Research Article

Educational Evaluation and Policy Analysis
September 2021, Vol. 43, No. 3, pp. 520–544
DOI: 10.3102/01623737211008919
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Do Students in Gifted Programs Perform Better? Linking Gifted Program Participation to Achievement and Nonachievement Outcomes

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Growing concerns about inequitable access have made public investment in gifted programs controversial in many school districts, yet advocates maintain that gifted services provide necessary enrichment for exceptional students to succeed at school. We provide evidence on whether the typical gifted program indeed benefits elementary students' achievement and nonachievement outcomes, using nationally representative data from the Early Childhood Longitudinal Study, 2010–2011 kindergarten cohort. Leveraging within-school and within-student comparisons, we find that participating in a school's gifted program is associated with reading and mathematics achievement for the average student, although associations are small. We find no evidence of a relationship between gifted participation and student absences, reported engagement with school, or student mobility. Black and low-income students do not see the academic gains that their peers experience when receiving gifted services.

Keywords: *educational policy, gifted education, equity, secondary data analysis, quasi-experimental analysis*

The primary argument for gifted programs in public schools is that many high-ability students are not adequately served in a traditional classroom setting and can benefit from additional enrichment or challenge to spur their academic or creative development (Subotnik et al., 2011). Participation in gifted programs may improve student outcomes through multiple potential mechanisms, including exposure to higher quality instruction, to a talented peer group, or to supplemental curricula or activities (Bui et al., 2014; Card & Giuliano, 2016; Subotnik et al., 2011). Indeed, rigorous studies of gifted programs in single-school districts demonstrate that participation *can* have positive effects on student achievement (Card & Giuliano, 2016).

Whether such positive effects hold *on average* across programs is less clear, however. Minimal centralized funding and a dearth of federal policy governing the identification of gifted students or the provision of gifted services has resulted in significant variation across districts and states (Bhatt, 2011). Furthermore, the delivery of gifted services is quite varied, including pullout, self-contained classes, out-of-school enrichment activities, subject- or grade-based acceleration, and gifted academies. Unfortunately, previous studies examining the effects of different service delivery models on achievement and nonachievement outcomes generally do not apply research designs that allow for causal inference (for reviews, see Assouline et al., 2015; Delcourt et al., 2007; Goldring, 1990; Steenbergen-Hu et al., 2016; Steenbergen-Hu & Moon, 2011; Vaughn et al., 1991). In studies where an experimental design was applied (e.g., Callahan et al., 2015; Gavin et al., 2009; Gubbels et al., 2014), the focus has been on the efficacy of a particular curriculum or enrichment program relative to the counterfactual of participation in a "business as usual" gifted program, not participation relative to nonparticipation. Critically, we do not have rigorous national estimates of the relationship between receiving gifted services at the elementary level and student academic and nonacademic outcomes. In addition, little gifted research has linked the outcomes of gifted students with state-level gifted education policies, such as whether or not a state provides funding for gifted education, mandates identification or services for gifted students, or requires teachers to have training in gifted education (Plucker et al., 2017).

Historically, and into the present, the hypothesized benefits of gifted programs also have been intertwined with the question of which students are given the opportunity to participate in them (Ford & King, 2014). For children whose families already have access to high levels of cultural, social, and economic capital, gifted programs are often characterized as an "accumulation of advantage" (Subotnik et al., 2011, p. 9). In contrast, for high-ability students without that same access—particularly low-income students and students of color—gifted programs may help compensate for what may otherwise be regular classroom settings with lower expectations or weak academic rigor (Card & Giuliano, 2016). Yet, amid structural inequities in American society that create unequal educational opportunities across students from different communities (Reardon, 2011), the students historically identified as gifted have tended to be affluent and White, with recent evidence showing that the most affluent students are 6 times more likely to be identified as gifted than the least affluent students (Grissom et al., 2019). Growing concerns about inequitable access have made public investment in gifted programs controversial in many school districts; as a prominent example, see the School Diversity Advisory Group in New York City (2019) calling for the elimination of the city's gifted programs.

The purpose of this study is to use national data to examine the relationship between gifted program participation and student academic and nonacademic outcomes, such as student engagement in school. We also contribute to discussions on equity in gifted services by testing whether these relationships differ by student race/ethnicity and other characteristics. In addition, given that individual states decide how to identify and support gifted students, we provide descriptive evidence regarding whether the relationship between gifted services and student achievement varies with state-level gifted policies. Specifically, we ask three research questions:

Research Question 1: What is the relationship between gifted program participation in elementary school and student achievement and nonachievement outcomes, including absences, engagement with school, and a student's likelihood of leaving their current school?

Research Question 2: To what extent do relationships between gifted program participation and student achievement differ for students of color and by socioeconomic status (SES) or disability status?

Research Question 3: To what extent do state-level gifted policies moderate the relationship between gifted program participation and student achievement?

We answer these questions with data from the nationally representative Early Childhood Longitudinal Study, 2010–2011 kindergarten cohort (ECLS-K:2011). The ECLS-K:2011 data provide rich student-level information, including whether a student receives gifted services, by year, as students entering kindergarten in the fall of 2010 proceed through fifth grade. In our main analysis, we leverage within-school and within-student comparisons over time to provide estimates of the association between gifted program participation and student achievement in elementary school, which account for numerous potential forms of selection bias. We then extend these analyses to examine relationships by student subpopulation and on nonachievement outcomes. In a final analysis, we merge in data on states' policies governing gifted services when the students entered kindergarten to test whether these policies moderate the association between gifted participation and achievement.

Mixed Evidence on the Effects of Gifted Program Participation on Student Outcomes

Student Achievement

Research on whether gifted programs improve student achievement is surprisingly inconclusive. Early studies on the topic made little attempt to account for underlying differences among participants and nonparticipants in gifted programs, preventing establishment of causal relationships (Assouline et al., 2015; Delcourt et al., 2007; Goldring, 1990; Steenbergen-Hu & Moon, 2011; Vaughn et al., 1991).[1] More recent studies that have applied quasi-experimental research designs to arrive at arguably causal effects have come to mixed conclusions.

The strongest evidence of the effectiveness of gifted programs comes from two studies applying regression discontinuity designs (RDDs). Bui and colleagues (2014) study gifted programming in a large urban district in the southwest. The authors leverage cutoffs in an index that is used in the district to screen fifth-grade students for gifted services in middle school. The index comprises student achievement, a nonverbal ability test, teacher recommendations, and free or reduced-price lunch (FRPL) status. With a sample of roughly 4,000 students, the authors find no evidence that just-identified students outperform comparison students on seventh-grade achievement tests across subjects, despite exposure to peers who are much higher achieving. In an extension, the authors also examine the extent to which eligible students who win the lottery to attend an oversubscribed gifted magnet school have higher test scores than students who were not admitted. Students eligible for gifted services, who attended the magnet gifted school outperformed other gifted students in science but not English language arts or mathematics.

In another study applying an RDD, Card and Giuliano (2016) examine the performance of students identified for gifted/high-achieving classes in a large unnamed district. With a sample of 4,144 fourth-grade students, the authors find that students identified for these gifted classes score three tenths of a standard deviation higher in reading and mathematics than students whose rank was below the cutoff. Notably, no evidence was found that White students scored higher when enrolled in a gifted class; benefits were concentrated among Black and Hispanic students.

Other studies applying quasi-experimental methodologies have also found mixed evidence on the effect of gifted program participation on student achievement. Drawing on data from the 1988–1989 ECLS-K, Adelson and colleagues (2012) examine the effect of participating in gifted programming in fifth grade on student achievement in reading and mathematics, and students' interest in these subjects. Using multilevel modeling with propensity score stratification, the authors find no relationship of gifted program participation with either outcome, although this strategy may not fully have accounted for selection on unobservable factors that may have led some students to be more or less likely to receive gifted services.

