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Abstract

Private tutoring chains like Kumon and Sylvan have expanded rapidly. Combining business location, household spending, and test score data, we find nearly half of tutoring users come from the top income decile, with Asian and White households the heaviest users. A staggered differences-in-differences design shows the opening of a district's first chain center raises district-wide math and reading scores. Math gains are largest for White and Asian students, widening racial achievement gaps, though less so in more integrated districts. These private investments help their users, deepen educational inequality, and shape outcomes by which accountability systems and families judge school quality.

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1 Introduction

Private tutoring centers have grown into a common feature of K-12 education in the United States. Chains like Kumon, Sylvan, and Mathnasium have expanded from a few hundred U.S. locations in the late 1990s to several thousand today, and many families now pay for instruction outside the regular school day. Despite this growth, remarkably little is known about who uses these centers or whether they improve student achievement. This gap matters for at least three reasons. First, a growing body of evidence suggests that intensive, individualized tutoring can substantially raise student achievement, making private tutoring centers a potentially important and scalable complement to or substitute for in-school instruction. Second, if access to this kind of instruction is unevenly distributed, private tutoring centers could widen existing achievement gaps.Third, these private investments may affect measures of school outcomes by which states and families judge school effectiveness, confusing the impact of public and private educational inputs. This paper provides the first comprehensive evidence on private tutoring center usage and impact across the United States. We ask two questions: Who uses chain tutoring centers? And what effect does their entry into a community have on student achievement?

We begin with descriptive analysis of tutoring centers and the families who use them. Business registry records from Data Axle provide 26 years of tutoring center locations across the United States. We focus on national chains, which are easiest to identify and about which we best understand the treatment. The business registry data show that chain tutoring centers have grown substantially since the late 1990s, expanding from roughly 500 locations in 1997 to a peak of over 4,000 by the mid-2010s. Of the over 7,000 unique center locations during our main period of interest, Kumon accounts for 38 percent, Sylvan Learning 25 percent, Mathnasium 10 percent, and Huntington Learning 10 percent, together representing over four-fifths of all chain locations in our data. A few chains focus solely on math instruction, and math appears to dominate the business model for chains offering both math and reading support. The typical center likely serves 160 to 170 students at a time, with the typical student spending two to three hours per week at the center or doing center-assigned homework.

The Consumer Expenditure Survey (CEX) allows us to understand who uses tutoring centers by measuring household spending on tutoring by geography, race, and income. Household spending

data suggests tutoring usage is rare overall but sharply stratified by income and race. Just 1.5 percent of households with school-age children report any monthly tutoring spending, averaging about $270 among spenders. Asian households are far more likely to use tutoring than any other group, with 4.5 percent spending on tutoring, triple the national rate. The fractions of White, Black, and Hispanic households who spend on tutoring are 1.7, 1.2, and 0.7 percent respectively. This racial gap is driven largely but not entirely by income. Top-decile households account for 46 percent of households who report spending on tutoring. Among top-decile households, White, Black, and Hispanic usage is roughly 4-6 percent, while Asian usage remains far higher at nearly 9 percent. Conditional on income at all levels, White, Black and Hispanic households have roughly similar spending patterns while Asian families are more likely to spend on tutoring. Geographically linking spending to local tutoring chain density shows that tutoring expenditures track the availability of physical chain centers, but only among White, Asian and above median income households. This suggests that chain centers in particular, rather than tutoring broadly, are disproportionately used by these groups.

To estimate the impact of tutoring centers on student outcomes, we link center locations to test score data and implement a staggered difference-in-difference approach. Test score data come from the Stanford Education Data Archive (SEDA), which provides standardized test score outcomes for nearly all school districts in the country, including by demographic subgroups. We match each tutoring center location to a school district in SEDA and merge center counts in a given calendar year with test scores from that year's spring. We then exploit variation in the timing of a district's first chain center opening, comparing districts that gain a first chain between 2010 and 2019 to districts that never have a chain center. Because staggered treatment timing can generate biased estimates under standard two-way fixed effects approaches with heterogeneous treatment effects, we use the two-stage difference-in-differences estimator of Gardner et al. (2024). The first stage estimates district-by-grade and year fixed effects using only never-treated and not-yet-treated observations, and the second stage regresses each observation's residual outcome (net of these fixed effects) on event-time indicators. This approach yields estimates of chain centers at each year relative to first opening that are not contaminated by comparisons to already-treated units.

Chain tutoring centers have a positive, growing, and persistent impact on student achievement. Following the opening of a district's first chain center, overall math test scores rise by 0.029 standard

deviations and reading scores by 0.016 standard deviations, with both effects growing over the five years following entry. Event-study estimates show flat pre-trends for both subjects, supporting the parallel trends assumption underlying our design. These results are robust to controlling for grade-level enrollment and racial composition and for district-level unemployment and median income, with controlled estimates of 0.034 standard deviations for math and 0.021 for reading. The relative size of the math and reading effects is roughly proportional to our best evidence on the fraction of students using tutoring centers for those subjects, suggesting that such differences may be accounted for by differences in implied first stage impacts on usage rates.

This overall effect masks substantial racial heterogeneity and is driven largely by White and Asian students. Math scores for White and Asian students rise by 0.040 standard deviations following chain entry, while effects for Black, Hispanic, and Native American students are statistically indistinguishable from zero. Consistent with this divergence, chain entry widens within-district test score gaps, increasing the White-Black math gap by 0.038 standard deviations and the White-Hispanic gap by 0.020 standard deviations. These gap widening effects are substantially larger in more racially segregated districts (0.047 standard deviations for the White-Black gap in highly segregated districts versus 0.023 in less segregated ones), consistent with a within-school spillover mechanism in which the benefits of tutoring also accrue to classmates less likely to be using tutoring centers.

In contrast to this racial heterogeneity, chain tutoring's effects show less variation along other dimensions. Evidence for heterogeneity by student economic status is mixed, with some specifications showing little heterogeneity and others showing larger effects for non-economically disadvantaged students. Effects are comparable for male and female students and for those in grades 3-5 versus grades 6-8. Non-chain tutoring centers and chains that provide in-home tutors show statistically insignificant effects on math and reading test scores across subgroups, suggesting that the achievement gains we document are specific to brick-and-mortar chain centers rather than a general feature of private tutoring.

Our headline estimates are robust to alternative empirical choices. In addition to being robust to controls for demographic and economic shifts, our results remain when: weighting by district size; limiting the set of comparison districts to "nearest neighbors" most similar at baseline to treated districts; using an alternative geographic definition of treatment, in which we define district

exposure as having a chain center within 10 miles of the district's center rather than within district boundaries; changing the extent to which the panel can be unbalanced; and replacing the two-stage difference-in-differences estimator with a standard two-way fixed effects approach or with a Callaway-Sant'Anna approach.

These effects are large enough to account for much of how treated districts' test scores actually evolved over our sample period. Our estimated center-entry effects explain nearly two-thirds of districts' improvement in math scores from 2009 to 2019; the analogous figure for reading is just over half. A similar exercise attributes to chain entry about half of White and Asian students' math gains and more than 40 percent of the increase in the within-district White-Black math gap. Private investments made outside the public system thus appear to have non-trivial effects on the outcomes by which districts are judged by families and state accountability systems. The tutoring that generates our district-average effect costs on the order of $75 per district pupil, an order of magnitude less than the public school spending Jackson and Mackevicius (2024) find is required to produce a comparable test-score gain, albeit for a different subset of students and for costs borne privately by families rather than by school systems.

How large are these effects on the students who actually attend these centers? Because our district-level estimates average over the many students who never enroll, the implied effect on tutored students is necessarily larger. Scaling our intent-to-treat estimates by the share of students induced to take up tutoring yields a per-student math effect of roughly 0.3-0.4 standard deviations when assuming perfect persistence of tutoring gains, or 0.5 standard deviations assuming partial persistence of gains. Reading effects are roughly 0.4-0.7 standard deviations depending on persistence assumptions. These estimates are on the higher end of the range of estimates from meta-analyses of the tutoring literature, where the average program raises achievement by about 0.4 standard deviations (Nickow et al., 2024) and the typical program at scale has effects closer to 0.2 standard deviations (Kraft et al., 2024). This is consistent with centers being particularly effective for the students who use them, with true take-up rates exceeding our best estimates, or with the existence of spillover effects to non-tutored peers in the school district.

This paper relates to three strands of research. First and most narrowly, it contributes to a small literature documenting the expansion and educational impacts of private tutoring centers. Kim et al. (2025) document the rapid recent expansion of U.S. tutoring centers, showing that

they tend to locate in higher-income, heavily Asian-American areas. We corroborate this pattern with household spending data and build on it using a causal design. The cleanest causal evidence on the efficacy of tutoring center curricula comes from a randomized controlled trial conducted in Bangladeshi primary schools, where Kumon directly provided instructional materials matching those provided in its U.S. centers. Sawada et al. (2024) find that this self-directed, mastery-based approach generated large gains in math achievement among participants, while a companion paper, Kawarazaki et al. (2023), shows positive peer effects. Most closely related to our paper is contemporaneous work by Chen (2026), who studies the expansion of private tutoring centers in Texas. He finds that access to such centers raises SAT and ACT scores, shifts students toward more selective and out-of-state colleges, and increases early-career earnings. The effects are entirely concentrated among non-economically disadvantaged students, who his original survey evidence suggests are more likely to take up tutoring. Whereas Chen (2026) focuses on one large state, college entrance exams, and postsecondary outcomes, our paper complements this evidence by studying the national landscape of chain tutoring centers and their impact on primary and middle grades academic skills, including those measured by state standardized exams unlikely to be a particular focus of tutoring center curricula.

Our paper also speaks to the broader literature on effective approaches to improving academic achievement, including other forms of tutoring. Tutoring centers' efficacy is likely due to some combination of the individualized instruction they provide and the increased instructional time they entail for students. Meta-analyses document the particular effectiveness of small group and one-on-one tutoring (Nickow et al., 2024; Fryer Jr, 2017; Kraft et al., 2024), a finding exemplified by recent experimental evaluations (Carlana and La Ferrara, 2025; Guryan et al., 2023; Kraft et al., 2022). Computer-assisted learning that personalizes instruction has also garnered attention for its positive impacts (Escueta et al., 2020; Muralidharan et al., 2019; Bhatt et al., 2024; Oreopoulos et al., 2024), as have "Teaching to the Right Level" and other interventions targeting curricula to students' ability (Banerjee et al., 2017; Duflo et al., 2024). Evidence that instructional time is also a key input into student achievement comes from quasi-experimental variation in individual absences (Aucejo and Romano, 2016; Liu et al., 2021; Goodman, 2014), policy-driven changes to daily or weekly school calendars (Figlio et al., 2018; Thompson, 2021), descriptive evidence from international achievement comparisons (Lavy, 2015; Rivkin and Schiman, 2015), and both

descriptive and experimental evidence showing that longer school days and years may partly explain charter school efficacy relative to traditional public schools (Angrist et al., 2013; Fryer Jr, 2014). Kraft and Novicoff (2024) conclude from this literature that the impact of one additional hour of instruction per week in a specific subject is roughly 0.02-0.07 standard deviations. Even doubling or tripling this figure to account for the typical weekly time students spend at tutoring centers yields a smaller effect than implied by our estimates, suggesting that tutoring centers may be particularly effective relative to typical instructional time.

Finally, this paper addresses the growing inequality in child development driven by unequal family investment. Tutoring centers are one example of the increasing temporal and financial investments high-income families are making in their children. Ramey and Ramey (2010) argue that competition for scarce college slots generates a "rug rat race", explaining why time spent by parents on childcare in the U.S. began rising dramatically in the mid-1990s, particularly among college-educated parents. Doepke and Zilibotti (2019) argue that increasing inequality in some countries, such as the United States, drives parents to push their children harder to ensure their success, transforming the hands-off parenting of the 1960s and '70s into something more like helicopter parenting with a full schedule of after-school activities. Blazar et al. (2026) find that average per-child investments from age 5 to 18 across all domains are 12 percent lower for bottom vs. top income quartile households, while Hastings (2026) shows that, since 1980, education spending among top income deciles families nearly quintupled while barely changing among lower income families. Our paper contributes to this literature by causally identifying private tutoring centers as one concrete channel through which these investment gaps translate into widening academic achievement gaps.

2 Tutoring Centers

Private tutoring centers in the United States operate under a variety of instructional models but share a common structure as for-profit supplemental education providers serving K–12 students outside of school hours. We focus our discussion on the chains most prevalent in our data: Kumon, Sylvan Learning, Mathnasium, and Huntington Learning Center.

Kumon, founded in Japan in 1954 and the largest chain in the sample by a substantial margin, operates on a self-directed mastery model in which students work independently through printed

worksheets calibrated to their current level, advancing only upon demonstrating both accuracy and speed. Instructors in a Kumon center do not teach in the conventional sense. They circulate, grade, and monitor, while the structured worksheet sequence serves as the primary vehicle of instruction. Students are also expected to complete daily homework packets at home, making consistent parental involvement a prerequisite for the program's intended effect. Parents must both transport their child to a center at least once weekly and supervise daily at-home practice on the remaining days. Kumon offers both math and reading curricula.

Sylvan Learning, the next largest chain, uses a more instructor-focused approach. It employs certified instructors who deliver direct instruction through a proprietary technology-assisted curriculum, with sessions conducted in small groups of two to three students. Sylvan's curriculum is deliberately aligned to local school standards, and instructors are encouraged to communicate with students' classroom teachers, positioning Sylvan less as an independent enrichment program and more as a school-adjacent intervention. Like Kumon, Sylvan begins with a diagnostic assessment and constructs an individualized learning plan, but the mechanism of instruction differs. Kumon relies on the student to work through material independently while Sylvan relies on the instructor to actively teach it. Parental involvement at Sylvan is more limited than at Kumon, as the program does not assign daily homework packets. Parents are primarily responsible for transporting their child to center sessions two to three times per week and attending periodic progress meetings with the center director. Sylvan offers both math and reading curricula.

Mathnasium focuses exclusively on math, with a proprietary instructional method delivered through verbal, visual, and written exercises by instructors trained in the "Mathnasium Method." Like Sylvan, it operates physical centers and serves multiple students simultaneously, but each student follows an individualized plan rather than a shared curriculum sequence. Mathnasium explicitly discourages students from bringing school homework to sessions, positioning itself as a standalone math curriculum rather than a supplement to classroom assignments. Parental involvement is limited to transportation and occasional progress check-ins. Huntington Learning Center, one of the oldest chains in the sample, similarly offers center-based tutoring and is notable for having sought formal accreditation for its locations, an unusual step in the supplemental education sector that signals a closer alignment with institutional schooling norms than most competitors pursue.

Despite their differences, these chains share several structural features relevant to understanding their potential effects on student achievement. All administer diagnostic assessments at intake and construct individualized learning plans; all focus primarily or exclusively on math and reading, the subjects measured in our outcome data; and all operate as franchise systems in which local owners hire and manage instructors under a corporate curriculum and brand. The standardized curricula, instructor training requirements, and quality oversight mechanisms embedded in franchise agreements may partly explain why chain centers produce measurable achievement gains while independent tutoring centers do not.

Franchise disclosure documents and press releases give us additional insight into the number of students using each center, which will be helpful for later consideration of treatment effect magnitudes. The clearest evidence exists for Kumon and comes from two approaches drawn from its franchise disclosure documents. The first infers enrollment from the fact that Kumon levies a fixed royalty per enrolled student per subject, so that dividing reported royalties by the contractual rate and the number of operating centers recovers average enrollment directly. The second aggregates the enrollment figures Kumon discloses for centers of different ages, weighting mature and newly opened centers by their shares of the system. The two approaches agree closely, placing average enrollment at roughly 166 subject-enrollments per center in 2011.[1] Another data point comes from a 2014 Kumon press release claiming its 1,492 centers served 265,000 U.S. students, implying about 178 students per center.[2] Applying the same royalty calculation to the most recent available disclosure implies about 150 subject-enrollments per Kumon center in 2023. Data on center average enrollments for the other chains are less clear.

The best evidence we have suggests that math is a larger focus of these centers than reading. For Kumon, three pieces of evidence suggest 60 to 75 percent of Kumon tutoring is focused on math. First, to become a fully licensed franchisee, a Kumon center must cumulatively enroll 600 math students and 400 reading students, suggesting 60 percent of its focus is math. Second, a Kumon franchising executive we spoke to suggested that 70 percent of current U.S. Kumon tutoring is in math, suggesting that roughly twice as many students use Kumon for math as for reading support.

[1] A 2012 franchise disclosure document has the most complete information we can obtain during the 2009-19 period of our testing data.

