Does Attendance in Private Schools Predict Student Outcomes at Age 15? Evidence From a Longitudinal Study

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By tracking longitudinally a sample of American children (n = 1,097), this study examined the extent to which enrollment in private schools between kindergarten and ninth grade was related to students' academic, social, psychological, and attainment outcomes at age 15. Results from this investigation revealed that in unadjusted models, children with a history of enrollment in private schools performed better on nearly all outcomes assessed in adolescence. However, by simply controlling for the sociodemographic characteristics that selected children and families into these schools, all of the advantages of private school education were eliminated. There was also no evidence to suggest that low-income children or children enrolled in urban schools benefited more from private school enrollment.

Keywords: adolescent outcomes; educational policy; longitudinal studies; NICHD SECCYD; private schools; public schools; school/teacher effectiveness

Among the focal points of efforts to reform the public education system in the United States and provide improved schooling experiences for vulnerable children, enrollment in private schools, largely through voucher or tax-credit financing, has been among the most frequently referenced as well as contentious (e.g., Dynarski, 2016; Lubienski & Lubienski, 2013; Urquiola, 2016). Policies and financing schemes that encourage enrollment in private schooling have been justified on the basis of increasing choice for low-income parents that cannot relocate to more affluent and better school districts, as a means of increasing pressure on public schools to compete in a market for parents' selection, and as a remedy for the achievement gap, presumably because of the superior capacity of private schools to educate (poor) students. Many of these same arguments are made in support of expanded charter schools, which bear similarities with private school programs (greater flexibility in hiring or curricula) but tend to be subject to greater oversight, with most charters considered public schools in some form (Carpenter, Keith, & Catt, 2016; Levchenko & Haidoura, 2016; Mills & Wolf, 2017).

At the core of these justifications for reforms that utilize schools outside of the typical K–12 governance and operational structures is the assumption that private schools are more effective in educating students, producing higher levels of achievement, fostering positive social adjustment and citizenship, and decreasing risky behavior (Dynarski, 2016; Flanders & DeAngelis, 2017; Levchenko & Haidoura, 2016; Lubienski & Lubienski, 2013). If true, these features could be leveraged through policies that enable access, such as voucher systems coupled with parental choice, but ultimately, any such policy rests on the presumption that private schools perform better with respect to fostering students' learning and development. In the present study, we take advantage of a unique longitudinal study of a large and diverse sample of children to examine the extent to which enrollment in private schools is predictive of achievement and social and personal outcomes at age 15 for students enrolled in the National Institute of Child Health and Human Development (NICHD) Study of Early Child Care and Youth Development (SECCYD; NICHD Early Child Care Research Network [ECCRN], 2001). The SECCYD data set has been used to document longitudinal patterns in exposure to learning opportunities in classrooms and their impacts on student

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Educational Researcher, Vol. XX No. X, pp. 1–16
DOI: 10.3102/0013189X18785632

achievement (Pianta, Belsky, Vandergrift, Houts, & Morrison, 2008) as well as associations between experiences in child care prior to school entry and high school performance outcomes (Vandell, Burchinal, & Pierce, 2016). Because of its longitudinal nature and assessment of a broad range of outcomes, the use of the SECCYD data set in this study may add a unique and relevant perspective to consideration of recent federal policy initiatives to increase financial support through vouchers and tax credits for families to enroll their children in private schools.

Literature Review

By and large, the evidence on the impact of school voucher programs casts doubt on any clear conclusion that private schools are superior in producing student performance (e.g., Abdulkadiroglu, Pathak, & Walters, 2015; Berends & Waddington, in press; Carolson, Chingos, & Campbell, 2017; DeAngelis & Burke 2017; Figlio & Karbowinc, 2016; Lubienski & Lubiekski, 2013; Mills & Wolf, 2017). In separate and well-controlled studies that typically rely on a lottery-type design to control for differences in family background and other factors often implicated in both school outcomes and enrollment into private or public education, independent investigations of programs in Ohio, Louisiana, Indiana, and New York City indicate that enrollment in private schools had mixed effects on achievement for low-income students compared with program-eligible peers not attending private schools. These studies, designed to support causal inferences regarding the impacts of private schooling, are among the strongest scientific examinations of such effects.

Dynarski (2016) summarized recent evaluations of voucher program impacts, noting the appearance of large negative impacts on student achievement, particulary in the early years of the program or for the first year or two in which a student is enrolled. For example, in the first years of the Louisiana voucher program, for a public school student who was average in math and began attending a private school using a voucher, their achievement scores declined by 16 percentile points after one year, a finding that was consistent across grades and subject areas (Abdulkadiroglu et al., 2015). In Indiana, the decline for the average student, again in the first one to two years of private enrollment, was 6 percentile points in math (Berends & Waddinton, in press). Similarly, in an evaluation of the Ohio voucher program, Figlio and Karbowinc (2016) reported that students who used vouchers to attend private schools performed worse academically in math and language arts compared with closely matched peers attending public schools.

However, subsequent examinations of Louisiana and Indiana students who remained enrolled in private schools longer than the initial one- to two-year evaluations showed those students' performance to be superior to their peers in public schools (Mills & Wolf, 2017), indicating that length of student enrollment in private education may be a key feature influencing any benefits accrued. And in New York City and Washington, D.C., the results of lottery assignment evaluations were more positive, with an uptick in graduation rates and college attendance, higher reading and math scores for Black students in New York City, and higher reading scores in the third follow-up year of the D.C. program (for a summary, see Dynarksi, 2016). So in causal inference studies, although there are indications that the early years of a program (e.g., in Louisiana) and short-term enrollment in private schooling (e.g., for Ohio, Indiana, and Louisiana) pose a degree of risk for lowering students' achievement, there is also indication that when poor students are enrolled for longer periods in more mature programs, performance is at or above publicly enrolled control groups.

In a follow-up evaluation of schools in the Louisiana program, Abdulkadiroglu et al. (2015) attributed the reduced academic achievement in math, reading, science, and social studies to the pool of private schools available in the first years of the voucher program. Participating private schools charged lower tuition than other private schools and showed declining enrollment rates, suggesting that participation was a strategy to boost enrollment in financially weak private schools. Thus, in the first years, low-income parents were only able to select from low-quality private schools, replicating the income stratification in school quality that voucher programs are designed and touted to disrupt. As the program in Louisiana matured, evidence showed that the available pool of private schools accepting vouchers was composed of a larger number of higher performing schools (Abdulkadiroglu et al., 2015). Whether the uptick in performance associated with this program is a result of improvements in the low-performing privates, stable and longer term enrollment of poor children in their new private schools, or some other factors remains to be seen.

Berends and Waddington (in press) also examined the Indiana program to evaluate the extent to which the type of private school may be part of the reason why negative impacts were initially reported for that state's voucher program. This research team analyzed data longitudinally across Grades 3 through 8 in Indianapolis to identify impacts associated with public school students enrolling in a charter, magnet, Catholic, or other private schools. Although not a lottery or experimental design, the analytic approach examined school type as it related to individual student learning performance over time. Students showed modest gains when switching to a charter and modest losses (between .09 and .18 standard deviation units) when transferring to a magnet or Catholic school. The authors point out that the evidence suggests that non–public schools can be highly variable within any specific catchment area and, as in the Louisiana studies, the quality of the pool of available schools is critical to the success of voucher programs as a means to address achievement or opportunity disparities.