In an unpublished manuscript using data from the National Educational Longitudinal Survey, Bhatt (2009) uses an instrumental variables approach to estimate the effect of gifted participation in 8th grade on student test scores in 8th, 10th, and 1th grades. Unfortunately, the instrument employed is weak, raising concerns about precision and bias; taken at face value, the results are mixed, with estimates suggesting positive effects on test scores in 8th grade but not later grades. In a second unpublished manuscript using national data, Murphy (2009) uses data from the original ECLS-K cohort (which began in 1998–1999) to estimate the effect of gifted participation in 1st, 3rd, or 5th grade on test scores. In models controlling for a rich set of student-, teacher-, and school-level covariates, gifted program participation is associated with a .12 *SD* increase in mathematics achievement and a .16 *SD* increase in reading achievement. In models employing student fixed effects, similar to the strategy we employ in the following, the estimate in mathematics decreases to .03 and the estimate in reading decreases to .01 and are no longer statistically significant.

Using longitudinal data on 1,362 students from the Study of Early Child care and Youth Development, Watts and colleagues (2015) examine how educational inputs, including gifted and talented program participation in second through fourth grades, mediate the relationship between early elementary academic and cognitive skills and mathematic achievement at the age of 15 years. Students who participated in a gifted program score .20 *SD*s higher in mathematics than nonparticipants, controlling for a rich set of covariates. Yet, unlike the previously discussed studies, the authors do not account for students' selection into gifted programs in elementary school, likely upwardly biasing their estimates.

Researchers have provided multiple explanations for the mixed evidence on the effects of gifted program participation on student achievement. Students in gifted programs may underperform on standardized assessments compared with their peers not in the gifted program because they miss course material in their general education classroom while engaging in pullout enrichment activities (Murphy, 2009). Participation in gifted education could also improve a variety of domain-specific abilities but not the dimensions measured on commonly used assessments, thereby giving the impression that participating in a gifted program is not effective (Kettler, 2016). In addition, the instruments used to assess student learning may suffer from ceiling effects, a particular concern among gifted students who often come from the top of the achievement distribution (Kell & Wai, 2019; Subotnik et al., 2011). These measurement challenges also suggest that gifted education may be one domain in which null or negative short-term test score outcomes mask positive long-term impacts (Goldhaber & Özek, 2019). The Study of Mathematically Precocious Youth—the longest running longitudinal survey of talented children—suggests that benefits associated with cultivating talented youth may emerge decades later, including advanced degrees in science, technology, engineering, and mathematics; books and scholarly publications; tenure at research universities; and patents (Lubinski et al., 2014).

There may also be differential effects from the varied gifted curricula and service delivery models that are used in practice (Adelson et al., 2012; Callahan et al., 2015), with students benefiting from more rigorous approaches to gifted education but not from others. As national evidence shows that the majority of elementary school gifted programs include 4 hours or less gifted education services a week (Callahan et al., 2013), the educational dose of gifted programs may be too slight to yield positive effects (Makel & Wai, 2016; Wai et al., 2010). Finally, the efficacy of gifted education may depend on state-level factors, such as whether or not a state provides funding for gifted education, mandates identification or services for gifted students, or requires teachers to have training in gifted education. In other words, it may only be the states with policy supports for gifted education where effects on student outcomes are observed (Plucker et al., 2017).

Nonachievement Outcomes

Researchers of gifted and talented education have suggested that the chief benefits of gifted programs in fact may be attitudinal or socioemotional, affecting domains such as engagement, motivation, or self-concept. Gifted programs expose high-ability students to additional rigor than they would receive in their standard curriculum, which might foster intrinsic motivation (Callahan et al., 2015; Delcourt et al., 2007). Programs might also bring social benefits that improve engagement with school, increasing pro-academic peer pressure, and protecting them from the animosity or social exclusion for their high academic performance that they may face in general education classrooms (Rinn et al., 2011). As an example, gifted students participating in an accelerated summer program described how they experienced both a stronger connection to their peers, reduced social stigma surrounding their giftedness, and increased academic motivation (Lee et al., 2015). In other words, this peer support could improve students' self-concept, a hypothesized facilitator of success among gifted students (Hoge & Renzulli, 1993; Marsh et al., 1995).

Unfortunately, few studies examining attitudinal outcomes account for the endogenous sorting of students into gifted programs. In their review of research on the socioemotional benefits of academic acceleration, Cross and colleagues (2015) conclude that acceleration is generally positive for students' socioemotional development but indicate that the lack of rigorous research weakens this conclusion. In one notable exception, Gubbels and colleagues (2014) randomly assigned 66 Dutch elementary students to a pullout enrichment program. The authors find that participating in this enrichment program was linked with improvements in motivation and self-conception, with no differences on an intelligence test. Applying a matched comparison design, Marsh and colleagues (1995) show that two separates studies of 148 students in gifted programs have improved nonacademic self-concept but not academic self-concept. The increased engagement from participating in gifted programs may also help to improve student attendance, although, to our knowledge, no studies have examined attendance as an outcome of participating in a gifted program.

Economists have suggested that from the vantage of school district administrators, an additional benefit of gifted programs might be improved student retention in the district to preserve per pupil funding (Bui et al., 2014; Davis et al., 2013). The strongest evidence for this phenomenon comes from Davis and colleagues' (2013) study of the effect of gifted program participation on retention in the district. Applying a modified RDD to account for manipulation around the gifted program admissions cutoff, the authors show that students just above the gifted admission cutoff were much more likely to remain in the district 2 years after the gifted admission decision.

Potential Heterogeneity in the Effects of Gifted Program Participation

The effectiveness of gifted programs may differ across student subgroups. Gifted programs may provide educational opportunities for talented students who have historically been underrepresented and would thrive once given these new academic supports. Card and Giuliano (2016) describe how enrollment in a self-contained accelerated class exposed Black and Hispanic students to higher teacher expectations than they would experience in a traditional classroom setting. Drawing on the oppositional culture literature (Ford et al., 2008; Fryer & Torelli, 2010; Obgu, 2004), Card and Giuliano (2016) also contend that participating in gifted programs may remove the negative peer pressure faced by Black and Hispanic students.

That said, gifted programs often are not inclusive of the diverse cultures of their students (Ford, 1998; Ford et al., 2008). School systems may promote a narrow, achievement-centered definition of giftedness that fails to encompass a greater diversity of talents (Ford, 1998). The gifted curriculum may also reflect the experiences and backgrounds of the predominantly White and affluent students who have historically filled gifted programs. For instance, in a case study of gifted English language learners (ELLs) in a diverse midwestern school district, Harris et al. (2009) describe how one school's highly regarded gifted program was based primarily around English language arts and was not immediately relevant to ELLs in the program. As a result, the curriculum or climate of gifted programs may not promote the development of students who have been historically underserved by these programs.

The relative effectiveness of gifted programs could also differ for students with disabilities. In one study, gifted students with attention deficit hyperactivity disorder had greater difficulty with working memory but much greater creativity (Fugate et al., 2013). Students with some autism spectrum disorders have been found to demonstrate exceptionally high mathematical abilities (Chiang & Lin, 2007). Individualized educational opportunities that support these twice-exceptional students' cognitive and socioemotional development may prove particularly beneficial (King, 2005; Willard-Holt et al., 2013). Yet Willard-Holt and colleagues' (2013) research indicates that the lack of flexibility in pacing, topics, and assessment has historically led school personnel to inadequately support twice-exceptional students.

