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Student Neighborhoods, Schools, and Test Score Growth: Evidence from Milwaukee, Wisconsin

Deven Carlson[1] and Joshua M. Cowen[2]

Abstract

Schools and neighborhoods are thought to be two of the most important contextual influences on student academic outcomes. Drawing on a unique data set that permits simultaneous estimation of neighborhood and school contributions to student test score gains, we analyze the distributions of these contributions to consider the relative importance of schools and neighborhoods in shaping student achievement outcomes. We also evaluate the sensitivity of estimated school and neighborhood contributions to the exclusion of an explicit measure of the other context, indicating the extent to which bias may exist in studies where either measure is unavailable. Taken together, results of these analyses provide substantial insight into the influences of two of the most important contextual settings in students' lives.

Keywords

education, schools, neighborhoods, student achievement

Schools and neighborhoods are thought to be two of the most important contextual influences on student academic outcomes. The perceived importance of these contexts is evidenced by the significant amount of policy attention they receive and the substantial scholarly literatures surrounding them. The school effects literature, starting with Coleman's pioneering work in the 1960s (Coleman et al. 1966), recognizes that schools are a major source of variation in student academic outcomes (e.g., Goldstein 1997; Hill and Rowe 1996; Konstantopoulos and Borman 2011; Konstantopoulos and Hedges 2008; Raudenbush and Bryk 1986) and that such variation has links to social and economic outcomes (e.g., Card and Krueger 1992; Hanushek 1986). Although explanations for differences in school quality vary, implicit in many recent educational reforms is the recognition that school context matters. School accountability systems (Booher-Jennings 2005;

Dee and Jacob 2011; Jennings and Sohn 2014) and alternative governance structures (Berends, Cannata, and Goldring 2011), such as charter schooling (Buddin and Zimmer 2005) and private school vouchers (Rouse 1998; Wolf et al. 2013), are intended to improve student outcomes by changing the schooling experience.

Policy makers have also initiated neighborhood-based interventions—most notably, the Moving to Opportunity (MTO) experiment—in the hopes of improving student achievement and attainment (see Clampet-Lundquist and Massey

[1]University of Oklahoma, Norman, OK, USA
[2]Michigan State University, East Lansing, MI, USA

Corresponding Author:
Deven Carlson, University of Oklahoma, 205 Dale Hall Tower, 455 W. Lindsey St., Norman, OK 73019, USA.
Email: decarlson@ou.edu

2008; Sanbonmatsu et al. 2011). Such interventions rest on a body of studies demonstrating that the quality of students' neighborhoods is associated with their educational outcomes (e.g., Aaronson 1998; Brooks-Gunn et al. 1993; Crane 1991; Duncan 1994; Owens 2010; Rosenbaum 1995; Sharkey and Elwert 2011; Wodtke, Harding, and Elwert 2011). Considered together, existing studies provide evidence that both schools and neighborhoods shape students' academic outcomes, but the tendency to study these contexts in isolation—studies typically analyze either school or neighborhood effects—has limited our understanding of the relative influence of these two contexts as well as how they interact to affect students' educational outcomes.

In this article, we analyze the relationship between neighborhoods, schools, and student achievement gains. Drawing on five years of student-level data from a large, urban school district, we simultaneously estimate the contributions of neighborhoods and schools to student test score gains and analyze the distributions of these estimated neighborhood and school contributions. This analysis provides insight into the relative importance of schools versus neighborhoods in shaping a major academic outcome. We also gauge the sensitivity of estimated school contributions to the exclusion of an explicit measure of the neighborhood in which a student resides. Similarly, we assess the sensitivity of estimated neighborhood contributions to the exclusion of a measure of the school a student attends. These assessments have several important implications for the inferences drawn from contextual analyses of schools and neighborhoods. Although our analyses are limited to the singular academic outcome of student achievement gains over the relatively short time period of a single year, these results provide useful insight into the influences of two of the most important contextual settings in students' lives.

SCHOOL AND NEIGHBORHOOD EFFECTS IN AN EVOLVING CONTEXT

That social contexts—schools, neighborhoods, and families—influence student academic outcomes is a relatively uncontroversial point of view among scholars. Well-developed literatures provide compelling theoretical accounts of the mechanisms through which neighborhood and schooling contexts can influence student achievement and attainment. The neighborhood-effects literature, for example, specifies four broad underlying mechanisms—social isolation, resources, individuals' physical environments, and social organization—through which students' neighborhoods could influence their achievement and attainment outcomes (Sampson, Morenoff, and Gannon-Rowley 2002). Similarly, the school-effects literature points to several factors, including policy, practice, peers, and resources—both human and financial—that have the potential to affect student achievement and attainment (Elliott 1998; Greenwald, Hedges, and Laine 1996; Raudenbush and Willms 1995; Sørensen and Morgan 2006; Wenglinsky 1997).

Empirically estimating the separate contributions of schools and neighborhoods to student academic outcomes has proven difficult for two main reasons. First, the historically dominant policy of assigning students to schools on the basis of their neighborhood of residence results in the observational equivalence of schools and neighborhoods—an equivalence that complicates any efforts to disentangle the effects of the two contexts. Second, efforts to validly estimate the contributions of neighborhoods and schools to student academic outcomes are complicated by the necessity of accounting for family context, the effects of which may manifest both directly, influencing students' achievement or attainment, and indirectly, via the selection of neighborhood and, implicitly, school.

Perhaps due to these difficulties, much of the previous work analyzing neighborhood or school effects on student outcomes examines one context without explicitly accounting for the other. For example, the early neighborhood-effects literature generally estimates neighborhood contributions by regressing academic outcomes on observable neighborhood characteristics and a set of socioeconomic and demographic controls (e.g., Ainsworth 2002; Brooks-Gunn et al. 1993; Brooks-Gunn, Klebanov, and Duncan 1996; Chase-Lansdale and Gordon 1996; Duncan, Boisjoly, and Mullan Harris 2001; Duncan, Brooks-Gunn, and Klebanov 1994; Kohen et al. 2002). By excluding school characteristics, these studies treat schools—either explicitly or implicitly—as a neighborhood resource. The general result emerging from these early analyses is one where the quality of a student's neighborhood—as measured by observable socioeconomic

characteristics—is positively associated with his or her cognitive test scores, although the studies demonstrate heterogeneity in the substantive magnitude of the detected relationships. Sharkey and Elwert (2011) note that these studies often control for factors that may be endogenous to neighborhood quality—they specify family income and health as examples—and thus potentially underestimate the influence of neighborhoods on relevant outcomes. Relying on data from the Panel Study of Income Dynamics and employing methods designed to mitigate the methodological issue noted earlier, the authors find statistically significant and substantively strong relationships between neighborhood and student achievement outcomes. Sampson, Sharkey, and Raudenbush (2008), using comparable methods and observational data from Chicago, find substantively similar results. A set of recent studies have relied on plausibly exogenous variation in neighborhood context as the basis for estimating effects on academic outcomes (e.g., Burdick-Will et al. 2011; Jacob 2004; Ludwig et al. 2009; Sanbonmatsu et al. 2011). The design of these studies, which have returned mixed evidence of sustained neighborhood effects on student achievement, allows for recovery of valid estimates of neighborhood effects without accounting for students' schooling context.

