditional on the baseline confounders, prior exposure, and the exposure-induced confounders. Finally, the third assumption states that the potential outcomes for the mediator under prior exposure, M_a, must be independent of the observed exposure conditional on the baseline confounders.[6] These assumptions would all be satisfied if there were not any unobserved confounding of the exposure-outcome, mediator-outcome, or exposure-mediator relationships.

These are strong assumptions, and if they are not satisfied in this analysis, then estimates of the effects outlined previously may be biased. We attempt to mitigate confounding bias by adjusting for an extensive set of baseline confounders, including baseline measures of academic achievement at both the child- and school-levels, together with measures of school composition, which may confound the effect of the mediator on the outcome. In addition, we also conduct a formal sensitivity analysis that evaluates whether our findings are robust to hypothetical patterns of unobserved confounding.

6.5. Estimation

The direct, indirect, and interaction effects of interest can be estimated from a set of regression models for the mediator, outcome, and exposure-induced confounders. The first model is for the conditional mean of school quality given neighborhood context and the baseline confounders. It can be formally expressed as follows:

$$E(M|C,A) = \theta_0 + \theta_1(C - \alpha_0) + \theta_2 A, \quad (1)$$

where $\alpha_0 = E(C)$ and thus $C - \alpha_0$ represents a transformation of the baseline confounders in which they are centered around their marginal means. The second model is for the conditional mean of academic achievement given neighborhood context, school quality, the baseline confounders, and finally, measures of school composition, which may be exposure-induced confounders. It can be formally expressed as follows

$$E(Y|C,A,Z,M) = \lambda_0 + \lambda_1(C - \alpha_0)$$

$$+\lambda_2 A + \lambda_3\big(Z - (\beta_0 + \beta_1 C + \beta_2 A)\big) + M(\lambda_4 + \lambda_5 A), \quad (2)$$

where $\beta_0 + \beta_1 C + \beta_2 A = E(Z|C,A)$ and thus $Z - (\beta_0 + \beta_1 C + \beta_2 A)$ represents a residual transformation of the exposure-induced confounders in which they are centered around their conditional means given prior exposure and the baseline confounders. This model is similar to a conventional linear regression except that it subsumes another model for $E(Z|C,A)$, which is used to residualize the exposure-induced confounders with respect to the observed past.

Under the ignorability assumptions outlined previously and under the assumption that our models for $(M|C,A)$, $E(Z|C,A)$, and $E(Y|C,A,Z,M)$ are correctly specified, the controlled direct effect is equal to

$$CDE = (\lambda_2 + \lambda_5 m)(a^* - a),$$

the reference interaction effect is equal to

$$RINT_{\text{ref}} = \lambda_5(\theta_0 + \theta_2 a - m)(a^* - a),$$

and the $RNDE$ is equal to the sum of these two expressions. Under the same set of assumptions, the pure interaction effect is equal to

$$RPIE = \theta_2(\lambda_4 + \lambda_5 a)(a^* - a),$$

the mediated interaction effect is equal to

$$RINT_{\text{med}} = \theta_2\lambda_5(a^* - a)^2,$$

and the $RNIE$ is equal to the sum of these two expressions. Lastly, the sum of the $RNDE$ and $RNIE$ gives the overall effect, or $RATE$. A derivation of these expressions is provided in Part C of the Online Supplement.

In the results section below, we focus on effects that contrast residence in a disadvantaged neighborhood at the 80th percentile of the exposure distribution with residence in an advantaged neighborhood at the 20th percentile. In addition, we evaluate the controlled direct effect and reference interaction effect by setting the level of school quality at its 75th percentile. Thus, the CDE in this analysis captures the direct effect of neighborhood context, and the $RINT_{\text{ref}}$ captures an interaction effect occurring in the absence of mediation, if all children attended a high-quality school during 1st grade.

We estimate these effects using the method of regression-with-residuals (RWR; Wodtke 2018; Wodtke and Almirall 2017; Wodtke et al. 2020; Zhou and Wodtke 2019), which is implemented as follows. First, the model for $E(M|C, A)$ is estimated by least squares after centering the baseline confounders around their sample means. Second, the model for $E(Z|C, A)$ is estimated by least squares and used to compute residual terms for the exposure-induced confounders. Third, the remaining parameters in the model for $E(Y|C, A, Z, M)$ are estimated by regressing the outcome, Y, on $\{\tilde{C}, A, Z^\perp, M, AM\}$, where $\tilde{C} = C - \bar{C}$ represents the baseline

confounders after centering them around their sample means and $Z^\perp = Z - \hat{E}(Z|C, A)$ denotes the residualized exposure-induced confounders. Finally, the estimated parameters from these different models are used to construct the effects of interest with the formulas outlined previously.

The key advantage of RWR is that it deals properly with exposure-induced confounders of the mediator-outcome relationship. In the presence of exposure-induced confounders, conventional regression and matching estimators that adjust for these variables naively are biased and inconsistent. This is because naively adjusting for confounders that are affected by prior exposure can engender bias due to over-control of intermediate pathways and endogenous selection (Elwert and Winship 2014; VanderWeele 2015). RWR avoids these biases by residualizing the exposure-induced confounders with respect to the observed past before including them in the regression model for the outcome. Adjusting for these residual terms sufficiently controls for mediator-outcome confounding while avoiding any bias due to over-control or endogenous selection, as the residuals are orthogonal to prior exposure by design. In this way, RWR properly isolates the explanatory role of school quality from the potentially confounding influence of school composition, unlike conventional methods.

Nevertheless, as a robustness check and for completeness, we also conduct an ancillary analysis in which we shift away from the goal of isolating the unique explanatory role of school quality from that of school composition and instead use conventional methods to examine whether school quality and school composition *jointly* explain neighborhood effects. Results from this analysis – discussed briefly below – affirm our general conclusions about the role of elementary schools in explaining neighborhood effects on achievement.

The mediator of primary interest in this analysis – school quality – is measured with error. This is because it is computed from sample rather than population data at the school level and because it is based on achievement test scores that are themselves subject to measurement error. When a mediator is measured with error, this can lead to attenuation bias in estimates of indirect effects and inflationary bias in estimates of direct effects. To correct for measurement error in the mediator, we implement a classical error-in-variables adjustment when fitting the outcome model (Draper and Smith 1998).[7] For this adjustment, we assume that the exposure and confounders are measured without error, that the mediator is measured with a reliability of $r_M = 0.7$, and that the exposure-by-mediator interaction term is measured with a reliability of $\frac{(r_A \times r_M) + \rho_{AM}^2}{1 + \rho_{AM}^2}$, where $r_A = 1$ denotes the assumed reliability of the exposure and ρ_{AM} denotes the correlation between the exposure and mediator (Bohrnstedt and Marwell 1978). An assumed reliability of $r_M = 0.7$ for our measure of school quality is consistent with estimates reported in prior research (e.g., von Hippel 2009). Moreover, experimentation with a range of plausible reliabilities generated substantively similar results, which are presented in Part D of the Online Supplement.

To adjust for the bias and inefficiency that may result from missing data, we simulate missing values for all variables using multiple imputation with 50 replications and then combine estimates following Rubin (1987). Overall, the proportion of missing information in this analysis is about 24 percent, which is due to a combination of panel attrition and item-specific nonresponse.[8] Standard errors are computed using the block bootstrap with 500 replications in order to adjust for the clustering of children within schools. Finally, although the ECLS-K is based on a complex sample design, we focus on unweighted estimates because they are very similar to results from a weighted analysis but are also more precise.

The methods we employ to examine mediation and interaction extend conventional approaches (e.g., Alwin and Hauser 1975; Baron and Kenney 1986) in several important ways. First, we delineate our estimands and identification assumptions precisely using counterfactual notation. Second, we introduce a decomposition that permits an assessment of mediation and interaction simultaneously, whereas conventional approaches typically assume away the latter to evaluate the former. Third, we resolve the problem of exposure-induced confounding, which is also typically assumed away, and in most cases, naively. All of these extensions align our analytic approach more closely with the theoretical models of neighborhood and school effects posited in prior work.

6. Results

6.1. Sample Characteristics

Table 1 presents descriptive statistics for math and reading test scores that have been standardized with respect to their mean and standard deviation at the fall of kindergarten. Several patterns are evident in these data, all of which are consistent with other recent studies based on the ECLS-K (e.g., von Hippel et al. 2018; von Hippel and Hamrock 2019). First, students learn at a rapid pace early on during elementary school, and they learn even faster during the school year than during the summer. Second, the variance, or inequality, in math and reading abilities is substantial at the start of kindergarten but tends to shrink over the course of students' elementary education. For example, by the spring of 3_{rd} grade, the standard deviation of math test scores is about 19% smaller than it was at the fall of kindergarten. Finally, during kindergarten and 1_{st} grade, inequality in student achievement appears to shrink primarily during the school year and to stagnate, or possibly even increase, over the summer. Taken together, these findings suggest

that elementary schooling has an equalizing effect on reading and math abilities, while factors outside of school have disequalizing effects.

Table 2 presents descriptive statistics for child, neighborhood, and school characteristics. They indicate that sampled children attend – during 1st grade – schools in which about 36 percent of students receive a free lunch and about 40% are nonwhite, on average. In addition, these results also indicate that the average school raises its students' monthly learning rates by about 0.11 and 0.17 standard deviations in math and reading, respectively, compared to the rates that would prevail were students not in school. There is, however, considerable variation in school quality around these averages, as indicated by our measure's sizeable standard deviation.

Table 3 presents descriptive statistics for the family covariates considered in this analysis. At the start of kindergarten, sampled children lived in households with an average income of about $49,000 and roughly 5 members. About 35% lived with parents whose highest level of education was a high school diploma or less, while about 31% had a parent with at least a bachelor's degree. A majority (66%) of sampled children had a mother who was married at the time of childbirth.

6.2. Neighborhood Context and School Quality

Figure 3 describes the bivariate relationship between school quality and neighborhood context. Specifically, it displays point estimates and confidence intervals from a linear regression of school quality on neighborhood disadvantage, with both variables standardized using their sample means and standard deviations.[9] Contrary to expectations, this figure suggests that children in more disadvantaged neighborhoods actually attend schools that are of *higher* quality, on average, than children in more advantaged neighborhoods, at least when a school's quality is

measured by isolating its contribution to math and reading achievement during 1st grade. For example, children in poor neighborhoods one standard deviation above the mean on the composite disadvantage index attend schools whose contribution to reading achievement is, on average, about one-quarter of a standard deviation above the mean for all schools. By contrast, children in wealthier neighborhoods one standard deviation below the mean of the composite disadvantage index attend schools whose contribution to reading achievement is, on average, about one-quarter of a standard deviation below the mean. In other words, children in disadvantaged neighborhoods appear more likely than those in advantaged neighborhoods to attend schools with above-average contributions to student learning.

The results in Figure 3 are counterintuitive. Schools serving poor neighborhoods have more chronic absenteeism and disciplinary problems, teachers with less experience and lower pay, and student populations that are not as well prepared for class (Owens and Candipan 2019). Nevertheless, many of these schools appear to provide large academic benefits to their students, and this seemingly contradictory finding is also consistent with other recent research on socioeconomic disparities in achievement. For example, von Hippel et al. (2018) show that achievement gaps between high- versus low-income students shrink during the first several years of schooling. Given the high degree of income segregation across schools, it is somewhat difficult to explain this finding without admitting the possibility that low-income students may be disproportionately served by elementary schools that provide a highly effective learning environment. Prior research on educational inequality also indicates that disadvantaged schools widely perceived to be "failing" are not, in fact, typically among the least impactful schools when evaluated in terms of their contributions to student learning (Downey et al. 2008, 2019; von Hippel 2009).