Using a nonexperimental design and analytic approach to evaluate private schooling effects, Wong, Cook, and Steiner (2009) used data from the National Assessment of Education Progress (NAEP) to compare public schools to private schools from the 1990s into the 2000s. At the start of this epoch, private schools on average started with higher scores, but a decade later, that gap had closed significantly, largely due to public schools' improvements, especially in math. The authors concluded that the accountability provisions of No Child Left Behind focused public school efforts in ways that improved test scores and that it also might be possible that factors such as competition (with charters, homeschooling, or privates) also exerted an influence on public schools such that they focused more intently on instruction and student outcomes (a finding suggested by Figlio & Karbownic, 2016, in their evaluation of the Ohio voucher

program in which achievement in public schools rose as a function of the voucher program). With data from NAEP and the Early Childhood Longitudinal Study (ECLS), Lubienski and Lubienski (2013) also find that differences in student performance outcomes appear to favor private school attendance, but once student and family demographics are considered, no such benefits are apparent, and in fact, the analyses suggest public schools appear superior in some ways. Again, these national-level analyses using NAEP or ECLS do not support causal inferences; nonetheless, the results from these data do not contradict the mixed evidence from experimental studies.

Private schools also represent a considerable proportion of the education sector in many developing countries and internationally more generally, and research from non-U.S. schools may also be relevant to understand the impacts and implementations of private schooling. Most of these studies are not experimental in nature, and selection effects are addressed through statistical methods. For example, Sakellariou (2016) used results from the 2012 administration of the PISA tests to examine private school effects on mathematics performance for students in 40 different countries. Using separate methods to adjust for selection bias and peer effects, a private school advantage was detected in a small minority of countries, with the majority of countries' results being null or negative. In contrast, Shakeel (2017) took a longer term perspective in estimating the effects on PISA of private schooling for 62 countries across the period of 2000 to 2012, controlling for selection bias by examining patterns of scores over time (using year and country fixed effects) and using an estimate of demand for schooling within a country and year as an instrument for private share of schooling enrollment. Using this analytic approach, evidence suggests that increases in private schooling led to improvements in PISA scores, on average, across all countries included. Moreover, estimates suggest that a 10 percentage point increase in the share of private school enrollment was associated with 28% of a standard deviation increase in math, 24% of a standard deviation increase in reading, and 18% of a standard deviation increase in science. Thus, the evidence from international studies is also mixed, showing both positive and negative effects, with suggestions that impacts are moderated both by the heterogeneity of the private school pool as well as the length of time that programs are in place.

There are a number of important considerations to keep in mind when interpreting results from the available well-controlled studies of voucher-like programs or evaluations of private school attendance. First, there is overwhelming evidence that household factors such as income and parent education drive selection and enrollment in private school. Higher income families can afford private school tuition and will choose schools that match an assortment of goals and values they hold for their children, including achievement, social adjustment, access to peer networks, and character (Levchenko & Haidoura, 2016). Or in the case of religious schools, income may play a somewhat less prominent role in selection, but alignment with values and curricular focus can be the determining factor for parents. Of note, these aspects of family background (income, education, expectations) are also implicated in stimulation and opportunity that drives children's learning and achievement. In this way, studies must try to separate the effects of private schooling per se from the family background factors that determine both enrollment in private school and children's achievement. Some studies achieve a level of control using randomized designs mostly embedded in lottery-type application processes, while others use quasi-experimental statistical modeling to estimate schooling effects net of assessed selection factors. As noted previously, however, the results from both experimental and quasi-experimental studies are mixed.

A second consideration in evaluating private school effects is the exceptionally wide variation in private schooling across the United States (Carpenter et al., 2016; Levchenko & Haidoura, 2016; Redford, Battle, & Bielick, 2016; Urquiola, 2016). Private schools encompass those that include various religious denominations and those with a specialized curriculum (e.g., Montessori, Waldorf), and the auspices under which they operate range from highly institutionalized and systemic (e.g., the Archdiocese of New York) to highly localized and small units. Evidence also suggests that urban and rural locations may have differential consequences for accessibility and enrollment in private schools (Carpenter et al., 2016). Moreover, governance, staff qualifications, and school size are also among the features of private schooling that are highly varied. Thus, when studying the *category* of private schooling as a policy choice, it is important to keep in mind the exceptional degree of heterogeneity in the sector, which on the one hand is often viewed as a desirable characteristic (by reformers, advocates, and parent) but as seen in a number of evaluations, has variable results on students.

Third, most well-controlled evaluations of voucher programs or private school enrollment tend to utilize outcome assessments in the domains of math and language arts (reading) available from the state standardized testing programs (e.g., Dynarski, 2016) or national assessments such as NAEP. Notably, although these assessments provide valuable information, they do not enable the detection of impacts on other desirable student outcomes, such as motivation, social adjustment, and behavior, which are often included in the rationale for private school enrollment and superiority (Carpenter et al., 2016; Lubienski & Lubienski, 2013; Urquiola, 2016). Relatedly, most of these programs have been in place or have outcome data available for only a short number of years, and so the longer term nature of any private schooling effects have not yet been identified or included in the research literature; a longitudinal analytic and temporal frame that extends from elementary through high school may illuminate effects not readily apparent in shorter timeframe analyses.

The Current Study

In the present study, we take advantage of a unique longitudinal study of a large and diverse sample of children to examine the extent to which enrollment in private schools is predictive of achievement and social and personal outcomes at age 15 for students enrolled in the NICHD SECCYD. The SECCYD offers a unique opportunity to examine private schooling effects with its comprehensive assessment of student outcomes at multiple intervals, a timeframe that is much longer than typical evaluations of private schooling, and the detailed and wide-ranging assessments of family background and contextual processes that

can help in estimating (and reducing) selection bias, as has been done in the study's evaluation of other policy-relevant factors, such as enrollment in child care or teacher quality (e.g., Belsky et al., 2007; Pianta et al., 2008; Vandell, Belsky, Burchinal, Steinberg, & Vandergrift, 2010; Vandell et al., 2016). And the SECCYD sample reflects a broad range of economic conditions, cultural beliefs, and childrearing practices, such as is the case in the United States. In sum, this study aims to evaluate the benefits of private school enrollment on a comprehensive set of student outcomes assessed in adolescence. We also consider the extent to which the the benefits of private school enrollment vary for children across the income distribution and children in urban and rural communities.

Method

Participants

The NICHD SECCYD is a multisite research project originally designed to determine the benefits of early child care on children's development. Participants were recruited in 1991 from designated community hospitals at 10 university-based data collection sites: (1) Little Rock, Arkansas; (2) Irvine, California; (3) Lawrence, Kansas; (4) Boston, Massachusetts; (5) Philadelphia and (6) Pittsburgh, Pennsylvania; (7) Charlottesville, Virginia; (8) Seattle, Washington; (9) Hickory and Morganton, North Carolina; and (10) Madison, Wisconsin. Recruitment and selection procedures are described in detail (NICHD ECCRN, 2001), and study procedures are described on the study website (http://secc.rti.org). Children were followed from birth to 15 years with a common study protocol, including interview and home, school, and neighborhood observations that occurred on a yearly basis. For all study data collection protocols, human subjects institutional review boards at each university and the data coordinating center approved voluntary, written informed consents from participating families.

Healthy newborns, discharged within one week of birth, of English-speaking mothers were recruited. When the target child was 2 weeks old, attempts were made to contact 3,015 families who met eligibility criteria to enlist their participation. Attempts to contact were unsuccessful for 512 families, and 151 families were deemed ineligible because the child remained in the hospital more than seven days or the family planned to move. An additional 641 families refused to participate, and 1-month interviews could not be scheduled for 185 families for other reasons. Out of 1,526 families scheduled, 1,364 families actually completed the 1-month home visit and became study participants. The resulting sample included (nonexclusively) 24% children of color, 15% single mothers, and 10% mothers without a high school diploma. At the 1-month home visit, mothers had an average of 14.23 years of education, and the average family income was 2.86 times the poverty threshold. There were no significant differences between these 1,364 families and the U.S. population (U.S. Census Bureau, 1990) based on ethnicity (80.3% White in U.S. population vs. 80.4% in cohort) and household income (household income information available on 1,271 families; $36,520 in U.S. population and $37,781 in cohort). However, the NICHD SECCYD cohort (missing marital status for 2 mothers) had a slightly higher percentage of parents who were married than the U.S. population (76.7% vs. 74.2%, $p = .04$).