Measures of neighborhood context are similarly absent from much of the school-effects literature. Implicitly assuming that neighborhoods either have no effect on student academic outcomes or only indirect effects through other contexts, such as family—and are thus accounted for by inclusion of those contextual measures—studies dating back to at least the Coleman Report (Coleman et al. 1966) contribute to the consensus that "schools matter." Recent work has focused on measuring the overall contribution of school context to variation in student outcomes (e.g., Konstantopoulos and Borman 2011; Konstantopoulos and Hedges 2008; Raudenbush and Bryk 1986), whether due to factors like human or financial resources (e.g., Card and Krueger 1996; Elliott 1998; Hedges, Laine, and Greenwald 1994; Wenglinsky 1997), peers (e.g., Hallinan and Williams 1990), or school practice, including "administrative leadership, curricular content, utilization of resources and classroom instruction" (Raudenbush and Willms 1995:310). Estimates generally indicate that between-school sources of variation account for 10 to 20 percent of all variance in

student achievement outcomes (Konstantopoulos 2006) and that schools vary considerably in their effectiveness in improving student outcomes (Hanushek et al. 2007; Kane and Staiger 2002).

In a context where school assignment rules produce an inextricable link between students' school and neighborhood environments, the analytic approach used in most prior empirical research on their effects—examining one context without explicitly accounting for the other—is unproblematic. In an important sense, these neighborhoods and schools are truly extensions of one another and can validly be treated as such analytically. Over the past two decades, however, policies have greatly weakened the relationship between the neighborhoods in which students reside and the schools they attend. Through charter schooling, intradistrict open enrollment, and other similar policies, states, cities, and school districts have implemented changes to enrollment rules that expand students' pool of potential public schooling options beyond their immediate residential area (National Center for Education Statistics 2013). Recent data indicate that such changes have resulted in 52 percent of students in urban areas being offered a choice within their public school system (National Center for Education Statistics 2009). Of these, more than half elect to attend a school other than the one to which they would typically be assigned. These students share their school experiences with a wholly different group of peers from those with whom they share daily neighborhood life— these students' schooling contexts are conceptually distinct from their neighborhood contexts. From an analytic perspective, the conceptual separation of the two contexts means that estimating the effects of one context without explicitly accounting for the other will likely produce biased estimates. This bias will be positive—overstating the effects of one context—if school and neighborhood conditions are positively correlated, but it will be negative—understating the true impact of school or neighborhood—if the two contexts are negatively correlated.

The separation of students' neighborhood and school contexts requires reconsideration of the relationship between schools, neighborhoods, and student academic outcomes. This reconsideration does not necessitate development or specification of new mechanisms through which neighborhoods and schools may affect academic outcomes. Rather, it requires recognizing that students increasingly grow, interact, and ultimately learn in these two distinct contexts (Owens 2010), and

the separate effects of each context must be assessed if we hope to gain a more complete understanding of the factors that influence students' academic outcomes. Fortunately, severance of the link between neighborhoods and schools also generates the analytic leverage necessary to inform this reconsideration. As we will describe, the break in the relationship between neighborhood and school location allows for consideration of the effects of one context alongside the effects of the other.

A small number of prior studies have simultaneously analyzed school and neighborhood relationships with student outcomes. For example, Cook and colleagues (2002) analyze data from a sample of more than 12,000 students in Prince George's County, Maryland, to examine how school, neighborhood, family, and friendship contexts affect adolescents' educational and developmental outcomes. Drawing on multiattribute indexes created for each of the four contexts, the authors found each context to be significantly related to changes in a "success index," which included student achievement outcomes, over the course of 19 months. Similarly, Owens's (2010) analysis of data from the National Longitudinal Study of Adolescent Health indicates that an individual's level of neighborhood advantage, relative to the average level of neighborhood advantage of peers in the same high school, predicts high school graduation. Owens's study also reveals, however, that an individual's absolute level of neighborhood advantage predicts bachelor degree completion.

We build on this work by exploiting the break in the relationship between students' schools and neighborhoods created by Milwaukee's intradistrict open enrollment program to simultaneously estimate the separate contributions of these two contexts to one-year gains in student test scores. In doing so, we explicitly treat schools and neighborhoods as distinct contexts that affect academic outcomes, which better reflects the empirical reality for an ever-increasing number of children. Furthermore, our simultaneous analysis of school and neighborhood effects decreases the likelihood of misattributing the effects of one context to another (Cook 2003) and can thus facilitate a more accurate understanding of the role each context plays in shaping an important student outcome. Finally, our joint estimation of contextual effects allows for a direct comparison of the magnitude of neighborhood and school influences on student achievement outcomes. In doing so, the results offer the

chance to expand on theories of both neighborhood and school influences rather than select between them. Our evidence also has the potential to inform debates over the sources of inequality in educational outcomes; it can shed light on the extent to which attendance at a high-quality school might offset the effects of residing in a disadvantaged neighborhood or how a student attending an advantaged school who resides in an advantaged neighborhood might be doubly advantaged, relative to peers who live day-to-day life in less advantaged contexts.

In summary, evolution in the relationship between students' neighborhood and schooling contexts provides a compelling theoretical case for jointly estimating the separate contributions of these contexts to student outcomes. This is not to say that simply accounting for both school and neighborhood contexts in a single empirical model is a panacea for understanding how these contexts shape student outcomes. Several analytic issues must be addressed, including the relationship between school and neighborhood quality and the roles that families play—directly and indirectly—in shaping student outcomes.