An alternative explanation for these findings is that children in poor neighborhoods may benefit more from elementary schooling, regardless of its quality, because they begin school with fewer academic skills. Although elementary curricula vary across districts and schools during the early years, they typically focus on a fairly uniform set of foundational abilities (e.g., letter, word, and number identification). Children from advantaged neighborhoods often enter school having already acquired many of these abilities, and they may therefore learn less from the instruction provided in their school because it is, at least in part, redundant with what they have already learned previously. Children from disadvantaged neighborhoods, on the other hand, often enter school with comparatively limited abilities, and they may be primed to absorb more during the school year because their instruction covers material to which they have not been previously exposed. This suggests that the positive relationship between school quality and neighborhood disadvantage may be confounded by, among other things, student abilities at the time of school entry.

To investigate this possibility, Figure 4 plots the partial relationship between neighborhood context and school quality during 1st grade after adjusting for the average ability levels of the students in each school at the fall of kindergarten. These estimates reveal that, conditional on school-average abilities at baseline, there is not a very strong relationship between neighborhood context and school quality measured later in 1st grade. Specifically, when school quality is assessed in terms of contributions to reading abilities, the partial regression line is nearly flat across the support of the neighborhood disadvantage index. When school quality is assessed in terms of contributions to math abilities, the partial regression line still has a nontrivial positive slope, but this relationship is far more modest and also rather noisy, leading to considerable imprecision in the tails of the disadvantage index. This indicates that, after

29

controlling for student inputs, children in more versus less disadvantaged neighborhoods attend schools that are, on average, of fairly similar quality during 1st grade. It also indicates that baseline ability levels are an important confounder of the relationship between neighborhood disadvantage and school quality.

In sum, our descriptive analyses provide little evidence that children in disadvantaged neighborhoods are frequently trapped in low-quality schools, while children in more advantaged neighborhoods disproportionately benefit from access to high-quality schools, as is commonly hypothesized in the literature on contextual effects and educational inequality. Rather, we find that children in disadvantaged neighborhoods attend schools that, on average, contribute more to their students' achievement than the schools attended by children in advantaged neighborhoods, although this relationship appears largely due to the confounding influence of student abilities at school entry. Regardless, both findings cast doubt on the hypothesis that attendance at low-quality schools mediates the negative effects of living in a disadvantaged neighborhood on academic achievement.

6.3. Effects of Neighborhood Context on Academic Achievement

Table 4 presents estimates for the effects of living in a disadvantaged neighborhood at the end of kindergarten on achievement test scores measured later during 3rd grade. Consistent with expectations and prior research, total effect estimates suggest that exposure to a disadvantaged neighborhood has a considerable negative impact on academic achievement. Specifically, estimates of the *RATE* indicate that earlier exposure to a disadvantaged neighborhood at the 80th percentile of the treatment distribution, rather than an advantaged neighborhood at the 20th percentile, reduces performance on 3rd grade math and reading assessments by about 0.13 and

30

0.15 standard deviations, respectively. These effects are substantively large and statistically significant at stringent thresholds. To put them in perspective, note that they are roughly equivalent in magnitude to missing about one month of instruction during elementary school.

Contrary to expectations, however, estimates of direct and indirect effects provide little evidence that the total effect of neighborhood context on academic achievement is mediated by school quality. For example, estimates of the $RNDE$ indicate that exposure to a disadvantaged neighborhood at the 80th percentile of the treatment distribution, rather than an advantaged neighborhood at the 20th percentile, would still reduce performance on math and reading assessments by about 0.13 and 0.16 standard deviations, respectively, even after an intervention to fix the school quality distribution to that observed in advantaged neighborhoods. Relatedly, estimates of the CDE indicate that living in a disadvantaged neighborhood, rather than an advantaged neighborhood, would also reduce test scores by about the same margins even after an intervention to place all students in high-quality schools at the 75th percentile of the mediator distribution. These effects are substantively large, statistically significant at stringent thresholds, and very similar to the total effect estimates discussed previously.

Conversely, estimates of the $RNIE$ indicate that, if children lived in a disadvantaged neighborhood, an intervention to shift the school quality distribution from that observed in advantaged neighborhoods to that observed in disadvantaged neighborhoods would barely change their test scores at all. Similarly, estimates of the $RPIE$ indicate that, if children lived in an advantaged neighborhood, their test scores also wouldn't change much at all after an intervention to shift the school quality distribution from that observed in advantaged neighborhoods to that observed in disadvantaged neighborhoods. Both the $RNIE$ and $RPIE$ are

31

substantively small and fail to reach conventional significance thresholds, despite being precisely estimated (i.e., having small standard errors).

Also contrary to expectations, the interaction effects of interest provide little evidence that neighborhood context dampens or amplifies the effects of school quality on achievement. Specifically, estimates for the $RINT_{\text{ref}}$, which captures interaction in the absence of mediation, and for the $RINT_{\text{med}}$, which captures interaction operating jointly with mediation, are all very close to zero, and they fail to approach conventional thresholds for statistical significance. This suggests that living in a disadvantaged neighborhood, rather than an advantaged neighborhood, does not meaningfully alter the effects of attending a higher versus lower quality school on later student achievement.

To illuminate *why* school quality does not appear to explain the effects of neighborhood context on academic achievement, Tables 5 and 6 present selected parameter estimates from Equations 1 and 2, which were used to construct the effect estimates discussed previously. Recall that Equation 1 models the conditional mean of the mediator—school quality during 1st grade—as a linear function of neighborhood disadvantage at the end of kindergarten and baseline covariates, including baseline measures of achievement at both the child- and school-level. Estimates from this equation indicate that living in a disadvantaged neighborhood does not impede access to quality schools, net of other factors. If anything, there is a modest, albeit statistically insignificant, positive link between neighborhood disadvantage and school quality when this construct is assessed in terms of contributions to math and reading abilities during 1st grade. The absence of a strong link between neighborhood context and school quality essentially precludes an important mediating role for schools in transmitting neighborhood effects on academic achievement.

Equation 2 models the conditional mean of the outcome—achievement test scores measured at the spring of 3rd grade—as a linear function of neighborhood disadvantage at the end of kindergarten, school quality and demographic composition during 1_{st} grade, and baseline covariates, which again include measures of achievement taken at the beginning of kindergarten. For reading abilities, estimates from this equation indicate that both neighborhood context and school quality have substantively large and statistically significant effects – in the expected directions – on test scores measured later during 3_{rd} grade. They also indicate, however, that these effects combine additively rather than multiplicatively, which precludes an explanatory role for school quality arising from an interaction with neighborhood context. For math abilities, estimates from this equation are similar, except that school quality measured during 1_{st} grade has a smaller and statistically insignificant positive effect on achievement measured later during 3_{rd} grade. This suggests that the benefits of attending a school that provides high-quality math instruction during the early years of a child's primary education may fade out over time, whereas the harmful consequences of earlier exposure to a disadvantaged neighborhood are lasting.

6.4. Sensitivity Analyses

The validity of causal inferences in this analysis depends on a number of strong assumptions about correct model specification, accurate measurement, and the absence of unobserved confounding. First, if Equations 1 or 2 are incorrectly specified, then the effect estimates discussed previously may be biased. In Part E of the Online Supplement, we present results from an ancillary analysis in which we experiment with several more flexible specifications, including models that permit the effects of treatment and the mediator to vary across race, gender, and

parental education. Effect estimates computed from these less restrictive specifications are nearly

identical to those presented in Table 4, which suggests that our results are fairly robust.

Second, faulty inferences may also arise if school quality has been inaccurately

measured. With our approach to measuring this construct, systematic errors might arise because

non-school determinants of achievement are less influential during the school year than during

the summer or because the influence of schools on summer learning is nonzero. In Part F of the

Online Supplement, we investigate whether our results are robust to this form of measurement

error by replicating our analysis using two alternative measures of school quality. The first

equates a school's quality with the difference between its school-year learning rate and *one-half*

the learning rate among its students during the summer, which assumes that non-school factors

are only half as influential when school is in session. The second measure equates a school's

quality with its school-year learning rate alone, that is, without any adjustment for non-school

factors. Results based on both of these alternative measures are nearly identical to those

discussed previously.

We also examined whether school quality and school composition might jointly mediate

the effects of neighborhood context, in addition to isolating the unique explanatory role of school

quality from that of school composition. Results from this ancillary analysis provide little

evidence that neighborhood effects are jointly mediated by school quality, school free lunch

participation, or school racial composition. For example, they indicate that residence in a

disadvantaged neighborhood at the 80th percentile of the treatment distribution, rather than an

advantaged neighborhood at the 20th percentile, would reduce 3rd grade math scores by about

0.13 standard deviations, even after a hypothetical intervention to expose all children to a high-

quality school with low levels of free lunch participation and a representative proportion of

nonwhite students. This direct effect is both highly significant and very similar to the total effect estimates reported previously, which suggests that neither the instructional quality nor the demographic composition of elementary schools can explain neighborhood effects on academic achievement.

Finally, if there are any unobserved confounders of the treatment-outcome, treatment-mediator, or mediator-outcome relationships, then our effect estimates may be biased. We attempted to mitigate this bias by controlling for an extensive set of putative confounders, including baseline measures of achievement as well as post-treatment variables that may affect both school quality and student outcomes. In addition, Part G of the Online Supplement presents a formal sensitivity analysis that investigates whether our inferences would change if there is any remaining confounding by unobserved factors. Results from this analysis indicate that our central conclusions about the explanatory role of school quality would remain valid even in the presence of unobserved confounding at fairly high levels.

7. Discussion

It is commonly hypothesized that neighborhood effects on educational outcomes are explained by differences in the schools to which children have access (e.g., Jencks and Mayer 1990; Johnson 2012; Sanbonmatsu et al. 2006), but few prior studies investigate the explanatory role of school quality in transmitting the effects of residential context. In this study, we examine whether school quality mediates or interacts with neighborhood effects on academic achievement using novel counterfactual methods and a more defensible measurement strategy for school quality designed to isolate a school's unique contribution to student learning. Based on this approach, data from the ECLS-K indicate that living in a disadvantaged neighborhood

substantially reduces academic achievement. At the same time, however, we find no evidence that neighborhood effects are mediated by school quality because differences in the socioeconomic composition of neighborhoods do not appear to be strongly linked with differences in school quality. Moreover, we also find no evidence that neighborhood context interacts with school quality, as attending a higher versus lower quality school appears to have similar effects whether children live in an advantaged versus disadvantaged neighborhood.

These findings are difficult to reconcile with institutional resource theory, which contends that schools are an especially important mediator of neighborhood effects on academic achievement (Jencks and Mayer 1990; Johnson 2012). They are also difficult to reconcile with either compound disadvantage or relative deprivation theories, which variously contend that the effects of school quality on achievement are dampened or amplified by living in an advantaged versus disadvantaged neighborhood (Crosnoe 2009; Jencks and Mayer 1990; Wodtke et al. 2016). Rather, our findings suggest that neighborhood effects on early academic achievement are most likely explained by other factors that are not directly linked to schools. More specifically, while prior research suggests that the characteristics of schools most closely linked with neighborhood context, such as the demographic composition of students, are not that consequential for student achievement (Lauen and Gaddis 2013; Wodtke and Parbst 2018), our findings indicate, conversely, that those aspects of the school environment that are most consequential for student achievement, are not that closely linked with neighborhood context.

The apparently weak link between the socioeconomic composition of neighborhoods and school quality, as measured by contributions to reading and math achievement during 1st grade, could be due to several different processes that warrant further study. First, it is possible that parents are poor judges of school quality and thus select schools for their children on the basis of

characteristics with little impact on student learning. Second, it is possible that school

contributions to different aspects of child development (e.g., social and emotional skills versus

reading and math abilities) are not highly correlated. In this situation, parents may be excellent

judges of school quality, but they may prioritize schools that contribute to non-academic

dimensions of their children's development. A third possibility is that parents may prioritize the

consumption value of schooling over its investment value, and that these different benefits are

also not very closely associated. All of these processes would tend to weaken the link between

parental resources and school impacts on achievement, and by extension, between neighborhood

composition and school quality as defined and measured in this study.

