

## Does Attendance in Private Schools across the K-12 Years Predict Individual Outcomes through Young Adulthood?

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### Does attendance in private schools across the K-12 years predict individual outcomes through young adulthood?

Forms of school choice, including charters, private schools, and homeschooling, is among the more prominent and possibly contentious forms of education policies in the United States. These alternatives to public schooling, some operated by local governments, are likely to receive even more attention and the prevalence of enrollments increase (particularly homeschooling) in the present and future COVID-era return to school. Moreover, supporting school choice is evident in efforts to use Federal COVID-related recovery funds in the mix of sources, including vouchers and tax-credits, that use public funds to finance non-public school educational enrollments (e.g., Dynarski, 2016; Urquiola, 2016). Extending this use of Federal funds for non-public enrollment, in *Espinosa v. Montana Department of Revenue*, the U.S. Supreme Court ruled that taxpayer funds in the forms of tax credits, could be used as incentives to donate to private school tuition scholarships which included religious schools. This latest ruling adds to the controversies concerning school choice, particularly the diversion of financial support from public schools that are coping with the consequences of the coronavirus pandemic, state and local budget shortfalls, and staffing challenges.

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Regardless of the policy focus, encouraging enrollment in private schooling is largely premised on the argument that private schools or charters provide a better educational experience and produce better educational outcomes than do public schools. Yet the evidence on the benefits of private or charter schools for academic achievement is mostly mixed (Dynarski, 2016) and positive effects typically are small (Mills & Wolf, 2017), with the vast majority of evaluations focused on student test scores or disciplinary records. In a recent series of reports, the Urban Institute compiled results from extended follow-up evaluations of long-running voucher programs in Milwaukee (Wolf, Witte, & Kasida, 2018), Florida (Chingos, Monarrez, & Kuehn, 2019) and Washington, D.C. (Chingos, 2018), focusing on impacts on college enrollment and attainment. Benefits were reported for both the Milwaukee and Florida programs' impacts on enrollment, and persistence in four-year colleges (Florida and Milwaukee) and two-year colleges (Florida), while the Washington D.C. study showed no impact of that relatively more recent program.

In the present investigation, we extend prior use of a unique longitudinal, prospective study to examine the extent to which enrollment in private schools between kindergarten and twelfth grade is predictive of educational attainment, employment, and personal outcomes for young adults enrolled in the NICHD Study of Early Child Care and Youth Development (SECCYD). In prior work on this sample, private school enrollment, once adjusted for family background and demographics, demonstrated no association with achievement or social outcomes through age 15 (Pianta & Ansari, 2018); the present investigation is among the first to include follow-up assessments through young adulthood (ages 18 and 26) associated with private and public school attendance across the K-12 years.

The core argument for school choice as it pertains to policies supporting enrollment in schools outside of the typical K-12 public school governance and operational structures, rests on the assumption that private or charter schools are more effective in educating students, and ultimately, produce students who are more successful later in life (Chingos, 2019; Dynarski, 2016; Flanders & DeAngelis, 2017; Levchenko & Haidoura, 2016). Short-term evaluations of school voucher programs, reporting mixed results with only modest benefits, suggest such assumptions may not warrant widespread use of these options as solutions for achievement gaps or for increasing the pool of high quality schooling (e.g., Carolson, Chingo, & Campbell, 2017). For example, a number of independent lottery studies of programs in Ohio, Louisiana, Indiana, and New York City, found that enrollment in private schools had mixed effects on achievement for low-income students as compared with program-eligible peers not attending private schools (Abdulkadiroglu, Pathak, & Walters, 2015; Berends & Waddington, 2018; Burke 2017; Figlio & Karbowinc, 2016; Mills & Wolf, 2017).

Longitudinal follow-up studies of students enrolled in regular and choice-based options also provide important perspectives on impact, at times demonstrating notable differences in results. Dynarski's (2016) review of voucher programs noted the presence of significant negative impacts on student achievement, at least for the first year or two in which a student had enrolled in a private school, with a shift in findings that demonstrate benefits in later years of the program. As one example of this shift, in the first years of Louisiana's program the average public-school student who began attending a private school using a voucher saw their test scores decline 16 percentile points after one year (Abdulkadiroglu, et al., 2015), whereas follow-up studies of students who remained enrolled in private schools indicate they outperformed their public school peers after a couple years (Mills & Wolf, 2017). Similar patterns of initial decline followed by improvement have also been documented in Indiana (Berends & Waddington, 2018; Mills & Wolf, 2017). Improved rates of graduation and college attendance and higher reading and math scores for Black students were also reported in New York City, as well as higher reading scores in a third follow-up year of the D.C. program (Dynarski 2016). Similarly, the Florida and Milwaukee voucher programs show benefits for college enrollment and persistence (Chingos, et al., 2019; Wolf, et al., 2018) and both are longer-running than the Washington D.C. program that shows no impact on those outcomes (Chingos, 2018).

Considering results across many controlled evaluations, it does appear that for students enrolled for longer periods in more mature school choice programs, performance often equals or exceeds that of public school attendees (Mills & Wolf, 2017; Wolf et al., 2018). These differences in findings may be attributed to the possibilities that students experience a considerable shift in expectations during the transition to a school of choice and require some time (years) to accommodate to the changed expectations or approaches. It may also be the case that the management and administration of these state school choice programs becomes better at identifying effectively operated non-public schools from the highly variable pool of non-public schools available within any specific catchment area (Berends & Waddington, 2018).

Private schools represent a sizable share of the education sector internationally, which therefore may be relevant when considering the merits of school choice policies or impacts of non-public schools. In one study, Sakellariou (2016) examined mathematics outcomes of private school programs in 2012 for students in 40 different countries. After adjusting for selection bias and peer effects, the majority of countries' results were largely null or negative. However, in a related analysis of such impacts across a much longer time frame (2000 to 2012), Shakeel (2017) reported increases in the rates of private schooling across that decade that contributed to improvements in test scores, on average, across the 62 countries included in the analysis. Accordingly, the evidence from international studies of the possible benefits of non-public schooling for student outcomes confirms the somewhat mixed evidence from the United States.