Data and Measures

We use data from the ECLS-K:2011. The ECLS-K:2011, administered by the National Center for Educational Statistics (NCES), followed a nationally representative cohort of 18,170 students, who began kindergarten in the 2010–2011 school year, through their elementary years. In the fall and spring of kindergarten and first grade, and in the spring of second, third, fourth, and fifth grades, the ECLS-K:2011 collected rich data on students, including gifted program participation, and their family and schooling settings. We restrict our sample to students in public schools with gifted programs. To combat missing data, which becomes an increasing problem as students progress through elementary school, we employ multiple imputation[2] and weight our estimates using longitudinal student–teacher survey weights to ensure representativeness. The imputed analytic sample includes 37,980 student-year observations.[3]

Achievement and Nonachievement Outcomes

We estimate the relationship between gifted program participation and four outcomes. The first is student achievement. At each wave of data collection, the ECLS-K:2011 administers an assessment to students to measure achievement in reading and mathematics. They then use item response theory (IRT) to create theta scores that account for item difficulty and discrimination (Tourangeau et al., 2019), which partially address "ceiling effects" concerns that arise in a study of gifted student achievement. We use these theta scores, which we standardize across students by year.

We also consider three nonachievement outcomes: teacher-reported student absences, student-reported engagement with school, and student mobility. The total number of absences in a school year is reported by the child's teacher, using the following categories: no absences, one to four absences, five to seven absences, eight to 10 absences, 11 to 19 absences, and 20 or more absences. While student absences is an ordinal variable, we code the midpoint of each category and treat it as continuous in our analysis to accommodate the fixed effects (described in the following). In sensitivity analyses, we also estimate an ordinal logistic regression model. Engagement comes from student surveys. In fourth and fifth grades, students were asked to report on the frequency with which they engage in the following behaviors: do well in school, work hard in class, participate in class discussions, pay attention in class, and listen carefully in class. These variables averaged to create a scale reflecting the student's engagement with school. This scale had moderate internal consistency ($\alpha = .70$). We standardize this measure by year. Student mobility captures whether the student stayed or left their school each year. It is a binary variable set equal to 1 in year t if the student's school identification numbers in years t and $t + 1$ differed. This value can only be captured for kindergarten through fourth grade.

Measuring Gifted Program Participation

Our main independent variable is a student's receipt of gifted services each year. We combine information from multiple sources to create this variable. First, at each survey wave, the student's classroom teacher was asked to report whether or not the child received instruction in a gifted and talented program in (a) reading/language arts, or (b) mathematics. Beginning in third grade, an additional response category was added to the survey to indicate participation in a gifted program with no specific content focus. In addition, for any child with an Individualized Education Program (IEP), the teacher responsible for the educational services outlined in the IEP completed a questionnaire that asked whether the child received gifted services through their IEP, although the subject was not captured. We code a child as *receiving gifted services* either if the classroom teacher reports gifted instruction in any subject or if the IEP-connected teacher specifies that the child is receiving gifted services. This operational definition of gifted program participation most closely aligns with an enrichment model of gifted education, as opposed to other approaches (e.g., acceleration, full-time schools for gifted students, and talent search programs; Subotnik et al., 2011). While enrichment programs do not have as extensive of a research base supporting their use, compared with other gifted service delivery approaches (Assouline et al., 2015; Steenbergen-Hu & Moon, 2011), they remain the most widely used in elementary schools (Callahan et al., 2017). By this measure, approximately 10% of students in our sample receive gifted services in at least 1 year.

In supplemental analyses, we rely on the classroom teacher's response only to separate participation into reading, mathematics, or no specified subject. Among students receiving services, teachers report gifted programming in both reading and mathematics for 54% of student-year observations, reading only for 17%, mathematics only for 14%, and no content focus for 14%. Because this last option was only available for third grade and beyond, this breakout likely underreports the fraction of students receiving gifted services that are not subject-specific.

Other Measures

Our models include a rich set of covariates at the student, teacher, and school levels. Student-level covariates gleaned from ECLS-K:2011 include race/ethnicity (i.e., indicators for whether the student is identified as White, Black, Hispanic, Asian, or another race/ethnicity[4]), gender, a scale measure of SES at entry to elementary school, the child's age in months at the start of kindergarten, disability status, whether English is the primary language spoken at home, and the parent's report of the child's health. The child health measure is a subjective measure rated on a 5-point scale (*excellent* to *poor*). Teacher-level variables include race/ethnicity, years of experience, and indicators for whether or not the teacher has a master's degree, a degree in education, or is certified, respectively. School characteristics include school enrollment size, the fraction of students on FRPL, and indicators of locale type.

We also examine the role of state-level gifted education policies in shaping the achievement of students in gifted programs. We draw on policies collected by the National Association for Gifted Children (NAGC, 2008a, 2008b) and reported by Bhatt (2011). The policies we examine include whether or not the state (a) has a formal definition of giftedness, (b) mandates gifted services, (c) mandates gifted identification, (d) provides guidance on gifted identification, (e) requires general education teachers to have training in gifted education, (f) monitors gifted programs, and (g) funds gifted programs. These policies were collected in the 2008–2009 school year, the most recent iteration of the NAGC State of the States in Gifted Education survey prior to the onset of the ECLS-K:2011. Because some states did not respond to the NAGC gifted policy survey, students in the following states were dropped from this analysis: Alaska, Massachusetts, New Hampshire, New Mexico, North Dakota, Oregon, and West Virginia.

Method

To estimate the relationship between gifted program participation and student outcomes, we implement two approaches: a gains model and a student fixed effect model. The baseline gains model is represented in Equation 1:

$$Y_{ist} = \beta_0 + \beta_1 Gifted_{ist} + \beta_2 Y_{it-1} + \beta_3 X_{ist} + \beta_4 W_{st} + \gamma_t + e_{ist}, \quad (1)$$

where Y_{ist} is the outcome (e.g., mathematics test score) for student i in school s in year t, $Gifted_{ist}$ is student participation in their school's gifted program, Y_{it-1} is prior achievement in reading and mathematics to account for the importance of achievement in gifted identification, X_{ist} represents a vector of characteristics of the student and the responding classroom teacher, W_{st} represents school characteristics, γ_t is a grade fixed effect, and e_{ist} is an error term. When examining nonachievement outcomes, we also control for the lagged outcome, given the strong association of students' prior behavior (e.g., absences, engagement) with their current behavior. In this model, standard errors are clustered at the student level to account for repeated observations of the same child. A positive estimate of β_1 means that the average student's outcome Y is higher in years in which the student receives gifted services than predicted by the prior year's outcome and the other covariates.[5]

We extend Equation 1 in two ways to account for unobserved factors that may influence both the likelihood that a student participates in a gifted program and their outcomes. First, we add a state fixed effect, which will account for state-level policy differences—including differences in gifted identification and services policies—that may affect both the outcome and the main independent variable of interest. Second, we instead add a school fixed effect to account for unobserved, time-invariant school-level factors that may bias the estimate of the association between receiving gifted services and student outcomes, such as the quality of the school's identification processes and gifted programming.

Yet this gains model only partially controls for student- or family-level factors that may impact gifted program participation and outcomes. Student motivation is one example; especially, motivated students may be more likely to be identified as gifted and have better outcomes, on average. Another is parental engagement or motivation; parents who are engaged with their child's schooling and are more motivated to seek their identification for gifted programming may also be more likely to provide other academic and nonacademic supports to their child. To address such potential sources of bias, we leverage, in our preferred specification, the longitudinal nature of ECLS-K:2011 to estimate within-student comparisons with a student fixed effect (ϕ_i):

$$Y_{ist} = \beta_0 + \beta_1 Gifted_{ist} + \beta_2 X_{ist} + \beta_3 W_{st} + \phi_i + \gamma_t + e_{ist}, \quad (2)$$

where X omits lagged test scores and time-invariant characteristics of students. To the extent that student and parent motivation and other such confounders are constant over time, they are removed by this approach. Estimates from Model 2 can still be biased by time-varying sources of endogeneity; for instance, a child's identification as gifted might result from unobserved parental investments in their learning (e.g., through out-of-school enrichment) that also lead to achievement increases. In this case, our estimate of the association between achievement and gifted program participation would be more positive than the true causal effect of the gifted program. We thus interpret β_1 as correlational, albeit a correlation that adjusts for many potential confounders of the causal relationship between gifted participation and student outcomes.