       Even if schools are not to blame for neighborhood-based disparities in academic

performance, they can still be part of the solution. Many studies show how different types of

school reforms can dramatically improve performance among disadvantaged students and narrow

achievement gaps (e.g., Chenoweth 2009; Hassrick et al. 2017). Caution is needed, however,

when singling out schools serving poor communities for criticism, complete overhaul, and

sometimes even outright closure, as often occurs in public discourse on school reform. Our

results suggest that the elementary schools serving children from poor communities are, on

average, educating their 1st grade students at least as effectively as the schools serving

advantaged communities. Consequently, overhauling schools in poor neighborhoods may not be

the most effective means for mitigating neighborhood effects on academic achievement. Many of

these schools are valuable community resources that have noteworthy positive impacts on their

students, perhaps despite outward appearances or public stereotypes to the contrary.

       An important methodological implication of this study is that the link between

neighborhood context and school quality is highly sensitive to the choice of metric used to

evaluate schools. In sharp contrast to our findings, prior studies that rely on proxy measures with poor construct validity (e.g., Owens and Candipan 2019; Wodtke and Parbst 2018), such as the demographic composition of students, characteristics of teachers, or financial expenditures, typically indicate that poor neighborhoods are disproportionately served by "low-quality" schools. The discrepancy between these results and ours underscores the importance of operationalizing school quality in a defensible manner that more closely corresponds with the value that schools provide to students.

In this study, we also introduced novel methods for decomposing effects into components due to mediation versus interaction and for estimating these components in the presence of exposure-induced confounding. Social scientists have become increasingly interested not only in establishing the existence of causal effects but also in explaining how they arise (e.g., Hallsten and Pfeffer 2017; Schneider and Harknett 2019). The decomposition outlined in the present study should therefore find wide application, wherever there is interest in understanding the process by which a cause produces its effects. Similarly, exposure-induced confounding is ubiquitous in the social sciences (VanderWeele 2015), as causal effects are typically transmitted through a confluence of interrelated mechanisms. Properly isolating these different mechanisms is essential for evaluating causal explanations, and thus the method of RWR is also widely relevant.

Although this study has important implications for theory, policy, and methods, it is not without limitations. The first is that our measure of school quality, despite its many advantages, only spans the 1_{st} grade. It's likely that schools become more differentiated in terms of their quality as the curriculum becomes more challenging and heterogeneous later in elementary, middle, and high school. Thus, it remains possible that the quality of secondary schools, for

example, is more strongly related to the socioeconomic composition of neighborhoods, and that school quality is more important for explaining neighborhood effects on educational outcomes during adolescence. Future research should explore whether school quality has a larger explanatory role in transmitting neighborhood effects later during the course of child development.

A second limitation is our narrow focus on achievement test scores both for evaluating school quality and for measuring student outcomes. School quality is a multidimensional construct that involves more than just academic skills, and some of these dimensions may be more or less closely linked with neighborhood contexts and student outcomes. For example, schools may differ in the degree to which they impart so-called "non-cognitive skills," such as conscientiousness, perseverance, and sociability, and these skills may be especially important for successfully navigating crucial academic transitions (Heckman et al. 2014). By focusing only on achievement test scores, our study may obscure the role of schools in explaining neighborhood effects on other important outcomes. Thus, an important direction for future research will be to measure school quality more holistically and to examine a broader set of student outcomes.

A third limitation of this study is our focus on population-average and point-in-time effects, when it's possible, or even likely, that the causal processes of interest may be more pronounced among certain subpopulations of children or when exposures are measured over a longer time horizon. Although we found little evidence of effect heterogeneity in an ancillary analysis focused on differences by race, gender, and parental education, future research should still examine cumulative effects over the early life course and whether they vary across certain subgroups of children.

Finally, this study is somewhat limited by its reliance on data collected between 1998 and 2007, given that many districts across the U.S. have recently undergone major changes that affect the schooling options available to residents. For example, the recent and rapid expansion of charter schools and intra-district open enrollment policies may have altered the relationship between neighborhood composition and school quality among contemporary cohorts of students. We focused on data from the ECLS kindergarten class of 1998 because it allows for the longest possible follow-up period – through the end of 8th grade – and thus for an assessment of whether contextual effects fade out over time. It will be important, however, to attempt a replication of our findings among more recent cohorts of students.

These limitations notwithstanding, our results provide considerable evidence that children growing up in disadvantaged neighborhoods perform worse academically than they would growing up elsewhere not because of differences in the quality of their elementary schools but rather because of other unmeasured causal mechanisms. This suggests that unpacking the "black box" through which neighborhood effects are transmitted will likely require a renewed focus on alternative social processes, including exposure to crime and violence, environmental health hazards, and differences in peer subcultures, among a variety of other possibilities.

Endnotes

1. Effect interaction is sometimes depicted stylistically with a graph that includes an arrow from the treatment into the arrow representing the direct effect of the mediator on the outcome. In a DAG, however, interactions are represented implicitly, and "arrows into arrows" are not defined.

2. Some of the data used in this analysis are based on restricted-access files from the NCES, which were obtained under special contractual arrangements designed to protect the anonymity of respondents. These data are not available from the authors. Researchers interested in obtaining restricted-access data from the ECLS-K should contact IESData.Security@ed.gov.

3. These scores are estimated from an item response model in which the probability that a child answers a test question correctly is a function of her ability (theta) and then the question's difficulty, discrimination, and guessability. Theta scores avoid the scaling problems that afflicted prior analyses of the ECLS-K because they properly isolate changes in a child's ability from changes in the characteristics of test questions (von Hippel and Hamrock 2019).

4. For intercensal years, we impute tract characteristics using linear interpolation.

5. Some baseline confounders could only be measured at the spring, rather than the fall, of kindergarten in the ECLS-K. These include family income, parental involvement, and maternal depression.

6. Identifying the components of alternative decompositions—for example, one in which the average total effect, $E\left(Y_{a^*M_{a^*}} - Y_{aM_a}\right)$, is expressed as the sum of a natural direct and a natural indirect effect, $E\left(Y_{a^*M_a} - Y_{aM_a}\right) + E\left(Y_{a^*M_{a^*}} - Y_{a^*M_a}\right)$, without invoking the concept of a randomized intervention on the mediator—requires the additional assumption that $Y_{am} \perp M_{a^*}|C$ (VanderWeele 2014, 2015). This assumption is problematic, and we therefore avoid it, because an independence restriction on the joint distribution of Y_{am} and M_{a^*} is violated when there are exposure-induced confounders of the mediator-outcome relationship, whether these variables are observed or not.

7. The classical error-in-variables correction assumes that $E(Y|\mathbf{X}) = \mathbf{X}\lambda$ and that $\widetilde{\mathbf{X}} = \mathbf{X} + \mathbf{U}$, where $\widetilde{\mathbf{X}} = \{\tilde{C}, A, Z^\perp, M, AM\}$ are the observed values of the predictors, $\mathbf{X}$ are the true values, and $\mathbf{U}$ are a set of independent and identically distributed random errors. In this situation, a consistent estimator for λ is $(\widetilde{\mathbf{X}}^{\mathbf{T}}\widetilde{\mathbf{X}} - \mathbf{C})^{-1}\widetilde{\mathbf{X}}^{\mathbf{T}}\mathbf{Y}$, where $\mathbf{C}$ is a diagonal matrix with elements equal to $N(1 - r_k)Var(\tilde{X}_k)$ and where N is the sample size, r_k is the reliability of the kth predictor, $Var(\tilde{X}_k)$ is the total variance of the kth predictor.

8. We also performed an analysis following von Hippel (2007) in which we multiply imputed all missing data but then dropped cases with missing values on the mediator or outcome prior to fitting Equations 1 and 2. Results from this analysis are substantively similar to those we report here.

9. Estimates from thin plate spline regressions, which allow for complex forms of nonlinearity, were substantively similar.

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Figures

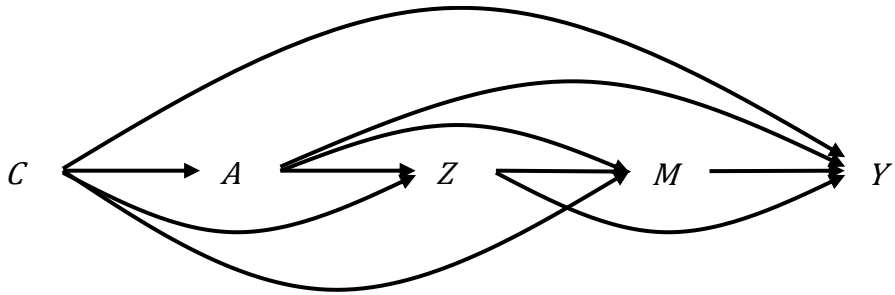Figure 1. Hypothesized causal relationships between the baseline confounders (C), neighborhood context (A), school composition (Z), school quality (M), and achievement test scores (Y).
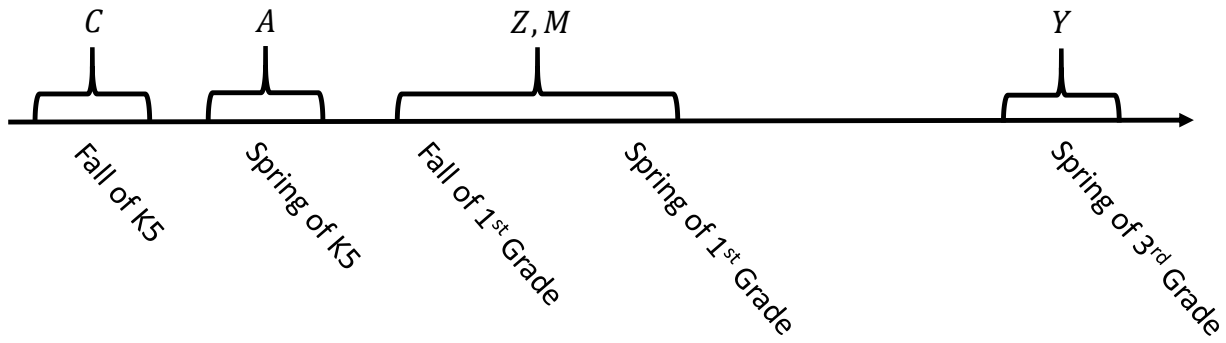
Figure 2. Longitudinal measurement strategy to ensure appropriate temporal ordering of baseline confounders (C), neighborhood context (A), school composition (Z), school quality (M), and achievement test scores (Y).
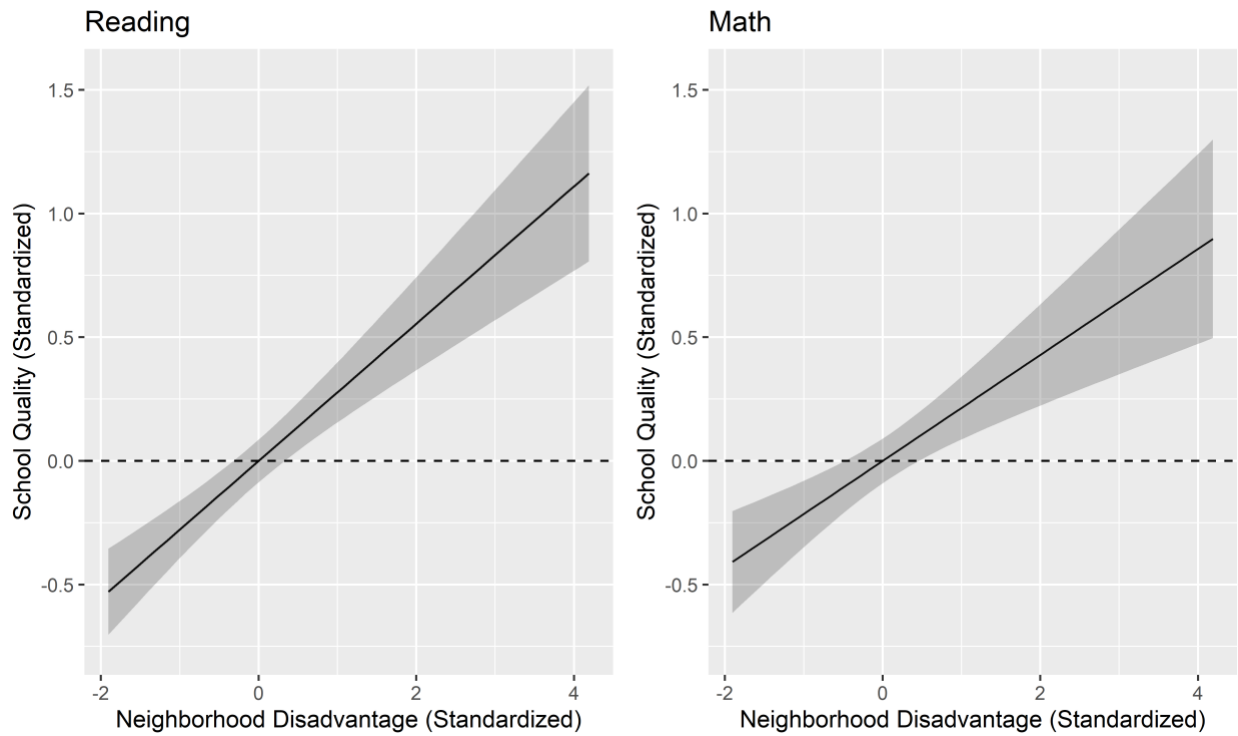
Figure 3. The bivariate relationship between school quality and neighborhood disadvantage, ECLS-K Class of 1998-99 (n=6,040, k=310).