DATA

Our analyses are based on a data set containing records from the universe of students enrolled in Milwaukee Public Schools (MPS) between 2006-07 and 2010-11, where a substantial intradistrict open enrollment system breaks at least part of the traditional relationship between neighborhood location and school assignment. In Milwaukee, parents select schools according to a "three-choice enrollment process": Parents file their three most preferred schools with the district and, subject to availability, MPS assigns students to schools on the basis of these listed preferences.[1] As we will describe in greater detail, this process, while perhaps limiting the generalizability of our results to cities with similar public school choice programs, organizes students across schools and neighborhoods in such a way as to permit simultaneous estimation of separate school and neighborhood contributions to student achievement outcomes.

Our data contain the scores of all MPS students who took the Wisconsin Knowledge and Concepts Examination (WKCE)—the assessment Wisconsin uses to comply with federal No Child Left Behind requirements—in the fall of the 2006-7,

2007-8, 2008-9, 2009-10, or 2010-11 academic years.[2] Along with the WKCE results, which are standardized using the districtwide mean and standard deviation for the proper grade, subject, and year, the data set contains additional valuable information, including a unique student identifier and standard student demographics, such as sex, race, grade, free or reduced-price lunch status, English language learner status, and special education status. These data also record the school attended by each student, which allows us to generate school-level characteristics for all test takers in the school, such as average school achievement in reading and math, the percentage of female students, the school's racial composition, the percentage of students eligible for free or reduced-price lunch, and the percentages of students who are English language learners or receive special education services. Finally, these data include an annual record of students' residential neighborhood, operationalized as U.S. Census tract.[3]

Nested within county boundaries, census tracts are small geographic units that generally contain between 1,500 and 8,000 individuals, with a targeted population of 4,000. According to the U.S. Census Bureau, tracts attempt to reflect a neighborhood's true character—efforts are made to make them homogenous along dimensions such as socioeconomic status, demographic characteristics, and quality of housing stock (Iceland and Steinmetz 2003). Tracts are also drawn to follow relevant physical boundaries, such as highways, waterways, and railroad tracks. Although undoubtedly imperfect representations of perceived neighborhood boundaries (Coulton et al. 2001), census tracts represent the best available measure. Within our data set, students reside in approximately 220 different census tracts and attend about 160 unique elementary and middle schools across the city of Milwaukee.

With a record of each student's annual census tract of residence in hand, we then extracted—for each census tract in Milwaukee—several observable tract characteristics from the American Community Survey and appended them to our data set. These tract-level characteristics are wide-ranging and diverse, including median rent, average income, income inequality, average poverty level, average educational attainment level, household structure, average employment rates, and other demographic and socioeconomic characteristics. Addition of these tract measures was the final stage in construction of the data set underlying the following analyses.

Table 1 presents descriptive statistics for the estimation sample that underlies our analyses. The top panel presents individual-level characteristics of students in our sample, the middle panel presents characteristics of the schools they attend, and the bottom panel depicts characteristics of the neighborhoods in which they reside. Over half of the students in our sample are black, about a quarter are Hispanic, and 80 percent are eligible for free or reduced-price lunch. These students attend schools that, on average, enroll about 385 students, are about 56 percent black and 23 percent Hispanic, and have approximately 17 percent of students with individualized education plans. These students disproportionately reside in disadvantaged neighborhoods. On average, students live in tracts where 35 percent of households are headed by a single parent, the unemployment rate is 15 percent, only 15 percent of adults have a bachelor's degree or higher, a full 12 percent of households have income less than half of the federal poverty level, and the median income is only about $38,500.

ESTIMATING SCHOOL AND NEIGHBORHOOD CONTRIBUTIONS TO STUDENT TEST SCORE GAINS

Valid estimation of neighborhood and school contributions to student test score gains is possible only if students are sufficiently cross-classified in these two contextual settings. That is, estimation of the two sets of parameters requires neighborhoods to be linked through the schools that students attend and schools to be linked through the neighborhoods in which students reside. The linkages of neighborhoods through schools and schools through neighborhoods need not be direct—they can be linked indirectly (i.e., there does not have to be a student from each neighborhood attending each school and a student from each school living in each neighborhood).

Our data have substantial cross-classification of students in schools and neighborhoods—a pattern explained, in part, by the fact that MPS provides families with substantial latitude in selecting the specific school their children will attend. To illustrate the broad distribution of students across neighborhoods and schools, consider Figures 1 and 2, recalling that students in our data set reside

Table 1. Descriptive Statistics for Analytic Sample of Value-Added Model Predicting Student Achievement

Characteristic	N	M	SD	Min	Max
Student characteristics					
Female	95,988	0.487	NA	0	1
Black	95,988	0.560	NA	0	1
Hispanic	95,988	0.233	NA	0	1
White	95,988	0.125	NA	0	1
Asian	95,988	0.049	NA	0	1
Other race	95,988	0.032	NA	0	1
Special education	95,988	0.177	NA	0	1
Eligible for free or reduced-price lunch	95,988	0.800	NA	0	1
English language learner	95,988	0.088	NA	0	1
Reading score (z score)	95,988	0.033	0.977	−3.668	5.632
Math score (z score)	95,988	0.042	0.977	−4.191	4.672
School characteristics					
Percentage female	95,988	48.8	4.1	0.0	100.0
Percentage black	95,988	55.7	34.7	0.0	100.0
Percentage Hispanic	95,988	22.9	28.7	0.0	99.4
Percentage white	95,988	13.1	16.6	0.0	74.1
Percentage Asian	95,988	4.9	9.7	0.0	96.6
Percentage special education	95,988	17.1	5.8	0.0	81.8
Percentage eligible for free or reduced-price lunch	95,988	79.8	15.1	0.0	100.0
Percentage English language learner	95,988	8.0	12.0	0.0	65.9
Enrollment	95,988	384.6	224.6	1	983
Average reading score (z score)	95,988	0.017	0.381	−1.990	1.143
Average math score (z score)	95,988	0.018	0.345	−3.148	1.100
Neighborhood characteristics					
Percentage single-parent households	95,988	34.9	13.4	0.8	75.5
Percentage of kids living with parents	95,988	85.4	9.4	55.1	100.0
Percentage unemployed	95,988	14.8	8.7	0.0	46.9
Percentage of adults with bachelor's degree	95,988	14.8	11.5	0.0	81.5
Percentage of houses owner occupied	95,988	46.1	17.3	0.0	96.7
Percentage of houses vacant	95,988	11.7	7.3	0.0	36.4
Percentage of households with income less than half the poverty level	95,988	12.1	7.6	0.0	53.6
Percentage of households with income twice the poverty level	95,988	43.9	18.8	5.4	91.7
Median family income ($)	95,988	38,548	15,322	11,458	134,722