An additional limitation of the student fixed effects model is that differences in outcomes between students who participate in gifted programs in some years but not others drive the estimates, raising potential concerns whether these students differ systematically from students who are observed in gifted programs in every year. Alleviating this concern to some extent, among all students in our sample who ever received gifted services in elementary school, fewer than 5% were observed as always receiving gifted services. Nevertheless, to test for the extent to which this potential nonrandom selection into identification shapes the interpretation of study results, we follow the recommendation of Miller et al. (2019) and (a) compare observable differences in the estimation sample ("switchers") with those students not included in the estimation sample because they were always in a gifted program ("nonswitchers"), and (b) estimate a model using observable student and school characteristics from when the child began kindergarten to predict whether or not a student is a switcher. Overall, this analysis, reported in Supplementary Table A2 in the online version of the journal, indicates that these two groups of students indeed differ. Being a nonswitcher was positively associated with SES, age at entry to kindergarten, test scores, living in a city or suburb, and attending a school with 750 students or more, accounting for other factors in the model.[6] Although these differences do not affect the validity of inferences drawn from the student fixed effect models, they do place bounds on the population to which we can generalize.

To supplement our main analyses, we examine the extent to which the relationship between gifted program participation and student achievement varies by the focus of gifted instruction (i.e., reading, mathematics, or other) by disaggregating the participation variable by subject. We also test for heterogeneity by student characteristics (race/ethnicity, SES, and whether or not the child was ever identified as having a disability) by including interactions between these variables and gifted participation. Finally, we examine the extent to which state-level gifted education policies moderate the association between gifted program participation and student achievement, again by including interactions between gifted program participation and state policies.

Results

Before turning to the main results, we briefly describe differences between students participating in gifted programs in a given year and those who are not. These comparisons are provided in Table 1. Students in gifted programs score 85% of standard deviation higher on standardized tests in reading and mathematics than those not in gifted programs. They are also absent at lower rates; 58% of students not participating in a school's gifted program are absent between 0 and 4 times a year compared with 66% of students in gifted programs. When engagement with school is measured in fourth and fifth grade, the self-reported engagement of students in gifted programs is 18% of a standard deviation unit (SDU) higher than students not receiving gifted services. Students in gifted programs and those who are not are similarly likely to remain in the same school the following year. Consistent with prior research, Black and Hispanic students are underrepresented in gifted programs; students in gifted programs also have much higher SES (.26 SDUs vs. –.20, $p < .001$). Students in gifted programs are more likely to speak English at home, be rated by their parent as having excellent health, and were slightly older when entering kindergarten. Students in gifted programs are most likely to attend a school enrolling 750 students or more, which is located in a city in the southern United States.

Gifted Program Participation and Student Achievement

Next, we turn to the main analysis examining the relationship between gifted program participation and student achievement. The left side of Table 2 shows results for reading achievement, first for the baseline gains model (Column 1), and then for models that add state fixed effects (Column 2) and school fixed effects (Column 3). Estimated associations are very similar across the three columns at roughly .12 *SD*. Column 4 shows our preferred specification with student fixed effects. The estimate is about half the size—.065 *SD*—but still positive and both substantively and statistically significant ($p < .001$). The typical student who ever receives gifted services scores at the 78th percentile in reading in years in which he or she does not receive services, but at the 80th percentile in years of service receipt.

For mathematics achievement, the estimates for gifted program participation from the gains models in Columns 5 to 7 are similar in magnitude to the estimates for reading. In the preferred student fixed effects model (Column 8), however, the estimate is much smaller than for reading, suggesting that students perform only .019 *SD* higher in mathematics in years that they receive gifted services than in other years ($p =$ .08). The typical student who ever receives gifted services scores at the 76th percentile in mathematics years in which he or she does not receive services, but at the 77th percentile in years of service receipt. These results suggest that most mathematics achievement benefits associated with gifted program participation are explained by unobserved fixed student traits.

Table 3 reports the results from several sensitivity analyses to assess the robustness of the estimates from the student fixed effect model. First, a concern with the use of survey data to identify gifted participation is that a teacher may erroneously report that a student did not receive gifted services when in fact they did. This issue is of particular concern with the student fixed effects model, as estimates are based on within-student variation. We identify 150 students who are observed with these gaps in gifted participation—around 11% of students ever in gifted programs—and temporarily drop them from the analytic sample. The estimates for reading and mathematics reported in Column 1 of Panels A and B are slightly larger with this sample restriction and still significant at the same levels as the estimates in Table 2.[7]

Second, students who participate in gifted programs from the onset of elementary school may have a developmental trajectory distinct from students who are identified later in elementary school, the period in which the majority of students are identified for giftedness. To account for this possibility, we exclude kindergarten, thereby restricting the analytic sample to students in first through fifth grades. Results, shown in Column 2 of Panels A and B are similar to the estimates reported in Table 2. Third, the use of probability weights allows to generalize to a national sample of kindergarten students although results can be sensitive to their inclusion. We follow the guidance of Solon et al. (2015) and report the unweighted estimates (Column 3). Results are similar to the weighted estimates. Fourth, while multiple imputation is commonplace in the presence of missing data, its assumptions are often not met in practice (Graham, 2009). To test the extent to which the results are sensitive to the use of multiple imputation, we report the unimputed results (Column 4). Results are again essentially the same as the estimates reported in Table 2.

As an additional sensitivity check, we restrict our analysis to students who switch in and out of gifted education throughout elementary school (Column 5).[8] For reading, estimates are roughly half the magnitude as the main results reported in Table 2 but still statistically significant at the 5% level. For mathematics, estimates are larger than what is reported in Table 3 (.031 vs. .019 *SD*s) and statistically significant at the 5% level.[9] As a final sensitivity check, in Column 6 we estimate a model with student and school fixed effects.

TABLE 1

Comparing Student and School Characteristics by Gifted Program Participation

	Does not participate in gifted program	Participates in gifted program	Difference
Student characteristics			
Mathematics test (standardized)	–0.056	0.805	0.86***
Reading test (standardized)	–0.046	0.800	0.84***
No absences	0.050	0.073	0.02***
1–4 absences	0.530	0.588	0.06***
5–7 absences	0.239	0.205	–0.03***
8–10 absences	0.096	0.084	–0.01*
11–19 absences	0.066	0.039	–0.03***
20+ absences	0.019	0.012	–0.01***
Engagement in school[a]	0.032	0.209	0.18***
Remains in current school	0.806	0.752	–0.06
White child	0.500	0.567	0.07***
Black child	0.138	0.089	–0.05***
Hispanic child	0.263	0.229	–0.04***
Asian child	0.041	0.056	0.01***
Other race/ethnicity child	0.059	0.059	–0.00
Female child	0.487	0.490	0.00
Family SES	–0.197	0.255	0.45***
Has a disability	0.161	0.107	–0.05***
Speaks English at home	0.829	0.861	0.03***
Fair/poor health	0.025	0.020	–0.01[†]
Good health	0.140	0.099	–0.04***
Very good health	0.299	0.284	–0.01
Excellent health	0.536	0.597	0.06***
Age in months at kindergarten entry	66.235	66.915	0.67***
Teacher characteristics			
Female teacher	0.927	0.925	–0.00
Black teacher	0.059	0.053	–0.01
Hispanic teacher	0.105	0.129	0.02***
Asian teacher	0.018	0.015	–0.00
Other race/ethnicity teacher	0.019	0.012	–0.01**
Years of teaching experience (10s)	1.428	1.472	0.04*
Master's degree	0.524	0.540	0.02
No education degree	0.224	0.248	0.02*
Certified	0.923	0.931	0.01
School characteristics			
Suburb	0.346	0.313	–0.03***
City	0.294	0.383	0.08***
Town	0.121	0.087	–0.03***
Rural	0.239	0.217	–0.02*
East	0.147	0.092	–0.05***
Midwest	0.220	0.235	0.01
South	0.382	0.492	0.11***
West	0.251	0.181	–0.07***

