



The Returns to Private Tutoring Centers

Marty Haoyuan Chen

University of Maryland

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The Returns to Private Tutoring Centers*

Marty Haoyuan Chen[†]

University of Maryland

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Abstract

Private tutoring centers have expanded rapidly across the United States, yet there is little evidence on whether these centers improve student outcomes. Using business records on tutoring establishments linked to administrative data covering over five million public school students in Texas, this paper estimates the short- and long-run impacts of tutoring centers. Exploiting variation from the entry and exit of tutoring centers near students' high schools, I show that increased tutoring center exposure has no effect on state standardized test scores or high school graduation, but raises college admission exam scores, shifts students toward higher-quality and out-of-state colleges, and increases their early-career earnings. These gains are entirely concentrated among students who are not economically disadvantaged. An original survey of Texas parents confirms that tutoring participation responds to the availability of nearby centers and documents wide gaps between advantaged and disadvantaged families in both tutoring use and spending. The rapid expansion of the private tutoring industry has thus widened the gap between economically disadvantaged students and their more advantaged peers in both educational investment and long-run outcomes.

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[†]Department of Economics, University of Maryland (hchen915@umd.edu).

1 Introduction

American families invest heavily in their children’s education, and over the past three decades that investment has intensified most among high-income, college-educated parents (Guryan, Hurst and Kearney, 2008). As admission to selective colleges has grown more competitive, these families have devoted increasing time and money to strengthening their children’s academic credentials (Bound, Hershbein and Long, 2009; Ramey and Ramey, 2010). Private tutoring centers have become a central feature of this landscape: the number of centers in the United States more than tripled between 1997 and 2022, from just over 3,000 to nearly 10,000 (Kim, Goodman and West, 2025). These centers offer a private supplement to classroom instruction, serving as a key academic resource outside mainstream schooling.

Despite the industry’s growth, little is known about the returns to private tutoring or about the distributional consequences of its expansion. Existing work largely measures effects on early-grade test scores and evaluates tutoring programs that are publicly provided or subsidized (Fryer Jr, 2014; Kraft, 2015; Guryan et al., 2023; Nickow, Oreopoulos and Quan, 2024). It therefore remains unknown whether tutoring affects the longer-run outcomes that drive intense parental investment, such as where students attend college and how they perform in the labor market. Unlike the publicly provided programs in existing evaluations, which typically target low-achieving or low-income students, private tutoring is costly, and its use is likely concentrated among children of high-income families. The growth of the industry may therefore widen existing gaps in educational attainment and earnings.

This paper provides causal evidence on how private tutoring centers affect students’ trajectories, from high school achievement through early-career earnings. Specifically, it addresses three questions. First, does exposure to tutoring centers during high school improve academic performance? Second, does it change where students apply to, enroll in, and graduate from college? Third, do any of these effects persist into the labor market? For each of these questions, I also examine how the effects vary across student demographics. To interpret my results, I complement the main analysis with an original survey of Texas parents that measures tutoring participation, spending, and how participation responds to the supply of tutoring centers nearby.

To answer these questions, I link two sources of data in Texas. The first consists of geocoded business records covering the universe of private tutoring establishments from 1997 to 2025, which allow me to measure the tutoring options near every public school in the state in each year. The second consists of statewide administrative records covering the universe of Texas public school students, which follow the same student through K–12 schooling and college and into the labor market. For more than five million students, the linked data connect exposure to local tutoring centers during high school to outcomes at each subsequent stage: achievement and graduation in high school; college applications, enrollment, and degree completion; and early-career earnings.

The empirical strategy exploits the rapid expansion of the private tutoring industry in Texas since the 1990s. The timing and location of tutoring center entry and exit varied widely across the state, so successive cohorts at the same school could face very different numbers of nearby

tutoring centers while enrolled in high school. I estimate the effects of tutoring center exposure on student outcomes using a two-way fixed effects specification that compares students who attended the same high school in different cohorts, relative to students at other schools in the same county and cohort. High school fixed effects absorb all time-invariant differences across schools, so identification comes from the within-school movement in nearby tutoring density that center entry and exit generate. County-by-cohort fixed effects further remove countywide shocks that could affect both tutoring supply and student outcomes.

To implement this strategy, I construct a measure of tutoring center exposure for every public high school cohort in Texas. The business records report the geographic coordinates and operating years of each tutoring establishment in the state, from national chains such as Kumon and Sylvan Learning to independent local providers. I match these establishments to the coordinates of every public high school and count the tutoring centers operating within two miles of each school in each year, a radius that approximates the neighborhood from which a Texas high school draws its students. The sample period spans substantial industry growth. The number of tutoring centers in Texas rose from 234 in 1997 to more than 900 by 2025, and the expansion occurred along both the extensive and the intensive margin: the share of public schools with at least one tutoring center within two miles increased from 19 to 37 percent, while the maximum number of centers within two miles of a single campus increased from 16 to 21. From the school-level counts, I construct the main treatment variable: the average number of tutoring centers near a student's ninth-grade campus over her four expected high school years. For outcomes realized during high school, such as state standardized test scores, the exposure window runs instead from the start of high school through the tested grade.

Throughout the main analysis, I focus on students who are not economically disadvantaged, the group most likely to take advantage of private, commercial tutoring.¹ First, I find that increased exposure to local tutoring centers has no effect on high school outcomes: my estimates rule out effects larger than 0.007 standard deviations in math and 0.011 in reading on state standardized test scores from one additional tutoring center near a student's school. Similarly, tutoring center exposure has no impact on high school graduation. Second, one additional tutoring center raises SAT scores by 1.66 (standard error 0.52) points and ACT scores by 0.07 (0.02) points. Scaling these intent-to-treat (ITT) estimates by first stages derived from my survey of Texas parents and from tutoring center capacity, which I discuss later in this section, implies treatment-on-the-treated (TOT) effects of roughly 8 to 13 SAT points and 0.3 to 0.6 ACT points per year of tutoring center attendance. The improved performance on college admission exams helps students enroll at higher-quality and more selective institutions: one additional tutoring center raises the average early-career earnings of enrollees at a student's college by \$61 (31) and increases enrollment at elite colleges by 0.20 (0.07) percentage points. Overall four-year enrollment does not change, but students shift from in-state toward out-of-state four-year colleges, with out-of-state enrollment rising

¹Economically disadvantaged students are defined as those who were ever eligible for free or reduced-price lunch before ninth grade. I estimate the same specifications for this group and find null effects across nearly every outcome.

by 0.34 (0.09) percentage points and in-state enrollment falling by 0.26 (0.11) percentage points. Together, the contrast between null effects on state assessments and gains on college outcomes points toward high school students mainly using private tutoring for college preparation, a pattern my survey of Texas parents confirms. Third, an additional tutoring center raises students' average annual earnings in their late twenties by \$241 (87), or 0.5 (0.2) percent. The earnings gain is large relative to the shift in college quality, which suggests that tutoring centers also affect earnings through channels beyond college choice, such as skills that carry directly into the labor market or a better match between students and colleges that the enrollment measures do not capture. These results are robust to an alternative treatment definition that measures exposure to tutoring centers in the ninth grade alone.

The mean effects on college admission exams and earnings do not reveal where in the distribution the gains arise. Estimating effects on unconditional quantiles of each outcome, I find that tutoring center exposure mainly shifts the top of the SAT, ACT, and earnings distributions. The effects on SAT and ACT scores rise from the bottom to the top of the distribution, reaching 6.30 and 0.19 points at the ninetieth percentile and widening the 90–10 interquantile range by 8.49 SAT points and 0.19 ACT points. The effect on earnings follows the same pattern, indistinguishable from zero through the sixth decile and rising to \$898 at the ninetieth percentile. The mean effects therefore reflect a shift in the upper part of each distribution rather than a uniform shift across all non-economically disadvantaged students.

The identifying assumption is that, conditional on predetermined student characteristics, time-varying school characteristics, high school fixed effects, and county-by-cohort fixed effects, the density of tutoring centers near a student's ninth-grade campus is mean independent of the counterfactual path of that high school's student outcomes. In other words, after absorbing time-invariant school differences and county-specific cohort shocks and accounting for student demographics and observable school characteristics, the timing and intensity of tutoring center entry near a high school are unrelated to the outcomes its students would have realized absent that entry. This is most plausible if remaining variation in tutoring center density reflects supply-side determinants of where and when firms operate, such as commercial real estate availability, lease turnover, and firm-level expansion decisions, none of which has a direct impact on student achievement. There are two main threats to this assumption. First, firms may enter neighborhoods where student outcomes are improving or declining, in which case the estimates would partially reflect the trajectories that attracted tutoring centers. Second, center entry may also respond to unobserved, time-varying local conditions, such as improving school quality or the sorting of more educated families into a neighborhood, that affect students through channels other than tutoring.

This paper addresses these threats to the identifying assumption using three exercises. First, I estimate a distributed lag model that recovers the cumulative effect of tutoring centers by a cohort's years of exposure. If tutoring centers entered neighborhoods based on local student trajectories, tutoring center entry would be correlated with student outcomes even when entry occurs after students have left high school and no longer use the centers. I find no such pattern: addi-

tional tutoring centers after a cohort's projected high school graduation have no effect on student outcomes, and the effects build over the years students spend in high school. Second, I estimate how the effect varies with the distance between tutoring centers and a student's school. The effects are concentrated among the nearest centers and decline to zero for centers too distant for students to plausibly attend, a pattern difficult to reconcile with broader neighborhood trends driving the results. Third, I conduct a placebo test using an identically constructed measure of exposure to pet care establishments, an industry that sorts across neighborhoods on the same student characteristics as tutoring centers but has no plausible channel to student outcomes. Substituting pet care exposure for tutoring exposure in the main specification yields null effects on most outcomes, and adding it alongside tutoring exposure leaves the tutoring estimates unchanged in sign, magnitude, and significance. Complementing these tests, I estimate the same specifications for economically disadvantaged students, who attend the same high school and face the same local conditions but for whom private tutoring is largely unaffordable. Their estimates are null across nearly every outcome, consistent with the observed effects operating through tutoring participation.

The null estimates for economically disadvantaged students indicate that the industry's expansion widens socioeconomic gaps in college outcomes and early-career earnings among students who face the same local availability of tutoring centers. These gaps could reflect differences in take-up or differences in the returns to tutoring. My survey, described below, points to differential take-up as an important contributing factor. Beyond economic status, I examine heterogeneity along several further dimensions. Across student subgroups, the effects are concentrated among White students, whose estimates are similar in magnitude to those of non-economically disadvantaged students, and are largely absent for Hispanic and Black students. Private tutoring centers have therefore also widened racial gaps in students' long-run outcomes. Gains emerge both for students in gifted programs and for students designated at risk of dropping out, suggesting that the benefits of tutoring centers depend on family resources more than on prior achievement. Across high schools, most of the effects are concentrated at schools with a below-median share of economically disadvantaged students, where even disadvantaged students gain on the SAT, though their gains do not extend to college enrollment or earnings. At schools with an above-median share, the estimates are mostly null or negative. The college and earnings effects are also concentrated at schools that already had a tutoring center nearby at the start of the sample period, suggesting that additional centers deepen advantages in markets the industry has already entered more than they extend access to new ones. Finally, both chain and independent tutoring centers contribute to the effects. Chain centers produce larger estimated effects on most outcomes, but the differences between the two types are generally not statistically significant.

To supplement the main analysis with direct evidence on tutoring participation and its response to local tutoring center availability, and to infer the effects of private tutoring on the students who participate, I design and implement an original survey of 1,150 parents of students in K-12 in Texas. The survey was fielded in July 2026 through Cint, an online survey panel widely used in academic research. The survey results show that private tutoring is widespread and un-

equally used.² In my sample, 25 percent of Texas children currently receive paid private tutoring and 9 percent attend a tutoring center, and participating households spend roughly \$2,800 per year. Participation and spending are concentrated among high-income, college-educated families: 13 percent of non-disadvantaged children attend tutoring centers compared with 3 percent of disadvantaged children, and conditional on participating, non-disadvantaged households spend roughly \$3,200 per year compared with \$1,200. Among parents of tutored high school students, the most commonly cited reasons for enrollment are standardized test preparation (58 percent), competitive college admission (54 percent), and the child struggling academically (47 percent). Consistent with these motives, 50 percent of tutored high school students prepare for the SAT or ACT and 31 percent receive college preparation or counseling services. Among tutored K–8 students, by contrast, most (54 percent) focus on state standardized tests. Linking each child’s school ZIP code to the business records, I estimate that one additional tutoring center within two miles is associated with a 2.8 percentage point increase in tutoring center participation among high school students, with the response concentrated among non-disadvantaged children at 3.1 percentage points and no detectable response among disadvantaged children. The increase in tutoring center participation is accompanied by a decline in one-on-one tutoring, suggesting that tutoring centers partly substitute for individual tutors.

This participation response provides a basis for deriving implied TOT effects from the ITT estimates. The treatment in the main analysis averages tutoring center exposure over the four years of high school, while the survey measures participation in a single year. Accumulating the annual response of non-disadvantaged students over the four years of high school implies that the effect of a year of center-based tutoring is roughly 8 times the ITT estimates. I supplement this first stage with an alternative calculation that derives take-up from the capacity of a typical tutoring center and the number of high school students it serves, which yields a scaling factor of roughly 5.

Scaling the ITT estimates by these factors generates the following TOT effects. A year of center-based tutoring raises the SAT scores of non-economically disadvantaged students by 8 to 13 points (0.04 to 0.07 standard deviations) and their ACT scores by 0.3 to 0.6 points (0.07 to 0.11 standard deviations). It moves students to colleges whose enrollees earn \$290 to \$490 more per year, raises elite college enrollment by 0.9 to 1.6 percentage points, and shifts enrollment from in-state toward out-of-state four-year colleges by roughly 1 to 3 percentage points on each margin. It also raises earnings in the late twenties by \$1,130 to \$1,940 per year, or 2 to 4 percent. The college admission exam gains are smaller than the 0.3 to 0.4 standard deviation effects reported for publicly provided tutoring programs, which serve struggling, low-income students and are evaluated on state tests rather than admission exams (Nickow, Oreopoulos and Quan, 2024; Dietrichson et al., 2017). The annual earnings gain is nevertheless a substantial share of what a participating household spends on tutoring in a year, indicating a high return to tutoring center participation. It is worth empha-

²The survey is weighted using iterative proportional fitting to match the 2024 American Community Survey on school type, household income, race and ethnicity, grade level, and economic disadvantage.

sizing that these estimates measure the effect of participation in a tutoring center, which captures the combined effect of the bundle of services that induced participants received, rather than the return to any particular service, such as test preparation or college counseling.

Related literature.— This paper contributes to four strands of literature. First, and most directly, it contributes to the literature on private tutoring. In the United States, existing work is largely descriptive: [Buchmann, Condron and Roscigno \(2010\)](#) link test preparation to SAT performance and college enrollment in survey data, and [Kim, Goodman and West \(2025\)](#) document the rapid growth of tutoring centers and their concentration in high-income, highly educated communities. A larger international literature studies private tutoring in the education systems of East Asia and elsewhere, where the industry is far more established (e.g., [Dang and Rogers, 2008](#); [Bray and Kwok, 2003](#); [Dang, 2007](#); [Kim and Lee, 2010](#); [Atalmis, Yilmaz and Saatcioglu, 2016](#); [Bhorkar and Bray, 2018](#); [Chen et al., 2025](#); [Kim, Tertilt and Yum, 2024](#); [Gortazar, Hupkau and Roldán-Monés, 2024](#); [Davies, 2004](#); [Sawada et al., 2024](#)). The paper most closely related to mine is a concurrent study by [Goodman and Nitkin \(2026\)](#), who examine the entry of chain tutoring centers into U.S. school districts and find positive effects on district-level test scores in grades three through eight. The math gains are concentrated among White and Asian students, so that chain entry widens racial gaps in math achievement. My paper complements their analysis along several dimensions. Using student-level administrative data, I compare students within the same school, control for individual characteristics measured before high school, and hold student composition fixed as exposure to tutoring centers changes. In addition, the linked records follow students for more than a decade after exposure, allowing me to trace effects from high school outcomes through college choice, degree completion, and early-career earnings.

Second, this paper contributes to the literature on tutoring as an educational intervention. Tutoring is among the most effective interventions education research has produced: meta-analyses of randomized evaluations report pooled effects of roughly 0.3 to 0.4 standard deviations on achievement, the largest gains among interventions targeting low-income students ([Nickow, Oreopoulos and Quan, 2024](#); [Dietrichson et al., 2017](#); [Fryer Jr, 2017](#)). Some of the strongest evidence comes from large-scale field experiments in public schools: high-dosage, individualized tutoring raised math and English achievement by 0.15 to 0.25 standard deviations per year in Houston and Boston ([Fryer Jr, 2014](#); [Kraft, 2015](#)).³ The gains extend to older students: [Guryan et al. \(2023\)](#) find that intensive two-on-one math tutoring raised the scores of Chicago high school students by 0.18 to 0.40 standard deviations, concluding that adolescence is not too late for individualized instruction to generate large gains. Not every program succeeds, with weaker effects for reading tutoring at the mean and for publicly contracted private providers ([Fryer Jr and Howard-Noveck, 2020](#); [Chappell et al., 2011](#)), but the central finding holds across settings. Critically, these programs share a common design: they are publicly provided, school-based, or philanthropically funded,

³A related literature finds that individualized instruction delivered through other channels, including online tutoring by volunteers and computer-assisted learning, also generates substantial gains ([Muralidharan, Singh and Gan-
imian, 2019](#); [Escueta et al., 2020](#); [Kraft et al., 2022](#); [Carlana and La Ferrara, 2021](#); [Gortazar, Hupkau and Roldán-Monés, 2024](#)).

they mostly target struggling, low-income students, and they measure effects on achievement while students are still in school. I study tutoring provided through the private market, where families decide whether to purchase it, and follow students through college and into the labor market. The benefits are concentrated entirely among non-economically disadvantaged students, with null effects for economically disadvantaged students, and they appear on long-run outcomes rather than on the state tests that in-school evaluations measure. Whether tutoring narrows or widens educational gaps thus depends on how it is allocated.

Third, this paper speaks to the literature on parental investment and the competition for selective college admission. Children from high-income families are heavily overrepresented at highly selective colleges, even relative to students with comparable test scores, and attending these colleges substantially raises the chances of reaching the top of the earnings distribution (Chetty, Deming and Friedman, 2026). Given these stakes, college-educated parents have sharply increased the time and money they invest in their children (Guryan, Hurst and Kearney, 2008; Bound, Hershebein and Long, 2009). Ramey and Ramey (2010) attribute this increase to a “rug rat race,” in which parents compete for scarce places at selective colleges by investing more in the credentials that admission rewards.⁴ Yet there is little evidence on whether this investment pays off. I identify one key channel of this investment and show that it yields real returns in college and labor market outcomes, concentrated among the families that use it most. The results thus link gaps in family investment to the overrepresentation of advantaged students at elite colleges and to gaps in later-life outcomes.

Finally, this paper relates to the literature on interventions that move students into better colleges and on the returns to college quality. Information, financial aid certainty, exam mandates, and intensive counseling shift students toward more selective or higher-quality institutions (Hoxby and Turner, 2013; Dynarski et al., 2021; Goodman, 2016; Castleman and Goodman, 2018). Studies using rich administrative data find that attending a more selective or higher value-added institution can raise degree completion and earnings, though the size of these returns varies across institutions and students (Zimmerman, 2014; Mountjoy and Hickman, 2021; Mountjoy, 2026; Chetty, Deming and Friedman, 2026; Hendricks, Herrington and Schoellman, 2021). Whereas these studies evaluate publicly provided programs and measure the returns to attending better colleges, my results identify a market-supplied input that both moves students into them and raises their earnings.

The remainder of the paper proceeds as follows. Section 2 describes the private tutoring industry and the business records I use to measure it. Section 3 describes the student data, the analysis sample, and the outcomes. Section 4 presents the research design. Section 5 reports the main results on high school, college, and labor market outcomes, and Section 6 presents the tests supporting their causal interpretation. Section 7 examines heterogeneity by student demographics, school composition, initial market presence, and type of tutoring business. Section 8 presents the

⁴Consistent with this scarcity, demand has increasingly concentrated on a small set of highly selective institutions whose enrollment has expanded little (Hoxby, 2009; Blair and Smetters, 2021).

survey evidence on tutoring participation, derives the implied treatment-on-the-treated effects, and places their magnitudes in the context of previous work. Section 9 concludes.

2 The Private Tutoring Industry

The private tutoring industry supplies the individualized instruction and test preparation that families purchase outside of school. By 2022, 6 to 7 percent of U.S. families with children aged 6 to 17 paid for tutoring. Among these families, spending averaged about \$437 in months with a tutoring purchase, and monthly spending reached roughly \$1,200 at the 90th percentile and \$2,000 at the 95th percentile (Kim, Goodman and West, 2025). These families are disproportionately higher-income and more educated, and tutoring centers locate in the communities where they live (Goodman and Nitkin, 2026). This section describes the business records on tutoring establishments and documents the expansion of tutoring centers over time.

2.1 Data on Tutoring Centers

I measure the supply of private tutoring centers using Data Axle’s Historical Business Data, a commercial dataset that records the location and characteristics of the universe of business establishments across the United States. The data are compiled annually beginning in 1997 from a combination of telephone directory listings, credit card transaction records, direct telephone verification, web research, news publications, and annual reports. Each record represents a distinct physical business location in a given year. For each establishment, the data contain the business name, street address with geocoded coordinates (longitude and latitude), industry classification codes, and measures of firm size including employee counts.

I define the private tutoring industry as establishments registered as “Exam Preparation and Tutoring” (NAICS code 611691).⁵ This code encompasses businesses primarily engaged in offering tutoring and exam preparation services, including academic tutoring centers, SAT/ACT preparatory instruction, and other forms of supplementary tutoring services provided outside of mainstream K–12 schooling.⁶

This paper focuses on the state of Texas, and I do so for two main reasons. First, Texas is the second largest U.S. state by population, land area, and GDP, and its public school system enrolls more than 11 percent of all K–12 students nationwide. With 254 counties and more than 1,200 independent school districts ranging from large metropolitan areas to mid-sized cities, suburbs, and

⁵The U.S. Census Bureau defines NAICS 611691 as establishments “primarily engaged in offering preparation for standardized examinations and/or academic tutoring services.” Goodman and Nitkin (2026) identify tutoring establishments using SIC codes 829909 (“Tutoring”) and 874868 (“Test Preparation Instruction”). I use NAICS 611691 because it has been the official U.S. industrial classification since 1997, and because its hierarchical structure permits clean separation from adjacent education sub-industries with different customer bases. Approximately 90 percent of NAICS 611691 establishments in my sample carry one of these SIC codes as their primary SIC.

⁶The vast majority of establishments classified under NAICS 611691 provide academic instruction. Fewer than three percent of these establishments have business names indicating music instruction.

rural communities, Texas exhibits substantial variation in both tutoring supply and local demographic composition. Second, Texas maintains linked administrative records on the universe of public school students, tracking individuals from kindergarten through postsecondary education and into the labor market. In the next section, I discuss these datasets in detail. The richness of these records enables a comprehensive assessment of how tutoring centers shape both students' short- and long-run outcomes. An additional advantage of Texas is its low outmigration rate, the lowest of any U.S. state, which limits the scope for attrition in the analyses of postsecondary and labor market outcomes (Aisch, Gebeloff and Quealy, 2014).

Across all years from 1997 to 2025, the historical business records contain 2,044 unique tutoring firms operating across 2,716 distinct physical locations in Texas, for a total of 16,338 establishment-year observations. These Texas observations represent approximately 7 percent of the national establishment-year records, below the state's share of the national K–12 student population.

National franchise chains account for a substantial share of the Texas tutoring market. Chains, defined as firms whose names appear more than five times in a single year, collectively account for 49.8 percent of all establishment-year observations recorded in Texas between 1997 and 2025.⁷ Kumon is the single largest provider, with 2,938 establishment-year observations (18 percent of the total), followed by Sylvan Learning (1,661), Mathnasium (1,014), and Huntington Learning Centers (555). Table A1 summarizes the ten largest chains. The remaining observations consist of independent providers and smaller regional operators. Although chain and independent establishments are similar in number, chains are larger on average, employing 9.0 employees per location compared with 5.5 for independents.