Note: Descriptive statistics for estimation sample from value-added model predicting student reading achievement (Equation 1). Sample contains all observations from students attending Milwaukee Public Schools between 2007-8 and 2010-11 who have valid test scores in both reading and math at time t and time $t − 1$ (e.g., 2007-8 and 2006-7). Student reading and math scores were standardized using the districtwide mean and standard deviation for the proper grade, subject, and school year.

in approximately 220 unique census tracts and attend about 160 different elementary and middle schools.[4] Figure 1 presents the distribution of schools by the number of unique tracts in which students attending that school reside. Across the five years our data span, most schools draw students from multiple neighborhoods, typically more than 50. Similarly, Figure 2 demonstrates that in most tracts, students attended more than 50 different elementary and middle schools across
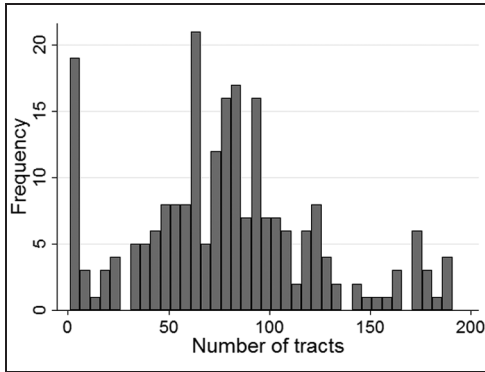
Figure 1. Distribution of schools, by number of unique tracts in which students reside.
Note: Figure presents a histogram of schools by the number of unique resident neighborhoods of students enrolled in the school over the five years our data span.
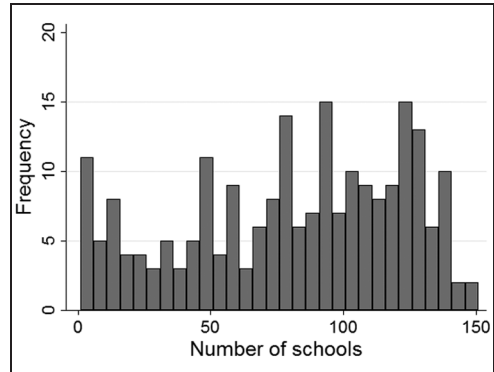

Figure 2. Distribution of tracts, by number of unique schools that students attend.
Note: Figure presents a histogram of neighborhoods by the number of unique schools attended by students residing in the neighborhood over the five years our data span.

the five years we observe. These figures illustrate the cross-classification necessary to simultaneously estimate separate neighborhood and school contributions to student test score gains.

Given the requisite cross-classification of students in neighborhoods and schools, we isolate the relationships between neighborhoods, schools, and student test score growth using the following model:

$$Y_{ijkt} = Y_{ijkt-1}\beta + G_{it}\rho + H_{ijkt}\tau + N_k\theta + S_j\gamma + \varepsilon_{ijkt}. \quad (1)$$

In this model, Y represents a measure of student achievement on the WKCE—the state test used for federal accountability purposes—standardized by the district mean and standard deviation for the proper year, grade, and subject for student i attending school j and living in neighborhood k at time t. This achievement measure is modeled as a function of a vector of lagged achievement measures; a vector of grade dummies, G; a vector of student characteristics, H; a census tract (i.e., neighborhood) fixed effect, N; a school fixed effect, S; and an error term, ϵ. The vector of lagged reading scores contains a one-year lag of the student's standardized score as well as squared and cubed terms of that lag. The vector of lagged math scores contains an identical set of terms. The vector of student characteristics includes indicators for gender, race, English language learner status, free or reduced-price lunch status, special-

needs status, a change in school of attendance from the previous year, and a change in neighborhood of residence from the prior year. We estimate the model separately for reading and math.[5] The coefficients associated with the neighborhood and school fixed effects—denoted by θ and γ in Equation 1, respectively—represent the estimated neighborhood and school contributions to student test score gains that, along with their standard errors, we recover after estimation of Equation 1.

The potential endogeneity of the relationship between school and neighborhood quality—schools may affect the quality of the surrounding neighborhood and vice versa—could obfuscate whether these neighborhood and school contributions represent total or direct effects. Setting aside the fact that our data span only five years, which is a short period of time for any potential feedback effects to manifest, the sources of identifying variation for the neighborhood and school fixed effects provide additional insight into this issue. In particular, our ability to estimate the neighborhood fixed effects comes from the fact that students residing in a given neighborhood do not all attend school in that neighborhood. If the school in their neighborhood—which they do not attend—affects neighborhood quality, then that should be considered part of the neighborhood effect; the students contributing identifying variation to the neighborhood effect do not attend the neighborhood school and benefit from (or are harmed by) that school only because they reside

in the neighborhood. Our estimates of neighborhood effects should thus be interpreted as total effects. A similar logic holds for characterization of our estimated school effects as total effects.

The recovered neighborhood and school fixed effects were each parameterized using sum-to-zero constraints, implying that neighborhood and school contributions to test score gains are estimated relative to the average neighborhood and school contribution, respectively, which is constrained to be zero.[6] Such a parameterization differs from the one typically used to estimate fixed effects, in which coefficients are estimated relative to some arbitrary holdout unit. The sum-to-zero parameterization has two primary benefits. First, it provides a natural metric for comparing units within some defined group—all coefficients are estimated relative to the average unit. Second, it produces the appropriate standard errors for assessing whether estimated unit differences are statistically significant (for further discussion of sum-to-zero parameterization, see Mihaly et al. 2010).

The model presented in Equation 1 is often referred to as a value-added model, a class of models commonly seen in the education literature. These models are often used to estimate school and teacher contributions to student achievement gains, and in many states, the results are used in formal evaluation and accountability systems. Because results of these models often inform high-stakes decisions, the assumptions underlying them are clear and their properties have been extensively examined. In our case, valid estimates of school and neighborhood contributions to student achievement gains hinge on the assumption that the vector of lagged achievement scores accounts for the accumulation of all factors—including, most importantly, family context—that affect student achievement.[7] If this assumption holds, then the coefficients associated with the vectors of neighborhood and school fixed effects represent estimates of their respective contributions to student achievement gains. Put differently, valid estimates of neighborhood and school contributions require the assumption that student assignment to schools and neighborhoods is effectively random after conditioning on prior achievement scores and other contents of the model.