[2] That figure likely includes students using Kumon for only part of the year, explaining why it is higher than the franchise disclosure document calculation, which assumes full-year participation. See the press release at: `https://www.kumonfranchise.com/us-en/assets/pressrelease/release2014-0820.html`.

Third, Google Trends suggests that, from 2009-19, online searches for "Kumon math" were twice as frequent as for "Kumon reading", consistent with two-thirds of Kumon tutoring involving math. Online searches for "Sylvan math" were 4-5 times more frequent than for "Sylvan reading", while searches for "Huntington math" and "Huntington reading" were similar. Mathnasium only provides math tutoring.

The typical tutored student appears to spend roughly two to three hours per week in these centers or doing center-assigned homework. Franchise disclosure documents give a partial picture of weekly instructional intensity. Kumon states that students attend the center twice weekly for 20 to 30 minutes per subject, and additionally assigns homework on the five non-Center days each week. Assuming 20 minutes of daily homework per subject, consistent with the length of a typical Kumon worksheet set, a single-subject Kumon student spends roughly 50 minutes in-center plus 100 minutes on homework, for a total of about 2.5 hours per week. Mathnasium, by contrast, discloses that students typically attend twice weekly for approximately 60 minutes per session, or about 2 hours per week, with no homework component. Sylvan and Huntington do not disclose comparable attendance figures but their marketing materials suggest a similar order of magnitude to Mathnasium.[3]

The typical current cost of these chain centers' services is a few thousand dollars a year, with some variation by chain model. The annual cost of a single subject of chain tutoring is best documented for Kumon, whose centers post tuition on their official pages and bill a flat monthly fee per subject. Recently posted U.S. prices run from $150 to $245 per subject per month, implying total costs over a nine-month school year of roughly $1,350 to $2,200 for one subject. Mathnasium, which teaches only math, charges a monthly membership of about $250 to $400, or $2,250 to $3,600 over a nine-month school year. Sylvan and Huntington charge by the hour, at rates of roughly $45 per hour for Sylvan and $40-$75 for Huntington. At two hours per week over a 36-week school year, those rates imply roughly $3,000 to $5,000 annually. A year of single-subject tutoring therefore costs about $2,000 at Kumon and $3,000 to $5,000 at the more instruction-intensive chains. These

[3]Sylvan's website and individual center pages recommend two to three one-hour sessions per week (`https://www.sylvanlearning.com/how-sylvan-works/faqs/`), implying roughly 2–3 hours per week with no structured homework component. Huntington's website recommends at least two sessions per week during the school year (`https://huntingtonhelps.com/why-huntington/`), and states explicitly that homework is not assigned for general academic tutoring (`https://huntingtonhelps.com/resources/faqs/`). At roughly one hour per session, this implies about 2 hours per week during the school year.

costs vary over time and geography, and with the intensity and duration of a given student's use of such services.

3 Data

Our descriptive analysis primarily relies on historical data on tutoring center locations, from Data Axle, and household spending data, from the Consumer Expenditure Survey. Our causal analysis relies on the tutoring center location data combined with data on student test scores from the Stanford Education Data Archive. We describe these three main data sources in further detail.

3.1 Tutoring Centers

We measure the locations of private tutoring centers using Data Axle's Historical Business Data, which collects information on businesses sourced and verified by yellow page directories, credit card billing data, phone verification, web research, news publications, and annual reports (Kim et al., 2025). We obtain records from 1997-2022, observing businesses' names, addresses, and industry codes. Each business included in the data represents a unique physical location, with identifiers that allow the business to be tracked across years.

We define tutoring centers as businesses labeled either "Tutoring" (SIC Code 829909) or "Test Preparation Instruction" (SIC Code 874868). We exclude the 5 percent of observations where the firm name clearly indicates a focus on college test-preparation, graduate, ESL, or non-academic (e.g. music) tutoring. We define chains as 10 or more unique locations that share a parent company (as identified by Data Axle) or a common name. We identify those common names primarily through our own inspection, supplemented with the use of Claude Sonnet 4.6 to catch repeated name patterns that we missed. We further restrict our main analysis to chains that provide in-center tutoring; we analyze chains that provide in-home tutors in a later sub-analysis. Of the initial set of 20,337 unique tutoring locations across all years, 35 percent (7,071) are identified as chains providing in-center tutoring services.

The business registry data show that chain tutoring centers have grown substantially since the late 1990s. As Figure 1 shows, the number of such centers grew from roughly 500 locations in 1997 to a peak of over 4,000 by the mid-2010s, plateauing in more recent years. Kumon is by far the

largest of these chains and has grown the most over this time, with Sylvan Learning, Mathnasium, and Huntington Learning being the next largest chains. Table 1 provides more details on these chains during the 2009-19 period for which we have test score data. Of the over 7,000 unique center locations during that period, Kumon accounts for 38 percent, Sylvan Learning 25 percent, Mathnasium 10 percent, and Huntington Learning 10 percent. Those four chains together represent over four-fifths of all chain locations, with 19 other identified chains comprising the remainder. All of the largest chains but Mathnasium offer both math and reading instruction, with some smaller chains specializing in either math or reading.

In the mid-to-late 2000s, some of this growth in chain tutoring centers was driven by the 2002 No Child Left Behind Act's Supplemental Educational Services (SES) provision, which required Title I schools missing academic targets to fund free private tutoring for eligible students using up to 20% of their Title I allocation. Beginning in 2011, the Obama administration granted states flexibility waivers that let districts opt out of the SES mandate. By 2013, most states had taken up these waivers, sharply curtailing SES demand. By 2015, NCLB had been formally repealed and replaced by the Every Student Succeeds Act, eliminating the SES mandate altogether. Also noteworthy is that the growth in tutoring chains documented here did not appear to come at the expense of non-chains, which grew substantially over this period (Kim et al., 2025).

3.2 Consumer Expenditure Survey

We measure household-level consumption of tutoring with the Consumer Expenditure Survey (CEX), a nationally representative survey conducted by the U.S. Census Bureau that tracks household spending and characteristics. Beginning in 2009, the CEX includes a dedicated expenditure category for "Test preparation or tutoring services." Using CEX public-use microdata from 2009 to 2019 (U.S. Bureau of Labor Statistics, 2024), we construct two measures of monthly tutoring usage: (1) a binary indicator for whether the household reports any spending on tutoring services in a month, and (2) the amount spent on tutoring services, inflation-adjusted to 2015 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). We note that these measures capture expenditures on all forms of tutoring (in-center, in-home, online), not just tutoring at the physical locations of chain centers, which are the focus of this study. We further record the race/ethnicity of the household reference person and the household's income percentile. To align measures of tutor-

ing expenditure with academic-year outcomes, we restrict the CEX to households with at least one child ages 6-17 and exclude spending reported in June, July, and August. This restriction yields a final sample of 138,055 household-month observations representing 56,624 unique households.

3.3 School District Test Scores

Our primary outcomes are district-level math and reading standardized test scores from the Stanford Education Data Archive (SEDA) (Reardon et al., 2024). SEDA provides mean test scores by school district for grades 3–8 over the period 2009–2019, including breakdowns by racial and ethnic subgroup, gender, and economic disadvantage status.[4] Scores are reported using SEDA's Cohort Scale, which enables consistent comparisons within a grade across districts and over time, expressed in standard deviations of the national distribution.

We match each tutoring center to a school district using 2019 district boundary shapefiles from SEDA, yielding a consistent geographic panel. Across all tutoring centers in the United States, 99.9 percent are successfully matched to a school district, with the remainder either lacking precise location data or falling outside any district boundary. We merge center counts in a given calendar year with test scores from the spring of that academic year. Center counts in 2014, for example, are matched to spring 2014 scores (i.e. from the 2013-14 school year). Because we observe only the calendar year of tutoring center entry rather than the precise date, we are uncertain whether a center opening in a given year was available to students prior to standardized testing. The year of center entry thus has ambiguous treatment status, whereas those before and after the entry year can be cleanly categorized as pre- and post-test administration. SEDA also provides school district characteristics that we later use as controls in some specifications, including total enrollments by grade, the racial composition of the test-taking population, the local unemployment rate, and the district's median household income.

4 Who Uses Tutoring Services?

Before causally estimating the impacts of tutoring centers on student achievement, we describe tutoring consumption in the United States. Among households with school-age children, 1.5 percent

[4]We follow SEDA convention and refer to academic years by their spring term, so that 2009 refers to the 2008–09 academic year.

report spending on tutoring in a given month, as seen in Figure 2. Households that spend on tutoring spend on average $272 per month, or about $2,500 if totaled over the 9-month school year.[5] This total spending is in line with our estimates of the annual costs of chain centers. There is heterogeneity in tutoring use by race, with usage concentrated among Asian and, to a lesser extent, White families. The leftmost columns in Figure 2 show that 4.5 percent of Asian households spend on tutoring services each month, three times the national average. In comparison, 1.7 percent of White households consume tutoring services each month, while only 1.2 percent of Black households and 0.7 percent of Hispanic households do so.

High-income households are much more likely to spend money on tutoring services than other households. As shown in the second group of columns in Figure 2, among households in the top 10 percent of the income distribution, 4.6 percent consume tutoring in any given month, compared to 1.4 percent in the next 40 percent of the income distribution and 0.5 percent of households in the bottom half of the income distribution.[6] Nearly half (46 percent) of all households that spend money on tutoring are in the top income decile. Families who spend money on private school tuition are three times more likely to spend on tutoring services than families who do not pay for private schooling.

Racial differences in spending are largely explained by differences in income. Among households in the top 10% of the income distribution, a similar share of White, Black, and Hispanic households consume tutoring services, and such shares are also relatively similar in other parts of the income distribution. Asian households are the exception, with tutoring take-up rates more than twice as high as other groups even conditional on income. These findings are consistent with geographic patterns documented by Kim et al. (2025), who find tutoring centers are concentrated in high-income and heavily Asian-American areas, and by family survey data from Chen (2026), who finds White, Asian and high-income families more likely to report tutoring usage.

Household spending on tutoring services is geographically correlated with the prevalence of tutoring chain centers. The CEX data captures all forms of tutoring, not just chain centers, so we explore whether tutoring expenditures in that data correlate with geographic availability of chain centers. For each Metropolitan Statistical Area (MSA) in each year, we calculate the number

[5] See Panel A of Appendix Figure A.1 for details.

[6] See Panel B of Appendix Figure A.1 for details on tutoring spending across the full income distribution.

of tutoring chain centers per 1,000 children ages 5-17.[7] As seen in the left panels of Figure 3, binned scatter plots of household tutoring participation against MSA-year tutoring chain center density show strong positive relationships between the number of chains per 1,000 children and the share of households using tutoring services, indicating that our measure of tutoring participation in the CEX may capture, at least partly, the use of physical tutoring centers. This relationship is particularly pronounced among White and Asian households and above median income households, but is statistically insignificant for other households. As the right panels show, the magnitude of these geographic correlations remain high (though less precise) even when controlling for MSA and year fixed effects, implying that within-MSA changes in tutoring chain center prevalence predict within-MSA changes in overall tutoring spending in the CEX data. That the correlation between tutoring consumption and private tutoring center availability is driven almost entirely by White, Asian and higher income households suggests these are the families largely using tutoring centers.

What share of tutoring participation measured in the CEX can be explained by the capacity of local chain tutoring centers? Answering this question requires converting our household-level measure of tutoring participation into student-level participation. Because a household with one child can pay for tutoring for at most one child, we estimate the number of tutored children per household as average tutoring expenditure divided by average expenditure among one-child households. This ratio implies 1.11 tutored students per tutoring household. Multiplying by the share of households with any tutoring expenditure (2.17 percent among households in identified MSAs) and dividing by the number of school-aged children per household (1.62) yields an average participation rate of 1.49 percent of students per MSA-year. We then measure capacity in each MSA-year as the number of chain tutoring centers multiplied by 160 seats per center. On average, chain centers can serve 1.34 percent of students, or 90 percent of observed participation. The slope from regressing the number of tutored students on chain-center capacity at the MSA-year level is 1.06, so participation rises roughly one-for-one with capacity.

[7] MSAs, clusters of counties with high population density, are the finest level of geography available in the CEX, and CEX sampling is stratified at the MSA level. Prior to 2016, geography is available for households in the 21 largest MSAs. From 2016 onward, the 23 largest MSAs are available. Overall, 45% of households in the CEX can be matched to an MSA.

5 Causal Sample and Method

5.1 Sample Construction and Summary Statistics

We now turn our attention to estimating the impact of tutoring center entry on student achievement. Our primary analysis sample is the panel of U.S. public school districts from 2009 to 2019 for which we observe test score data in at least 10 of those 11 years. We observe test scores and demographic characteristics for students in grades 3 through 8. Column (1) of Table 2 reports summary statistics for all districts at the beginning of the sample period. In 2009, we observe over 40,000 unique district-grade observations across nearly 9,000 districts. The average district has just under 5,000 children aged 5 to 17 and enrolls 344 students per grade. Test scores are normalized relative to the national distribution, so the average math and reading scores across all districts are very close to the national mean. In the typical district, 75 percent of students are White, 2 percent are Asian, 44 percent are economically disadvantaged, and 23 percent attend suburban schools.

Our main empirical strategy estimates the effects of a private tutoring chain opening in a district for the first time. For each district, we identify the first school year in which a chain center operated within district boundaries.[8] We denote treated districts, the 400 or so that received their first chain center between 2010 and 2019, as "First Chain" districts. Control districts are "Never Chain", the nearly 7,000 that never had a chain center between 1997 and 2019.[9] There are 1,924 first-chain district-grade observations across 402 districts and 33,758 never-chain district-grade observations across 6,959 districts.

Treated (First Chain) districts are larger, higher scoring, more Asian, higher income, and more suburban than Never Chain districts. Columns (2) and (3) of Table 2 summarize baseline characteristics for First Chain and Never Chain districts. First Chain districts' average math and reading scores are roughly 0.17 to 0.18 standard deviations higher than the national average, suggesting tutoring chains tend to open in already high-achieving areas. Those districts are also higher income, as seen by their lower rate of economic disadvantage and their higher median income. Asian students represent nearly 5 percent of First Chain districts' populations, compared to 1-2 percent

[8] Our tutoring center data begin in 1997. If a center opened and closed in a district prior to 1997, we would misclassify a later opening as the first opening. Our test score data begins in 2009, and we are generally unconcerned with the effects of centers that closed more than twelve years before the outcomes panel begins.

[9] This design excludes many large urban districts, which already had private tutoring centers prior to 2010. We later use an alternative empirical strategy to include these large urban districts.

in all districts or in Never Chain districts specifically. Over half of students in First Chain districts attend suburban schools, more than twice the fraction in comparison districts. These patterns are consistent with Kim et al. (2025), who show that private tutoring centers tend to locate in higher-income areas.

Though our default specification uses all Never Chain districts as comparisons, we do additional analyses restricting the set of comparison district-grades to be more similar to our treated units. For each First Chain district-grade, we find the nearest neighbor Never Chain district-grade that is most similar in median income, total enrollment, share Asian, and share of students in suburban schools. We draw district-grades with replacement and obtain a sample from 247 unique school districts. The characteristics of these "Nearest Neighbors" are presented in Column (4) of Table 2. Across nearly every observable dimension, Nearest Neighbors appear much more similar to the First Chain sample than the typical Never Chain district, but remain slightly smaller, lower scoring, less Asian, lower income, and less suburban than the typical treated district.

5.2 Empirical Strategy

To causally estimate the effect of tutoring centers on student test scores, we use a staggered event-study design driven by the first opening of a tutoring chain in a school district. There is large variation in the timing of tutoring chain entry, which allows us to compare changes in test scores within districts before and after center entry while absorbing common year shocks.[10]

Districts that receive a first center typically continue to have centers operating, but do not see a further increase in the number of private tutoring centers. As seen in Figure 4, in the five years after first entry, the average treated district has roughly 0.8-0.9 tutoring centers operating within its boundaries, consistent with the vast majority of new centers remaining open but not attracting competition from other centers. Scaled by the local population, first chain entry increases the number of centers per 1,000 school-aged children by 0.271 over the five years following first entry. This magnitude will later be helpful for understanding the magnitudes of the achievement effects we observe. Both measures of exposure imply that tutoring chain entry predicts sustained exposure to tutoring centers, and such magnitudes can be interpreted as a quasi-first stage for the stock of private tutoring in a district.

[10] See Appendix Figure A.2 for more details.