When interpreting results from past studies of private school attendance (both in the United States and internationally), several points warrant attention. Most importantly, it is widely known that household factors such as family income and parent education (among others) drive both selection of and enrollment in private school as well as provide opportunities and experiences that are implicated in students' educational and life success. Thus, a key challenge for empirical studies that contrast public and non-public schooling concerns how to remove effects of endogeneity or selection bias that stem from student, family, and neighborhood factors. Ideally, such concerns are addressed by some form of randomization of students to either condition, however it is also the case that statistical modeling (e.g., propensity score analysis, regression discontinuity, or the use of instrumental variables) can also be used to estimate schooling outcomes net of assessed selection factors.

A point also to be emphasized, particularly when evaluating large-scale school choice programs or non-public schooling more generally, is the exceptional variation across private schools in the United States (Carpenter, et al., 2016; Chingos et al., 2019; Levchenko & Haidoura, 2016; Redford, Battle, & Bielick, 2016; Urquiola, 2016). The category includes private schools operated by religious denominations (itself a highly heterogeneous group) or those organized around specialized curricula or instructional models (e.g., Montessori, Waldorf). In addition, non-public schools differ widely with regard to the auspices under which they operate, which can range from highly institutionalized and systemic (e.g. the Archdiocese of New York) to very localized and small (e.g., community schools), factors that may have implications for governance, teacher qualifications, management and operational supports, finance, and evaluation. Thus, when the *category* of private schooling is considered as a policy choice or target of evaluation studies, it is important to keep in mind the heterogeneity of the sector, which may be desirable from one set of perspectives (reformers, advocates, parents), but can also lead to challenges when interpreting the results of evaluations that aggregate these various types into the category of "private."

Finally, the design features of evaluation studies play a role in interpreting results. For example, it does appear that evaluations across larger time spans provide unique perspectives on impacts. Longer-term evaluations allow program effects to be estimated using more stable indicators of student performance (e.g., not reliant on only one year of test scores) and enable the typically small annual impacts of schooling to accrue. A second aspect of study design that may provide information on program benefits is the inclusion of student outcomes other than state achievement test scores (e.g., Chingos, 2018). Nearly all well-controlled evaluations of voucher programs or private school enrollment utilize only outcome assessments in the domains of math and language arts (reading) available from the state standardized testing programs (e.g., Dynarski, 2016) and do not include multi-year assessments of outcomes such as discipline and behavior, motivation, or adjustment, each of which are typically included in the rationale for private school enrollment and superiority.

## The Present Study

In the present study, we draw on our prior use of a unique longitudinal, prospective study of children and families to extend an examination of the extent to which enrollment in private schools is: (a) predictive of study participants' outcomes at the end of high school and as these individuals attain young adulthood, with assessments at ages 18 and 26; and (b) whether the outcomes of private school education vary for key subgroups of students. In the prior investigation of achievement, social, and personal outcomes at age 15 for students enrolled in the NICHD SECCYD, in unadjusted models students with a history of enrollment in private schools performed notably better on nearly all outcomes assessed in adolescence. However, by controlling for only variation in family income, the majority of the differences in participants' outcomes were eliminated (Pianta & Ansari, 2018). Moreover, follow-up analyses examining whether private schooling might differentially benefit age 15 outcomes for poor students found no evidence in support of this hypothesis. In short, prior use of the NICHD SECCYD to investigate associations between private school attendance across the K-9 grades on a range of age 15 student outcomes found no evidence that private schools, net of selection factors, are more effective for promoting student success than public schools.

This study has a focus on more distal assessments of educational and employment outcomes as indicators of behavioral success, and civic engagement, variation in which could be attributable to public or private school enrollment. At age 18, outcomes include indicators of educational success such as grades in high school, highest level(s) of courses taken in math and science, the number of honors or AP courses taken, and whether a student ever failed a course in high school. Such academic performance metrics are part of many employment screens, most college admission reviews, as well as used in summary statistics on state and national levels to gauge quality of an education system. As such these are indicators of educational attainment that have validity as gateways for individuals' access to further opportunity and as policy-relevant markers of success. Also at age 18, participants reported on behavioral outcomes that included engaging in risky behaviors (drugs, crime) and their sense of connection to school, both of which have been part of the rationale for students to attend private schools that presumably reduce exposure to "risky" peers and build education as a personal value. At age 26, attainment outcomes relevant for this period of young adulthood included cumulative years of education, employment status and annual salary, accrued debt, and arrest record. Such outcomes are regularly the focus of government reports related to population performance, inputs to economic success, and impacts of investments in education. In addition to attainment, outcomes at age 26 also included engagement in risky behavior as well as civic engagement in terms of voting and volunteerism.

This portfolio of assessments enabled us to examine public and private school attendance relative to a wide range of individually and societally important metrics. These developmentally-relevant indicators of behavioral success and civic engagement in this period of life are consistent with those assessed in other studies of young adults (e.g., Allen & Worrell, 2016) as related to contemporaneous and future performance in employment, family life, and society. With regard to the outcomes of schooling, educational attainment post-high school and employment can each be considered reflections of investments in education, at both the individual and societal levels.

However, as was the case in our prior report using the SECCYD to examine the outcomes of private school education, the present study is observational and descriptive in nature; the design does not eliminate selection bias. Thus, although the study does include comprehensive assessment of outcomes at multiple intervals, a time frame that is much longer than typical studies of private schooling, and detailed and wide-ranging assessments of family background and contextual processes help estimate (and reduce) selection bias (e.g., Pianta et al., 2008; Belsky et al., 2007; Vandell et al., 2010, 2016), the design cannot support causal inferences. The SECCYD sample does reflect a broad range of economic conditions, cultural beliefs, and child rearing practices, such as is the case in the United States. However the overall sample size was modest at the start (approximately 1,300), with attrition over 25-plus years. Compared to many lottery-based studies of private schooling that leverage state achievement scores, the sample size at the young adult follow-up is small. In sum, as in our prior use of this longitudinal sample to assess private schooling effects, the present study trades off depth of information available on individuals across a longer time scale and across a broader set of domains against sample size and using statistical in contrast to experimental controls for selection.

## Method

### *Participants*

The NICHD SECCYD is a multi-site research project originally designed to determine the effects of early childcare on children's development. Participants were recruited in 1991 from designated community hospitals at 10 University-based data collection sites: (1) Little Rock, AR; (2) Irvine, CA; (3) Lawrence, KS; (4) Boston, MA; (5) Philadelphia and (6) Pittsburgh, PA; (7) Charlottesville, VA; (8) Seattle, WA; (9) Hickory and Morganton, NC; and (10) Madison, WI. Recruitment and selection procedures are described in detail (NICHD ECCRN, 2001) and study procedures are described on the study website (<http://secc.rti.org>). The original study followed children from birth to 15 years with a common study protocol, including interview, home, school, and neighborhood observations. 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.