Several studies, primarily in the context of teacher effects, have assessed the ability of value-added models to return unbiased estimates. Rothstein (2010) presents evidence that a basic value-added model specification may not eliminate bias induced by nonrandom sorting. Subsequent work, however, indicates that such bias is rendered insignificant with a more detailed model specification and use of multiple years of data (Koedel and Betts 2011). More recent studies provide additional evidence—both experimental and quasi-experimental—that bias in value-added estimates stemming from nonrandom selection is negligible (Chetty, Friedman, and Rockoff 2014a; Kane et al. 2013). Especially germane to our case is Deming's (2014) work that exploits random assignment of students to schools to assess the validity of school value-added estimates. Deming fails to reject a null hypothesis of unbiasedness in even fairly simple models so long as they include prior achievement. Together, these studies provide evidence that value-added models can return valid estimates of contextual contributions—in our case, neighborhoods and schools—to student achievement gains. This is particularly true when the models go beyond a basic specification and are estimated over several years of data, as is the case in our analysis. We acknowledge, however, that the identifying assumptions are not directly testable, which prevents us from categorically eliminating the possibility that families' selection into schools or neighborhoods could introduce bias into the estimated effects of these contexts. To the extent that these estimates contain bias, it is likely to be positive, as more advantaged families likely select into higher-quality schools and neighborhoods. However, issues of bias stemming from selection into schools or neighborhoods on the basis of unobservable characteristics are not unique to this study—they also pertain to earlier work on school or neighborhood effects.

Reliability is a concern particularly linked to value-added models. Studies examining the reliability of value-added estimates show they can exhibit non-negligible year-to-year variation—particularly for teacher effects (McCaffrey et al. 2009). To gauge the reliability of our estimated neighborhood and school contributions, we estimated Equation 1 separately for the 2007-8, 2008-9, 2009-10, and 2010-11 school years and examined the year-to-year correlations. The estimated school effects correlate from about .5 to .7 on a year-to-year basis in math and from .2 to .4 in reading; these correlations compare favorably to those observed in studies of teacher value added (McCaffrey et al. 2009). Annual estimates of

neighborhood effects correlate at somewhat lower levels—generally in the range of .1 to .2.

We take two actions designed to maximize the reliability of our estimated neighborhood and school contributions. First, we estimate Equation 1 over all four years of data—analyzing multiple years of data substantially improves the reliability of value-added estimates (McCaffrey et al. 2009). Second, we apply an empirical Bayes shrinkage technique to the estimated school and neighborhood contributions recovered after estimating Equation 1. Shrinkage is a standard feature of many random-effects estimators (Raudenbush and Bryk 2002), and it has been applied to fixed-effects estimates with increasing frequency in recent years (e.g., Hanushek et al. 2007; Jacob and Lefgren 2005). In our case, shrinkage is designed to account for the fact that the estimated neighborhood and school effects consist of both their true contributions and measurement error. Shrinking imprecisely estimated contributions— both neighborhood and school—toward the overall mean of the respective distribution results in a more efficient estimate of each contribution (see Jacob and Lefgren 2005). These shrunken estimates serve as the basis of all analyses that follow.

DISTRIBUTION OF NEIGHBOR-HOOD AND SCHOOL EFFECTS

As the first step in analyzing the relationship between schools, neighborhoods, and student test score growth, we present Table 2, which characterizes the distribution of school and neighborhood effects recovered after estimation of Equation 1. Recalling that the sum-to-zero parameterization results in a mean contribution of zero, the top panel of the table presents the estimated school contribution at the 5th, 25th, 50th, 75th, and 95th percentile of the distribution of estimated school effects. It also presents the difference in the estimated contribution for schools at the 25th and 75th percentiles as well as for schools at the 5th and 95th percentiles. Finally, the table presents the standard deviation of the distribution of estimated school effects. The first two columns of the table present results based on the unweighted estimated school contributions—each school is a single observation in the data; results in the third and fourth columns are based on data where the estimated school contributions are weighted by the number of students enrolled in the school

over the period our data span. The weighted and unweighted estimates are presented separately for reading and math. The bottom panel of the table presents an analogous set of results for the estimated neighborhood contributions.

Results presented in Table 2 begin to address several issues concerning the contributions of schools and neighborhoods to student test score growth. First, the distributions show substantial variability in the estimated contribution of schools to student achievement gains.[8] All else being equal, test score gains for a student attending a school near the top of the distribution are estimated to be .4 to .5 standard deviations larger than gains for a student attending a school near the bottom of the distribution. For the unweighted results, the standard deviation of the distribution of estimated school effects is about .13 in reading and .17 in math. Such findings are broadly consistent with previous research on the magnitude of school effects (e.g., Hanushek et al. 2007; Kane and Staiger 2002). Relative to the unweighted results, the weighted results reveal that students are underrepresented in the least effective schools; the estimated reading and math contributions at the 5th percentile are −.22 and −.26, respectively, in the unweighted estimates but only −.07 and −.13 in the weighted results. From a normative perspective, it is encouraging that students are not disproportionately concentrated in the least effective schools. However, comparison of the weighted and unweighted results also indicates that students are slightly underrepresented in the most effective schools—the 95th percentile of estimated school contributions to reading and math gains were a respective .18 and .22 in the unweighted results but a slightly smaller .15 and .21 in the weighted results. This pattern of results implies that students disproportionately attend schools in the middle of the distribution of estimated effectiveness.

In addition to variability in estimated school contributions, results in Table 2 also indicate variation in estimated neighborhood contributions to student test score gains.[9] In both reading and math, results indicate that a student residing in a neighborhood in the 95th percentile of the distribution would, on average, exhibit one-year test score gains about .1 standard deviations greater than those of a student residing in a neighborhood at the 5th percentile of the distribution. These differences correspond to standard deviations of about .029 to .036—in math and reading,

Table 2. Distribution of Estimated School and Neighborhood Contributions to One-Year Student Achievement Gains, by Weighting Status and Subject

Statistic	Unweighted		Weighted	
	Reading	Math	Reading	Math
School				
Percentile (*SD* units)				
5th	−.216	−.261	−.069	−.126
25th	−.027	−.065	−.002	−.013
50th	.051	.020	.051	.030
75th	.107	.084	.103	.084
95th	.181	.220	.147	.206
Percentile differences (*SD* units)				
5th-95th	.397	.481	.216	.332
25th-75th	.134	.149	.105	.097
Standard deviation of distribution	.128	.171	.072	.106
Neighborhood				
Percentile (*SD* units)				
5th	−.065	−.046	−.074	−.047
25th	−.035	−.020	−.040	−.023
50th	−.013	−.001	−.024	−.008
75th	.007	.016	−.009	.013
95th	.051	.050	.021	.038
Percentile differences (*SD* units)				
5th-95th	.116	.096	.095	.085
25th-75th	.042	.036	.031	.036
Standard deviation of distribution	.036	.029	.028	.028