(continued)

TABLE 1 (CONTINUED)

	Does not participate in gifted program	Participates in gifted program	Difference
0–149 students	0.012	0.018	0.01**
150–299 students	0.074	0.059	–0.01**
300–499 students	0.322	0.274	–0.05***
500–749 students	0.403	0.374	–0.03**
750+ students	0.190	0.274	0.08***
School fraction FRPL	0.562	0.530	–0.03***
Observations	33,150	3,960	

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates adjusted using probability weights. SES = socioeconomic status; FRPL = free or reduced-price lunch.
[a]Only measured in fourth and fifth grades. Observations = 12,610.
[†]$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

While this model relies on a more limited source of variation that arises from students who switch schools, and in and out of gifted programs, it accounts for time-invariant school-level factors that may bias the estimates. When conditioning on student and school fixed effects, again, the estimates are consistent with the main results for reading but smaller in magnitude and not statistically significant for mathematics.

In summary, the sensitivity analysis provides consistent evidence of a positive relationship between gifted program participation and student achievement in reading, with estimates ranging from 0.036 to 0.077. In mathematics, the estimates range from 0.011 (and not significant) to 0.031 (and significant at the 5% level).[10,11]

Gifted Program Participation and Subgroup Differences in Student Achievement

We next consider a couple of sources of heterogeneity of our main findings. First, in Table 4, we separate the results by subject focus of the gifted program. We separate students into mutually exclusive groups of students who were described as only participating in a gifted program in reading, only a gifted program in mathematics, or all other gifted programs (including students whose teachers indicated that they participated in both reading and mathematics). For reading achievement, estimated associations in the gains models (Columns 1–3) and student fixed effects model (Column 4) are consistently the largest for students in reading-only gifted programs and smallest for students in mathematics-only gifted programs but are similar to the estimates reported in Table 2. For mathematics achievement, we see a similar pattern for the gains models (Columns 5–7), with the estimates largest for students in mathematics-only gifted programs and smallest for students in reading-only gifted programs. The student fixed effects model (Column 8), unintuitively, shows a smaller and statistically insignificant relationship between mathematics-only gifted programs and mathematics achievement. The estimate of participating in a gifted program without a subject focus is .032 *SD*s.

We also test for heterogeneity in the relationship of gifted program participation and student achievement by student subgroup. Table 5 reports the results from the student fixed effect model that includes interactions for student race/ethnicity, SES quartiles, and disability status. Columns 1 and 4 report the results from the interaction between gifted program participation and student race/ethnicity. The net gains of Black students in gifted programs are .177 SDUs lower in reading than those of White students in gifted programs, holding all else constant. Other-race/ethnicity students in gifted programs are predicted to make smaller achievement gains in mathematics than White students. The net gains of Asian students in gifted programs are .109 SDUs higher in mathematics than those of White students in gifted programs.

The most affluent students benefit more from participating in gifted programs than the least

TABLE 2

The Effect of Gifted Program Participation on Student Achievement

	Reading				Mathematics			
	1	2	3	4	5	6	7	8
Gifted program participation	0.116***	0.114***	0.124***	0.065***	0.109***	0.107***	0.117***	0.019†
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lagged reading test (standardized)	0.611***	0.604***	0.561***		0.119***	0.124***	0.136***	
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)	
Lagged mathematics test (standardized)	0.211***	0.215***	0.228***		0.717***	0.709***	0.675***	
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)	
Black child	–0.013	–0.017	–0.003		–0.150***	–0.145***	–0.163***	
	(0.01)	(0.01)	(0.02)		(0.01)	(0.01)	(0.02)	
Hispanic child	0.005	0.010	0.005		–0.053***	–0.056***	–0.058***	
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)	
Asian child	0.012	0.013	0.032†		0.021	0.024†	0.007	
	(0.02)	(0.02)	(0.02)		(0.01)	(0.01)	(0.02)	
Other race child	0.026†	0.039*	0.034†		–0.015	–0.023	–0.030†	
	(0.02)	(0.02)	(0.02)		(0.01)	(0.02)	(0.02)	
Female	0.061***	0.062***	0.064***		–0.072***	–0.072***	–0.080***	
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)	
SES	0.051***	0.051***	0.058***		0.037***	0.039***	0.038***	
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)	
Age in months at kindergarten entry	–0.001	–0.001	–0.000		0.000	–0.000	0.001	
	(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)	
Disability	–0.066***	–0.066***	–0.069***	–0.020	–0.081***	–0.082***	–0.082***	–0.016
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Speak English at home	0.021†	0.022†	0.020	–0.008	–0.033**	–0.031**	–0.020	0.036†
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Good health	0.019	0.024	0.010	0.023	0.029	0.032	0.040	0.039
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)

(continued)

TABLE 2 (CONTINUED)

	Reading				Mathematics			
	1	2	3	4	5	6	7	8
Very good health	0.045	0.049†	0.034	0.025	0.051*	0.054*	0.058*	0.044†
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Excellent health	0.054*	0.058*	0.044	0.041	0.053*	0.055*	0.059*	0.054*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
Female teacher	0.015	0.012	0.007	–0.005	0.007	0.014	0.016	0.010
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Black	0.003	0.011	0.027	–0.016	–0.001	0.014	0.031	0.013
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Latino/a	–0.017	–0.014	–0.006	–0.008	0.005	0.002	0.026†	–0.022†
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Asian	–0.011	–0.014	0.000	0.014	0.030	0.028	0.035	0.014
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Other race	–0.051	–0.030	–0.021	0.033	–0.023	–0.029	–0.026	0.019
	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
Teaching experience (10s)	0.001	–0.001	–0.001	0.002	–0.003	–0.005	–0.004	0.002
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Master's degree	0.007	–0.000	–0.005	0.011	–0.003	0.007	0.004	0.002
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
No education degree	0.003	0.002	–0.002	–0.009	–0.008	–0.009	–0.011	–0.011
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Certified	0.014	0.014	0.014	–0.013	0.012	0.011	0.018	–0.009
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
City	0.013	0.016	0.025	0.010	0.022**	0.006	0.045	0.021
	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)
Town	0.018	0.024†	0.032	0.036	0.005	–0.005	0.044	0.024
	(0.01)	(0.01)	(0.04)	(0.03)	(0.01)	(0.01)	(0.04)	(0.03)
Rural	0.005	0.014	0.031	0.043†	0.014†	0.003	0.034	0.013
	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)

(continued)

TABLE 2 (CONTINUED)