In terms of sampling, 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 the US population vs. 80.4% in cohort) and household income (household income information available on 1,271 families; \$36,520 in US 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%,  $p = 0.04$ ).

Of the 1,364 children who were originally enrolled into the study at the 1-month home visit, we: (a) excluded 207 children who had no record of school type and (b) 60 children who were ever homeschooled or ever enrolled in a public charter school. Moreover, the current investigation also draws from three separate data collections that occurred at age 18, 23, and 26 that included any of the original study participants that could be contacted and agreed to participate. Those who participated through the age 26 wave of data collection constituted our final analytic sample, which included 772 of the 1,097 participants who also were the focus of the prior investigation comparing public and private school enrollment associations with their age 15 outcomes. For sample descriptives for the 772 study participants, 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 twelfth grade (all 10 sites had public school kindergarten). Between kindergarten and ninth grade, schools provided yearly information on students' school sector. Then, at the age 23 follow-up, study participants provided consent for their school records to be checked. From this information, we were able to determine school enrollment information for students between tenth and twelfth grade. These school records were used to measure students' exposure to public and private school. For the purposes of the current study, students were allowed to have missing data on the school type variable over time in order to maximize the focal predictor (i.e., students were included if they had missing data at later waves on school type). On average, students had information on school sector available for 11.27 years ( $SD = 1.78$ ) of the 13 years of school enrollment data, and approximately 86% of students had at least nine years of school type data. However, because of our sample inclusion requirements, all students had at least one wave of data on school type (with the minimum being five).

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 13 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 students experienced private school by 13 (i.e., the years of data collection between kindergarten and twelfth grade).

**Age 18 outcomes.** Young adult functioning at age 18 was based on nine benchmarks collected through self-report at the end of high school (see Table 2 for descriptives).

**Educational outcomes.** Five dimensions of young adults' educational outcomes were measured at age 18. To begin, study participants reported on their typical grades in high school by selecting among the following options (mostly As, about half As and half Bs, mostly Bs, about half Bs and half Cs, mostly Cs, about half Cs and half Ds, mostly Ds, or mostly below Ds). We used this information to create a grade point scale (e.g., mostly below Ds = 1; mostly As = 4). In addition to their high school grades, study participants also reported on their final high school science and math coursework and their advanced coursework. In terms of math courses, study participants selected from one of eight options (1 = *none*, 2 = *general math*, 3 = *pre-algebra*, 4 = *algebra I*, 5 = *geometry*, 6 = *algebra II*, 7 = *precalculus or trigonometry*, or 8 = *calculus*), whereas for science coursework young adults selected from one of six options (1 = *none*, 2 = *basic science*, 3 = *physical/earth science*, 4 = *biology I*, 5 = *chemistry I*, 6 = *physics or advanced science*). In terms of advanced coursework, study participants reported on the total number of honors courses taken (0 = *no honors classes*; 4 = *Four or more honors classes*) and advanced placement (AP) courses taken (0 = *no AP classes*; 4 = *four or more AP classes*). Similar to past studies (Vandell et al., 2016), these variables for honors and AP courses were summed together to yield the total advanced coursework. Finally, study participants reported on the number of classes they had failed (0 = *none*, 1 = *once*, 2 = *twice*, 3 = *three times or more*).

**Behavioral outcomes.** Four dimensions of students' behavioral outcomes at age 18 were reported on. First, students were asked how many times they skipped school during their senior year (0 = *never*, 1 = *once or twice*, 2 = *about once a month*, 3 = *skipped every week*). We recoded the responses options to correspond to the number of days skipped across the year (i.e., 0 = 0 days, 1 = 1.5 days, 3 = 10 days, 4 = 36 days).

Students were also asked whether they were ever suspended or expelled during high school, with responses options on a 4-point Likert scale (0 = *no*, 1 = *once*, 2 = *twice*, 3 = *more than twice*). In addition to these single item measures, questions were asked of students' engagement in risky behavior and their school attachment. Risky behavior was based on a 3-point scale (1 = *never*, 2 = *once*, 3 = *more than twice*) with 53 items (e.g., using drugs, drinking alcohol etc.; alpha = .85, Halpern-Felsher et al., 2001), whereas school attachment was based on five questions from the National Longitudinal Study of Adolescent Health (e.g., I am happy to be at my school; I feel close to others in my school; alpha = .78).

**Age 26 outcomes.** Young adult functioning at age 26 was also based on a variety self-report benchmarks across nine dimensions (see Table 2 for descriptives).

*Educational and employment outcomes.* To begin, respondents reported on their total years of education and annual salary. Given issues of skew ( $>15$ ), we top coded the top 1% of salary responses, which reduced skew to a more acceptable level of approximately two. Study participants were also asked to report on their debts that did not include student loans or a mortgage on a home. Responses were made on an 8-point Likert scale (1 = *less than \$1,000*, 8 = *\$250,000 or more*), which we recoded to dollar figures using the midpoint of responses. For example, responses of \$1,000 to \$4,999 were recoded to \$2,999.50. The bottom and top responses were recoded to \$500 and \$250,000, respectively. Finally, in terms of employment, young adults reported: (a) how many weeks they were unemployed and looking for work, or laid off from a job, with responses ranging from 0 (*0 weeks*) to 8 (*27 weeks or more*); and (b) how satisfied they were with their current employment (1 = *very unsatisfied*, 5 = *very satisfied*). We recoded the unemployment responses using the midpoint to generate the weeks study participants were unemployed (e.g., 5-9 weeks = 7 weeks).