Note: Table characterizes the distribution of estimated school (top panel) and neighborhood (bottom panel) contributions to one-year student test score gains. Estimated contributions recovered from ordinary least squares model predicting student achievement that contains lagged measures of students' achievement and school and neighborhood fixed effects. In the model, the estimated school fixed effects represent the estimated school contributions, and the estimated neighborhood fixed effects represent the estimated neighborhood contribution. Unweighted results do not weight the estimated school (top panel) or neighborhood (bottom panel) contribution by the number of students attending the school or residing in the neighborhood, respectively—each school or neighborhood is treated as a single unit (i.e., with equal weight). Weighted results weight estimated school contributions (top panel) by the number of students attending the school over the five years our data span. Similarly, the weighted results weight estimated neighborhood contributions by the number of students residing in the neighborhood over the five years our data span.

respectively—for the distribution of estimated neighborhood contributions. In contrast to school results, comparison of the unweighted and weighted neighborhood contributions indicates that students disproportionately reside in neighborhoods with below-average contributions, if only slightly. At the 50th percentile of the distribution, the unweighted results reveal estimated contributions of −.013 and −.001 in reading and math, respectively. These numbers are slightly higher than the median contributions, −.024 and −.008 in reading and math, respectively, in the weighted results.

The fact that neighborhoods exhibit variation in their estimated contributions to student achievement gains is directly relevant to the questions of whether and how much neighborhoods matter with respect to student test score outcomes. As reviewed earlier, existing research draws competing conclusions on the extent to which neighborhoods are important determinants of student academic outcomes. By demonstrating that—conditional on the school they attend, their prior achievement levels, and their demographic characteristics—students residing in different neighborhoods exhibit different average levels of test score

growth, the results presented in Table 2 align more closely with research concluding that neighborhoods are an important determinant of student academic outcomes, particularly, test scores (e.g., Aaronson 1998; Sharkey and Elwert 2011). The magnitude of the difference, however—about one tenth of a standard deviation in test score gains for students residing in a neighborhood at the 5th versus 95th percentile of the distribution—would generally be considered substantively modest in the context of educational interventions. That said, these estimates are one-year differences in growth. If these annual differences accrue over time—and there are reasons to suspect they can and do (see Wodtke et al. 2011)—then neighborhood contributions alone could account for nearly a standard deviation difference in the test scores of students residing in the best versus worst neighborhoods by the time students reach high school.

An appealing feature of our analytic approach is the ability to directly compare—on an identical scale—the distributions of estimated school versus neighborhood contributions to student test score gains. Such a comparison provides insight into the relative importance of these contexts in shaping student achievement outcomes. Results in Table 2 make clear that—in both math and reading—the distribution of estimated school contributions is much more variable than the distribution of estimated neighborhood contributions.[10] In reading, the standard deviation of the distribution of estimated school contributions is nearly four times larger than the standard deviation of the estimated neighborhood contributions. In math, the school standard deviation is six times larger. For one-year test score gains, the school a student attends is more important than the neighborhood in which the student resides. Indeed, any gains a student receives from living in a high-quality neighborhood would be more than offset by attending one of the lowest-quality schools in the city. Of course, the reverse of that statement holds as well—neighborhood disadvantage can be more than offset by attendance at a high-quality school. These findings have important implications for educational inequality, which we discuss in the Conclusion.

We acknowledge that neighborhoods' smaller effects on achievement—relative to schools—may be at least partially attributable to the attenuating effects of measurement error. As suggested by the lower year-to-year correlations described earlier, neighborhoods are measured with greater imprecision than schools. Even though we took steps to maximize the reliability of our estimates, the error inherent in measuring neighborhoods may attenuate their estimated effects on student achievement.

ROBUSTNESS OF ESTIMATED SCHOOL AND NEIGHBORHOOD CONTRIBUTIONS

As noted earlier, multicontextual analyses can decrease the likelihood of misattributing the effects of one context to another (Cook 2003). In this study, the breadth and structure of our data put us in a position to empirically assess the extent to which a singular analysis of either school or neighborhood context would have resulted in biased inferences about the achievement-related effects of that context. To perform this assessment, we first estimate school and neighborhood contributions using models that do not contain a measure of the other context; we then compare these estimates to those obtained from Equation 1, which contains an explicit measure of each context. For example, to assess the robustness of estimated school contributions to the exclusion of a measure of a student's neighborhood context, we estimate the following model:

$$Y_{ijkt} = Y_{ijkt-1}\beta + G_{it}\rho + H_{ijkt}\tau + S_j\gamma + \varepsilon_{ijkt}, \quad (2)$$

where the only difference between this model and that presented in Equation 1 is the lack of a neighborhood fixed effect. We then recover the estimates of γ obtained from this model, shrink them using the empirical Bayes procedure described previously, and compare them to the estimated school contributions obtained from Equation 1. Similarly, to assess the robustness of estimated neighborhood contributions to the exclusion of a measure of school context, we estimate the following:

$$Y_{ijkt} = Y_{ijkt-1}\beta + G_{it}\rho + H_{ijkt}\tau + N_k\theta + \varepsilon_{ijkt}, \quad (3)$$

and compare the shrunken estimates of θ to the estimated neighborhood contributions obtained from Equation 1.