Notes: Estimates are combined across MI datasets.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-4" and "Student record abstract form (kindergarten, 1st grade)"; GeoLytics Neighborhood Change Database, 2010.

Figure 4. The partial relationship between school quality and neighborhood disadvantage conditional on school-average achievement at baseline, ECLS-K Class of 1998-99 (n=6,040, k=310).

Notes: Estimates are combined across MI datasets.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-4" and "Student record abstract form (kindergarten, 1st grade)"; GeoLytics Neighborhood Change Database, 2010.
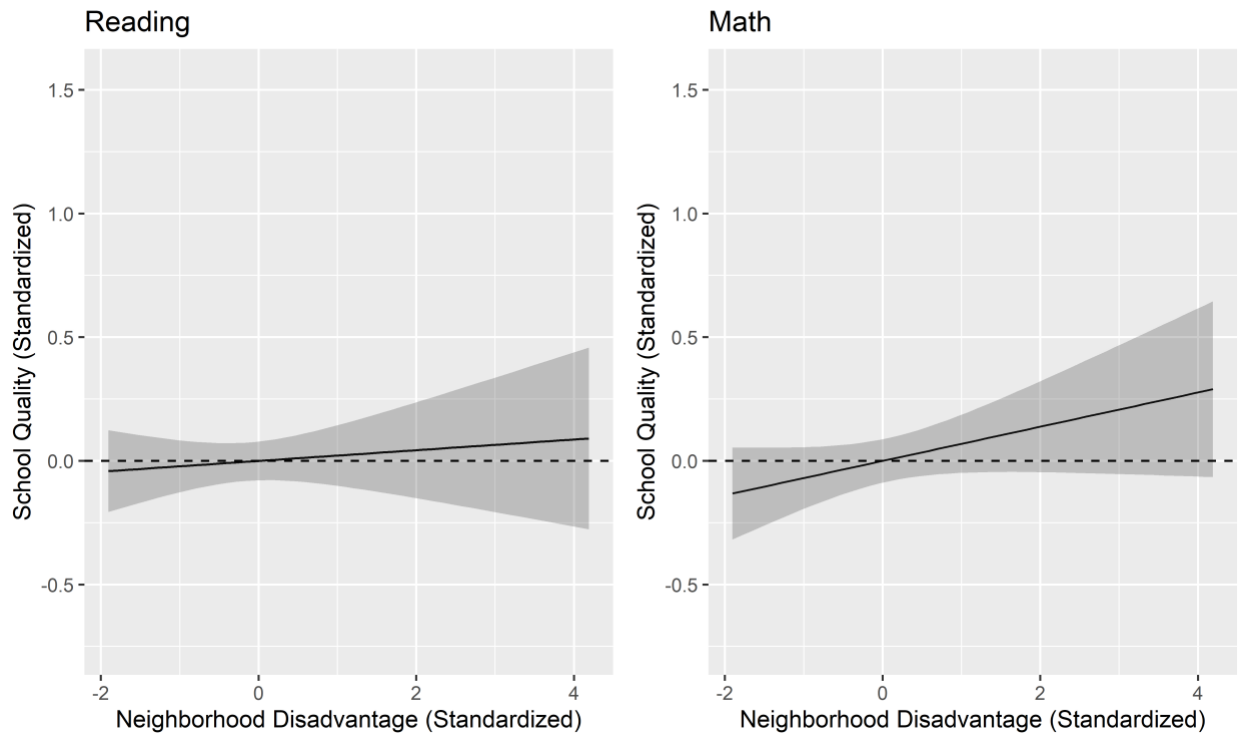
Tables

Table 1. Child test scores, ECLS-K Class of 1998-99 (n=6,040, k=310)

Variable	Mean	SD
Math test scores		
Fall of kindergarten	0.00	1.00
Spring of kindergarten	1.00	0.97
Fall of 1st grade	1.49	0.97
Spring of 1st Grade	2.52	0.88
Spring of 3rd Grade	3.86	0.81
Spring of 5th Grade	4.66	0.85
Spring of 8th Grade	5.33	0.93
Reading test scores		
Fall of kindergarten	0.00	1.00
Spring of kindergarten	1.11	0.99
Fall of 1st grade	1.53	1.00
Spring of 1st Grade	2.69	0.90
Spring of 3rd Grade	3.98	0.62
Spring of 5th Grade	4.47	0.58
Spring of 8th Grade	4.95	0.74

Notes: Estimates are combined across MI datasets.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-7".

Table 2. Child, neighborhood, and school characteristics, ECLS-K Class of 1998-99 (n=6,040, k=310)

Variable	Mean	SD
Contextual measures		
Neighborhood disadvantage (kindergarten)	0.00	1.00
School poverty (first grade)	35.90	27.36
School proportion non-white (first grade)	40.08	36.34
School quality (math, first grade)	0.11	0.05
School quality (reading, first grade)	0.17	0.05
Child measures		
Gender		
Male	0.51	---
Female	0.49	---
Race		
White (non-Hispanic)	0.55	---
Black or African American (non-Hispanic)	0.15	---
Hispanic	0.17	---
Asian	0.05	---
Other	0.07	---
Birth weight		
Low (<88 ounces)	0.08	---
Not low	0.92	---

Notes: Estimates are combined across MI datasets.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-4," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Wave 1 Parent Interview"; GeoLytics Neighborhood Change Database, 1999; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table 3. Family characteristics, ECLS-K Class of 1998-99 (n=6,040, k=310)

Variable	Mean	SD
Cognitive stimulation scale	0.01	0.48
Mother's age at birth	27.50	6.32
Parental practices scale	0.00	0.38
Parental mental health scale	17.59	5.51
Parental income ($1000s)	49.01	36.98
Household size	4.54	1.43
Parental education		
Less than high school diploma	0.10	---
High school diploma or equivalent	0.25	---
Vocational/technical degree	0.05	---
Some college	0.27	---
Bachelor's degree	0.17	---
Graduate degree	0.14	---
Mother married at birth	0.67	---
Father's employment status		
35 hours or more per week	0.86	---
Less than 35 hours per week	0.04	---
Other	0.10	---
Mother's employment status		
35 hours or more per week	0.45	---
Less than 35 hours per week	0.22	---
Other	0.33	---

Notes: Estimates are combined across MI datasets.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Parent Interview, Waves 1 and 2".

Table 4. Decomposition of the Overall Effect of Neighborhood Context on 3rd Grade Achievement Test Scores into Direct, Indirect, and Interaction Effects, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	p-value	Est.	SE	p-value
RATE	-0.130	(0.033)	<0.001	-0.153	(0.057)	0.007
RNDE	-0.132	(0.033)	<0.001	-0.161	(0.058)	0.006
CDE	-0.128	(0.027)	<0.001	-0.161	(0.040)	<0.001
RINT$_\text{ref}$	-0.005	(0.015)	0.739	0.000	(0.026)	1.000
RNIE	0.002	(0.003)	0.505	0.008	(0.010)	0.424
RPIE	0.001	(0.003)	0.739	0.008	(0.010)	0.424
RINT$_\text{med}$	0.001	(0.002)	0.617	0.000	(0.003)	1.000

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table 5. Selected Coefficients from Models of the Mediator (School Quality Defined in Terms of Contributions to Math Achievement during 1_{st} Grade) and the Outcome (3_{rd} Grade Math Test Scores), ECLS-K Class of 1998-99 (n=6,040, k=310)

Coefficients	Math Achievement					
	School Quality (Eq. 1)			3_{rd} Grade Test Scores (Eq. 2)		
	Est.	SE	p-val	Est.	SE	p-val
Neighborhood Disadvantage (standardized)						
A	0.040	(0.043)	0.352	-0.082	(0.015)	<0.001
School quality (standardized)						
M	---	---	---	0.025	(0.020)	0.211
A x M	---	---	---	0.005	(0.015)	0.739

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table 6. Selected Coefficients from Models of the Mediator (School Quality Defined in Terms of Contributions to Reading Achievement during 1_{st} Grade) and the Outcome (3_{rd} Grade Reading Test Scores), ECLS-K Class of 1998-99 (n=6,040, k=310)

Coefficients	Math Achievement					
	School Quality (Eq. 1)			3_{rd} Grade Test Scores (Eq. 2)		
	Est.	SE	p-val	Est.	SE	p-val
Neighborhood Disadvantage (standardized)						
A	0.037	(0.043)	0.390	-0.100	(0.017)	<0.001
School quality (standardized)						
M	---	---	---	0.138	(0.032)	<0.001
A x M	---	---	---	0.000	(0.023)	1.000

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

ONLINE SUPPLEMENT

Part A: Measuring School Quality

In this appendix, we explain our approach to measuring quality. We operationalize school quality as the difference between a school's average learning rate among its 1st grade students during the school year and the average learning rate among those same students during the previous summer. If all students in the ECLS-K were tested on the first and last days of both kindergarten and first grade, then school-year versus summer learning rates could be estimated directly by subtracting successive test scores. The ECLS-K, however, visited schools to administer assessments on a staggered schedule. As a result, students at different schools may have been tested anywhere from one to three months from the beginning or end of the school year as part of the spring and fall assessments. To adjust for the differential timing of these tests, we follow Downey et al. (2008, 2019) and model test scores as a linear function of the amount of time that each child had spent in kindergarten, on summer break, and in first grade at the time each test was administered.

Specifically, we model test scores measured at time t for child i in school j, which are here denoted by SCR_{tij}, as follows:

$$SCR_{tij} = \left(\gamma_0 + \mu_{0j} + \tau_{0ij}\right) + KND_{tij}\left(\gamma_1 + \mu_{1j} + \tau_{1ij}\right) + SUM_{tij}\left(\gamma_2 + \mu_{2j} + \tau_{2ij}\right) +$$
$$FST_{tij}\left(\gamma_3 + \mu_{3j} + \tau_{3ij}\right) + \varepsilon_{tij},$$

where there are $t = 1, \ldots, 4$ testing occasions between the start of kindergarten and the end of first grade and where KND_{tij}, SUM_{tij}, and FST_{tij} respectively denote the amount of time in months that a child had spent in kindergarten, on summer break, and in first grade prior to each testing occasion. In this model, $\boldsymbol{\gamma} = (\gamma_0, \gamma_1, \gamma_2, \gamma_3)$ is a vector of fixed effects that capture the achievement level and learning rates during kindergarten, summer, and first grade averaged

63

across all schools; $\boldsymbol{\mu}_j = \left(\mu_{0j}, \mu_{1j}, \mu_{2j}, \mu_{3j}\right)$ is a vector of random effects that capture each

school's departure from the overall average achievement level and learning rates; and $\boldsymbol{\tau}_{ij} =$

$\left(\tau_{0ij}, \tau_{1ij}. \tau_{2ij}, \tau_{3ij}\right)$ is another vector of random effects that capture each child's deviation from

their school's average achievement level and learning rates. We assume that $\boldsymbol{\mu}_j$ and $\boldsymbol{\tau}_{ij}$ are

uncorrelated and that both follow multivariate normal distributions with zero means and

unrestricted covariance matrices. The disturbance term in this model, ε_{tij}, represents random

measurement error, whose variance at each time t is constrained to equal the total variance of the

test scores multiplied by one minus their reliability.