*Behavioral outcomes.* Young adults also reported on four dimensions of their behavioral outcomes. First, participants reported how many times they were ever arrested (0 = *never*, 1 = *one time*, 2 = *two times*, 3 = *three or more times*). In addition to records of arrest, young adults reported on their risky behavior in the past year. These were based on 56 survey items (alpha = .77) that were developed for the NICHD SECCYD and draw on work by Conger and Elder (1994), the Fast Track project, and the New Hope project. Sample questions include: sold drugs illegally, carried a hidden weapon, and taken or stolen something worth more than \$500. The risk-taking composite is computed as the mean of all risky behavior items using the original 1 (*never*) to 3 (*more than twice*) scale. Higher values indicate higher mean level of risky behaviors. At age 23 and 26, study participants were asked whether they voted in the last presidential election (corresponding to 2012 and 2016). We used these indicators to create a composite of the proportion of presidential elections participants voted in. Finally, young adults were asked whether in the last 12 months, they participated in, or helped out, in any type of volunteer work or helped a charitable organization (e.g., gave time or money). Responses were made on a 7-point Likert scale (1 = *never*, 7 = *every day*).

**Covariates.** To address concerns of omitted variable bias and reduce the possibility of spurious associations, our models adjust for a large number of covariates drawn from the period before children entered public or private schools. 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). At the child-level, models adjust for: gender, race/ethnicity, birthweight, birth order, temperament at 36 months of age (measured with the Infant Temperament Questionnaire; Medoff-Cooper, Carey, & McDevitt, 1993), proportion of time spent in center care and maternal care, and child care quality (measured with Observational Record of the Caregiving Environment). As part of the child-level characteristics, we also adjust for student's academic achievement, working memory, and social-behavioral functioning prior to kindergarten entry, which is recognized as one of the strongest adjustments for omitted variable bias (National Institute of Child Health and Human Development & Duncan, 2003).

Next, in terms of family characteristics, we included: 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.

Finally, through census data, we also had access to neighborhood characteristics during early childhood, including the percent of: households in poverty, single parent households, households receiving government assistance, unemployed adults, adults with less than a high school education, and non-minority adults. As part of the neighborhood characteristics, we also adjust for site fixed effects.

Overall these child, family, and neighborhood covariates reflect measurement of a large and substantial set of factors implicated in possible selection bias relative to estimating the outcomes of private school education.

**Analytic Plan.** All analyses were estimated in Stata (StataCorp, 2009) and missing data were accounted for using 50 imputed datasets. The focal research objectives were addressed within a regression framework and all continuous outcomes were standardized to have a mean of 0 and standard deviation of 1 and, therefore, coefficients can be interpreted as effect sizes. Within this general framework, we estimated a series of different statistical models to determine whether study participants demonstrated different outcomes based on two parametrizations of school type: 1) ever enrolled in private school and 2) number of years enrolled in private school.

We begin with simple bivariate models (Model 1), which do *not* adjust for other factors that may be related to students' educational and behavioral outcomes and their enrollment in public as compared with private schools. We then estimate models that include potential confounds and selection effects from prior to kindergarten entry, including child characteristics and experiences during early childhood, family characteristics, and neighborhood characteristics (Model 2). Even though Model 2 corresponds to our primary analytic specification, we then estimated several additional models that test the robustness of findings and determine the extent to which detected findings are at all biased due to analytic decisions.

First, given the long window of private school enrollment, Model 3 included additional time-varying covariates. Specifically, these time-varying covariates 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). The next set of models (Model 4), adjust for early childhood experiences (as in Model 1) and add weighting for "treatment" length. These models were examined because some children had more waves of information for school type. Thus, in this analytic specification, students who had more data points received greater weight than those students with fewer data points.

In Model 5, adjustments again include the base early childhood covariates and add propensity scores to further adjust for selection. Even though propensity scores do not address omitted variables nor the change the causal identification strategy, this methodology does consider whether there is overlap in the unmatched sample and the functional form assumptions that are driving findings (or lack thereof). As part of the matching processes, we matched children who never and ever attended private school and used the nearest neighbor method (with four matches) with a caliper of .05, ensuring a sufficient overlap between the two conditions on their propensity scores. Across the 50 imputed data sets, we successfully matched 542-580 of the 772 students (the number of matches varied across the 50 imputed datasets). And even though propensity score matching is designed to be used with dichotomous indicators, meaning that this method could not be directly applied for our continuous predictor for years of private school enrollment, we used the matched samples of ever and never attendees, which greatly reduced the group differences on years of enrollment as well. Before matching, the average standardized mean difference between those ever in a private school and those only in public school was .25; after matching the difference was only .04. And, when regressing the years of private school enrollment on the covariates, we found that the average standardized mean before matching was .11; after matching, the average difference was only .04.

Next, to address concerns regarding attrition, Model 6 uses the base early childhood covariate set and multiple imputation to retroactively include all 1,097 participants from the age 15 sample that were part of the prior study (Pianta&Ansari, 2018). Finally, Model 7 considers whether there is any evidence of heterogeneity in the outcomes of private school education across different subgroups of children and families. Specifically, we include (separately) interaction terms between family income, material education, urban vs rural, and parenting quality with student's school type to test for moderation.

## Results

We first present a descriptive snapshot of private school enrollment rates. In total, 15-25% of students attended private school in any given year between kindergarten and twelfth grade and 32% attended a private school for at least one year (see Table 3). Moreover, across the entire span of participants' K-12 education, on average, students attended private school for 2.05 years ( $SD = 3.78$ ); among students who ever attended private school, they averaged 6.37 years ( $SD = 4.11$ ).

### The outcomes associated with private school education

The base set of bivariate analyses of the outcomes of private school enrollment, presented in Model 1 of Table 4, demonstrate that, without adjustments for selection, students who *ever* attended a private school demonstrated significantly better outcome performance in nearly all areas at age 18 and several key areas at age 26 as compared with students who *only* attended public schools between kindergarten and twelfth grade. These associations were statistically significant for 11 of the 18 outcomes of interest, with absolute effect sizes of 16-37% of a standard deviation. More specifically, students who ever attended a private school between kindergarten and twelfth grade earned better grades, took more advanced and rigorous courses, reported more attachment to school, and were less likely to fail a course, skip school, or be suspended or expelled from school. By age 26, private school attendance was associated with more years of education and a higher annual salary, as well as an increased engagement in volunteerism. With that said, *no* differences emerged in these bivariate analyses between public and private school attendees in terms of their employment, job satisfaction, debt, and engagement in risky and criminal behavior. Similar patterns emerged when looking at the total accrued years students experienced private school education (see Model 1 of Table 5), with effect sizes of 9-18% of a standard deviation.