2.2 Exposure to Private Tutoring in Texas

I construct school-level measures of exposure to private tutoring by matching the geocoded coordinates of each tutoring center to the physical addresses of all public schools in Texas. School locations are obtained from the Texas Education Agency Public Open Data Site, which records the street address and geographic coordinates of every public school in the state.⁸ For each school-year cell, I compute the geodesic distance between the school and every active tutoring center operating in that year. I then count the number of tutoring centers located within 1, 2, 3, 5, 10, and 20 miles of each school in each year.

This count is the primary measure of school-level tutoring exposure. Table 1 summarizes this measure across all school-year cells. The average school has 0.2 tutoring centers within 1 mile, 0.7 within 2 miles, 3.4 within 5 miles, and 33.6 within 20 miles. The distribution is highly skewed: the median school has no center within 3 miles, while the most exposed school-year has 33 centers within that radius. The measure varies widely across schools and over time, as centers enter and

⁷Each business is classified as chain or independent based on its name, allowing for minor variation in naming. For example, "Kumon Math & Reading CTR" and "Kumon Math-Reading CTR-Austin" are both counted as Kumon.

⁸I use the 2009 address data as the primary source, supplementing it with 2024 address data for schools whose coordinates are missing in the 2009 records. This gives me 11,258 unique schools and 326,482 unique school-year observations between 1997 and 2025.

exit the market. Over the past several decades, the private tutoring industry in Texas has expanded substantially: Panel (a) of Figure 1 plots the number of active tutoring centers in the state, which rose from 234 in 1997 to 907 by 2025, nearly quadrupling over the period. This growth raised exposure along both the extensive and intensive margins: the fraction of campuses with at least one tutoring center within two miles rose from 19 percent in 1997 to 37 percent in 2025, and the maximum number of centers within two miles of a single campus increased from 16 to 21.

Panels (b) and (c) of Figure 1 map the number of private tutoring centers across Texas school districts in 1997 and 2025. In the late 1990s, centers were concentrated almost exclusively in the four largest metropolitan areas: Dallas-Fort Worth, Houston, Austin, and San Antonio. Although these markets remained the core of the industry over the following decades, centers also entered districts that had previously been unserved, with the fraction of districts hosting at least one center rising from 8.5 percent in 1997 to 14.2 percent in 2025.

3 Data on Texas Students

3.1 Data Sources

My empirical analysis uses administrative data maintained by the University of Texas at Dallas Education Research Center (UTD-ERC), which links several statewide registries to track Texas public school students from kindergarten through the labor market. The analysis sample begins with student-level data from the Texas Education Agency (TEA) covering the population of Texas public school students.⁹ The TEA data report annual information on student demographics, such as free and reduced-price lunch eligibility, limited English proficiency, special education status, gifted and talented program participation, and at-risk designation. They also include information on each student's grade, enrolled school, state standardized test scores, and dropout or graduation status.

I link these students to two statewide registries maintained by the Texas Higher Education Coordinating Board (THECB). The first consists of college application and admission records from all Texas public four-year universities, which include application-level SAT and ACT scores and admission decisions. The second consists of college enrollment, degree completion, and financial aid records at all Texas public universities, private universities, community colleges, and health-related institutions.¹⁰

One key limitation of the THECB data is that they do not contain enrollment records at out-of-state universities. Thus, I merge in data from the National Student Clearinghouse (NSC), which contains information on initial college enrollments of all Texas public high school graduates across nearly all colleges in the United States, allowing me to estimate effects both on in-state and out-

⁹Private school enrollments account for roughly 5 percent of all Texas K–12 students ([National Center for Education Statistics, 2021](#)).

¹⁰I do not observe Texas for-profit college enrollments.

of-state college enrollments.¹¹

A further limitation of the THECB data is that SAT and ACT scores are observed only for students who apply to a Texas public four-year university. This selection could bias my estimates if tutoring centers change the share of students who submit at least one application and the effect on scores differs between applicants and non-applicants. To address this concern, I supplement the analysis with publicly available school-level SAT and ACT data from the Texas Education Agency (Texas Education Agency, 2011–2020). Although these files are not at the individual level, they cover all examinees regardless of application behavior and contain several variables unavailable in THECB, including average scores by subject and the share of students taking either test.

Finally, to study employment and earnings outcomes, I merge in quarterly earnings data from the Texas Workforce Commission (TWC) that cover all Texas employees subject to the state unemployment insurance system.¹² In Section 5.3, I show that private tutoring center exposure does not have a significant impact on the probability of appearing in the earnings data.

3.2 Analysis Sample and Outcomes

The main analysis sample consists of Texas public high school students enrolled in ninth grade between 1998 and 2016.¹³ To construct this sample, I begin with the population of ninth-grade students in each cohort with valid student identifiers, covariates, and high school locations.¹⁴ I then drop students who transfer out of the Texas public school system during high school, whether to a private school or to an out-of-state institution. Because the outcomes span horizons from high school through the early career, each is observed for a different set of cohorts, and for each outcome I use the largest set of cohorts for which the outcome is fully observed. Table 2 reports the set of cohorts and the years over which each outcome is measured, and Appendix B includes details of the construction of each sample and outcome. Below, I discuss each outcome in turn.

The first set of outcomes captures academic performance and progression during high school. High school standardized test scores come from the Texas Assessment of Knowledge and Skills (TAKS), the statewide assessment administered from 2003 to 2011, which offers grade-level exams for students in ninth through eleventh grade.¹⁵ I focus on math and reading scores, standardized to have mean zero and unit variance within each subject, grade, and test year. The second high

¹¹The NSC records cover over 90 percent of nationwide college enrollment (Dynarski, Hemelt and Hyman, 2015).

¹²The state UI records exclude self-employed workers, independent contractors, military personnel, some federal employees, and informal-sector workers, but cover roughly 90 percent of the civilian labor force (Stevens, 2007).

¹³The Texas ERC data are available between 1997 and 2024. The 1997 cohort is excluded from the main analysis sample because all regressions on post-ninth-grade outcomes control for eighth-grade standardized test scores, which are unavailable for this cohort. I further restrict the sample to cohorts projected to apply to college before the COVID-19 pandemic, which disrupted both the college application process and the SAT/ACT testing infrastructure.

¹⁴For students observed in ninth grade in multiple years, I keep the first ninth-grade record.

¹⁵Students in the 2011 ninth-grade cohort continued to take TAKS during 10th and 11th grades. TAKS is the only statewide assessment regime in Texas that offers grade-level tests to all high school students. Its predecessor, the Texas Assessment of Academic Skills (TAAS), was administered in high school only as an exit exam, and its successor, the State of Texas Assessments of Academic Readiness (STAAR), consists of end-of-course exams that students take in varying grades depending on course enrollment. Roughly 30 percent of students, for example, take the STAAR Algebra I exam in eighth grade.

school outcome is graduation. I construct two measures of high school completion: an indicator for graduating within four years of ninth-grade enrollment, and an indicator for graduating within five years, which captures the small additional share of students who complete high school after a brief delay.

The college application outcomes come from the college application records that Texas public universities report to the THECB, which are available for ninth-grade cohorts from 1998 onward. I restrict to students who apply to a Texas public four-year university in the application cycle exactly four years after ninth-grade enrollment, which corresponds to on-time application for students who progress through high school without delay. From these records, I first measure college admission exam scores. For each student, I take the maximum SAT score and the maximum ACT score across all applications submitted in that cycle. For students with both, I convert the ACT score to the SAT scale using the official 2018 ACT/SAT concordance tables ([ACT, Inc. and The College Board, 2018](#)) and keep the higher of the two, so the resulting measure can be interpreted as an applicant's maximum college admission exam score.¹⁶ The same records allow me to construct several college application and admission outcomes: an indicator for submitting at least one application to any Texas public four-year university, an indicator for being admitted to at least one such institution, and an indicator for being admitted through the Texas Top Ten Percent admission policy, which guarantees admission to any Texas public university for students graduating in the top ten percent of their high school class.¹⁷

For college enrollment, I combine in-state and out-of-state enrollment records to capture enrollment at any U.S. postsecondary institution. The primary college enrollment outcome is an indicator for enrolling in any college in the fall semester four years after ninth-grade enrollment. I also construct separate indicators for two-year and four-year enrollment and for in-state and out-of-state enrollment. In addition, I construct two measures of the quality of enrolled college. The first is an indicator for enrolling at an elite institution, defined as those rated "most competitive" (Tier 1) by the 2009 Barron's selectivity index ([Barron's Educational Series, 2009](#)), together with the University of Texas at Austin and Texas A&M University, the two public flagship universities in Texas. The second is the average earnings of a college's enrollees, which assigns to each student the average earnings of all other students at the same institution. College degree outcomes are indicators for completing any college degree, an associate degree, and a bachelor's degree. A degree is counted if completed within six years of projected high school graduation, that is, within ten years of ninth-grade enrollment.¹⁸

Finally, I measure earnings using TWC quarterly wage records. For each individual, I sum within-quarter earnings across employers, deflate by the quarterly U.S. consumer price index to real 2015 dollars, and winsorize at the 99th percentile. The main earnings outcome averages the

¹⁶The THECB data file records the SAT and ACT scores used in each admission decision rather than the scores from a single test sitting. Taking the maximum across applications yields the highest score the student was willing to send to any Texas public four-year university.

¹⁷Being admitted through the Top Ten Percent policy is an indicator directly observed in the THECB data.

¹⁸For students who complete both an associate and a bachelor's degree within the window, I count only the bachelor's degree.

non-missing quarters within person over the period twelve to fifteen calendar years after ninth-grade enrollment, which corresponds to ages 26 to 29 for students who enter ninth grade at typical ages. For this outcome, I restrict the sample to cohorts with complete earnings records over the full window. I keep the full ninth-grade analysis sample regardless of educational attainment, so that the earnings estimates capture the combined effect of tutoring center exposure on labor market outcomes through any pathway, including direct labor force entry and college enrollment.¹⁹ I also estimate effects on earnings separately for each year after on-time high school graduation, using the largest set of cohorts for which earnings are observed at each year, to trace how the earnings effects evolve over the early career.

3.3 Summary Statistics

Table 3 presents summary statistics for the analysis sample. Column 1 covers all students in my sample. For the full set of cohorts, those enrolled in ninth grade between 1998 and 2016, my sample includes 5,474,264 unique students. The means of the demographic variables in Panel A are computed over this sample. The outcomes in Panels B through D are each observed for a subset of cohorts, so their means are computed over the cohorts reported in Table 2. Columns 2 and 3 split the sample by economic status, defined as ever qualifying for free or reduced-price lunch before ninth grade.²⁰ Economically disadvantaged students represent roughly 60 percent of Texas ninth graders and come from families for whom private, commercial tutoring is likely a substantial expense. Throughout the paper, I report results separately for the two groups and focus on non-economically disadvantaged students in the main analysis.

The Texas student sample is racially and ethnically diverse: 44 percent of students are Hispanic, 13 percent are Black, 38 percent are White, and 4 percent are Asian. Just under half of students are female, about 60 percent are ever designated as at risk of dropping out before ninth-grade enrollment, and 15 percent participate in gifted and talented programs. Students enter high school with average eighth-grade test scores slightly above the statewide mean, reflecting the exclusion of students who leave the Texas public school system before completing high school. Eighth-grade scores are missing for 15 percent of students in reading and 19 percent in math, and I retain these students with missing-score indicators.

In my analysis sample, roughly 85 percent of students graduate within four years of entering ninth grade, and an additional 2.5 percent graduate within five years. Just over a quarter of students apply to a Texas public four-year university, and among these applicants the average SAT score is 1058 (on a 400 to 1600 scale) and the average ACT score is 22 (on a 1 to 36 scale). In Table A2, I report summary statistics on SAT and ACT participation and performance from the publicly available school-level data, covering the 2011 to 2020 high school graduation cohorts. Average performance is similar to that of the applicant sample: the average SAT score is 1001, with

¹⁹The TWC data do not distinguish between non-employment in Texas and employment outside Texas, so a missing earnings record may reflect either.

²⁰Free lunch eligibility requires family income below 130 percent of the federal poverty line, and reduced-price lunch eligibility requires family income below 185 percent.

averages of 506 in math and 495 in reading, and the average ACT composite is 21. Participation differs across the two tests, with 65 percent of graduates taking the SAT and 41 percent taking the ACT.

About 48 percent of students enroll in college immediately after high school, defined as enrollment in the fall semester four years after entering ninth grade. Four-year and two-year institutions each account for roughly half of this enrollment. Twenty-five percent of all students are admitted to at least one Texas public four-year university, including 7 percent admitted automatically under the Top Ten Percent rule. Most students who enroll do so within Texas: 22 percent of all students enroll in Texas four-year universities, compared with 4 percent out-of-state enrollment. About 4 percent enroll in an elite institution. Within ten years of ninth-grade enrollment, 23 percent of students complete a degree at a Texas institution: 18 percent earn a bachelor's degree and 5 percent earn an associate degree. By ages 26 to 29, twelve to fifteen years after entering high school, about three quarters of students appear in the Texas earnings data with positive earnings, and among these students average annual earnings are about \$35,000 in 2015 dollars.

Columns 2 and 3 show that non-economically disadvantaged students differ from their disadvantaged peers at every point of this trajectory, entering high school with stronger academic preparation and extending their advantage at each subsequent stage. Non-economically disadvantaged students are predominantly White (66 percent), while economically disadvantaged students are predominantly Hispanic (61 percent) and Black (17 percent). Non-disadvantaged students enter ninth grade with eighth-grade math and reading scores about 0.6 standard deviations higher, are more likely to participate in a gifted and talented program (20 versus 11 percent), and are far less likely to be designated as at risk of dropping out (31 versus 80 percent). The gaps then widen at each stage that follows. About 90 percent of non-disadvantaged students graduate from high school within four years, compared with 81 percent of disadvantaged students, and 66 percent enroll in college immediately after high school compared with 38 percent. Among applicants to Texas public four-year universities, the SAT and ACT gaps are about 150 points and 4 points, respectively. The differences are large at the college enrollment margins: non-disadvantaged students are nearly six times as likely to enroll at an elite institution (9.6 versus 1.7 percent), more than four times as likely to enroll out of state (9.1 versus 2.0 percent), and three times as likely to complete an in-state bachelor's degree (31 versus 10 percent). In the labor market, the two groups appear in the Texas earnings data at nearly identical rates. However, among students with positive earnings, non-disadvantaged students earn roughly 40 percent more per year.

4 Research Design

The empirical challenge is that the density of private tutoring centers a student is exposed to is not randomly assigned across space. Firms locate where demand is strongest, and demand for supplemental instruction is shaped by the same family resources, parental aspirations, and local school quality that independently determine the outcomes I study. My strategy identifies the

causal effect of increased tutoring center exposure by comparing the outcomes of students who attend the same high school in different years, as the number of nearby tutoring centers changes, relative to students at other schools in the same county and cohort. This section presents the main specification, discusses the threats to its validity, and outlines the tests that address them.

4.1 Main Specification

I measure exposure to private tutoring by the stock of tutoring centers operating within a two-mile radius of a student’s ninth-grade campus. This choice approximates the residential neighborhood that the school serves, because Texas students typically live within about two miles of the school they attend.²¹ Tutoring centers within this radius therefore proxy the tutoring options available to families in the neighborhood from which the school draws its students.

The treatment variable is the average number of tutoring centers within two miles over the years of exposure. For outcomes realized during high school, such as state standardized tests, the exposure window spans the years from the start of high school through the test grade. For a student i in cohort t and tested in grade g , the level of exposure is

$$T_{stg} = \frac{1}{g-8} \sum_{\tau=9}^g N_{st\tau} \quad (1)$$

where $N_{st\tau}$ denotes the number of tutoring centers within a two-mile radius of school s in the calendar year in which cohort t enters grade τ , and the reference school s is the school that cohort t attended in ninth grade.

For outcomes realized after twelfth grade, such as high school graduation, college enrollment, and early-career earnings, the exposure window spans the full four years of high school. The level of exposure is thus

$$T_{st} = \frac{1}{4} (N_{st9} + N_{st10} + N_{st11} + N_{st12}) \quad (2)$$

which applies Equation 1 with $g = 12$.²² Because these outcomes are all realized after this window has closed, they share the same exposure measure.

This construction treats cumulative exposure to private tutoring centers, rather than the presence of a center in any single year, as the relevant margin. Exposure remains anchored to the ninth-grade campus throughout, so that subsequent transfers within the Texas public school system do not alter the location against which centers are counted. This anchoring has two desirable features. First, it makes the geography of exposure predetermined with respect to the outcomes:

²¹In my survey, described in Section 8, roughly half of students live within two miles of the school they attend, and 80 percent live within five miles. Moreover, Texas law sets two miles as the distance beyond which a student qualifies for district-provided transportation (Texas Education Code §48.151), and observed distances cluster near this threshold. As additional evidence, in the Houston Independent School District, the average student lives 2.3 miles from the zoned high school (Houston Education Research Consortium, 2023).

²²The count for grade g is taken from the calendar year in which a student begins that grade. For a student who repeats a high school grade, the measure captures average tutoring exposure over the first four years of high school.

the location at which centers are counted is settled at the start of high school, before the test-taking, course-selection, and enrollment decisions that the outcomes capture, so a student’s exposure cannot be a consequence of those decisions. Second, it removes student mobility as a source of identifying variation. If exposure were allowed to follow students across schools, a student who transferred toward or away from a tutoring-dense area would experience a change in exposure driven by the family’s choice of school rather than by the entry of new centers. Holding the reference school fixed restricts the identifying variation to the opening and closing of centers near a stable location.

Let T_{stg} denote the level of exposure for cohort t and grade g at reference school s , as defined in Equations 1 and 2. Let Y_{istg} be the outcome of student i at school s in cohort t and grade g . I estimate

$$Y_{istg} = \mu_s + \delta_{ct} + \beta T_{stg} + \gamma \mathbf{X}_{ist,g-1} + \varepsilon_{istg} \quad (3)$$

where μ_s is a high school fixed effect and δ_{ct} is a county-by-cohort fixed effect for county c and cohort t . The latter absorbs any contemporaneous county-level shocks, so that county-wide changes in economic conditions, housing development, or family composition that affect both tutoring supply and student outcomes do not contaminate the estimate. The vector $\mathbf{X}_{ist,g-1}$ contains base-line demographics of student i and lagged, time-varying characteristics of school s . The student-level controls include gender, race, standardized eighth-grade math and reading scores, and indicators for ever, through eighth grade, being at risk of dropping out, eligible for free or reduced-price lunch, bilingual, gifted, an immigrant, limited English proficient, or in special education. These characteristics are all measured before the student enters high school and are therefore predetermined with respect to exposure. The lagged school-level controls include total K–12 enrollment, the racial composition of enrolled students, and the shares of students who are at risk of dropping out, eligible for free or reduced-price lunch, gifted, limited English proficient, or in special education. The parameter β represents the causal effect of increased exposure to private tutoring centers on the outcome.

In the main specification, the treatment variable is the number of nearby tutoring centers, averaged over a student’s expected years of exposure during high school. I supplement this with two alternative measures. The first, as a heterogeneity analysis, distinguishes chain from independent centers, generating separate counts of national-franchise and locally owned centers based on the name of each business, since the two likely differ in instructional model, price, and the type of family they serve. The second, as a robustness check, replaces the average level of exposure during high school with exposure in the ninth grade alone, which predates the high school choices that might themselves respond to tutoring center exposure.

4.2 Validity of the Research Design

The identifying assumption is that, conditional on predetermined student characteristics, time-varying school characteristics, high school fixed effects, and county-by-cohort fixed effects, the

density of tutoring centers near a student's ninth-grade campus is mean independent of the counterfactual path of that high school's student outcomes. In other words, after absorbing time-invariant school differences and county-specific cohort shocks and accounting for student demographics and observable school characteristics, the timing and intensity of tutoring center entry near a school are unrelated to the outcomes its students would have realized absent that entry. This is most plausible if remaining variation in tutoring density reflects supply-side determinants of where and when firms operate, such as commercial real estate availability, lease turnover, and firm-level expansion decisions, none of which has a direct impact on student achievement.

The information tutoring chains use to choose locations supports this reading. Kumon, the largest chain in the sample, supplies prospective franchisees with the number of children, educational attainment, and income levels of a candidate area, and these characteristics determine whether a location is approved.²³ Consistent with these criteria, Kim, Goodman and West (2025) find that income, parental education, and Asian American population share dominate the predictors of tutoring center prevalence. Each of these determinants is either controlled for directly, through student race, economic disadvantage, and eighth-grade scores at the student and school level, or absorbed by high school fixed effects and county-by-cohort fixed effects, which hold fixed a location's proximity to schools and the county-wide evolution of its population. The demand-side factors that firms observe when siting centers are therefore largely held fixed, leaving the timing of entry to be driven by supply-side considerations.

Threats to identification.— The first threat is selection on local trends: tutoring firms may site centers in response to a school's own achievement trajectory, entering neighborhoods where outcomes are already trending upward or downward for reasons unrelated to the centers themselves. A firm that opens a branch where test scores have been declining would have its estimated effect biased downward. The second is confounding by unobserved local conditions: the estimate may reflect neighborhood characteristics that attract commercial entry rather than tutoring centers specifically, whether that influence operates at the scale of the immediate neighborhood or at a wider scale. A retail corridor that draws cafes, gyms, and tutoring centers alike may also be one whose residents are becoming more educated and engaged, so that nearby tutoring density picks up the rising advantage of the area rather than the effect of exposure to the centers themselves.

I address the possibility that the identifying assumption fails in three ways. First, I estimate a distributed lag model, testing whether outcomes move in advance of changes in exposure. If firms enter where potential outcomes are already trending, the entry of tutoring centers after a student is projected to graduate would itself appear to affect the outcomes of interest. Second, I estimate how the effect of center density varies with distance from the school, testing whether the effect is concentrated at the short distances at which tutoring take-up is more likely. A comparable effect at distances too far to be a realistic option for local families would indicate confounding by broader unobserved community characteristics rather than tutoring center exposure itself. Third,

²³See "Demand for Enrichment Fuels Kumon's Expansion of Centers," Kumon North America press release, <https://www.kumonfranchise.com/us-en/assets/pressrelease/release2023-0101.html>.

I conduct a placebo test with an alternative industry, replacing tutoring centers with the density of an industry whose location is governed by the same local determinants as tutoring but which has no plausible effect on academic outcomes. Together, these exercises bound the identification concern from the time, space, and industry dimensions. Section 6 presents each exercise and its results.

5 Main Results

This section presents the estimated effects of exposure to private tutoring centers on students' high school, college, and labor market outcomes. For each outcome I report estimates for the full sample of students and for those who are not economically disadvantaged, defined as students never eligible for free or reduced-price lunch before ninth grade. Because private tutoring is costly and its use is concentrated among highly educated, high-income families, I expect tutoring centers to have limited effects on economically disadvantaged students. I report estimates for this group in the next section.

While in the main text I report results from the regression specification in Equation 3, in Appendix Table A3 I present the main set of outcomes under alternative specifications. Adding student covariates, including race and eighth-grade test scores, reduces the estimates for most outcomes. Within a high school, cohorts exposed to more tutoring centers may differ in observable composition from cohorts exposed to fewer, and the main specification addresses the bias from selection on observable student characteristics. Beyond that, most estimates are stable across specifications when I add time-varying school covariates, county-by-cohort fixed effects, or both. Conditional on the student covariates, the results appear insensitive to the school-level composition changes and county-level shocks that these additional controls absorb, and the stability of the coefficients as the set of controls expands indicates that the main specification is unlikely to suffer from omitted variable bias.

5.1 Effects on High School Outcomes

I begin with outcomes observed during high school. Panel A of Table 4 shows effects on state standardized test scores in math and reading. Increased tutoring center exposure has no effect on either subject for the full sample or for non-economically disadvantaged students. For non-economically disadvantaged students, the 95 percent confidence intervals rule out effects larger than 0.007 standard deviations in math and 0.011 in reading from one additional tutoring center. Panel B shows that tutoring center exposure also has no effect on the probability of graduating from high school within four or five years of ninth-grade enrollment.

One explanation for these null results is the test preparation orientation of the private tutoring sector for high school students. For them, tutoring centers likely focus on preparation for college admission exams rather than for state standardized tests. If so, the effect of tutoring centers is more likely to appear on outcomes tied to college admission, which I examine next.