A standard two-way fixed effects (TWFE) event-study regression is problematic in this setting because of staggered treatment timing (Goodman-Bacon, 2021; Sun and Abraham, 2021). With staggered adoption, already-treated districts serve as controls for newly treated districts, and when treatment effects vary across units or over time, the resulting TWFE coefficients may place negative or otherwise counterintuitive weights on some comparisons. To address these concerns, we use the two-stage difference-in-differences event-study estimator proposed by Gardner et al. (2024). The estimator first estimates untreated potential outcomes using time and unit fixed effects from never-treated and not-yet-treated units, and then compares treated observations to their estimated untreated counterfactual. In the first stage, we estimate

$$Y_{dgt} = \alpha_{dg} + \lambda_t + X_{dgt}\beta + \epsilon_{dgt} \tag{1}$$

using only never-treated and not-yet-treated district-grade-year observations. Here, d indexes school districts, g indexes grade levels, and t indexes years. Y_{dgt} is the mean test score in district d, grade g, and year t; α_{dg} are district-by-grade fixed effects; λ_t are year fixed effects; and X_{dgt} is a vector of time-varying district-level characteristics. Each district-grade observation is given equal weight in this specification.

We then use the first-stage estimates to construct an adjusted outcome for every observation in the sample,

$$\widehat{Y}_{dgt} = Y_{dgt} - \widehat{\alpha}_{dg} - \widehat{\lambda}_t - X_{dgt}\widehat{\beta}. \tag{2}$$

This adjusted outcome removes the district-grade fixed component, common year shocks, and estimated effect of time-varying characteristics as recovered from untreated observations. In the second stage, we regress the adjusted outcome on event-time indicators:

$$\widehat{Y}_{dgt} = \sum_k \delta_k D^k_{dgt} + u_{dgt}, \tag{3}$$

where D^k_{dgt} is an indicator equal to one if district-grade dg in year t is k years relative to the opening of its first tutoring chain. Because the unit and time fixed effects are absorbed in the first stage rather than the second, the second-stage regression does not difference out a within-

unit normalization, and all event-time indicators are separately identified; there is no mechanically omitted period. Standard errors are obtained by bootstrapping both stages and clustering at the district level. The bootstrap is essential here: because the second-stage outcome depends on first-stage estimates, it corrects the standard errors for the generated-regressor problem induced by the two-step procedure.

The coefficients δ_k have a natural event-study interpretation. For post-treatment periods, δ_k estimates the average effect on district-wide test scores k years after the first tutoring center opens, among district-grade-year observations observed at that relative time. These event-study coefficients are intent-to-treat (ITT) effects, measuring the change in average test scores across all students in a treated district, rather than treatment-on-treated (TOT) effects on the relatively small fraction of students who use tutoring centers. These effects are measured relative to the untreated counterfactual implied by the first-stage fixed effects and controls. For pre-treatment periods, the coefficients are placebo estimates: under parallel trends, they should be close to zero. In our main exhibits, we present the coefficients δ_k in event study figures.

We aggregate these event study coefficients to produce a single estimate of the effect of tutoring centers on test scores. Let $\#_{Pre}$ and $\#_{Post}$ denote the number of pre- and post-treatment event-study periods in the baseline specification; in practice, we set both equal to five. Our main summary estimand is the average post-treatment event-study coefficient minus the average pre-treatment coefficient:

$$ATT = \frac{1}{\#_{Post}} \sum_{k=1}^{\#_{Post}} \delta_k - \frac{1}{\#_{Pre}} \sum_{k=-\#_{Pre}}^{-1} \delta_k. \tag{4}$$

This estimator summarizes the change in adjusted test scores after treatment relative to the average pre-treatment placebo estimate. Rather than normalizing to a single reference period such as $k = -1$, we subtract the average of all pre-treatment coefficients, which differences out any residual level shift in the pre-period and avoids sensitivity to the choice of a single base year. We exclude $k = 0$ from the post-treatment average because center openings may occur at different points within the school year, making exposure during the opening year partial and difficult to interpret.

Our causal interpretation relies on the parallel trends assumption: absent the opening of a

tutoring center, test scores in treated districts would have followed the same trends as test scores in control districts. The most natural threat to this assumption is that chains select into districts on the basis of anticipated test-score gains, so that treated districts were already on differential trajectories at the time of entry. We provide evidence against this concern by confirming that pre-treatment coefficients are statistically indistinguishable from zero and exhibit no systematic trends. We note that this evidence speaks to selection on pre-trends rather than selection on levels, and our design accommodates the latter through the district-by-grade fixed effects.

6 Results

Chain tutoring centers have a positive, growing, and persistent impact on math achievement and a more moderate impact on reading achievement. Following the opening of a first chain center, overall math (reading) test scores increase by 0.029 (0.016) standard deviations. Figure 5a plots the event study coefficients from equation 3 separately for math and reading test scores. Both series exhibit flat pre-trends, lending credence to the parallel trends assumption necessary for a causal interpretation of the two-stage difference-in-difference design.[11] In the first year following chain entry, test scores immediately rise and continue to increase at least five years out.

The observation that private tutoring has a larger impact on math scores than on reading scores is consistent with the curriculum focus of most tutoring centers. Evidence suggests that 60 to 75 percent of Kumon instruction is dedicated to mathematics. Ignoring potential differences in pedagogical quality or within-school spillovers across subjects, this distribution of instructional time should theoretically yield a roughly twice greater impact on math than on reading achievement. Our findings are consistent with this expectation.

A major identification concern is that tutoring centers may open in response to changes in local demand for tutoring services that are themselves correlated with future changes in student test scores (e.g., demographic shifts, changing school quality, increasing parental investment). To address these concerns, Figure 5b presents results that additionally control for grade-level enrollment, racial composition of the test-taking student body, and district-level unemployment and median income. Reassuringly, inclusion of these additional controls does not significantly change

[11] In Appendix Figure A.3 we replicate our baseline results using the staggered event study method from Callaway and Sant'Anna (2021) and find similar point estimates and flat pre-trends.

the magnitudes of our estimates. With controls, we estimate the impact of a first-chain center on math (reading) scores as 0.034 (0.021) standard deviations, a slight increase from the specification without additional controls. Thus our measured impact does not appear to be driven by changing demographic or economic characteristics of the school district at the time of tutoring entry.[12]

6.1 Heterogeneity by Race

The overall positive effect of chain tutoring centers masks large racial heterogeneity. Consistent with the patterns in tutoring consumption documented above, we find larger effects for White and Asian students than Black, Hispanic, and Native American students. Focusing on math test scores, we estimate that tutoring center entry raises White and Asian students' math scores by 0.040 standard deviations, a highly significant and persistent effect (Figure 6a). In contrast, the estimated impact for Black, Hispanic, and Native American students is less than half of that magnitude (0.019 standard deviations) and is not statistically distinguishable from zero. The difference between these groups is marginally statistically significant. These figures also exhibit flat pre-trends.[13]

Consistent with racial heterogeneity in effects across districts, tutoring centers also appear to widen within-district math test score gaps between White students and their Black and Hispanic counterparts. Figure 6b shows chain tutoring entry in a school district increases the gap between White and Black students' average math scores by a highly significant 0.038 standard deviations and the gap between White and Hispanic students' math scores by 0.020 standard deviations. While the estimates in Figure 6a are estimated on different subsamples of school districts (because SEDA suppresses subgroup test scores when there are fewer than 20 test-takers), the racial test score gap estimates presented in Figure 6b isolate the impact on students of different races in the same district. Thus the racial heterogeneity in tutoring chain impacts is driven by within-district effects, rather than compositional differences between our estimating samples or heterogeneity of tutoring center effects across locations that serve different student populations.

One potential mechanism for the large effects of private tutoring centers is within-school spillovers

[12] Further evidence that endogeneity of center openings is not driving our main results comes from Appendix Figure A.4, which shows the evolution of predicted test scores based on changing district enrollment, racial demographics, local unemployment rates and median household income. On average, first chain centers appear to open in districts where gradually changing student demographics would predict slight declines in test scores over the next few years.

[13] Appendix Figure A.5a shows complementary results on reading test scores, which do not exhibit heterogeneity by race.

(Yao et al., 2024). These spillovers could arise if tutored students require less instructional attention, thereby freeing teachers to spend more time with other students, or if tutored students affect their classmates through peer learning. The spillovers channel has a distinctive implication: because private tutoring is disproportionately consumed by White and Asian students, tutoring centers should increase racial test score gaps more in settings where White and Asian students primarily interact with same-race peers, concentrating the spillovers by race.

We find evidence consistent with a spillover hypothesis. As seen in Figure 7a, in districts with above-median White-Black segregation, where White students are more likely to attend schools with other White students, the entry of a first tutoring chain increases the White-Black test score gap by 0.047 standard deviations.[14] In contrast, the corresponding effect is less than half as large, 0.023 standard deviations in districts with below-median segregation, where Black students have greater exposure to White peers and therefore more scope to benefit from any within-school spillovers. Figure 7b shows a similar pattern for the White-Hispanic gap. Tutoring-chain entry increases the gap by 0.028 standard deviations in highly segregated districts, but has no discernible effect in less segregated districts. Although neither of these differences between districts are statistically significant, these patterns suggest that the racial composition of students' peer environments mediates the effect of tutoring centers on racial achievement gaps, lending support to the hypothesis that there are spillovers from center-based private tutoring.

6.2 Additional Heterogeneity and Robustness

We explore further dimensions of heterogeneity and a variety of alternative specifications in Table 3 (for math effects) and Table 4 (for reading effects). The top panel of each table shows effects across all students, while subsequent panels show effects for subgroups of students by race/ethnicity, economic status, gender, and grade level, as well within-district measures of various racial and economic score gaps. The first column is our baseline specification, to which the second column adds time-varying district controls. The third column adds weights for the number of test-takers in each district-grade-year cell, which may better represent the underlying student population given

[14]SEDA's segregation measure is Theil's H (the enrollment-weighted multigroup entropy index), computed by comparing each school's racial composition to its district's overall racial composition. This yields a value from 0 (every school mirrors the district's composition) to 1 (schools are each entirely single-race), capturing how unevenly students of different races are sorted across schools within the same district.

variation in district size. The fourth column modifies the baseline specification by limiting the comparison districts to nearest neighbors to each district-grade matched on a variety of characteristics at baseline. The final column modifies the baseline specification by defining a school district as having a tutoring center if a tutoring chain operates within a 10-mile radius of the district centroid, rather than within district boundaries.

The overall math effects of tutoring centers are remarkably robust to all of these alternative specifications, with estimates ranging from 0.026-0.035 standard deviations and always statistically significant. Overall reading effects are also generally stable, ranging from 0.016-0.021 standard deviations and statistically significant in all specifications but the nearest neighbor model, where the point estimate drops to 0.009 standard deviations and becomes statistically insignificant. Patterns by race are also fairly stable. In math, the coefficient for White students ranges from 0.030-0.037 standard deviations and is always highly statistically significant. Consistent with usage patterns, coefficients for Asian students are larger in magnitude than for White students in all but one specification, but noisier given fewer districts with sufficient numbers of Asian students to report scores. Growth in the White-Black math gap is statistically significant across nearly all specifications and growth in the White-Hispanic gap is sometimes marginally statistically significant.[15] We do not observe consistent heterogeneity by gender or grade level, with male and female students and students in grades 3-5 and 6-8 all seeing clear improvements in math and reading scores across nearly every specification.

Evidence for heterogeneity by student economic status is mixed. Given tutoring attendance patterns, we would expect non-economically disadvantaged students to benefit more than economically disadvantaged students from the introduction of chain tutoring centers. Our baseline specification, with and without controls, shows very similar point estimates for both sets of students, as does the nearest neighbor model. Using the baseline specification, Figure 8a plots the event study coefficients of tutoring chain entry on math scores by economic status. The two series closely track each other, showing a modest rise in math test scores (0.02 standard deviations) following chain-entry, and Figure 8b shows the within-district math gap by economic status exhibiting no clear change. In contrast, as Table 3 shows, the models using student weights and measuring exposure to centers

[15]Event study graphs show little clear heterogeneity in the impacts on reading scores by race, as seen in Figures A.5 and A.6.

in a 10-mile radius both show the expected heterogeneity, with large and statistically significant impacts of center entry on non-economically disadvantaged students and smaller and statistically insignificant impacts on economically disadvantaged students. One potential explanation for the mixed results is that the measure of economic disadvantage captures over 40 percent of students, so that we are roughly comparing above and below median income students, rather than focusing on the top decile families where tutoring takeup is highest.[16]

Our estimates of the impact of tutoring centers are insensitive to the requirement that we observe test scores in a district-grade for at least ten of the eleven years from 2009 to 2019. Table 5 shows that as we change alternative minimum-observation requirements from eleven years to two years of observed test-score data, the pooled estimate and racial heterogeneity patterns on math test scores are stable across specifications even as the sample nearly doubles. The one margin along which the estimates shift is economic status. As the requirement is relaxed, the effect for non-economically-disadvantaged students holds between 0.021-0.026 standard deviations, while the effect for economically-disadvantaged students declines from 0.025 to roughly 0.015 standard deviations, losing statistical significance and widening the gap by economic status from near zero under the most restrictive requirement to nearly 0.01 standard deviation.

We also find evidence consistent with diminishing returns to additional tutoring centers in a community. Our primary analysis focuses on school districts gaining their first chain center, so that the marginal district is mid-sized and often suburban. Larger, often urban, districts are excluded from our First Chain and Never Chain categories. In Table 6, we study the full set of school districts and changes to tutoring center exposure by comparing our two-stage DiD analysis (in column 1) to a TWFE model where the treatment variable is the lagged count of chain centers per 1,000 school-age children in the district (in column 2). This alters the scale of the coefficient, which now measures the effect of one (first) additional center per 1,000 children rather than the binary effect of first-entry, but continues to show large positive impacts on math and reading scores. We then expand the sample (in column 3) to include all districts, not just First Chain and Never Chain districts. That substantially shrinks the coefficients, potentially consistent with diminishing returns to additional tutoring centers in large districts that contain multiple tutoring centers.

[16]Event study graphs show little clear heterogeneity in the impacts on reading scores by economic status, as seen in Figure A.7.

In contrast to our findings on chain tutoring centers, we find no impacts on math and reading scores from non-chain tutoring centers (those operating fewer than 10 locations) or from in-home chains that send tutors into students' homes. As Table 7 shows, the overall point estimates for both non-chains and in-home chains are close to zero and never statistically significant. Point estimates for various student subgroups are also statistically insignificant in nearly every case. The lack of detectable impacts from non-chain centers may reflect the role of standardized curricula and franchise oversight, features specific to chain tutoring centers, in delivering achievement gains. Tutoring center location may also be a poor-proxy for exposure to in-home tutoring chains, as employees traveling to a client's home need not be based near the registered business.[17]

7 Magnitudes

The magnitudes of tutoring chain center effects suggest that entry of such centers explains a substantial portion of the evolution of test scores in treated districts. From 2009-19 in such districts, overall math (reading) scores increased by 0.046 (0.030) standard deviations, so that our estimated center-entry effect of 0.029 (0.016) standard deviations explains 63 (53) percent of districts' overall improvement. For White and Asian students in those districts, math (reading) scores increased by 0.084 (0.053) standard deviations, so that our estimated center-entry effect of 0.040 (0.018) standard deviations explains 48 (34) percent of such students' improvement. Similar calculations suggest that center entry explains 44 (27) percent of the increased White-Black math (reading) gap in treated districts, and 95 (-26) percent of the change in the White-Hispanic math (reading) gap.[18] Such private investments outside of public school systems have non-trivial effects on the outcomes by which school districts are evaluated by families and state accountability systems.

Tutoring centers appear cost-effective relative to typical public school spending. Under our capacity-based estimate described in more detail below, a first chain induces about three percent of students to take up tutoring, each spending about $2,500 over a nine-month academic year. Spread across every pupil in the district, this amounts to roughly $75 per pupil. By comparison,

[17] We also explore heterogeneity by chain brand but are generally underpowered to distinguish the efficacy of Kumon, Sylvan, and other chains. The magnitudes of our chain-specific effects are also sensitive to whether we use as controls all never-chain districts or just the nearest neighbor districts. See Appendix Table A.1 for details.

[18] From 2009-19, the White-Hispanic reading test score gap decreased by 0.065 standard deviations in treated districts, meaning that Hispanic scores improved relative to White scores.

Jackson and Mackevicius (2024) estimate that a $1,000 per-pupil increase in public school spending sustained over four years raises average test scores by 0.032 standard deviations. Our first-chain ITT of 0.029 standard deviations is nearly identical in magnitude, yet the tutoring expenditure that produces it is an order of magnitude smaller per pupil. Private tutoring centers thus purchase a district-average achievement gain at a small fraction of the public cost required to produce the same effect, albeit with the expense borne privately by families rather than by school systems. In contrast to public school spending, which Jackson and Mackevicius (2024) show has the largest effects on economically disadvantaged students, gains from tutoring spending accrue largely to more advantaged students. Tutoring centers may appear cost-effective in part because they target students with fewer challenges and more family support to complement the tutoring itself.