	Reading				Mathematics			
	1	2	3	4	5	6	7	8
Midwest	–0.035**				0.034**			
	(0.01)				(0.01)			
South	–0.025*				–0.006			
	(0.01)				(0.01)			
West	–0.011				0.019†			
	(0.01)				(0.01)			
150–299 students	0.007	0.014	–0.098*	–0.020	–0.021	–0.018	–0.028	0.001
	(0.03)	(0.03)	(0.05)	(0.04)	(0.02)	(0.02)	(0.04)	(0.03)
300–499 students	–0.015	–0.010	–0.083†	–0.026	–0.028	–0.032	–0.008	–0.012
	(0.03)	(0.03)	(0.05)	(0.04)	(0.02)	(0.02)	(0.04)	(0.03)
500–749 students	–0.004	–0.002	–0.099*	–0.026	–0.020	–0.032	–0.024	–0.028
	(0.03)	(0.03)	(0.05)	(0.04)	(0.02)	(0.02)	(0.04)	(0.03)
750+ students	–0.006	–0.008	–0.121*	–0.022	–0.030	–0.044†	–0.052	–0.025
	(0.03)	(0.03)	(0.05)	(0.04)	(0.02)	(0.03)	(0.05)	(0.03)
School fraction FRPL	–0.057***	–0.055**	–0.018	0.039	–0.069***	–0.068***	–0.022	–0.031
	(0.02)	(0.02)	(0.04)	(0.03)	(0.01)	(0.02)	(0.04)	(0.03)
Constant	0.031	–0.012	0.054	0.022	0.089	0.134†	–0.001	–0.016
	(0.08)	(0.08)	(0.09)	(0.06)	(0.07)	(0.07)	(0.09)	(0.05)
Grade fixed effect	x	x	x	x	x	x	x	x
State fixed effect		x				x		
School fixed effect			x				x	
Student fixed effect				x				x
Observations	37,980	37,650	37,980	37,980	37,980	37,650	37,980	37,980

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates adjusted using probability weights. Standard errors clustered at the unit of analysis in parentheses. SES = socioeconomic status; FRPL = free or reduced-price lunch.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

TABLE 3

The Effect of Gifted Program Participation on Student Achievement, Sensitivity Analysis

	1	2	3	4	5	6
	Drop students with gaps in gifted participation	Exclude kindergarten	No survey weights	Unimputed	Only "switchers"	Student and school fixed effect
Panel A. Reading achievement						
Gifted program participation	0.077***	0.047***	0.061***	0.068***	0.036*	0.069***
	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	31,770	31,370	59,090	28,150	7,880	28,150
Panel B. Mathematics achievement						
Gifted program participation	0.024†	0.020†	0.019*	0.022†	0.031*	0.011
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	31,770	31,370	59,090	28,150	7,880	25,990
Student characteristics	x	x	x	x	x	x
School characteristics	x	x	x	x	x	x
Teacher characteristics	x	x	x	x	x	x
Grade fixed effect	x	x	x	x	x	x
School fixed effect						x
Student fixed effect	x	x	x	x	x	x

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates adjusted using probability weights. Standard errors clustered at the student level in parentheses.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

affluent students, at least in reading. Results in column 2 indicate that gifted students in the top quartile of SES have learning gains that are .099 SDUs higher than gifted students in the bottom quartile. Column 5 does not show differences by student SES in math.

Next, we examine disability status. Columns 3 and 6 show no evidence of a differential relationship in reading or mathematics for students who ever had a disability in elementary school compared with those students who were not identified as having a disability.[12,13]

Gifted Program Participation and Nonachievement Outcomes

Next, we consider the extent to which gifted program participation is associated with other student outcomes, including absences, student-reported engagement in school, and mobility from their current school. Overall, Table 6 shows little evidence that gifted participation is related to these nonachievement outcomes. Point estimates are consistently near zero, and none of the estimates in the student fixed effects models are statistically significant, although, contrary to expectation, we do find evidence in the gains models that students in gifted programs may be slightly more likely to change schools.[14,15]

The Moderating Role of State Gifted Policies

We conclude our analysis by considering the extent to which state-level gifted education policies are associated with larger student achievement gains (Table 7). For this analysis, we focus on the baseline ordinary least squares gains model without state, school, or student fixed effects because the policies we examine are time-invariant. This analysis tests the extent to which

TABLE 4

The Effect of Gifted Program Participation on Student Achievement by Gifted Programs in Reading and Mathematics

	Reading				Mathematics			
	1	2	3	4	5	6	7	8
Reading gifted program only	0.124***	0.123***	0.131***	0.075***	0.088***	0.082***	0.092***	–0.001
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Math gifted program only	0.086***	0.087***	0.088***	0.071**	0.137***	0.132***	0.139***	0.009
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Other gifted program	0.118***	0.114***	0.128***	0.058***	0.114***	0.113***	0.124***	0.032*
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Student characteristics	x	x	x	x	x	x	x	x
School characteristics	x	x	x	x	x	x	x	x
Teacher characteristics	x	x	x	x	x	x	x	x
Lagged achievement	x	x	x		x	x	x	
Grade fixed effect	x	x	x	x	x	x	x	x
State fixed effect		x				x		
School fixed effect			x				x	
Student fixed effect				x				x
Observations	37,980	37,650	37,980	37,980	37,980	37,650	37,980	37,980

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates adjusted using probability weights. Standard errors clustered at the student level in parentheses.

$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

TABLE 5

The Effect of Gifted Program Participation on Student Achievement by Student Subgroup

	Reading			Mathematics		
	1	2	3	4	5	6
Gifted program participation	0.092***	0.000	0.062***	0.019	0.001	0.016
	(0.02)	(0.03)	(0.02)	(0.01)	(0.03)	(0.01)
Gifted × Black child	–0.177***			0.021		
	(0.05)			(0.04)		
Gifted × Hispanic child	–0.036			–0.005		
	(0.03)			(0.03)		
Gifted × Asian child	–0.031			0.109**		
	(0.05)			(0.04)		
Gifted × Other-race/ethnicity child	0.004			–0.095*		
	(0.07)			(0.04)		
Gifted × SES quartile 2		0.049			0.009	
		(0.05)			(0.04)	
Gifted × SES quartile 3		0.071			0.021	
		(0.04)			(0.04)	
Gifted × SES quartile 4		0.099*			0.030	
		(0.04)			(0.03)	
Gifted × Ever had disability			0.012			0.010
			(0.03)			(0.02)
Student characteristics	x	x	x	x	x	x
School characteristics	x	x	x	x	x	x
Teacher characteristics	x	x	x	x	x	x
Grade fixed effect	x	x	x	x	x	x
Student fixed effect	x	x	x	x	x	x
Observations	37,980	37,980	37,980	37,980	37,980	37,980

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates adjusted using probability weights. Standard errors clustered at the student level in parentheses. SES = socioeconomic status.
$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

the learning gains of students in gifted programs in a state with a particular gifted policy were greater than students in programs in a state without that policy. A positive and significant coefficient on the interaction between gifted participation and the state gifted policy would indicate that a particular policy was associated with steeper learning gains.

We generally do not find evidence that state policies are associated with differential gains. The coefficients on most interactions are not statistically significant and, when they are, they are negative. For example, students in gifted programs in states with a definition of giftedness make smaller learning gains in reading than students in states without such a policy. Students in gifted programs in states that monitor and provide funding for gifted programs make smaller learning gains in mathematics than students in states without these policies. Yet in each of these cases, results are not consistent for the other subject.