The distributions of estimated school contributions recovered from Equation 1, containing neighborhood fixed effects, and Equation 2, which did not contain neighborhood fixed effects, exhibit
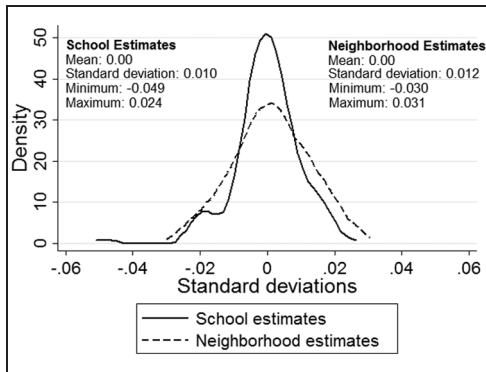
Figure 3. Distribution of differences between value-added estimates with and without fixed effects for other context: Reading.
Note: Figure presents kernel density plot of difference in school value-added estimates recovered from models with and without neighborhood fixed effects (solid line). Figure also presents kernel density plot of difference in neighborhood value-added estimates recovered from models with and without school fixed effects (dashed line).
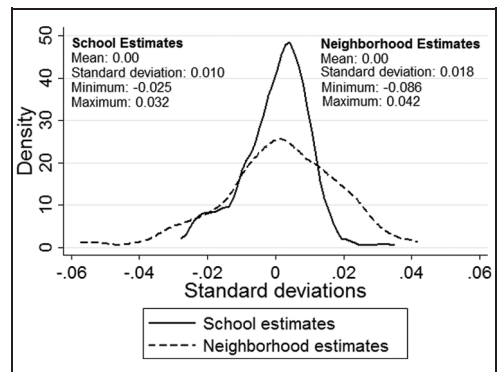

Figure 4. Distribution of differences between value-added estimates with and without fixed effects for other context: Math.
Note: Figure presents kernel density plot of difference in school value-added estimates recovered from models with and without neighborhood fixed effects (solid line). Figure also presents kernel density plot of difference in neighborhood value-added estimates recovered from models with and without school fixed effects (dashed line).

a very strong correlation. Indeed, for both math and reading, the correlation between the two estimates exceeds .99. This suggests that any bias in estimated school contributions resulting from the exclusion of a measure of neighborhood context is likely to be minimal, at least in a context where gains in achievement test scores are the outcome of interest and school contributions are estimated with five years of data. The correlations for the two sets of estimated neighborhood effects—those recovered from a model containing school fixed effects and those recovered from a model that does not explicitly account for schooling context—are somewhat lower. Specifically, the correlations for the two sets of estimated neighborhood contributions are .87 for math and .96 for reading. Although these correlations would generally be considered high in the social sciences, they are meaningfully lower than the correlations between the two sets of school effects. As such, they are suggestive of the danger that Cook (2003) notes: Analyses of neighborhood contributions to academic achievement that do not account for a student's schooling context may result in biased estimates of neighborhood contributions.[11]

To facilitate a more precise understanding of this issue, Figures 3 and 4 present kernel density plots of the difference between the estimated

school contributions from models with and without neighborhood fixed effects—Figure 3 presents this plot for reading while Figure 4 presents the plot for math. To ease direct comparison, we overlay that distribution with a density plot of the difference in estimated neighborhood contributions from models with and without school fixed effects. Looking first at school contributions, the plot indicates that the estimated school contributions exhibit minimal change when neighborhood fixed effects are excluded from the model. The standard deviation of the distribution is .01, and empirically, the difference between the two estimates is less than one hundredth of a standard deviation for approximately 80 percent of the estimates. These results suggest that exclusion of a measure of student's neighborhood from the model used to estimate school contributions does not introduce appreciable bias into those estimates.

Figures 3 and 4 paint a somewhat less rosy picture for the robustness of estimated neighborhood contributions to the exclusion of a measure of a student's schooling context. In both subjects, but particularly in math, the distribution of the difference in the two sets of estimated neighborhood contributions is more variable than the distribution of differences between the two sets of estimated

school contributions. This fact, coupled with the earlier finding that neighborhood contributions are substantially less variable overall (i.e., smaller in magnitude) than school contributions to student test score gains, suggests that meaningful bias can be introduced into neighborhood-effects estimates when an analysis does not explicitly account for a student's schooling context. In such cases, a consequential portion of any estimated neighborhood effect—positive or negative—could be attributable to the school a student attends. These findings show the necessity of explicitly accounting for a student's schooling context in order to recover valid estimates of neighborhood contributions to educational outcomes.

DISCUSSION AND CONCLUSION

Despite the rich sociological literatures examining students' school and neighborhood contexts, few studies have been able to disentangle neighborhood and school sources of variation in student outcomes. These difficulties contribute to different theoretical emphasis—neighborhoods versus schools—when interpreting empirical evidence regarding the roles these contexts play in shaping student outcomes. In Milwaukee, there is sufficient cross-classification of students in neighborhoods and schools to permit simultaneous estimation of neighborhood and school contributions to test score gains, a fact explained by a particularly broad open-enrollment environment in the public school system. We exploited this cross-classification to estimate neighborhood and school contributions to one-year student achievement gains using a single model estimated over a single sample of students in metrics that are directly comparable, permitting an examination of one context after accounting for the influence of the other.

We find that school effects on one-year test score gains are meaningfully more variable than the estimated contributions of neighborhoods. Perhaps more to the point, our analyses make clear that the school students attend makes a larger difference with respect to achievement outcomes than do the neighborhoods in which they live, a finding consistent with earlier work that examines the two contexts simultaneously (Cook et al. 2002). To be clear, though, we did not find that neighborhoods do not matter for student achievement outcomes. To the contrary, our analysis reveals significant variation across neighborhoods

with respect to their estimated contributions to student test score gains. If one implication of our results is that students attend schools with substantially different effects on learning, it is also apparent that student outcomes are affected by differences in where they live.

These findings have important implications for educational inequality. From a theoretical standpoint, the results indicate that accounts of educational inequality should explicitly include both school and neighborhood contexts as potential drivers of educational disparities, at least with respect to student achievement but likely for other outcomes as well. Furthermore, the direct comparability of the estimated school and neighborhood contributions can inform theories about the relative importance of each context in producing, or reproducing, educational inequalities. In the short term, it is clear that schools are more influential—and substantially so—than neighborhoods with respect to student achievement gains. The long-term picture may be very different, however. Because children often reside in a single, or at least comparable, neighborhood context for much longer periods of time than they attend any specific school, a neighborhood's cumulative contribution to students' achievement levels may be at least as large as, if not larger than, the cumulative contribution of the schools they attend. In this respect, our short-term results are consistent with several recent studies that examine neighborhood effects in a longitudinal setting and find substantively large effects (see Sharkey and Elwert 2011; Wodtke et al. 2011). Finally, our results indicate that students' school and neighborhood contexts may interact to create situations in which students are doubly advantaged or, less optimistically, doubly disadvantaged with respect to educational outcomes. These insights can inform further development and refinement of theories of educational inequality; we plan to explore such issues in future work.