What do our district-level estimates imply about the effect of tutoring on the students who actually use these centers? Our event-study coefficients are intent-to-treat (ITT) effects, measuring the change in average test scores across all students in a treated district, the great majority of whom never enroll in a center. Assuming no spillover effects to district peers not receiving tutoring services, recovering a treatment-on-treated (TOT) effect requires two parameters: an estimate of how many students chain entry induces into tutoring, and an assumption about how long a tutored student's gains persist. We take each in turn.

We measure take-up from the physical capacity of the centers themselves. As previously discussed, the average Kumon center tutors about 160-170 students, two-thirds of whom study math. Assuming that the typical chain in our data thus serves 110 students a year with math tutoring, one additional center per 1,000 children mechanically tutors 11 percent of those children. As Figure 4 shows, the opening of a district's first chain raises the density of centers by 0.271 per 1,000 children. Multiplying that number by 11 percent suggests first-chain entry raises the share of children receiving math tutoring by 3.0 percentage points and reading tutoring by 1.5 percentage points.[19]

We model persistence to account for the fact that a district's test scores in a given year reflect not only the students currently attending a center but also every student who attended in prior years and retained some of what they gained. Define the TOT as the effect of one year of tutoring

[19]One implication of this assumption is that private tutoring centers should have larger impacts in smaller school districts, where a larger share of students are treated. Appendix Table A.2 shows that this is indeed the case. Estimated math achievement impacts are 70 percent larger in school districts with below median enrollment size relative to those with above median enrollment size.

on a tutored student, and let s denote the share of students receiving a year of tutoring in a given year, the 3.0 and 1.5 percent estimated above for math and reading respectively.[20] Let r denote the fraction of a gain retained one year later, so that a student tutored j years ago still exhibits r^j of the original effect. We assume the first tutored cohort enrolls in the year after entry, $k = 1$, and that a new cohort enrolls each year thereafter.[21] At event year $k \geq 1$, the district thus contains k tutored cohorts, and the district-average effect is $s \times \text{TOT} \times M(k)$, where

$$M(k) = \sum_{j=0}^{k-1} r^j = \frac{1 - r^k}{1 - r} \tag{5}$$

is the stock of retained tutoring, measured in units of a single year's cohort (with $M(k) = k$ when $r = 1$). Because our summary estimator averages event years $k = 1$ through 5, the ITT is scaled by $\bar{M} = \frac{1}{5} \sum_{k=1}^{5} M(k)$, so that

$$\text{TOT} = \frac{\text{ITT}}{\bar{M} \times s}. \tag{6}$$

We take full persistence, $r = 1$, as our baseline. This is the most conservative assumption available given this timing, as it credits the district-level effect to the largest possible number of students. Assuming $r = 1$ means $M(k) = k$, meaning that the cohort tutored in the first post-entry year is joined by a new cohort each subsequent year and none of them lose anything. The stock of students carrying a tutoring-induced gain thus grows from one cohort at $k = 1$ to five at $k = 5$, and $\bar{M} = 3$. The average post-entry ITT thus reflects three cohorts' worth of math tutoring, each comprising 3.0 percent of students, spread across the district's entire tested population. The effective share of students treated is the product of these ($\bar{M} \times s = 3 \times 0.030 = 0.089$) and dividing our math ITT of 0.029 standard deviations by it yields a TOT of 0.32 standard deviations (or 0.38 standard deviations in the specification with controls). These magnitudes are similar to the central estimate from meta-analyses of the tutoring literature, where the average program raises achievement by about 0.4 standard deviations (Nickow et al., 2024), with those tutoring programs

[20] Defining the treatment as a student-year of tutoring rather than a completed tutoring spell sidesteps the fact that franchise disclosure documents do not report how long the typical student remains enrolled. A center serving three percent of a district's students at any moment delivers three percent student-years per year whether those are the same students for five years or different students each year. The cost is an assumption that the second year of tutoring raises achievement as much as the first, which we cannot test.

[21] Centers typically open partway through the school year and take time to build enrollment, so we treat the entry year $k = 0$ as a start-up period with negligible take-up. Crediting the entry year with a partially tutored cohort would raise $\bar{M}$ and lower the implied TOTs.

implemented at scale having effects closer to 0.2 standard deviations (Kraft et al., 2024).

Full persistence may not hold given that intervention effects typically fade at least somewhat after treatment ends. As an alternative, we assume that students retain three-fourths of their gain each year after leaving, or $r = 0.75$. Under that assumption, $M(k) = 4(1 - 0.75^k)$, which rises from one cohort at $k = 1$ to about three at $k = 5$, so $\bar{M} = 2.17$. The implied math TOT is 0.45 standard deviations, or 0.53 standard deviations using the estimate with controls. These are large but still within the range of estimates from the tutoring literature.

The same machinery applied to reading produces comparable magnitudes. First-chain entry raises reading take-up by 1.5 percentage points, and the reading ITT of 0.016 standard deviations implies a TOT of 0.36 standard deviations under full persistence and 0.49 standard deviations under $r = 0.75$. The corresponding estimates using the specification with controls are 0.47 and 0.65 standard deviations, respectively.

The TOT estimates we calculate rest on substantial assumptions about the lack of spillovers, the take-up rate, and the depreciation of such gains over time. The above magnitudes are large given existing evidence about tutoring effects. This could be driven by high efficacy of these chain centers or high responsiveness of the marginal student using such centers to their treatment. Conversely, the large magnitude of these TOT estimates may imply that we have underestimated the take-up rate or the extent to which spillovers to peers may be driving our ITT estimates.

8 Conclusion

This paper provides the first national, causal evidence on the effects of private tutoring chains on student achievement. Using business registry data linked to household spending and school district test scores, we show that tutoring usage is concentrated among Asian, White and high-income households, and that the opening of a district's first chain tutoring center raises district-wide math and reading scores. Math effects are driven largely by White and Asian students, widening within-district racial achievement gaps, particularly in racially segregated districts, consistent with a within-school spillover mechanism. Non-chain centers and in-home tutoring chains show no detectable effects, suggesting the standardized curricula and franchise oversight specific to chains, rather than tutoring generally, may drive these gains.

These findings have implications for interpreting public measures of school performance. State accountability systems rate schools and districts in part on standardized test scores, and those ratings shape everything from state intervention and school choice decisions to housing prices and public perceptions of district quality. Our estimates suggest that some portion of the achievement those systems measure comes through private investments made by a subset of families outside of the schools themselves. Because families' willingness and ability to pay for supplemental instruction differ across communities, private tutoring markets may help already advantaged districts, with the scores used to compare districts partly reflecting differences in private spending rather than differences in public school quality alone. More broadly, our results suggest that the boundary between public and private education inputs is harder to define than accountability systems assume. Policymakers and researchers who use test scores to evaluate schools, allocate resources, or measure the returns to public investment should recognize that those scores reflect the full set of educational inputs students receive, a growing share of which is bought on the private market.

Under reasonable assumptions, the implied magnitude of the effect of tutoring chain centers on treated students is very high, raising the question of whether public schools could improve their own instruction by learning from these chains' approaches. Kumon's worksheet-based mastery model and Mathnasium's individualized math pathways implement principles (frequent low-stakes practice, work calibrated to a student's actual level rather than grade level, rapid feedback) that the broader tutoring literature has already identified as effective (Nickow et al., 2024). Chains add to this execution, delivering franchise standardization, instructor accountability, and diagnostic placement at a scale and consistency that individual schools and classrooms may struggle to achieve.

A related question is what to make of the demand these centers satisfy. Parents likely pay for tutoring centers because they perceive schools as not providing enough individualized instruction or practice time. That many families are willing to pay several hundred dollars a month for this suggests unmet demand for a service that public schools are positioned, in principle, to provide directly. Whether the binding constraint is school funding, instructional time, teacher capacity to differentiate within a classroom, or something else entirely is beyond what our design can answer, but the willingness-to-pay revealed by chain tutoring's growth is itself informative, highlighting a gap between what schools offer and what at least some parents believe their children need. This may be particularly relevant in the post-pandemic world in which public schools are increasingly

struggling to attract high-income and Asian and White families (Francis and Goodman, 2025).

Because usage and benefits are concentrated among already advantaged families, the same mechanism that makes tutoring centers valuable to their users currently makes them a force for widening, not narrowing, achievement gaps. Efforts to bring tutoring-style instruction into public schools at scale might ensure that the benefits of such pedagogical approaches are not limited to the families who can afford them in the private market.

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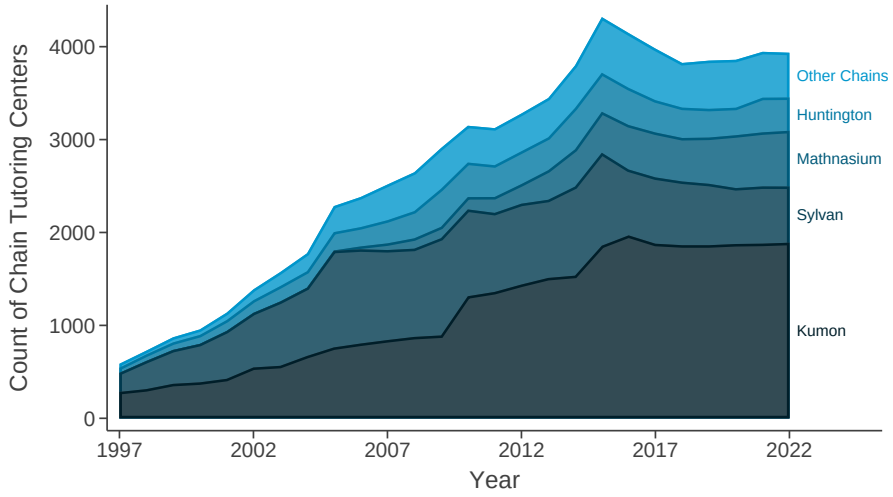
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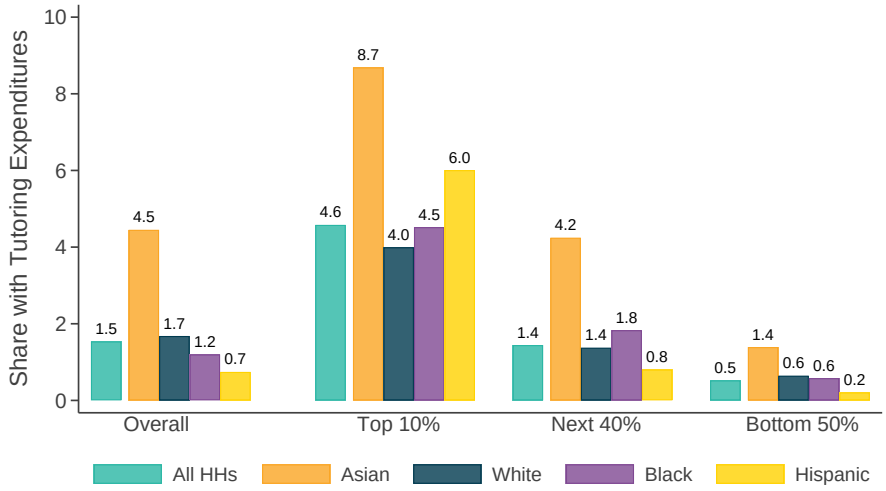
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Figure 1: Count of Chain Tutoring Centers in the United States, 1997-2022



Notes: Figure 1 plots the total number of chain private tutoring centers in operation in the United States from 1997 to 2022. Series are shown separately for the four tutoring chains with the most locations: Kumon, Sylvan Learning, Mathnasium, and Huntington Learning, as well as for all other chains. Data are from Data Axle.

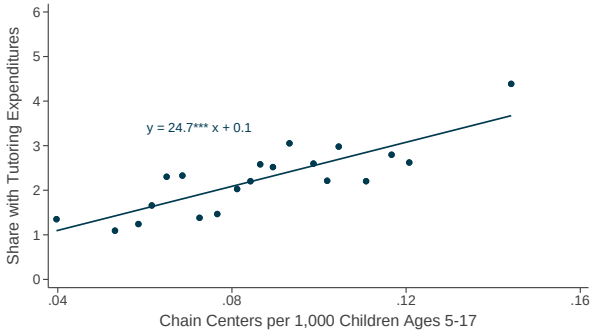
Figure 2: Share of Households with Tutoring Expenditure by Race/Ethnicity and Income Percentile



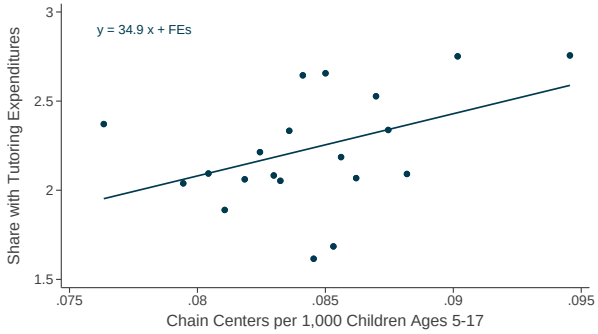
Notes: Figure 2 plots the share of households reporting non-zero expenditures on "Test preparation, tutoring services" in a given month, weighted by the CEX sample weights. Data come from the Consumer Expenditure Survey, 2009-2019. The leftmost group ('Overall') shows the share for all households. The three rightmost groups of bars compare households of different races within the same bin of the national income distribution. Top 10% refers to households with incomes above the 90th percentile on the national income distribution, Next 40% are households with incomes between the 50th and 90th income percentiles, and Bottom 50% are households with incomes below the 50th percentile. Within each income bin, shares are shown for all households ('All HHs') as well as separately by race/ethnicity. For more details on the sample construction, see section 3.2.

Figure 3: Tutoring Chain Center Density and Household Tutoring Participation

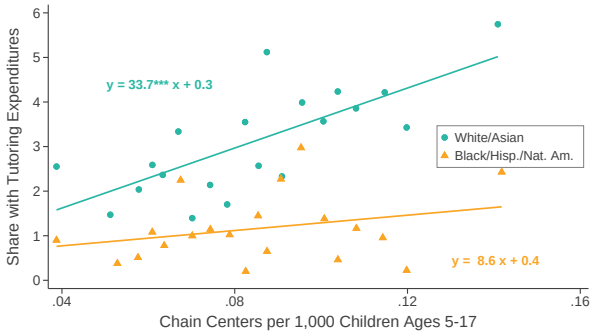
(a) All Households with Children Ages 6-17



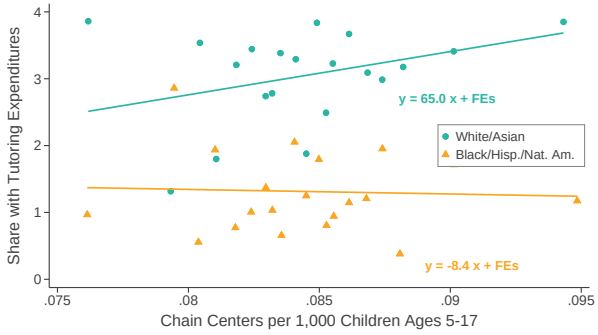
(b) All Households with MSA + Year FEs



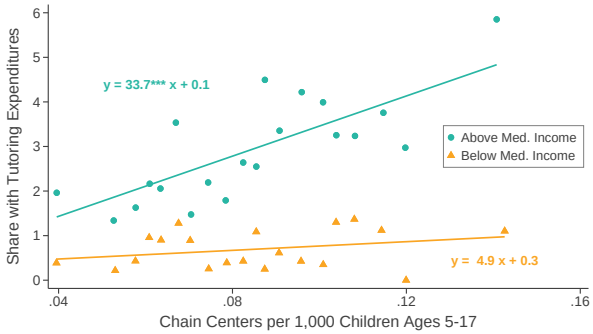
(c) By Race/Ethnicity



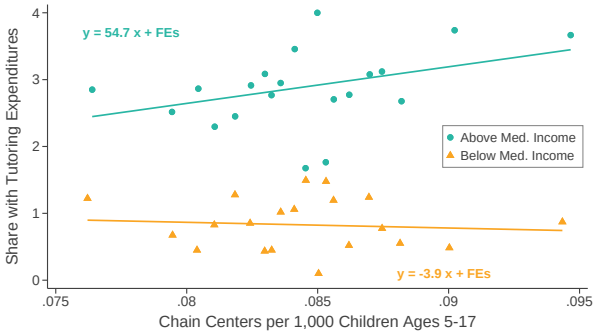
(d) By Race/Ethnicity with MSA + Year FEs