Having established the unadjusted bivariate associations between school type and students' age 18 and 26 outcomes, we next estimated a series of models that were more methodologically rigorous and accounted for selection effects. Results from these models revealed that, after accounting for potential confounds in early childhood (see Model 2 of Tables 4 and 5), students who attended private schools *did not* perform better than their peers who experienced public school education on 9 of the 11 findings that were significantly different at a bivariate level. Put another way, the apparent advantages of private school education discussed above in the bivariate models appeared to be almost entirely due to the other confounds that were associated with both private school enrollment and the outcomes under study and *not* attributable to private school education itself. The two exceptions were that private school attendees, even when accounting for selection effects, were less likely to fail a class in high school and less likely to skip school as a high school senior than public school students.

All of the focal associations (or lack thereof) reported above largely replicated when: (a) accounting for time varying covariates from middle childhood and adolescence (see Model 3 of Tables 4 and 5); (b) accounting for the number years of data for which children had school type data (see Model 4 of Tables 4 and 5); (c) re-estimating the models within the propensity score matched samples (see Model 5 of Tables 4 and 5); and (d) re-estimating models with the original sample of 1,097 students (see Model 6 of Tables 4 and 5). Accordingly, the focal associations of interest reported above were *not* biased as a result of our analytic decisions.

### Heterogeneity in the outcomes of private school education

Having established the average associations between private school education and students' age 18 and 26 outcomes, we next considered whether the associations between private school enrollment and the focal outcomes of interest varied for different subgroups of children. Of the 144 interactions estimated, approximately 5% were statistically significant, which is no more than chance. Accordingly, there was *no* evidence of heterogeneity in the outcomes of private school education for students in urban versus rural communities, as a function of families' socioeconomic status (as measured by mothers' education and family income), nor as a function of household quality. Put another way, the outcomes of private school education, at least in this study sample, were far more similar than different across key subgroups of interest.

### Discussion

The present investigation used data from the NICHD SECCYD to examine the extent to which enrollment in private schools across the K-12 years was associated with young adulthood outcomes assessed at ages 18 and 26. Results from unadjusted models revealed that private school attendees performed significantly better on nearly all educational and behavioral benchmarks measured at age 18 and several key benchmarks at age 26. These associations were largely eliminated when predictive models controlled for selection factors; there was no evidence of heterogeneity of associations as a function of urbanicity, socioeconomic status, and household quality. Accordingly, despite the arguments in support of enrollment in private schools (e.g., Dynarski, 2016; Shakeel, Anderson, & Wolf, 2017) and recent policies that promote private school enrollment (see *Espinosa v Montana Department of Revenue*), little to no evidence was detected for the premise that enrollment in private schools, net of child, family, and neighborhood characteristics, was more strongly linked with success as young adults, for this cohort of individuals.

One 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 more effective educational experience (Dynarski, 2016; Feigenberg, Rivkin, & Yan, 2017; Flanders & DeAngelis, 2017;



Levchenko & Haidoura, 2016) that results in better outcomes for students and are a potential solution for achievement gaps associated with poverty or race. The evidence in support of this argument is at best, mixed, particularly once selection factors are taken into account. As one illustration, in prior analysis using the SECCYD sample observations in first, third, and fifth grade across public and private school classrooms detected few differences in the amount or quality of instruction, features of teacher-student interaction, or the delivery of content. Notably this was the case *without* adjusting for the students' family income levels (O'Brien & Pianta, 2010). In the present study no differences were detected for students' sense of attachment to school, which may reflect aspects of school experience, as well as exclusionary discipline (suspension/expulsion). However, students enrolled in public schools were more likely to engage in skipping school in their senior year, which could be a reflection of attitudinal or motivational differences or aspects of the school experience. Although challenging to interpret, these and prior findings pertaining to private and public school enrollment associations with outcomes other than achievement tests suggest intriguing areas for further examination as investigators attempt to better understand the nature and impact of varying forms of schooling.

As we have noted, the lack of differences reported herein and in the prior SECCYD paper (Pianta & Ansari, 2018) may in part be a function of the highly heterogeneous nature of the non-public school sector, in part a function of the lack of regulations pertaining to it. Berend and Waddington (2018) have also emphasized that private schools are highly heterogeneous, demonstrating that school type may moderate the extent of potential benefits associated with enrollment; note that voucher program evaluations are either state or district specific whereas the present investigation was national in scope and sample and therefore perhaps more heterogeneous. The matter of private school heterogeneity was also identified in the studies of Louisiana's voucher program as a reason for why the first-year results were negative; it took a few years of operating the program to identify stronger and better-managed private schools (). Although variation across school or classroom environments could reflect educationally or developmentally appropriate alignment with the needs of various individual students or groups, 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). It may be the case that the more highly regulated public school sector, with standards for teacher certification, class size, curriculum, and student assessments (to name a few features) create a floor, or baseline, for operations that may reduce some of the negative aspects of heterogeneity seen within non-public sector schools.

In our prior investigation of private schooling in the SECCYD, as in the current study, we examined enrollment not only as a static factor, but included length of enrollment in analysis of effects (Pianta & Ansari, 2018). Consistent with the prior study, in the present analysis no association was detected between the length of enrollment in private school and outcomes through age 26, once adjusting for selection factors. It should be emphasized that several well-controlled random assignment evaluations do indicate that the longer a low-income student remains enrolled in a private school (at least up to 4 years) the higher the likelihood of accruing substantial benefits (Berends & Waddington, 2018; Chingos, et al., 2019; Mills & Wolf, 2017; Wolf, et al., 2018). The differences in study designs and samples, in which controlled lottery evaluations include much larger samples overall and many more low-income students than the SECCYD may account for some of these discrepancies in results. However, for the roughly 30% of the SECCYD sample enrolled at any time in private school, on average these students attended private schools for almost 6 years, 2 years longer than many follow-up evaluations of voucher programs (Berends & Waddington, 2018; Mills & Wolf, 2017). The findings of the present study related to length of private school enrollment perhaps point out the need for continued longitudinal follow-up of samples included in larger-scale evaluations of voucher programs.

With regard to specific outcomes in young adulthood, the present study found no differences in educational attainment (measured in years of education) through age 26. This contrasts with evaluations of the Florida and Milwaukee voucher programs that show benefits for college enrollment and persistence (Chingos, et al., 2019; Wolf, et al., 2018). Although in the present study we did not detect any differential benefit for educational attainment for low-income children, it is possible that the voucher program samples, which are much larger and more uniformly low-income, demonstrate the benefits of non-public schooling for attainment outcomes for low-income students. However it was also the case that non-public enrollment, once adjusted, showed no benefits for other educational attainment outcomes, including high school grades and course-taking, with the notable exception that across all model specifications public school-enrolled students were more likely to take an Advanced Placement course, perhaps reflecting the availability of those courses.