5.2 Effects on College Outcomes

Table 5 reports the effects of tutoring center exposure on college outcomes. Panel A considers performance on college admission exams. For non-economically disadvantaged students, one additional tutoring center within two miles of a student’s campus during high school raises SAT scores by 1.66 points and ACT scores by 0.07 points, each significant at the 1 percent level. The full-sample ACT effect is comparable at 0.07 points, while the SAT effect is positive but not statistically significant.

Because the THECB data record SAT and ACT scores only for students who apply to a Texas public four-year university, the estimates in Panel A could reflect changes in who applies rather than changes in performance. To address this concern, Table A4 reports estimates from the publicly available school-level data, which cover all high school graduates regardless of whether they applied to a Texas public four-year university. The regression specification matches the main analysis, controlling for high school and county-by-cohort fixed effects and time-varying school covariates.²⁴ The estimates are generally larger than those in Table 5 but follow the same pattern. One additional tutoring center raises the school-average SAT score by 3.16 points among non-economically disadvantaged graduates, and raises the ACT composite by 0.12 points. As in the individual-level data, the effects appear only among non-economically disadvantaged graduates and are indistinguishable from zero for economically disadvantaged graduates.²⁵ When separated by subject, the SAT effect on math is roughly twice the effect on reading, at 2.11 versus 1.05 points among non-economically disadvantaged graduates. Moreover, the school-level data allow me to check whether these gains are accompanied by changes in test taking. I find no clear effect on participation among non-economically disadvantaged graduates: the estimates are small and statistically insignificant.

The higher SAT and ACT scores are accompanied by enrollment at higher quality and more selective institutions. Panel B of Table 5 reports effects on the quality of college attended, conditional on enrolling. For non-economically disadvantaged students, one additional tutoring center raises the average annual earnings of enrollees at the college a student attends by \$61.²⁶ Following prior work (e.g., Chetty et al., 2020), I further classify institutions using the 2009 Barron’s selectivity index (Barron’s Educational Series, 2009). I then define elite colleges as those rated “most competitive” (Tier 1) in that index, together with the University of Texas at Austin and Texas A&M University, the two public flagship universities in Texas. Based on this definition, I find that increased tutoring center exposure raises the probability of attending an elite college by 0.20 percentage points. The full-sample effects are smaller but follow the same pattern. Conditional on enrolling in any college, I find no effects on four-year enrollment, so these gains reflect movement

²⁴Individual demographic controls are not included because the public data are at the school level. Time-varying school covariates include cohort size and the shares of White, Black, Hispanic, Asian, and economically disadvantaged graduates.

²⁵Dividing by race, the SAT effects are concentrated among White graduates, consistent with the racial heterogeneity I later document in Section 7.1.

²⁶College quality is measured as the leave-one-out mean of early-career earnings among enrollees of each institution, so a student’s own earnings do not enter the measure for their college.

toward better four-year institutions rather than a shift from two-year to four-year colleges.

The gain in college quality coincides with a shift in where students enroll. Panel C shows that, for non-economically disadvantaged students, an additional tutoring center reduces unconditional in-state four-year enrollment by 0.26 percentage points while raising out-of-state four-year enrollment by 0.34 percentage points, leading to a net increase in overall four-year enrollment of 0.08 percentage points that is not statistically distinguishable from zero. Following the decline of in-state four-year enrollment, in-state bachelor's degree completion falls by 0.23 percentage points. The out-of-state enrollment gain is likely accompanied by a corresponding gain in out-of-state degree completion, though those records are unavailable during my sample period. Panel D shows that the two-year margin contributes little to this reallocation: the enrollment estimates are small and mostly statistically insignificant, and in-state associate degree completion declines modestly, mirroring the bachelor's degree result.

Table A5 reports additional college results. Panel A shows that, for non-economically disadvantaged students, increased tutoring center exposure lowers the probability of applying to a Texas public four-year university by 0.46 percentage points and of being admitted to one by 0.38 percentage points, indicating that the in-state enrollment decline operates through applications. Admission through the Texas Top Ten Percent policy falls by 0.20 percentage points. Because this outcome is measured unconditionally, the decline is consistent with fewer applications playing an important role, rather than necessarily reflecting weaker standing among applicants. Aggregating across two-year and four-year institutions, enrollment at any in-state college falls by 0.40 percentage points while enrollment at any out-of-state college rises by 0.33 percentage points, consistent with the four-year reallocation in Table 5. Panel B shows no effect on the receipt of merit-based or need-based financial aid, suggesting that tutoring centers do not help students obtain aid of either type. Panel C conditions on earning an in-state degree and shows that tutoring center exposure raises the probability of graduating from a flagship university by 0.38 percentage points, and increases the average earnings of graduates from the same institution by \$44 and from the same institution and major by \$88, with no effect on STEM degree receipt. Among students who remain in Texas, tutoring centers therefore shift degrees toward higher-paying institutions and programs rather than toward particular fields.

5.3 Effects on Early-Career Earnings

Does the improvement in the quality of enrolled colleges translate into earnings gains for treated students? I begin by tracing how the effect of tutoring center exposure on earnings evolves over time. I estimate the effect separately for each year following ninth grade and plot the resulting trajectories for non-economically disadvantaged students in Figure 2. The top left panel shows the effect on the probability of appearing in the Texas earnings data. Increased tutoring center exposure lowers this probability by 0.2 to 0.4 percentage points in years five through seven after ninth grade, when treated students are expected to be enrolled in college, and has little effect thereafter. The dip matches the shift toward out-of-state enrollment in both timing and size: stu-

dents induced to enroll out of state are absent from the Texas earnings records while in college, and many return to work in Texas afterward.²⁷ As is common in state earnings data, individuals absent in a given year are either not working or working outside the state. Because the effect on appearing in the earnings data disappears after the years of projected college enrollment, the shift toward out-of-state colleges likely produces little differential attrition from the earnings sample in later years. Any remaining bias could take either sign, because its direction depends on the earnings of students who leave Texas, which the state records do not capture.

The remaining panels examine earnings levels. In the top right panel, I include all missing quarters and assign them a value of zero. In the bottom panels, I restrict the sample to quarters with positive earnings, multiply the quarterly average in each year by four, and estimate effects on the level and the log of earnings. Across both level measures, the effects are indistinguishable from zero from the fifth through the seventh year after ninth-grade enrollment, before any four-year college enrollees are expected to graduate. Positive effects emerge thereafter and grow steadily through the fifteenth year. The effect on log earnings is negative in the first few years of expected college enrollment and turns positive in most later years, though not always statistically significant.

Table 6 summarizes these profiles by averaging earnings over the final four years of the window, years 12 to 15 after ninth-grade enrollment. This corresponds to ages 26 to 29 for a representative student. First, increased tutoring center exposure has no effect on the probability of appearing in the earnings data. Then, for non-economically disadvantaged students, an additional tutoring center during high school raises average annual earnings by \$140 when all missing quarters are included and assigned a value of zero, and by \$241 when the sample is restricted to students with positive earnings. The effect on log earnings is a statistically significant increase of 0.46 percent. The full-sample estimates point in the same direction but are smaller: average earnings conditional on positive earnings rise by \$123, significant at the ten percent level, while the unconditional level effect and the log effect are not statistically distinguishable from zero.

In Figure A1 and Table A6, I report results using “sandwich” earnings, a measure common in work on quarterly state earnings records (Sorkin, 2018; Card, Rothstein and Yi, 2025; Mountjoy, 2026). The measure keeps only quarters of positive earnings that are “sandwiched” between other quarters with positive earnings, thus ignoring the high-variance quarters that mark transitions into and out of employment. The results are similar to those in Figure 2 and Table 6. The one exception is the log earnings effect, which attenuates to 0.27 percent and is no longer significant, though it remains statistically indistinguishable from the main estimate.

²⁷ Among cohorts for whom I observe both college enrollment and earnings, 60.5 percent of students who initially enrolled at an out-of-state college appear in the Texas earnings data in their late twenties, compared with 87.1 percent of in-state enrollees and 59.2 percent of students who did not enroll in college immediately after on-time high school graduation.

5.4 Effects Across the Test Score and Earnings Distributions

The mean effects reported above may mask movements within the outcome distributions. I therefore estimate effects of tutoring center exposure on unconditional quantiles of each continuous outcome using recentered influence function regressions (Firpo, Fortin and Lemieux, 2009). For each decile, I regress the recentered influence function of the outcome on tutoring exposure and the same set of controls and fixed effects as in the main specification, so each estimate describes how exposure shifts a decile of the statewide outcome distribution among students with observed outcomes. Figure 3 reports estimates for non-economically disadvantaged students. Estimates for economically disadvantaged students, reported in Figure A2, show no gains through most of the distribution and are consistent with the null mean effects.²⁸

Panels (a) and (b) of Figure 3 show that the null mean effects on state test scores hold throughout the distribution. Estimates for mathematics are close to zero at every decile. Estimates for reading are positive at every decile and rise to 0.016 standard deviations at the ninetieth percentile, although none are statistically significant. Together, these patterns indicate that the mean nulls reflect an absence of effects rather than offsetting movements at different points of the distribution. Panels (c) and (d) show a different pattern for college admission exams. Effects on SAT scores rise from -2.19 points at the tenth percentile to 6.30 points at the ninetieth, and effects on ACT scores rise from near zero at the bottom of the distribution to 0.19 points at the ninetieth percentile. A formal test confirms that the effect is larger at the top of the distribution than at the bottom: increased tutoring center exposure widens the 90–10 interquantile range of SAT scores by 8.49 points (p -value < 0.01) and of ACT scores by 0.19 points (p -value < 0.01). Panel (c) shows that the estimated effects of exposure on SAT scores are negative at the lower deciles of the score distribution. These estimates need not reflect score losses. Because exposure lowers the probability of applying to a Texas public four-year university and shifts high-scoring students toward out-of-state institutions, the composition of students with observed SAT scores changes with exposure.

Panels (e) and (f) show the same shape for early-career earnings. Effects on earnings levels are indistinguishable from zero through the sixth decile and rise to $\$898$ at the ninetieth percentile, widening the 90–10 interquantile range by $\$905$ (p -value < 0.01). Effects on log earnings follow the same pattern, turning statistically significant at the seventieth percentile and reaching 1.2 percent at the ninetieth. The corresponding interquantile estimate is 1.2 percent (p -value $= 0.02$).

Taken together, the distributional estimates sharpen the interpretation of the mean effects. Tutoring centers leave the state accountability test distribution unchanged, but shift the top of the admission exam and early-career earnings distributions. The mean effects on college admission exams and earnings therefore reflect a shift in the upper part of each distribution rather than a uniform shift across students, even among those who are not economically disadvantaged. This

²⁸The SAT figure for economically disadvantaged students is the one exception to the flat pattern: estimates are negative at the bottom deciles and positive at the top deciles, offsetting to the null mean effect. The positive estimate at the top suggests that high-scoring disadvantaged test takers may benefit from tutoring centers, while the negative estimates at the bottom likely reflect the compositional forces discussed below.

pattern mirrors the market the industry serves: tutoring centers locate in highly educated, high-income areas, and paid tutoring is concentrated among high-income families.

5.5 Alternative Treatment Definition

The main specification uses the average tutoring center exposure across the high school years as the treatment measure. I consider an alternative measure: exposure level in the ninth grade alone. Ninth-grade exposure predates the high school choices, such as course-taking and program placement, that themselves might affect the longer-term outcomes I study.

Table A7 reports estimates under this alternative definition. The estimates reproduce the pattern of the main results: no effect on state standardized test scores or high school graduation, and for non-economically disadvantaged students, higher SAT and ACT scores, improved quality of enrolled colleges, a shift from in-state to out-of-state institutions, and higher early-career earnings. The ninth-grade estimates match the main results in sign for every outcome with a significant main estimate. They are generally smaller in magnitude, indicating that tutoring center exposure throughout the high school years matters and that center entry and exit after the ninth grade carry independent weight.

6 Assessing the Causal Interpretation

The previous section documented positive effects on several college and early-career outcomes among non-economically disadvantaged students. In this section, I present three exercises supporting the causal interpretation of those estimates. I first estimate a distributed lag model that recovers the cumulative effect of tutoring centers by years of exposure. This exercise serves two purposes: it reveals how the effect of tutoring centers accumulates with each additional year of exposure, and it allows for a check of “pretrends,” namely the estimated effects of tutoring entry in the years after a student is expected to have graduated from high school. Second, I examine how the effect varies with the distance between tutoring centers and a student’s school, testing whether the impact of an additional center diminishes as centers lie farther from the school. In the main text, I report estimates from these analyses for a representative subset of the outcomes examined in Section 5. Appendix Figures A3 and A4 report the remaining outcomes, which likewise support a causal interpretation of the main estimates. Third, I run a placebo test that estimates the effects of pet care services exposure, whose entry follows a similar pattern but has no plausible impact on student outcomes. Complementing these tests, I also estimate the effect of tutoring center exposure on economically disadvantaged students, who are unlikely to participate in private tutoring.

6.1 Effects by Years of Tutoring Exposure

I examine how a change in the availability of nearby private tutoring centers in each calendar year, from the year a cohort enters seventh grade through the fifth year after its projected graduation, affects outcomes. Specifically, I estimate a distributed lag model of the following form:

$$Y_{ist} = \mu_s + \delta_{ct} + \sum_{g=7}^{18} \beta_g T_{stg} + \gamma X_{ist} + \varepsilon_{ist} \quad (4)$$

where T_{stg} is nearby tutoring density in the calendar year in which the cohort enters projected grade g , and β_g is the incremental effect of tutoring exposure in that year. I report the results in years of exposure, defined as $j = 13 - g$, the number of calendar years between the year a cohort enters grade g and the calendar year of its projected graduation. Entry into ninth grade corresponds to $j = 4$, entry into twelfth grade to $j = 1$, and the graduation year to $j = 0$. Nonnegative j corresponds to calendar years in which the cohort is enrolled in school for at least part of the year, and negative j corresponds to calendar years after the graduation year.

The cumulative effect of j years of exposure can be recovered from the distributed lag coefficients β_g as

$$\theta_j = \begin{cases} -\sum_{k=j+1}^{-1} \beta_{13-k} & \text{if } -6 \leq j \leq -2 \\ 0 & \text{if } j = -1 \\ \sum_{k=0}^j \beta_{13-k} & \text{if } 0 \leq j \leq 6 \end{cases} \quad (5)$$

which expresses the effect θ_j relative to one year after projected high school graduation. Thus, I normalize the coefficient θ_{-1} to be zero. Standard errors for θ_j are obtained by the delta method from the full variance-covariance matrix of the estimated $\hat{\beta}$.²⁹

For $j \geq 0$, θ_j is the cumulative effect of one additional nearby tutoring center that opens j calendar years before the graduation year and remains open through graduation. For example, θ_1 is the effect of an additional center that opens in the calendar year a cohort enters twelfth grade, θ_4 is the effect of an additional center present from ninth-grade entry through graduation, and θ_6 is the effect of an additional center present from seventh-grade entry through graduation. For $j \leq -1$, the post-graduation years are summed separately, starting one year after graduation.

Figure 4 plots the estimates and the 95 percent confidence intervals for non-economically disadvantaged students. The horizontal axis measures years of exposure prior to on-time high school graduation, defined as the gap in calendar years between the observed year and the fourth year after ninth-grade enrollment. The two dashed lines represent projected high school graduation,

²⁹Schmidheiny and Siegloch (2023) show that estimating the distributed lag coefficients β_g and constructing the cumulative effects θ_j as their partial sums generalizes the standard event-study framework to settings with multiple treatment events of varying size. Truncating the distributed lag to five leads and six lags corresponds to binning the endpoints of the equivalent event-study at six years before and six years after the event.

where exposure is zero, and ninth-grade enrollment, where exposure equals the full four-year high school window. Negative values correspond to years after on-time graduation.

The vertical axis reports the cumulative effect of one additional tutoring center within two miles of the student's school. For example, the coefficient at three years of exposure represents the effect of one additional center that opens in the calendar year a student begins tenth grade and remains open through the completion of twelfth grade. For negative values, for instance, the coefficient at negative two years of exposure compares a center that opens two calendar years after the graduation year with one that opens in the reference year, one calendar year after the graduation year. Because take-up is unlikely once a student has graduated, the estimates at negative years of exposure should be zero and serve as a check for pretrends.

In almost all panels, the cumulative effect is flat and statistically indistinguishable from zero to the left of the graduation line, confirming that centers entering or exiting after a student graduates do not predict her outcomes. To the right of this line, the effects emerge and accumulate over the high school window, reproducing the pattern in the main results. SAT and ACT scores rise, elite college enrollment increases, in-state four-year enrollment falls while out-of-state four-year enrollment rises, and early-career earnings rise.

The way the effects emerge is itself informative. In most panels, the cumulative effect remains near zero through two years of exposure and emerges at three years. One and two years of exposure correspond to centers that enter during twelfth and eleventh grade, so the pattern indicates that centers arriving late in high school do little to change a student's outcomes. Several factors could produce this pattern. A center that enters late offers only a short window of potential use; a new center takes time to establish its operations and build a local presence; and families need time to learn that a center exists before they enroll. Tutoring centers appear to affect outcomes only once they have been present for some time.

A complementary pattern appears at the other end of the window. To the right of the ninth-grade enrollment line, the cumulative effect flattens and shows little further increase with additional years of exposure. Points in this region correspond to centers that enter near a student's future ninth-grade campus while she is still in middle school. The absence of any further effect suggests that, conditional on tutoring availability at the start of high school, the earlier presence of centers near the school does not affect student outcomes. The median distance between a student's high school and middle school is 1.8 miles, so for most students, centers near the future ninth-grade campus were already nearby while they attended middle school.³⁰ The flat pre-enrollment trend is therefore consistent with two mechanisms: families begin considering the centers near the high school only once the student enrolls there, or middle school students attend these centers for instruction that does not affect the college outcomes measured here. Both imply that tutoring entry before high school generates no additional benefit relative to entry after the student arrives. The accumulation of effects during the high school window, together with their absence on either side, is consistent with the dose-response pattern that a causal effect of tutoring center exposure

³⁰The mean distance is 11.1 miles, reflecting a small share of students whose schools are far apart.

would produce.

6.2 Effects by Distance to School

I next exploit the spatial structure of the treatment. If proximity to tutoring centers has a causal impact on student outcomes, the effect should attenuate with distance: a center closer to a school is more likely to be used by its students, so its effect on outcomes should be larger, and the relationship should weaken as distance grows. To test this, I divide the area around a school into six mutually exclusive distance bands and count the centers in each band separately: within one mile, one to two miles, two to three miles, three to five miles, five to ten miles, and ten to twenty miles. I then estimate

$$Y_{ist} = \mu_s + \delta_{ct} + \sum_{b=1}^6 \beta_b T_{st}^b + \gamma \mathbf{X}_{ist} + \varepsilon_{ist} \quad (6)$$

where T_{st}^b is the number of centers in distance band b , and β_b is the effect of one additional tutoring center in band b .

Figure 5 plots these estimates for non-economically disadvantaged students. The horizontal axis is the distance ring and the vertical axis is the effect of one additional tutoring center located in that ring, holding fixed the number of centers in all other rings. If the effect reflects students attending nearby centers, it should be largest for the closest centers and decline with distance, reaching zero for centers too far away to be within a student's reach.

In every panel, the effect is concentrated among the nearest tutoring centers and attenuates to zero with distance. For SAT and ACT scores, tutoring centers within three miles of the school have a positive effect that drops to zero for centers farther away. The estimates are similar in magnitude for centers within one mile, between one and two miles, and between two and three miles, which suggests that take-up for college preparation is comparable across centers anywhere within three miles. For centers this close to the school, distance-related frictions such as travel cost do not appear to bind.

The panels in the second and third rows report effects on college enrollment and earnings. The rise in elite and out-of-state enrollment and the decline in in-state enrollment appear only for centers within two miles of the school.³¹ For centers in the three-to-five-mile band and beyond, the estimates are small and generally statistically insignificant. The effects on early-career earnings are positive and largest for centers within three miles of the school and fall to zero for more distant centers.

The attenuation with distance supports the two-mile radius used in the main specification, since the effects are concentrated within that band and have largely dissipated beyond three miles. A center too far from the school to be within a student's reach has no detectable effect on any out-

³¹The in-state four-year enrollment estimate in the two-to-three-mile band is positive, in contrast to the negative estimates in the surrounding bands. The comparison across distance bands rests on the overall pattern rather than on any single estimate. Within two miles of the ninth-grade campus, additional tutoring centers lower in-state four-year enrollment and raise out-of-state four-year enrollment, and no distance band beyond two miles shows this combination.

come. Since county-by-cohort fixed effects absorb conditions common to a county, this attenuation is difficult to reconcile with the estimates reflecting conditions that vary within a county over areas several miles wide rather than exposure to tutoring centers itself.

6.3 Placebo Test: Alternative Industry

To assess whether the baseline estimates reflect unobserved neighborhood trajectories rather than tutoring centers themselves, I use an identically constructed measure of exposure to pet care establishments (NAICS code 812910). Pet care services are a useful placebo for this purpose because they sort across neighborhoods on the same student characteristics as tutoring centers, yet have no plausible channel to student achievement or postsecondary outcomes. I first substitute pet care exposure for tutoring exposure in the main specification, which tests whether an industry with the same locational pattern reproduces the main results. I then add pet care exposure alongside tutoring exposure, which allows me to observe whether pet care exposure predicts outcomes conditional on tutoring exposure and whether the tutoring estimates survive the additional control.

Table A8 establishes that the two industries sort on the same student characteristics. Each cell reports the association between a student characteristic and the number of establishments of each industry within two miles, and the estimates for the two industries align in sign and statistical significance throughout. Both are more prevalent near schools with more Asian, Black, and gifted students and with higher eighth-grade reading and math scores, and less prevalent near schools with more Hispanic, economically disadvantaged, and at-risk students. Importantly, the estimates suggest that a given proportional increase in the number of establishments near a school is associated with the same change in student composition in both industries: the tutoring coefficients are about three times larger across characteristics, while pet care establishments are roughly three times as common near schools. The two industries are thus drawn to the same student populations, despite one offering an academic service and the other offering none. These estimates are also consistent with previous literature establishing that pet care expenditure rises with household income (Schwarz, Troyer and Walker, 2007; Henderson, 2013). Figure A5 reinforces the parallel pattern: pet care establishments grew steadily over the past three decades and concentrate in large metropolitan areas, mirroring the expansion and geography of the tutoring sector documented in Figure 1.

Table A9 reports the regression estimates. Columns 1 and 2 re-estimate the main specification with pet care exposure in place of tutoring exposure, in the full sample and among non-economically disadvantaged students. Columns 3 through 6 add pet care exposure to the main specification, so within each sample the coefficients on tutoring and pet care exposure come from a single regression and each is estimated conditional on the other.

The placebo in columns 1 and 2 reproduces little of the main results. Across the college and earnings outcomes, pet care exposure has no effect on SAT scores, the quality of enrolled colleges, in-state and out-of-state four-year college enrollment, degree completion, or early-career earnings, in either sample. The point estimates are close to zero and, for most outcomes, much smaller than

the tutoring estimates in Section 5. These nulls do not reflect imprecision. Because pet care establishments are far more common near schools, the placebo coefficients are estimated with smaller standard errors than the tutoring coefficients throughout the table. Columns 3 through 6 confirm that the tutoring estimates are not sensitive to conditioning on pet care exposure. The tutoring coefficients retain the sign, magnitude, and significance of the main results. The pet care coefficients in the joint specification are nearly identical to those estimated alone, which indicates that the two measures of exposure carry distinct information despite their geographic correlation. Together, the null placebo estimates and the stability of the tutoring coefficients indicate that the effects operate through tutoring centers specifically rather than through the conditions that accompany their entry.

A few pet care coefficients depart from zero. Pet care exposure is associated with higher ACT scores, less than half the tutoring magnitude, and with small declines in state standardized test scores and in four-year high school graduation among non-economically disadvantaged students. Nevertheless, these estimates likely do not pose a threat to the causal interpretation of the tutoring estimates. A local trajectory that raises college readiness would appear in the SAT as well as the ACT, yet the pet care coefficient on the SAT is a precisely estimated zero in every column. The negative test score and graduation coefficients arise only for outcomes that tutoring does not affect, and they run in the opposite direction from a confounding story in which commercial entry proxies for improving local conditions. Overall, the pet care placebo does not reproduce the coherent outcome pattern generated by tutoring exposure.