(e) By Income

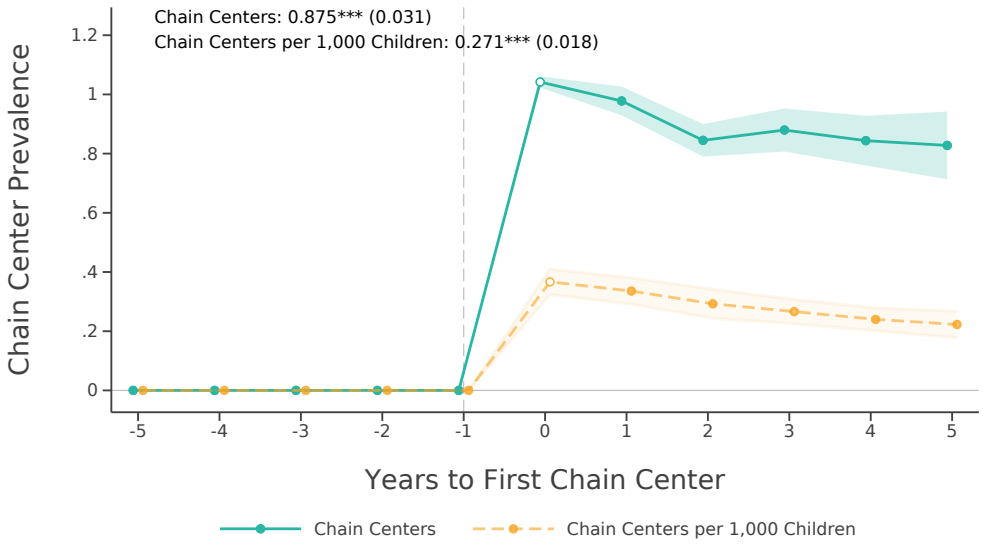


(f) By Income with MSA + Year FEs



Notes: Figure 3 plots the relationship between tutoring chain center prevalence and tutoring consumption across Metropolitan Statistical Areas (MSAs). For each MSA-year observation in the CEX, tutoring chain density is the number of chain centers per 1,000 children ages 5–17. Share with tutoring expenditures is the share of household reporting positive spending on "Test preparation or tutoring services" in a given month. The panels on the left report the line of best fit and show a binned scatter plot of household tutoring participation on MSA-year chain center density, weighted by CEX sample weights. The panels on the right additionally control for MSA and Year fixed effects and do not report the intercept, which depends on the excluded fixed effect. Standard errors are clustered by household. Panels 3a and 3b show the relationship for all households with children ages 6-17. Panels 3c and 3d plot separate series for White or Asian households and Black, Hispanic, or Native American households. Panels 3e and 3f plot separate series for households with income above and below the national median. Tutoring chain center locations come from Data Axle and child population counts come from the American Community Survey (ACS). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

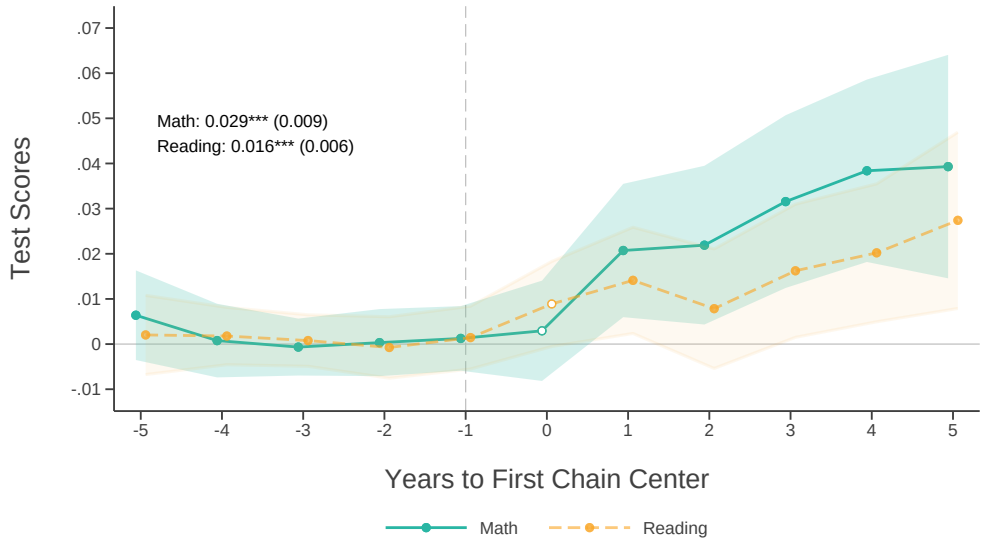
Figure 4: Impact of First Chain on Chain Center Prevalence



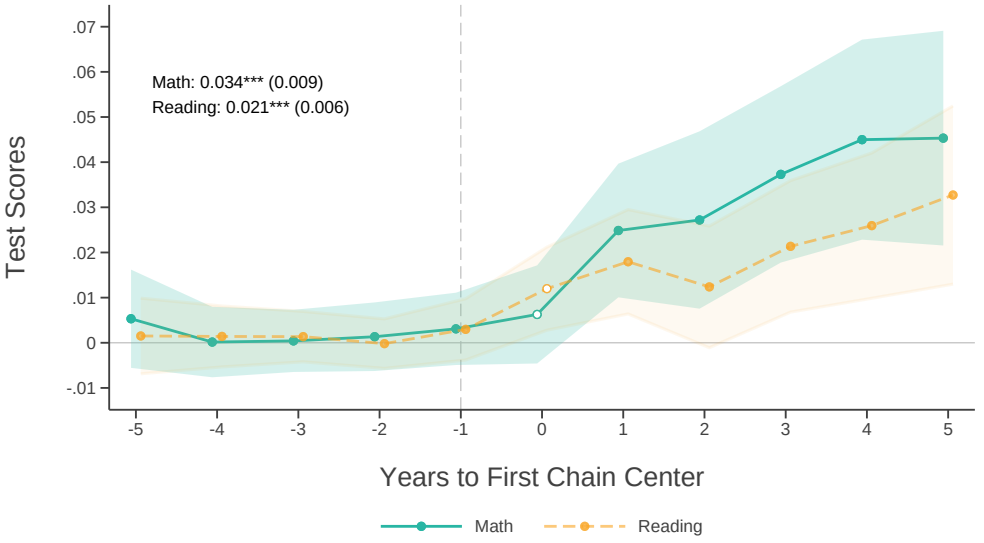
Notes: Figure 4 plots event-time estimates of the impact of first chain entry on the number of chain tutoring centers operating in a school district. The solid blue line shows event time coefficients on the number of chains, the dashed orange line shows event time coefficients for the number of chains per 1,000 children ages 5-17. Estimates include district and year fixed effects, but no additional controls. See equation 3 for the estimating equation. The average treatment effect is calculated using equation 4, omitting period 0. Standard errors are bootstrapped and clustered by school districts, the shaded region shows 95% confidence intervals. Tutoring chain locations come from Data Axle. School district boundaries come from SEDA. Number of children ages 5-17 comes from the ACS. The sample contains first-chain and never-chain school districts from 2009-2019 with at least 10 years of observed test score data. For more details on sample construction, see section 5.1. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 5: Effect of First Tutoring Chain on Test Scores, All Students

(a) Baseline



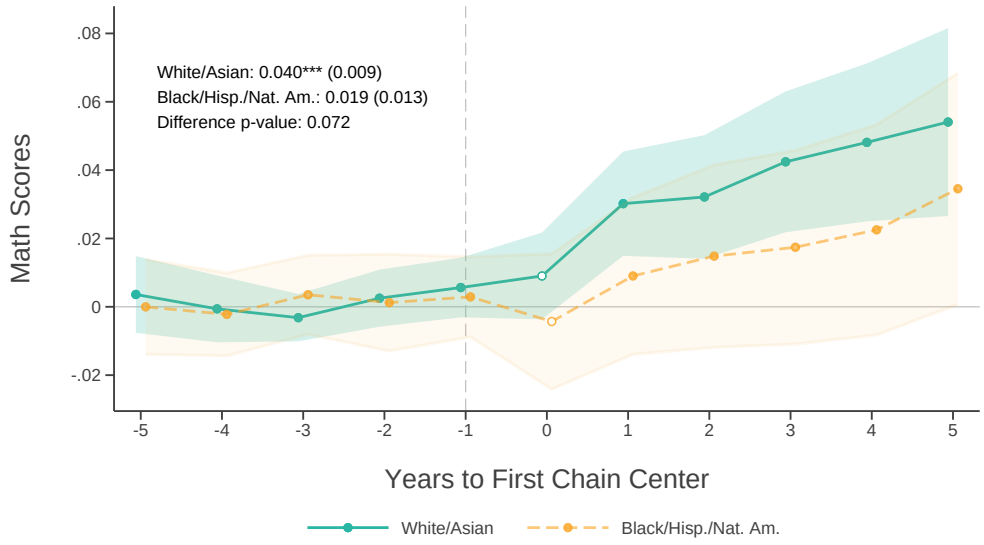
(b) With Additional Controls



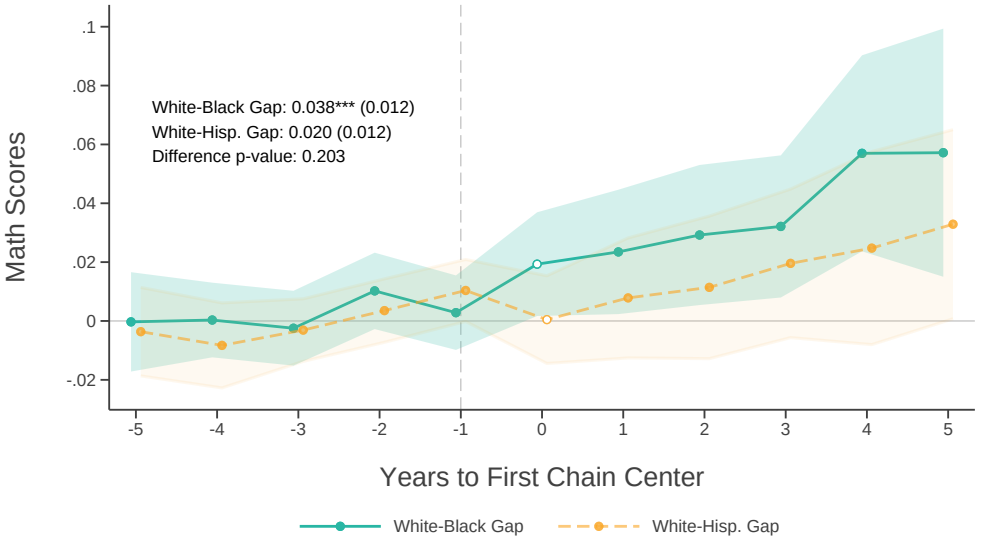
Notes: Figure 5 plots event-time estimates of chain tutoring entry on district-grade test scores. The solid blue line in panel 5a plots event-study coefficients from math test score regressions, the dashed orange line plots coefficients on reading test scores. Estimates include district-grade and year fixed effects, but no additional controls. See equation 3 for the estimating equation. The average treatment effects are calculated using equation 4, omitting period 0. Panel 5b replicates panel 5a but additionally controls for grade-level enrollment and racial shares, and district-level unemployment and median income. Standard errors are bootstrapped and clustered by school districts, the shaded region shows 95% confidence intervals. Tutoring chain locations come from Data Axle. School district boundaries, test scores, and additional controls come from SEDA. The sample contains first-chain and never-chain school districts from 2009-2019 with at least 10 years of observed test score data. For more details on sample construction, see section 5.1. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 6: Racial Heterogeneity in Math Effects

(a) By Race/Ethnicity



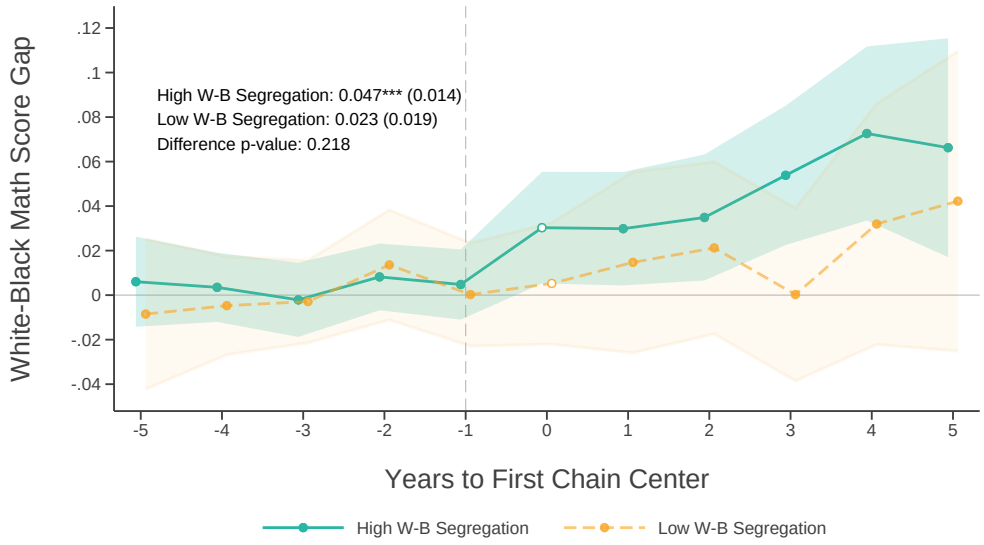
(b) Racial Test Score Gaps



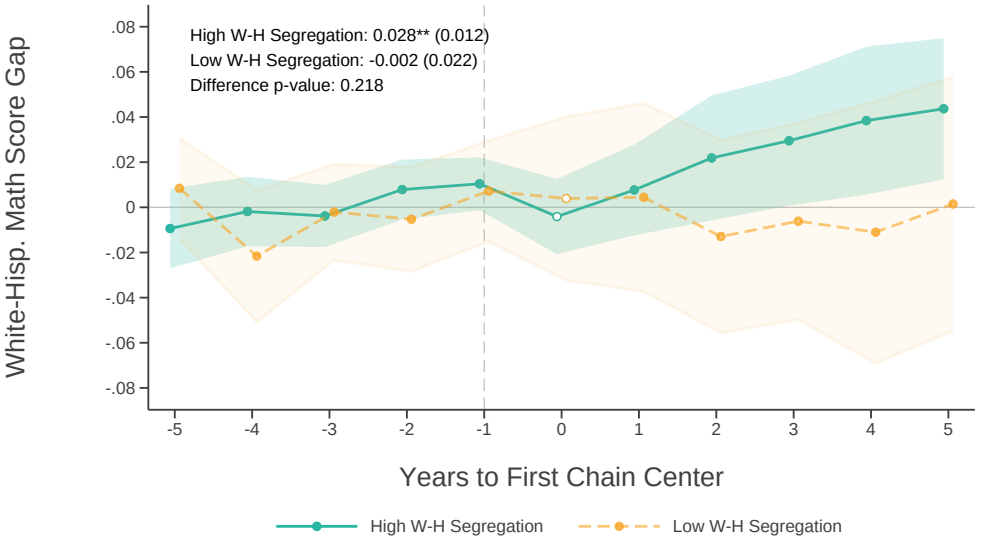
Notes: Figure 6 plots event-time estimates of chain tutoring entry on district-grade math test scores by race. In panel 6a, the solid blue line plots estimates using White and Asian students' test scores as the outcome. The dashed orange line is estimated with the test scores of Black, Hispanic, and Native American students. In panel 6b, the blue line plots estimates of the within-district gap between White and Black students' math test scores, the orange line plots estimates of the within-district gap between White and Hispanic students' math test scores. Estimates include district-grade and year fixed effects, but no additional controls. See equation 3 for the estimating equation. The average treatment effects are calculated using equation 4, omitting period 0. Standard errors are bootstrapped and clustered by school districts, the shaded region shows 95% confidence intervals. Tutoring chain locations come from Data Axle. School district boundaries and test scores come from SEDA. The sample contains first-chain and never-chain school districts from 2009-2019 with at least 10 years of observed test score data. For more details on sample construction, see section 5.1. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 7: Effect of First Tutoring Chain on Math Score Gaps by Racial Segregation

