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Does Special Education in Elementary and Middle School Mitigate the Effects of Early Childhood Lead Exposure?

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Abstract

We examine the relationship between childhood lead exposure and special education using data on over 800,000 North Carolina 3rd-8th grade students. We use matching and panel data techniques to estimate the effect of lead exposure on the probability of having a learning disability that qualified students for special education and to estimate the effect of special education on lead-exposed students' academic performance. We find that higher lead exposure significantly increased participation in special education, and special education significantly increased lead-exposed students' test scores. These results indicate that special education can help mitigate academic deficits for lead-exposed students with learning disabilities.

Key Words

Lead exposure, blood lead level, education, academic achievement, children, learning disabilities

JEL Classification

I18, I26, I28, Q53

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Introduction

News outlets and advocacy groups writing about childhood lead exposure tend to repeat a common refrain: the effects of lead are irreversible. A 2016 New York Times article prompted by the Flint, Michigan, water crisis was titled "The Facts About Lead Exposure and Its Irreversible Damage" and stated that, "There is no way of reversing damage done by lead poisoning."[1] UNICEF (2022) called the effects of lead poisoning "impossible to reverse." [2] The American Academy of Pediatrics Council on Environmental Health (2016, 2021) asserted that, "No effective treatments ameliorate the permanent developmental effects of lead toxicity." This perception is consistent with the results of many studies that have found that the cognitive and behavioral damages from early childhood lead exposure persist into adolescence and adulthood (EPA 2013; Reyes 2015; Shadbegian et al. 2019; Grönqvist et al. 2020; Banzhaf and Banzhaf 2023).

Persistence, however, does not necessarily imply permanence. While the effects of lead exposure can be long-lasting, it is possible that they could be mitigated with the right interventions (Schneider 2023). The extent to which the neurocognitive damages from early childhood exposure can be reversed is not well understood (EPA 2013; Schneider 2023). Few studies have examined the effects of secondary interventions—those intended to mitigate the damages from lead exposure after it has occurred—on cognitive and behavioral outcomes. Those few studies have shown mixed results (Bui et al. 2024).

Notwithstanding the lack of empirical evidence on the effectiveness of special education for lead-exposed students, the State of Michigan established as part of a legal settlement a $10 million special education fund for children affected by elevated water lead levels in Flint, Michigan during 2014-2015.[3] Special education enrollment in Flint increased from 13 percent in 2012 to 23 percent in 2022 in the wake of the water crisis (EdWeek 2024).[4] The Individuals with Disabilities Education Act (IDEA) lists lead poisoning as a health impairment that can qualify students for special education, and a majority of US states include exposure to lead or other toxic substances as a qualifying criterion for pre-school aged children to receive early intervention under IDEA (NCES 2024; Bui et al. 2024).

This study tackles two research questions. First, what is the effect of early childhood lead exposure on participation in special education by elementary and middle school students? Second, what effect does special education have on the academic performance of students who were exposed to lead in early childhood?

To address these questions, we rely on a rich panel data set including over 800,000 3rd-8th grade students who were born in North Carolina during 1990-2008, screened for lead exposure before age

[1] New York Times, January 30, 2016 [accessed May 7, 2025]
[2] UNICEF 2022 [accessed May 7, 2025]
[3] Education Week, August 20, 2020 [accessed May 7, 2025]
[4] Education Week, September 26, 2024 [accessed May 7, 2025]. Some authors have argued that the increase in special education in Flint represents a "nocebo" effect that was not associated with elevated blood lead levels but was instead spurred by community concerns anticipating adverse effects (Roy et al. 2023).

six, and attended school in the state. We use covariate matching and school-grade-year fixed effects to estimate the effect of early-childhood lead exposure on the probability that a student had a documented learning disability or other mental or emotional condition qualifying them for special education.

To identify the causal effect of special education on lead-exposed students' math and reading scores, we estimate an extended two-way fixed effects (ETWFE) model that accounts for the staggered timing at which students began to receive services (Wooldridge 2021; 2023). This approach relies on variation from the subset of students who were not enrolled in special education at the beginning of the study period but entered special education in a later grade. We include fixed effects to control for time-variant school and peer effects and time-invariant student characteristics affecting academic performance. Treatment effects are allowed to vary by grade and by the cohort in which students entered special education. We interact the special education treatment variables with early childhood blood lead level (BLL) to estimate whether the effect of special education on test scores varies by lead exposure status.

This analysis makes several unique contributions. We build on previous studies of the association between BLL and learning disabilities by using multiple grades and incorporating covariate matching and school-grade-year fixed effects to isolate the effect of BLL, bolstering a causal interpretation of the findings. Ours is the first study to estimate the effects of special education on cognitive outcomes for school-aged children who were previously exposed to lead. In our analysis of the effect of special education on academic outcomes, our ETWFE approach accounts for the staggered timing of students' entry into special education, again supporting a causal interpretation.

Our results demonstrate that higher lead levels increased participation in special education for learning disabilities in 3^{rd}-8^{th} grades. We find that each unit increase in BLL resulted in a 6.6% increase in the probability of having a learning disability that qualified a student for special education services in each school year. Consistent with past literature, we also find that special education had a positive and statistically significant effect on math and reading scores for students who were initially classified as general education students but later switched status. These effects are largest for cohorts that entered special education in earlier grades, and they increase with the number of years in special education.

The results also indicate that the effectiveness of special education did not vary significantly with the level of early childhood lead exposure. This result implies that higher lead exposure did not dampen the effectiveness of special education, including for students with blood lead levels considered elevated according to current and past CDC thresholds. In other words, students with higher BLLs experienced similar test score improvements as less-exposed students in the same grade and special education entry cohort. A key implication of this finding is that at least some of the adverse neurocognitive effects of early childhood lead exposure can be addressed by educational interventions that do not occur until school age. Furthermore, special education to address lead-exposed students' academic difficulties may be more effective if they are identified and start receiving services in earlier grades.

Background

An extensive literature documents the cognitive and behavioral effects of early childhood lead exposure, which include lower academic achievement in childhood and adolescence (Aizer et al. 2018; EPA 2024a; Gazze et al. 2024; Miranda et al. 2009; Shadbegian et al. 2019).[5] These effects occur even at relatively low levels of exposure (e.g., < 5 micrograms per deciliter (μg/dL)), supporting the scientific consensus that there is no known safe level of lead exposure (EPA 2024a; CDC 2024).[6] While regulations to ban the use of lead in gasoline, paint, food cans, and plumbing in the United States have caused childhood BLLs to drop by 95% since the 1970s, legacy lead paint, dust, and water pipes in older homes, as well as leaded aviation fuel and other environmental exposures still pose a hazard to millions of households (EPA 2024a; EPA 2024b; EPA 2024c). Primary prevention by reducing exposure to lead in homes yields cognitive and health benefits that are estimated to greatly outweigh the costs (Pew 2010; EPA 2024b; EPA 2024c).

The literature on secondary interventions to help children who have been exposed to lead is smaller and has found mixed effects on children's BLL (Nussbaumer-Streit et al. 2020; Bui et al. 2024). None of the three randomized controlled trials of medical, nutritional, or residential interventions that examined the effects on later cognitive and behavior outcomes found statistically significant improvements (Rogan et al. 2001; Bouhouch et al. 2016; Braun et al. 2018). However, three quasi-experimental studies found evidence of improved school outcomes. Young children in New York City with BLL ≥ 4 μg/dL who participated in an early intervention program providing specialized therapies and instruction for preschoolers had significantly higher 3rd grade reading and math scores than students with similar characteristics (based on propensity score matching) who did not participate (Stingone et al. 2022). Children with BLL ≥ 10 who received or were eligible for early childhood interventions through North Carolina's property investigation and remediation program had significantly better academic and behavioral outcomes in elementary and secondary school than children just below the program's eligibility cutoff (Billings and Schnepel 2018; Bui et al. 2025). In addition, toxicological studies on rats have demonstrated that an enriched environment can mitigate learning and memory deficits from lead exposure (Schneider 2023). EPA (2013) identified caregiving quality and exposure duration as factors that could affect the persistence of lead exposure's effects.

We are not aware of any studies examining whether special education or other secondary interventions for *school-aged* children who were exposed to lead in early childhood have improved academic performance. Prior work found that higher early childhood BLLs were significantly associated with learning disabilities that qualified students for special education services (Miranda et al. 2010; Delgado et al. 2018). Miranda et al. (2010) found that North Carolina 4th graders with early childhood BLL ≥ 4 μg/dL were significantly more likely to have learning and behavioral

[5] Lead exposure also results in adverse health for adults, including cognitive, cardiovascular, renal, hematological, reproductive, and developmental damages (EPA 2024a; Hollingsworth and Rudik 2021; Klemick et al. 2022).

[6] CDC has progressively lowered the BLL at which children are recommended to receive interventions, from 60 μg/dL in the 1960s to 10 μg/dL in 1991, 5 μg/dL in 2012, and 3.5 μg/dL in 2021. 3.5 μg/dL represented the 97.5th percentile BLL among children ages 1-5 in the United States in 2015-2018 (Allwood et al. 2022).

problems requiring specialized services compared to students at or below the detectable limit of 1 μg/dL, with odds ratios increasing from 1.16 at a BLL of 4 μg/dL to 1.87 at BLLs ≥ 10 μg/dL. Delgado et al. (2018) found that Florida preschoolers with BLL ≥ 5 μg/dL were significantly more likely to be intellectually disabled (with a risk ratio of 1.58) or developmentally delayed (with a risk ratio of 1.11) than children with BLL < 5 μg/dL. These studies estimated associations that are not necessarily causal effects, and they did not examine the effects of the special education on academic outcomes. One of our analyses extends Miranda et al. (2010) by including grades 3-8 and using covariate matching and school-grade-year fixed effects to isolate the effects of BLL on special education participation.

The Individuals with Disabilities Education Act (IDEA) (1975; 2004) requires that public schools provide a free and appropriate public education to students with disabilities that impede academic performance. Approximately 7.5 million students with disabilities in the United States receive special education or related services annually, comprising roughly 15 percent of public school students (NCES 2024). The most common category of disability qualifying students for special education is termed specific learning disability,[7] followed by speech or language impairment. The third most common category is "other health impairment." IDEA mentions lead poisoning in the list of other health impairments.

Several existing studies have evaluated the effects of special education on the academic performance of students with disabilities. The studies with the strongest causal identification strategies relied on panel data from students who moved into or out of special education and compared academic outcomes before and after a change in status. Hanushek et al. (2002) first employed this approach, estimating the effects of special education on Texas students' 3rd-7th grade math scores in the 1990s, when only 30% of special education students took the standardized tests. Subsequent studies have used similar approaches using data from Kentucky (Hurwitz et al. 2020), New York City (Schwartz et al. 2021), and a national survey (Woods et al. 2023) during the 2000s and 2010s, when testing participation was higher among special education students.[8] Some of these studies included a comparison group consisting of either students who never received special education (Hanushek et al. 2002) or students who always received special education (Schwartz et al. 2021). Hanushek et al. (2002), Hurwitz et al. (2020), and Schwartz et al. (2021) found significant improvements in test scores of roughly 0.1 to 0.2 of a standard deviation per year of special education, though Woods et al. (2023) found null effects. In addition, Ballis and Heath (2021) found that reductions in special education enrollment caused by a one-time policy change in Texas caused significant decreases in high school graduation and college enrollment.

These studies primarily used two-way fixed effects (TWFE) models to estimate the effects of special education on school outcomes while controlling for time invariant student characteristics. Most of the study designs did not account for the staggered timing at which students entered or exited

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[7] Specific learning disability means a difficulty with a specific ability that inhibits learning, rather than a more general intellectual deficit (Wettach 2017).
[8] The 2002 No Child Left Behind Act required 95% of students in all subgroups—including students with disabilities—to participate in end-of-year standardized testing. Prior to this law, participation in testing was low among students with disabilities.

special education.[9] When treatment is staggered and treatment effects vary over time, use of the TWFE model can yield biased estimates of the average treatment effects due to the use of already-treated observations as controls (Goodman-Bacon 2021). Schwartz et al. (2021) also estimated an event-study model and found evidence of cumulative academic gains with more years in special education. This result is important because it demonstrates a key condition in which the bias due to staggered treatment occurs. We rely on a similar panel data estimation approach but use the Extended Two Way Fixed Effects (ETWFE) model to ensure consistent estimates of special education treatment effects given staggered enrollment into special education over time.

Data

Data for this study include individual birth, lead surveillance, and school records for children who were born in North Carolina from 1990 to 2008, were screened for lead exposure before age 6, and attended at least one grade during 3rd-8th grades in North Carolina public schools in academic years 1999-2000 to 2016-2017. The analysis was conducted under an agreement with the Children's Environmental Health Institute (CEHI) at the University of Illinois-Chicago and a protocol approved by the University of Chicago-Illinois Institutional Review Board. CEHI obtained birth data from the North Carolina Department of Health and Human Services, lead surveillance data from the North Carolina Childhood Lead Poisoning Prevention Program (NCCLPP), and education data from the North Carolina Education Research Data Center (NCERDC). We linked data sets using a common identifier created by CEHI based on child name, date of birth, and county.

Birth records include characteristics of the child and parents, including date of birth; birth order; parents' age, educational attainment, and marital status at the time of birth; and mothers' smoking and alcohol use during pregnancy. Lead surveillance data include blood lead measurements in micrograms per deciliter (μg/dL) rounded to the nearest integer; date, child's age, and child's participation in Medicaid during the test; type of blood draw (capillary or venous); and analysis laboratory.[10] Fifty-four percent of children screened for lead during our study period had a single BLL measurement. For the remainder, we follow Aizer et al. (2018) and take the geometric mean of all of a child's BLL values before age 6 as our measure of lead exposure, which we then round to the nearest integer to remain consistent with the level of precision in the data provided by NCCLPP. Children were typically screened for lead before age 3 (78% of children) using a capillary blood draw (88%) that was analyzed by the North Carolina State Laboratory of Public Health (89%).[11] The detection limit for lead in blood in the data is 1 μg/dL.