We fit this model by the method of maximum likelihood to data from our analytic sample

of children in the ECLS-K after imposing several additional sample restrictions. Specifically, we

exclude children who do not have valid school identifiers in waves 1 to 4, who attended a school

with a year-round academic calendar or that required attendance at a summer school program, or

who transferred schools during either school year. With maximum likelihood estimates (MLEs)

of the fixed effects and variance components, we then compute best linear unbiased predictions

(BLUPs) of the school-level random effects. Finally, for each school j, we compute its quality as

$\left(\hat{\gamma}_3 + \tilde{\mu}_{3j}\right) - \left(\hat{\gamma}_2 + \tilde{\mu}_{2j}\right)$, where "hats" denote MLEs and "tildes" denote BLUPs. In this

expression, $\left(\hat{\gamma}_3 + \tilde{\mu}_{3j}\right)$ is the predicted learning rate among students in school j during first

grade, and $\left(\hat{\gamma}_2 + \tilde{\mu}_{2j}\right)$ is the predicted learning rate among the same students over the previous

summer. Under the assumptions outlined previously, the difference between them isolates the

degree to which a school increases its students' learning rates above those that would prevail had

its students not attended school. It thereby reflects a school's quality more accurately than other

measures that confound the influence of school- and non-school factors or that have only tenuous

connections to student achievement.

Part B: Parallel Analyses of 5th and 8th Grade Achievement Test Scores

In this appendix, we present results from a parallel analysis of neighborhood effects on achievement test scores measured during the spring of 5th grade and the spring of 8th grade. Table B.1 presents estimated effects on 5th grade achievement, and Table B.2 presents estimated effects on 8th grade achievement. These effect estimates are very similar to those presented in the main text that focus on achievement measured at the spring of 3rd grade. This suggests that living in a disadvantaged neighborhood during kindergarten has lasting effects on achievement through the end of middle school. It also suggests that these effects, like those on 3rd grade achievement, cannot be explained by differences in school quality measured earlier during 1st grade.

Table B.1. Estimated Effects of Neighborhood Context on 5th Grade Achievement Test Scores, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.157	0.040	<0.001	-0.147	0.053	0.006
RNDE	-0.158	0.040	<0.001	-0.153	0.053	0.004
CDE	-0.143	0.031	<0.001	-0.146	0.038	<0.001
$RINT_{ref}$	-0.015	0.018	0.405	-0.007	0.024	0.770
RNIE	0.001	0.003	0.739	0.006	0.007	0.391
RPIE	0.000	0.003	1.000	0.005	0.007	0.475
$RINT_{med}$	0.002	0.003	0.505	0.001	0.003	0.739

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-4 and 6," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table B.2. Estimated Effects of Neighborhood Context on 8th Grade Achievement Test Scores, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.158	0.040	<0.001	-0.153	0.052	0.003
RNDE	-0.158	0.041	0.001	-0.154	0.053	0.004
CDE	-0.146	0.035	<0.001	-0.135	0.040	0.001
*RINT*ref	-0.012	0.018	0.505	-0.019	0.022	0.388
RNIE	0.001	0.003	0.739	0.000	0.003	1.000
RPIE	-0.001	0.003	0.739	-0.001	0.004	0.803
*RINT*med	0.001	0.003	0.739	0.002	0.004	0.617

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-4 and 7," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Part C: Derivation of Parametric Expressions for the CDE, $RINT_{\text{ref}}$, $RPIE$, and $RINT_{\text{med}}$

In this appendix, we derive parametric expressions for the direct, indirect, and interaction effects of interest. If $Y(a,m) \perp A|C$; $Y(a,m) \perp M|C,A,Z$; and $M(a) \perp A|C$, VanderWeele et al. (2014) show that the $RNDE$ and $RNIE$ can be expressed in terms of the observed data as follows:

$$RNDE = E\left(Y\left(a^*, M^R(a|C)\right) - Y\left(a, M^R(a|C)\right)\right) = \sum_c \sum_m \sum_z \left(E(Y|c,a^*,z,m)P(z|c,a^*) - \right.$$

$$\left. E(Y|c,a,z,m)P(z|c,a)\right)P(m|c,a)\,P(c) \text{ and}$$

$$RNIE = E\left(Y\left(a^*, M^R(a^*|C)\right) - Y\left(a^*, M^R(a|C)\right)\right) = \sum_c \sum_m \sum_z \left(P(m|c,a^*) - \right.$$

$$\left. P(m|c,a)\right)E(Y|c,a^*,z,m)P(z|c,a^*)P(c).$$

If, in addition, the conditional mean of M given $\{C, A\}$ is equal to

$$E(M|C,A) = \theta_0 + \theta_1(C - \alpha_0) + \theta_2 A,$$

and the conditional mean Y given $\{C, A, Z, M\}$ is equal to

$$E(Y|C,A,Z,M) = \lambda_0 + \lambda_1(C - \alpha_0) + \lambda_2 A + \lambda_3\left(Z - (\beta_0 + \beta_1 C + \beta_2 A)\right) + M(\lambda_4 + \lambda_5 A),$$

where $E(C) = \alpha_0$ and $E(Z|C,A) = \beta_0 + \beta_1 C + \beta_2 A$, then

$$RNDE = \sum_c \sum_m \sum_z \big(E(Y|c,a^*,z,m)P(z|c,a^*) - E(Y|c,a,z,m)P(z|c,a)\big) P(m|c,a)\, P(c)$$

$$= \sum_c \sum_m \sum_z \bigg(\big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a^* + \lambda_3(z - E(Z|c,a^*))$$

$$+ m(\lambda_4 + \lambda_5 a^*)\big) P(z|c,a^*)$$

$$- \big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a + \lambda_3(z - E(Z|c,a))$$

$$+ m(\lambda_4 + \lambda_5 a)\big) P(z|c,a)\bigg) P(m|c,a)\, P(c)$$

$$= \sum_c \sum_m \bigg(\big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a^* + \lambda_3\big(E(Z|c,a^*) - E(Z|c,a^*)\big)$$

$$+ m(\lambda_4 + \lambda_5 a^*)\big)$$

$$- \big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a + \lambda_3\big(E(Z|c,a) - E(Z|c,a)\big)$$

$$+ m(\lambda_4 + \lambda_5 a)\big)\bigg) P(m|c,a)\, P(c)$$

$$= \sum_c \sum_m \bigg(\big(\lambda_2 a^* + m(\lambda_4 + \lambda_5 a^*)\big) - \big(\lambda_2 a + m(\lambda_4 + \lambda_5 a)\big)\bigg) P(m|c,a)\, P(c)$$

$$= \sum_c \bigg(\big(\lambda_2 a^* + E(M|c,a)(\lambda_4 + \lambda_5 a^*)\big) - \big(\lambda_2 a + E(M|c,a)(\lambda_4 + \lambda_5 a)\big)\bigg) P(c)$$

$$= \sum_c \bigg(\big(\lambda_2 a^* + (\theta_0 + \theta_1(c - E(C)) + \theta_2 a)(\lambda_4 + \lambda_5 a^*)\big)$$

$$- \big(\lambda_2 a + (\theta_0 + \theta_1(c - E(C)) + \theta_2 a)(\lambda_4 + \lambda_5 a)\big)\bigg) P(c)$$

$$= \bigg(\big(\lambda_2 a^* + (\theta_0 + \theta_1(E(C) - E(C)) + \theta_2 a)(\lambda_4 + \lambda_5 a^*)\big)$$

$$- \big(\lambda_2 a + (\theta_0 + \theta_1(E(C) - E(C)) + \theta_2 a)(\lambda_4 + \lambda_5 a)\big)\bigg)$$

$$= \big(\lambda_2 + \lambda_5(\theta_0 + \theta_2 a)\big)(a^* - a)$$

and

$$RNIE = \sum_c \sum_m \sum_z \big(P(m|c, a^*) - P(m|c, a)\big)E(Y|c, a^*, z, m)P(z|c, a^*)P(c)$$

$$= \sum_c \sum_m \sum_z \big(P(m|c, a^*) - P(m|c, a)\big)\Big(\lambda_0 + \lambda_1\big(c - E(C)\big) + \lambda_2 a^*$$

$$+ \lambda_3\Big(z - E\big(Z|c, a^*\big)\Big) + m(\lambda_4 + \lambda_5 a^*)\Big)P(z|c, a^*)P(c)$$

$$= \sum_c \sum_m \big(P(m|c, a^*) - P(m|c, a)\big)\Big(\lambda_0 + \lambda_1\big(c - E(C)\big) + \lambda_2 a^*$$

$$+ \lambda_3\Big(E\big(Z|c, a^*\big) - E\big(Z|c, a^*\big)\Big) + m(\lambda_4 + \lambda_5 a^*)\Big)P(c)$$

$$= \sum_c \Big(\big(E(M|c, a^*)(\lambda_4 + \lambda_5 a^*)\big) - \big(E(M|c, a)(\lambda_4 + \lambda_5 a^*)\big)\Big)P(c)$$

$$= \sum_c \Big(\big((\theta_0 + \theta_1\big(c - E(C)\big) + \theta_2 a^*)(\lambda_4 + \lambda_5 a^*)\big)$$

$$- \big((\theta_0 + \theta_1\big(c - E(C)\big) + \theta_2 a)(\lambda_4 + \lambda_5 a^*)\big)\Big)P(c)$$

$$= \Big((\theta_0 + \theta_1\big(E(C) - E(C)\big) + \theta_2 a^*)(\lambda_4 + \lambda_5 a^*)\Big)$$

$$- \Big((\theta_0 + \theta_1\big(E(C) - E(C)\big) + \theta_2 a)(\lambda_4 + \lambda_5 a^*)\Big)$$

$$= \theta_2(\lambda_4 + \lambda_5 a^*)(a^* - a).$$

Under the same ignorability assumptions defined previously, the CDE can be expressed in terms of the observed as

$$CDE = E\big(Y(a^*, m) - Y(a, m)\big) = \sum_c \sum_z \big(E(Y|c, a^*, z, m)P(z|c, a^*) -$$

$$E(Y|c, a, z, m)P(z|c, a)\big)P(c),$$

and given correct models for the outcome, mediator, and exposure-induced confounders, it is equal to

$$CDE = \sum_c \sum_z \left(E(Y|c, a^*, z, m)P(z|c, a^*) - E(Y|c, a, z, m)P(z|c, a) \right) P(c)$$

$$= \sum_c \sum_z \left(\left(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a^* + \lambda_3(z - E(Z|c, a^*)) \right. \right.$$

$$+ m(\lambda_4 + \lambda_5 a^*) \Big) P(z|c, a^*)$$

$$- \Big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a + \lambda_3(z - E(Z|c, a))$$

$$+ m(\lambda_4 + \lambda_5 a) \Big) P(z|c, a) \Big) P(c)$$

$$= \sum_c \left(\Big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a^* + \lambda_3(E(Z|c, a^*) - E(Z|c, a^*)) \right.$$

$$+ m(\lambda_4 + \lambda_5 a^*) \Big)$$

$$- \Big(\lambda_0 + \lambda_1(c - E(C)) + \lambda_2 a + \lambda_3(E(Z|c, a) - E(Z|c, a))$$

$$+ m(\lambda_4 + \lambda_5 a) \Big) \Big) P(c) = \sum_c \left((\lambda_2 a^* + \lambda_5 m a^*) - (\lambda_2 a + \lambda_5 a m) \right) P(c)$$