(a) White-Black Math Score Gap



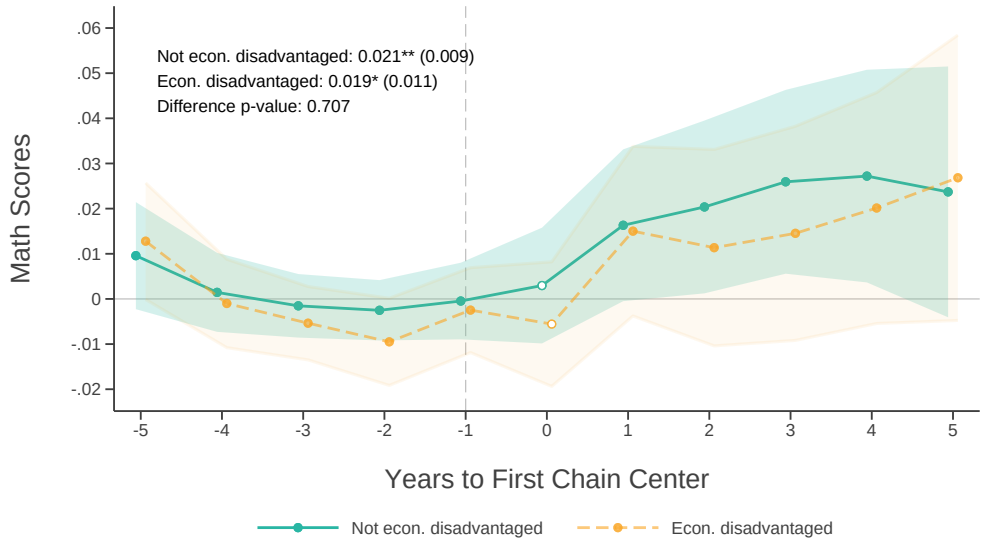
(b) White-Hispanic Math Score Gap



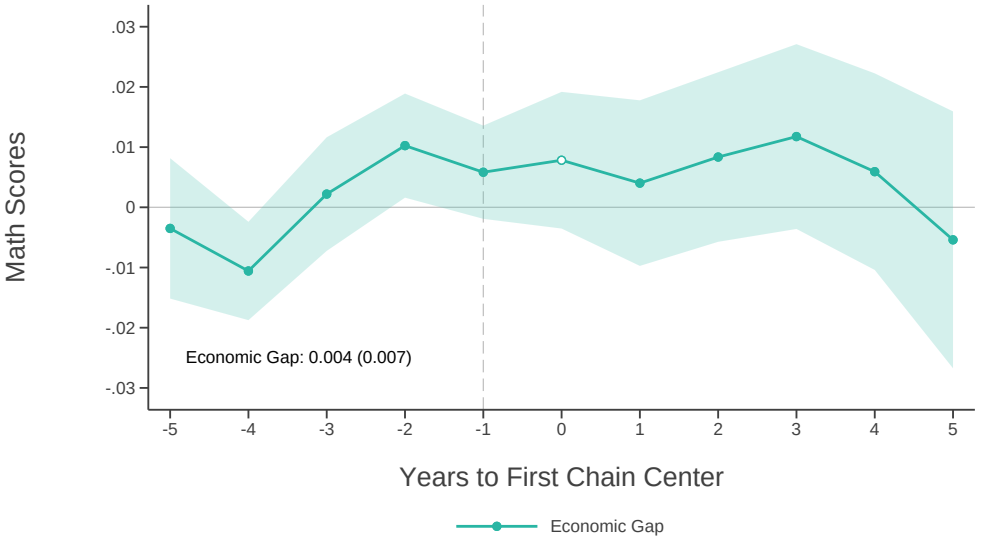
Notes: Figure 7 plots event-time estimates of the effect of a first chain tutoring center on within-district math test score gaps, separately by level of racial segregation. Panel 7a shows estimates for the White-Black math score gap; panel 7b shows estimates for the White-Hispanic gap. In each panel, district-grades are split at the median of the relevant information index based on the district-grade mean level of segregation across all observed years. The solid blue line includes districts with above-median segregation and the dashed orange line uses below-median districts. Estimates include district-grade and year fixed effects but no additional controls. See equation 3 for the estimating equation; average treatment effects are calculated using equation 4, omitting period 0. Standard errors are bootstrapped and clustered by school districts, the shaded region shows 95% confidence intervals. Tutoring chain locations come from Data Axle. School district boundaries and test scores come from SEDA. The sample contains first-chain and never-chain school districts from 2009–2019 with at least 10 years of observed test score data. For more details on sample construction, see section 5.1. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 8: Economic Heterogeneity of First Tutoring Chain on Math Test Scores

(a) By Economic Disadvantaged Status



(b) Economic Test Score Gap



Notes: Figure 8 plots event-time estimates of chain tutoring entry on district-grade math test scores by economic status. In panel 8a, the blue line uses the math test scores of non-economically disadvantaged students as the outcome, the orange line uses the math test scores of economically disadvantaged students. In panel 8b, the outcome is the within-district math score gap between non-economically disadvantaged and economically disadvantaged students (a positive gap indicates non-economically disadvantaged students are performing better). Estimates include district-grade and year fixed effects, but no additional controls. See equation 3 for the estimating equation. The average treatment effects are calculated using equation 4, omitting period 0. Standard errors are bootstrapped and clustered by school districts, the shaded region shows 95% confidence intervals. Tutoring chain locations come from Data Axle. School district boundaries and test scores come from SEDA. The sample contains first-chain and never-chain school districts from 2009-2019 with at least 10 years of observed test score data. For more details on sample construction, see section 5.1. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1: Counts of Chain Tutoring Brands

Brand Name	Subject (1)	Unique Locations from 2009–2019 (2)	Unique Locations in 2009 (3)	School Districts Treated (4)
Kumon	Math and reading	2,658	892	163.5
Sylvan Learning	Math and reading	1,780	1,048	139
Mathnasium	Math only	723	122	34.5
Huntington Learning	Math and reading	705	409	16
Eye Level Learning	Math and reading	271	0	8
Tutoring Club	Math and reading	215	160	10
The Tutoring Center	Math and reading	133	32	14
C2 Education	Math and reading	116	37	3
LearningRx	Math and reading	89	25	3
KnowledgePoints	Math and reading	63	55	1
The Learning Center	Math and reading	39	22	3
Enopi	Math and reading	38	9	1
JEI Learning Center	Math and reading	31	16	1
Lindamood-Bell	Reading only	31	20	1
Oxford Learning	Math and reading	23	22	0
Masonic Learning Center	Reading only	17	15	0
Reading Town	Reading only	17	7	0
Gideon Math & Reading	Math and reading	15	5	2
Math Monkey	Math only	14	12	0
Omega Learning	Math and reading	13	1	0
Aloha Mind Math	Math only	13	0	0
Grade Power Learning	Math and reading	9	0	2
Russian Math	Math only	8	1	0
Chain Totals		7,071	2,910	402

Notes: Table 1 presents the number and characteristics of chain tutoring center brands. Column (1) reports the primary subject(s) offered. Column (2) reports the cumulative number of unique center locations observed across the sample period from 2009 to 2019, and Column (3) reports the number of unique active locations in 2009. Column (4) reports the number of school districts treated by each chain brand. A school district is defined as treated when a chain opens a location within its boundaries for the first time. For more details on the construction of our estimation sample, see section 5.1. Fractional values in Column (4) (e.g., 163.5 for Kumon, 34.5 for Mathnasium) occur if multiple brands open in a school district in the year of first entry; the treatment count is proportionally split among those brands. The bottom row aggregates these metrics across all listed chains. Tutoring center location and operation data come from Data Axle; school district boundaries come from SEDA.

Table 2: Summary Statistics of School Districts in 2009

	All Districts (1)	First Chain (2)	Never Chain (3)	Nearest Neighbor (4)
Enrollment				
Number of Children Ages 5-17	4,867	5,831	2,236	5,249
Enrollment per Grade	344	432	163	392
Math Achievement				
Overall	0.007	0.179	-0.028	0.045
White	0.125	0.301	0.076	0.176
Asian	0.614	0.721	0.340	0.445
Black	-0.510	-0.416	-0.581	-0.529
Hispanic	-0.330	-0.252	-0.378	-0.348
Non-Econ. Disadvantaged	0.228	0.355	0.190	0.240
Econ. Disadvantaged	-0.300	-0.260	-0.302	-0.302
Reading Achievement				
Overall	-0.002	0.167	-0.036	0.025
White	0.119	0.294	0.070	0.164
Asian	0.390	0.533	0.139	0.274
Black	-0.471	-0.374	-0.543	-0.476
Hispanic	-0.413	-0.328	-0.483	-0.417
Non-Econ. Disadvantaged	0.230	0.349	0.195	0.236
Econ. Disadvantaged	-0.324	-0.299	-0.324	-0.332
Demographics				
Percent White	74.9%	71.0%	77.6%	69.3%
Percent Hispanic	10.7%	13.2%	9.7%	14.4%
Percent Black	9.8%	9.7%	8.7%	11.0%
Percent Asian	2.1%	4.7%	1.2%	3.6%
Percent Econ. Disadvantaged	44.1%	34.0%	46.1%	41.7%
Share in Suburban Schools	23.3%	50.2%	16.0%	41.9%
Employment and Income				
Median Income	52,917	65,907	50,066	59,787
Unemployment Rate	6.8%	6.2%	6.8%	7.0%
Segregation				
White-Black Information Index	0.056	0.060	0.043	0.080
White-Hispanic Information Index	0.045	0.052	0.032	0.063
Economic Information Index	0.028	0.043	0.016	0.042
Unique Districts	8,943	402	6,959	247
District-Grade Observations	43,203	1,924	33,758	1,204

Notes: Table 2 presents summary statistics of our primary analysis sample in 2009, as described in section 5.1. Data cover grades 3-8 of all U.S. public school districts for which we observe test score data ten times between 2009 and 2019. Column (1) reports the mean characteristics for all district-grades. Column (2) reports mean characteristics for districts that gain their first chain tutoring center between 2010 and 2019, the treated sample. Column (3) reports mean characteristics for districts that never have a chain tutoring center between 1997 and 2019. Column (4) restricts Column (3) to Never Chain district-grades that are a nearest neighbor to a First Chain district grade, matched on median income, total enrollment, percent Asian, and share in suburban schools. Number of Children Ages 5-17 comes from the American Community Survey (ACS). All other variables come from SEDA. Demographic variables are based on the share of math test-takers in a district-grade. Variables in the "Employment and Income" panel are reported at the district level.

Table 3: Tutoring Chain Center Effects on Math Scores

Model	First/Never Chains (1)	+ Controls (2)	+ Weights (3)	Nearest Neighbor (k=1) (4)	10-Mile Radius (5)
All Students	0.029***	0.034***	0.027**	0.035***	0.026**
(N=381,356)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)
Race/Ethnicity					
White	0.030***	0.034***	0.034***	0.037***	0.035***
(N=318,371)	(0.009)	(0.009)	(0.010)	(0.011)	(0.011)
Asian	0.066**	0.048*	0.041	0.047	-0.018
(N=6,462)	(0.031)	(0.027)	(0.028)	(0.031)	(0.041)
Black	0.003	0.009	0.027	0.006	-0.038**
(N=54,013)	(0.016)	(0.016)	(0.020)	(0.019)	(0.018)
Hispanic	0.015	0.018	0.013	0.024	0.019
(N=58,792)	(0.014)	(0.015)	(0.019)	(0.016)	(0.024)
Economic Status					
Non-Econ. Disadvantaged	0.021**	0.025**	0.030***	0.031***	0.037***
(N=254,742)	(0.009)	(0.010)	(0.010)	(0.011)	(0.013)
Econ. Disadvantaged	0.019*	0.026***	0.013	0.031***	-0.001
(N=261,807)	(0.011)	(0.010)	(0.013)	(0.012)	(0.012)
Gender					
Male	0.026***	0.030***	0.027***	0.032***	0.032***
(N=289,383)	(0.010)	(0.009)	(0.010)	(0.011)	(0.011)
Female	0.028***	0.033***	0.023**	0.037***	0.035***
(N=282,753)	(0.009)	(0.009)	(0.010)	(0.010)	(0.013)
Grade Level					
Grades 3-5	0.033***	0.038***	0.031***	0.041***	0.020
(N=218,722)	(0.010)	(0.010)	(0.012)	(0.011)	(0.013)
Grades 6-8	0.022**	0.028***	0.023**	0.027**	0.033***
(N=162,634)	(0.010)	(0.011)	(0.010)	(0.012)	(0.011)
Test Score Gaps					
White-Black Gap	0.038***	0.035***	0.023*	0.034**	0.045**
(N=41,115)	(0.012)	(0.011)	(0.013)	(0.014)	(0.019)
White-Hispanic Gap	0.020	0.019	0.034*	0.014	0.034*
(N=43,150)	(0.012)	(0.013)	(0.019)	(0.013)	(0.019)
White-Asian Gap	-0.023	-0.017	-0.002	-0.024	0.043
(N=5,614)	(0.026)	(0.027)	(0.025)	(0.026)	(0.052)
Econ. Status Gap	0.004	0.003	0.008	0.010	0.015**
(N=187,714)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Add. Controls		×	×		
Weighted			×		

Notes: Table 3 presents estimates of the impact of tutoring chain centers on math test scores across models and student subgroups. Each cell is a separate regression. All estimates include calendar-year and district-grade fixed effects. Column (1) presents the average treatment effect from Equation 4, calculated using two-stage difference-in-differences, estimated on our sample of first-chain and never-chain districts from 2009-2019, and omitting period 0. Column (2) replicates Column (1) with additional controls for grade-level enrollment, grade-level racial shares, district-level unemployment and median income. Column (3) replicates Column (2), weighting district-grade-subgroup observations by the number of test-takers. Column (4) restricts the sample to First Chain districts and their nearest neighbors in terms of median income, total enrollment, share Asian, and share of students in suburban schools. Column (5) defines districts as treated if there is a chain center in a 10-mile radius of the district centroid. Standard errors are bootstrapped and clustered by school district. "Test Score Gaps" refer to the within-district gap in test scores between student subgroups. Test score data and additional controls come from SEDA. Tutoring center locations come from Data Axle. Counts of child population come from the ACS. Number of observations are the count of district-grade-years in the subgroup specifications from Columns (1)-(3). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Tutoring Chain Center Effects on Reading Scores

Model	First/Never Chains (1)	+ Controls (2)	+ Weights (3)	Nearest Neighbor (k=1) (4)	10-Mile Radius (5)
All Students	0.016***	0.021***	0.021***	0.009	0.017**
(N=418,618)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)
Race/Ethnicity					
White	0.011	0.014*	0.011	0.009	0.017**
(N=346,442)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
Asian	-0.021	-0.010	-0.002	-0.022	-0.055*
(N=5,916)	(0.020)	(0.022)	(0.027)	(0.024)	(0.032)
Black	0.006	0.009	0.023	-0.002	0.007
(N=57,139)	(0.011)	(0.011)	(0.015)	(0.014)	(0.019)
Hispanic	0.008	0.011	0.001	0.008	0.017
(N=59,940)	(0.011)	(0.012)	(0.022)	(0.014)	(0.023)
Economic Status					
Non-Econ. Disadvantaged	0.010	0.013*	0.013	0.008	0.018*
(N=279,857)	(0.008)	(0.007)	(0.009)	(0.009)	(0.010)
Econ. Disadvantaged	0.013*	0.019**	0.024**	0.008	0.002
(N=284,914)	(0.008)	(0.008)	(0.011)	(0.008)	(0.010)
Gender					
Male	0.012*	0.016**	0.019**	0.010	0.022**
(N=315,853)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)
Female	0.020***	0.025***	0.021***	0.011	0.026***
(N=309,002)	(0.007)	(0.007)	(0.008)	(0.009)	(0.010)
Grade Level					
Grades 3-5	0.018**	0.022***	0.020**	0.009	0.019*
(N=207,713)	(0.008)	(0.007)	(0.008)	(0.008)	(0.010)
Grades 6-8	0.014*	0.019***	0.023***	0.010	0.015*
(N=210,905)	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)
Test Score Gaps					
White-Black Gap	0.016	0.016	0.008	0.020	-0.002
(N=43,265)	(0.013)	(0.012)	(0.015)	(0.014)	(0.019)
White-Hispanic Gap	0.018	0.017	0.030	0.009	0.031
(N=42,735)	(0.012)	(0.012)	(0.019)	(0.013)	(0.022)
White-Asian Gap	0.004	-0.011	-0.028	-0.007	-0.004
(N=4,934)	(0.023)	(0.025)	(0.028)	(0.026)	(0.029)
Econ. Status Gap	0.000	-0.001	-0.000	0.003	0.007
(N=204,510)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Add. Controls		×	×		
Weighted			×		

Notes: Table 4 presents estimates of the impact of tutoring chain centers on reading test scores across models and student subgroups. Each cell is a separate regression. All estimates include calendar-year and district-grade fixed effects. Column (1) presents the average treatment effect from Equation 4, calculated using two-stage difference-in-differences, estimated on our sample of first-chain and never-chain districts from 2009-2019, and omitting period 0. Column (2) replicates Column (1) with additional controls for grade-level enrollment, grade-level racial shares, district-level unemployment and median income. Column (3) replicates Column (2), weighting district-grade-subgroup observations by the number of test-takers. Column (4) restricts the sample to First Chain districts and their nearest neighbors in terms of median income, total enrollment, share Asian, and share of students in suburban schools. Column (5) defines districts as treated if there is a chain center in a 10-mile radius of the district centroid. Standard errors are bootstrapped and clustered by school district. "Test Score Gaps" refer to the within-district gap in test scores between student subgroups. Test score data and additional controls come from SEDA. Tutoring center locations come from Data Axle. Counts of child population come from the ACS. Number of observations are the count of district-grade-years in the subgroup specifications from Columns (1)-(3). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Alternative Restrictions on Required Years of Observation