Discussion and Conclusion

The goal of this article was to investigate whether gifted program participation in elementary school is, on average across the United States, positively related to student achievement and nonachievement outcomes, including absences and reported engagement with schooling. Our preferred estimates, which make comparisons within the same student in years with

TABLE 6

The Effect of Gifted Program Participation on Nonacademic Student Outcomes

	Absences				Student engagement in school				Remain in current school			
	1	2	3	4	5	6	7	8	9	10	11	12
Gifted program participation	0.034	0.092	0.036	0.036	–0.008	–0.007	–0.014	0.031	–0.015	–0.019†	–0.010	–0.015
	(0.09)	(0.09)	(0.09)	(0.10)	(0.03)	(0.03)	(0.04)	(0.05)	(0.01)	(0.01)	(0.01)	(0.01)
Student characteristics	x	x	x	x	x	x	x	x	x	x	x	x
School characteristics	x	x	x	x	x	x	x	x	x	x	x	x
Teacher characteristics	x	x	x	x	x	x	x	x	x	x	x	x
Lagged achievement	x	x	x		x	x	x		x	x	x	
Lagged dependent variable	x	x	x		x	x	x		x	x	x	
Grade fixed effect	x	x	x	x	x	x	x	x	x	x	x	x
State fixed effect		x				x				x		
School fixed effect			x				x				x	
Student fixed effect				x				x				x
Observations	31,310	30,990	31,310	37,980	6,240	6,150	6,240	12,600	31,460	31,130	31,460	37,980

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates adjusted using probability weights. Standard errors clustered at the student level in parentheses.
†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

TABLE 7

The Moderating Role of State Gifted Policies

	State definition of gifted		State mandates gifted services		State mandates gifted identification		State provides guidance on identification		State requires general ed teachers to have gifted training		State monitors gifted programs		State funding for gifted programs	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14
	Read	Math	Read	Math	Read	Math	Read	Math	Read	Math	Read	Math	Read	Math
Gifted program participation	0.187*	0.084*	0.120*	0.099*	0.125*	0.095*	0.161*	0.087*	0.121*	0.113*	0.123*	0.133*	0.092*	0.127*
	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)
State gifted policy	–0.018	–0.016	–0.035*	0.039*	–0.037*	0.017*	–0.037*	–0.020	–0.028*	–0.013	0.012	0.004	0.006	0.064*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Gifted × State gifted policy	–0.075†	0.031	–0.002	0.014	–0.010	0.022	–0.044	0.026	–0.017	–0.015	–0.005	–0.034*	0.026	–0.032†
	(0.05)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Observations	35,010	35,010	36,850	36,850	36,850	36,850	34,870	34,870	35,910	35,910	34,850	34,850	28,310	28,310

Note. Author's calculations from the Early Childhood Longitudinal Study, 2011 Kindergarten cohort; National Center for Education Statistics. Estimates from the ordinary least squares gains model. Estimates adjusted using probability weights. Standard errors clustered at the student level in parentheses.

†$p < .10$. *$p < .05$.

and without participation in gifted services, find consistent evidence of a positive—although small—relationship between participation and reading achievement for the typical gifted student, amounting to approximately 2 percentage points in the reading achievement distribution (Kraft, 2020). This association with student learning is smaller than meta-analytic estimates of the academic benefits of various ability-grouping and acceleration strategies (i.e., within-class grouping, cross-grade grouping, and grouping for gifted students), which range from .19 to .70 *SD*s, but it is more comparable to observed differences between accelerated students and same-grade unaccelerated peers (.09 *SD*s; Steenbergen-Hu et al., 2016). We also find evidence of an association in math, although only about one-third as large. These differences in magnitudes are consistent with findings from a national survey that English/language arts are much more likely to be a content focus in elementary gifted programs than is mathematics (Callahan et al., 2017). In contrast, we found no evidence that gifted programs were positively related to the nonachievement outcomes we tested.

How should we interpret these main findings? Advocates for gifted education might point to these results as evidence that gifted services benefit high-ability students' achievement, on average, nationwide. On the contrary, the observed relationships of the typical program are quite small in magnitude, and we do not find evidence that student engagement with schooling and other nonachievement outcomes are related to gifted program participation. Critics of gifted education might reasonably question whether these small impacts are worth the collective investment. To this point, we highlight a key limitation of our analysis, which is that we are unable to differentiate estimates by service delivery model. The ECLS-K:2011 data do not record the type or intensity of the services a student receives. Survey evidence suggests that services provided in many elementary schools may be a relatively "light touch"; close to a quarter of elementary gifted programs center on differentiation or cluster grouping within the general classroom environment and, while half include part-time pull-out classes, their frequency and duration vary (Callahan et al., 2017). The relatively small estimates of the typical gifted program may reflect the fact that the "treatment" many students receive is not sufficiently intensive. In addition, evidence on the average benefits of the typical gifted program mask considerable heterogeneity, given that programs vary so much in their curricular and instructional approaches. We suspect that proponents of gifted education may well conclude that what our results suggest is that investment in gifted services needs to be increased, not decreased, so that gifted students are afforded higher quality, more challenging opportunities by teachers trained in gifted education over more of their school day. Further research that couples designs capable of credible causal inference with systematic collection of information about service delivery to compare the effects of different approaches with gifted education would help shed further light on this issue and offer a great deal to the field more generally (Makel & Wai, 2016).

The relationships we document also should be considered alongside evidence of differences in the relationship between gifted program participation and achievement across student subgroups. Most importantly, our results suggest that Black and low-SES students do not see the increases in reading achievement that their peers participating in gifted programs experience. Gifted programs have faced long-standing criticisms of elitism and that they represent hoarding of opportunities for already advantaged students, criticisms that often are grounded in patterns of underrepresentation in access to gifted programs for marginalized students (Ford, 1998; Grissom et al., 2019; Subotnik et al., 2011). Our findings show that concerns about how gifted education serves Black and low-SES students may extend beyond access. Even among students who gain this access, our results suggest that the benefits of gifted services may not be equally distributed, amplifying questions raised by other scholars about the capacity of the typical gifted program to support and enrich the increasingly diverse students who receive gifted services (Donovan & Cross, 2002; Ford, 1998; Grissom et al., 2019; Grissom & Redding, 2016; Stambaugh & Ford, 2015). Identifying why Black and low-SES students appear not to realize these achievement benefits is an important topic for future research. One explanation relates to the aforementioned study limitations. It could be that resource constraints in the schools Black and low-SES students attend result in limited frequency or duration of gifted services. In the meantime, we hope that this finding might lead practitioners in gifted education to take close looks at their offerings to assess whether they are adequate for serving the needs of high-ability students from historically marginalized student populations.

State policymakers can also take steps to increase the effectiveness of gifted programs in their states, both on average and for diverse student populations. Unfortunately, in our response to recent calls to link gifted education policies to student outcomes (Plucker et al., 2017), we uncover little evidence to guide which steps would be most productive. Across state-level policies—including the presence of a formal definition of giftedness, mandates for gifted services or identification, guidance on gifted identification, required teacher training in gifted education or monitoring of gifted programs, and funding for gifted education—we find no evidence that policies moderate the relationship between gifted program participation and achievement. Here, we suspect that what matters is not the presence of a particular policy but its details, which are idiosyncratic enough across states that they are difficult to study with our research design. For example, two states may both mandate that districts provide gifted services but provide very different guidance on what those services should be or how students should be identified to receive them. We suspect that future research, on which gifted program requirements are most important, may gain further insight by leveraging district-level differences in gifted service delivery with links to student outcomes.

Another fruitful approach might be to rigorously evaluate the effect of various forms of acceleration on student outcomes. Although used sparingly in elementary schools (Callahan et al., 2017), gifted researchers contend that acceleration is an effective and cost-effective way to support the learning needs of exceptionally talented students and deserves additional study (Assouline et al., 2015).