[9] Ballis and Heath (2021) examined a single, non-staggered policy change and so methods to account for the staggered timing of special education were not needed.
[10] Universal blood lead testing was recommended but not required in North Carolina. Surveillance was targeted to children participating in Medicaid, living in a pre-1978 built home (hence with potential exposure to lead-based paint hazards), living in a zip code designated as high-risk for lead exposure, and other factors recommended by NCCLPP (NC DHHS 2019). Dividing the number of children with linked birth-lead surveillance records by the total number of children in the birth data indicates that 54% of children born in North Carolina during 1990-2008 were screened for lead before age 6.
[11] The North Carolina State Laboratory of Public Health analyzed blood samples using inductively coupled plasma mass spectrometry (NC DHHS 2019).

Education records include students' race or ethnicity, sex, school, grade, economically disadvantaged status,[12] end-of-grade math and reading achievement scores, and test year.[13] There is a high degree of consistency between North Carolina end-of-grade scores and the Measures of Academic Progress (MAP), an assessment commonly used by school systems nationwide (NWEA 2016).

The education data also include indicators for whether each student fell into one or more "exceptionality" categories each year. North Carolina uses the term "exceptional" to refer to children who receive special education services for disabilities or intellectual giftedness.[14] There are several steps to identify and provide services to children who may qualify as exceptional, including a referral from a teacher or parent, an evaluation to identify the student's deficits and strengths, determination of eligibility, development of an individualized education program, and periodic reviews (Wettach 2017). Parental consent is required before a school can evaluate a student for eligibility and provide services (Wettach 2017), which indicates that a student would not be classified in our data has having an exceptionality without parental consent. There are three categories of exceptionalities in our data: mental or emotional disability; physical disability; and advanced or intellectually gifted. Similar to nationwide trends, the most common exceptionality is specific learning disability, affecting 7% of students overall and 61% of students with a mental or emotional exceptionality.[15]

Given the robust evidence on the causal link between lead exposure and cognitive and behavioral effects, we focus on the first category—students with a mental or emotional exceptionality. We use the shorthand "learning disability" to refer to the mental or emotional exceptionality category for the remainder of the paper. We dropped all students who ever had a *physical* disability from our analysis. While lead exposure can cause hearing loss, kidney damage, and other physical health problems (EPA 2024a), prior research with a similar data set on North Carolina 4th graders found that lead exposure was largely not associated with physical disabilities (Miranda et al. 2010), and we wanted to avoid any confounding associated with physical disabilities and learning. To maintain representativeness of the broader student population, we retain students with an advanced or intellectually gifted exceptionality in our analysis but group them together with general education students (i.e., students who had no exceptionality during the study period).

We lack data on the specific educational interventions received by students with an exceptionality. IDEA requires that schools provide individualized services and support for students with disabilities to allow them to receive an appropriate education. Consistent with the definition of exceptionality,

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[12] Economically disadvantaged students are indicated by a binary variable denoting participation in the National Lunch Program.

[13] The North Carolina end-of-grade reading test is aligned to the state's English Language Arts curriculum (North Carolina Department of Public Instruction, accessed September 5, 2025).

[14] North Carolina Department of Instruction [accessed September 5, 2025]

[15] Other exceptionalities in the mental/emotional category include autism, emotional disability, intellectual disability, and traumatic brain injury. Students can have up to three exceptionalities recorded in the data, but most have a single exceptionality listed. Data about the specific condition are missing for fourteen percent of students categorized as having a mental/emotional exceptionality.

our understanding is that all students classified as having a mental or emotional exceptionality in a given year received special education in that year. Our indicator for "received special education" is therefore equal to our "learning disability" variable.

For our first analysis, which examines the effect of lead exposure on a student's likelihood of having a learning disability, we start with all students with linked birth, lead surveillance, and school records, which included 4,183,527 student-year observations from 915,211 unique students. Excluding all students who were ever classified as having a physical disability resulted in dropping about 10% of the sample (N = 405,063 student-year observations). We drop student-year observations with missing or miscoded values for student sex, Medicaid participation, and blood lead level (N = 34 observations). We then drop student-year observations from individuals with a rounded geometric mean early childhood BLL above 9 μg/dL (N = 86,062) because these students may have received early childhood residential and nutritional interventions targeted towards children with elevated BLLs.[16] This restriction does not limit meaningfully the policy relevance of our analysis because children with BLL ≥ 10 μg/dL have become increasingly rare in the U.S. population in recent decades.[17] We also drop all observations from students who repeated two or more grades during 3rd-8th (N = 12,372), and singletons that are the only remaining student-year observation in their school, grade and year (N = 1,648). Our final sample for the first analysis is an unbalanced panel including 3,678,448 student-year observations from 810,138 unique students.

For our second analysis, which estimates the effect of special education on reading and math scores for students with learning disabilities and varying levels of early childhood lead exposure, we further restrict the sample. First, we exclude all observations from students who were already receiving special education during their first year in the data set (typically 3rd grade) because they were already "treated" when entering the study (N = 279,175 student-year observations).

For students who entered special education in a later grade but then exited by 8th grade, we drop all observations from years after the student stopped receiving special education (N = 3,020). Prior research on special education has shown that the effects of entering and exiting special education are not symmetric; entering special education yields substantial improvements in test scores, while exiting results in much smaller, often statistically insignificant decreases in scores because the benefits of prior special education can persist into future grades (Hanushek et al. 2002; Schwartz et al. 2021). We are most interested in estimating the effect of entering special education, which best represents the benefits that accrue to participating students.

We also exclude observations that represent a repeated grade; i.e., when a student completed a given grade more than once, we dropped the observation corresponding to the second year in that

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[16] At the beginning of the study period, 15 μg/dL was the BLL that qualified a child for referral to secondary (non-educational) interventions in North Carolina, which was lowered to 10 μg/dL in late 1999. It was lowered to 5 μg/dL in 2017, after our study period.

[17] The 97.5th percentile BLL among children age 1-5 years was 3.5 μg/dL in 2015-2018 (CDC 2024), and BLLs ≥ 10 μg/dL represented 0.3% of children screened for BLL in 2021, the most recent year for which CDC (2025) reported data.

grade (N = 31,947).[18] Special education students were more likely to repeat a grade than general education students in our sample: 13% of observations from students who entered special education and 5% of observations from students who were never in special education repeated a grade at some point in our data. If grade retention improves test scores, we could inappropriately attribute the test score gains to special education if we did not drop repeated grades from our analysis.

The second analysis sample also excludes student-year observations with missing test scores (N = 7,219). Test scores are missing for <1% of the full sample and 4% of the observations from students who entered special education.[19] We convert math and reading test scores to percentiles (and to z-scores for a sensitivity analysis) based on the statewide distributions for each grade and subject (i.e., math or reading) each year. We also drop singleton student observations (i.e., students who only appear for one grade in the data set) since this analysis relies on within-student variation for identification. Last, we drop observations from students with no pre-treatment test scores remaining after these other deletions (N = 77,752). The final data set for the second analysis includes 3,279,335 student-year observations from 662,014 unique students. Three percent of this sample represents students who entered special education for a learning disability between 4th and 8th grade, and the remainder were general education students (including advanced or intellectually gifted students) in all years.

Empirical Approach

Our first analysis examines the effect of early childhood lead exposure on the likelihood of having a learning disability (i.e., a mental or emotional exceptionality) qualifying the student for special education. Equation (1) describes this relationship.

$$Z_{isgt} = X_i\alpha_1 + \alpha_2 BLL_i + \gamma_{sgt} + \varepsilon_{isgt} \quad (1)$$

Z_{isgt} is a dichotomous variable indicating whether student *i* attending school *s* received special education for a learning disability during grade *g* in year *t*. X_i is a vector of dummy variables describing student and parental characteristics. Student characteristics include sex, race or ethnicity, birth month, and birth order relative to any siblings. Student socioeconomic status is reflected by three variables: participation in Medicaid at the time of the BLL test, classification as economically disadvantaged in at least one year in the education data set, and classification as

[18] We conduct a sensitivity analysis in which all students who ever repeated a grade are excluded. The results are similar to those from the primary analysis.

[19] Learning disabled students who do not follow the standard curriculum under their individualized education program may be exempt from standard end-of-year testing (Wettach 2017), resulting in a higher rate of missing test scores compared to general education students. Missing test scores among special education students is a potential source of bias in our analysis, but the observed 96% of non-missing scores in our data is a historically high rate of participation in testing among students with learning disabilities (Schwartz et al. 2021).

economically disadvantaged in every year in the education data set. Students who were classified as economically disadvantaged in every year were likely to be more severely disadvantaged than those who were economically disadvantaged in only some years. Parental characteristics include the mother's and father's education levels and ages at the time of the student's birth, mother's marital status at the time of the student's birth, and maternal smoking and alcohol use during pregnancy.

BLL_i represents student *i*'s early childhood blood lead level. We use two different specifications for the relationship between lead exposure and special education participation. In some of our models, we use a flexible functional form in which BLL_i is a vector of eight dummy variables representing whether student *i*'s early childhood rounded geometric mean BLL equaled an integer ranging from 2 to 9 μg/dL. The omitted category is BLL ≤ 1 μg/dL, meaning that BLL was at or below the detectable limit.

We also consider a specification in which BLL_i is a single scalar variable equal to an integer from 1 to 9. The specification in which BLL_i enters as a scalar imposes a linear functional form on the relationship between blood lead and the probability of having a learning disability, so we term this specification the linear model when reporting our results.

γ_{sgt} is a vector of high-dimensional fixed effects for school *s* and grade *g* during year *t*, which flexibly control for any unobserved factors that contribute to variation in average special education participation rates. Such factors could be associated with school quality, peer effects, local time trends, and other factors that affected all students in the same grade and school in a particular time period. ε_{isgt} is a random error term. We cluster standard errors by student.

The α terms are coefficients to be estimated. The key coefficient of interest is α_2, the effect of BLL on special education participation. We estimate (1) using a linear probability model. The linear probability model provides a reasonable approximation when the objective is to estimate the partial effects of the explanatory variables on the probability of an event (Wooldridge 2010). This model allows us to interpret α_2 as the percentage point change in a student's probability of special education participation in a single year from a given BLL, or, in the case of the linear BLL model, from a one-unit change in BLL. The linear probability model can estimate the parameters of both continuous and dichotomous variables, allowing us to include high-dimensional fixed effects (Angrist and Pischke 2009). We also estimate the BLL dummy variable model specification using a fixed effects logit estimator.

To better isolate the causal effects of lead exposure on special education participation, we estimate a version of equation (1) using an analytical sample constructed by coarsened exact matching (CEM) (Iacus et al. 2012). CEM allows us to construct weighted samples that are perfectly balanced across the treatment and control groups in terms of selected observable characteristics. In this analysis, we define the treatment group for the purposes of deriving CEM weights as students with BLL > 1 μg/dL and the control group as students with BLL ≤ 1 μg/dL. The covariates we use for matching are race/ethnicity, sex, economic disadvantage in at least one year, economic disadvantage in every year, participation in Medicaid, mother's education, mother's age, grade, year, and county of residence during the school year.[20] The CEM algorithm drops observations in the treatment group that do not have an exact match in the control group and vice versa, and derives weights that simultaneously equate the mean values of these characteristics across the treatment and control groups in the matched sample.

Our second analysis focuses on end-of-grade math and reading scores as the outcomes of interest. We include student fixed effects as a key identification strategy to isolate the effects of special education participation on test scores and to estimate whether this effect varies with lead exposure. As noted, the identifying variation in this analysis comes from students who were not initially in special education but then started receiving services during our study period. While not our preferred model, we first present a traditional two-way fixed effects (TWFE) model allowing treatment effects to vary with lead exposure, as shown in equation (2a).

$$Y_{isgt} = \beta_1 Z_{isgt} + \beta_2 Z_{isgt} * BLL_i + F_i + \delta_{sgt} + \omega_{isgt} \quad (2a)$$

Y_{isgt} is a continuous variable representing the test score percentile for student *i* at school *s* in grade *g* during year *t*, for either math or reading. As in (1), Z_{isgt} indicates whether the student received special education for a learning disability in year *t*, but it is the "treatment" in this model. We interact Z_{isgt} with BLL_i to allow the effect of special education to vary with early childhood lead exposure. In this analysis, we only use the linear BLL specification in which BLL_i is a scalar equal to the student's rounded geometric mean BLL, which limits the number of coefficients to be estimated.

F_i is a vector of student fixed effects representing all time-invariant, student-specific factors that affect academic performance. The X_i vector of student and parental characteristics in equation (1) does not appear in equation (2a) because these characteristics are time invariant and are subsumed by the student fixed effects. We exclude BLL_i (except when interacted with Z_{isgt}) for the same reason; the level of early childhood lead exposure does not change as a child progresses through grade school and is thus absorbed by F_i. δ_{sgt} is a vector of school-grade-year fixed effects, reflecting the mean reading or math score in each school, grade, and year, conditional on the other observed characteristics. ω_{isgt} is a random error term clustered by student.

β_1 and β_2 are the coefficients of interest. β_1 represents the effect of special education on math or reading test scores. β_2 reflects the degree to which the effects of special education on test scores vary with early childhood lead exposure.

The TWFE model represented by equation (2a) can yield results biased towards zero when used to estimate the effect of a treatment that is introduced at different points in time (Goodman-Bacon 2021). In this situation, the TWFE estimate of β_1 is a weighted average of all simple difference-in-difference comparisons contained within the data, including comparisons between just-treated observations and already-treated observations, erroneously using already-treated units as controls (Goodman-Bacon 2021). This type of comparison is problematic in the presence of dynamic

[20] Race/ethnicity is coarsened into three bins (White; Black; Hispanic, Asian, or other race). Mother's education is coarsened into four categories (less than high school, high school graduate, some college, and college graduate). Mother's age is coarsened into three categories (under 21, 21-29, and 30 or more years).

treatment effects, underestimating the effect of a treatment that yields larger benefits the longer an individual participates. This concern is relevant for our study because there is no reason to suspect that the effects of special education are constant over time. The effects of receiving special education in a given year can persist into later grades, resulting in larger treatment effects the greater the number of years a student participates (Schwartz et al. 2021).