Required Years of Obs.	11 years	**10 years**	9 years	7 years	5 years	2 years
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.029***	**0.029*** **	0.030***	0.028***	0.028***	0.029***
	(0.011)	**(0.008)**	(0.008)	(0.007)	(0.008)	(0.009)
White/Asian	0.050***	**0.040*** **	0.042***	0.041***	0.042***	0.043***
	(0.011)	**(0.009)**	(0.009)	(0.008)	(0.008)	(0.008)
Black/Hisp./Nat. Am.	0.021	**0.018**	0.022*	0.024**	0.018*	0.018
	(0.015)	**(0.013)**	(0.012)	(0.011)	(0.011)	(0.011)
Non-Econ. Disadvantaged	0.026**	**0.021** **	0.023**	0.023**	0.024***	0.025***
	(0.011)	**(0.009)**	(0.009)	(0.009)	(0.008)	(0.009)
Econ. Disadvantaged	0.025**	**0.018** **	0.016*	0.016	0.014	0.016
	(0.013)	**(0.009)**	(0.009)	(0.010)	(0.011)	(0.010)
Number of Obs.	261,536	**381,356**	417,536	448,108	483,868	499,132

Notes: Table 5 presents the average treatment effect of tutoring chain entry on math test scores under alternative minimum-observation requirements, replicating the specification of Column (1) of Table 3. Each Column imposes a different threshold for the minimum number of years a district-grade observation must appear in the SEDA test score panel to be included in the sample: 11 years (Column (1)), 10 years (Column (2), bolded, the baseline specification used throughout the paper), 9 years (Column 3), 7 years (Column 4), 5 years (Column 5), and 2 years (Column 6). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Impact of Tutoring Chain Centers, Continuous Measure

	2-Stage DiD (1)	TWFE per 1,000 (2)	TWFE per 1,000 (3)
Panel A. Math			
All	0.034***	0.044**	0.026***
	(0.008)	(0.017)	(0.009)
White/Asian	0.043***	0.063***	0.031***
	(0.009)	(0.018)	(0.010)
Black/Hisp./Nat. Am.	0.021*	0.068	0.042**
	(0.013)	(0.042)	(0.019)
Non-Econ. Disadvantaged	0.025***	0.035**	0.019**
	(0.010)	(0.017)	(0.009)
Econ. Disadvantaged	0.025**	0.046	0.027*
	(0.010)	(0.032)	(0.014)
Number of Obs.	381,356	381,356	461,015
Panel B. Reading			
All	0.021***	0.026*	0.011
	(0.007)	(0.015)	(0.008)
White/Asian	0.021***	0.036**	0.013
	(0.008)	(0.016)	(0.009)
Black/Hisp./Nat. Am.	0.022***	0.033	0.021
	(0.008)	(0.037)	(0.017)
Non-Econ. Disadvantaged	0.013*	0.026*	0.010
	(0.007)	(0.015)	(0.008)
Econ. Disadvantaged	0.018**	0.004	0.007
	(0.008)	(0.026)	(0.012)
Number of Obs.	418,618	418,738	508,907
Add. Controls	X	X	X
Sample	First/Never Chain	First/Never Chain	All Districts

Notes: Table 6 presents estimates of the impact of tutoring chain centers on test scores across models and student subgroups. Each cell is a separate regression. All estimates include calendar-year and district-grade fixed effects and controls for grade-level enrollment, grade-level racial shares, district-level unemployment and median income. Column (1) presents the average treatment effect from Equation 4, calculated using two-stage difference-in-differences, estimated on our sample of first-chain and never-chain districts from 2009-2019, and omitting period 0. Column (2) defines treatment as a continuous measure of the lagged chain tutoring centers per 1,000 children ages 5-17, and reports the coefficient on this treatment variable from a TWFE model. Column (3) replicates Column (2), extending the analysis sample to all school districts from 2009-2019. Column (1) uses bootstrapped standard errors; all standard errors are clustered by school district. Test score data and additional controls come from SEDA. Tutoring center locations come from Data Axle. Counts of child population come from the ACS. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

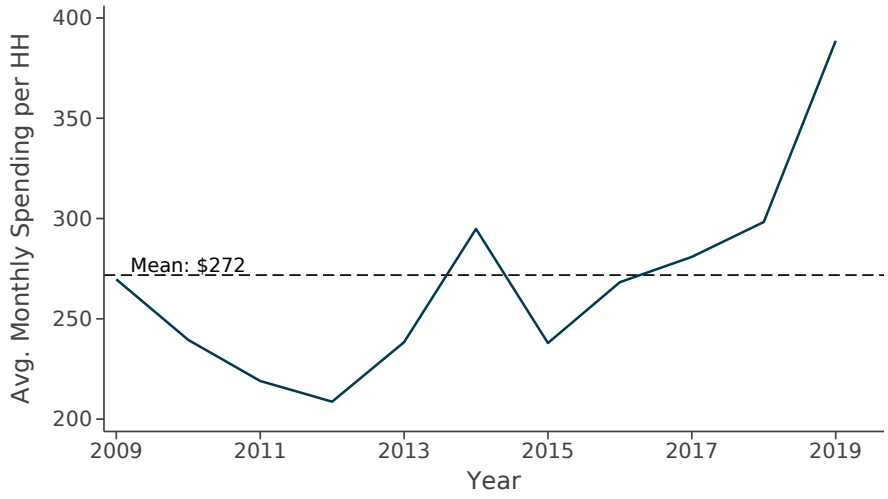
Table 7: Effect of Non-Chain and In-Home Tutoring Centers on Test Scores

Model	Non-Chains		In-Home Chains	
	Math	Reading	Math	Reading
	(1)	(2)	(3)	(4)
All Students	-0.002	0.003	0.009	0.005
	(0.009)	(0.006)	(0.011)	(0.008)
Race/Ethnicity				
White	-0.007	0.002	0.015	0.002
	(0.009)	(0.007)	(0.010)	(0.008)
Asian	-0.012	-0.044	0.026	-0.020
	(0.039)	(0.030)	(0.019)	(0.015)
Black	0.006	0.017	0.001	-0.005
	(0.017)	(0.014)	(0.019)	(0.013)
Hispanic	0.018	0.025**	0.010	0.008
	(0.015)	(0.012)	(0.012)	(0.010)
Economic Status				
Non-Econ. Disadvantaged	-0.013	-0.003	0.016	0.005
	(0.009)	(0.007)	(0.011)	(0.009)
Econ. Disadvantaged	-0.003	0.007	-0.003	0.009
	(0.010)	(0.007)	(0.011)	(0.009)
Gender				
Male	-0.008	-0.002	0.014	0.003
	(0.009)	(0.007)	(0.011)	(0.007)
Female	-0.008	0.007	0.005	0.005
	(0.009)	(0.006)	(0.010)	(0.008)
Grade Level				
Grades 3-5	-0.008	-0.001	0.008	0.000
	(0.010)	(0.009)	(0.011)	(0.010)
Grades 6-8	0.006	0.007	0.008	0.010
	(0.009)	(0.006)	(0.012)	(0.008)
Test Score Gaps				
White-Black Gap	-0.018*	-0.022**	0.024**	0.012
	(0.010)	(0.011)	(0.012)	(0.009)
White-Hispanic Gap	-0.008	-0.004	0.011*	-0.001
	(0.010)	(0.010)	(0.007)	(0.007)
White-Asian Gap	0.043	0.043	0.010	0.029**
	(0.032)	(0.027)	(0.015)	(0.012)
Econ. Status Gap	-0.007	-0.002	0.006	-0.004
	(0.006)	(0.006)	(0.007)	(0.007)
Number of Obs.	339,303	372,672	441,540	486,508

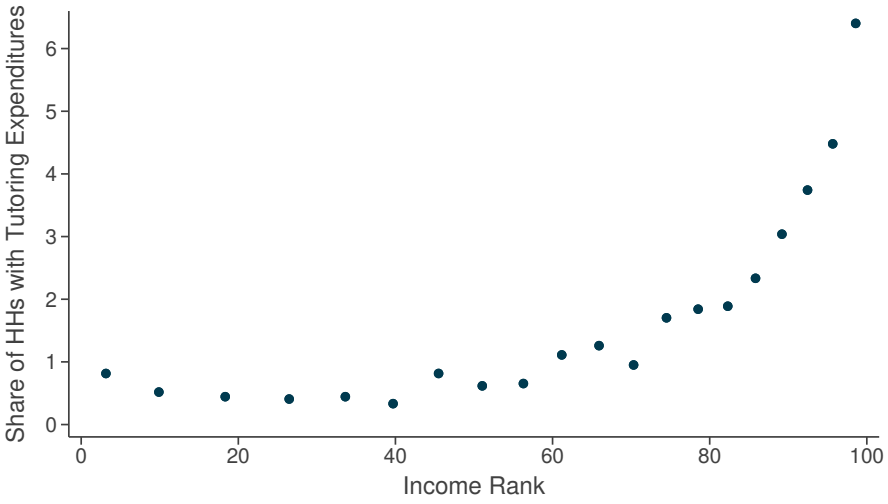
Notes: Table 7 presents two-stage difference-in-differences estimates of the effect of non-chain and in-home chain tutoring center entry on district-grade test scores. The treatment variable is an indicator for whether a school district has received its first non-chain (Columns (1) and (2)) or in-home chain tutoring center (Columns (3) and (4)), respectively. Columns (1) and (3) report impacts on math test scores; Columns (2) and (4) on reading. Each cell is a separate regression. All specifications include district-grade and year fixed effects but no additional controls. The average treatment effect for each regression is calculated using equation (4), omitting period 0. Standard errors are bootstrapped and clustered by school district. Rows report estimates for all students and for subgroups defined by race/ethnicity, economically disadvantaged status, gender, grade level, and within-district test score gaps. Tutoring center locations and chain classifications come from Data Axle. School district boundaries and test score data come from SEDA. The sample contains first-treated and never-treated school districts from 2009–2019 with at least 10 years of observed test score data. For more details on sample construction, see section 5.1. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Tutoring Expenditure Patterns

(a) Monthly Tutoring Expenditure Among HHs with Tutoring Expenditures

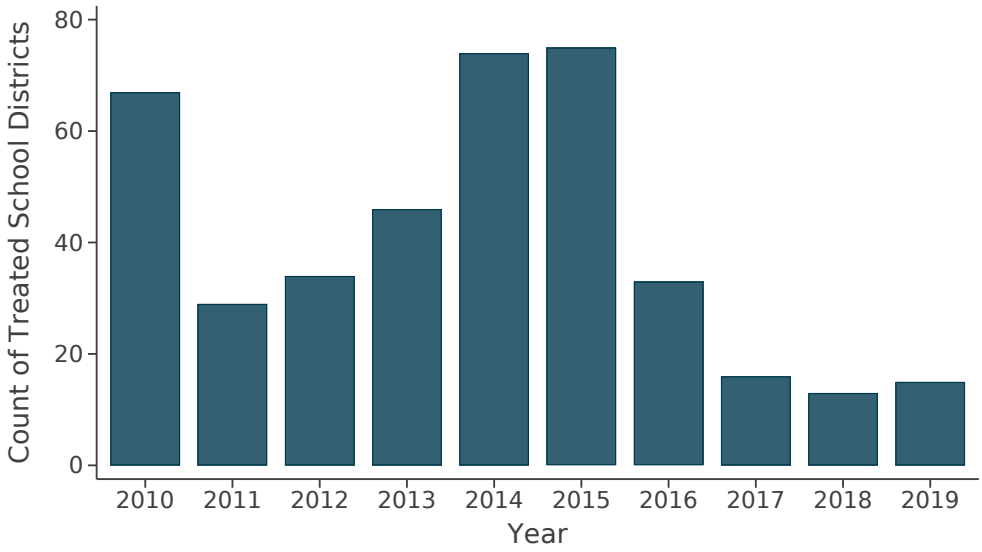


(b) Share of HHs with Tutoring Expenditure by Income Rank



Notes: Figure A.1a plots the average monthly spending of households on tutoring services by year among households with nonzero tutoring expenditures. The dashed line shows the overall mean, $272. Dollar values are inflation-adjusted to 2015 dollars. Figure A.1b plots the share of households reporting nonzero tutoring expenditures for each ventile of the national income distribution. All statistics use CEX sample weights. Data come from the Consumer Expenditure Survey, 2009-2019. For more details on the sample construction, see section 3.2.

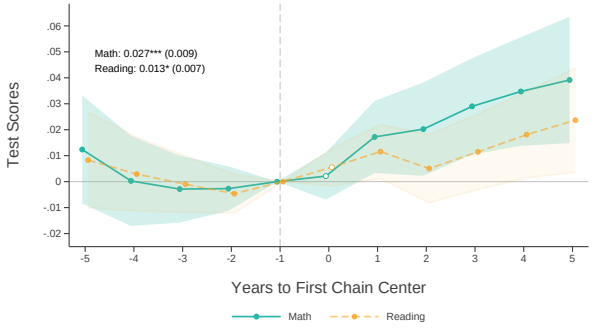
Figure A.2: Count of Treated Districts by Year



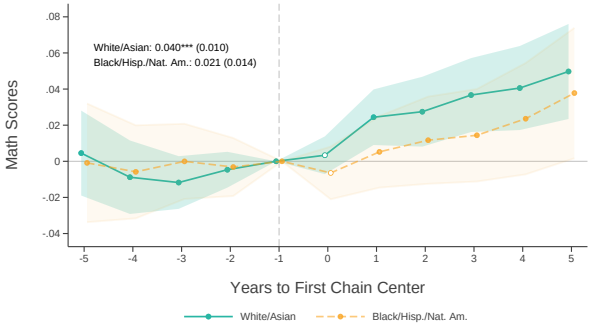
Notes: Figure A.2 plots the number of school districts that gained their first chain tutoring center by year from 2010 to 2019. Tutoring chain locations come from Data Axle. School district boundaries come from SEDA.

Figure A.3: First Chain Entry with Callaway and Sant'Anna (2021)

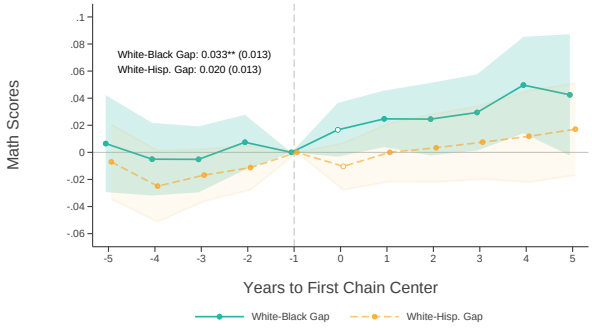
(a) All Students



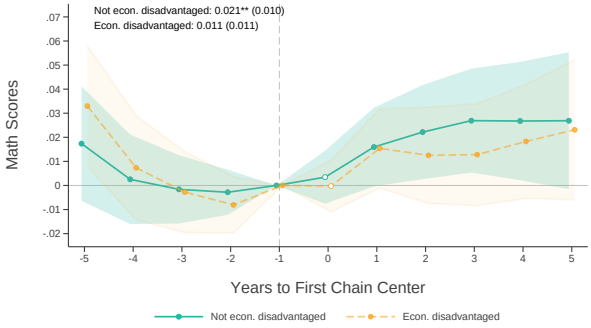
(b) By Race/Ethnicity



(c) Racial Test Score Gaps

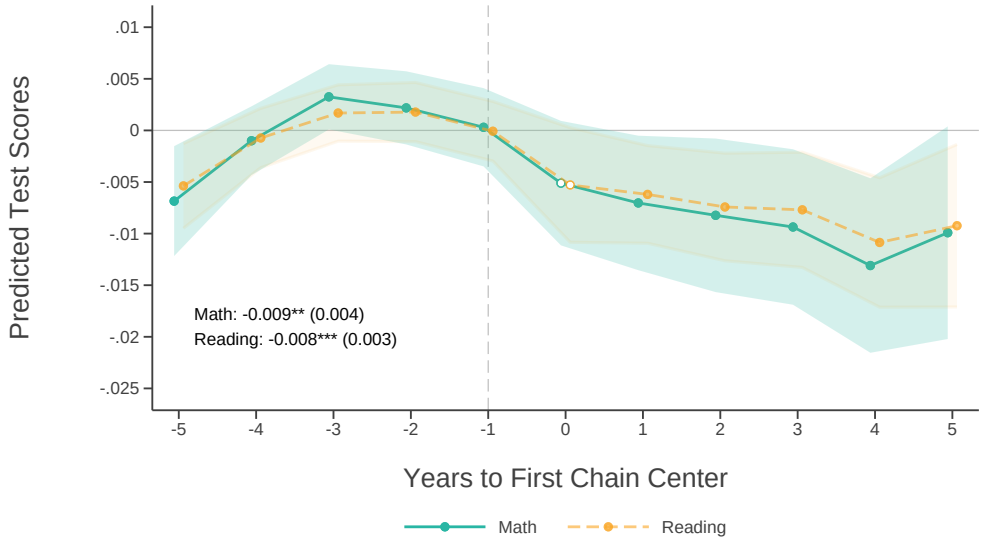


(d) By Economic Disadvantaged Status



Notes: Figure A.3 replicates our baseline two-stage difference-in-difference results using the staggered event study method from Callaway and Sant'Anna (2021). Panel (a) replicates Figure 5a. Panels (b)-(d) replicate Figures 6a, 6b, and 8a respectively. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