This placebo addresses confounders that operate through the local conditions that attract consumer service entry in general. It does not rule out unobserved confounders specific to tutoring: if tutoring firms respond to local demand for academic services that pet care establishments ignore, and that demand independently improves college outcomes, the tutoring estimates would be biased while the pet care estimates would remain null. As discussed in Section 4.2, however, the site-selection criteria tutoring firms rely on are largely held fixed by the controls and fixed effects, so the scope for such a confounder is narrow.

6.4 Effects on Economically Disadvantaged Students

Finally, I estimate the effect of increased tutoring center exposure on economically disadvantaged students. These students attend the same schools and are exposed to the same set of nearby tutoring centers as their non-disadvantaged peers, but the price of private tutoring is a more binding constraint for their families. If the main results reflected local conditions that affect all students at a school, some effect would be expected among disadvantaged students as well. If they reflect tutoring participation, the effects should be concentrated among students whose families can more readily afford to enroll.

Table 7 reports the regression estimates, with the first column reproducing the results for non-economically disadvantaged students from Section 5. For economically disadvantaged students, the same specification yields no estimate that is statistically significant at the 5 percent level across

high school, college, and labor market outcomes. The contrast holds across the main outcomes, with estimates of -0.11 points for the SAT, $-\$18$ for college quality as measured by the average earnings of a college's students, -0.02 percentage points for elite college enrollment, -0.01 and 0.01 percentage points for in-state and out-of-state four-year enrollment, and $-\$85$ for average annual earnings.

7 Heterogeneous Effects

In this section I explore heterogeneity in the effects of tutoring centers on student outcomes. I estimate the main specification separately by student subgroup, by school-level economic composition, by initial market presence, and by type of tutoring business.

7.1 Effects by Student Subgroup

I divide the sample by gender, by race and ethnicity, and by academic standing, comparing gifted with non-gifted students and at-risk with not at-risk students.³² Figure 6 plots the estimates for the main outcomes. The full set of regression estimates is reported in Appendix Table A10.

The effects are concentrated among White students, and their magnitudes closely track the estimates for non-economically disadvantaged students, a similarity consistent with the substantial overlap between the two groups.³³ For Hispanic and Black students, the estimates are small and statistically insignificant for nearly every outcome. The exceptions are a modest increase in out-of-state four-year enrollment among Hispanic students (0.07 percentage points) and a decline in in-state bachelor's degree receipt among Black students (0.22 percentage points). Estimates for Asian students are imprecise and statistically insignificant at the 5 percent level throughout. The null may appear at odds with characterizations of the private tutoring industry as serving predominantly Asian families, but Asian students make up less than 5 percent of the Texas student population, which limits the precision of the subgroup estimates. The small share also implies that tutoring entry and exit in Texas depend less on the local Asian population than in states where Asian families constitute a larger part of the market.

The patterns differ by gender. The shift away from in-state colleges is concentrated among female students, whose in-state four-year enrollment and in-state bachelor's degree receipt fall by 0.35 and 0.37 percentage points, compared with a decline of 0.06 and an increase of 0.07 percentage points among male students. Male students instead show larger increases in elite college enrollment and early-career earnings. The earnings estimate for male students ($\$185$) is more than three times that for female students ($\$55$), and the female estimate is statistically indistinguishable from zero.

³²All classifications are measured before a student enters ninth grade and therefore predate the tutoring exposure that defines treatment.

³³Whereas 60.2 percent of students in my sample are economically disadvantaged, only 30.5 percent of White students are. Conversely, 65.6 percent of non-economically disadvantaged students are White.

Estimates are larger for students not classified as at-risk, for whom every effect that is significant in the main results reappears, yet the at-risk estimates are not uniformly zero: at-risk students also score higher on the ACT and enroll out of state at higher rates. The comparison between gifted and non-gifted students shows no clear pattern, and none of the differences between the two groups is statistically significant.

These patterns are consistent with tutoring benefiting both students at the top of the class competing for selective admission and students in danger of falling behind. While economic disadvantage eliminates the effects altogether, at-risk status attenuates them and gifted status leaves them largely unchanged. The benefits of tutoring centers therefore track family resources more closely than prior achievement.

7.2 Heterogeneity by School-Level Economic Composition

Effects on exam scores, enrollment destinations, and earnings concentrate among students who are not economically disadvantaged. I next ask whether these effects also depend on the economic composition of a student's school. I divide schools at the median share of economically disadvantaged students, measured over the earliest cohorts observed at each school and held fixed thereafter, and re-estimate the main specification within each group. School composition may matter because tutoring centers are denser near affluent schools, because competition for selective college seats is more intense among students at those schools, or because the effects spill over to students who do not use tutoring centers themselves.

Table A11 shows that most of the effects are concentrated at schools with a below-median share of economically disadvantaged students. Among students who are not economically disadvantaged at these schools, exposure raises SAT scores by 1.94 points, ACT scores by 0.05 points, out-of-state four-year enrollment by 0.32 percentage points, elite college enrollment by 0.22 percentage points, and early-career earnings by \$285. For students who are not economically disadvantaged at schools above the median, the reallocation from in-state toward out-of-state four-year colleges also appears, with out-of-state enrollment rising by 0.20 percentage points, but none of the other estimates is statistically significant at the 5 percent level.

The estimates for economically disadvantaged students follow a different pattern. At schools with a below-median share of economically disadvantaged students, exposure raises their SAT scores by 1.79 points, similar to the gain for their non-disadvantaged classmates, and their probability of graduating within four years by 0.36 percentage points. Neither gain carries over to college enrollment, degree completion, or earnings, where the estimates are small and statistically insignificant. At above-median schools, their SAT and graduation estimates are not statistically significant, while their earnings estimates are negative and significant, at \$174 in levels and 0.9 percent in logs.

The score gain for economically disadvantaged students at affluent schools has two potential explanations. First, tutoring centers may generate spillovers to classmates who do not use them. Second, economically disadvantaged students at these schools may use tutoring centers them-

selves, since tutoring centers locate disproportionately near affluent schools. Because students sort into schools, the comparison describes where effects arise rather than isolating a single channel. Tutoring centers therefore appear to raise exam scores for a broader set of students than they change where students enroll or what they later earn, and only students who are not economically disadvantaged show gains on longer-run outcomes.

7.3 Heterogeneity by Initial Market Presence

The effects of additional tutoring centers may differ between markets the industry had already entered and markets it had not. To examine this, I divide schools by whether any tutoring center operated within two miles at the beginning of the sample period and estimate the main specification separately for each group. Table A12 reports the results.

The college and earnings effects are concentrated among schools that already had a tutoring center. For non-economically disadvantaged students at these schools, additional centers raise SAT and ACT scores, elite college enrollment, and out-of-state four-year college enrollment, and increase early-career earnings, closely matching the main results. At schools with no initial tutoring center, the SAT, ACT, and in-state enrollment estimates are similar in magnitude to those at served schools, although only the ACT estimate is significant at the 5 percent level. The elite and out-of-state enrollment estimates are considerably smaller. The earnings estimates are also much smaller, at \$70 in levels and close to zero in logs, and neither is statistically significant. High school graduation is the one outcome with a significant negative estimate at these schools, falling by 0.24 percentage points. Taken together, the sharpest evidence that the effects operate through entry into already-served markets comes from elite college enrollment, out-of-state college enrollment, and earnings.

This comparison should be read as descriptive heterogeneity rather than a causal decomposition of entry margins, because initial presence is not random. Schools with a center nearby at baseline differ from those without along the dimensions that attract the industry in the first place, including income, parental education, and the local demand for college preparation, and any of these could shape how students respond to additional tutoring centers. The comparison across the two groups therefore bundles the effect of market maturity with the effects of the market characteristics that produced it. This exercise suggests that the effects of within-school changes in exposure on college destinations and earnings arise mainly at schools the industry had already entered by the late 1990s, even though gains in test scores appear in both groups. Combined with the finding that effects accrue to non-economically disadvantaged students within those schools, this locates the industry's long-run impact at the intersection of served markets and advantaged families within them. The marginal center appears to deepen advantages where exposure is already high more than it extends access where it is absent.

7.4 Effects by Type of Tutoring Business

The tutoring sector comprises national chains and independent centers, the latter varying more widely in services and quality. To examine whether the two types differ in effectiveness, I decompose the treatment into separate counts of chain and independent establishments. Table A13 reports the estimates. The two columns within each sample report coefficients on chain and independent exposure, jointly estimated in one regression, so each coefficient reflects exposure to one type holding the other fixed.

The point estimates favor chain tutoring centers. The chain coefficients are larger for most outcomes in which the main specification finds a significant effect, including the SAT, the ACT, the quality of enrolled colleges, elite college enrollment, and in-state degree attainment, and are more often statistically distinguishable from zero. The independent coefficients are larger only for out-of-state enrollment and for log earnings among non-economically disadvantaged students, and the early-career earnings estimates for the two types are similar in that sample, at \$264 and \$223. The differences between the chain and independent coefficients are statistically insignificant for most outcomes, however, so the advantage of chain centers is suggestive rather than conclusive.

The one divergence is high school graduation, for which the main specification finds no effect. The chain coefficient is negative and significant while the independent coefficient is positive and insignificant. Because chains account for roughly half of all establishments, the two coefficients average to approximately zero, consistent with the null in the main specification.

8 Survey Evidence and Treatment-on-the-Treated Effects

The research design exploits variation in exposure to local tutoring centers, so the estimates are intent-to-treat (ITT) effects of tutoring proximity. Interpreting the effects of private tutoring on the students who use it, the treatment-on-the-treated (TOT) effects, requires evidence on tutoring participation. Existing survey evidence offers little help. The most widely cited participation estimates come from NCES longitudinal cohort studies whose most recent cohorts date to the early 2000s (Bound, Hershbein and Long, 2009; Buchmann, Condron and Roscigno, 2010). More recent national surveys ask parents about tutoring but either pool paid commercial tutoring with school-provided, publicly funded, and informal arrangements, restrict attention to elementary grades, or tie the question to pandemic learning loss.³⁴ Moreover, none contains enough observations to support descriptive analysis of Texas by itself. The Consumer Expenditure Survey (CEX) also provides evidence from more recent years. However, the CEX records expenditure rather than participation, does not separate commercial tutoring from other paid instruction, and identifies geography only for the largest metropolitan areas.

Thus, I design and implement an original survey of parents of school-age children in Texas.

³⁴These include the National Household Education Survey Parent and Family Involvement supplements (2003, 2007), the ECLS-K and ECLS-K:2011 parent interviews, the TIMSS home questionnaires (2015, 2019), and the Census Household Pulse Survey (2021–2022).

The survey serves four main purposes. First, it provides contemporary evidence on private tutoring participation in one of the largest states in the country. Second, it collects detailed information on both student and household demographics and the nature of tutoring, including the type of tutoring provider, the content of tutoring, the time and money households spend on tutoring, and parents’ reasons for enrolling their children. Third, the survey records the ZIP code of the child’s school, so participation can be related directly to the local supply of tutoring centers. This relationship provides direct evidence on the identifying variation exploited in this paper: the availability of tutoring centers near a student’s school. Finally, the participation rates and their dependence on local tutoring center presence and student demographics provide the basis for translating the ITT estimates into TOT effects.

8.1 Survey Design

The survey targets parents and guardians of children enrolled in kindergarten through twelfth grade in Texas schools. I recruit respondents through Cint, an online survey panel provider whose sample marketplace, formerly known as Lucid, is widely used in academic research (Coppock and McClellan, 2019; Allcott et al., 2026; Andre, 2025). The survey was distributed in July 2026. A screener at the start of the questionnaire restricts participation to respondents who report at least one child currently enrolled in a Texas K–12 school. Recruitment quotas on race and ethnicity target the composition of the Texas student population. Respondents with more than one K–12 child are instructed to answer all questions about the child with the most recent birthday. This instruction is intended to approximate random selection of a child within the household and prevent parents from selectively reporting on a tutored child (Salmon and Nichols, 1983; O’Rourke and Blair, 1983; Gaziano, 2005). The final sample contains 1,150 completed responses. The average respondent completes the survey in 7 minutes and is compensated through the platform’s rewards system. The full questionnaire appears in Appendix C.

Because the survey platform recruits from an opt-in panel rather than a probability sample, the raw sample need not represent Texas families with school-age children, even with the recruitment quotas. I therefore construct weights by iterative proportional fitting, also known as raking (Deming and Stephan, 1940; Valliant, Dever and Kreuter, 2013). The procedure adjusts each respondent’s weight so that the weighted sample matches the Texas K–12 population on private versus public school enrollment, household income, race and ethnicity, grade level, and economic disadvantage.³⁵ All survey estimates in this section are weighted.

Table 8 reports summary statistics for the survey sample, unweighted and weighted. The unweighted sample under-represents Hispanic families and over-represents lower-income house-

³⁵Statistics on the Texas K–12 population are computed using the 2024 American Community Survey (ACS), the most recent year available. Household income enters in five ranges with cutoffs at \$25,000, \$50,000, \$100,000, and \$150,000. Race and ethnicity enter in five categories (Hispanic, non-Hispanic White, non-Hispanic Asian, non-Hispanic Black, and other). Grade level enters as elementary, middle, and high school. Economic disadvantage enters as the share of children who are economically disadvantaged, defined as those in households with income below 185 percent of the federal poverty line.

holds relative to the state. The children covered by the survey are 43 percent White, 35 percent Hispanic, and 15 percent Black, and 85 percent attend public schools. In the weighted sample, the corresponding shares are 33, 48, 12, and 90 percent. About 36 percent of parents hold a college degree in the unweighted sample and 46 percent in the weighted sample. Based on reported income and household size, 41 percent of households in the unweighted sample and 35 percent in the weighted sample fall below 185 percent of the federal poverty line, the income-eligibility limit for free or reduced-price lunch, and are classified as economically disadvantaged.³⁶ After weighting, households have 1.9 K–12 children on average, and 37 percent of children live in urban areas.

8.2 Survey Results

I restrict tutoring participation to private tutoring for which the household pays the full cost, thereby excluding children who receive free, subsidized, or school-provided tutoring.³⁷ In Table 9, I report participation rates and spending under this definition. In my sample, 25 percent of Texas K–12 children currently receive paid private tutoring, 17 percent receive paid one-on-one tutoring, and 9 percent receive paid tutoring at a tutoring center.³⁸ One-on-one tutoring refers to instruction from an individual tutor hired by the household, such as a schoolteacher, a college student, or an independent professional tutor, and can take place through informal arrangements that operate outside any business establishment. These categories overlap because a child can receive more than one type of tutoring.

Participation differs sharply by economic status, with 30 percent of non-disadvantaged children receiving paid tutoring, nearly twice the 16 percent rate among disadvantaged children. The disparity is relatively narrow for one-on-one tutoring, at 20 percent compared with 12 percent, and proportionally much wider for center-based tutoring, at 13 percent compared with 3 percent. Panel B reports the resources that participating households commit. Participating households spend \$2,761 per year on average, and the average tutored child receives 3.2 hours of tutoring per week at an average rate of \$48 per hour.³⁹ The spending gap persists conditional on participation: non-disadvantaged households spend \$3,188 per year, nearly three times the \$1,215 among disadvantaged households. Weekly hours are nearly identical across the two groups, at about 3.2 hours,

³⁶Because the survey records income in brackets, I assign each household the midpoint of its reported bracket when computing the ratio of income to the poverty line, and I compute household size as two plus the reported number of children in K–12. Both steps introduce measurement error, so the classification is a proxy for economic disadvantage rather than an eligibility determination.

³⁷The survey instructions define the scope directly: “By ‘private tutoring,’ we mean academic instruction provided outside of regular school hours by a private company, a tutoring center, or an individual tutor hired by your household. Please do not include free tutoring offered by the selected child’s school or district, and please do not include homework help provided by family members.” Some respondents nonetheless report private tutoring at no cost, suggesting the exclusions were not uniformly applied. I therefore rely on a separate question that asks whether the household pays the full cost of the tutoring.

³⁸These rates may exceed participation in the broader population of Texas families. Although the weights match the Texas K–12 population on observable characteristics, parents who participate in an online survey panel could be more engaged in their children’s education than other parents. Nevertheless, the analysis below relies mainly on differences in participation across households and across levels of local tutoring supply rather than on its level.

³⁹Because the survey records spending in brackets, annual spending and hourly rates assign each response its bracket midpoint.

so the spending gap does not reflect more hours of tutoring each week. Table A14 replicates this table for high school students, and the patterns are very similar.

In Figure 7, I plot center-based participation rates by the student's current grade, race and ethnicity, parental education, and household income. Participation shows no clear pattern across grades: children use tutoring centers throughout their school careers.⁴⁰ Across racial and ethnic groups, participation in center-based tutoring differs only modestly. About 10 percent of White children and 13 percent of Asian and other-race children use tutoring centers, compared with about 8 percent of Hispanic children and 9 percent of Black children. Participation instead rises sharply with parental education and household income. Over 15 percent of children whose parents hold a college degree use tutoring centers, compared with less than 5 percent of children whose parents do not, and participation climbs from under 1 percent among households earning below \$25,000 to 17 percent among households earning above \$150,000. These patterns mirror the participation gap by economic status in Table 9: the households purchasing center-based tutoring are concentrated in the upper part of the income and education distributions.

Next, I document six additional facts from the survey, reported separately for high school students (grades 9–12) and younger students (K–8). The corresponding figures are presented in Figures A8 and A9. Unless noted otherwise, all shares below are among children currently receiving paid private tutoring. First, one-on-one tutoring is the most common form of tutoring used in both groups: 75 percent of tutored high school students work with a one-on-one tutor, 36 percent attend a tutoring center, and 28 percent use an online platform, with many children combining more than one type.⁴¹ Second, when asked why they enrolled their children, parents of high school students most often select standardized test preparation (58 percent), competitive college admission (54 percent), and the child struggling academically (47 percent). Among K–8 students, struggling academically ranks first and college admission falls to 34 percent, indicating that tutoring demand shifts toward admission-related goals as students approach college. In both groups, about one in six parents report enrolling because a provider opened nearby. Third, among children not receiving paid private tutoring, the most common explanation in both groups is that the child does not need tutoring (57 percent for high school students), and the most common barrier is cost (22 percent). Fourth, the content of tutoring aligns with the tests and transitions students face: 50 percent of tutored high school students prepare for the SAT or ACT, 34 percent prepare for the state standardized test, and 31 percent receive college preparation or counseling services, while K–8 tutoring focuses on the state test (54 percent). Fifth, tutoring is concentrated in core academic subjects: 73 percent of tutored high school students receive tutoring in mathematics, followed by English or reading at 41 percent and science at 35 percent, with a similar concentration among K–8 students. Sixth, parents overwhelmingly perceive tutoring as effective: among high

⁴⁰Similar patterns are observed when I plot the corresponding participation rates for private tutoring of any form in Figure A6. Figure A7 presents the distribution of the grade at which tutoring began. The distribution is spread across the early grades rather than concentrated at a single entry point. First grade is the modal starting year, at over 15 percent of current users, but every grade from kindergarten through sixth accounts for over 8 percent.

⁴¹These are not mutually exclusive categories, so a respondent can select more than one type.

school students, 98 percent of parents agree that the child’s academic performance has improved since tutoring began, and none disagree. The corresponding shares for K–8 students are 83 and 8 percent.

8.3 First Stage: Local Supply of Tutoring Centers and Tutoring Participation

I now examine how tutoring center participation responds to the local supply of centers, the first stage of my research design. I proceed in three steps. First, I match the ZIP code of the selected child’s school to its ZIP Code Tabulation Area and obtain the area’s centroid from the Census Bureau Gazetteer Files ([U.S. Census Bureau, 2025](#)). I also match the ZIP code to county using the HUD-USPS ZIP crosswalk files ([United States Department of Housing and Urban Development, 2026](#)).⁴² Second, I count the tutoring centers in the 2025 Historical Business data within two miles of the centroid, the same radius that defines the treatment in the administrative analysis. Third, I estimate

$$\text{Tutor}_{izc} = \beta \text{Centers}_z + X_i' \gamma + \delta_c + \varepsilon_{izc} \quad (7)$$

where Tutor_{izc} is an indicator for whether child i , attending a school in ZIP code z in county c , currently receives paid tutoring at a tutoring center, Centers_z is the number of tutoring centers within two miles of the school ZIP code centroid, X_i is the vector of demographic controls from Table 8, and δ_c are county fixed effects. β is identified from variation in local center supply across school ZIP codes within the same county. Standard errors are clustered at the school ZIP code level.

Two caveats apply if these estimates are used to understand the first stage behind the ITT estimates and to recover TOT effects from my earlier analyses. First, the centroid approximates the school’s location, so the tutoring center count measures the true level of exposure with error.⁴³ Second, the survey asks about participation in the 2025–2026 school year, after the period covered by the main analysis, so the estimates describe the current relationship between exposure and take-up rather than the relationship during the study period. Nevertheless, these estimates provide a useful approximation of the first stage.

Table 10 reports the regression results. One additional center within two miles of the school is associated with a 2.2 percentage point increase in tutoring center participation, and the association declines to 1.8 percentage points once demographic controls are included.⁴⁴ Against a mean participation rate of 9 percent, the estimate with controls implies that an additional nearby center

⁴²I use the latest (December 2024) county–ZIP crosswalk file. When a ZIP code spans multiple counties, I assign it to the county containing the largest share of the ZIP code’s residential addresses. The school ZIP codes reported by eight respondents cannot be matched to a county in the crosswalk, so the first-stage regressions exclude these respondents.

⁴³The median radius of the school ZIP code areas in the survey data, computed from land area in the same Gazetteer Files, is 6.7 miles. Large ZIP code areas, where the centroid may lie far from the school, are concentrated in rural regions where center counts are near zero. In the urban and suburban areas where centers locate, ZIP code areas are smaller and the centroid lies closer to the school.

⁴⁴Table A15 reports the corresponding estimates using a five-mile radius. The estimates are roughly one fifth to one third of the two-mile estimates, consistent with centers farther from the school exerting less influence on participation, and they remain statistically significant for the full sample and for non-disadvantaged students.

raises participation by about 20 percent. Restricting the sample to high school students generates slightly higher estimates, at 2.8 percentage points both with and without demographic controls.

To examine whether tutoring centers draw children into tutoring or away from other providers, I replace tutoring center participation on the left-hand side of the regression with an indicator for paid one-on-one tutoring, the most common form of private tutoring in the survey. The second row of each panel reports these estimates. Once demographic controls are included, an additional center within two miles is associated with a 1.4 percentage point decline in one-on-one tutoring, significant at the 5 percent level, which offsets much of the 1.8 percentage point increase in center participation. Among high school students, the decline in one-on-one tutoring, at 2.4 percentage points, is similar in magnitude to the increase in tutoring center participation but statistically insignificant. Because the two categories are not mutually exclusive, the estimates do not net out exactly, but they suggest that tutoring centers partly substitute for one-on-one tutors rather than only drawing children into tutoring for the first time. This substitution matters for interpreting the main estimates. For children who would otherwise have used a one-on-one tutor, exposure to a center changes the type of tutoring they receive rather than whether they receive it, so the reduced-form effects capture the return to center-based tutoring relative to a counterfactual that includes other paid tutoring for some children. To the extent that one-on-one tutoring also improves outcomes, the estimates understate the effect of center-based tutoring relative to no tutoring.

The participation response to local tutoring supply appears only among non-economically disadvantaged children, both for all K–12 students and for high school students. For high school students, an additional tutoring center within two miles is associated with a 3.1 percentage point increase in tutoring center participation among non-disadvantaged children, significant at the 1 percent level, compared with 0.6 percentage points among disadvantaged children, which is not statistically distinguishable from zero. The substitution away from one-on-one tutoring is likewise concentrated among non-disadvantaged children. In the full sample, an additional center is associated with a 1.9 percentage point decline in one-on-one participation, nearly matching the 2.0 percentage point increase in their tutoring center participation, whereas the corresponding estimates for disadvantaged children are close to zero and imprecise. This pattern suggests that the heterogeneous effects in the main analysis reflect differential take-up among students from different economic backgrounds. Disadvantaged students show no detectable response to the local supply of tutoring centers, so greater exposure has little scope to improve their outcomes. The participation and spending evidence is consistent with this interpretation. Disadvantaged children participate in center-based tutoring at about one fourth the rate of non-disadvantaged children, and conditional on participating, disadvantaged households spend about 40 percent as much as non-disadvantaged households, at lower hourly rates.