Of the original 1,364 study participants, 1,226 participated in Phase II (through first grade; 1995–1999), 1,061 participated in Phase III (through sixth grade; 2000–2004), and 1,009 participated in Phase IV of the study (through ninth grade; 2005–2007). And of the 1,364 children who were originally enrolled into the study, we: (a) excluded 207 children who had no record of school type between kindergarten and ninth grade and (b) 60 children who were ever homeschooled or ever enrolled in a public charter school. For sample descriptives for the 1,097 study children who made up our analytic sample, see Table 1.

Measures and Procedures

School sector. Through administrative archives from schools, we had access to students' school enrollment records for each year between kindergarten and ninth grade (all 10 sites had public school kindergarten). These school records were used to measure students'exposure to public and private school education in two different ways. Before discussing these two measures, it is important to note that for the purposes of the current investigation, children were allowed to have missing data on the school type variable over time in order to maximize the focal predictor (i.e., children were included if they had missing data at later waves on school type). On average, children had information on school sector available for 8.81 years ($SD = 2.25$) of the 10 years of study participation, and approximately 90% of children had at least six years of school type data. Roughly 7% to 17% of children had missing data for these measures at any given wave (7%, 13%, 17%, 7%, 9%, 12%, 12%, 11%, 13%, and 16% had missing data on school type in kindergarten and first, second, third, fourth, fifth, sixth, seventh, eighth, and ninth grades, respectively). However, as discussed earlier, because of our sample inclusion requirements, all 1,097 children had at least one wave of data on school type.

With the aforementioned information in mind, we first measured *any* exposure to private school education (0 = *no*, 1 = *yes*), which captured in binary form whether participants ever attended a private school during their first 10 years of formal schooling. Second, we measured the number of years participants attended a private school. To create this indicator of years, we multiplied the proportion of waves children experienced private school by 10 (i.e., the years of data collection between kindergarten and ninth grade). As a precaution, we also estimated models that included an analytic weight that contained the number of years of data for which children had school type data. In doing so, children who had more data points received greater weight than children who had fewer data points (results discussed in more detail in the following).

Ninth-grade outcomes. Adolescents' school performance and functioning in ninth grade was based on a variety of benchmarks collected through direct assessments, administrative records, self-report, and/or parent report (see Table 2 for descriptives). School records were pulled at the end of the year, and direct assessments and self- and parent reports generally occurred during the spring of the school year or right after the completion of ninth grade.

Table 1
Sample Descriptives for All Students and Separated for Students Who Ever Attended a Private School Versus Those Who Only Experienced Public School Education Between Kindergarten and Ninth Grade

		Student Ever Attend a		
	Overall	Public School	Private School	Significant Difference
Child characteristics and experiences during early childhood				
Preschool functioning				
Cognition	459.57 (14.15)	457.89 (14.23)	463.40 (13.22)	***
Literacy	369.31 (21.62)	365.51 (20.80)	377.98 (20.95)	***
Math	424.82 (19.45)	422.46 (20.65)	430.17 (15.14)	***
Working memory	457.05 (18.40)	455.20 (18.43)	461.21 (17.68)	***
Externalizing behavior	51.71 (9.42)	52.28 (9.18)	50.40 (9.83)	**
Internalizing behavior	47.23 (8.90)	47.56 (8.77)	46.46 (9.15)	
Social skills	98.21 (13.60)	96.57 (13.67)	101.91 (12.69)	***
Race				
White	0.81	0.80	0.86	*
Black	0.12	0.14	0.07	**
Other	0.07	0.07	0.07	
Male	0.51	0.51	0.50	
Birthweight (pounds)	7.73 (1.12)	7.71 (1.14)	7.78 (1.08)	
Birth order	1.81 (0.92)	1.85 (0.93)	1.72 (0.89)	*
Temperament	3.17 (0.40)	3.20 (0.39)	3.12 (0.40)	**
Proportion of time in center care	0.21 (0.26)	0.19 (0.25)	0.26 (0.27)	***
Proportion of time in maternal care	0.29 (0.27)	0.31 (0.27)	0.24 (0.24)	***
Child care quality	2.93 (0.41)	2.91 (0.42)	2.97 (0.38)	*
Family characteristics during early childhood				
Mother's age	28.45 (5.60)	27.64 (5.58)	30.30 (5.18)	***
Psychological adjustment	59.27 (13.87)	58.21 (13.90)	61.66 (13.53)	***
Maternal education	14.41 (2.48)	13.98 (2.42)	15.37 (2.35)	***
Maternal vocabulary	99.53 (18.36)	96.64 (17.80)	105.90 (18.00)	***
Parenting quality	0.01 (0.72)	–0.10 (0.76)	0.25 (0.54)	***
Income-to-needs ratio	3.73 (2.86)	3.12 (2.22)	5.11 (3.57)	***
Proportion employed	0.66 (0.36)	0.65 (0.36)	0.70 (0.36)	*
Maternal depression	9.30 (6.62)	9.92 (6.84)	7.89 (5.85)	***
Proportion two-parent household	0.85 (0.31)	0.82 (0.33)	0.91 (0.24)	***
Neighborhood characteristics during early childhood				
Percent of households in poverty	9.84 (9.69)	10.40 (10.05)	8.56 (8.68)	**
Percent of single parent households	8.49 (7.86)	9.01 (8.31)	7.30 (6.59)	**
Percent of households receiving assistance	5.72 (6.48)	6.10 (6.69)	4.86 (5.90)	**
Percent of individuals unemployed	4.89 (4.26)	4.97 (4.31)	4.70 (4.14)	
Percent of adults with less than high school education	19.50 (13.53)	20.68 (13.82)	16.80 (12.43)	***
Percent of adults White	83.60 (20.49)	83.33 (20.64)	84.23 (20.16)	

Note. Descriptives were generated prior to multiple imputation. Estimates in parentheses correspond to standard deviations.
*$p < .05$.**$p < .01$.***$p < .001$.

Academic achievement and educational aspirations. To begin, adolescents' *cognitive skills* (Picture Vocabulary, alpha = 0.81), *literacy achievement* (Passage Comprehension, alpha = 0.81), and *math achievement* (Applied Problems, alpha = 0.87) were directly assessed with subtests from the Woodcock-Johnson Educational Battery–Revised (Woodcock & Johnson, 1989). Student's *working memory* was also directly assessed with the Operation Span Task (OSPAN; Turner & Engle, 1989), which required that they complete a series of arithmetic problems, remember a list of letters, and then do these tasks at the same time.

Through administrative records, we also had access to students overall *grade point average* at the end of ninth grade along with their *math coursework* (0 = no math course, 1= below Algebra I, 2 = Algebra I, 3 = geometry, 4 = Algebra II, and 5= advanced math) and *science coursework* (0 = no science coursework, 1 = survey science, 2 = earth science, 3 = biology, 4 = chemistry, 5 = physics, and 6 = advanced science).