At an applied level, our results suggest that educational inequities will not be eliminated by policies or practices exclusively focused on improving either school or neighborhood conditions. Even if we make the seemingly implausible assumption that current efforts to increase educational quality will result in every student gaining access to an education of high and uniform quality in the coming years, our results suggest that inequalities in educational outcomes will persist due to differences in the neighborhoods where students reside. Furthermore, the potential for

cumulative disadvantage suggests that the magnitude of these disparities—while perhaps reduced relative to present levels—would likely remain substantial. Our results suggest that efforts to mitigate inequality need to span multiple contexts, and efforts focusing on either schools or neighborhoods in isolation may generate some improvements but will likely fall short of fully addressing issues of equality.

When gauging the sensitivity of the estimated effects of one context to the exclusion of the other, we found that estimated school effects are quite robust to the exclusion of explicit measures of students' neighborhood contexts. In contrast, estimated neighborhood contributions are somewhat more sensitive to the exclusion of a measure of students' schooling context. These results have important implications for research examining students' schools, neighborhoods, and academic outcomes. In particular, they illustrate how an analysis of neighborhood effects that does not explicitly account for students' schooling context runs the risk of producing biased estimates. Even though our results suggest these concerns are less acute in the case of analyzing school effects, we interpret these findings as evidence that the increasing separation of students' neighborhood and schooling contexts renders it important to simultaneously analyze both contexts if we hope to more fully understand how they separately and jointly contribute to student outcomes.

Because our results consider the relationship between schools, neighborhoods, and student outcomes in a different way than commonly seen, several aspects of the study warrant particular discussion. First, this study addresses only how school and neighborhood context shape a single academic outcome—student achievement—over a single year for only elementary and middle school students. As a society, we clearly value a broad array of academic outcomes, including educational attainment, self-regulation, interpersonal ability, and other noncognitive skills. This study provides minimal information about the extent to which the two contexts we examine might shape these other outcomes or even how they might shape achievement outcomes for older students. Indeed, prior work suggests that schools and neighborhoods might affect different dimensions of students' lives, with schools more closely related to academic outcomes and neighborhoods primarily affecting health and social outcomes

(Cook et al. 2002; Fryer and Katz 2013; Garner and Raudenbush 1991). If such suggestions prove accurate, then the outcome we analyze in this study would highlight the importance of schools relative to neighborhoods. A very different picture of the relative influence of schools and neighborhoods might emerge in an analysis of a different outcome.

Even recognizing that student achievement represents an outcome worthy of analysis—recent work demonstrates a connection with valued later-life outcomes (e.g., Chetty, Friedman, and Rockoff 2014b)—from a societal standpoint, we are more interested in how schools and neighborhoods shape long-term achievement trajectories than single-year gains. However, in our case, extending the time period over which achievement gains are analyzed would require trading off confidence in the validity of our estimated neighborhood and school contributions. Gaining a longer time period would come at the expense of a smaller sample size and greater uncertainty about accounting for student selection into schools and neighborhoods. As this analysis takes a somewhat different approach to considering school and neighborhood effects than does much of the literature, we chose to prioritize the validity of our estimates. However, we view this analysis as laying the foundation for future work that gauges the longer-term influences these contexts have on student achievement and other outcomes.

RESEARCH ETHICS

Our research protocol was reviewed and approved by the University of Oklahoma Institutional Review Board and the Michigan State University Institutional Review Board. Our research involves analysis of secondary data that do not include personal identifiers or allow for deductive disclosure of respondents' identities.

NOTES

1. For more details on this process, see Milwaukee Public Schools' enrollment guide at http://www5.mil waukee.k12.wi.us/school/selections/how-to-enroll-online/ and http://mpsportal.milwaukee.k12.wi.us/portal/server.pt/doc/74435/Directions+Booklet+-+ 2011.
2. These data contain students attending charter schools operated by the Milwaukee Public School district.
3. These data represent a portion of a larger set of data collected on public and private school students in

Milwaukee for an evaluation of the city's school voucher program that occurred during these years. The primary analytic strategy used in that larger study was a matching procedure based on student neighborhoods as measured by census tract. The present article explicitly considers a key assumption behind that procedure: that academic heterogeneity between Milwaukee neighborhoods and schools represents meaningful substantive variation in outcomes.

4. We restrict our analysis to elementary and middle schools because students are tested only once in high school (10th grade), a reality that renders us unable—because of the inclusion of lagged achievement in Equation 1—to estimate a reliable school contribution to student test score gains at the high school level.

5. We estimated the reading model using 95,988 observations from 44,445 unique students in grades 3 through 8. We estimated the math model using 95,976 observations from 44,445 unique students in grades 3 through 8.

6. To recover neighborhood and school contributions that were each parameterized using sum-to-zero constraints, we estimated Equation 1 twice using Stata's user-written "felsdvregdm" command (Mihaly et al. 2010). In the first estimation, neighborhood fixed effects were estimated and subsequently recovered using sum-to-zero parameterization; school fixed effects were eliminated through the subtraction of group means. The reverse occurred in the second estimation—neighborhood fixed effects were eliminated using the within transformation; school fixed effects were estimated under a sum-to-zero parameterization and subsequently recovered.

7. See Reardon and Raudenbush (2009) for an in-depth discussion of the assumptions that underlie value-added models.

8. We performed an F test to assess the joint significance of the estimated school effects. In both reading and math, results of the test rejected the null hypothesis that the estimated effects were jointly equal to zero at $p < .001$.

9. We performed an F test to assess the joint significance of the estimated neighborhood effects. In reading, the test rejected the null hypothesis that the estimated effects were jointly equal to zero at $p < .001$. In math, the null hypothesis was rejected at $p < .05$.

10. We performed a Kolmogorov-Smirnov test to assess whether the distributions of estimated school and neighborhood effects are significantly different. In both reading and math, results of the test reject the null hypothesis of no difference at $p < .001$.

11. Tests for equality in the distribution of school effects recovered from models with and without neighborhood fixed effects were unable to reject the null hypothesis of no difference (p values in excess of .5). Tests for equality in the distribution of neighborhood effects recovered from models with and without neighborhood fixed effects reject the null hypothesis of no difference at $p < .15$ but not at lower p values.

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Author Biographies

Deven Carlson is an assistant professor of political science at the University of Oklahoma. His research interests include education policy, housing policy, and the intersection of the two.

Joshua M. Cowen is an associate professor of education policy at Michigan State University. His research interests include school choice, teacher quality and school accountability, and recent work has been published in American Educational Research Journal, Educational Evaluation and Policy Analysis, the Journal of Policy Analysis and Management, and Teachers College Record.