8.4 Treatment-on-the-Treated Effects

I next recover the implied TOT effects of private tutoring using two approaches: the first based on the participation response estimated in the survey, and the second based on the capacity of a

typical tutoring center.

Survey-based first stage.— The estimates in Table 10 measure how tutoring participation responds to local supply at a point in time, and applying them to the ITT effects estimated earlier in the paper requires one adjustment. The treatment in the main analysis spans the four years of high school, so the relevant first stage is the total amount of center-based tutoring that one additional center induces over those four years, not the change in participation in a single year. In this section, I therefore measure tutoring in student-years.

I define a student-year of tutoring as one year in which a student attends a tutoring center, so that a student accumulates between zero and four student-years over high school. Because expected student-years equal the sum of annual participation probabilities, one additional tutoring center present throughout high school raises them by four times its effect on annual participation. The high school sample in the survey spans grades 9 through 12, so the high school estimate in Table 10 measures this annual effect averaged across grades. For non-disadvantaged students, the estimate is 0.031 with or without demographic controls, so the first stage is $4 \times 0.031 = 0.124$ student-years. That is, one additional tutoring center present throughout high school raises the expected time a student spends in center-based tutoring by 0.124 years, and every eight students exposed to an additional center accumulate about one additional year of center-based tutoring among them. This first stage does not depend on how long tutoring spells last or on how participation is distributed across students, since a center that induces one student to attend for four years and a center that induces four students to attend for one year each generate the same four student-years. Dividing each ITT estimate by this first stage yields the implied effect of a year of center-based tutoring, equivalent to multiplying each ITT estimate by a factor of roughly 8.

Capacity-based first stage.— The second approach infers the first stage from the number of students a tutoring center serves, which measures tutoring in student-years directly. Kumon, the largest tutoring chain, reports roughly 265,000 students enrolled across 1,492 U.S. centers in 2014, an average of 178 students per center (Kumon North America, 2014). Assuming that the number of students served is proportional to the number of employees at each establishment, the average center in Texas serves approximately 191 students each year.⁴⁵

Only some of these seats serve the students in my analysis. Assuming that a center's students are spread evenly across the 13 grades from kindergarten through grade 12, roughly 59 of the 191 students are in high school. I further assume that all of these seats are filled by non-disadvantaged students attending public high schools near the center. The seats are spread across the four high school grades, so a single cohort occupies about one quarter of them in each of its four years of high school. Over high school, each cohort therefore receives as many student-years as the center fills in a single year, or roughly 59. These student-years are shared among the non-disadvantaged students at public high schools within two miles of the center. In a representative year of my analysis sample, the average center has 2.3 such schools nearby, whose ninth-grade cohorts to-

⁴⁵In the Texas business data, Kumon employs 6.8 workers per center, while all other establishments employ 7.4 workers on average. Given that Kumon represents 18.0 percent of all establishment-years in Texas, scaling Kumon's enrollment by relative employment implies that the average center in Texas serves roughly 191 students.

gether contain an average of 276 non-disadvantaged students.⁴⁶ Dividing the student-years by this cohort size yields a first stage of $59/276 \approx 0.21$ student-years per student.

This first stage can be interpreted as an upper bound on the tutoring that an additional center induces. The calculation assigns all of a center's high school seats to non-disadvantaged students at public schools within two miles of the center, although some seats are filled by private school students, by economically disadvantaged students, and by students who travel from farther away. It also treats every seat at an additional center as new tutoring, although part of its enrollment may come from students who would otherwise have attended another center. The implied TOT effects are therefore a lower bound. Dividing the ITT estimates by this first stage is equivalent to multiplying them by a factor of roughly 5, compared with 8 under the survey-based approach.

TOT effects.— Taken together, the two approaches imply that a year of center-based tutoring raises the SAT scores of non-economically disadvantaged students by 7.8 to 13.4 points (0.04 to 0.07 standard deviations) and ACT scores by 0.33 to 0.56 points (0.07 to 0.11 standard deviations).⁴⁷ It raises the average early-career earnings of enrollees at a student's college by \$290 to \$490 and the probability of enrolling at an elite college by 0.9 to 1.6 percentage points. It also lowers in-state four-year enrollment by 1.2 to 2.1 percentage points and raises out-of-state four-year enrollment by 1.6 to 2.7 percentage points. For early-career earnings, I scale the estimate conditional on positive earnings, consistent with the main analysis. Annual earnings in the late twenties rise by roughly \$1,130 to \$1,940, or 2.2 to 3.7 percent using the log earnings specification.⁴⁸

These implied estimates measure the effect of a year of participation in a tutoring center, not the effect of any particular service the center provides. Students attend tutoring centers for different reasons and receive different combinations of instruction, so the SAT and ACT gains do not isolate the return to test preparation, and the shift toward selective and out-of-state colleges does not isolate the return to counseling or application guidance. Each estimate captures the combined effect of whatever bundle of services the induced participants received. The estimates also compare center-based tutoring with what induced participants would otherwise have received. Because part of the increase in tutoring center participation likely comes at the expense of one-on-one tutoring, as Table 10 shows, the counterfactual for some induced student-years is another form of paid tutoring rather than no tutoring, which makes the scaled estimates conservative if one-on-one tutoring is itself effective.

These magnitudes can be compared with the cost of tutoring. Participating non-disadvantaged households in the survey spend \$3,188 per year, or roughly \$2,300 in 2015 dollars, the units of the earnings estimates. A year of center-based tutoring thus raises annual earnings in the late twenties

⁴⁶I measure these quantities in 2010, a year that falls within the window of all outcomes in the analysis. I count the public high schools within two miles of each center and average the total non-disadvantaged ninth-grade enrollment of these schools across centers with at least one such school. Because the main estimates average over students, each center enters in proportion to the number of non-disadvantaged ninth graders within two miles of it, so the relevant cohort size is this average across centers rather than the product of the average number of schools and the average school cohort.

⁴⁷The standard deviations of SAT and ACT scores in the analysis sample are 192 and 5.0.

⁴⁸Scaling the estimate that includes students without positive earnings instead yields roughly \$650 to \$1,130 per year.

by a substantial share of its cost. If the gain persists, the earnings it generates would repay the cost of a year of tutoring within a couple of years of work.

8.5 Discussion of Magnitudes

Three comparisons place the size of the TOT estimates in the context of the existing literature. The first places the college admission exam effects against the tutoring literature, the second situates the college and earnings effects among estimates from the college choice and college returns literature, and the third connects my own estimates, asking whether the gains on admission exams line up with the gains in college destinations and earnings that follow.

Estimates from the tutoring literature.— A year of center-based tutoring raises the SAT scores of non-economically disadvantaged students by 0.04 to 0.07 standard deviations and their ACT scores by 0.07 to 0.11 standard deviations. These effects are smaller than the gains from the tutoring programs discussed in the introduction, which are publicly provided, school-based, or philanthropically funded and mostly serve struggling, low-income students. Meta-analyses of randomized evaluations report pooled effects of 0.3 to 0.4 standard deviations ([Nickow, Oreopoulos and Quan, 2024](#); [Dietrichson et al., 2017](#)), with 0.33 standard deviations for middle and high school students and 0.16 to 0.22 standard deviations for programs serving 400 or more students ([Kraft, Schueler and Falken, 2026](#)). The school-based programs above differ from tutoring centers in who provides the service and who receives it. The closest comparison is therefore [Goodman and Nitkin \(2026\)](#), who study the same market-provided treatment. Their district-level effects of chain tutoring center entry on test scores in grades three through eight imply an effect of roughly 0.25 to 0.5 standard deviations per year of tutoring, comparable to the school-based programs.

Three differences likely explain why my estimates are smaller. First, families purchase tutoring for several purposes. Among tutored high school students in my survey, half prepare for the SAT or ACT, while others prepare for state tests or receive college counseling, so the TOT effect on admission exams averages over participants whose tutoring targets other outcomes. Scaling by the share who prepare for the exams yields 0.08 to 0.14 standard deviations on the SAT and 0.13 to 0.22 on the ACT, closer to the estimates for tutoring programs implemented at scale. Second, the students who receive tutoring differ across settings. The school-based programs serve low-income students who are struggling academically, whereas the high school clients of tutoring centers are predominantly from high-income families, so effects for the two populations need not align. Third, the outcomes differ as well. The studies above evaluate programs using state standardized tests or assessments designed for the intervention, whereas my outcomes are the SAT and ACT. The closest evidence on college admission exams comes from observational studies of SAT coaching, which report gains of roughly 20 to 40 points on the 1600-point scale, or 0.1 to 0.2 standard deviations on my scale ([Powers and Rock, 1999](#); [Buchmann, Condrón and Roscigno, 2010](#)). Because students self-select into coaching on motivation and family resources, these estimates are likely biased upward. My estimates fall below this range but overlap its lower end once scaled by the share preparing for the exams, and are reasonable by comparison.

Estimates from the college choice and college returns literature.— The reallocation of roughly 1 to 3 percentage points from in-state toward out-of-state four-year colleges, and the 0.9 to 1.6 percentage point rise in elite enrollment among students who enroll in college, are also below the effects of interventions designed to change where students enroll. Intensive advising for low-income students in Massachusetts raised enrollment at the colleges the program encouraged by roughly 50 percentage points (Castleman and Goodman, 2018), and a scholarship offer to low-income high achievers in Michigan raised applications to the University of Michigan by 42 percentage points and enrollment by 15 (Dynarski et al., 2021). As above, the comparison is imperfect because those programs target low-income students and college choice directly, whereas tutoring centers serve high-income families for purposes that extend beyond college enrollment.

The earnings effect of roughly 2 to 4 percent per year of tutoring can be situated among estimates from the related literature. A one standard deviation improvement in teacher value added in a single grade raises earnings at age 28 by about 1.3 percent (Chetty, Friedman and Rockoff, 2014), enrolling at a state flagship raises the earnings of White men in their late twenties and early thirties by roughly 20 percent (Hoekstra, 2009), and students who gained automatic admission to UT Austin under the Top Ten Percent rule saw earnings gains of about 5 percent 9 to 11 years after high school (Black, Denning and Rothstein, 2023). My estimate lies above the return to a one standard deviation improvement in teacher value added and below the return to attending a flagship.

Connecting the admission exam, college, and earnings effects.— Two benchmarks link scores to college destinations. The first isolates the effect of a higher admission exam score. Haggag et al. (2024) use rounding of the ACT composite to estimate that one additional point raises four-year enrollment by 0.4 to 0.7 percentage points and moves students to colleges whose former students earn about \$100 more per year. Applied to my 0.3 to 0.6 point ACT effect, these estimates imply a college quality gain of \$30 to \$60, below the \$290 to \$490 I estimate. The gap is expected because a rounding shock is mechanical and changes nothing else about the student. Goodman, Gurantz and Smith (2020) partially illustrate how much the rest of the treatment matters. Retaking induced by round-number targets, which at a minimum involves additional preparation, raises scores by 0.3 standard deviations and four-year college enrollment by 13 percentage points. By comparison, estimates from Haggag et al. (2024) would imply that a score gain of the same size raises four-year college enrollment by only 1 percentage point. Tutoring centers provide far more than a retake, including sustained instruction, college counseling, and other services that help students gain admission to more selective colleges, so an effect several times the rounding benchmark is not unreasonable. The second benchmark uses the relationship between college selectivity and peer earnings. Among Texas public universities, a 100 point difference in average SAT score corresponds to roughly \$3,000 in enrollee earnings (Mountjoy, 2026), so the \$290 to \$490 gain from my estimates implies that a year of tutoring moves students to colleges 10 to 16 SAT points more selective, roughly matching the 8 to 13 point gain in their own scores.

Next, I use my own estimates to link college destinations to earnings. The effect on the average

early-career earnings of enrollees at the college attended is \$290 to \$490, whereas the effect on students' own earnings is \$1,130 to \$1,940. These magnitudes suggest that college destination alone is unlikely to account for the full earnings effect.

The two links, from admission exam scores to college destinations and from college destinations to earnings, thus point in the same direction. Scores translate into college destinations by more than a mechanical score change would predict, and the shift in college destinations accounts for only part of the earnings effect. Private tutoring therefore appears to do more than teach to the test or provide an advantage in admissions competition. Its gains persist beyond the exam and beyond college placement, consistent with skills that carry directly into the labor market or a better match between students and colleges that the enrollment measures do not capture.

9 Conclusion

This paper provides causal evidence on how private tutoring centers affect students' outcomes, from high school achievement through early-career earnings. I link geocoded business records covering the universe of tutoring establishments in Texas to statewide administrative data that follow more than five million public school students through K–12 schooling, college, and the labor market. Exploiting the entry and exit of tutoring centers near high schools, I compare students who attended the same school in different cohorts and therefore faced different local tutoring supply, relative to students at other schools in the same county and cohort.

I find that increased exposure to tutoring centers has no effect on state standardized test scores or high school graduation, but raises college admission exam scores, college quality, and early-career earnings. Among students who are not economically disadvantaged, one additional center near a student's school raises SAT scores by 1.66 points and ACT scores by 0.07 points, raises the average early-career earnings of enrollees at a student's college by \$61, increases enrollment at elite colleges by 0.20 percentage points, shifts students from in-state toward out-of-state four-year colleges by roughly 0.3 percentage points on each margin, and raises annual earnings in students' late twenties by \$241. These gains concentrate at the top of the outcome distributions. Estimates on unconditional quantiles show that tutoring centers widen the 90–10 interquantile range of SAT scores by 8.49 points and of early-career earnings by \$905, while leaving the state test score distribution unchanged at every decile.

The effects vary widely across students and markets. Across student subgroups, outcomes improve mainly for White students and remain largely unchanged for Hispanic and Black students. Gains appear both for gifted students and for students designated at risk of dropping out, suggesting that the benefits of tutoring centers track family resources more than prior achievement. Moreover, the effects are mostly concentrated at schools with a below-median share of economically disadvantaged students, and those on college and earnings arise mainly at schools the industry had already entered by the late 1990s. Across the industry itself, both chain and independent centers contribute to the effects. The estimates are larger for chain centers on most

outcomes, but the differences are generally not statistically significant.

To assess the validity of a causal interpretation of these estimates, I conduct three exercises. First, I estimate a distributed lag model that traces the cumulative effect of tutoring centers by a cohort's years of exposure. The effects build over the years students spend in high school and are absent for centers arriving after a cohort's graduation. Second, I estimate how the effects vary with the distance between tutoring centers and a student's school. The effects are concentrated among the nearest centers and decline to zero for centers too distant for students to plausibly attend. Third, I conduct a placebo test using pet care establishments, an industry that selects into the same neighborhoods as tutoring centers but offers no service that could affect student outcomes. Pet care establishments produce null effects on most outcomes, and the tutoring estimates are unchanged conditional on pet care exposure. Complementing these tests, I estimate the same specifications for economically disadvantaged students, who attend the same schools and face the same local conditions as their classmates but for whom private tutoring is largely unaffordable. Their estimates are null across nearly every outcome.

To provide direct evidence on tutoring participation, I design and implement an original survey of Texas parents with children in K–12. The survey shows that participation is concentrated among high-income, college-educated families. Disadvantaged families participate far less and spend less than half as much when they do. Using the survey data, I estimate that one additional tutoring center within two miles is associated with a 3.1 percentage point increase in the probability that a non-disadvantaged high school student attends a tutoring center. Accumulating this annual increase over the four years of high school implies that the effect of a year of center-based tutoring is roughly 8 times the ITT estimates. An alternative calculation that derives take-up from the capacity of a typical tutoring center and the number of high school students near it yields a scaling factor of roughly 5. Applying these factors, a year of center-based tutoring raises SAT scores by 8 to 13 points (0.04 to 0.07 standard deviations) and ACT scores by 0.3 to 0.6 points (0.07 to 0.11 standard deviations), moves students to colleges whose enrollees earn \$290 to \$490 more per year, raises elite college enrollment by 0.9 to 1.6 percentage points, and raises earnings in the late twenties by \$1,130 to \$1,940 per year, or 2 to 4 percent.

Together, my results show that private tutoring centers improve students' long-run outcomes. Because take-up is concentrated among high-income, college-educated households, the expansion of the tutoring industry has widened socioeconomic and racial gaps in educational investment and student outcomes among those who face the same local availability of tutoring centers. In the survey, the participation of disadvantaged families shows no detectable response to the local supply of tutoring centers. Disadvantaged families who do participate purchase similar weekly hours at lower hourly rates, which suggests that cost, rather than a lack of interest, is an important constraint on their use. Policies that lower this cost, such as tutoring subsidies or vouchers for low-income families, or that provide comparable services through schools, may extend the documented long-run gains to the students the private market leaves behind.

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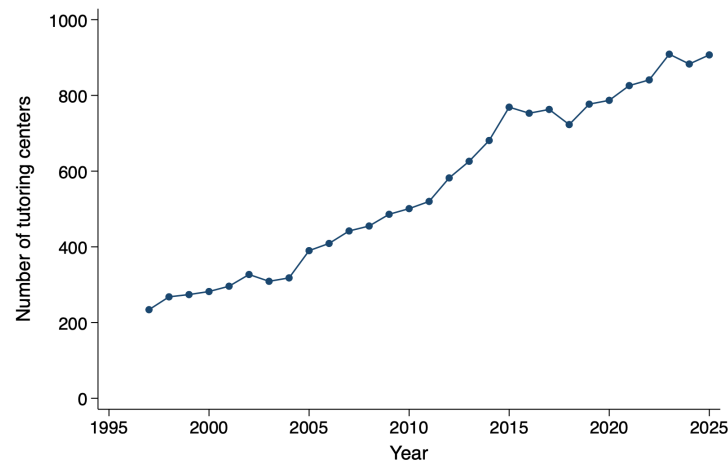
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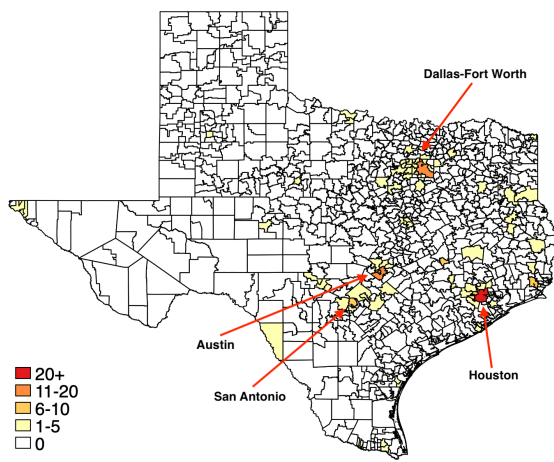
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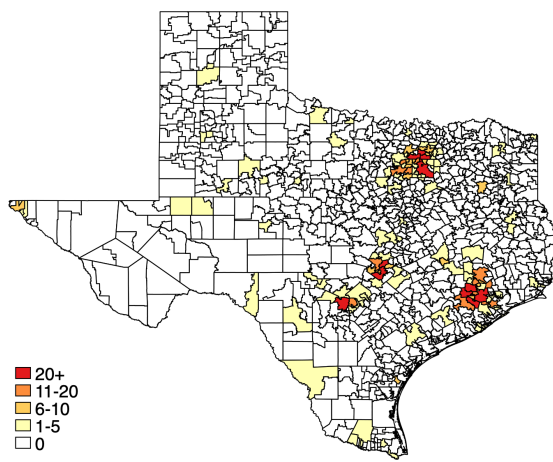
Figures and Tables



(a) Number of tutoring centers in Texas over time



(b) By district, 1997



(c) By district, 2025

Figure 1: Prevalence of Tutoring Centers in Texas

Notes: This figure describes the private tutoring industry in Texas from 1997 to 2025. Tutoring centers are establishments classified under NAICS code 611691, Exam Preparation and Tutoring. Panel (a) plots the number of active tutoring centers in the state in each year. Panels (b) and (c) map the number of active tutoring centers in each school district in 1997 and 2025.

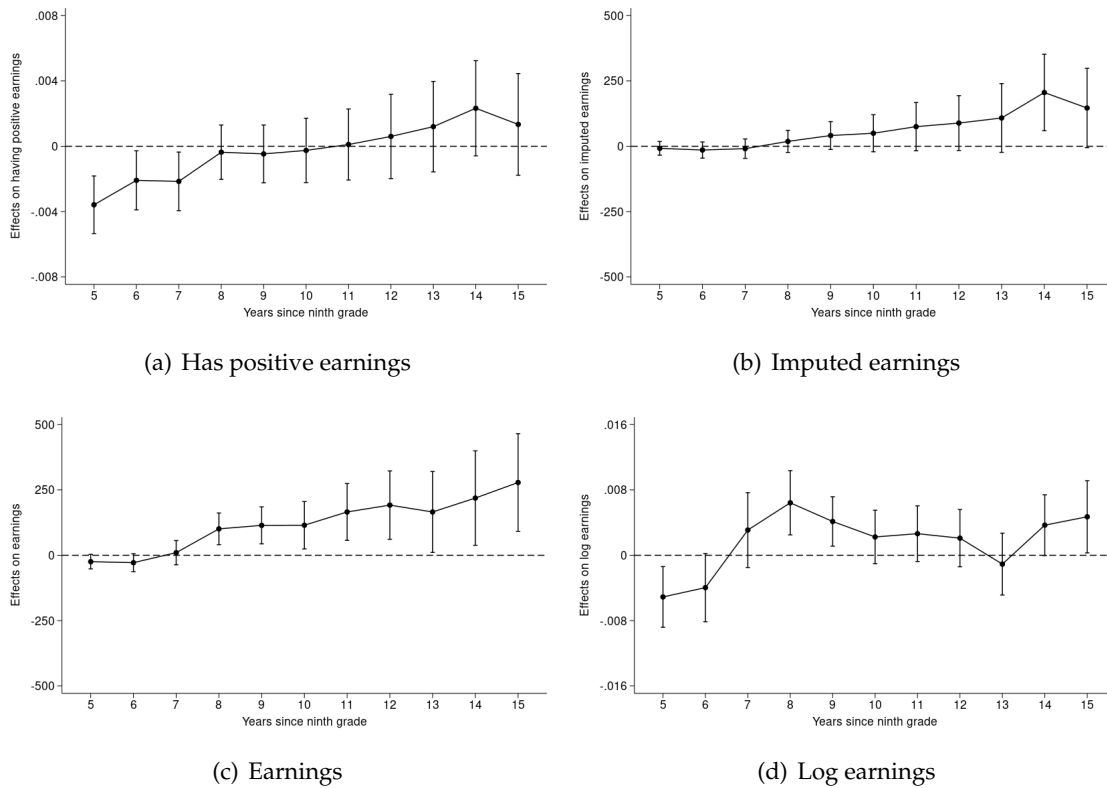


Figure 2: Effects on Earnings Trajectories

Notes: This figure plots the effect of one additional tutoring center on earnings outcomes in each year since five years after ninth grade and the 95 percent confidence intervals, for non-economically disadvantaged students. Standard errors are clustered at the school level. Each point is the estimate from a separate regression of the outcome in that year, and each regression uses the largest set of cohorts for which earnings are observed in the relevant year. Panel (a) reports effects on an indicator for positive earnings in the Texas earnings data. Panel (b) reports effects on annual earnings with quarters of no observed earnings coded as zero. Panels (c) and (d) restrict the sample to quarters with positive earnings and report effects on the level and the log of annual earnings. Earnings are quarterly earnings summed across employers, deflated to real 2015 dollars, winsorized at the 99th percentile, and annualized.