Students also reported on their *mathematics* (alpha = 0.84) and *literacy self-concepts* (alpha = 0.83) using 10 items that were

Table 2
Age 15 Outcomes for All Students and Separated for Students Who Ever Attended a Private School Versus Those Who Only Experienced Public School Education Between Kindergarten and Ninth Grade

		Student Ever Attend a		
	Overall	Public School	Private School	Significant Difference
Ninth-grade adolescent outcomes				
Cognition	518.53 (13.24)	516.76 (13.06)	522.44 (12.79)	***
Literacy	520.45 (12.59)	518.88 (13.08)	523.89 (10.68)	***
Math	524.90 (16.96)	523.36 (17.12)	528.31 (16.12)	***
Working memory	32.51 (17.09)	31.30 (16.88)	35.16 (17.28)	**
Grade point average	3.05 (0.70)	2.99 (0.71)	3.17 (0.65)	**
Math course level	2.35 (0.89)	2.27 (0.91)	2.51 (0.84)	***
No math course (0)	0.05	0.06	0.04	
Below Algebra I (1)	0.04	0.05	0.02	*
Algebra I (2)	0.51	0.53	0.45	*
Geometry (3)	0.32	0.28	0.39	**
Algebra II (4)	0.08	0.07	0.10	
Advanced math (5)	0.00	0.00	0.00	
Science course level	2.05 (1.24)	1.94 (1.21)	2.29 (1.27)	***
No science course (0)	0.08	0.10	0.05	
Survey science (1)	0.31	0.32	0.30	
Earth science (2)	0.20	0.25	0.10	***
Biology (3)	0.34	0.28	0.46	***
Chemistry (4)	0.01	0.00	0.02	
Physics (5)	0.05	0.05	0.06	
Advanced science (6)	0.00	0.00	0.00	
Mathematics self-concepts	5.18 (1.16)	5.16 (1.18)	5.22 (1.11)	
Literacy self-concepts	5.65 (1.07)	5.62 (1.11)	5.71 (0.96)	
Likelihood of finishing high school	4.88 (0.42)	4.86 (0.45)	4.92 (0.34)	*
Likelihood of going to college	4.51 (0.88)	4.43 (0.94)	4.67 (0.69)	***
Likelihood of finishing college	4.48 (0.92)	4.41 (0.98)	4.63 (0.75)	***
Mother report of externalizing behavior	45.48 (10.42)	46.15 (10.48)	43.99 (10.16)	**
Mother report of internalizing behavior	46.73 (9.86)	46.91 (9.93)	46.33 (9.69)	
Mother report of social skills	104.10 (14.71)	102.91 (14.91)	106.77 (13.90)	***
Sexual risk taking	0.07 (0.19)	0.08 (0.19)	0.05 (0.16)	*
Risky behavior	0.18 (0.15)	0.19 (0.16)	0.16 (0.14)	*
Victimization	0.08 (0.12)	0.08 (0.13)	0.08 (0.11)	
Future outlook	2.62 (0.49)	2.62 (0.49)	2.62 (0.48)	

Notes. Descriptives were generated prior to multiple imputation. Estimates in parentheses correspond to standard deviations.
*$p < .05$.**$p < .01$.***$p < .001$.

adapted from the Self and Task Perception Questionnaire (1 = *not at all good* to 7 = *very good*; Jacobs, Lanza, Osgood, Eccles, & Wigfield, 2002), which captured the cognitive representation students had of themselves (e.g., "How good at SUBJECT are you?" and "How well do you expect to do in SUBJECT this year?"). Finally, students responded to three items that tapped into their educational aspirations, including the likelihood they would complete high school, attend college, and complete college (1 = *not sure at all* to 5 = *very sure*).

Social behavior. In addition to these academic-oriented outcomes, mothers also reported on their children's *externalizing behavior problems* (33 items that captured delinquent and aggressive behavior; alpha = .91), *internalizing behavior problems* (31 items that captured withdrawn, somatic complaints, and anxious/depressed behavior; alpha = 0.86), and *social skills* (40 items that captured cooperation, assertiveness, responsibility, and self-control; alpha = 0.91) with the Child Behavior Checklist (Achenbach, 1991) and the Social Skills Rating System (Gresham & Elliot, 1990), which were both based on a 3-point Likert scale (0 = *never* or *not true*, 2 = *very often* or *very true*).

Adolescents also used an audio computer-assisted self-interview to respond to a series of questions about risk taking over the course of the past year (0 = *never*, 2 = *never more than once*), which captured *sexual risk taking* (e.g., had sexual intercourse, diagnosed with an STD; 4 items; alpha =.58), *other risky behavior* (e.g., gotten into fights, drank, smoked; 36 items; alpha = .87), and *victimizatio*n (e.g., been injured, harassed, mugged; 15

items; alpha = .76). For each measure of risk taking, we top coded the top 2% to 3% of responses to address issues of skew. Finally, students used the Future Outlook Inventory (1 = *never* to 4 = *always*; alpha = 0.72; Cauffman & Woolard, 1999) to respond to eight questions that captured their *future outlook*, which tapped into their ability to consider the longer term consequences and implications for others as a result of their decisions.

Covariates. To address concerns of omitted variable bias and reduce the possibility of spurious associations, our multivariate models adjust for a large number of covariates. It is important to note that for all time-varying factors, we take the average of children's and families' experiences during early childhood when children were 6, 15, 24, 36, and 54 months of age (unless otherwise noted). We discuss these covariates in blocks, but in general, these covariates reflect the measurement of a large and substantial set of factors implicated in possible selection bias relative to estimating private school effects.

Family characteristics. Our first block of covariates taps into families' capacity and resources, namely: mothers' age and years of education at birth of child, mothers' psychological adjustment at 6 months of age (measured with the NEO Personality Inventory; Costa & McCrae, 1985), mothers' vocabulary skills at 36 months of age (measured with the Peabody Picture Vocabulary Test–Revised; Dunn & Dunn, 1981), parenting quality (measured with the Home Observation for Measurement of the Environment Scale, Caldwell & Bradley, 1984; and videotaped interactions that captured maternal sensitivity), household income-to-needs ratio, maternal employment, maternal depressive symptoms (measured with the Center for Epidemiological Studies Depression Scale; Radloff, 1977), and an indicator of whether children lived in a two-parent household.

Child characteristics. Our next block of covariates taps into children's own characteristics and experiences before kindergarten entry, which are likely correlated both with their later school performance and their parents' school choice. More specifically, at the child level, our statistical models adjust for: gender, race/ethnicity, birthweight, birth order, and temperament at 36 months of age (measured with the Infant Temperament Questionnaire; Medoff-Cooper, Carey, & McDevitt, 1993). As part of the child-level characteristics, we also adjust for student's academic achievement, working memory, and social-behavioral functioning as measured at 54 months of age, which is recognized as one of the strongest adjustments for omitted variable bias (NICHD ECCRN & Duncan, 2003). In addition to these characteristics of children themselves, we also control for children's early education experiences between 6 and 54 months of age, including the proportion of time spent in center care, proportion of time spent in maternal care, and child care quality (as measured with Observational Record of the Caregiving Environment).

Neighborhood characteristics. Because there is likely to be geographic heterogeneity in private school offerings and access, we also controlled for a rich set of neighborhood characteristics. More specifically, through census data, we had access to block group data for children's neighborhoods at 1, 15, 36, and 54 months of age, which we averaged together to capture neighborhood characteristics during the early childhood epoch. These variables include the percent of: households in poverty, single-parent households, households receiving government assistance, unemployed adults, adults with less than a high school education, and nonminority adults. As part of the neighborhood characteristics, we also adjusted for site fixed (i.e., Boston, Charlottesville, etc.), which captured the broader differences across communities.

Analytic plan. All analyses were estimated within the Stata program (StatCorp, 2009). To address missing data, we imputed 50 data sets via chained equations, and our focal research objectives were addressed within an ordinary least squares regression framework. Although two of our outcomes might best be thought of as ordinal categorical variables (i.e., math and science coursework), for simplicity, we specify all outcomes as continuous. As a precaution, however, we estimated parallel ordered logistic regression models for these two outcomes, and all conclusions were the same as those presented in the following (results available from authors).

Within this general framework, we estimated a series of regression models in stages to demonstrate the influence of covariate adjustments on the simple bivariate (and likely biased) "effects" of private school enrollment. First, we begin with simple bivariate models (Models 1), which do not adjust for other factors that may be related to children's school performance and their enrollment in public as compared with private schools. We then estimate a series of models that iteratively include different sets of covariates. In the first set of adjusted models (Models 2), we include family characteristics. In the second set of adjusted models, we add child characteristics and experiences during early childhood (Models 3), and then we include neighborhood characteristics and site fixed effects (Models 4).