To obtain an unbiased estimate of the effects of special education on test scores for children with varying levels of lead exposure, we use Wooldridge's (2025)extended two-way fixed effects (ETWFE) model. Similar to approaches proposed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021), the ETWFE model avoids using already-treated units as controls. The ETWFE model is also more efficient than these alternatives when not-yet-treated observations are used as controls (as in our analysis), and it allows for heterogeneity in treatment effects based on other characteristics besides time (Wooldridge 2025). Instead of estimating a single average treatment effect and a single treatment-BLL interaction term, the ETWFE model estimates a set of separate average treatment effects for each grade and cohort. It also allows us to estimate a corresponding set of parameters associated with the BLL interaction term $Y_{isgt} * BLL_i$, which capture any moderating effects of special education on the relationship between early childhood lead exposure and later test scores. We use the term "cohort" to describe a group of students that started special education in the same grade (though not necessarily in the same year).[21] These separate treatment effects can then be aggregated into summary measures to calculate average treatment effects. Equation (2b) describes the ETWFE model that we estimate.

$$Y_{isgt} = \beta_1 Z_{isgt} * c_i * G_g + \beta_2 Z_{isgt} * BLL_i * c_i * G_g + \beta_3 BLL_i * G_g + F_i + \delta_{sgt} + \omega_{isgt} \quad (2b)$$

Equation (2b) is similar to (2a) but introduces c_i, a vector of dummy variables indicating the student's cohort, i.e., the grade in which they first entered special education. It also includes G_g, a vector of dummy variables indicating the grade in which the student took the math or reading test. We interact the cohort and grade dummy variables with the treatment and treatment * BLL variables. We also interact BLL with grade to allow the effect of lead exposure on academic performance to vary as students progress through school. A previous study by Shadbegian et al. (2019) using a similar dataset found that the adverse effects of lead on school performance did not attenuate at higher grades, but we wanted to allow for this possibility in the model. We do not include BLL alone or BLL interacted with cohort dummies in equation (2b) because these variables are subsumed by the student fixed effects F_i.

In equation (2b), β_1 is now a vector of coefficients representing the treatment effects for each cohort in each grade once that cohort has entered special education. For example, it includes the treatment effects for the 4th grade cohort in 4th grade, the 4th grade cohort in 5th grade, the 4th grade cohort in 6th grade, and so on. We hypothesize that $\beta_1 > 0$, which would mean that special education significantly improved test scores for the average student with learning disabilities in our sample.

[21] We used grade rather than calendar year to define cohorts. Grade and year are perfectly correlated for each student in our sample. This approach is similar to Marcus's (2023) use of age rather than year to define cohorts for estimating the effects of replacing lead water pipes on children's BLL.

This hypothesis is consistent with several past studies using panel data techniques to estimate the effect of special education on academic performance.

β_2 is now a set of coefficients that reflect any heterogeneity in the effects of special education across students with different levels of early childhood lead exposure for the 4th grade cohort in 4th grade, the 4th grade cohort in 5th grade, etc. A finding of $\beta_2 < 0$ would indicate that special education is less effective for students with higher lead exposure. $\beta_2 = 0$ indicates equal effectiveness of special education regardless of lead exposure. $\beta_2 > 0$ indicates greater effectiveness of special education at higher lead exposures. The net effect of special education for student *i* is given by $\beta_1 + BLL_i * \beta_2$. If this expression equals 0 for students with $BLL_i \geq 1$ µg/dL, this indicates that special education yields no improvement in test scores for students with detectable lead levels, which would be expected if the adverse effects of lead are irreversible.

The vector of coefficients β_3 captures how the effects of early childhood lead exposure on school performance vary across grades. As in equation (1), we cluster the standard errors at the student level.

The ETWFE model estimates unbiased treatment effects if conditional parallel trends hold across the treatment and control groups (Wooldridge 2025). There is uncertainty about the counterfactual trend in test scores for special education students since it is unobserved. Therefore, we use two different approaches to construct the control group. In the first approach, we exclude all never treated (i.e., general education) students. We only include students who entered special education during grades 4 through 8 and had reading and math test scores both before and after entering special education. The control group consists of observations from students who had not yet entered special education, and the treated group includes observations from students in the same grade who had already started special education. For example, to estimate the effectiveness of special education in 4th grade, we compare the change in test scores from 3rd to 4th grade for treated students (who started special education in 4th grade) to those of control students (who started special education in 5th, 6th, 7th, or 8th grades). We exclude students' 8th grade test scores from this sample because there were no remaining untreated observations by 8th grade to serve as controls.

The not-yet-treated control group is appropriate for estimating the causal effects of special education because students who entered special education in a later year are likely to have similar academic challenges and other characteristics as those who entered in an earlier grade. The ideal control group in a quasi-experimental analysis has the same characteristics as the treatment group except for receipt of the treatment. This approach is similar to Schwartz et al.'s (2021) analysis of the effects of special education, which excluded never-treated students due to concerns about their appropriateness as a control group. However, Schwartz et al. included always-treated and already-treated units in their analysis, which could have biased their estimates of special education effectiveness in the presence of dynamic treatment effects.

Our second approach for estimating (2b) adds never-treated students to the control group–that is, students who did not receive special education at any point during 3rd-8th grades. These students outnumber treated students in our sample by an order of magnitude (see Table 3). While never-treated students may diverge from treated-students in terms of learning challenges and other characteristics related to educational outcomes, it could still be of interest to understand how special education affects the performance of treated students relative to the broader never-treated group of students. This approach is similar to Hanushek et al.'s (2002) analysis, which included both treated and never-treated students. In the next section, we examine pre-treatments trends among the not-yet-treated and never-treated groups as an indicator of whether parallel trends would have been likely across the different groups in the absence of treatment.

Results

Table 1 presents select student-year summary statistics for the sample used in the first analysis, which estimates the effect of lead exposure on the probability of students having a learning disability qualifying them for special education. We compare the group who had a BLL at or below the detectable limit (BLL ≤ 1 μg/dL) to the higher-BLL group (BLL > 1 μg/dL). The columns on the left show unweighted means for the full sample, while the columns on the right use the matched sample and weights derived from coarsened exact matching (CEM). In the full, unweighted sample, the BLL ≤ 1 μg/dL group has a lower percentage of students with a learning disability, and reading and math scores are about seven percentile points higher, on average, than the BLL > 1 μg/dL group. The BLL ≤ 1 μg/dL group has a higher percentage of students who were female, White or Hispanic, not economically disadvantaged, and had a mother who graduated from college than the BLL > 1 μg/dL group. These differences are all statistically significant. In the CEM-weighted sample, the BLL ≤ 1 μg/dL group still has a significantly lower percentage of students with a learning disability and significantly higher math and readings scores, but these differences are less pronounced. Other student characteristics are evenly balanced across the two groups in the matched sample. Appendix Table A1 provides summary statistics for a larger set of variables, and Table A2 provides select summary statistics for the full sample disaggregated by special education status rather than BLL.

Table 1. Select student-year summary statistics by BLL for learning disability analysis sample

	(1) BLL ≤ 1 μg/dL	(2) BLL > 1 μg/dL	p	(3) BLL ≤ 1 μg/dL, CEM	(4) BLL > 1 μg/dL, CEM	p
BLL	1.00 (0.00)	3.80 (1.75)	< 0.001	1.00 (0.00)	3.43 (1.58)	< 0.001
Learning Disability	0.07 (0.26)	0.09 (0.29)	< 0.001	0.07 (0.26)	0.08 (0.28)	< 0.001
Math Score Percentile	52.01 (28.14)	45.23 (27.72)	< 0.001	50.44 (28.70)	49.31 (28.83)	< 0.001
Reading Score Percentile	52.05 (28.16)	45.30 (27.84)	< 0.001	50.21 (28.68)	48.79 (28.95)	< 0.001
Male	0.47 (0.50)	0.49 (0.50)	< 0.001	0.48 (0.50)	0.48 (0.50)	1
White	0.56 (0.50)	0.46 (0.50)	< 0.001	0.52 (0.50)	0.52 (0.50)	1
Black	0.20 (0.40)	0.36 (0.48)	< 0.001	0.30 (0.46)	0.30 (0.46)	1
Hispanic	0.17 (0.37)	0.11 (0.31)	< 0.001	0.14 (0.35)	0.14 (0.34)	< 0.001
Other Race	0.06 (0.25)	0.06 (0.24)	0.004	0.04 (0.20)	0.04 (0.21)	< 0.001
Economically Disadvantaged (all years)	0.36 (0.48)	0.46 (0.50)	< 0.001	0.44 (0.50)	0.44 (0.50)	1
Mother Graduated College	0.23 (0.42)	0.13 (0.33)	< 0.001	0.21 (0.41)	0.21 (0.41)	1
Observations	500,089	3,178,359		390,955	1,126,013	
Unique Students	133,969	676,169		109,813	300,299	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Appendix A1 provides summary statistics for a larger set of variables.

Table 2 shows the key results from the regressions estimating the effects of early childhood lead exposure on the probability of having a learning disability. The results across all models show that students with higher BLLs were significantly more likely to have a learning disability each year. The models that include BLL as a series of dummy variables (columns 1-2) show that even BLLs below CDC's current blood lead reference value of 3.5 μg/dL had a statistically significant effect on the probability of having a learning disability. In the CEM sample results using BLL dummy variables (column 2), the point estimates for higher BLLs above 5 µg/dL, which are less common in the data set than lower BLLs, are not always monotonically increasing, but the relationship between lead exposure and learning disabilities is positive across a wide range of BLLs. In addition, we cannot reject that the relationship between BLL and learning disabilities is linear when using the CEM sample and weights. The coefficient from the CEM linear model (column 3) indicates that the probability of having a learning disability in a given year increases by 0.46 percentage points for every 1-µg/dL increase in BLL above 1 µg/dL during early childhood. Given that the probability of having a learning disability among all student-year observations in the BLL ≤ 1 µg/dL group was 7%, this effect is equivalent to a 6.6% increase in the probability of having a learning disability in each year per unit increase in BLL.[22] Figure 1 displays these results graphically.

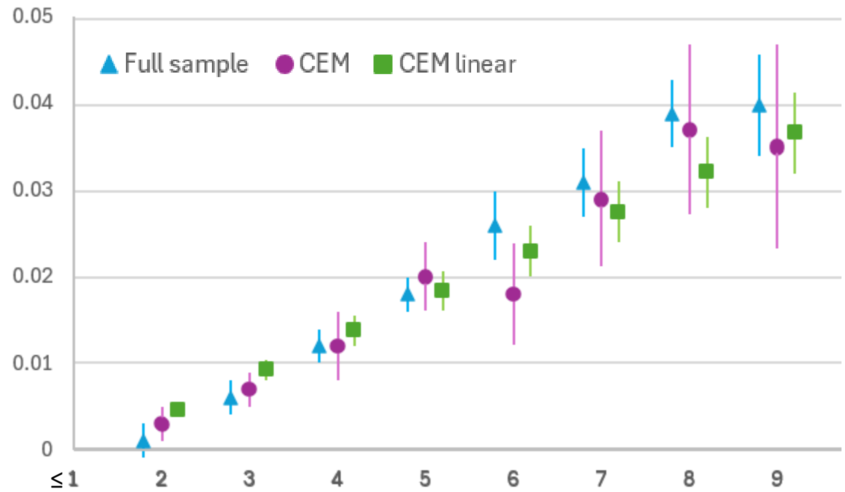
Table 2. Full results: Linear probability model of probability of having a learning disability

	(1) Full sample	(2) CEM	(3) CEM linear
Blood lead level			
2 µg/dL	0.001	0.003**	
	(0.001)	(0.001)	
3 µg/dL	0.006***	0.007***	
	(0.001)	(0.001)	
4 µg/dL	0.012***	0.012***	
	(0.001)	(0.002)	
5 µg/dL	0.018***	0.020***	
	(0.001)	(0.002)	
6 µg/dL	0.026***	0.018***	
	(0.002)	(0.003)	
7 µg/dL	0.031***	0.029***	
	(0.002)	(0.004)	
8 µg/dL	0.039***	0.037***	
	(0.002)	(0.005)	
9 µg/dL	0.040***	0.035***	
	(0.003)	(0.006)	
BLL (continuous)			0.0046***
			(0.0003)
Observations	3,678,448	1,510,951	1,510,951
R^2	0.077	0.112	0.112

[22] The coefficient estimate of 0.0046 (Table 2, column 3) divided by 0.07 (Table 1, column 3) equals 0.066, or 6.6%.

All specifications include school-grade-year fixed effects. Standard errors clustered at student level.
* p<0.10, ** p<0.05, *** p<0.01

Figure 1. Estimated effect of BLL on probability of having a learning disability (linear probability model estimates)



The markers depict coefficients reported in Table 2. Blue triangles correspond to the full sample dummy variable model (column 1), purple circles correspond to the CEM dummy variable model (column 2), and green squares correspond to the CEM linear model (column 3). Whisker bars represent 95% confidence intervals for each coefficient estimate. Standard errors are clustered at the student level.

Appendix Table A3 presents the full set of coefficients for these regressions. Appendix Table A4 reports results from a fixed effects logit estimator using the full sample BLL dummy variable specification and shows very similar results.

The results are also similar to those of Miranda et al. (2010). Miranda et al.'s logistic analysis of North Carolina 4th graders with a learning or behavioral exceptionality found odds ratios increasing from 1.16 for BLL = 4 μg/dL to 1.49 for BLL = 9 μg/dL, suggesting a roughly 6% increase per each 1-unit increase in BLL. The similarity to our results indicates that the relationship estimated in Miranda et al. (2010) is robust to our inclusion of older elementary and middle school students and the use of school-grade-year fixed effects and covariate matching to strengthen a causal interpretation.

Table 3 presents select summary statistics for the analysis of the effects of special education on end-of-grade math and reading performance. Column (1) shows the sub-sample of the data including only observations from students who entered special education between 4th and 8th grade (ever-treated students). Because the treatment and control students in this analysis are the same, just at different points in time, we do not disaggregate key characteristics by treatment and control group. Column (2) presents summary statistics from general education (never-treated) students, who are included as a comparison group in the alternative analysis. Ever-treated students were classified as having a learning disability or exceptionality requiring special education in 60% of observations. They had higher BLLs, much lower math and reading score percentiles, and other significant differences in student and family characteristics from general education students.