Moreover, the inclusion in the prior and present study of behavioral outcomes, and particularly the young adult outcomes included herein point out the importance of considering a broad set of indicators as markers of success at that phase of life.

Adjusting for family background factors, students enrolled in public school throughout their entire school career had economic (salary, employment history, debt), personal (risky behavior), and civic engagement (volunteerism, voting) outcomes equivalent to peers who enrolled in private school. Given the pattern of results and differences in samples described above for contrasting the present study with evaluations of voucher programs, showing benefits of those programs for educational attainment, it would be informative to know whether such putative benefits might carry over into this broader set of life outcomes.

When interpreting the findings from the present investigation, there are a number of limitations that should be taken under consideration. First and foremost, students in the NICHD SECCYD were not assigned randomly to attend public or private schools. Instead, we employed several different analytic strategies to adjust for selection bias, which limits causal inference. But even though causal claims cannot be made, rigorous descriptive and correlational methods, such as those used in the present investigation, can help expand our understanding of the outcomes of different educational institutions (Loeb et al., 2017). Second, the age 26 sample was only about 70% of the original study's public and private school participants and was more advantaged than that sample in certain ways. With that said, the findings replicated when using contemporary missing data techniques, which may mitigate concerns about generalizability and attrition. As noted above, the sample used in the present investigation was small when compared with other program evaluations (e.g., Abdulkadiroglu et al, 2015; Figlio & Karbowmic, 2016; Flanders & DeAngelis, 2017; Mills & Wolf, 2017), which limits power to detect small differences between public and private schools. Even so, the covariate-adjusted models revealed that the absolute effect size for the non-significant associations between private school education and the young adulthood outcomes of interest were roughly 2 and 5% of a standard deviation for the ever-enrolled in private school and number of years enrolled in private school parametrizations, respectively. In many instances, net of selection effects, the direction of associations (although not significant) favored public schools.

A possible limitation is that the outcome measures at both ages 18 and 26 were based on self-report. Although self-report is often the most valid way to measure several of the outcomes of interest, some could have been pulled from administrative records (e.g., school grades, coursework). However, the use of self-report is bolstered by evidence from prior meta-analyses suggesting that self-reports are strongly correlated with school records (correlations  $> .80$ ; Kuncel et al., 2005), meaning that self-reports are adequate substitutes. Finally, the NICHD SECCYD recruited children and families from tens of states across the United States, reflecting many schools and school systems. Accordingly, the variability within educational sectors might also reduce the likelihood of detecting differences, which requires attention in future studies.

With these limitations in mind, the results of the present study found little to no evidence for policies that would support widespread enrollment in private schools, or public funding for private school education, as a means of promoting long-term life success. In most discussions of such gaps and educational opportunities, it is assumed that most poor children attend poor quality schools, and that their families, given resources and flexibility, could choose among a robust supply of high-quality private schools in which to enroll children. The evidence reported here and in our prior study (Pianta & Ansari), as well as studies showing rather mixed effects for voucher program impacts, call these assumptions and the underlying logic into question. In what is one of the longest-term follow-up studies of students enrolled in private schools (for an average of 6 years), after adjustments for family factors public and private school attendance had virtually no association with self-reported markers of educational attainment, employment, civic engagement or behavioral success in early adulthood. Whether such null effects would hold for long-term life course outcomes of students enrolled in contemporary charter or other non-public school auspices remains to be seen, and it may be of even more value to not only examine long-term impacts of schooling of any type, but also to conduct such impact studies with attention to a breadth of outcomes and a focus on the mechanisms of schooling and the accumulation of impacts over time.

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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 twelfth grade.

	Overall	Student ever attend a		Significant difference
		Public school	Private school	
<b>Child characteristics and experiences during early childhood</b>				
Preschool functioning				
Cognition	460.85 (13.40)	459.25 (13.47)	464.15 (12.66)	***
Literacy	371.18 (20.99)	367.60 (19.99)	378.59 (21.12)	***
Math	427.39 (16.96)	425.61 (17.64)	431.07 (14.85)	***
Working memory	458.47 (17.87)	456.47 (17.92)	462.55 (17.09)	***
Externalizing behavior	51.07 (9.42)	51.67 (8.99)	49.82 (10.14)	*
Internalizing behavior	47.02 (8.83)	47.52 (8.69)	45.99 (9.04)	*
Social skills	98.71 (13.02)	96.91 (12.96)	102.42 (12.37)	***
Race				
White	0.84	0.83	0.88	
Black	0.09	0.10	0.06	*
Other	0.07	0.07	0.06	
Male	0.53	0.53	0.53	
Birth weight (pounds)	7.73 (1.11)	7.74 (1.13)	7.71 (1.06)	
Birth order	1.80 (0.89)	1.84 (0.89)	1.73 (0.88)	
Temperament	3.15 (0.39)	3.17 (0.39)	3.10 (0.39)	**
Proportion of time in center care	0.21 (0.26)	0.19 (0.26)	0.25 (0.26)	**
Proportion of time in maternal care	0.28 (0.26)	0.30 (0.27)	0.22 (0.23)	***
Child care quality	2.95 (0.40)	2.93 (0.41)	2.99 (0.38)	
<b>Family characteristics during early childhood</b>				
Mothers age	29.11 (5.39)	28.33 (5.46)	30.76 (4.88)	***
Psychological adjustment	59.60 (13.54)	58.69 (13.38)	61.48 (13.68)	**
Maternal education	14.69 (2.42)	14.29 (2.42)	15.54 (2.19)	***
Maternal vocabulary	101.70 (18.02)	98.87 (17.60)	107.53 (17.51)	***
Parenting quality	0.11 (0.64)	0.01 (0.68)	0.31 (0.46)	***
Income to needs ratio	3.93 (2.81)	3.35 (2.27)	5.16 (3.39)	***
Proportion employed	0.69 (0.35)	0.66 (0.36)	0.73 (0.34)	*
Maternal depression	8.98 (6.41)	9.60 (6.71)	7.67 (5.50)	***
Proportion two parent household	0.87 (0.28)	0.85 (0.31)	0.92 (0.22)	***
<b>Neighborhood characteristics during early childhood</b>				
Percent of households in poverty	8.86 (8.67)	9.25 (9.08)	8.50 (7.70)	
Percent of single parent households	7.87 (7.34)	8.20 (7.70)	7.16 (6.46)	
Percent of households receiving assistance	5.26 (5.82)	5.53 (6.01)	4.68 (5.34)	
Percent of individuals unemployed	4.61 (4.00)	4.66 (4.04)	4.68 (3.93)	
Percent of adults with less than HS education	18.21 (12.48)	19.16 (12.65)	16.19 (11.90)	**
Percent of adults White	85.71 (17.72)	85.81 (17.43)	85.50 (18.35)	
Sample size	772	524	248	

Notes. Descriptives were generated prior to multiple imputation.