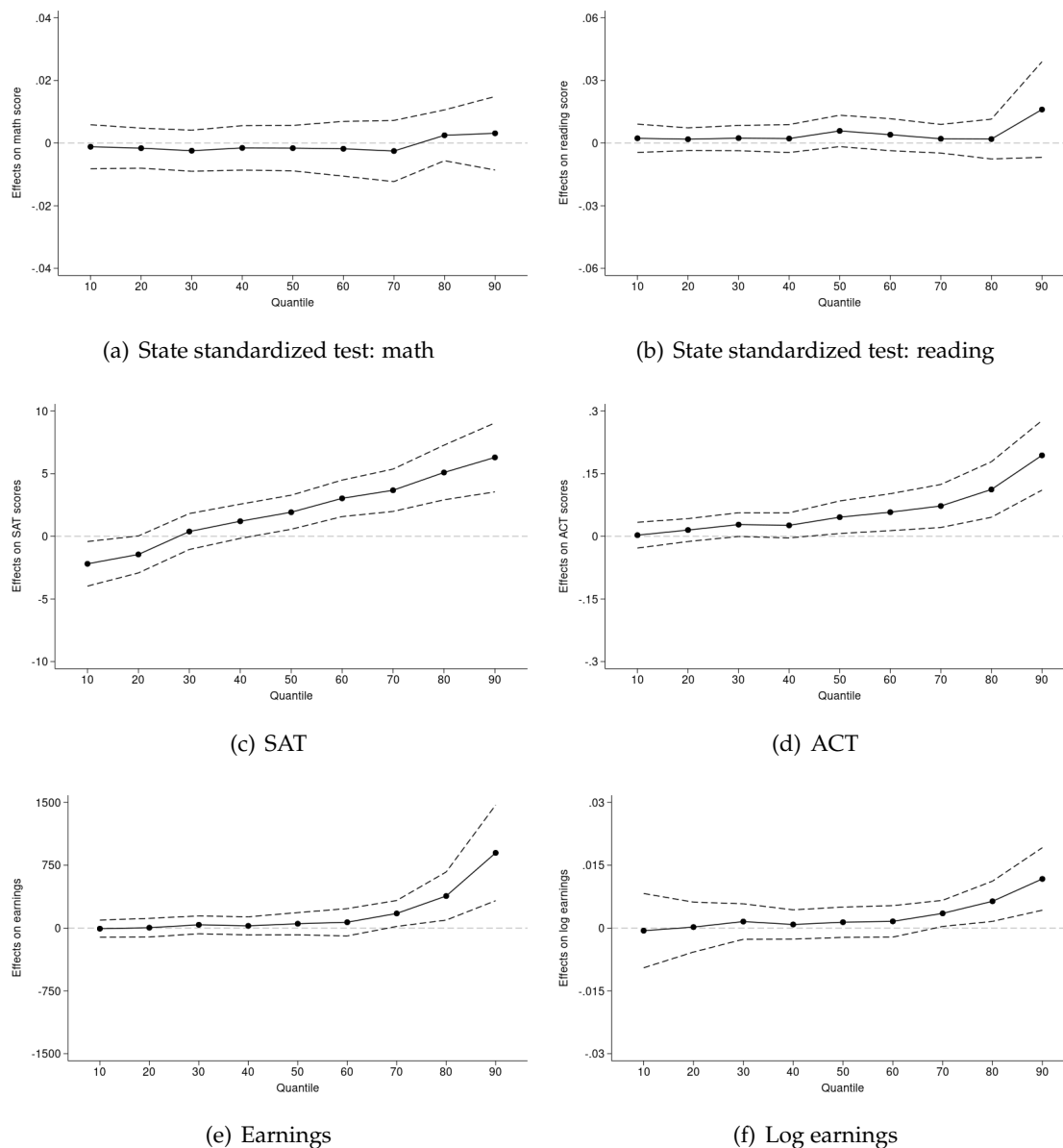


Figure 3: Effects of Tutoring Center Exposure Across Outcome Distributions

Notes: This figure plots the effect of one additional tutoring center on unconditional quantiles of each outcome and the 95 percent confidence intervals, for non-economically disadvantaged students. Standard errors are clustered at the school level. Each point is the estimate from a separate recentered influence function regression at the indicated decile of the outcome distribution. Panels (a) and (b) report effects on state standardized test scores, standardized within subject, test year, and grade. Panels (c) and (d) report effects on SAT and ACT scores among test takers who submit applications to Texas public four-year universities. Panels (e) and (f) report effects on annual earnings, in levels and in logs, 12–15 years after ninth grade, restricting to workers with positive earnings. Earnings are quarterly earnings summed across employers, deflated to real 2015 dollars, winsorized at the 99th percentile, and annualized.

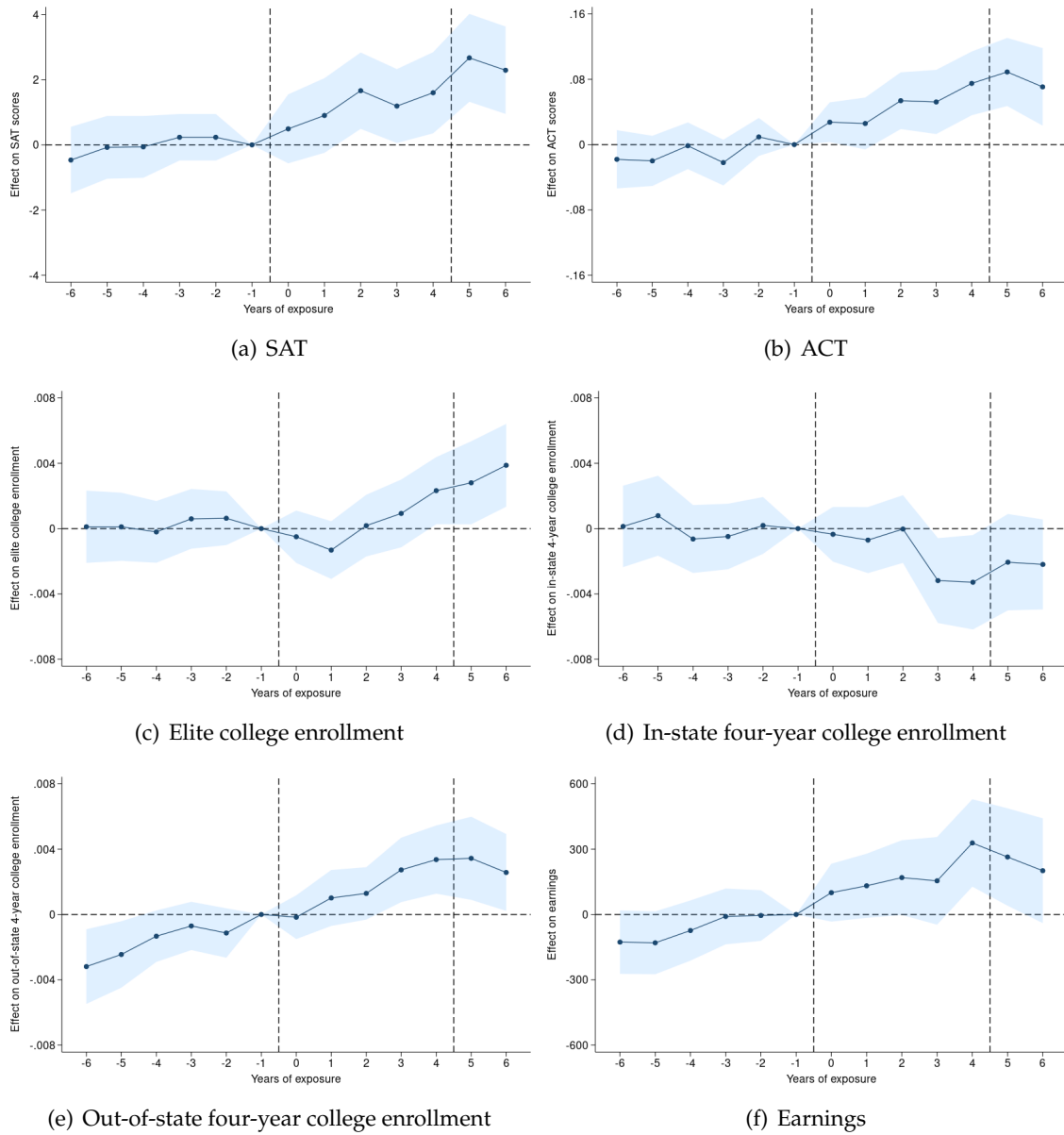


Figure 4: Effects by Years of Tutoring Exposure

Notes: This figure plots the effect of one additional tutoring center on the main outcomes by years of exposure and the 95 percent confidence intervals, for non-economically disadvantaged students. Standard errors are clustered at the school level. The horizontal axis measures years of exposure prior to projected high school graduation. The right dashed line marks the calendar year of ninth-grade enrollment, and the left dashed line marks graduation. The estimate at j years of exposure is the cumulative effect of one additional center that opens j calendar years before graduation and remains open through graduation. Negative values correspond to the years after, where the estimates capture the effect of center entry and exit occurring after the cohort has left high school.

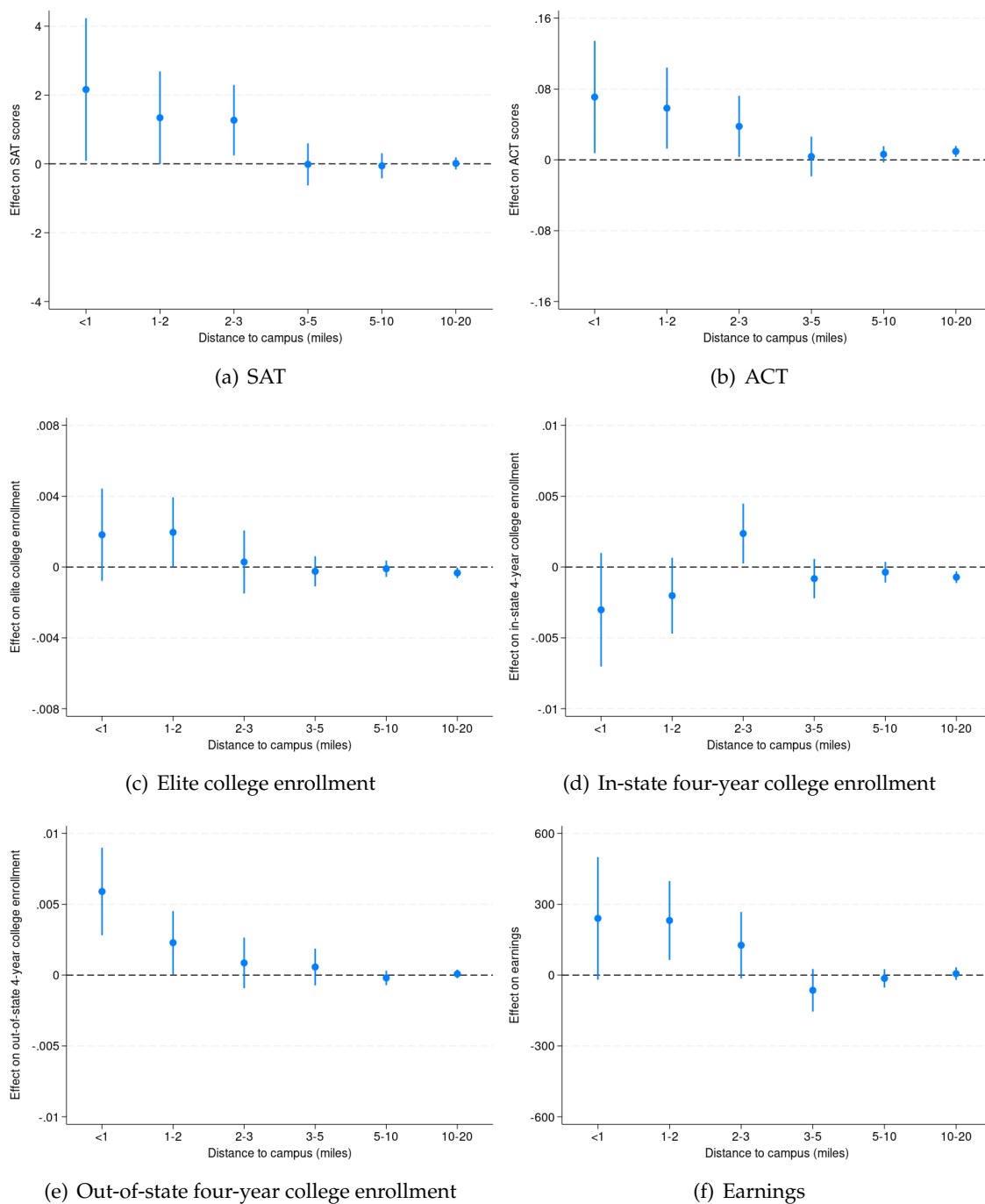
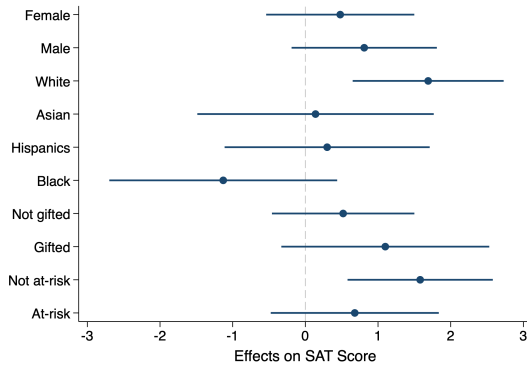
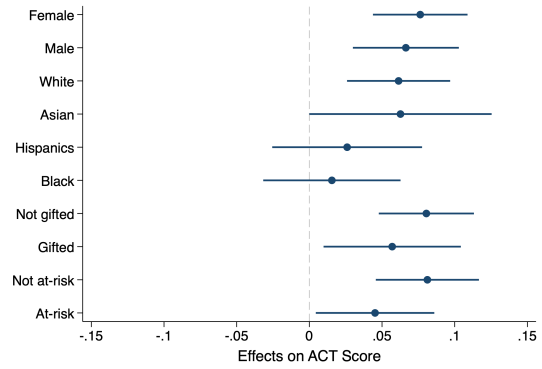


Figure 5: Effects by Distance to School

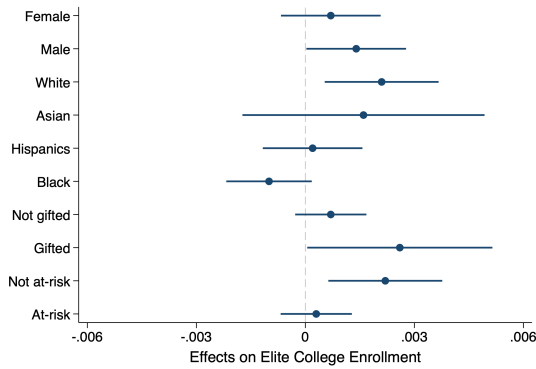
Notes: This figure plots the effect of one additional tutoring center on the main outcomes by the distance between the center and a student's ninth-grade campus, and the 95 percent confidence intervals, for non-economically disadvantaged students. Standard errors are clustered at the school level. Each panel reports estimates from a single regression in which the counts of centers in six mutually exclusive distance bands enter simultaneously, so each estimate is the effect of one additional center in that band holding the counts in all other bands fixed.



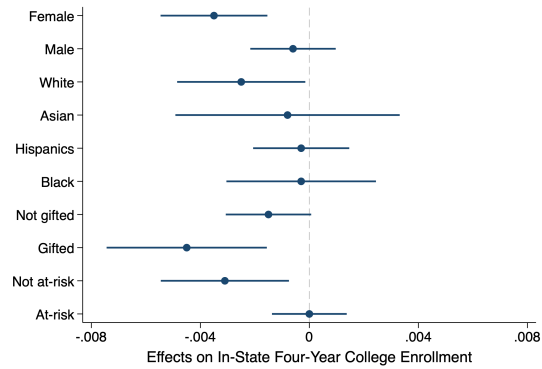
(a) SAT



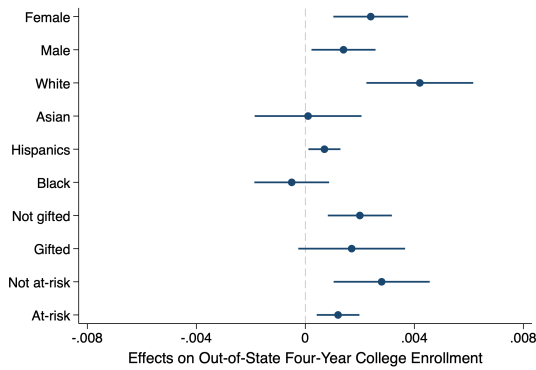
(b) ACT



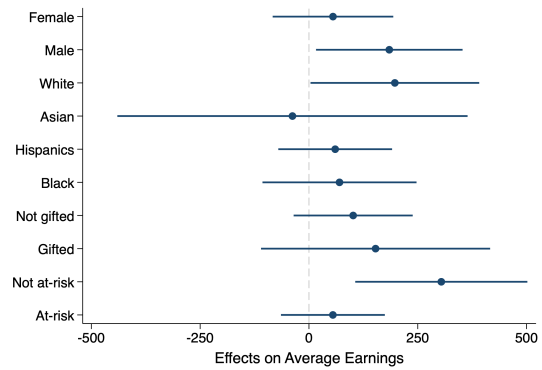
(c) Elite college enrollment



(d) In-state four-year college enrollment



(e) Out-of-state four-year college enrollment



(f) Earnings

Figure 6: Heterogeneity: Effects by Student Subgroup

Notes: This figure plots the main effects estimated separately by student subgroup. Standard errors are clustered at the school level. Subgroups are defined by gender, race and ethnicity, gifted program participation, and at-risk of dropping out status, all measured before ninth-grade enrollment. The full set of estimates is reported in Table A10.

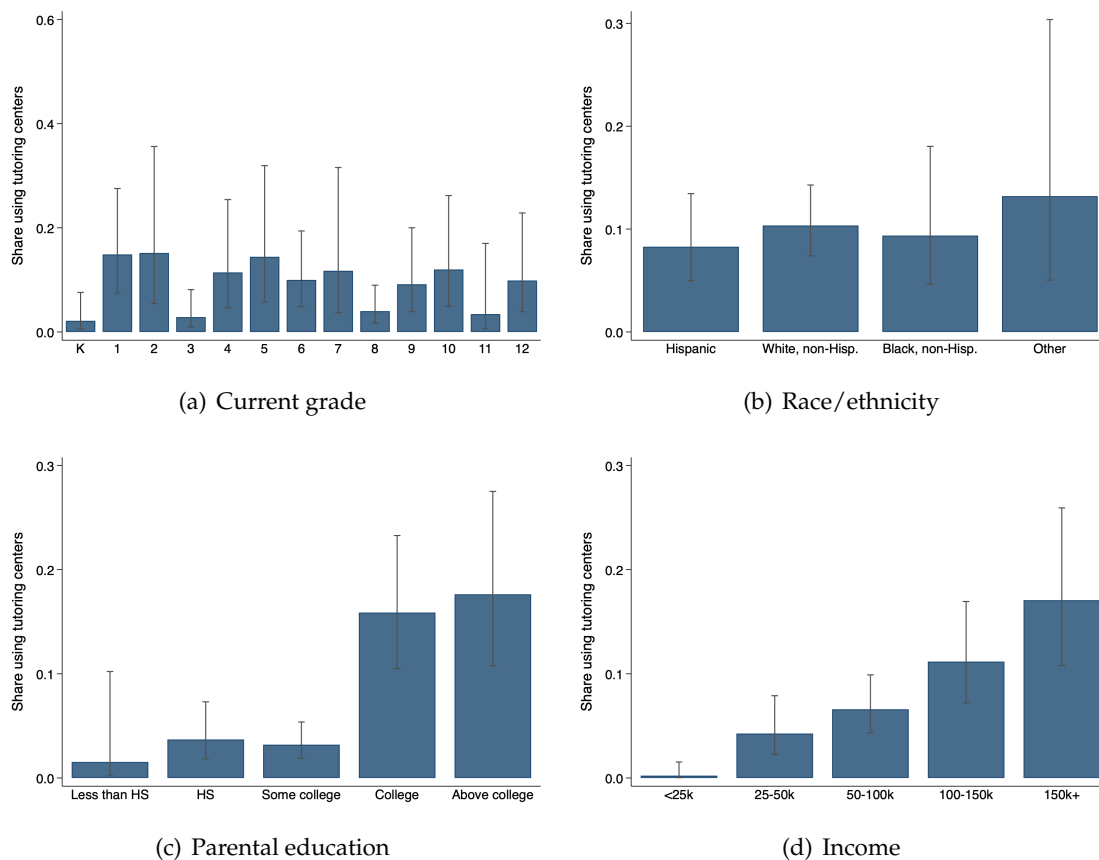


Figure 7: Tutoring Center Participation Rates

Notes: This figure plots the share of surveyed children currently receiving paid tutoring at a tutoring center and the 95 percent confidence intervals, by the child's current grade, race and ethnicity, parental education, and household income. Participation refers to paid private tutoring at a tutoring center for which the household pays the full cost.

Table 1: School-Level Exposure to Private Tutoring Centers in Texas

Distance to school	Number of tutoring centers			
	Mean	Median	Min	Max
Less than 1 mile	0.2	0.0	0.0	10.0
Less than 2 miles	0.7	0.0	0.0	22.0
Less than 3 miles	1.4	0.0	0.0	33.0
Less than 5 miles	3.4	1.0	0.0	54.0
Less than 10 miles	11.4	4.0	0.0	123.0
Less than 20 miles	33.6	9.0	0.0	237.0

* *Notes:* This table reports summary statistics for the number of active tutoring centers within the indicated radius of each public school in Texas, pooling all years. This includes 11,258 unique campuses and 326,482 unique campus-year observations between 1997 and 2025.

Table 2: The Analysis Sample

Outcomes	Ninth-grade cohorts	Outcome years	Number of students
<i>High school outcomes</i>			
State standardized test	2003–2011	2003–2013	2,414,061
High school graduation	1998–2016	2001–2020	5,474,264
<i>College outcomes</i>			
College admission exam [†]	2001–2016	2004–2019	1,333,734
College applications and admissions [†]	1998–2016	2001–2019	5,474,264
College enrollment	2005–2016	2008–2019	3,771,259
College degree completion [†]	2000–2014	2003–2023	4,315,668
<i>Labor market outcomes</i>			
Earnings	1998–2010	2009–2024	3,470,854

* *Notes:* This table reports the analysis sample underlying each of the main outcomes studied in the paper. The ninth-grade cohort range identifies the set of cohorts for whom the outcome is fully observed in the Texas ERC data. A cohort year denotes the year in which a student completes ninth grade: the 2003 cohort consists of students who completed ninth grade in spring 2003, entered ninth grade in fall 2002, and are projected to graduate from high school in spring 2006. Outcome years report the calendar years over which the outcome is measured for these cohorts.

[†] *Additional sample restrictions:* College admission exam (SAT/ACT) scores are only observed among students who submit at least one application to a Texas public four-year university. SAT and ACT scores are only available starting with the application cycle corresponding to the 2001 ninth-grade cohort. For the full eligible student sample, college application and admission outcomes measure applications to, and offers of admission from, Texas public four-year universities. College degree completion is restricted to degrees awarded by Texas postsecondary institutions.

Table 3: Summary Statistics

	All	Non-economically disadvantaged	Economically disadvantaged
<i>Panel A: Demographics</i>			
Female	0.492	0.490	0.493
Age	14.2	14.2	14.2
Asian	0.035	0.053	0.023
Black	0.134	0.080	0.170
Hispanic	0.442	0.195	0.605
White	0.376	0.656	0.191
At risk	0.604	0.313	0.796
Free or reduced-price lunch	0.602	0.000	1.000
Gifted	0.145	0.196	0.111
Eighth-grade reading	0.080	0.465	-0.166
Eighth-grade math	0.083	0.459	-0.153
Missing eighth-grade reading	0.145	0.164	0.133
Missing eighth-grade math	0.189	0.215	0.171
Number of students	5,474,264	2,180,722	3,293,542
<i>Panel B: High school outcomes</i>			
State standardized test: reading	0.062	0.403	-0.173
State standardized test: math	0.068	0.443	-0.191
High school graduation (in 4 years)	0.847	0.902	0.811
High school graduation (in 5 years)	0.872	0.916	0.844
<i>Panel C: College outcomes</i>			
SAT score	1057.9	1128.8	977.4
ACT score	22.0	24.0	19.6
Any in-state four-year college application	0.283	0.386	0.215
Any in-state four-year college admission	0.249	0.349	0.182
Top Ten Percent admission	0.068	0.107	0.041
Enrolled in any college	0.475	0.660	0.379
Enrolled in four-year college	0.262	0.437	0.171
Enrolled in two-year college	0.213	0.222	0.208
Enrolled in elite college	0.044	0.096	0.017
Enrolled in in-state four-year college	0.218	0.346	0.151
Enrolled in out-of-state four-year college	0.044	0.091	0.020
Any in-state college degree	0.231	0.365	0.146
In-state bachelor's degree	0.182	0.314	0.098
In-state associate degree	0.049	0.051	0.048
<i>Panel D: Labor market outcomes</i>			
Has positive earnings (in 12–15 years)	0.745	0.738	0.750
Average annual imputed earnings (in 12–15 years)	22,811	26,911	19,551
Average annual earnings (in 12–15 years)	34,971	41,208	30,094
Average log earnings (in 12–15 years)	10.2	10.4	10.0

* Notes: This table reports means for the primary analysis sample of ninth-grade students in Texas public schools. Demographic characteristics are measured for the 1998–2016 ninth-grade cohorts. Each outcome is measured for the cohorts listed in Table 2. Columns 2 and 3 report means separately by eligibility for free or reduced-price lunch. Eighth-grade test scores are standardized within subject and test year. Earnings are measured 12 to 15 calendar years after ninth-grade enrollment, winsorized at the 99th percentile, and reported in real 2015 dollars. Average annual imputed earnings include quarters with no observed earnings coded as zero; average annual earnings condition on positive earnings.