Although our primary model specification corresponds to Model 4, we test an additional set of models that includes covariates that capture family (parenting quality, income-to-needs ratio, proportion of mothers employed, maternal depression, and two-parent household) and neighborhood characteristics during middle childhood (a composite of first, third, and fifth grades) and adolescence (ninth grade) as a means of providing a more conservative estimate of the associations between school experiences and adolescent outcomes. It is important to note that if private school enrollment is associated with the covariates during the later years (i.e., during middle childhood and adolescence), then their inclusion in the model might result in a biased estimate for private school enrollment. We nonetheless estimate these alternative models given the 10-year window for private school enrollment and to ensure that our findings are robust to various possible specifications.

With regard to whether private or public school is superior for certain children, we look at the experiences of lower income students and students in rural versus urban communities. To assess for heterogeneity across the income distribution, we recode our income variable to demarcate children and families who were: (a) at or below 300% of the federal poverty line (FPL)

Table 3
Descriptive Statistics for Private Versus Public School Enrollment

	M (*SD*) or %
Proportion of students enrolled in private school during . . .	
Kindergarten	0.23
First grade	0.19
Second grade	0.17
Third grade	0.17
Fourth grade	0.18
Fifth grade	0.16
Sixth grade	0.16
Seventh grade	0.15
Eighth grade	0.15
Ninth grade	0.14
Proportion of students who ever attended private school	0.31
Number of years students attended private school (full sample)	1.75 (3.30)
Number of years students attended private school (private school sample only)	5.73 (3.59)

Note. Descriptives were generated prior to multiple imputation.

during the early childhood years (mean income-to-needs ratio of 1.76, *SD* = 0.75) and (b) those whose income-to-needs ratio exceeded 300% of the FPL (mean income-to-needs ratio of 5.65, *SD* = 2.84). We also tested continuous income by private school enrollment interactions and considered different income groupings (i.e., at or below 225% of the FPL; 225%–450% of the FPL; greater than 450% of the FPL), and in each case, our results were the same as those presented in the following. Finally, to capture heterogeneity as a function of urbanicity, we grouped the 10 study sites into either rural (39% of students) or urban (61% of students) locations. In the following analyses, we present a within-group examination of the benefits of private school enrollment (i.e., we estimate the benefits of private school enrollment within the different groups), and interaction terms were entered into full sample regressions to formally test for the moderation. It is important to note that these models that capture heterogeneity in the associations between private school enrollment and ninth-grade functioning correspond to the Model 4 specification outlined previously and adjust for child, family, and neighborhood factors derived from the early childhood years (but omit income or site, depending on the moderator).

Results

We begin with a brief descriptive portrait of private school enrollment rates in the NICHD SECCYD. Then, we present bivariate and multivariate models that illustrate the differences in students' ninth-grade outcomes as a function of school type. To begin, roughly 14% to 23% of students attended a private school between kindergarten and ninth grade, and 31% attended a private school for at least one year (see Table 3). We also find that across the first 10 years of schooling, on average, students attended private school for 1.75 years (*SD* = 3.30); among students who ever attended private school, they averaged 5.73 years (*SD* = 3.59).

Next, our simple bivariate analyses of the associations between private school enrollment and ninth-grade outcomes, presented in Model 1 of Table 4, demonstrate that without adjusting for any selection or family background factors, students who *ever* attended a private school performed significantly better in ninth grade compared with students who *only* attended public schools on 14 of the 19 outcomes of interest, with absolute effect sizes of roughly 15% to 42% of a standard deviation. That is, students who ever attended a private school between kindergarten and ninth grade performed better academically, were more likely to take courses that were more rigorous, were more likely to expect to finish high school and attend (and complete) college, exhibited more optimal social-behavioral skills, and were less likely to engage in risky behaviors during ninth grade. The only exceptions to this general pattern was that adolescents who ever attended a private school did not demonstrate fewer internalizing behavior problems, they were not more (or less) likely to be victimized, nor did they exhibit higher self-concepts. Similar associations emerged when looking at the number of years students experienced private school education (see Model 1 of Table 5), with effect sizes of 8% to 18% of a standard deviation. In total, 24 of the 27 significant main effects of private school education had a p value of .01 or lower (and 15 of the 27 were significant at $p < .001$).

Again, these results are *not* adjusted for the factors that account for families' capacity or choice to enroll their child in a private school; as such, they are biased to the extent that unmeasured factors tap into selection effects. We present them here as the first step in a sequence of analyses intended to illustrate the confounding role of various factors as they pertain to students' age 15 outcomes, one of which is enrollment in private school.

Having established the bivariate associations between school type and adolescents' ninth-grade functioning, we next estimated a series of models that iteratively included different blocks of covariates that are regularly implicated in students' school performance and their likelihood of attending a public or private school (i.e., confounding or selection factors). Results from these adjusted models revealed that after accounting for any number

Table 4
Bivariate and Multivariate Results of Ever Attending Private School Versus Public School Education for Students' Ninth-Grade Outcomes

	Student Ever Attended a Private School						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Ninth-grade outcomes							
Cognition	0.42***	0.09	0.04	0.03	0.02	0.04	0.02
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)
Literacy	0.40***	0.08	0.04	0.03	0.03	0.02	0.02
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Math	0.30***	–0.00	–0.09	–0.09	–0.11	–0.10	–0.09
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)
Working memory	0.24***	0.13	0.07	0.05	0.05	0.05	0.03
	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)
Grade point average	0.23***	–0.11	–0.11	–0.09	–0.07	–0.10	–0.09
	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)	(0.07)	(0.06)
Math course level	0.26***	–0.04	–0.10	–0.11	–0.10	–0.11	–0.11
	(0.08)	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
Science course level	0.28***	0.05	–0.01	–0.03	–0.04	–0.02	–0.04
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Mathematics self-concepts	0.06	0.05	0.01	0.05	0.05	0.04	0.06
	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.07)
Literacy self-concepts	0.07	–0.04	–0.06	–0.08	–0.09	–0.08	–0.08
	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)
Likelihood of finishing high school	0.15*	0.03	0.01	0.03	0.02	0.03	0.02
	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)
Likelihood of going to college	0.27***	0.07	0.03	0.03	0.03	0.02	0.02
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Likelihood of finishing college	0.25***	0.05	0.02	0.03	0.03	0.02	0.03
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Externalizing behavior	–0.21**	–0.04	0.02	0.03	0.03	0.01	0.04
	(0.07)	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)
Internalizing behavior	–0.07	0.00	0.05	0.08	0.07	0.06	0.09
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Social skills	0.25***	0.07	0.00	0.00	0.02	–0.00	0.00
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)
Sexual risk taking	–0.16*	–0.02	–0.01	–0.02	–0.02	–0.03	–0.01
	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)
Risky behavior	–0.19**	–0.00	0.00	0.01	0.01	0.01	0.02
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Victimization	–0.09	0.05	0.05	0.03	0.06	0.02	0.04
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)
Future outlook	0.00	–0.02	–0.02	–0.04	–0.05	–0.02	–0.05
	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)
Covariates included in models							
Family characteristics during early childhood		X	X	X	X	X	X
Child characteristics and experiences during early childhood			X	X	X	X	X
Neighborhood factors during early childhood and site fixed effects				X	X	X	X
Middle childhood and adolescent factors					X		
Model specification							
Propensity score matched samples						X	
Weighted by number of waves "treatment" is observed							X

Note. All continuous outcomes have been standardized to have a mean of 0 and standard deviation of 1, and thus, estimates correspond to effect sizes. Estimates in parentheses correspond to standard errors.
*$p < .05$. **$p < .01$. ***$p < .001$.