$$= (\lambda_2 + \lambda_5 m)(a^* - a).$$

By extension, the reference interaction effect is equal to

$$RINT_{\text{ref}} = RNDE - CDE$$

$$= \left((\lambda_2 + \lambda_5(\theta_0 + \theta_2 a))(a^* - a) \right) - \left((\lambda_2 + \lambda_5 m)(a^* - a) \right)$$

$$= \lambda_5(\theta_0 + \theta_2 a - m)(a^* - a).$$

Similarly, VanderWeele (2014) shows that the pure indirect effect can be expressed in terms of the observed data as

$$RPIE = E\left(Y(a, M^R(a^*|C)) - Y(a, M^R(a|C)) \right) = \sum_c \sum_m \sum_z \left(P(m|c, a^*) - \right.$$

$$P(m|c, a) \Big) E(Y|c, a, z, m)P(z|c, a)P(c),$$

which, under the models outlined previously, is equal to

$$RPIE = \sum_c \sum_m \sum_z \big(P(m|c,a^*) - P(m|c,a)\big)E(Y|c,a,z,m)P(z|c,a)P(c)$$

$$= \sum_c \sum_m \sum_z \big(P(m|c,a^*) - P(m|c,a)\big)\Big(\lambda_0 + \lambda_1\big(c - E(C)\big) + \lambda_2 a$$

$$+ \lambda_3\big(z - E(Z|c,a)\big) + m(\lambda_4 + \lambda_5 a)\Big)P(z|c,a)P(c)$$

$$= \sum_c \sum_m \big(P(m|c,a^*) - P(m|c,a)\big)\Big(\lambda_0 + \lambda_1\big(c - E(C)\big) + \lambda_2 a$$

$$+ \lambda_3\big(E(Z|c,a) - E(Z|c,a)\big) + m(\lambda_4 + \lambda_5 a)\Big)P(c)$$

$$= \sum_c \big(E(M|c,a^*)(\lambda_4 + \lambda_5 a)\big) - \big(E(M|c,a)(\lambda_4 + \lambda_5 a)\big)P(c)$$

$$= \sum_c \Big(\big(\theta_0 + \theta_1\big(c - E(C)\big) + \theta_2 a^*\big)(\lambda_4 + \lambda_5 a)\Big)$$

$$- \Big(\big(\theta_0 + \theta_1\big(c - E(C)\big) + \theta_2 a\big)(\lambda_4 + \lambda_5 a)\Big)P(c)$$

$$= \Big(\big(\theta_0 + \theta_1\big(E(C) - E(C)\big) + \theta_2 a^*\big)(\lambda_4 + \lambda_5 a)\Big)$$

$$- \Big(\big(\theta_0 + \theta_1\big(E(C) - E(C)\big) + \theta_2 a\big)(\lambda_4 + \lambda_5 a)\Big) = \theta_2(\lambda_4 + \lambda_5 a)(a^* - a).$$

And by extension, the mediated interaction effect is equal to

$$RINT_{med} = RNIE - RPIE$$

$$= \big(\theta_2(\lambda_4 + \lambda_5 a^*)(a^* - a)\big) - \big(\theta_2(\lambda_4 + \lambda_5 a)(a^* - a)\big)$$

$$= \theta_2 \lambda_5 (a^* - a)^2.$$

Part D: Effect Estimates under Alternative Reliabilities for School Quality

In this appendix, we present effect estimates across a range of assumed reliabilities for our measure of school quality when implementing the classical error-in-variables correction. In the main text, we implemented this correction assuming a reliability of 0.7, which is consistent with estimates reported in prior research (von Hippel 2009). Tables D.1 and D.2 report effect estimates from models that assume a reliability of 0.6 and 0.8, respectively. Across the entire range of reliabilities considered here, results from the ECLS-K are substantively similar. In general, they indicate that neighborhood disadvantage negatively affects academic achievement and that school quality does not mediate or interact with these effects.

Table D.1. Effects of Neighborhood Context on 3rd Grade Achievement Test Scores Estimated from Models that Assume a Reliability of 0.8 for School Quality, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
$RATE$	-0.130	0.031	<0.001	-0.153	0.049	0.002
$RNDE$	-0.132	0.031	<0.001	-0.160	0.050	0.001
CDE	-0.128	0.027	<0.001	-0.158	0.036	<0.001
$RINT_{ref}$	-0.004	0.013	0.758	0.002	0.021	0.924
$RNIE$	0.002	0.003	0.505	0.007	0.008	0.382
$RPIE$	0.001	0.002	0.617	0.007	0.008	0.382
$RINT_{med}$	0.000	0.002	1.000	0.000	0.003	1.000

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table D.2. Effects of Neighborhood Context on 3rd Grade Achievement Test Scores Estimated from Models that Assume a Reliability of 0.6 for School Quality, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.131	0.036	<0.001	-0.151	0.074	0.041
RNDE	-0.133	0.037	<0.001	-0.162	0.074	0.029
CDE	-0.127	0.029	<0.001	-0.168	0.047	<0.001
$RINT_{ref}$	-0.006	0.018	0.739	0.006	0.034	0.860
RNIE	0.002	0.004	0.617	0.011	0.013	0.398
RPIE	0.002	0.003	0.505	0.011	0.014	0.432
$RINT_{med}$	0.001	0.003	0.739	-0.001	0.005	0.841

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Part E: Sensitivity of Effect Estimates to Alternative Model Specifications

The models we focus on in the main text constrain the effects of treatment and the mediator to be invariant across levels of the confounders. If these constraints are inappropriate and the effects of interest are not in fact invariant, then the estimates we report in the main text may suffer from model misspecification bias. In this appendix, we present effect estimates from models for school quality and achievement that permit effect heterogeneity by race, gender, and parental education.

Specifically, we present effect estimates from models of school quality with form

$$E(M|C,A) = \theta_0 + \theta_1 \delta(C) + \theta_2 A + \theta_3 \delta(C^*)A$$

and from models of the outcome with form

$$E(Y|C,A,Z,M) = \lambda_0 + \lambda_1 \delta(C)$$

$$+\lambda_2 A + \lambda_3 \delta(Z) + M(\lambda_4 + \lambda_5 A) + \delta(C^*)\big(\lambda_6 A + M(\lambda_7 + \lambda_8 A)\big),$$

where $\delta(C) = C - E(C)$, $\delta(Z) = Z - E(Z|C,A)$, and $\delta(C^*)$ denotes selected elements of $\delta(C)$, such as the (residualized) dummy variable denoting whether or not a child is white. In the first model, the interaction term $\theta_3 \delta(C^*)A$ allows the effect of treatment on the mediator to differ across levels of C^*. In the second model, the interaction term $\delta(C^*)\big(\lambda_6 A + M(\lambda_7 + \lambda_8 A)\big)$ allows the effects of treatment and the mediator on the outcome to differ across levels of C^*. A convenient property of these terms is that they are equal to zero when averaged over C^* (Wodtke et al. 2019; Zhou and Wodtke 2019). This implies that the direct, indirect, and interaction effects of interest can be constructed using exactly the same parametric expressions as provided in the main text, even though the models on which they are based no longer constrain these effects to be invariant in C^*.

Tables E.1, E.2, and E.3 present results from models that permit the effects of interest to differ by race, gender, and parental education, respectively. These estimates are very similar to those reported in the main text, which suggests that our key findings are robust to effect heterogeneity across key demographic subgroups.

Table E.1. Effects of Neighborhood Context on 3rd Grade Achievement Test Scores Estimated from Models that Permit Heterogeneity by Race, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.135	0.035	<0.001	-0.151	0.061	0.013
RNDE	-0.138	0.035	<0.001	-0.161	0.062	0.009
CDE	-0.131	0.029	<0.001	-0.165	0.042	<0.001
RINT$_{ref}$	-0.007	0.016	0.661	0.004	0.027	0.882
RNIE	0.003	0.004	0.453	0.011	0.010	0.271
RPIE	0.002	0.003	0.505	0.011	0.011	0.317
RINT$_{med}$	0.001	0.003	0.739	0.000	0.004	1.000

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table E.2. Effects of Neighborhood Context on 3rd Grade Achievement Test Scores Estimated from Models that Permit Heterogeneity by Gender, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.130	0.033	<0.001	-0.152	0.058	0.009
RNDE	-0.132	0.033	<0.001	-0.161	0.058	0.006
CDE	-0.127	0.028	<0.001	-0.160	0.040	<0.001
RINT$_{\text{ref}}$	-0.005	0.015	0.739	0.000	0.026	1.000
RNIE	0.002	0.003	0.505	0.008	0.010	0.424
RPIE	0.001	0.003	0.739	0.008	0.010	0.424
RINT$_{\text{med}}$	0.001	0.002	0.617	0.000	0.003	1.000

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table E.3. Effects of Neighborhood Context on 3rd Grade Achievement Test Scores Estimated from Models that Permit Heterogeneity by Parental Education, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.133	0.039	0.001	-0.144	0.061	0.018
RNDE	-0.135	0.039	0.001	-0.154	0.061	0.012
CDE	-0.129	0.031	<0.001	-0.161	0.042	<0.001
RINT$_{ref}$	-0.006	0.019	0.752	0.007	0.028	0.803
RNIE	0.002	0.004	0.617	0.010	0.011	0.363
RPIE	0.001	0.003	0.739	0.010	0.012	0.405
RINT$_{med}$	0.001	0.003	0.739	-0.001	0.004	0.803

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. Reliability of school quality is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Part F: Sensitivity of Effect Estimates to Alternative Measures of School Quality

In the main text, we operationalize school quality as the difference between a school's average learning rate among its 1_{st} grade students during the school year and the average learning rate among those same students during the previous summer. This measure captures a school's "value added" with respect to its students' reading and math skills under the following two assumptions: first, the influence of non-school factors on achievement must operate similarly during the school year and the summer, and second, schools must not effect summer learning. If either of these assumptions are violated, then our measure would suffer from systematic error, possibly leading to invalid inferences about the role of school quality in explaining neighborhood effects.

One approach to evaluating the sensitivity of our results to potential violations of these assumptions is to reanalyze the data with measures of school quality that subtract only a fraction of the summer learning rate from the school-year learning rate, which adjusts for the possibility that school and non-school contributions to learning differ from that assumed above for the school year versus the summer (Downey et al. 2008). The proper weight to give summer learning is unknown, but it must lie somewhere between one, which is the weight given to it in our featured analysis from the main text, and zero. In this appendix, we therefore replicate our analysis using, first, a measure that equates a school's quality with the difference between its school-year learning rate and one-half the learning rate among its students during the summer, and second, a measure that gives the summer learning rate a weight of zero and thus equates a school's quality with its school-year learning rate alone.

Results from this analysis are presented in Tables F.1 and F.2. They are very similar to those presented in the main text, regardless of the weight given to the summer learning rate when

81

operationalizing school quality. This suggests that our findings are highly robust to potential violations of the assumptions motivating our favored measure of school quality.

Table F.1. Estimated Effects of Neighborhood Context on 3rd Grade Achievement Test Scores based on Measures of School Quality that Assume Non-school Determinants of Achievement are One-half as Influential during the School Year compared with the Summer, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.131	0.034	<0.001	-0.151	0.056	0.007
RNDE	-0.133	0.034	<0.001	-0.157	0.056	0.005
CDE	-0.128	0.028	<0.001	-0.165	0.039	<0.001
RINT$_{ref}$	-0.006	0.015	0.689	0.008	0.024	0.739
RNIE	0.003	0.004	0.453	0.006	0.012	0.617
RPIE	0.002	0.003	0.505	0.007	0.013	0.591
RINT$_{med}$	0.001	0.002	0.617	-0.001	0.003	0.739

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. School quality is computed as the difference between a school's first grade learning rate and one-half the learning rate of its students during the previous summer. Its reliability is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Table F.2. Estimated Effects of Neighborhood Context on 3rd Grade Achievement Test Scores based on Measures of School Quality that Assume Non-school Determinants of Achievement are Inoperative during the School Year, ECLS-K Class of 1998-99 (n=6,040, k=310)

Estimand	Math Test Scores			Reading Test Scores		
	Est.	SE	P-value	Est.	SE	P-value
RATE	-0.130	0.037	<0.001	-0.151	0.051	0.003
RNDE	-0.131	0.037	<0.001	-0.149	0.050	0.003
CDE	-0.129	0.029	<0.001	-0.158	0.037	<0.001
RINT$_{\text{ref}}$	-0.003	0.016	0.851	0.010	0.021	0.634
RNIE	0.001	0.006	0.867	-0.003	0.009	0.739
RPIE	0.001	0.006	0.867	-0.003	0.010	0.764
RINT$_{\text{med}}$	0.000	0.002	1.000	0.000	0.002	1.000

Notes: Estimates are combined across MI datasets. SEs are computed using the block bootstrap. P-values come from the standard normal distribution. School quality is equated with a school's first grade learning rate. Its reliability is assumed to be 0.7.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.