Table 3: Select summary stats for math/reading score analysis sample

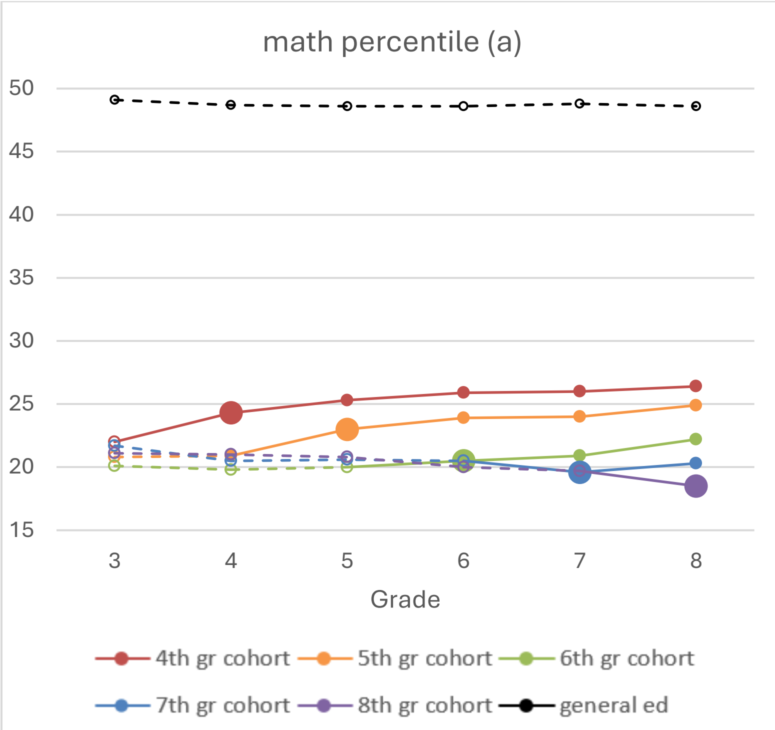
	Ever-treated students (1)	Never-treated students (2)	p
BLL	3.71 (1.97)	3.40 (1.87)	< 0.001
Learning disability/ Special education	0.60 (0.49)	0.00 (0.00)	< 0.001
Math Score Percentile	22.61 (21.24)	48.75 (27.32)	< 0.001
Reading Score Percentile	20.63 (20.85)	49.08 (27.22)	< 0.001
Male	0.58 (0.49)	0.47 (0.50)	< 0.001
White	0.39 (0.49)	0.49 (0.50)	< 0.001
Black	0.43 (0.49)	0.33 (0.47)	< 0.001
Hispanic	0.12 (0.33)	0.12 (0.32)	< 0.001
Other Race	0.06 (0.24)	0.06 (0.25)	< 0.001
Economically disadvantaged (all years)	0.57 (0.49)	0.43 (0.49)	< 0.001
Mother Graduated College	0.07 (0.25)	0.15 (0.36)	< 0.001
Observations	104,419*	3,174,916	
Unique students	21,762	640,252	

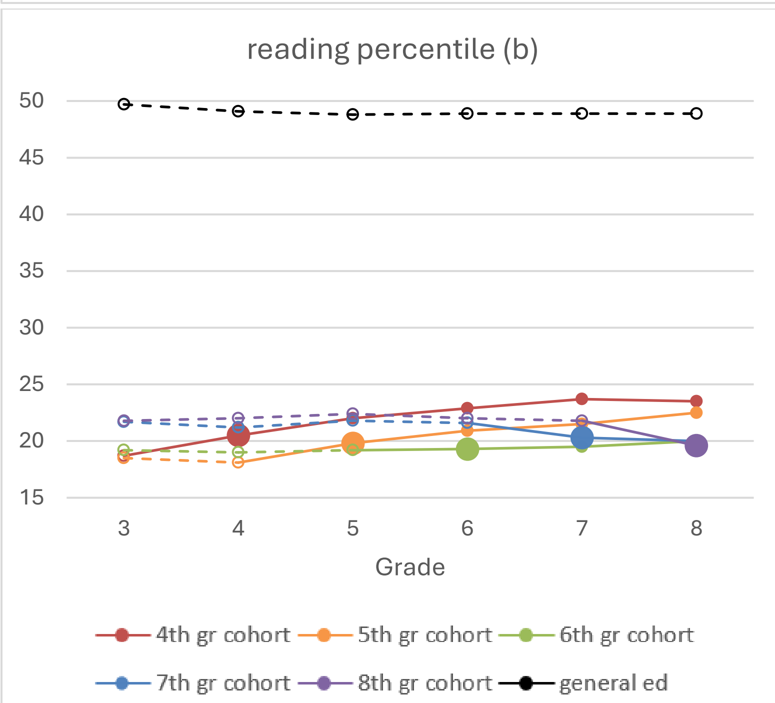
* Note that the estimates using not-yet-treated controls shown in columns (1) and (2) of Table A6 drop all student-year observations from 8th grade because there are no remaining never-treated observations in this group by 8th grade, resulting in a sample size of 91,306.

Figure 2 plots average end-of-grade math and reading scores by grade and special education cohort. It shows that, on average, general education students in our sample had largely stable math and reading scores across grades hovering around the 49th percentile. Students who entered special education had much lower average scores in every grade. For the cohorts that did not enter special education until the later grades, there was a slight downward trend in test scores prior to entering special education. Average scores largely increased once each cohort entered special education.

We cannot formally test for parallel trends among the ever-treated group as a whole because the largest treated cohort—students who entered special education in 4th grade—have only a single pre-treatment observation, so there is no trend to examine. We assess pre-treatment trends among the other cohorts, as well as the trend across grades among general education students, in Appendix Table A5. These results show significant downward trends for the 6th-8th grade cohorts relative to general education students, though the pre-treatment trend for the 5th grade entry cohort is similar to general education students' 3rd to 4th grade trend. These patterns suggest that test scores among the treated group as a whole would have been highly unlikely to increase in the absence of special education. On the contrary, it is more likely that they would have continued to decline over time, though we cannot rule out the possibility that they that they would have been parallel to the relatively stable pattern seen among general education students or declined a rate somewhere between the general education trend and the trend among special education students for which pre-treatment data are available.

Figure 2: Math (a) and reading (b) end-of-grade test score percentiles for general education and special education students by special education entry cohort and grade

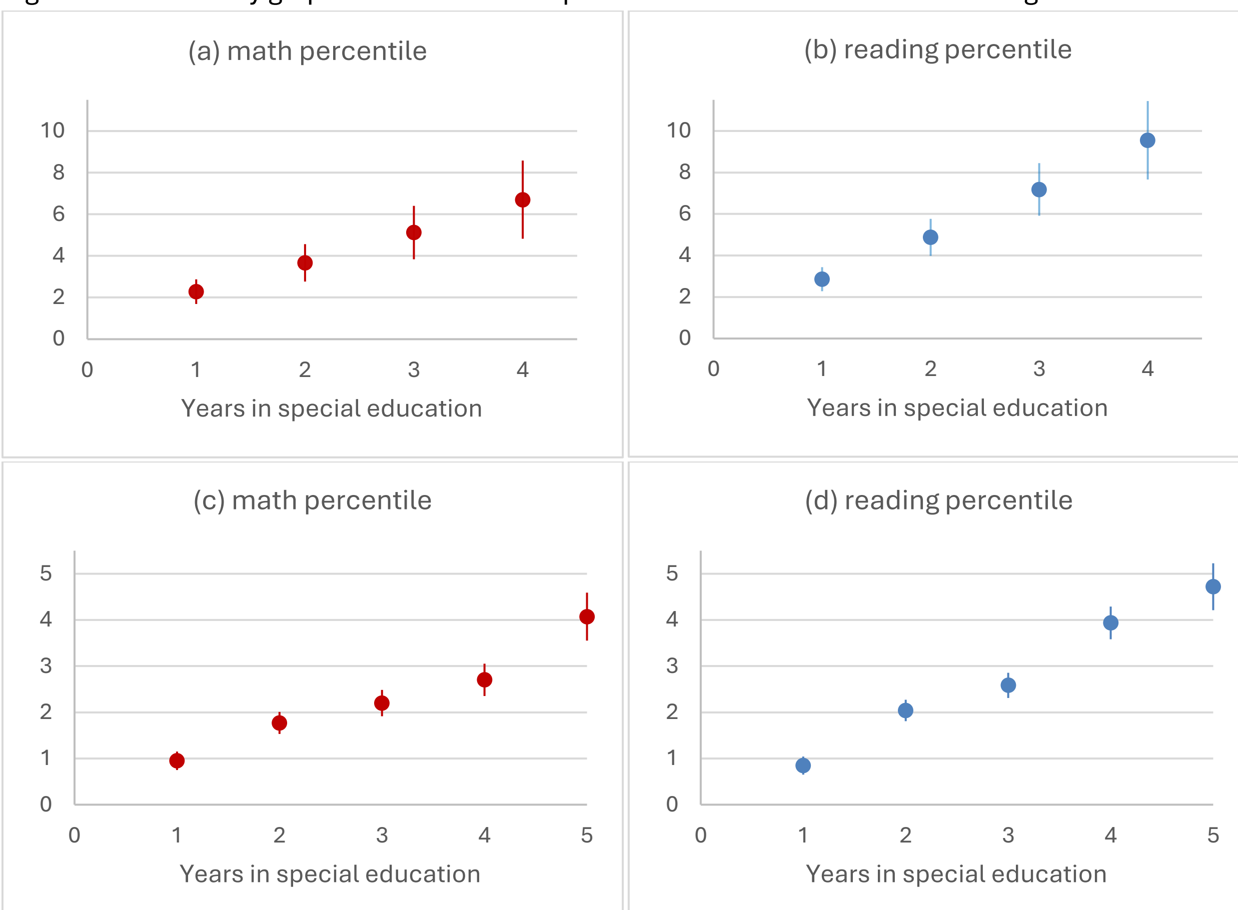




Dashed lines represent pre-treatment trends in mean test scores for each cohort, and solid lines represent post-treatment trends. Hollow circles represent pre-treatment means, and solid circles represent post-treatment means. Large solid circles represent means the first year post-treatment.

Moving on to the regression results for the effects of special education on academic performance, we estimated separate coefficients for each cohort of students in each grade in our extended two-way fixed effects (ETWFE) analysis. Appendix Table A6 presents these disaggregated results. It shows that a year of special education had a positive and statistically significant effect on students' math and reading scores for most cohorts and grades. Columns 1 and 2 present math and reading effects, respectively, for the model that only includes ever-treated students and uses the not-yet-treated student-year observations as controls. Columns 3 and 4 present the effects for the alternative analysis that also includes observations from general education students in the control group.

Figure 3: Event study graphs of the effect of special education on math and reading scores



Panels a-d correspond to columns 1–4, respectively, of Appendix Table A6, which presents the full coefficient estimates used to derive these results. Panels a and b present results from the model using not-yet-treated controls, and c and d present results including never-treated controls. Standard errors used to derive confidence intervals were calculated using the Delta method. Bars represent 95% percentile confidence intervals.

Across all models, the effects are largest for cohorts that entered special education in earlier grades. The effects also increase with the number of years in special education, suggesting cumulative benefits and underscoring the need to account for time-variant treatment effects. For example, the results in column 1 of Table A6 indicate that students who entered special education in 4th grade had a statistically significant, 2 percentile point gain in end-of-grade math scores that year. Once students in the 4th grade cohort reached the end of 7th grade, they experienced a 7 percentile point gain in math scores that year relative to students who had not yet entered special education. Students in the cohort who did not enter special education until 7th grade did not experience a statistically significant gain in math scores. Figure 3 presents event study figures illustrating how the effect of each year of special education increased with the cumulative number of years in special education for both math and reading when compared to either the not-yet-treated control group (panels a and b) or the control group including never-treated students (panels c and d).

The bolded terms in Appendix Table A6 show the individual special education x BLL interaction effects, our key coefficients of interest. None of the interaction terms in columns 1 or 2 are statistically significant. In the sample including never-treated controls (columns 3-4), a few of the individual interaction effects are statistically significant, but most are not. A test of joint significance of these interactions shows they are not significant for three out of the four sets of estimates. These null results suggest that special education effectiveness did not vary with the level of early childhood lead exposure when considering the full range of exposures experienced by most U.S. children today.

We aggregate the coefficients in Table A6 to estimate the average treatment effect on the treated (ATT) and average BLL interaction effect (AIE) across all treated student-years according to the following calculations. Note that n_{cG} represents the number of treated student-years in cohort *c* and grade *G*, N is all treated observations in the dataset, $\overline{BLL}_{cG}$ represents the mean BLL for this cohort-grade group, β_{1cG} is the uninteracted effect of each year of special education for this group, and β_{2cG} is the special education x BLL interaction effect for this group.

$$ATT = \sum_{c}^{4}\sum_{G}^{4}\frac{n_{cG}}{N}(\beta_{1cG} + \overline{BLL}_{cG}\beta_{2cG}) \qquad (3)$$

$$AIE = \sum_{c}^{4}\sum_{G}^{4}\frac{n_{cG}}{N}\beta_{2cG} \qquad (4)$$

Table 4 reports these average effects. Using the estimates in columns 1 and 2, each year of special education generates an almost 4 percentile point gain in math scores and 5 percentile point gain in reading scores, effects that are both statistically significant. The average interaction effect between special education and BLL on both math and reading scores is close to zero and not statistically significant. As noted above, the individual BLL interaction terms (the β_2 coefficients from equation 2b) are not jointly statistically significant, as shown in row 3. These results show that higher levels of lead exposure do not dampen the effectiveness of special education; students with higher BLLs received roughly the same average test score improvements as less-exposed students in the same cohort and grade.