Table 5
Bivariate and Multivariate Results for Years of Private Schooling Versus Public School Education for Students' Ninth-Grade Outcomes

	Number of Years Student Attended a Private School						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Ninth-grade outcomes							
Cognition	0.18***	0.04	0.03	0.02	0.02	0.02	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Literacy	0.15***	0.01	–0.00	–0.00	0.00	–0.01	–0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Math	0.12***	–0.02	–0.05	–0.04	–0.05	–0.05	–0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Working memory	0.10**	0.05	0.03	0.02	0.02	0.02	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
Grade point average	0.09**	–0.05	–0.05	–0.04	–0.03	–0.05	–0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Math course level	0.10**	–0.04	–0.06	–0.06	–0.06	–0.07	–0.06
	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)
Science course level	0.11**	0.01	–0.02	–0.03	–0.03	–0.04	–0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mathematics self-concepts	0.00	–0.01	–0.02	–0.01	–0.01	–0.02	–0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
Literacy self-concepts	–0.01	–0.06	–0.07*	–0.07*	–0.08*	–0.07*	–0.07*
	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
Likelihood of finishing high school	0.06	0.01	0.01	0.02	0.02	0.02	0.01
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Likelihood of going to college	0.11**	0.01	–0.00	0.00	0.01	–0.01	0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Likelihood of finishing college	0.09**	0.00	–0.00	0.00	0.00	–0.01	0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Externalizing behavior	–0.11***	–0.03	–0.00	0.00	–0.01	–0.01	0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Internalizing behavior	–0.04	–0.00	0.02	0.03	0.02	0.01	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Social skills	0.11***	0.02	–0.01	–0.01	0.01	–0.01	–0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Sexual risk taking	–0.09**	–0.04	–0.03	–0.04	–0.04	–0.04	–0.04
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Risky behavior	–0.08*	0.01	0.01	0.01	0.01	0.02	0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Victimization	–0.05	0.01	–0.00	–0.00	–0.00	–0.01	–0.01
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
Future outlook	0.01	0.00	0.00	–0.00	0.00	0.00	–0.00
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
Covariates included in models							
Family characteristics during early childhood		X	X	X	X	X	X
Child characteristics and experiences during early childhood			X	X	X	X	X
Neighborhood factors during early childhood and site fixed effects				X	X	X	X
Middle childhood and adolescent factors					X		
Model specification							
Propensity score matched samples						X	
Weighted by number of waves "treatment" is observed							X

Note. All continuous outcomes have been standardized to have a mean of 0 and standard deviation of 1, and thus, estimates correspond to effect sizes. Estimates in parentheses correspond to standard errors.
*$p < .05$. **$p < .01$. ***$p < .001$.

of potential confounds, students who attended private schools *did not* perform better than their peers who experienced public school education on any of the outcomes of interest (see Models 2, 3, and 4 of Tables 4 and 5). In fact, after accounting for families' income-to-needs ratio alone, only 1 of the 13 (when looking at the years of private school education) and 3 of the 14 (when looking at whether children ever enrolled in a private school) findings that were significantly different at a bivariate level remained significantly different. Put another way, the apparent "advantages" of private school education discussed previously in the simple bivariate comparisons that were not adjusted for confounds were almost entirely due to the socioeconomic advantages that selected families into these types of schools and were not attributed to private school education itself. And when we included middle childhood and adolescent covariates, we found no differences in our general conclusions (see Model 5 of Tables 4 and 5).

Finally, when examining whether private schools are superior for lower income students and students in rural versus urban communities, we found that none of the 152 coefficients was statistically significant (see Table 6). When we entered interaction terms into the full sample, we found that only 1 of the 76 interactions was statistically significant, and therefore, it was not interpreted. That is, there was no evidence of differential "effects" of private school enrollment across different locations or the income distribution.

Robustness Check

To ensure that our reported findings were robust, we estimated a series of supplemental models. First, as a means of addressing potential confounds, we estimated propensity score matching models (Rosenbaum & Rubin, 1983). It is important to note that these matching models are designed to be used with dichotomous predictors. Thus, as part of this algorithm, we matched children who never and ever attended private school and used the nearest neighbor method (with four matches) with a caliper of .01, ensuring a sufficient overlap between the two conditions on their propensity scores. Across the 50 imputed data sets, we successfully matched roughly 70% of the 1,097 students.

We assessed the quality of the matches in two ways. We first checked that the standardized mean difference (SMD) between groups for all of the covariates were less than 10% of a standard deviation, a benchmark used in the literature to indicate negligible differences (Austin, 2011). We also regressed each of the covariates, individually, on the indicator variable that distinguished children by school sector within the matched samples. Before matching, the average SMD in covariates between students who ever and never attended private school was a little over 25% of a standard deviation, suggesting that children who attended private school were qualitatively different from public school attendees. However, after matching, the average SMD was approximately 3% of a standard deviation, suggesting that balance was achieved (see Appendix Table A1). As noted previously, propensity score matching is designed to be used with dichotomous predictors, meaning that this method could not be applied for our continuous predictor for years of private school enrollment. However, within the matched samples of ever and never attendees, we found that the SMD between the covariates and years of enrollment was also considerably smaller. When regressing the years of private school enrollment on the covariates, we found that the average SMD before matching was 10% of a standard deviation; after matching, the average SMD was only 3% of a standard deviation. Additionally, before matching, over 80% of the covariates were significantly different as a function of private school enrollment; after matching, there were no longer any significant group differences (see Appendix Table A1).

Having successfully achieved balance, we ran a second set of regression models within these matched samples. To guard against any remaining bias, our analyses within the matched samples also controlled for all of the early childhood covariates listed in Table 1, which is recognized as doubly robust estimation (for more information on this methodology, see Funk et al., 2011). Results from these analyses within the matched samples confirmed our general conclusions discussed previously (see Model 6 of Tables 4 and 5): Among children who were equally likely to experience a public or private school education, there was no benefit through age 15 of enrollment in a private school.

Finally, because not all children had school records available for each wave of data collection, we estimated additional models that included an analytic weight that contained the number of years of data for which children had school type data. In doing so, children who had more data points received greater weight than children who had fewer data points. When weighted in this manner, we found that overall, students attended 1.71 years (SD = 3.23) of private school (vs. 1.75 years in our primary specification), and among students who ever attended a private school, they attended a private school for 5.52 years (SD = 3.57 vs. 5.73 in our primary specification). Despite this minor fluctuation in the mean years of private school enrollment, results from these weighted regressions analyses examining the benefits of such enrollment (net of child, family, and neighborhood covariates) were also the same as those discussed earlier (see Model 7 of Tables 4 and 5).

Discussion

Using a unique longitudinal study of children from 10 locations across the United States, the present study examined the extent to which enrollment in private schools, adjusted for a wide range of family background, child, and schooling factors, was related to academic, social, psychological, and attainment outcomes at age 15. In unadjusted models, children with a history of enrollment in private schools performed notably better on nearly all outcomes assessed in adolescence. However, by simply controlling for variation in family income, the majority of these differences in outcomes were eliminated. In follow-up analyses examining whether private schooling might differentially benefit children across the income distribution and children across different communities, we found no evidence of heterogeneity.