\*\*\*  $p < .001$ . \*\*  $p < .01$ . \*  $p < .05$ .

Table 2.

Age 18 and 26 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 twelfth grade.

	Overall	Student ever attended a		Significant difference
		Public school	Private school	
<b>Age 18 education outcomes</b>				
High school grades	3.26 (.71)	3.21 (.76)	3.35 (.61)	*
Final high school math course	6.53 (1.65)	6.44 (1.63)	6.71 (1.66)	*
None	.02	.02	.03	
General Math	.04	.04	.04	
Pre-algebra	.01	.01	<.01	
Algebra I	.04	.05	.02	

Geometry	.05	.06	.03	
Algebra II	.18	.21	.13	*
Pre-calculus or trigonometry	.38	.36	.43	
Calculus	.28	.26	.33	
Final high school science course	5.03 (1.29)	4.97 (1.27)	5.17 (1.31)	
None	.03	.02	.04	
Basic science	.02	.02	.01	
Physical/earth science	.12	.14	.09	*
Biology I	.11	.13	.08	
Chemistry I	.19	.19	.18	
Physics or Advanced Science	.54	.50	.60	*
Number of honors and AP classes	4.81 (2.19)	4.62 (2.23)	5.20 (2.05)	***
Frequency of failing a class	0.46 (.90)	.56 (.98)	.27 (.68)	***
<b>Age 18 behavioral outcomes</b>				
Number of days skipped in senior year	5.46 (10.00)	6.36 (11.03)	3.67 (7.29)	***
Frequency of suspension/expulsion	0.25 (.69)	.30 (.75)	.16 (.54)	*
Engagement in risky behavior	1.05 (.93)	1.12 (0.92)	0.90 (0.94)	
School attachment	3.25 (0.60)	3.22 (0.60)	3.31 (0.60)	
<b>Age 26 education and employment outcomes</b>				
Total years of education	14.91 (2.12)	14.66 (2.19)	15.43 (1.86)	***
Annual salary (\$)	44,128.62 (32,929.44)	41,352.77 (28,986.81)	50,099.09 (39,533.08)	***
Financial debt (\$)	13,911.99 (31,009.4)	14,941.75 (29,444.52)	11,743.65 (34,032.01)	
Weeks of unemployment	2.34 (6.26)	2.50 (6.46)	1.99 (5.80)	
Job satisfaction	3.61 (1.34)	3.64 (1.34)	3.55 (1.33)	
<b>Age 26 behavioral outcomes</b>				
Ever been arrested	.20 (.40)	.22 (.41)	.16 (.37)	
Engagement in risky behavior	1.11 (0.13)	1.11 (.13)	1.11 (.14)	
Voting in presidential elections (2012 and 2016)	.67 (.42)	.66 (.42)	.69 (.40)	
Volunteering	2.16 (1.09)	2.11 (1.08)	2.29 (1.09)	

Notes. Descriptives were generated prior to multiple imputation.

\*\*  $p < .001$ . \*  $p < .01$ . \*  $p < .05$ .

Table 3.

*Descriptive statistics for private versus public school enrollment.*

	Mean or proportion
Proportion of students enrolled in private school during . . .	
Kindergarten	.25
First grade	.20
Second grade	.18
Third grade	.18
Fourth grade	.19
Fifth grade	.17
Sixth grade	.17
Seventh grade	.16
Eighth grade	.16
Ninth grade	.15
Tenth grade	.16
Eleventh grade	.16
Twelfth grade	.16
Proportion of students who ever attended private school	.32
Number of years students attended private school (full sample)	2.05 (3.78)
Number of years students attended private school (private school only)	6.37 (4.11)

Table 4.

*Bivariate and multivariate results of ever attending private school versus public school education for age 18 and 26 outcomes.*