Table 4: Average treatment effects of special education and special education x BLL interaction on math and reading percentiles

	(1) Math Percentile, Not-yet-treated control	(2) Reading Percentile, Not-yet-treated control	(3) Math Percentile, Not-yet-treated & never-treated control	(4) Reading Percentile, Not-yet-treated & never-treated control
Average treatment effect of special education on the treated	3.71*** (0.43)	4.99*** (0.42)	1.85*** (0.10)	2.17*** (0.10)
Average BLL x special education interaction effect	0.04 (0.20)	-0.01 (0.20)	-0.05 (0.05)	-0.08* (0.05)
Joint test for all BLL interaction coefficients = 0 (Prob > F)	0.73	0.25	0.04	0.52

Appendix Table A6 presents the full coefficient estimates used to derive these results. * p<0.10, ** p<0.05, *** p<0.01. Standard errors estimated using the Delta method

The estimates shown in columns 3 and 4 of Table 4, which include never-treated students as control observations, show a similar pattern: positive and significant academic gains from special education that are largely invariant to the level of early childhood lead exposure. The average treatment effects are smaller—roughly half as large as those from columns 1 and 2. The average special education x BLL interaction effect for math (column 3, second row) is small and not significantly different from zero. The individual interaction terms are jointly statistically significant but not consistent in sign. The average special education x BLL interaction effect for reading (column 4, second row) is negative and marginally statistically significant, but the individual interaction terms are not jointly significant. These estimates suggest that there could be relatively small average declines in special education effectiveness at higher BLLs that are not considered statistically significant at conventional levels.

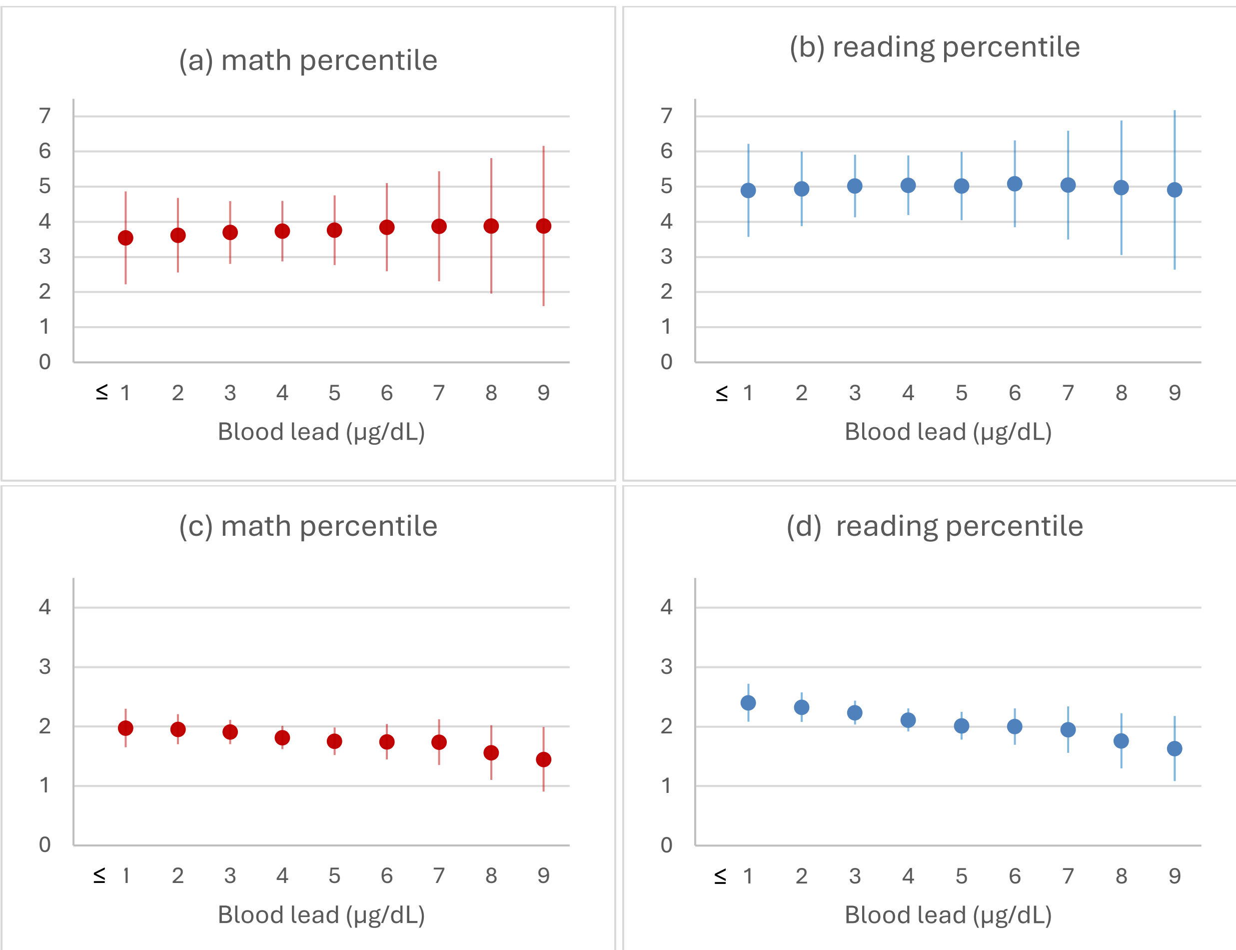
Recall that if treated students' scores would not have followed the same trend as those of general education students in the absence of treatment, then the results from this model are biased towards the null. Inclusion of a large number of never-treated student-years as controls imposes a counterfactual in which learning disabled students' test scores would have neither improved nor worsened without special education. However, Figure 2 suggests that test scores among learning disabled students may have instead declined without special education. The estimates in columns 1 and 2 capture are our preferred estimates because they incorporate this declining counterfactual.

Figure 4 displays the key findings of our analysis graphically. To create this figure, we calculated the following BLL-specific average treatment effects for students with a BLL value of k. Here, n_{cGk} represents the number of treated observations in cohort *c* and grade *G* with and early childhood BLL of k, while N_k represents the total number of treated observations with that BLL in the dataset.

$$ATT_{BLL=k} = \sum_{c}^{4} \sum_{G}^{4} \frac{n_{cGk}}{N_k} (\beta_{1cG} + k\beta_{2cG}) \qquad (5)$$

Figure 4 makes apparent that the average treatment effect of special education is relatively consistent for students regardless of their early childhood BLL. In other words, special education has a positive and statistically significant effect on both reading and math scores, including for students with the highest BLL values in our sample. This stable trend is particularly apparent in our preferred model using not-yet-treated students as the control group (panels a and b). It is notable that $\beta_1 + BLL_i * \beta_2 > 0$ at all BLL values considered in our analysis, including those well above recent CDC thresholds for recommended interventions. Results are less precise at higher BLL values due to smaller sample sizes.

Figure 4. Average effect of one year of special education on math and reading test scores by blood lead level



Panels a-d correspond to columns 1-4, respectively, of Appendix Table A6, which presents the full coefficient estimates used to derive these results. Panels a and b present results from the model using not-yet-treated controls, and c and d present results including never-treated controls. Standard errors used to derive confidence intervals were calculated using the Delta method. Bars represent 95% percentile confidence intervals.

Appendix Table A7 presents the results of the traditional TWFE estimator shown in equation (2). Using the not-yet-treated student-year observations as controls (columns 1 and 2), we find that the average treatment effects across all treated students are about half as large as the corresponding ETWFE estimates for both math and reading shown in Table 4. These smaller estimates likely result due to the downward bias caused by implicitly including already-treated observations as controls in a context where treatment effects increase over time.

However, our key effects of interest—the interactions between special education and BLL—are again small and not significantly different from zero in columns 1 and 2, which is consistent with the ETWFE estimates and indicates that special education is equally effective in improving math and reading performance for students with learning disabilities regardless of early childhood lead exposure level. When never-treated general education student-year observations are included in the control group of the TWFE model (columns 3 and 4 of Table A7), the average treatment effects are very similar to the corresponding ETWFE estimates in Table 4. However, the estimated interaction effects are negative, larger in absolute value than the ETWFE estimates, and statistically significant at conventional levels for both math and reading, which would suggest that special education is less effective for students with higher lead exposure levels. These results underscore that use of the TWFE estimator in this context in which treatment effects are staggered and changing over time can yield biased estimates of the effects of special education and its interaction with lead exposure.

To examine the robustness of our results, we re-estimate the models presented in Table A6 and summarized in Table 4 but drop all observations from students that ever repeated a grade at any point in our sample. Table A8 presents these results, and Table A9 provides aggregated results comparable to Table 4. These results are very similar to our primary estimates, indicating that grade retention does not confound our findings.

We also estimate ETWFE regressions using math and reading z-scores as the dependent variables to facilitate comparisons to other studies (Table A10). We find that each year of special education improved test scores by 0.11-0.16 standard deviations for math and 0.11-0.24 standard deviations for reading on average (Table A11). As in our primary estimates focused on math and reading percentiles, there is no significant interaction effect between special education and BLL, suggesting that these standard deviation gains accrue to students regardless of early childhood BLL.

Our estimates of a 2-5 percentile point or 0.11-0.24 standard deviation increase in math and reading test scores for each year of special education are similar to the results of previous studies on lead interventions. Stingone et al.'s (2022) analysis of an early education intervention program for preschoolers found a 0.07 standard deviation increase in math scores and a 0.10 standard deviation increase in reading scores in 3rd grade for children with BLL ≥ 4 μg/dL. Studies on the effects of North Carolina's property intervention program for children with BLL ≥ 10 μg/dL found academic gains of 0.10 to 0.22 (Billings and Schnepel 2018) and 4 to 6 percentile points in 3rd-8th grade (Bui et al. 2025). Our results are also similar to those from the panel data literature on the effect of special education on test scores, which has largely found gains around 0.1 to 0.2 standard deviations in 3rd-8th grade (Hanushek et al. 2002, Hurwitz et al. 2020. Schwartz et al. 2021).

Discussion

Our results show that special education is a highly effective way to raise the academic performance of students with learning disabilities, including those with relatively high early childhood BLL. The finding that educational deficits can be mitigated for students who had elevated BLL as young children is notable because it runs counter to the claim that early childhood lead exposure causes irreversible neurocognitive damages. It is also novel given the dearth of studies examining the effects of educational interventions or any type of intervention for school-aged children previously exposed to lead on neurodevelopmental outcomes (Bui et al. 2024; Nussbaumer-Streit et al. 2020). We also find that the positive effects of special education increase with the number of years of treatment. This result suggests that educational interventions can be more effective if students are identified and receive interventions in earlier grades.

Our results can be incorporated into analyses of policies affecting children's lead exposure. As an example, the estimated effect of early childhood lead exposure on the probability of having a learning disability can be used to project the special education cost savings from a recent EPA regulation lowering the level of lead in dust at residential properties. EPA's (2024c) economic analysis estimated that between 125,715 and 284,868 young children will be affected by this policy annually and that these children will experience an average decrease in blood lead levels in the range of 0.1-0.3 µg/dL. The linear CEM estimates indicate that each 1-µg/dL increase in BLL above 1 µg/dL is associated with a 6.6% increase in the probability of having a learning disability in each school year. Therefore, a 0.1-0.3 µg/dL decrease in BLL implies a 0.66%-2% decrease in the probability of having a diagnosed learning disability each year.

We calculate that the lifetime net present value of the incremental cost of providing special education each year to a student with a mental or emotional disability during K through 12 is about $150,000.[23] Aggregating over the 125,715 to 284,868 young children affected by the regulation each year yields an estimated education cost saving of approximately $9 million to $55 million.[24] This figure represents about 2% of the IQ and lifetime earnings benefits and less than 1% of the total

[23] The average annual incremental expenditure for students with disabilities was $13,127 in FY2020 (Kaput and Scheiss 2024), which is $15,010 when inflated to 2022 dollars. The net present value of the annual $15,010 expenditure in every year of K through 12 equates to $150,467 discounting back to age 3 at a 3% discount rate. The incremental expenditure data is not specific to mental and emotional disabilities, since recent disaggregated expenditure data by disability type are not available. However, data from 1999-2000 on education expenditures by disability suggest that the weighted average cost ratio for mental and emotional disabilities was higher at that time than the average cost ratio across all disabilities (Chambers et al. 2003), so our estimate based on average expenditures across all disabilities may be conservative.

[24] Given a 0.46 percentage point decrease in the probability of special education participation per unit decrease in BLL, a 0.1-0.3 µg/dL decrease in BLL would decrease the probability of having a learning disability by 0.046-0.14%, resulting in an average net present value decrease in expected special education costs of $63 to $190 per student.

benefits of the regulation that EPA (2024c) monetized in the economic analysis, which also included reductions in cases of attention deficit and hyperactivity disorder and premature adult cardiovascular mortality. Accounting for special education cost savings would not meaningfully change the estimated net benefits of this particular regulation, but it could provide a more comprehensive accounting of the neurocognitive effects of this and other policies that reduce lead exposure.

Our results also shed light on the benefits of special education for students, including those with early childhood lead exposure. The academic gains estimated in our analysis from special education are similar in magnitude to the 0.13 standard deviation increase in test scores estimated by Chetty et al. (2014) in response to a one standard deviation improvement in teacher quality. Chetty et al. (2014) also estimated that a one standard deviation improvement in end-of-grade test scores during 3rd-8th grade was associated with a 12% increase in earnings at age 28. Combining our preferred estimates from Table A11 (columns 1 and 2) and Chetty et al.'s earnings effect, we estimate that each year of special education is expected to increase earnings by 2.4%.[25] Assuming that the Chetty et al. findings on test scores and earnings at age 28 can be extrapolated to lifetime earnings and using the EPA's (2024) estimated net present value of lifetime earnings for a child of $1.09 million (in 2022 dollars discounted at 3% back to age 3) implies that each year of special education yields a net present value increase in lifetime earnings of $25,506.

We estimate that these gains would accrue to students with elevated lead levels up to at least 9 μg/dL. In fact, our results imply that special education can eliminate the average academic deficits associated with lead exposure at these levels for students with learning disabilities. To arrive at this conclusion, we note that Shadbegian et al. (2019), using a similar dataset as the present study, found that a BLL of 9 μg/dL decreased test scores by 1.5 to 2.5 percentile points on average. Furthermore, Miranda et al. (2009) found using a quantile regression that the negative effect of BLL on academic test scores was roughly twice as large for students in the bottom decile of the test score distribution compared to students at the median of the test score distribution. Our results suggest that special education can lead to improvements of 1.5-5 percentile points for a student with a BLL of 9 μg/dL—similar or larger than the deficit estimated by Shadbegian et al (2019). Therefore, the academic deficit resulting from lead exposure for a student towards the bottom of the test score distribution would be roughly balanced out by the gain from special education for those who receive those services.