Part G: Sensitivity of Effect Estimates to Unmeasured Confounding

RWR estimates of direct and indirect effects are biased if there are any unobserved confounders of the treatment-outcome, mediator-outcome, or treatment-mediator relationships. Following Wodtke and Zhou (2019), we conduct a formal sensitivity analysis that examines whether our inferences about these effects are sensitive to hypothetical patterns of unobserved confounding.

Consider the following set of linear structural equations for neighborhood disadvantage, school quality, and achievement test scores:

$$A = \gamma_0 + \gamma_1(C - \alpha_0) + \varepsilon_A$$

$$M = \theta_0 + \theta_1(C - \alpha_0) + \theta_2 A + \varepsilon_M$$

$$Y = \lambda_0 + \lambda_1(C - \alpha_0) + \lambda_2 A + \lambda_3\big(Z - (\beta_0 + \beta_1 C + \beta_2 A)\big) + M(\lambda_4 + \lambda_5 A) + \varepsilon_Y.$$

If there is no unobserved confounding of the treatment-outcome, mediator-outcome, or treatment-mediator relationships, then the error terms, $\{\varepsilon_A, \varepsilon_M, \varepsilon_Y\}$, are pairwise independent.

If, however, the treatment-outcome relationship is confounded by unobserved factors, then ε_A and ε_Y will be correlated, and RWR estimates of the $RNDE$ will be biased. Specifically, if $\varepsilon_Y = \phi_{AY}\varepsilon_A + \psi_{AY}$ and $E(\psi_{AY}|C, A, Z, M) = 0$, the bias in estimates of the $RNDE$ due to unobserved treatment-outcome confounding is equal to

$$\text{Bias}_{AY}(RNDE) = \frac{\text{sd}(\psi_{AY})}{\text{sd}(\varepsilon_A)} \frac{\rho_{AY}}{\sqrt{1-\rho_{AY}^2}}(a^* - a),$$

where $\text{sd}(\varepsilon_A)$ can be estimated from a regression of A on C, $\text{sd}(\psi_{AY})$ can be estimated from our model for the outcome, and $\rho_{AY} = \text{corr}(\varepsilon_A, \varepsilon_Y)$ is the unknown correlation between the errors. With this expression, we can construct and plot a set of bias-adjusted estimates by evaluating $\text{Bias}_{AY}(RNDE)$ across a range of values for ρ_{AY} and then subtracting this bias term from the RWR point and interval estimates of the $RNDE$. If bias-adjusted estimates of the $RNDE$ remain

significant even under fairly strong error correlations, this would bolster confidence that our causal inferences are robust to unobserved treatment-outcome confounding.

Figure G.1 plots bias-adjusted estimates of the $RNDE$ on reading and math test scores as a function of the error correlation, $\rho_{AY} = \text{corr}(\varepsilon_A, \varepsilon_Y)$. A value of $\rho_{AY} = 0$ indicates no unobserved treatment-outcome confounding and simply reproduces the estimates reported in Table 4 from the main text. A value of $\rho_{AY} < 0$ implies that families select into disadvantaged neighborhoods on the basis of unobserved factors that hinder the academic achievement of their children, net of observed covariates. These factors might include parental drug abuse or incarceration, for example. A value of $\rho_{AY} > 0$, by contrast, implies that families select into disadvantaged neighborhoods on the basis of unobserved factors that improve their children's academic achievement. We view this scenario as unlikely, and thus we only report bias-adjusted estimates for $-0.3 \leq \rho_{AY} \leq 0$.

This figure indicates that bias-adjusted estimates of the $RNDE$ on both reading and math achievement would reach zero under an error correlation of about -0.10. It also indicates that, under an error correlation of about -0.05, bias-adjusted estimates of the $RNDE$ would no longer achieve statistical significance at conventional thresholds. By way of reference, the partial correlation between parental education (measured in years of completed schooling) and reading test scores is about 0.10 after adjusting for all other observed confounders. Thus, the correlation between error terms in the treatment and outcome models would need to be comparable in absolute value to that between education and achievement, net of other controls, in order to alter our conclusions about the $RNDE$. Given that parental education is among the most powerful predictors of child academic achievement, this suggests that our estimates are moderately robust to unobserved treatment-outcome confounding.

Next, consider the scenario where the mediator-outcome relationship is confounded by unobserved factors. In this case, ε_M and ε_Y will be correlated, and RWR estimates of both the $RNDE$ and $RNIE$ will be biased. Specifically, if $\varepsilon_Y = \phi_{MY}\varepsilon_M + \psi_{MY}$ and $E(\psi_{MY}|C, A, Z, M) = 0$, the bias in estimates of the $RNDE$ due to unobserved mediator-outcome confounding is equal to

$$\text{Bias}_{MY}(RNDE) = -\theta_2 \frac{\text{sd}(\psi_{MY})}{\text{sd}(\varepsilon_M)} \frac{\rho_{MY}}{\sqrt{1-\rho_{MY}^2}} (a^* - a),$$

and the bias in estimates of the $RNIE$ is equal to

$$\text{Bias}_{MY}(RNIE) = \theta_2 \frac{\text{sd}(\psi_{MY})}{\text{sd}(\varepsilon_M)} \frac{\rho_{MY}}{\sqrt{1-\rho_{MY}^2}} (a^* - a),$$

where $\text{sd}(\varepsilon_M)$ and θ_2 can be estimated from our model for the mediator, $\text{sd}(\psi_{MY})$ can be estimated from our model for the outcome, and $\rho_{MY} = \text{corr}(\varepsilon_M, \varepsilon_Y)$ is the unknown error correlation. As before, we can use these expressions to construct and plot a set of bias-adjusted estimates across a range of values for ρ_{MY} that allow us to assess whether our inferences about the $RNDE$ and $RNIE$ are sensitive to unobserved mediator-outcome confounding.

Figure G.2 plots bias-adjusted estimates of the $RNDE$ and $RNIE$ as a function of the error correlation, $\rho_{MY} = \text{corr}(\varepsilon_M, \varepsilon_Y)$. A value of $\rho_{MY} = 0$ indicates no unobserved mediator-outcome confounding and reproduces our estimates from the main text. A value of $\rho_{MY} > 0$ implies that, net of observed covariates, families select into higher quality schools on the basis of unobserved factors that improve the academic achievement of their children. These factors might include parental commitment to academic learning, for example. A value of $\rho_{AY} < 0$, by contrast, implies that families select into higher quality schools on the basis of unobserved factors that actually hinder their children's academic achievement. This might occur if deficient parents recognize their limitations and consequently seek out better schools for their children in

order to compensate for their own personal shortcomings. We view both scenarios as at least minimally plausible, and thus we report bias-adjusted estimates for $-0.3 \leq \rho_{MY} \leq 0.3$.

The upper panel of Figure G.2 displays bias-adjusted estimates of the $RNDE$. For both reading and math achievement, these estimates are highly robust. Specifically, under any error correlation from -0.3 to 0.3, the bias-adjusted estimates indicate that exposure to a disadvantaged neighborhood has a negative direct effect that is substantively large and statistically significant at conventional thresholds. The lower panel of Figure G.2 displays bias-adjusted estimates of the $RNIE$. These estimates only achieve statistical significance at extreme values of the error correlation, and even then they remain substantively trivial in magnitude. This suggests that our main substantive conclusions about the explanatory role of school quality are highly robust to unobserved mediator-outcome confounding.

Finally, consider the scenario where the treatment-mediator relationship is confounded by unobserved factors. In this case, ε_A and ε_M will be correlated, and RWR estimates of both the $RNDE$ and $RNIE$ will be biased. Specifically, if $\varepsilon_M = \phi_{AM}\varepsilon_A + \psi_{AM}$ and $E(\psi_{AM}|C,A) = 0$, the bias in estimates of the $RNDE$ due to unobserved treatment-mediator confounding is equal to

$$\text{Bias}_{AM}(RNDE) = \frac{\text{sd}(\psi_{AM})}{\text{sd}(\varepsilon_A)} \frac{\rho_{AM}}{\sqrt{1-\rho_{AM}^2}} \lambda_5 (a - \gamma_0)(a^* - a),$$

and the bias in estimates of the $RNIE$ is equal to

$$\text{Bias}_{AM}(RNIE) = \frac{\text{sd}(\psi_{AM})}{\text{sd}(\varepsilon_A)} \frac{\rho_{AM}}{\sqrt{1-\rho_{AM}^2}} (\lambda_4 + \lambda_5 a^*)(a^* - a),$$

where $\text{sd}(\varepsilon_A)$ can be estimated from a regression of A on C, $\text{sd}(\psi_{AM})$ can be estimated from our model for the mediator, $\{\lambda_4, \lambda_5\}$ can be estimated by RWR applied to our model for the outcome, and $\rho_{AM} = \text{corr}(\varepsilon_A, \varepsilon_M)$ is the unknown error correlation. With these expressions, we can examine whether our inferences about the $RNDE$ and $RNIE$ are sensitive to unobserved

treatment-mediator confounding by constructing and plotting a set of bias-adjusted estimates across different values of ρ_{AM}.

Figures G.3 plot bias-adjusted estimates of the $RNDE$ and $RNIE$, respectively, as a function of the error correlation, $\rho_{AM} = \text{corr}(\varepsilon_A, \varepsilon_M)$. A value of $\rho_{AM} = 0$ indicates no unobserved treatment-mediator confounding and reproduces our estimates from the main text. A value of $\rho_{AM} < 0$ implies that families select into disadvantaged neighborhoods on the basis of unobserved factors that lead their children to attend lower quality schools, whereas a value of $\rho_{AY} > 0$ implies that unobserved selection into disadvantaged neighborhoods occurs on the basis factors that promote attendance at higher quality schools, net of observed covariates. We view the second of these scenarios as implausible and therefore report bias-adjusted estimates only for $-0.3 \leq \rho_{MY} \leq 0.0$.