We highlight three other limitations of our analysis. First, our analytic strategy was designed to account for time-invariant, school- and student-level characteristics that could bias the observed relationship between gifted program participation and student outcomes. That said, selection effects may still drive study findings to the extent to which gifted student performance is related to changes in student motivation or parental investments that correspond with their gifted identification and participation in the school's gifted program. Second, while we document evidence of a positive association between gifted education and student achievement, data limitations prevent us from exploring mechanisms. For instance, our supplementary analysis only found suggestive evidence of a moderating relationship between a school's racial composition and gifted program participation on student achievement for Black students. Future research should investigate the relative contributions of other potential mechanisms, such as instructional quality and peer effects, with attention to the possibility that mechanisms may differ by student subgroup. Third, our nonachievement results should not be taken to mean that gifted programs are unrelated to nonachievement outcomes. Measurement issues such as the fact that attendance is captured on an ordinal scale in the ECLS-K:2011 weaken these results. Moreover, researchers have suggested that the chief benefits of gifted education may be attitudinal ones, such as academic self-concept, which we do not measure (Gubbels et al., 2014; Hoge & Renzulli, 1993). We do capture a measure of student engagement, but only in two grades. It is possible that benefits to engagement and other attitudinal measures would be evident with more years of data or with information from older students. Further investigation of such outcomes, especially as they can be linked to fine-grained information about gifted program delivery, would be useful.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

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Notes

1. Among the research on gifted education, a more extensive research base has been conducted on practices related to acceleration (e.g., early admission to kindergarten, grade-skipping, subject-matter acceleration, and dual enrollment) than enrichment (Assouline et al., 2015). Although comprised of case studies and correlational research designs, this research generally shows that accelerated students make greater academic gains when compared with same-age peers (Steenbergen-Hu et al., 2016; Steenbergen-Hu & Moon, 2011) and also have other indicators of preeminence later in life, such as doctorates earned or number of patents (Park et al., 2013). As acceleration is used in less than 2% of school districts (Callahan et al., 2017), we have not included a review of this research.

2. Missing rates for some variables are as high as 20%. Using the multiple imputation suite of commands in Stata, we impute 15 data sets using the multivariate imputation and through chained equations command with a burn in of 100 to account for arbitrary missing data.

3. Sample sizes are rounded to the nearest 10 in accordance with National Center for Educational Statistics (NCES) reporting standards.

4. The *other-race/ethnicity* category includes Native Hawaiian/Pacific Islander, American Indian/Alaska Native, and multiracial students.

5. Estimates from the gains model may be biased under two conditions. First, our estimates may be susceptible to mean reversion to the extent to which students have very high achievement in year t–1, which results in an increased likelihood of gifted assignment. Second, as many students are also in gifted in year t–1, lagged achievement may be endogenous to gifted program participation in more than 1 year. If anything, we would expect these two sources to downwardly bias our estimates, making the main estimates more conservative. In sensitivity analyses, in Supplementary Table A1 in the online version of the journal, we report the estimates when controlling for student achievement at entry to kindergarten rather than in the prior year. Estimates are close to twice as large in magnitude with this specification, ranging from .23 to .27 *SD*s for reading and from .22 to .26 *SD*s for mathematics.

6. In addition to these differences between switchers and nonswitchers, we also examined the extent to which students switched in and out of gifted education. For the majority of students (60%), once they were identified for gifted services, they continued to participate in gifted education for as long as they are observed in the data. The remainder of the students were observed as participating in gifted education in one year and not in at least one subsequent year. In terms of observable student characteristics, students who consistently received gifted services were observationally similar to those students who switched out of gifted education (see Supplementary Table A3 in the online version of the journal). Their schools were more likely to be rural, located in the South, be larger in size, and have a greater relative share of free or reduced-price lunch (FRPL) students.

7. We conduct a slight variation on this sensitivity analysis and get slightly different results. Rather than outright dropping the student, we just drop the survey wave in which they were not consistently participating in the school's gifted program. The estimate for reading is .072 ($p < .001$). The estimate for math is .02 ($p = .101$). Results available upon request.

8. The sensitivity analyses reported in Columns 5 and 6 of Table 3 are on the unimputed sample.

9. Among this group of switchers, we observe two predominant patterns: students who are identified for gifted services and continue to receive gifted education for as long as they are observed in the data; the remainder of the students were observed as receiving gifted education in one year and not in at least one subsequent year. The former group is more common in the data and we suspect also reflects a more typical experience in gifted education (see Supplementary Table A3 in the online version of the journal). For this latter group, switching out of gifted education likely reflects mismeasurement due to survey nonresponse and actual attrition from gifted education (such as when a student switches schools and does not continue to receive gifted services). Given that there may be unobserved differences between these two groups that are ostensibly related to receipt of gifted services and their performance when in a school's gifted program, we estimate the student fixed effect model for these two subsamples (see Supplementary Table A4 in the online version of the journal). For the sample who persisted in gifted education, the estimate for reading is consistent with the student fixed effect estimate reported in Table 2. For math, the estimate is more than twice as large as the corresponding estimate in Table (.050 *SD*s). For students who switch out of gifted education, the estimates are negative but not statistically significant.

10. The student fixed effect model might miss trends in improved academic performance that could be associated with identification for gifted services and achievement. As an additional sensitivity check, we add to the model prior student achievement, despite the fact that the model now fails the strict exogeneity assumption (Wooldridge, 2010). The estimates for reading and mathematics are slightly smaller in magnitude than the main estimates reported in Table 2, but still statistically significant in the case of reading. Results available upon request.

11. In addition to these sensitivity analyses, we also conducted the bounding technique developed by Oster (2019). To quantify the extent to which additional

selection on unobservables would change the estimates under different assumptions of the degree of selection on unobservables (δ), and a realistic maximum R^2 (R_{max}), in Supplementary Table A5 in the online version of the journal, we select different values for each. Oster's analysis of randomized results suggests that $1.3 \times R^2$ provides a reasonable bound to eliminate unstable coefficients. For the within-R^2 from the student fixed effects model, this results in a R_{max} of .005 (Columns 1 and 3). We show that the reading estimates from a student fixed model change little at different values of δ (.062–.069 *SD*s). When increasing R_{max} to .01 in column 2, the range of estimates is larger (.062–.099 *SD*s), suggesting that if observable characteristics in the model and unobservables were equally important in predicting the relationship between gifted participation and reading achievement, the estimates would only increase further from the main estimates. When assuming more overall explained variance in mathematics, the estimate bounds range from .011 to .025 *SD*s.

12. In supplementary analysis, we examine the extent to which disability status in a given year moderates the relationship with student achievement. With no evidence in reading, we find a marginally significant, positive estimate for the relationship between gifted program participation and disability status, suggesting that students with disabilities benefit more from gifted participation in math than students who are not classified as having a disability in a given year (.064, p = .06). Results are available upon request.

13. In supplementary analysis, we also examine the extent to which a school's sociodemographic characteristics moderate these relationships (see Supplementary Table A6 in the online version of the journal). Specifically, we examine the extent to which Black, Hispanic, and students within the different socioeconomic status (SES) quartiles are predicted to have differences in achievement depending on whether they attend a school with above or below average Black, Hispanic, and free or FRPL enrollments, respectively. This analysis suggests that the lower net gains in reading for Black students in gifted programs might be driven by students attending schools with a majority of Black students. Predicted margins from this model, however, are imprecise and do not show statistically significant differences between gifted and nongifted students in the two types of schools. A similar pattern is present in the analysis for Hispanic students and low-SES students. Thus, we find little overall evidence that school demographic composition moderates the observed relationship between gifted program participation and student achievement for Black, Hispanic, and low-SES students although we consider this issue a useful one for future research.

14. In a separate analysis, we estimated Equation 1 using ordered logistic regression but with no evidence of a relationship between gifted program participation and student absences. Results are available upon request.

15. We also examined the possibility of heterogeneous treatment effects on nonachievement outcomes. We found no such evidence, with the exception that gifted program participation appeared to predict lower absences for Asian students (β = –.24, $p < .01$). Estimates are available upon request.

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Manuscript received July 27, 2020
Revision received January 31, 2021
Accepted March 16, 2021