Importantly, the present study examined not only achievement, which has been the sole focus of all evaluations of private schooling reported to date, but also students' social adjustment, attitudes and motivation, and even risky behavior, all of which one assumes might be associated with private school education

Table 6
Heterogeneity in the Benefits of Private School Enrollment for Students' Ninth-Grade Outcomes

	Student Ever Attended a Private School				Number of Years Student Attended a Private School			
	Income <300% FPL	Income >300% FPL	Rural	Urban	Income <300% FPL	Income >300% FPL	Rural	Urban
Cognition	0.02	0.01	0.08	–0.01	0.00	0.02	–0.01	0.03
	(0.10)	(0.07)	(0.10)	(0.07)	(0.05)	(0.03)	(0.05)	(0.03)
Literacy	0.13	–0.05	–0.01	0.04	0.03	–0.02	–0.02	0.00
	(0.11)	(0.07)	(0.12)	(0.07)	(0.06)	(0.03)	(0.06)	(0.03)
Math	–0.03	–0.10	–0.02	–0.11	–0.03	–0.04	–0.00	–0.06
	(0.12)	(0.08)	(0.12)	(0.08)	(0.06)	(0.04)	(0.06)	(0.04)
Working memory	0.13	–0.02	0.02	0.07	0.07	–0.00	0.03	0.03
	(0.12)	(0.09)	(0.13)	(0.09)	(0.07)	(0.04)	(0.06)	(0.04)
Grade point average	–0.05	–0.12	–0.14	–0.08	–0.05	–0.03	–0.03	–0.06
	(0.12)	(0.08)	(0.12)	(0.08)	(0.06)	(0.04)	(0.06)	(0.04)
Math course level	–0.21	–0.02	–0.11	–0.12	–0.10	–0.02	–0.05	–0.07
	(0.13)	(0.09)	(0.14)	(0.09)	(0.07)	(0.04)	(0.07)	(0.04)
Science course level	–0.01	–0.01	–0.07	–0.02	–0.04	–0.01	–0.00	–0.04
	(0.13)	(0.10)	(0.13)	(0.10)	(0.07)	(0.05)	(0.07)	(0.05)
Mathematics self-concepts	0.12	0.06	0.04	0.06	–0.02	0.01	–0.01	–0.01
	(0.14)	(0.10)	(0.14)	(0.10)	(0.07)	(0.04)	(0.07)	(0.04)
Literacy self-concepts	–0.09	–0.06	–0.15	–0.02	–0.07	–0.06	–0.10	–0.05
	(0.14)	(0.09)	(0.14)	(0.09)	(0.08)	(0.04)	(0.07)	(0.04)
Likelihood of finishing high school	0.12	–0.02	–0.15	0.14	0.10	–0.01	–0.01	0.03
	(0.15)	(0.08)	(0.13)	(0.09)	(0.08)	(0.03)	(0.06)	(0.04)
Likelihood of going to college	0.17	–0.03	–0.04	0.08	0.08	–0.02	–0.02	0.01
	(0.14)	(0.08)	(0.14)	(0.08)	(0.08)	(0.03)	(0.07)	(0.04)
Likelihood of finishing college	0.20	–0.03	–0.12	0.13	0.09	–0.02	0.07	0.04
	(0.15)	(0.08)	(0.14)	(0.09)	(0.08)	(0.03)	(0.07)	(0.04)
Externalizing behavior	–0.04	0.09	0.06	0.01	–0.00	0.00	–0.00	0.00
	(0.12)	(0.08)	(0.13)	(0.08)	(0.07)	(0.04)	(0.06)	(0.04)
Internalizing behavior	–0.08	0.20	0.14	0.03	0.00	0.05	0.04	0.01
	(0.12)	(0.08)	(0.13)	(0.08)	(0.06)	(0.04)	(0.06)	(0.04)
Social skills	0.09	–0.03	–0.02	0.02	0.02	–0.01	0.01	–0.01
	(0.12)	(0.08)	(0.12)	(0.08)	(0.06)	(0.04)	(0.06)	(0.04)
Sexual risk taking	0.03	–0.04	–0.04	0.01	–0.04	–0.04	–0.04	–0.03
	(0.15)	(0.08)	(0.14)	(0.09)	(0.08)	(0.04)	(0.07)	(0.04)
Risky behavior	0.08	–0.06	0.09	–0.01	0.06	–0.03	0.02	0.02
	(0.13)	(0.08)	(0.13)	(0.09)	(0.07)	(0.04)	(0.06)	(0.04)
Victimization	0.16	–0.10	0.02	0.05	0.05	–0.05	–0.04	0.02
	(0.14)	(0.09)	(0.15)	(0.09)	(0.08)	(0.04)	(0.07)	(0.04)
Future outlook	–0.15	0.05	–0.01	–0.04	–0.06	0.04	–0.04	0.02
	(0.14)	(0.09)	(0.14)	(0.09)	(0.07)	(0.04)	(0.07)	(0.04)

Note. All continuous outcomes have been standardized to have a mean of 0 and standard deviation of 1, and thus, estimates correspond to effect sizes. Estimates in parentheses correspond to standard errors. FPL = federal poverty level.

given studies demonstrating schooling effects on such factors (Pianta et al., 2008; Vandell et al., 2016). In short, despite the frequent and pronounced arguments in favor of the use of vouchers or other mechanisms to support enrollment in private schools as a solution for vulnerable children and families attending local or neighborhood schools (e.g., Dynarski, 2016; Lubienski & Lubienski, 2013; Shakeel, Anderson, & Wolf, 2017), the present study found no evidence that private schools, net of family background (particularly income), are more effective for promoting student success.

As was noted earlier, the argument for policies and financial supports that enable parents to choose private schools as an alternative educational opportunity rests in part on assumptions that private schools provide a superior and more effective educational experience (Dynarski, 2016; Feigenberg, Rivkin, & Yan, 2017; Flanders & DeAngelis, 2017; Levchenko & Haidoura, 2016)

and create a market for school choice (Lubienski & Lubienski, 2013). In prior analysis using the present SECCYD sample, observations in first, third, and fifth grades across public and private school classrooms detected virtually no differences in qualities of instruction, teacher-student interaction, or delivery of content, even without adjusting for the students' family income levels (O'Brien & Pianta, 2010). Of note was the very wide variation in the nature and quality of private school classrooms, which in part might account for the lack of observed differences in classroom environments and the age 15 student outcomes reported in this paper. As was noted by Berends and Waddington (in press), the type of private school children attend may moderate the extent of potential benefits, and as with public schools, heterogeneity of school and classroom environments could be an asset in the sense of reflecting alignment with the needs of various students or groups, but it may also reflect a lack of an organized and programmatic approach to curriculum, teacher development, or school organization and structure (DeAngelis & Burke, 2017; Mills & Wolf, 2017).

Although recent studies separating enrollment from length of attendance suggest that the longer lower income students remain enrolled in a private school (at least up to four years), the higher the likelihood of accruing substantial benefits, the present report finds that length of enrollment was not associated with student outcomes once family income was taken into consideration, consistent with other nonexperimental work (Lubienski & Lubienski, 2013). For the one-third of the sample enrolled at any time in private school, on average, these students attended private schools for five to six years, which is longer than the most recent follow-up evaluations of voucher programs (Berends & Waddington, in press; Mills & Wolf, 2017). Thus, even for students who remained in private school for almost half of their K–12 experience, there was no discernable association with any of the wide range of outcomes we assessed at age 15.

Despite these contributions to the literature, we readily acknowledge a number of limitations of the present investigation. Notably, this is not an experimental study in which students enrolled in public or private school at random, thus eliminating family background, parenting, or related factors as explanations for any benefits detected for private (or public) school enrollment. Rather, the analytic models adjusted for a host of family, student, and contextual variables that hypothetically account for selection into private schooling or as explanations for any detected differences. We should note that this list of covariates is among the most extensive and comprehensive that has been applied to the question of school effects. And once family income was accounted for, the large differences in student outcomes that apparently favored private school enrollment were erased, so in fact, it is not at all clear that the operative selection mechanism is other than what is carried by income. We also estimated propensity score models, which supported our general conclusions when controlling for an extensive set of covariates. Ultimately, although correlation does not imply causation, for a causal impact, a correlation is necessary.

Furthermore, compared to many of the contemporary evaluations of voucher programs (Abdulkadiroglu et al., 2015; Figlio & Karbowmic, 2016; Flanders & DeAngelis, 2017; Mills & Wolf, 2017), the sample size in the present study is not as large. Although our sample had adequate power to detect main effect sizes of .10 (for our continuous specification) and .20 (for our binary specification) and greater, our analyses of heterogeneity were more limited. Thus, those findings should be interpreted with caution. And because the NICHD SECCYD sample was recruited from 10 sites across the country, reflecting literally hundreds of schools (public and private) and school systems, there is likely to be heterogeneity in the actual educational programs. Thus, in the future, closer attention must be paid to heterogeneity within the private school sector, which was beyond the data we had available.