The upper panel of Figure G.3 displays bias-adjusted estimates of the $RNDE$. These results suggest that our direct effect estimates are highly robust to unobserved treatment-mediator confounding. Specifically, they indicate that exposure to a disadvantaged neighborhood has a negative direct effect that is substantively large and statistically significant across the full range of error correlations considered here. The lower panel of Figure G.3 displays bias-adjusted estimates of the $RNIE$. These estimates become positive and statistically significant at moderate values of the correlation between errors for treatment and the mediator. This suggests that, if anything, differences in school quality across neighborhoods may mitigate, or suppress, the harmful effects of living in a disadvantaged neighborhood. Nevertheless, the bias-adjusted estimates remain substantively small at all but fairly extreme levels of the error correlation.
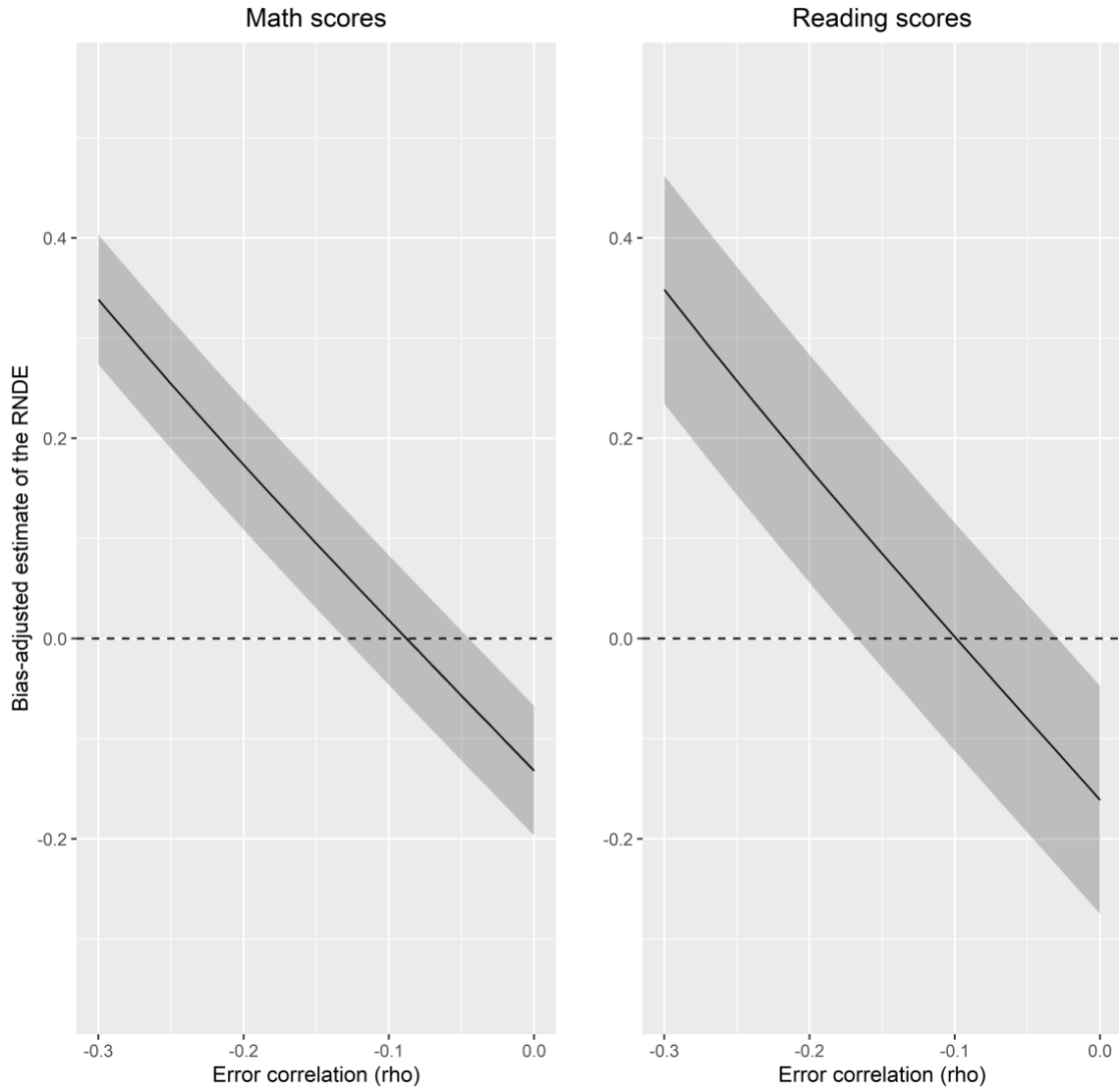
Figure G.1. Bias-adjusted Estimates of the $RNDE$ as Functions of the Error Correlation $\rho_{AY} = \text{corr}(\varepsilon_A, \varepsilon_Y)$.

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.
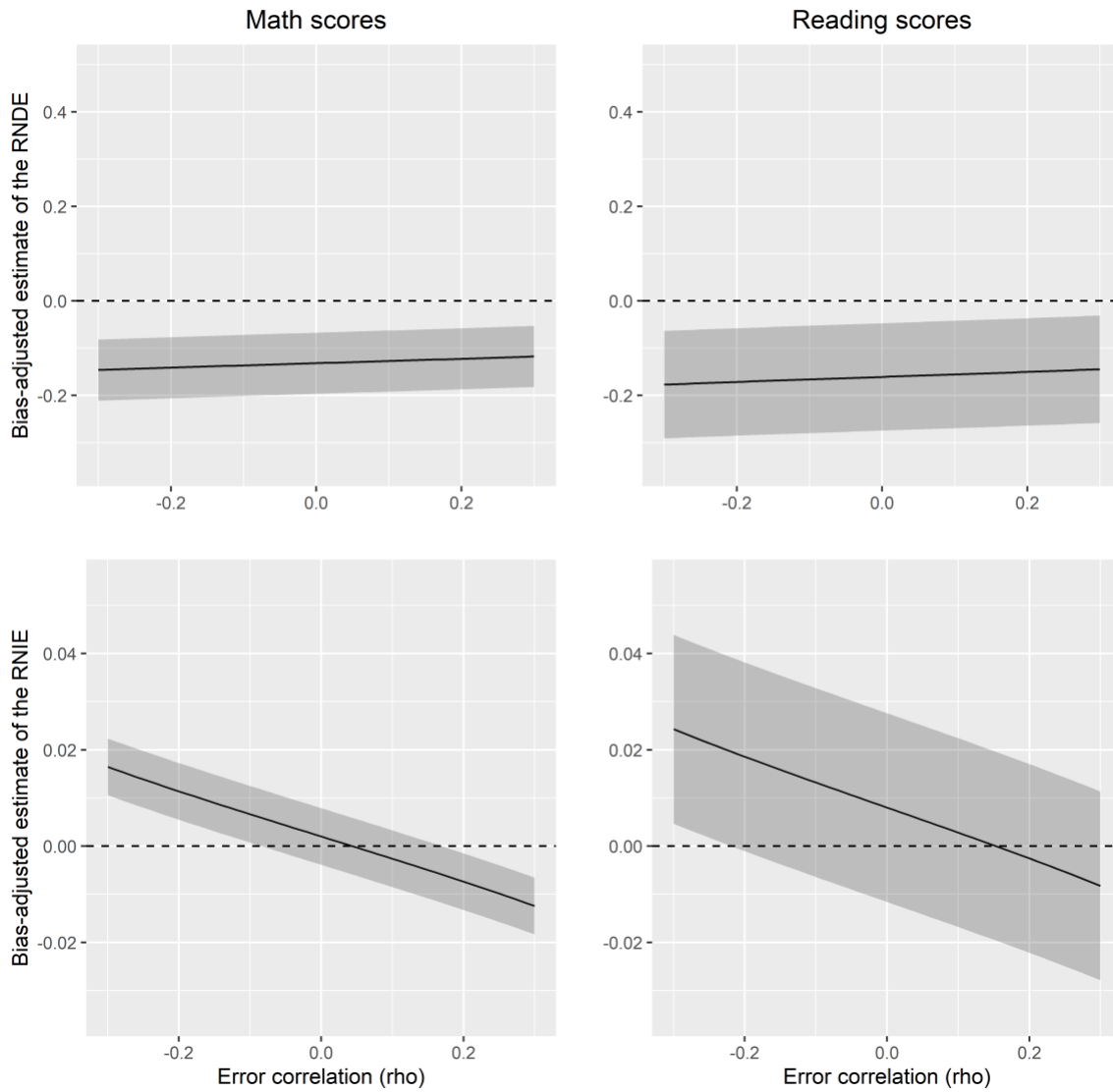
Figure G.2. Bias-adjusted Estimates of the $RNDE$ and $RNIE$ as Functions of the Error Correlation $\rho_{MY} = \text{corr}(\varepsilon_M, \varepsilon_Y)$

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.
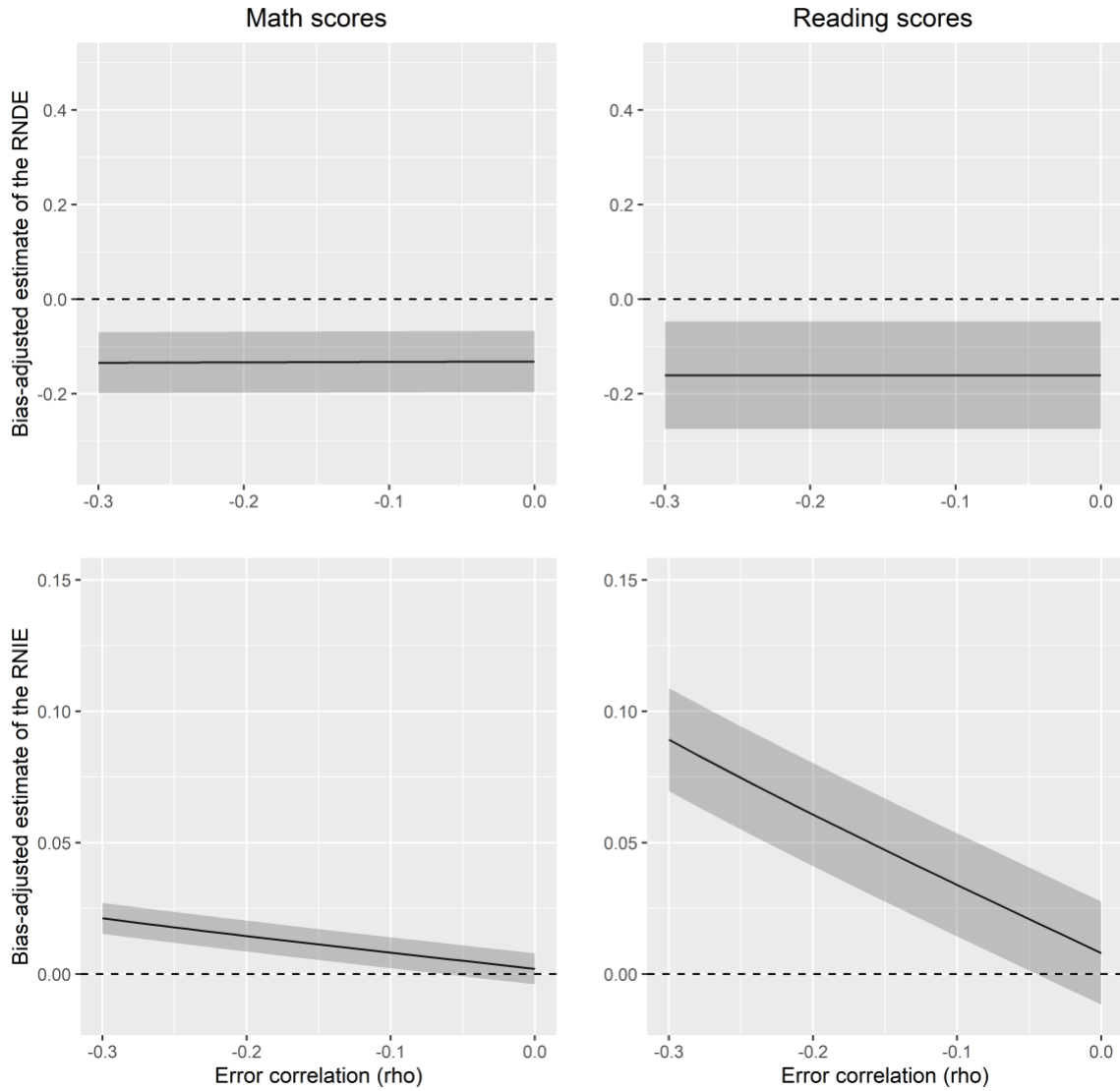
Figure G.3. Bias-adjusted Estimates of the $RNDE$ and $RNIE$ as Functions of the Error Correlation $\rho_{AM} = \mathrm{corr}(\varepsilon_A, \varepsilon_M)$

Source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, Early Childhood Longitudinal Survey - Kindergarten Class of 1998, "Direct Child Assessments in waves 1-5," "Student record abstract form (kindergarten and 1st grade)," "School Administrator Questionnaire in First Grade," and "Parent Interview, Waves 1 and 2"; GeoLytics Neighborhood Change Database, 2010; U.S. Department of Education, Institute of Education Sciences, Common Core of Data, 1999-2000.