Table 4: Effects of Private Tutoring Centers on High School Outcomes

	All	Non-economically disadvantaged
<i>Panel A: Standardized test scores</i>		
State standardized test: math	-0.0030 (0.0040)	0.0003 (0.0036)
Observations (student-year)	6,808,234	2,718,616
State standardized test: reading	-0.0002 (0.0030)	0.0038 (0.0039)
Observations (student-year)	6,827,428	2,718,366
<i>Panel B: High school graduation</i>		
High school graduation (in 4 years)	-0.0013 (0.0011)	-0.0002 (0.0010)
High school graduation (in 5 years)	-0.0011 (0.0009)	-0.0003 (0.0009)
Observations	5,473,883	2,180,426

* Notes: This table reports estimates of the effect of one additional tutoring center within two miles of a student's ninth-grade campus on high school outcomes. Each cell reports the coefficient from a separate regression, with standard errors clustered at the school level. In Panel A, the outcome is the score on the state standardized test, standardized within subject, grade, and test year, and the treatment averages tutoring exposure from the start of high school through the tested grade. In Panel B, the outcomes are indicators for graduating within four and five years of ninth-grade enrollment, and the treatment averages tutoring exposure over the four expected high school years. Column 2 restricts the sample to non-economically disadvantaged students. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effects of Private Tutoring Centers on College Outcomes

	All	Non-economically disadvantaged
<i>Panel A: College admission exams</i>		
SAT score	0.65 (0.47)	1.66*** (0.52)
ACT score	0.07*** (0.02)	0.07*** (0.02)
<i>Panel B: Quality of college attended</i>		
<i>Conditional on any college enrollment</i>		
Any four-year college enrollment	-0.0007 (0.0012)	0.0013 (0.0015)
Average earnings of enrollees	36.3 (24.5)	61.1** (30.9)
Elite college enrollment	0.0010* (0.0006)	0.0020*** (0.0007)
<i>Panel C: Four-year college enrollment and degree</i>		
Any in-state four-year college enrollment	-0.0020** (0.0008)	-0.0026** (0.0011)
Any out-of-state four-year college enrollment	0.0019*** (0.0006)	0.0034*** (0.0009)
Any four-year college enrollment	-0.0001 (0.0008)	0.0008 (0.0013)
Any in-state bachelor's degree	-0.0015** (0.0007)	-0.0023** (0.0010)
<i>Panel D: Two-year college enrollment and degree</i>		
Any in-state two-year college enrollment	-0.0018** (0.0008)	-0.0015 (0.0011)
Any out-of-state two-year college enrollment	-0.0002*** (0.0001)	-0.0001 (0.0001)
Any two-year college enrollment	-0.0020** (0.0008)	-0.0016 (0.0010)
Any in-state associate degree	-0.0011*** (0.0003)	-0.0012*** (0.0003)

* Notes: This table reports estimates of the effect of one additional tutoring center within two miles of a student's ninth-grade campus on college outcomes. Each cell reports the coefficient from a separate regression, with standard errors clustered at the school level. In Panel A, the sample is restricted to students who apply to a Texas public four-year university, and the outcomes are the maximum SAT and ACT scores across applications. In Panel B, the average earnings of enrollees assigns to each student the leave-one-out mean of early-career earnings among enrollees at the student's college, and elite college enrollment is an indicator for enrolling at colleges classified as "most competitive" (Tier 1) in the Barron's College Selectivity Ratings, plus UT Austin and Texas A&M. Panels C and D report immediate enrollment and degree outcomes, where degrees are counted if completed at a Texas institution within six years of projected high school graduation. Column 2 restricts the sample to non-economically disadvantaged students. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effects of Private Tutoring Centers on Annual Earnings

	All	Non-economically disadvantaged
<i>Full sample</i>		
Has positive earnings	0.0007 (0.0016)	0.0013 (0.0019)
Imputed earnings	56.4 (48.5)	139.5** (68.0)
Observations	3,470,504	1,537,186
<i>Conditional on positive earnings</i>		
Average earnings	122.8* (68.5)	241.0*** (87.2)
Log earnings	0.0004 (0.0018)	0.0046** (0.0020)
Observations	2,584,064	1,133,943

* *Notes:* This table reports estimates of the effect of one additional tutoring center within two miles of a student's ninth-grade campus on early-career earnings. Each cell reports the coefficient from a separate regression, with standard errors clustered at the school level. Earnings are measured 12 to 15 calendar years after ninth-grade enrollment, deflated to real 2015 dollars, winsorized at the 99th percentile, annualized, and averaged within person over the window. In the top panel, the outcomes are an indicator for positive earnings in the Texas earnings data and average annual earnings with quarters of no observed earnings coded as zero. In the bottom panel, the sample is restricted to, and averaging over, quarters with positive earnings, and the outcomes are the level and the log of average annual earnings. Column 2 restricts the sample to non-economically disadvantaged students. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects by Economic Status

	Non-economically disadvantaged	Economically disadvantaged
<i>Panel A: High school outcomes</i>		
State standardized test: math	0.0003 (0.0036)	-0.0045 (0.0043)
State standardized test: reading	0.0038 (0.0039)	-0.0023 (0.0029)
High school graduation (in 4 years)	-0.0002 (0.0010)	0.0019 (0.0013)
<i>Panel B: College outcomes</i>		
SAT score	1.66*** (0.52)	-0.11 (0.61)
ACT score	0.07*** (0.02)	0.00 (0.02)
Average earnings of enrollees	61.1** (30.9)	-17.8 (27.2)
Elite college enrollment	0.0020*** (0.0007)	-0.0002 (0.0005)
In-state four-year college enrollment	-0.0026** (0.0011)	-0.0001 (0.0008)
Out-of-state four-year college enrollment	0.0034*** (0.0009)	0.0001 (0.0003)
Any four-year college enrollment	0.0008 (0.0013)	-0.0000 (0.0009)
Any in-state bachelor's degree	-0.0023** (0.0010)	0.0005 (0.0007)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>		
Average earnings	241.0*** (87.2)	-85.0 (61.6)
Log earnings	0.0046** (0.0020)	-0.0048* (0.0026)

* Notes: This table reports the estimates from the main specification for non-economically disadvantaged students and economically disadvantaged students. The non-economically disadvantaged estimates are identical to those reported in Tables 4 through 6. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Student and Household Demographics from the Survey

	Unweighted	Weighted
Female	0.44	0.44
White	0.43	0.33
Hispanic	0.35	0.48
Black	0.15	0.12
Other race	0.08	0.06
Attends public school	0.85	0.90
Elementary school (K–5)	0.46	0.45
Middle school (6–8)	0.21	0.23
High school (9–12)	0.32	0.32
Parental education (college degree or above)	0.36	0.46
Household income (below \$50,000)	0.37	0.25
Household income (\$50,000–\$100,000)	0.35	0.28
Household income (above \$100,000)	0.26	0.47
Economically disadvantaged	0.41	0.35
Number of K–12 children	1.76	1.89
Urban	0.38	0.37
Suburban	0.45	0.49
Rural	0.18	0.14
Observations	1,150	1,150

Notes: This table reports means of student and household demographics in the survey sample, unweighted and weighted. The weights match the sample to the 2024 ACS population of Texas children enrolled in K–12 on private versus public school enrollment, household income, race and ethnicity, grade level, and economic disadvantage. The student refers to the respondent’s K–12 child with the most recent birthday. Economically disadvantaged indicates a household income that falls below 185 percent of the federal poverty line, computed from reported income and household size.

Table 9: Private Tutoring Participation and Spending from the Survey

	(1)	(2)	(3)	(4)
	All	Non-econ. disadv.	Econ. disadv.	<i>p</i> -value (2)–(3)
<i>Panel A: Participation</i>				
Paid private tutoring	0.25	0.30	0.16	<0.001
Paid one-on-one tutoring	0.17	0.20	0.12	0.007
Paid tutoring at a tutoring center	0.09	0.13	0.03	<0.001
Observations	1,150	679	471	
<i>Panel B: Spending, conditional on participation</i>				
Annual spending (\$)	2,761	3,188	1,215	<0.001
Hourly rate (\$)	48.2	51.8	35.4	0.001
Hours per week	3.2	3.2	3.2	0.912
Observations	237	179	58	

Notes: This table reports weighted means of tutoring participation and spending in the survey sample. Participation refers to paid private tutoring for which the household pays the full cost, and the participation categories are not mutually exclusive. Panel B restricts the sample to children who currently participate in paid private tutoring. Spending and hourly rates are computed from bracketed responses using bracket midpoints. Non-econ. disadv. and econ. disadv. divide households by economic disadvantage as defined in Table 8. Column (4) reports *p*-values from weighted regressions of each outcome on an indicator for economic disadvantage.

Table 10: First Stage: Local Supply of Tutoring Centers and Tutoring Participation

	All		Non-econ. disadv.		Econ. disadv.	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>All students</i>						
Paid tutoring at a tutoring center	0.022*** (0.006)	0.018*** (0.007)	0.021** (0.008)	0.020** (0.009)	0.009 (0.007)	0.007 (0.007)
Paid one-on-one tutoring	-0.005 (0.007)	-0.014** (0.007)	-0.012 (0.007)	-0.019** (0.008)	0.002 (0.021)	0.006 (0.019)
Observations	1,142	1,142	674	674	468	468
<i>High school students</i>						
Paid tutoring at a tutoring center	0.028** (0.011)	0.028*** (0.010)	0.031*** (0.012)	0.031*** (0.010)	0.003 (0.010)	0.006 (0.011)
Paid one-on-one tutoring	-0.013 (0.012)	-0.024 (0.015)	-0.016 (0.014)	-0.025 (0.017)	-0.027 (0.023)	-0.025 (0.027)
Observations	372	372	229	229	143	143
Demographic controls		✓		✓		✓
County FE	✓	✓	✓	✓	✓	✓

Notes: This table reports weighted estimates from regressions of tutoring participation on the number of tutoring centers within two miles of the centroid of the ZIP code of the child's school. Each row within a panel reports a separate regression. In the first row of each panel, the dependent variable is an indicator for currently receiving paid tutoring at a tutoring center. In the second row, it is an indicator for currently receiving paid one-on-one tutoring. The top panel uses the full sample, and the bottom panel restricts the sample to students in grades 9 through 12. Demographic controls include those reported in Table 8. Columns (3) through (6) divide the sample by economic disadvantage. The sample excludes eight respondents whose school ZIP codes cannot be matched to a county. Standard errors are clustered at the school ZIP code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

SUPPLEMENTAL APPENDIX

A Figures and Tables

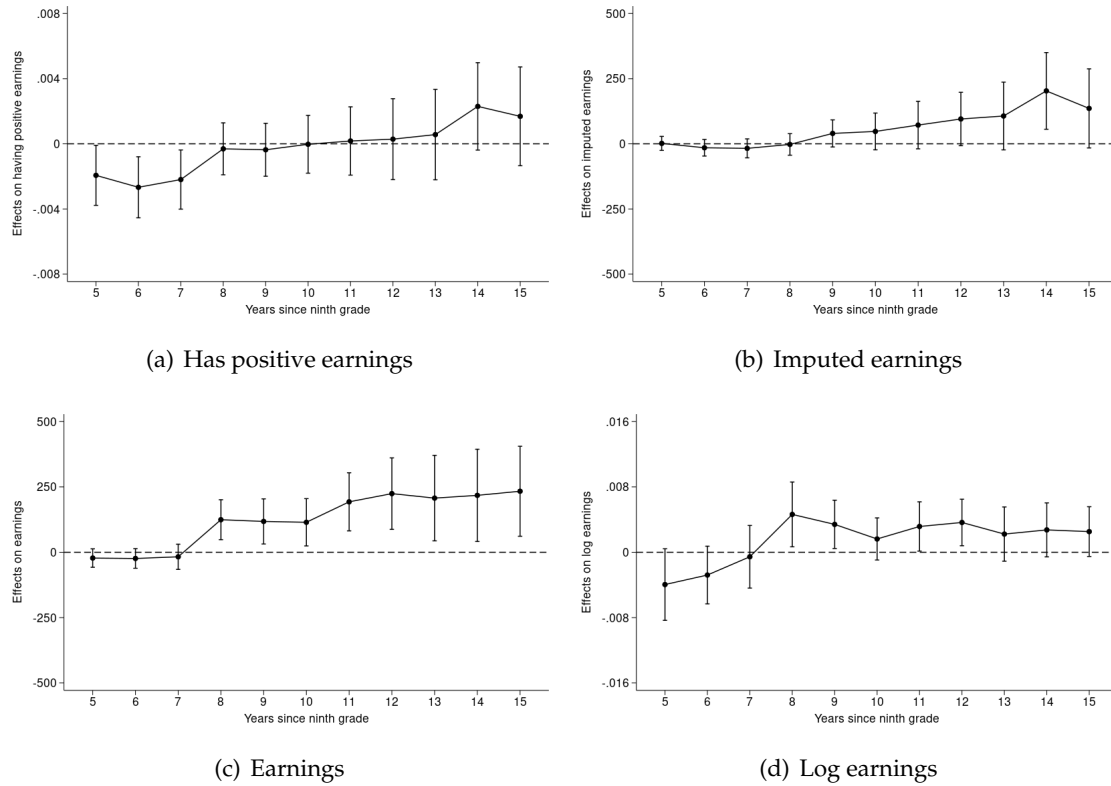
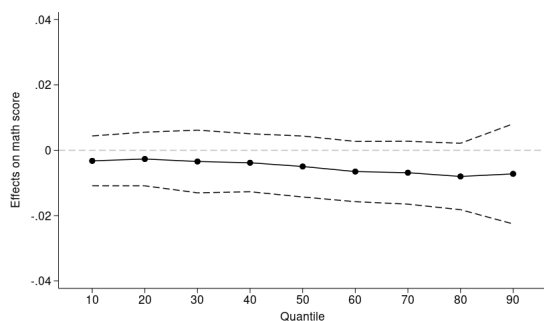
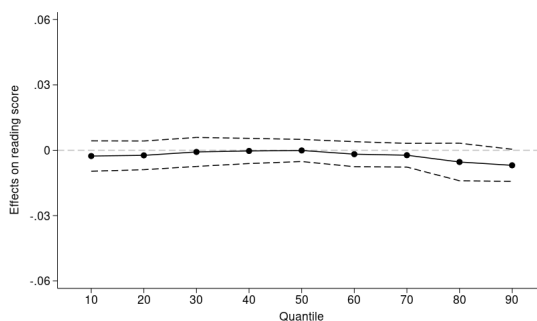


Figure A1: Effects on Sandwich Earnings Trajectories

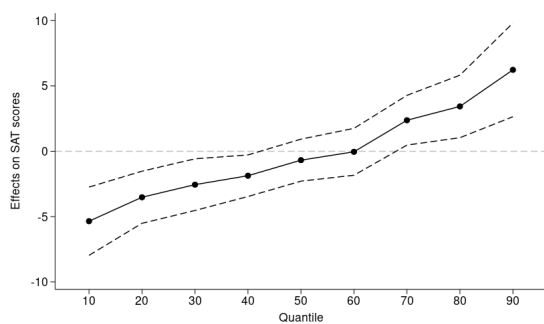
Notes: This figure replicates Figure 2 using sandwich earnings, which retain only quarters of positive earnings that are immediately preceded and followed by quarters with positive earnings. Each point is the estimate of the effect of one additional tutoring center on the earnings outcome in the corresponding year since five years after ninth grade, for non-economically disadvantaged students.



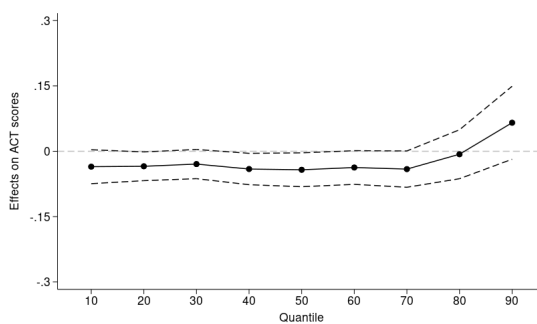
(a) State standardized test: math



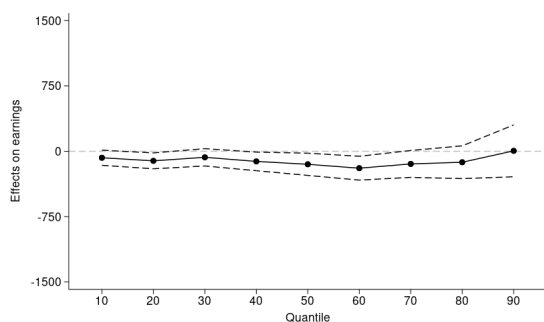
(b) State standardized test: reading



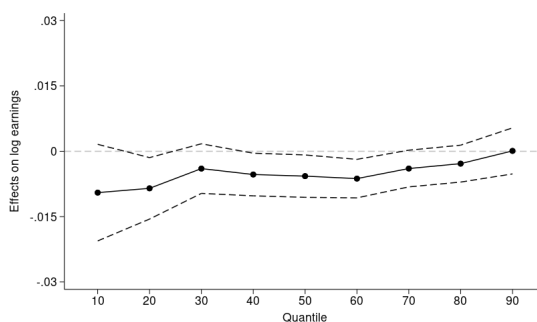
(c) SAT



(d) ACT



(e) Earnings



(f) Log earnings

Figure A2: Effects Across Outcome Distributions (Economically Disadvantaged)

Notes: This figure replicates Figure 3 for economically disadvantaged students. It plots the effect of one additional tutoring center on unconditional quantiles of each outcome and the 95 percent confidence intervals. Standard errors are clustered at the school level. Each point is the estimate from a separate recentered influence function regression at the indicated decile of the outcome distribution.

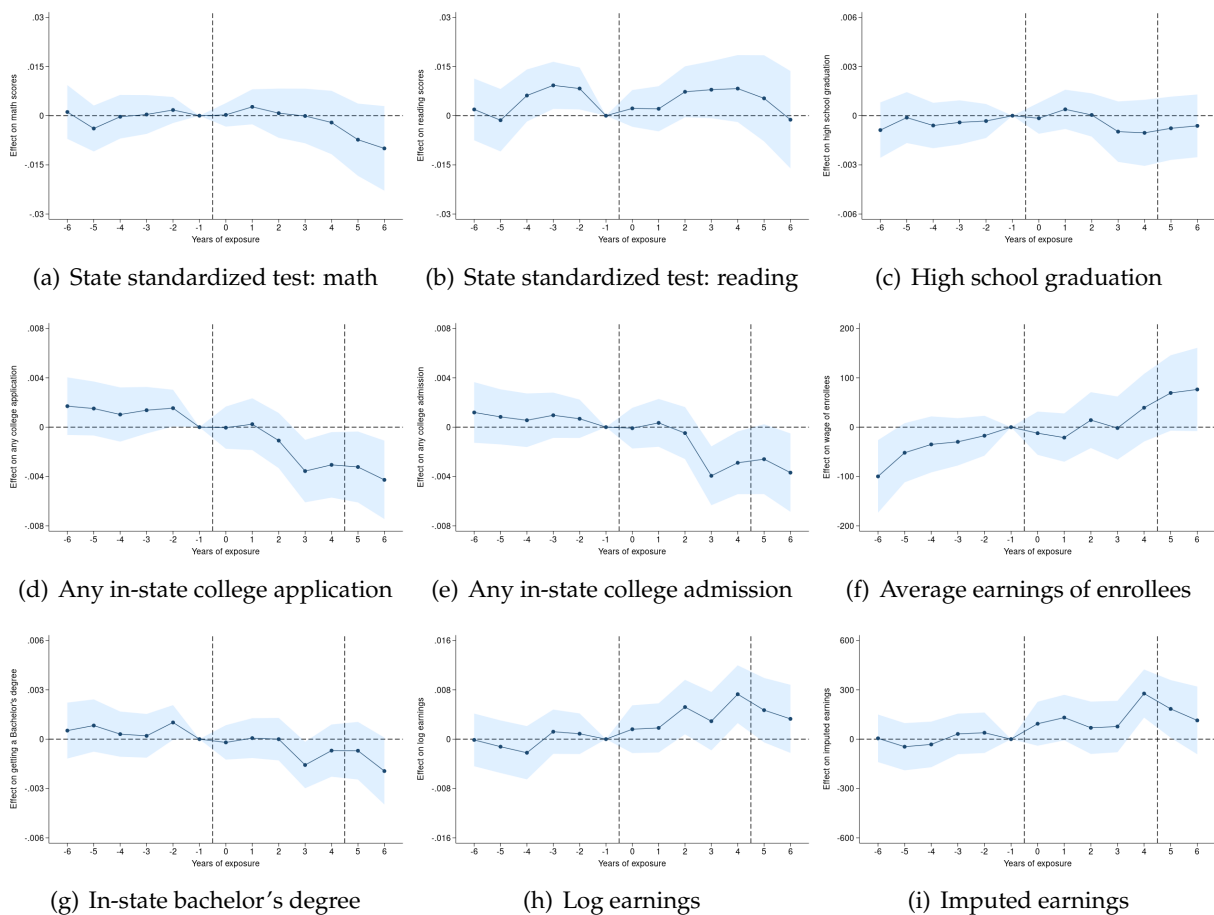


Figure A3: Additional Effects by Years of Tutoring Exposure

Notes: This figure replicates Figure 4 for additional outcomes. Each point is the estimate of the cumulative effect of one additional tutoring center by years of exposure, for non-economically disadvantaged students. For the state standardized test outcomes in panels (a) and (b), zero years of exposure corresponds to the year in which the test is taken rather than projected high school graduation, so the exposure window ends in the year the test is taken. For all other outcomes, zero years of exposure corresponds to projected high school graduation, as in Figure 4. Panels (d) and (e) refer to applications and admissions to in-state public four-year universities.