It is also important to acknowledge that three of our measures were based on parental report of youth outcomes and might be less valid than direct student assessments because of the central role parents play in school choice. Although this is certainly a limitation, the parent reports of behavior problems parallel youths' own report of their risky behavior. Relatedly, because the standards for assigning grades differ by school district, and perhaps by schools within a district, the interpretation of the associations discussed previously with regards to school grades requires caution. Nonetheless, the associations between private school enrollment (or lack thereof) and students' grade point average in ninth grade parallel the associations with the direct assessments of students' working memory and their cognitive, literacy, and math skills, which lends confidence to our general conclusions. Finally, future studies should also consider the timing of private school enrollment as it may be that private school enrollment takes on different meaning during the early (e.g., kindergarten and first grade) versus later years (e.g., seventh and eighth grades). Unfortunately, this was beyond the scope of the data we had available because few students transitioned from a public to a private school in the later years.

In sum, we find no evidence for policies that would support widespread enrollment in private schools, as a group, as a solution for achievement gaps associated with income or race. In most discussions of such gaps and educational opportunities, it is assumed that poor children attend poor quality schools and that their families, given resources and flexibility, could choose among the existing supply of private schools to select and then enroll their children in a school that is more effective and a better match for their student's needs. It is not at all clear that this logic holds in the real world of a limited supply of effective schools (both private and public) and the indication that once one accounts for family background, the existing supply of heterogeneous private schools (from which parents select) does not result in a superior education (even for higher income students). Given that there is some evidence from the experimental literature that voucher programs may produce a slight benefit for the achievement of poor children to the extent they enroll steadily, it appears most important to better understand the mechanisms *in schools of any type* and in families that support student success and strengthen those resources accordingly.

NOTE

This study was directed by a steering committee and supported by the National Institute of Child Health and Human Development (NICHD) through a cooperative agreement (U10), which calls for

scientific collaboration between the grantees and the NICHD staff. Participating investigators, listed in alphabetical order, are: Jay Belsky, Birkbeck University of London; Cathryn Booth-LaForce, University of Washington; Robert Bradley, University of Arkansas, Little Rock; Celia A. Brownell, University of Pittsburgh; Margaret Burchinal, University of North Carolina, Chapel Hill; Susan B. Campbell, University of Pittsburgh; K. Alison Clarke-Stewart, University of California, Irvine; Sarah L. Friedman, CNA Corp., Alexandria, Virginia; Kathyrn Hirsh-Pasek, Temple University; Renate Houts, Research Triangle Institute; Aletha Huston, University of Texas, Austin; Jean F. Kelly, University of Washington; Bonnie Knoke, Research Triangle Institute, Research Triangle, North Carolina; Nancy Marshall, Wellesley College; Kathleen McCartney, Harvard University; Fred Morrison, University of Michigan; Marion O'Brien, University of North Carolina at Greensboro; Margaret Tresch Owen, University of Texas, Dallas; Robert Pianta, University of Virginia; Wendy Robeson, Wellesley College; Susan Spieker, University of Washington; Deborah Lowe Vandell, University of California, Irvine; Marsha Weinraub, Temple University. We express our appreciation to the study coordinators at each site who supervised the data collection, the research assistants who collected the data, and especially the families and child care providers who welcomed us into their homes and workplaces and cooperated willingly with our repeated requests for information.

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Manuscript received November 14, 2017
Revision received April 24, 2018
Accepted June 1, 2018

Appendix

Table A1
Sample Descriptives for Students After Propensity Score Matching

	After Matching: Student Ever Attend a		Standardized Mean Difference: Ever Attended a Private School		Standardized Mean Difference: Years Attended a Private School	
	Public School	Private School	Unmatched	Matched	Unmatched	Matched
Child characteristics and experiences during early childhood						
Preschool functioning						
Cognition	462.92 (13.25)	462.38 (13.50)	0.35*	–0.04	0.13*	–0.02
Literacy	376.49 (20.31)	376.67 (20.77)	0.56*	0.01	0.24*	0.04
Math	429.55 (17.26)	429.13 (15.94)	0.39*	–0.03	0.14*	–0.02
Working memory	460.21 (18.57)	460.31 (17.82)	0.31*	0.01	0.13*	0.02
Externalizing behavior	50.74 (8.68)	50.42 (9.81)	–0.20*	–0.03	–0.09*	–0.04
Internalizing behavior	46.92 (8.31)	46.50 (9.26)	–0.12	–0.05	–0.07*	–0.06
Social skills	101.29 (12.82)	101.47 (12.75)	0.37*	0.01	0.17*	0.04
Race						
White	0.88	0.87	0.17*	–0.04	0.07*	0.01
Black	0.06	0.07	–0.21*	0.04	–0.09*	–0.00
Other	0.06	0.06	0.01	0.02	0.00	–0.01
Male	0.51	0.50	–0.03	–0.02	–0.00	–0.00
Birth weight (pounds)	7.79 (1.14)	7.79 (1.07)	0.06	–0.01	0.00	–0.03
Birth order	1.76 (0.87)	1.74 (0.89)	–0.15*	–0.02	–0.07*	–0.02
Temperament	3.13 (0.40)	3.13 (0.40)	–0.20*	0.00	–0.09*	–0.02
Proportion of time in center care	0.24 (0.28)	0.25 (0.26)	0.27*	0.04	0.11*	0.02
Proportion of time in maternal care	0.26 (0.25)	0.25 (0.25)	–0.27*	–0.03	–0.11*	–0.01
Child care quality	2.98 (0.41)	2.97 (0.39)	0.15*	–0.01	0.09*	0.05
Family characteristics during early childhood						
Mother's age	30.37 (5.05)	30.17 (5.23)	0.42*	–0.04	0.23*	0.06
Psychological adjustment	61.39 (13.16)	61.19 (13.57)	0.24*	–0.02	0.13*	0.05
Maternal education	15.32 (2.35)	15.26 (2.33)	0.56*	–0.03	0.24*	0.03
Maternal vocabulary	105.00 (17.28)	104.97 (17.88)	0.50*	–0.00	0.21*	0.03
Parenting quality	0.24 (0.57)	0.23 (0.54)	0.48*	–0.03	0.21*	0.04
Income-to-needs ratio	4.76 (2.81)	4.65 (2.72)	0.70*	–0.04	0.33*	0.05
Proportion employed	0.69 (0.36)	0.70 (0.35)	0.13*	0.02	0.05	–0.00
Maternal depression	8.30 (6.16)	8.04 (5.90)	–0.30*	–0.02	–0.15*	–0.07
Proportion two-parent household	0.92 (0.23)	0.91 (0.24)	0.30*	–0.04	0.12*	0.05
Neighborhood characteristics during early childhood						
Percent of households in poverty	8.04 (8.85)	8.72 (8.78)	–0.19*	0.08	–0.09*	0.00
Percent of single-parent households	7.19 (6.95)	7.42 (6.61)	–0.22*	0.03	–0.12*	–0.05
Percent of households receiving assistance	4.80 (5.58)	4.94 (5.95)	–0.19*	0.03	–0.11*	–0.05
Percent of individuals unemployed	4.64 (3.82)	4.74 (4.15)	–0.07	0.02	–0.05	–0.04
Percent of adults with less than high school education	16.47 (12.70)	17.04 (12.48)	–0.29*	0.05	–0.14*	–0.04
Percent of adults White	85.25 (18.26)	83.98 (20.18)	0.04	–0.07	0.03	0.00

Note. Unmatched sample size was 1,097. Matched sample size ranged from 743 to 798 across the 50 imputed data sets. Estimates in parentheses correspond to standard deviations.
*Significantly different at $p < .05$.