Our analysis has several limitations. It is limited to 3rd-8th graders—the years in which students typically take standardized tests in the US—and to one US state. Our policy applications assume that the results can be extrapolated nationally and to grades K through 12, but we cannot empirically test this assumption. In addition, we relied on a limited population of students who entered special education during 4th-8th grade for identification in our panel data analysis. The majority of special education students in our dataset were already enrolled by 3rd grade. If students who enrolled in later grades tend to have milder learning disabilities than those enrolled earlier, the

[25] An average effect of 0.20 of a standard deviation across reading and math scores multiplied by Chetty et al.'s estimate of a 12% increase in earnings per standard deviation increase in test scores implies a 2.4% increase in earnings per year of special education.

results might not generalize to the full special education population. We also lack a fully exogenous natural experiment such as the Texas policy change examined in Ballis and Heath (2021) and rely on student and school-grade-year fixed effects and observable characteristics to isolate the effects of interest.

Conclusions

We estimate the effect of early childhood lead exposure on special education participation and the effect of special education for students with varying lead exposure levels on academic test scores using a linked dataset of North Carolina students. We find that lead exposure results in a higher probability of a learning disability or other mental or emotional difficulty. For students with a learning disability, each year of special education participation boosts math and reading scores by 2 to 5 percentile points, or 0.11 to 0.24 standard deviations. This significant increase in academic performance occurs for students with low and high early childhood lead exposure alike. These results demonstrate that special education programs can help mitigate the deficits in academic achievement caused by lead exposure.

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Appendix

Table A1. Full student-year summary statistics for learning disability analysis sample by BLL group

	(1) BLL <= 1 ug/dL	(2) BLL > 1 ug/dL	p	(3) BLL <= 1, CEM	(4) BLL > 1 ug/dL, CEM	p
BLL	1.00 (0.00)	3.80 (1.75)	< 0.001	1.00 (0.00)	3.43 (1.58)	< 0.001
Learning disability 1=Y	0.07 (0.26)	0.09 (0.29)	< 0.001	0.07 (0.26)	0.08 (0.28)	< 0.001
Math Score Percentile	52.01 (28.14)	45.23 (27.72)	< 0.001	50.44 (28.70)	49.31 (28.83)	< 0.001
Reading Score Percentile	52.05 (28.16)	45.30 (27.84)	< 0.001	50.21 (28.68)	48.79 (28.95)	< 0.001
Math Z-score	0.09 (0.96)	-0.14 (0.95)	< 0.001	0.03 (0.99)	-0.01 (0.99)	< 0.001
Reading Z-score	0.09 (0.96)	-0.13 (0.97)	< 0.001	0.03 (0.99)	-0.02 (1.01)	< 0.001
Test score missing	0.01 (0.07)	0.01 (0.11)	< 0.001	0.01 (0.08)	0.01 (0.09)	< 0.001
Male	0.47 (0.50)	0.49 (0.50)	< 0.001	0.48 (0.50)	0.48 (0.50)	1
White	0.56 (0.50)	0.46 (0.50)	< 0.001	0.52 (0.50)	0.52 (0.50)	1
Black	0.20 (0.40)	0.36 (0.48)	< 0.001	0.30 (0.46)	0.30 (0.46)	1
Hispanic	0.17 (0.37)	0.11 (0.31)	< 0.001	0.14 (0.35)	0.14 (0.34)	< 0.001
Other Race	0.06 (0.25)	0.06 (0.24)	0.004	0.04 (0.20)	0.04 (0.21)	< 0.001
Economically disadvantaged (any year)	0.59 (0.49)	0.74 (0.44)	< 0.001	0.60 (0.49)	0.60 (0.49)	1

Economically disadvantaged (all years)	0.36 (0.48)	0.46 (0.50)	< 0.001	0.44 (0.50)	0.44 (0.50)	1
Economically disadvantaged missing	0.00 (0.01)	0.00 (0.01)	0.007	0.00 (0.00)	0.00 (0.00)	1
Medicaid	0.45 (0.50)	0.54 (0.50)	< 0.001	0.49 (0.50)	0.49 (0.50)	1
Birth order = 2	0.35 (0.48)	0.33 (0.47)	< 0.001	0.36 (0.48)	0.34 (0.48)	< 0.001
Birth order = 3 or higher	0.23 (0.42)	0.24 (0.43)	< 0.001	0.21 (0.41)	0.22 (0.41)	< 0.001
Birth order missing	0.00 (0.02)	0.00 (0.02)	0.019	0.00 (0.01)	0.00 (0.01)	0.417
Mother's Characteristics						
Did not Complete High School	0.08 (0.27)	0.07 (0.26)	< 0.001	0.08 (0.27)	0.08 (0.27)	< 0.001
Completed Some High School	0.16 (0.36)	0.23 (0.42)	< 0.001	0.19 (0.39)	0.19 (0.39)	< 0.001
Graduated High School	0.31 (0.46)	0.37 (0.48)	< 0.001	0.33 (0.47)	0.33 (0.47)	1
Some College	0.22 (0.42)	0.20 (0.40)	< 0.001	0.19 (0.39)	0.19 (0.39)	1
Graduated College	0.23 (0.42)	0.13 (0.33)	< 0.001	0.21 (0.41)	0.21 (0.41)	1
<21 years	0.16 (0.36)	0.26 (0.44)	< 0.001	0.19 (0.39)	0.19 (0.39)	1
21-29 years	0.50 (0.50)	0.50 (0.50)	< 0.001	0.54 (0.50)	0.54 (0.50)	1
30+ years	0.35	0.24	< 0.001	0.27	0.27	1

	(0.48)	(0.43)		(0.44)	(0.44)	
Age Missing	0.00	0.00	0.004	0.00	0.00	--
	(0.01)	(0.01)		(0.00)	(0.00)	
Education Missing	0.00	0.00	0.629	0.00	0.00	1
	(0.04)	(0.04)		(0.01)	(0.01)	
Smoked	0.12	0.17	< 0.001	0.11	0.12	< 0.001
	(0.33)	(0.38)		(0.31)	(0.33)	
Smoked Missing	0.00	0.00	0.204	0.00	0.00	< 0.001
	(0.03)	(0.03)		(0.03)	(0.03)	
Not Married	0.35	0.47	< 0.001	0.41	0.41	1
	(0.48)	(0.50)		(0.49)	(0.49)	
Not Married Missing	0.00	0.00	0.419	0.00	0.00	--
	(0.01)	(0.01)		(0.00)	(0.00)	
Used Alcohol	0.01	0.01	< 0.001	0.01	0.01	< 0.001
	(0.08)	(0.10)		(0.08)	(0.08)	
Used Alcohol Missing	0.00	0.00	< 0.001	0.00	0.00	0.003
	(0.04)	(0.04)		(0.03)	(0.03)	
Father's Characteristics						
Did not Complete High School	0.07	0.06	< 0.001	0.06	0.06	< 0.001
	(0.25)	(0.23)		(0.24)	(0.24)	
Completed Some High School	0.12	0.15	< 0.001	0.12	0.12	< 0.001
	(0.33)	(0.36)		(0.32)	(0.33)	
Graduated High School	0.30	0.32	< 0.001	0.28	0.28	0.025
	(0.46)	(0.47)		(0.45)	(0.45)	
Some College	0.16	0.13	< 0.001	0.15	0.15	0.921
	(0.37)	(0.34)		(0.35)	(0.35)	
Graduated College	0.20	0.10	< 0.001	0.19	0.18	< 0.001
	(0.40)	(0.30)		(0.40)	(0.39)	
Education Missing	0.15	0.24	< 0.001	0.20	0.20	< 0.001

	(0.35)	(0.43)		(0.40)	(0.40)	
<21 years	0.05 (0.23)	0.09 (0.28)	< 0.001	0.07 (0.25)	0.07 (0.25)	0.658
21-29 years	0.50 (0.50)	0.50 (0.50)	0.005	0.51 (0.50)	0.51 (0.50)	0.001
30+ years	0.44 (0.50)	0.31 (0.46)	< 0.001	0.38 (0.49)	0.38 (0.49)	0.004
Age Missing	0.12 (0.32)	0.20 (0.40)	< 0.001	0.16 (0.37)	0.17 (0.37)	< 0.001
Birth Month						
Jan	0.08 (0.28)	0.09 (0.28)	< 0.001	0.09 (0.28)	0.09 (0.28)	0.017
Feb	0.08 (0.27)	0.08 (0.27)	< 0.001	0.08 (0.27)	0.08 (0.27)	< 0.001
Mar	0.09 (0.29)	0.08 (0.27)	< 0.001	0.09 (0.29)	0.08 (0.28)	< 0.001
Apr	0.09 (0.28)	0.08 (0.27)	< 0.001	0.09 (0.28)	0.08 (0.27)	< 0.001
May	0.09 (0.28)	0.08 (0.27)	< 0.001	0.09 (0.28)	0.08 (0.27)	< 0.001
Jun	0.09 (0.28)	0.08 (0.28)	< 0.001	0.08 (0.28)	0.08 (0.28)	0.582
Jul	0.09 (0.28)	0.09 (0.29)	< 0.001	0.08 (0.28)	0.09 (0.28)	< 0.001
Aug	0.09 (0.28)	0.09 (0.29)	0.142	0.09 (0.28)	0.09 (0.29)	0.003
Sept	0.08 (0.27)	0.08 (0.28)	< 0.001	0.08 (0.27)	0.08 (0.27)	< 0.001
Oct	0.08 (0.27)	0.08 (0.28)	< 0.001	0.08 (0.27)	0.08 (0.28)	< 0.001

Nov	0.07	0.08	< 0.001	0.08	0.08	< 0.001
	(0.26)	(0.27)		(0.26)	(0.27)	
Dec	0.08	0.08	< 0.001	0.08	0.08	< 0.001
	(0.27)	(0.28)		(0.27)	(0.28)	
Observations	500,089	3,178,359		390,955	1,126,013	
Unique students	133,969	676,169		109,813	300,299	

MOB = child's month of birth. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2. Select student-year summary statistics for math and reading score analysis sample by special education status

	(1) Entrants	(2) Exiters	(3) Always special ed	(4) Never special ed
Learning disability	0.60	0.55	1.00	0.00
	(0.49)	(0.50)	(0.00)	(0.00)
Math Score Percentile	22.96	33.10	22.51	48.57
	(21.40)	(25.25)	(22.03)	(27.33)
Reading Score Percentile	20.95	33.40	19.31	48.90
	(20.99)	(25.54)	(20.83)	(27.25)
Math Z-score	-0.95	-0.57	-0.99	-0.02
	(0.85)	(0.93)	(0.89)	(0.92)
Reading Z-score	-1.06	-0.56	-1.11	-0.00
	(0.94)	(0.97)	(1.02)	(0.91)
BLL	3.76	4.10	3.72	3.38
	(1.98)	(2.02)	(2.03)	(1.87)
Male	0.58	0.66	0.68	0.47
	(0.49)	(0.47)	(0.47)	(0.50)
White	0.38	0.52	0.45	0.48
	(0.49)	(0.50)	(0.50)	(0.50)
Black	0.43	0.38	0.39	0.34
	(0.50)	(0.49)	(0.49)	(0.47)
Hispanic	0.12	0.05	0.10	0.12
	(0.33)	(0.22)	(0.31)	(0.32)
Other Race	0.06	0.05	0.06	0.06
	(0.24)	(0.22)	(0.24)	(0.25)
Economically disadvantaged (all years)	0.58	0.49	0.60	0.43
	(0.49)	(0.50)	(0.49)	(0.50)
Mother Graduated College	0.06	0.08	0.07	0.15
	(0.25)	(0.27)	(0.25)	(0.36)
Observations	119,763	45,513	228,410	3,284,762
Unique students	22,976	8,024	61,206	717,932

Table A3. Full results: Linear probability model of probability of having a learning disability

	(1) Full sample	(2) CEM	(3) CEM linear
Blood lead level			
2 µg/dL	0.001 (0.001)	0.003** (0.001)	
3 µg/dL	0.006*** (0.001)	0.007*** (0.001)	
4 µg/dL	0.012*** (0.001)	0.012*** (0.002)	
5 µg/dL	0.018*** (0.001)	0.020*** (0.002)	
6 µg/dL	0.026*** (0.002)	0.018*** (0.003)	
7 µg/dL	0.031*** (0.002)	0.029*** (0.004)	
8 µg/dL	0.039*** (0.002)	0.037*** (0.005)	
9 µg/dL	0.040*** (0.003)	0.035*** (0.006)	
BLL (continuous)			0.0046*** (0.0003)
Black	-0.002* (0.001)	0.001 (0.002)	0.001 (0.002)
Hispanic	-0.047*** (0.001)	-0.045*** (0.002)	-0.045*** (0.002)
Asian/PI, NA, Other	-0.017*** (0.001)	-0.018*** (0.003)	-0.018*** (0.003)
Economically disadvantaged (any year)	0.023*** (0.001)	0.020*** (0.002)	0.020*** (0.002)
Economically disadvantaged (all years)	0.026*** (0.001)	0.025*** (0.002)	0.025*** (0.002)
Economically disadvantaged missing	-0.001 (0.012)	0.000 (.)	0.000 (.)