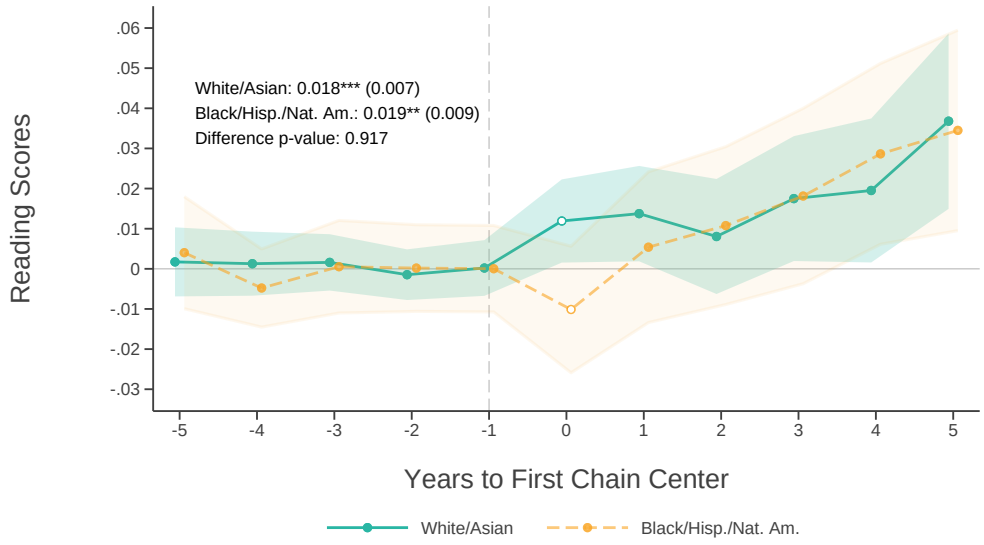
Figure A.4: Effect of First Tutoring Chain on Predicted Test Scores



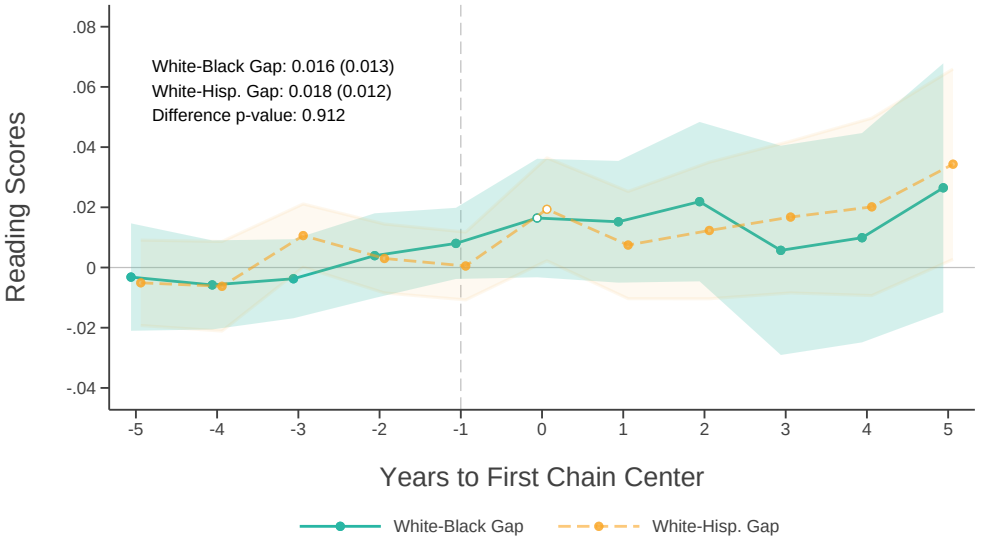
Notes: Figure A.4 replicates Figure 5 with predicted test scores as the outcome. Predicted test scores are constructed by regressing 2009 test district-grade test scores on grade-level enrollment and racial shares, grade-level indicators, and district-level unemployment and median income, and then generating predicted scores from this model for all years. The solid blue line does this exercise for math test scores, the dashed orange line for reading test scores. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.5: Racial Heterogeneity of First Tutoring Chain on Reading Test Scores

(a) By Race/Ethnicity



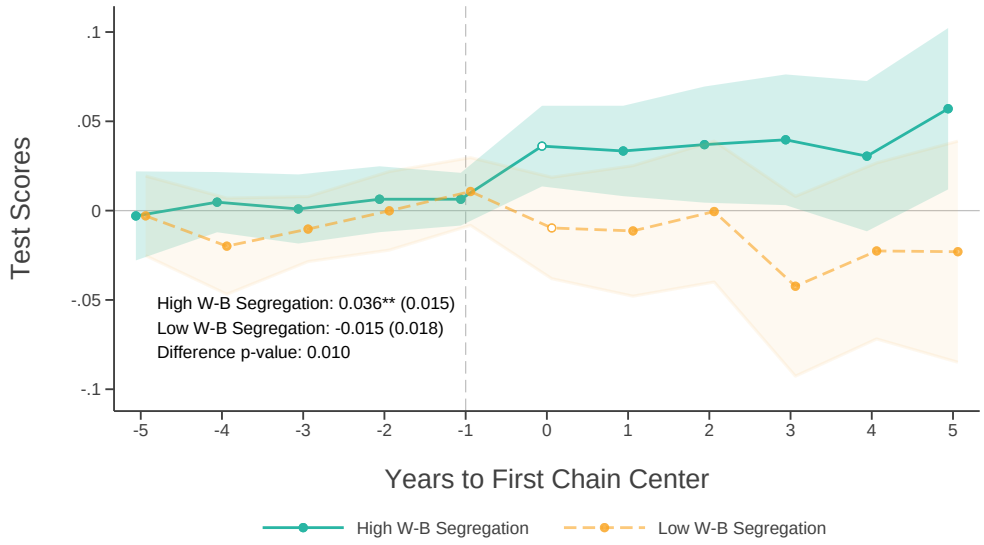
(b) Racial Test Score Gaps



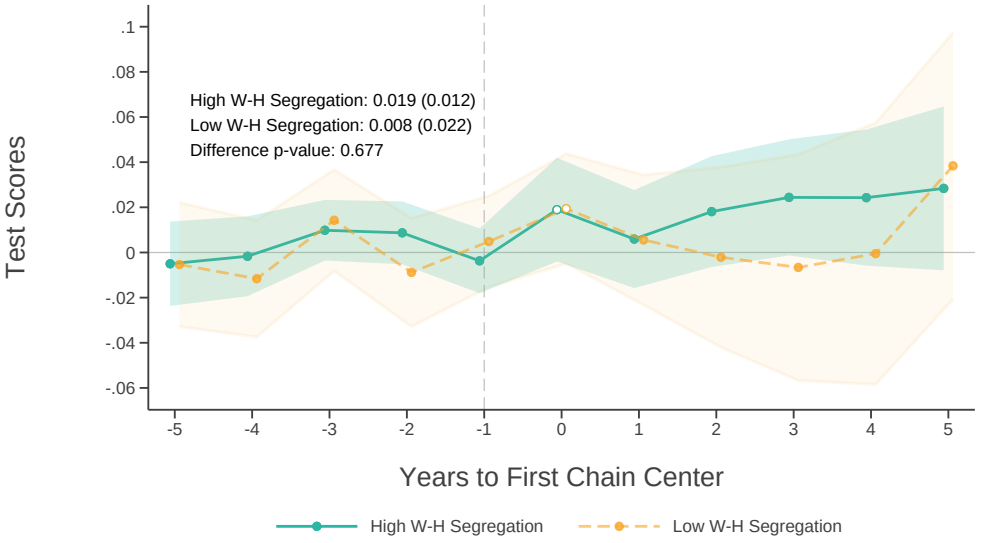
Notes: Figure A.5 replicates Figure 6 using reading test scores. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.6: Effect of First Tutoring Chain on Reading Score Gaps by Racial Segregation

(a) White-Black Reading Score Gap



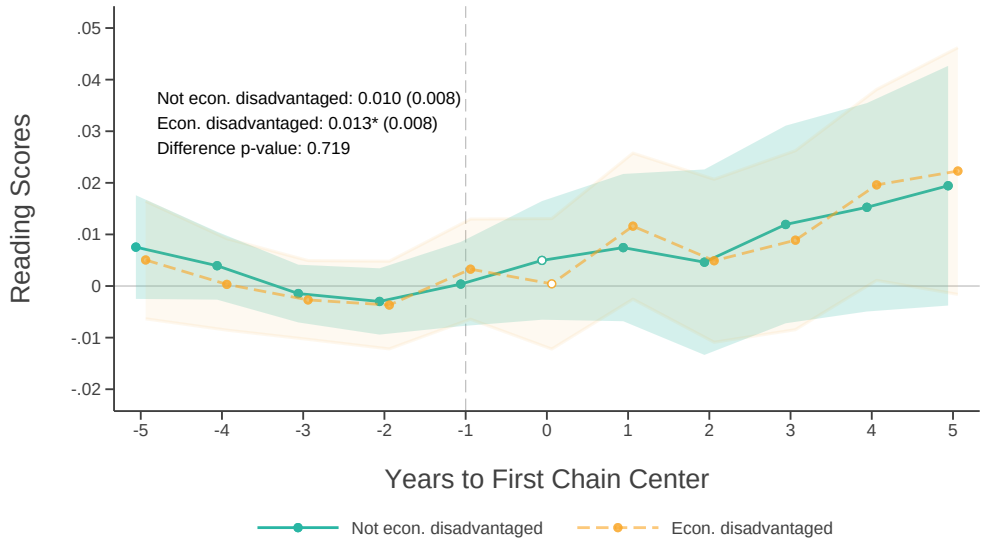
(b) White-Hispanic Reading Score Gap



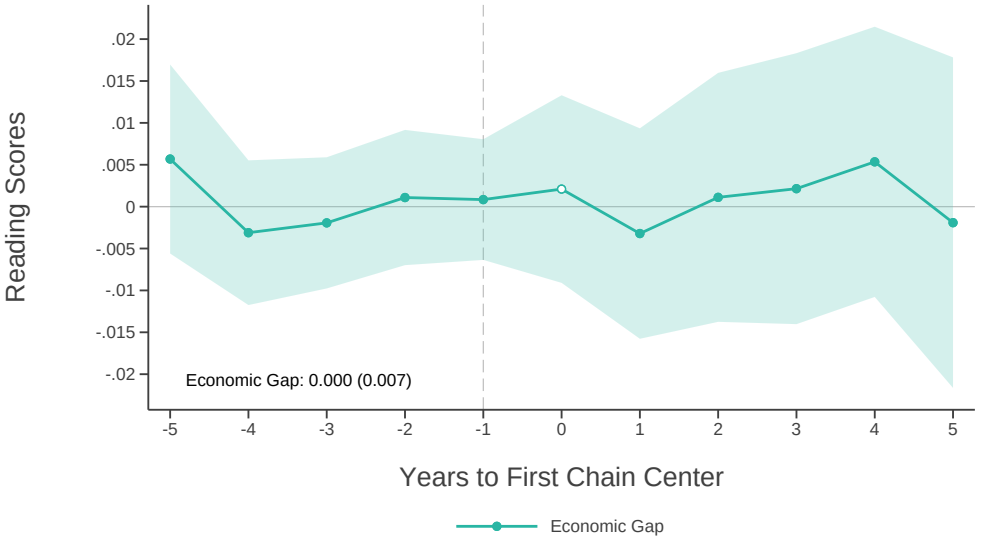
Notes: Figure A.6 replicates Figure 7 using reading test scores. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.7: Economic Heterogeneity of First Tutoring Chain on Reading Test Scores

(a) By Economic Disadvantaged Status



(b) Economic Test Score Gap



Notes: Figure A.7 replicates Figure 8 using reading test scores. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.1: Heterogeneity by Tutoring Brand

	Kumon		Sylvan		Other Chains	
	Math (1)	Reading (2)	Math (3)	Reading (4)	Math (5)	Reading (6)
Panel A: All Control Districts						
All	0.029***	0.018*	0.019	0.005	0.041**	0.024
	(0.011)	(0.010)	(0.013)	(0.011)	(0.019)	(0.016)
White/Asian	0.047***	0.030***	0.026	0.006	0.043**	0.012
	(0.013)	(0.010)	(0.018)	(0.011)	(0.019)	(0.018)
Black/Hisp./Nat. Am.	0.024	0.020*	0.005	0.001	0.001	0.016
	(0.018)	(0.012)	(0.018)	(0.016)	(0.029)	(0.020)
Non-Econ. Disadvantaged	0.035***	0.020*	0.006	-0.005	0.024	0.013
	(0.013)	(0.010)	(0.018)	(0.013)	(0.018)	(0.017)
Econ. Disadvantaged	0.016	0.022*	0.005	-0.009	0.048**	0.033**
	(0.015)	(0.011)	(0.017)	(0.013)	(0.023)	(0.016)
Number of Obs.	413,446	453,983	405,793	445,775	416,571	458,121
Panel B: Nearest Matching Neighbor						
All	0.004	-0.009	0.035*	0.007	0.044**	0.012
	(0.014)	(0.012)	(0.019)	(0.012)	(0.022)	(0.018)
White/Asian	0.014	0.001	0.046***	0.010	0.032	-0.007
	(0.014)	(0.013)	(0.017)	(0.013)	(0.021)	(0.018)
Black/Hisp./Nat. Am.	0.004	0.005	0.016	-0.003	0.021	0.019
	(0.019)	(0.016)	(0.026)	(0.019)	(0.032)	(0.021)
Non-Econ. Disadvantaged	0.012	-0.004	0.026	-0.004	0.016	0.002
	(0.014)	(0.012)	(0.020)	(0.015)	(0.022)	(0.019)
Econ. Disadvantaged	0.004	0.004	0.020	-0.010	0.067**	0.023
	(0.017)	(0.013)	(0.022)	(0.015)	(0.027)	(0.017)
Number of Obs.	13,818	14,768	13,225	14,799	8,055	9,227

Notes: Table A.1 presents two-stage difference-in-difference estimates of the effect of first tutoring chain entry on district-grade test scores by tutoring brand. Panel A replicates our baseline specification from Column (1) of Table 3, splitting treated districts by the brand of their first tutoring chain and comparing to all control districts. Panel B limits control districts to the nearest matching neighbors for the treated districts of each brand, where nearest matching neighbors is defined as in Column (4) of Table 3. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Treatment Effects by District Enrollment

	Below Median Size		Above Median Size	
	Math	Reading	Math	Reading
	(1)	(2)	(3)	(4)
All	0.039***	0.020*	0.023**	0.007
	(0.013)	(0.011)	(0.011)	(0.008)
White/Asian	0.043***	0.017	0.035***	0.014
	(0.015)	(0.012)	(0.012)	(0.009)
Black/Hisp./Nat. Am.	0.020	0.026	0.020	0.011
	(0.025)	(0.021)	(0.014)	(0.011)
Non-Econ. Disadvantaged	0.028*	0.012	0.027**	0.012
	(0.016)	(0.012)	(0.013)	(0.009)
Econ. Disadvantaged	0.007	-0.002	0.024*	0.012
	(0.018)	(0.015)	(0.013)	(0.008)
Number of Obs.	331,252	363,015	50,104	55,603

Notes: Table A.2 presents two-stage difference-in-difference estimates of the effect of first tutoring chain entry on district-grade test scores by district enrollment size. School districts are above median size when mean per-grade enrollment over the panel years is greater than the median of that measure among districts that ever receive a chain center during the panel window, and below median size otherwise. Test score data and additional controls come from SEDA. Tutoring center locations come from Data Axle. Counts of child population come from the ACS. Number of observations are the count of district-grade-years in specification from the first row, where the outcome is all students' test scores. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.