	Ever attended a private school					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	b (se)	b (se)	b (se)	b (se)	b (se)	b (se)
<b>Age 18 education outcomes</b>						
High school grades	0.23 ** (0.08)	-0.08 (0.08)	-0.07 (0.08)	-0.07 (0.07)	-0.03 (0.09)	-0.02 (0.07)
Final high school science course	0.17 * (0.08)	-0.03 (0.09)	-0.02 (0.09)	-0.03 (0.08)	-0.03 (0.09)	-0.01 (0.08)
Final high school math course	0.20 * (0.08)	-0.04 (0.08)	-0.01 (0.09)	-0.04 (0.09)	-0.02 (0.09)	0.00 (0.08)
Number of honors and AP classes	0.30 *** (0.08)	-0.08 (0.08)	-0.10 (0.08)	-0.10 (0.08)	-0.13 (0.09)	-0.06 (0.07)
Frequency of failing a class	-0.35 *** (0.08)	-0.13 (0.08)	-0.16 (0.08)	-0.14 * (0.07)	-0.12 (0.08)	-0.15 (0.08)
<b>Age 18 behavioral outcomes</b>						
Frequency of skipping school in senior year	-0.28 *** (0.08)	-0.21 * (0.09)	-0.21 * (0.09)	-0.20 ** (0.08)	-0.21 * (0.09)	-0.20 * (0.08)
Frequency of suspension/expulsion	-0.22 ** (0.08)	-0.01 (0.08)	0.01 (0.08)	-0.01 (0.06)	0.00 (0.08)	-0.03 (0.08)
Engagement in risky behavior	-0.11 (0.08)	0.01 (0.08)	0.05 (0.09)	0.03 (0.09)	-0.01 (0.10)	-0.01 (0.08)
School attachment	0.19 * (0.08)	0.03 (0.09)	0.02 (0.09)	0.03 (0.09)	0.06 (0.10)	0.05 (0.08)
<b>Age 26 education and employment outcomes</b>						
Total years of education	0.37 *** (0.08)	-0.04 (0.07)	-0.04 (0.07)	-0.05 (0.07)	-0.05 (0.08)	-0.01 (0.07)
Annual salary	0.27 *** (0.08)	0.11 (0.08)	0.08 (0.09)	0.10 (0.09)	0.09 (0.10)	0.11 (0.08)
Financial debt	-0.10 (0.08)	0.00 (0.08)	0.02 (0.09)	0.01 (0.09)	0.03 (0.10)	-0.03 (0.09)
Weeks of unemployment	-0.09 (0.08)	0.01 (0.08)	-0.01 (0.09)	0.01 (0.08)	0.07 (0.08)	-0.00 (0.08)
Job satisfaction	-0.07 (0.08)	-0.09 (0.09)	-0.11 (0.09)	-0.09 (0.09)	-0.10 (0.10)	-0.08 (0.08)
<b>Age 26 behavioral outcomes</b>						
Ever been arrested	-0.06 (0.03)	-0.01 (0.03)	-0.01 (0.03)	-0.00 (0.03)	0.00 (0.04)	-0.01 (0.03)
Engagement in risky behavior	0.01 (0.08)	0.12 (0.08)	0.14 (0.08)	0.10 (0.08)	0.11 (0.09)	0.08 (0.08)
Voting in 2012 and 2016 elections	0.09 (0.08)	-0.06 (0.08)	-0.06 (0.09)	-0.07 (0.09)	-0.01 (0.10)	-0.03 (0.08)
Volunteering	<b>0.16 *</b> <b>(0.08)</b>	0.13 (0.09)	0.11 (0.09)	0.11 (0.09)	0.12 (0.10)	0.11 (0.08)
<b>Model specification</b>						
Covariates from early childhood		X	X	X	X	X
Covariates from middle childhood and adolescence			X			
Weighted by the number of waves "treatment"				X		
Propensity score matched samples					X	
With imputation for original sample of 1,097						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.

\*\*\*  $p < .001$ . \*\*  $p < .01$ . \*  $p < .05$ .

Table 5.

*Bivariate and multivariate results for years of private schooling versus public school education for age 18 and 26 outcomes.*

	Number of years in a private school					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	b (se)	b (se)	b (se)	b (se)	b (se)	b (se)
<b>Age 18 education outcomes</b>						
High school grades	0.11 ** (0.04)	-0.04 (0.04)	-0.03 (0.04)	-0.04 (0.03)	-0.04 (0.03)	-0.01 (0.03)
Final high school science course	0.08 * (0.04)	-0.01 (0.04)	-0.01 (0.04)	-0.01 (0.04)	-0.01 (0.04)	0.01 (0.04)
Final high school math course	0.09 * (0.04)	-0.02 (0.04)	-0.01 (0.04)	-0.02 (0.04)	-0.01 (0.04)	-0.01 (0.04)
Number of honors and AP classes	0.11 ** (0.04)	-0.06 (0.04)	-0.06 (0.04)	-0.07 * (0.03)	-0.08 * (0.04)	-0.06 (0.03)
Frequency of failing a class	-0.18 *** (0.04)	-0.07 (0.04)	-0.08 * (0.04)	-0.07 * (0.03)	-0.08 * (0.03)	-0.09 ** (0.04)
<b>Age 18 behavioral outcomes</b>						
Frequency of skipping school in senior year	-0.14 *** (0.04)	-0.11 ** (0.04)	-0.10 * (0.04)	-0.11 *** (0.03)	-0.11 ** (0.04)	-0.11 ** (0.04)
Frequency of suspension/expulsion	-0.13 *** (0.04)	-0.03 (0.04)	-0.02 (0.04)	-0.02 (0.03)	-0.02 (0.03)	-0.04 (0.04)
Engagement in risky behavior	-0.06 (0.04)	0.00 (0.04)	0.01 (0.04)	0.01 (0.04)	-0.00 (0.04)	-0.01 (0.04)
School attachment	0.10 ** (0.04)	0.03 (0.04)	0.03 (0.04)	0.03 (0.04)	0.04 (0.04)	0.05 (0.04)
<b>Age 26 education and employment outcomes</b>						
Total years of education	0.18 *** (0.04)	-0.00 (0.03)	0.00 (0.03)	-0.01 (0.03)	-0.00 (0.04)	0.01 (0.03)
Annual salary	0.14 *** (0.04)	0.08 * (0.04)	0.07 (0.04)	0.08 (0.04)	0.08 (0.05)	0.08 * (0.04)
Financial debt	-0.05 (0.04)	-0.02 (0.04)	-0.02 (0.04)	-0.02 (0.03)	-0.01 (0.03)	-0.02 (0.04)
Weeks of unemployment	-0.04 (0.04)	-0.00 (0.04)	-0.01 (0.04)	-0.00 (0.03)	0.01 (0.03)	0.00 (0.04)
Job satisfaction	-0.03 (0.04)	-0.04 (0.04)	-0.04 (0.04)	-0.03 (0.04)	-0.05 (0.04)	-0.04 (0.04)
<b>Age 26 behavioral outcomes</b>						
Ever been arrested	-0.03 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.00 (0.01)	-0.01 (0.02)	-0.01 (0.02)
Engagement in risky behavior	-0.03 (0.04)	0.01 (0.04)	0.01 (0.04)	0.01 (0.03)	0.01 (0.04)	0.01 (0.04)
Voting in 2012 and 2016 elections	0.09 ** (0.04)	0.03 (0.04)	0.04 (0.04)	0.03 (0.04)	0.06 (0.04)	0.03 (0.04)
Volunteering	0.05 (0.04)	0.02 (0.04)	0.01 (0.04)	0.02 (0.04)	0.01 (0.04)	0.02 (0.04)
<b>Model specification</b>						
Covariates from early childhood		X	X	X	X	X
Covariates from middle childhood and adolescence			X			
Weighted by the number of waves “treatment”				X		
Propensity score matched samples					X	
With imputation for original sample of 1,097						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.

\*\*\*  $p < .001$ . \*\*  $p < .01$ . \*  $p < .05$ .