Medicaid	-0.003***	0.003*	0.003*
	(0.001)	(0.002)	(0.002)
Birth Order 2nd	0.011***	0.009***	0.009***
	(0.001)	(0.001)	(0.001)
Birth Order 3rd+	0.019***	0.017***	0.017***
	(0.001)	(0.001)	(0.001)
Birth Order is Missing	0.013	0.011	0.011
	(0.018)	(0.033)	(0.033)
Mother's characteristics			
Not Married	-0.001	-0.000	-0.000
	(0.001)	(0.002)	(0.002)
Not Married Missing	0.051	0.000	0.000
	(0.067)	(.)	(.)
Used Alcohol	0.012***	0.012*	0.012*
	(0.004)	(0.007)	(0.007)
Used Alcohol Missing	0.005	0.020	0.020
	(0.014)	(0.029)	(0.029)
Smoked	0.005***	0.006***	0.006***
	(0.001)	(0.002)	(0.002)
Smoked Missing	0.019	0.016	0.016
	(0.018)	(0.036)	(0.036)
Completed Some High School	-0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)
Graduated High School	-0.028***	-0.023***	-0.023***
	(0.002)	(0.003)	(0.003)
Some College	-0.041***	-0.035***	-0.035***
	(0.002)	(0.003)	(0.003)
Graduated College	-0.049***	-0.043***	-0.043***
	(0.002)	(0.003)	(0.003)
Education Missing	-0.005	0.005	0.005
	(0.008)	(0.041)	(0.041)
21-29 years	0.009***	0.008***	0.008***
	(0.001)	(0.002)	(0.002)

30+ years	0.019***	0.019***	0.019***
	(0.001)	(0.003)	(0.003)
Age Missing	0.000	0.000	0.000
	(0.034)	(.)	(.)
Father's characteristics			
Completed Some High School	0.003*	0.000	0.000
	(0.002)	(0.003)	(0.003)
Graduated High School	-0.015***	-0.016***	-0.016***
	(0.002)	(0.003)	(0.003)
Some College	-0.026***	-0.024***	-0.024***
	(0.002)	(0.003)	(0.003)
Graduated College	-0.037***	-0.037***	-0.037***
	(0.002)	(0.003)	(0.003)
Education Missing	-0.005**	-0.002	-0.002
	(0.002)	(0.004)	(0.004)
21-29 years	0.011***	0.010***	0.010***
	(0.001)	(0.002)	(0.002)
30+ years	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)
Age Missing	0.015***	0.011***	0.011***
	(0.002)	(0.004)	(0.004)
Birth month			
Jan	0.002	0.002	0.002
	(0.001)	(0.002)	(0.002)
Feb	0.009***	0.008***	0.008***
	(0.001)	(0.002)	(0.002)
Mar	0.007***	0.007***	0.007***
	(0.001)	(0.002)	(0.002)
Apr	0.008***	0.007***	0.007***
	(0.001)	(0.002)	(0.002)
May	0.010***	0.008***	0.008***
	(0.001)	(0.002)	(0.002)
Jun	0.010***	0.009***	0.009***

	(0.001)	(0.002)	(0.002)
Jul	0.012***	0.010***	0.010***
	(0.001)	(0.002)	(0.002)
Aug	0.014***	0.014***	0.014***
	(0.001)	(0.002)	(0.002)
Sept	0.012***	0.009***	0.009***
	(0.001)	(0.002)	(0.002)
Oct	0.005***	0.000	0.000
	(0.001)	(0.002)	(0.002)
Nov	-0.000	-0.000	-0.000
	(0.001)	(0.002)	(0.002)
Constant	0.036***	0.039***	0.033***
	(0.003)	(0.004)	(0.004)
Observations	3,678,448	1,510,951	1,510,951
R^2	0.077	0.112	0.112

All specifications include school-grade-year fixed effects. Standard errors clustered at student level.
* p<0.10, ** p<0.05, *** p<0.01

Table A4. Odds Ratios from Fixed Effects Logit Estimates of probability of having a learning disability: Full sample dummy variable model

	Full Sample
Blood lead level	
2 µg/dL	1.020*** (0.007)
3 µg/dL	1.084*** (0.008)
4 µg/dL	1.184*** (0.009)
5 µg/dL	1.271*** (0.011)
6 µg/dL	1.381*** (0.013)
7 µg/dL	1.442*** (0.016)
8 µg/dL	1.553*** (0.019)
9 µg/dL	1.556*** (0.023)
Black	2.193*** (0.009)
Hispanic	0.966*** (0.006)
Asian/PI, NA, Other	0.548*** (0.005)
Economically disadvantaged (any year)	0.793*** (0.007)
Economically disadvantaged (all years)	1.472*** (0.010)
Economically disadvantaged missing	1.345*** (0.007)
Medicaid	1.045 (0.191)

Birth Order 2nd	0.973*** (0.005)
Birth Order 3rd+	1.172*** (0.006)
Birth Order is Missing	1.266*** (0.007)
Mother's characteristics	
Not Married	0.996 (0.005)
Not Married Missing	1.485* (0.342)
Used Alcohol	1.110*** (0.020)
Used Alcohol Missing	1.059 (0.075)
Smoked	1.046*** (0.005)
Smoked Missing	1.206** (0.100)
Completed Some High School	0.953*** (0.008)
Graduated High School	0.727*** (0.006)
Some College	0.600*** (0.006)
Graduated College	0.495*** (0.006)
Education Missing	0.952 (0.042)
21-29 years	1.095*** (0.006)
30+ years	1.234*** (0.011)

Age Missing	0.977 (0.257)
Father's characteristics	
Completed Some High School	1.020** (0.010)
Graduated High School	0.826*** (0.008)
Some College	0.696*** (0.008)
Graduated College	0.550*** (0.008)
Education Missing	0.943*** (0.012)
21-29 years	1.120*** (0.008)
30+ years	1.028*** (0.006)
Age Missing	1.153*** (0.012)
Birth month	
Jan	1.023** (0.010)
Feb	1.123*** (0.011)
Mar	1.092*** (0.010)
Apr	1.106*** (0.011)
May	1.146*** (0.011)
Jun	1.135*** (0.011)

Jul	1.178***
	(0.011)
Aug	1.198***
	(0.011)
Sept	1.167***
	(0.011)
Oct	1.069***
	(0.010)
Nov	0.994
	(0.010)
Observations	3,381,896
Pseudo R^2	0.061

Table reports odds ratios instead of coefficients, with standard errors in parentheses. All specifications include school-grade-year fixed effects. 296,552 observations were dropped due to lack of variation in the outcome within school-grade-year groups. Coefficient standard errors reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A5: Pre-treatment trends in math (a) and reading (b) score percentiles

Panel a: Math percentile	(1) 3rd-4th grade	(2) 3rd-5th grade	(3) 3rd-6th grade	(4) 3rd-7th grade
Grade	-0.344***	-0.214***	-0.102***	-0.021***
	(0.022)	(0.012)	(0.009)	(0.007)
cohort5 X grade	-0.071			
	(0.216)			
cohort6 X grade	-1.001***	-0.630***		
	(0.289)	(0.164)		
cohort7 X grade	-2.163***	-1.726***	-1.365***	
	(0.354)	(0.214)	(0.146)	
cohort8 X grade	-1.253***	-1.325***	-1.376***	-1.368***
	(0.443)	(0.252)	(0.177)	(0.142)
Constant	49.665***	49.424***	49.098***	48.821***
	(0.076)	(0.048)	(0.038)	(0.033)
Observations	1,249,048	1,808,373	2,311,819	2,766,442
R^2	0.907	0.873	0.854	0.843
Prob > F (cohort x grade effects are equal to each other)	<0.01	<0.01	0.96	N/A

Panel b: reading percentile	(1) 3rd-4th grade	(2) 3rd-5th grade	(3) 3rd-6th grade	(4) 3rd-7th grade
Grade	-0.514***	-0.433***	-0.228***	-0.160***
	(0.023)	(0.013)	(0.009)	(0.007)
cohort5 X grade	-0.388*			
	(0.210)			
cohort6 X grade	-1.138***	-0.652***		
	(0.276)	(0.155)		
cohort7 X grade	-2.236***	-1.444***	-1.290***	
	(0.357)	(0.206)	(0.144)	
cohort8 X grade	-0.862**	-0.892***	-1.083***	-1.104***
	(0.427)	(0.250)	(0.167)	(0.133)
Constant	50.711***	50.678***	50.041***	49.827***

	(0.080)	(0.050)	(0.038)	(0.032)
Observations	1,249,048	1,808,373	2,311,819	2,766,442
R^2	0.896	0.858	0.840	0.827
Prob > F (cohort x grade effects are equal)	<0.01	<0.01	0.35	N/A

All specifications include student fixed effects. Standard errors clustered at student level. * p<0.10, ** p<0.05, *** p<0.01

Table A6: Extended Two-Way Fixed Effects Estimates of Special Education on Math and Reading Score Percentiles with BLL Heterogeneity

	(1) Math Percentile, Not-yet-treated control	(2) Reading Percentile, Not-yet-treated control	(3) Math Percentile, Not-yet-treated & never-treated control	(4) Reading Percentile, Not-yet-treated & never-treated control
4th grade cohort, 4th gr	2.043**	2.636***	1.550***	1.946***
	(0.927)	(0.912)	(0.346)	(0.342)
4th grade cohort, 5th gr	3.689***	3.957***	2.714***	3.628***
	(1.179)	(1.170)	(0.386)	(0.397)
4th grade cohort, 6th gr	5.541***	6.526***	3.135***	3.633***
	(1.467)	(1.424)	(0.447)	(0.440)
4th grade cohort, 7th gr	7.107***	9.205***	3.590***	5.026***
	(1.918)	(2.048)	(0.493)	(0.498)
4th grade cohort, 8th gr			4.867***	5.369***
			(0.582)	(0.569)
5th grade cohort, 5th gr	1.231	2.082*	0.530	0.929**
	(1.086)	(1.085)	(0.397)	(0.382)
5th grade cohort, 6th gr	4.564***	7.269***	1.830***	2.435***
	(1.409)	(1.383)	(0.469)	(0.439)
5th grade cohort, 7th gr	5.176***	9.347***	1.648***	3.393***
	(1.885)	(2.007)	(0.526)	(0.500)
5th grade cohort, 8th gr			2.767***	3.810***
			(0.616)	(0.602)
6th grade cohort, 6th gr	1.717	3.672***	0.434	0.369
	(1.392)	(1.409)	(0.520)	(0.483)
6th grade cohort, 7th gr	2.676	5.198**	0.515	0.456
	(1.894)	(2.033)	(0.593)	(0.581)
6th grade cohort, 8th gr			3.207***	1.196*
			(0.709)	(0.657)
7th grade cohort, 7th gr	0.846	1.655	-1.449**	-1.423**
	(1.883)	(2.000)	(0.677)	(0.700)

7th grade cohort, 8th gr			1.460* (0.871)	-0.395 (0.804)
8th grade cohort, 8th gr			0.020 (0.849)	-1.059 (0.866)
4th grade cohort, 4th gr X BLL	**0.214 (0.223)**	**0.071 (0.228)**	**0.129 (0.089)**	**-0.086 (0.088)**
4th grade cohort, 5th gr X BLL	**-0.013 (0.273)**	**0.098 (0.278)**	**-0.027 (0.096)**	**-0.151 (0.100)**
4th grade cohort, 6th gr X BLL	**-0.155 (0.337)**	**0.153 (0.327)**	**-0.121 (0.108)**	**-0.096 (0.108)**
4th grade cohort, 7th gr X BLL	**-0.107 (0.446)**	**0.092 (0.472)**	**-0.255** (0.115)**	**-0.192 (0.118)**
4th grade cohort, 8th gr X BLL			**-0.204 (0.135)**	**-0.167 (0.131)**
5th grade cohort, 5th gr X BLL	**0.242 (0.251)**	**0.149 (0.250)**	**0.207** (0.098)**	**0.072 (0.095)**
5th grade cohort, 6th gr X BLL	**-0.204 (0.317)**	**-0.445 (0.312)**	**-0.017 (0.113)**	**-0.130 (0.105)**
5th grade cohort, 7th gr X BLL	**0.042 (0.434)**	**-0.530 (0.456)**	**-0.024 (0.123)**	**-0.228* (0.116)**
5th grade cohort, 8th gr X BLL			**0.011 (0.141)**	**-0.108 (0.141)**
6th grade cohort, 6th gr X BLL	**-0.027 (0.313)**	**-0.108 (0.313)**	**-0.096 (0.121)**	**-0.074 (0.113)**
6th grade cohort, 7th gr X BLL	**0.216 (0.437)**	**-0.033 (0.460)**	**-0.090 (0.141)**	**0.035 (0.138)**
6th grade cohort, 8th gr X BLL			**-0.309* (0.163)**	**-0.073 (0.153)**
7th grade cohort, 7th gr X BLL	**0.230 (0.436)**	**0.275 (0.458)**	**-0.012 (0.148)**	**0.189 (0.160)**

7^{th} grade cohort, 8^{th} gr X BLL			**-0.166**	**0.153**
			(0.192)	**(0.186)**
8^{th} grade cohort, 8^{th} gr X BLL			**-0.234**	**-0.040**
			(0.186)	**(0.181)**
4^{th} grade X BLL	-0.115	-0.102	-0.030**	-0.066***
	(0.148)	(0.147)	(0.012)	(0.014)
5^{th} grade X BLL	-0.261	-0.085	-0.062***	-0.060***
	(0.204)	(0.202)	(0.014)	(0.015)
6^{th} grade X BLL	0.030	0.087	-0.052***	-0.106***
	(0.266)	(0.261)	(0.014)	(0.015)
7^{th} grade X BLL	-0.258	-0.012	-0.072***	-0.118***
	(0.390)	(0.414)	(0.015)	(0.016)
8^{th} grade X BLL			-0.055***	-0.131***
			(0.017)	(0.017)
Constant	20.897***	17.866***	48.033***	48.395***
	(0.572)	(0.569)	(0.032)	(0.034)
Observations	91,306	91,306	3,279,335	3,279,335
R^2	0.936	0.935	0.864	0.839

Bolded coefficients represent key BLL interaction terms. All specifications include student and school-grade-year fixed effects. Standard errors clustered at student level.
* p<0.10, ** p<0.05, *** p<0.01

Table A7. Traditional Two-Way Fixed Effects Estimates of Special Education on Math and Reading Score Percentiles with BLL Heterogeneity

	(1) Math Percentile, Not-yet-treated control	(2) Reading Percentile, Not-yet-treated control	(3) Math Percentile, Not-yet-treated & never-treated control	(4) Reading Percentile, Not-yet-treated & never-treated control
Special education	1.698***	1.956***	1.742***	2.043***
	(0.454)	(0.444)	(0.196)	(0.191)
Special education X BLL	**-0.018**	**-0.002**	**-0.108****	**-0.138*****
	(0.096)	**(0.096)**	**(0.047)**	**(0.046)**
Constant	21.594***	19.421***	47.895***	48.147***
	(0.153)	(0.151)	(0.002)	(0.002)
Observations	91,306	91,306	3,279,335	3,279,335
R^2	0.936	0.935	0.864	0.839

All specifications include student and school-grade-year fixed effects. Standard errors clustered at student level.
* p<0.10, ** p<0.05, *** p<0.01

Table A8. Extended Two-Way Fixed Effects Estimates of Special Education on Math and Reading Score Percentiles with BLL Heterogeneity Excluding Students Who Ever Repeated a Grade

	(1) Math Percentile, Not-yet-treated control	(2) Reading Percentile, Not-yet-treated control	(3) Math Percentile, Not-yet-treated & never-treated control	(4) Reading Percentile, Not-yet-treated & never-treated control
4th grade cohort, 4th gr	1.801*	2.175**	1.502***	1.984***
	(1.035)	(1.019)	(0.363)	(0.358)
4th grade cohort, 5th gr	3.554***	3.547***	2.762***	3.650***
	(1.348)	(1.339)	(0.403)	(0.415)
4th grade cohort, 6th gr	4.712***	6.265***	3.034***	3.722***
	(1.732)	(1.677)	(0.471)	(0.460)
4th grade cohort, 7th gr	5.530**	8.847***	3.540***	5.173***
	(2.253)	(2.479)	(0.520)	(0.520)
4th grade cohort, 8th gr			4.727***	5.354***
			(0.612)	(0.596)
5th grade cohort, 5th gr	1.686	2.332*	0.782*	1.149***
	(1.250)	(1.250)	(0.427)	(0.405)
5th grade cohort, 6th gr	4.098**	7.335***	2.076***	2.917***
	(1.702)	(1.638)	(0.509)	(0.470)
5th grade cohort, 7th gr	3.585	9.057***	1.887***	3.765***
	(2.265)	(2.456)	(0.570)	(0.538)
5th grade cohort, 8th gr			2.938***	4.170***
			(0.667)	(0.642)
6th grade cohort, 6th gr	1.343	3.235*	0.616	0.256
	(1.720)	(1.679)	(0.566)	(0.523)
6th grade cohort, 7th gr	1.755	3.779	1.043	0.504
	(2.274)	(2.480)	(0.643)	(0.616)
6th grade cohort, 8th gr			3.890***	1.142
			(0.772)	(0.707)
7th grade cohort, 7th gr	-0.371	1.357	-0.612	-0.495
	(2.214)	(2.407)	(0.747)	(0.770)

7^{th} grade cohort, 8^{th} gr			2.164**	0.085
			(0.940)	(0.888)
8^{th} grade cohort, 8^{th} gr			-0.395	-1.256
			(0.911)	(0.937)
4^{th} grade cohort, 4^{th} gr X BLL	**0.257**	**0.143**	**0.159***	**-0.031**
	(0.253)	**(0.261)**	**(0.095)**	**(0.094)**
4^{th} grade cohort, 5^{th} gr X BLL	**0.063**	**0.233**	**-0.020**	**-0.058**
	(0.319)	**(0.327)**	**(0.103)**	**(0.106)**
4^{th} grade cohort, 6^{th} gr X BLL	**0.009**	**0.230**	**-0.044**	**-0.005**
	(0.403)	**(0.391)**	**(0.116)**	**(0.115)**
4^{th} grade cohort, 7^{th} gr X BLL	**0.068**	**0.129**	**-0.208***	**-0.100**
	(0.539)	**(0.577)**	**(0.124)**	**(0.126)**
4^{th} grade cohort, 8^{th} gr X BLL			**-0.093**	**-0.008**
			(0.145)	**(0.141)**
5^{th} grade cohort, 5^{th} gr X BLL	**0.134**	**0.143**	**0.153**	**0.107**
	(0.299)	**(0.298)**	**(0.108)**	**(0.104)**
5^{th} grade cohort, 6^{th} gr X BLL	**-0.109**	**-0.457**	**0.003**	**-0.153**
	(0.393)	**(0.380)**	**(0.125)**	**(0.115)**
5^{th} grade cohort, 7^{th} gr X BLL	**0.201**	**-0.526**	**-0.020**	**-0.209**
	(0.541)	**(0.566)**	**(0.137)**	**(0.128)**
5^{th} grade cohort, 8^{th} gr X BLL			**0.068**	**-0.035**
			(0.156)	**(0.154)**
6^{th} grade cohort, 6^{th} gr X BLL	**-0.046**	**-0.013**	**-0.038**	**0.042**
	(0.398)	**(0.386)**	**(0.136)**	**(0.126)**
6^{th} grade cohort, 7^{th} gr X BLL	**0.178**	**0.312**	**-0.127**	**0.132**
	(0.540)	**(0.574)**	**(0.158)**	**(0.151)**
6^{th} grade cohort, 8^{th} gr X BLL			**-0.377****	**0.061**
			(0.179)	**(0.169)**
7^{th} grade cohort, 7^{th} gr X BLL	**0.399**	**0.314**	**-0.035**	**0.150**
	(0.528)	**(0.559)**	**(0.168)**	**(0.176)**

7th grade cohort, 8th gr X BLL			**-0.188**	**0.220**
			(0.214)	**(0.212)**
8th grade cohort, 8th gr X BLL			**0.059**	**0.158**
			(0.200)	**(0.198)**
4th grade X BLL	-0.098	-0.121	-0.033***	-0.071***
	(0.174)	(0.173)	(0.013)	(0.014)
5th grade X BLL	-0.183	-0.126	-0.065***	-0.062***
	(0.247)	(0.247)	(0.014)	(0.015)
6th grade X BLL	0.080	0.029	-0.050***	-0.101***
	(0.333)	(0.322)	(0.015)	(0.016)
7th grade X BLL	-0.362	-0.101	-0.060***	-0.104***
	(0.482)	(0.518)	(0.016)	(0.017)
8th grade X BLL			-0.039**	-0.109***
			(0.017)	(0.018)
Constant	21.813***	18.593***	49.354***	49.653***
	(0.683)	(0.684)	(0.033)	(0.034)
Observations	79721	79721	3114453	3114453
R^2	0.941	0.942	0.862	0.836

Bolded coefficients represent key BLL interaction terms. All specifications include student and school-grade-year fixed effects. Standard errors clustered at student level. * p<0.10, ** p<0.05, *** p<0.01

Table A9: Average treatment effects of special education and special education x BLL interaction on math and reading percentiles excluding students who ever repeated a grade

	(1) Math Percentile, Not-yet-treated control	(2) Reading Percentile, Not-yet-treated control	(3) Math Percentile, Not-yet-treated & never-treated control	(4) Reading Percentile, Not-yet-treated & never-treated control
Average treatment effect of special education on the treated	3.40*** (0.51)	4.94*** (0.50)	2.12*** (0.11)	2.62*** (0.10)
Average BLL x special education interaction effect	0.11 (0.24)	0.06 (0.25)	-0.02 (0.05)	-0.01 (0.05)
Joint test for all BLL interaction coefficients = 0 (Prob > F)	0.96	0.33	0.24	0.69

Appendix Table A7 presents the full coefficient estimates used to derive these results. * p<0.10, ** p<0.05, *** p<0.01. Standard errors estimated using the delta method.

Table A10: Extended Two-Way Fixed Effects Estimates of Special Education on Math and Reading Z-Scores with BLL Heterogeneity

	(1) Math Z-score, Not-yet-treated control	(2) Reading Z-score, Not-yet-treated control	(3) Math Z-score, Not-yet-treated & never-treated control	(4) Reading Z-score, Not-yet-treated & never-treated control
4th grade cohort, 4th gr	0.073*	0.079	0.060***	0.062***
	(0.039)	(0.051)	(0.014)	(0.021)
4th grade cohort, 5th gr	0.115**	0.126**	0.111***	0.150***
	(0.050)	(0.059)	(0.016)	(0.022)
4th grade cohort, 6th gr	0.176***	0.251***	0.147***	0.156***
	(0.062)	(0.071)	(0.018)	(0.023)
4th grade cohort, 7th gr	0.239***	0.336***	0.165***	0.216***
	(0.081)	(0.097)	(0.019)	(0.024)
4th grade cohort, 8th gr			0.221***	0.240***
			(0.022)	(0.026)
5th grade cohort, 5th gr	0.030	0.076	0.024	0.064***
	(0.047)	(0.052)	(0.016)	(0.019)
5th grade cohort, 6th gr	0.130**	0.309***	0.086***	0.114***
	(0.060)	(0.067)	(0.019)	(0.020)
5th grade cohort, 7th gr	0.145*	0.373***	0.088***	0.164***
	(0.079)	(0.094)	(0.021)	(0.022)
5th grade cohort, 8th gr			0.142***	0.202***
			(0.024)	(0.027)
6th grade cohort, 6th gr	0.076	0.174***	0.085***	0.030
	(0.060)	(0.067)	(0.022)	(0.023)
6th grade cohort, 7th gr	0.118	0.197**	0.102***	0.059**
	(0.080)	(0.095)	(0.024)	(0.027)
6th grade cohort, 8th gr			0.198***	0.112***
			(0.028)	(0.030)
7th grade cohort, 7th gr	0.020	0.048	-0.006	-0.026
	(0.079)	(0.093)	(0.028)	(0.034)

7^{th} grade cohort, 8^{th} gr			0.105*** (0.036)	0.004 (0.035)
8^{th} grade cohort, 8^{th} gr			0.078** (0.037)	0.002 (0.039)
4^{th} grade cohort, 4^{th} gr X BLL	**0.018*** **(0.010)**	**0.016** **(0.012)**	**0.008**** **(0.004)**	**0.001** **(0.005)**
4^{th} grade cohort, 5^{th} gr X BLL	**0.015** **(0.012)**	**0.024*** **(0.014)**	**-0.000** **(0.004)**	**-0.004** **(0.005)**
4^{th} grade cohort, 6^{th} gr X BLL	**0.008** **(0.015)**	**0.020** **(0.016)**	**-0.003** **(0.004)**	**0.003** **(0.005)**
4^{th} grade cohort, 7^{th} gr X BLL	**0.008** **(0.019)**	**0.029** **(0.023)**	**-0.006** **(0.005)**	**-0.003** **(0.006)**
4^{th} grade cohort, 8^{th} gr X BLL			**-0.004** **(0.005)**	**-0.005** **(0.006)**
5^{th} grade cohort, 5^{th} gr X BLL	**0.018** **(0.011)**	**0.017** **(0.012)**	**0.011**** **(0.004)**	**0.001** **(0.005)**
5^{th} grade cohort, 6^{th} gr X BLL	**0.005** **(0.014)**	**-0.013** **(0.015)**	**0.006** **(0.005)**	**0.001** **(0.005)**
5^{th} grade cohort, 7^{th} gr X BLL	**0.013** **(0.018)**	**-0.007** **(0.023)**	**0.004** **(0.005)**	**-0.007** **(0.005)**
5^{th} grade cohort, 8^{th} gr X BLL			**0.007** **(0.006)**	**-0.009** **(0.007)**
6^{th} grade cohort, 6^{th} gr X BLL	**-0.002** **(0.014)**	**-0.009** **(0.015)**	**-0.010*** **(0.005)**	**-0.001** **(0.005)**
6^{th} grade cohort, 7^{th} gr X BLL	**0.008** **(0.019)**	**0.009** **(0.023)**	**-0.008** **(0.006)**	**-0.006** **(0.007)**
6^{th} grade cohort, 8^{th} gr X BLL			**-0.013**** **(0.007)**	**-0.012** **(0.007)**
7^{th} grade cohort, 7^{th} gr X BLL	**0.017** **(0.019)**	**0.027** **(0.023)**	**0.002** **(0.006)**	**0.004** **(0.008)**
7^{th} grade cohort, 8^{th} gr			**-0.001**	**0.009**

X BLL			**(0.008)**	**(0.008)**
8th grade cohort, 8th gr X BLL			**-0.009**	**-0.007**
			(0.008)	**(0.008)**
4th grade X BLL	-0.010	-0.010	-0.001***	-0.004***
	(0.006)	(0.008)	(0.000)	(0.001)
5th grade X BLL	-0.020**	-0.014	-0.002***	-0.004***
	(0.009)	(0.010)	(0.000)	(0.001)
6th grade X BLL	-0.008	0.005	-0.001***	-0.005***
	(0.012)	(0.013)	(0.001)	(0.001)
7th grade X BLL	-0.018	-0.014	-0.003***	-0.005***
	(0.017)	(0.021)	(0.001)	(0.001)
8th grade X BLL			-0.003***	-0.006***
			(0.001)	(0.001)
Constant	-1.015***	-1.185***	-0.044***	-0.017***
	(0.024)	(0.028)	(0.001)	(0.001)
Observations	91,306	91,306	3,279,335	3,279,335
R^2	0.929	0.921	0.857	0.817

Bolded coefficients represent key BLL interaction terms. All specifications include student and school-grade-year fixed effects. Standard errors clustered at student level.
* p<0.10, ** p<0.05, *** p<0.01

Table A11: Average treatment effects of special education and special education x BLL interaction on math and reading z-scores

	(1) Math z-score, Not-yet-treated control	(2) Reading z-score, Not-yet-treated control	(3) Math z-score, Not-yet-treated & never-treated control	(4) Reading z-score, Not-yet-treated & never-treated control
Average treatment effect of special education on the treated	0.16*** (0.02)	0.24*** (0.02)	0.11*** (0.004)	0.11*** (0.005)
Average BLL x special education interaction effect	0.01 (0.01)	0.01 (0.01)	0.0005 (0.002)	-0.002 (0.003)
Joint test for all BLL interaction coefficients = 0 (Prob > F)	0.53	0.15	0.04	0.62

Appendix Table A10 presents the full coefficient estimates used to derive these results. * p<0.10, ** p<0.05, *** p<0.01. Standard errors estimated using the Delta method