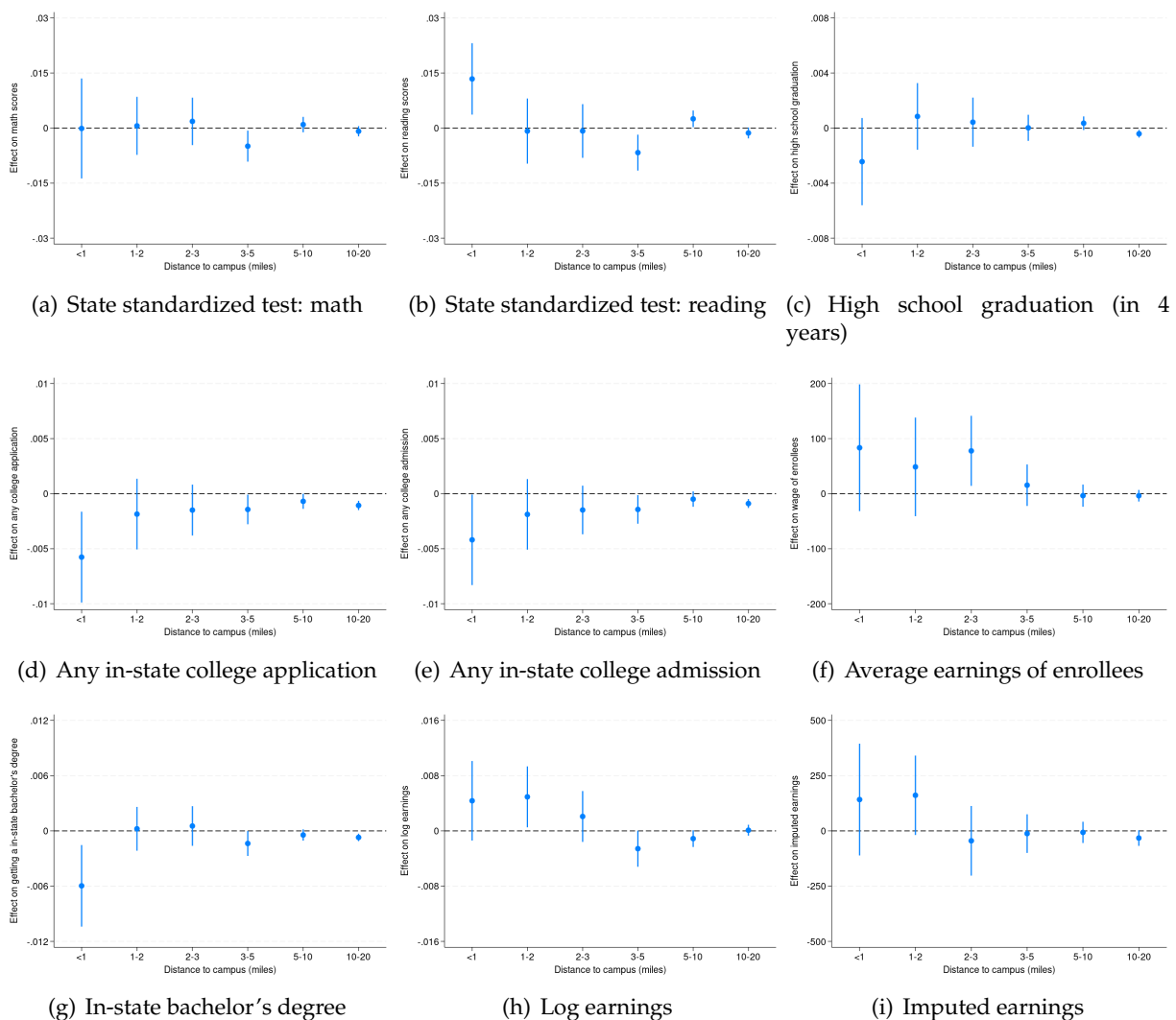
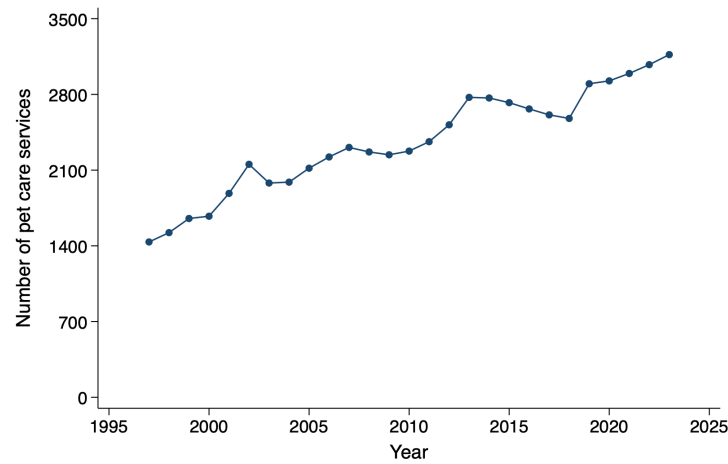
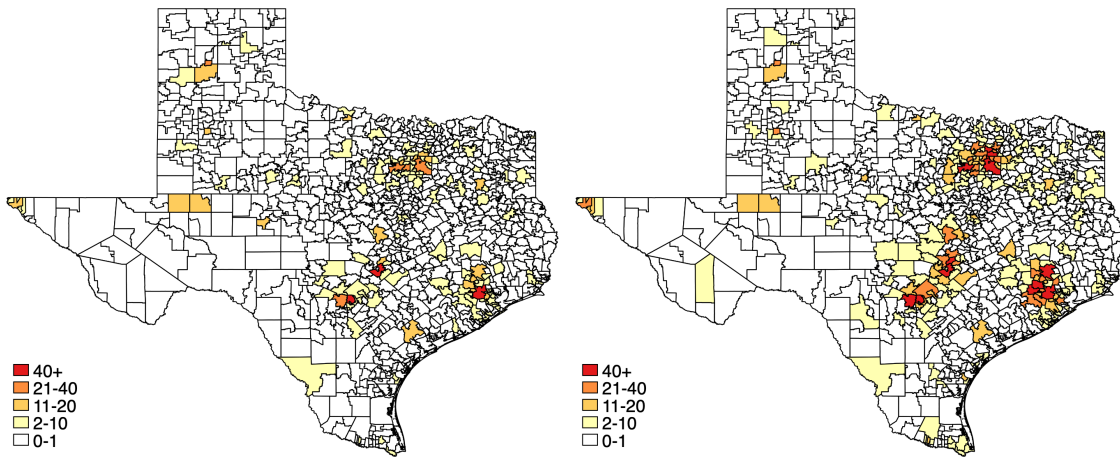


Figure A4: Additional Effects by Distance to School

Notes: This figure replicates Figure 5 for additional outcomes. Each point is the estimate of the effect of one additional tutoring center in the indicated distance band from a student's ninth-grade campus, for non-economically disadvantaged students. Panels (d) and (e) refer to applications and admissions to in-state public four-year universities.



(a) Statewide count over time



(b) By district, 1997

(c) By district, 2023

Figure A5: Prevalence of Pet Care Services in Texas

Notes: This figure replicates Figure 1 using pet care establishments, classified under NAICS code 812910, measured from Data Axle Historical Business Data. Panel (a) plots the number of active establishments in the state in each year. Panels (b) and (c) map the number of active establishments in each school district in 1997 and 2023. Districts are shaded by the number of establishments according to the legend.

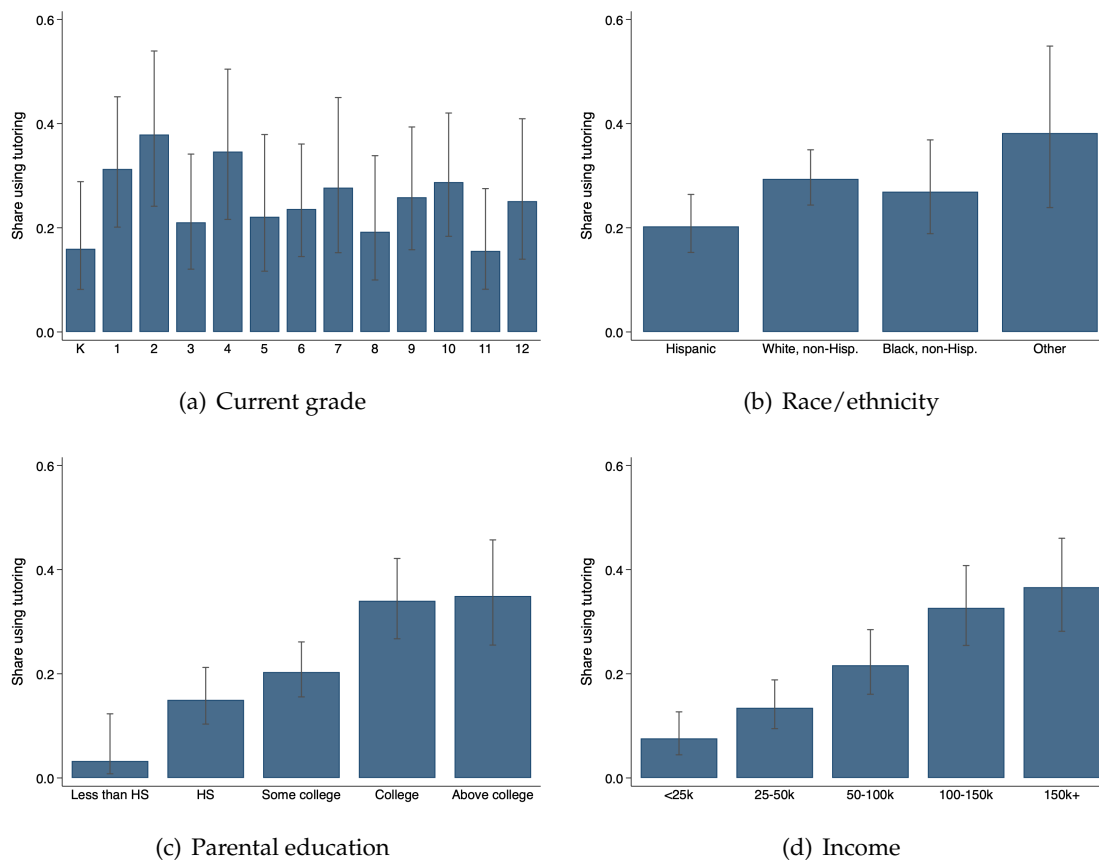


Figure A6: Private Tutoring Participation Rates

Notes: This figure replicates Figure 7 using participation in paid private tutoring of any form, rather than tutoring at a tutoring center. Each bar is the share of children currently receiving paid private tutoring for which the household pays the full cost, by the child's current grade, race and ethnicity, parental education, and household income.

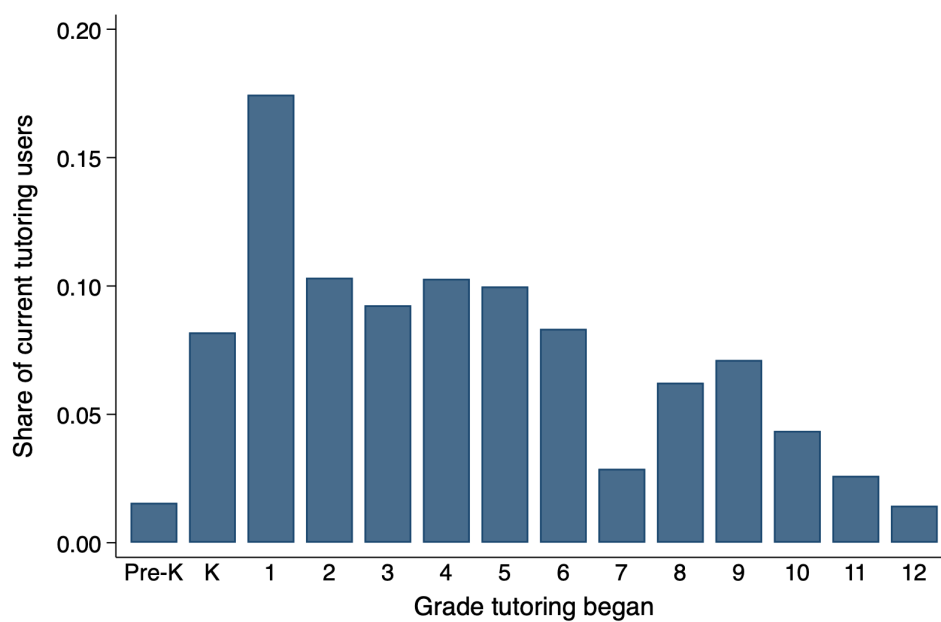
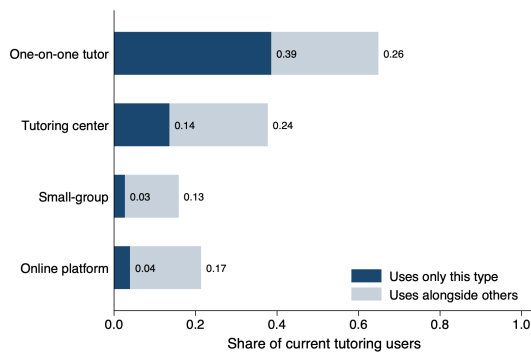
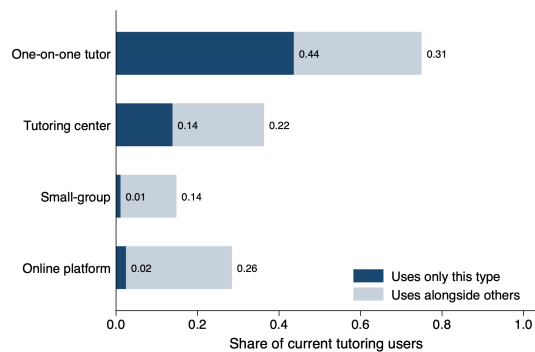


Figure A7: Grade tutoring began

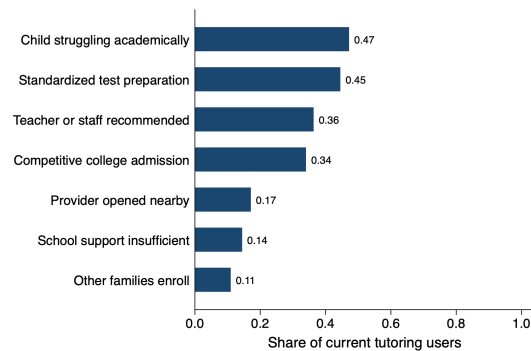
Notes: This figure plots the distribution of the grade in which the child first began receiving private tutoring in any form, among children currently receiving paid private tutoring. Each bar is the share of current tutoring users who began in the indicated grade.



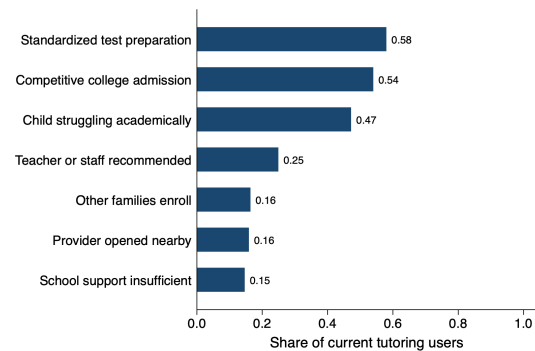
(a) Types of tutoring used (K–8)



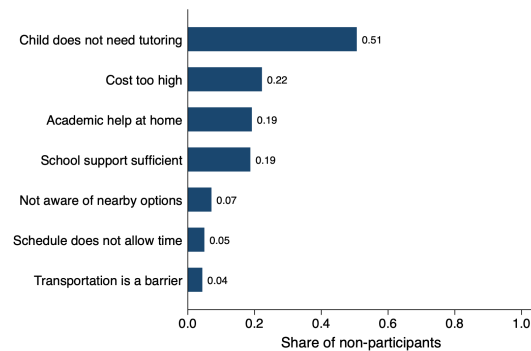
(b) Types of tutoring used (9–12)



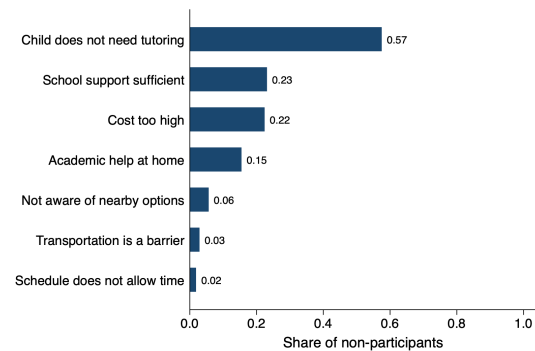
(c) Reasons for enrollment (K–8)



(d) Reasons for enrollment (9–12)



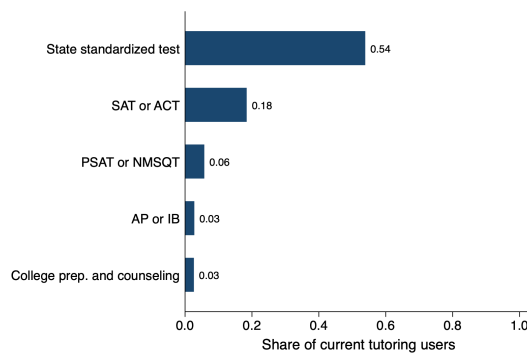
(e) Reasons for non-enrollment (K–8)



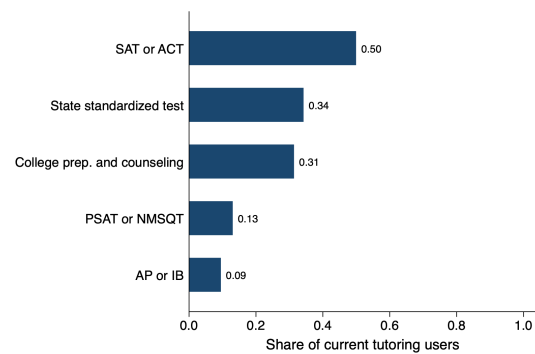
(f) Reasons for non-enrollment (9–12)

Figure A8: Types of Tutoring Used and Reasons for (Non-)Enrollment

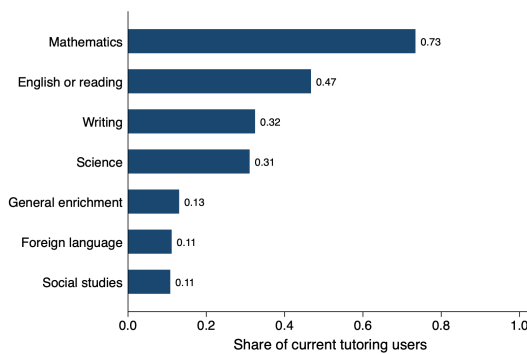
Notes: This figure describes the characteristics of private tutoring use in the survey sample, separately for children in kindergarten through grade 8 (left panels) and grades 9 through 12 (right panels). Panels (a) through (d) restrict the sample to children currently receiving paid private tutoring, and panels (e) and (f) restrict the sample to children not currently receiving paid private tutoring. In panels (a) and (b), the dark bars report the share of tutored children using only the indicated type and the light bars report the share using it alongside other types. All panels report responses to questions in which respondents can select multiple options, so the shares do not sum to one.



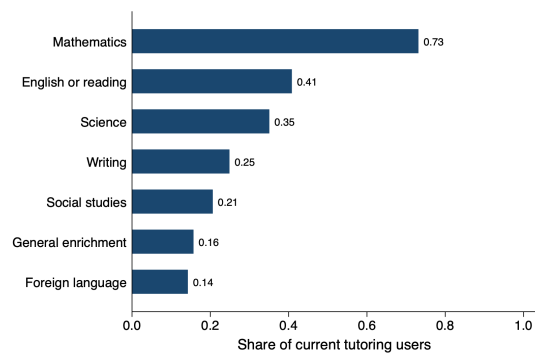
(a) Tutoring content (K–8)



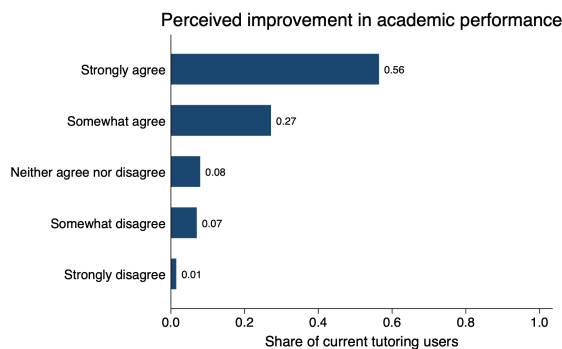
(b) Tutoring content (9–12)



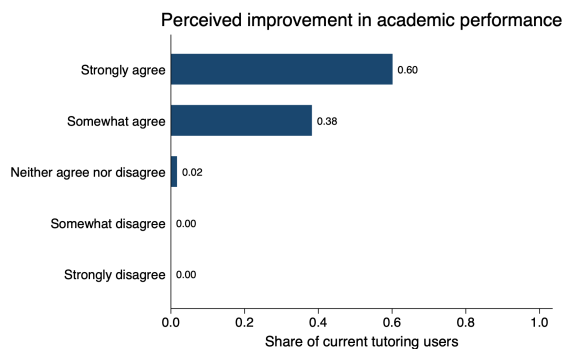
(c) Subjects tutored (K–8)



(d) Subjects tutored (9–12)



(e) Perceived effectiveness (K–8)



(f) Perceived effectiveness (9–12)

Figure A9: Tutoring Content and Perceived Effectiveness

Notes: This figure describes the content and perceived effectiveness of private tutoring in the survey sample, separately for children in kindergarten through grade 8 (left panels) and grades 9 through 12 (right panels). All panels restrict the sample to children currently receiving paid private tutoring. Panels (a) through (d) report responses to questions in which respondents can select multiple options, so the shares do not sum to one. Panels (e) and (f) report responses to the statement: “Since the selected child began receiving private tutoring, the selected child’s academic performance has improved.”

Table A1: Largest Tutoring Chains in Texas (1997–2025)

Company	Establishments	Share (%)	Employees	Share (%)
Kumon	2,938	17.98%	19,975	16.86%
Sylvan Learning	1,661	10.17%	27,299	23.04%
Mathnasium	1,014	6.21%	6,290	5.31%
Huntington Learning	555	3.40%	8,502	7.18%
Eye Level	313	1.92%	1,135	0.96%
Club Z	292	1.79%	1,510	1.27%
Taylor Robinson Music	280	1.71%	1,027	0.87%
Tutor Doctor	202	1.24%	787	0.66%
The Tutoring Center	174	1.07%	819	0.69%
Gideon Math & Reading	134	0.82%	1,098	0.93%
Total of Top 10	7,563	46.29%	68,442	57.77%
All Chains	8,136	49.80%	73,010	61.63%
All	16,338	100%	118,471	100%

* *Notes:* This table reports the ten largest private tutoring chains in Texas, ranked by total establishment-year observations across all years. The first and third columns report the total number of establishment-year observations and total employee-years, respectively, while the second and fourth columns report their corresponding shares of the Texas total.

Table A2: School-Level SAT and ACT Participation and Performance

	All		Non-econ. disadv.		Econ. disadv.	
	Sample size	Mean	Sample size	Mean	Sample size	Mean
SAT math	14,496	505.7 (52.7)	9,119	539.4 (48.8)	8,747	469.2 (40.2)
SAT reading	14,496	495.2 (57.3)	9,119	531.0 (52.6)	8,747	453.2 (49.6)
SAT total	14,496	1000.8 (108.5)	9,119	1070.4 (99.5)	8,747	922.4 (87.5)
SAT participation rate	17,048	0.645 (0.235)	13,310	0.730 (0.191)	13,310	0.614 (0.274)
ACT composite	14,777	20.9 (2.7)	8,299	23.0 (2.4)	8,171	18.7 (2.1)
ACT participation rate	17,048	0.405 (0.250)	13,310	0.648 (0.327)	13,310	0.678 (0.369)
Number of graduates	8,025	489 (252)	5,178	354 (227)	4,697	274 (173)

* *Notes:* This table reports summary statistics for school-level SAT and ACT outcomes among graduates of Texas public high schools in the 2011 to 2020 graduation cohorts, weighted by the number of graduates at each school in the indicated group, from data published by the Texas Education Agency. Standard deviations are in parentheses. Sample sizes are school-year cells with non-missing values of the indicated variable. Participation rates range from zero to one. Schools with a small number of test takers or graduates are masked in the source files and are therefore absent from the sample; as a result, the statistics from the full sample do not necessarily lie between the two subsamples.

Table A3: Effects on Non-Economically Disadvantaged Students Across Specifications

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: High school outcomes</i>					
State standardized test: math	0.0034 (0.0043)	-0.0058 (0.0043)	-0.0035 (0.0039)	-0.0005 (0.0041)	0.0003 (0.0036)
State standardized test: reading	0.0003 (0.0045)	-0.0061 (0.0043)	-0.0032 (0.0041)	0.0027 (0.0041)	0.0038 (0.0039)
High school graduation (in 4 years)	-0.0005 (0.0012)	-0.0011 (0.0010)	-0.0002 (0.0010)	-0.0011 (0.0010)	-0.0002 (0.0010)
<i>Panel B: College outcomes</i>					
SAT score	4.24*** (0.91)	1.56** (0.64)	1.31** (0.60)	1.99*** (0.59)	1.66*** (0.52)
ACT score	0.24*** (0.03)	0.15*** (0.02)	0.11*** (0.02)	0.09*** (0.02)	0.07*** (0.02)
Average earnings of enrollees	119.0*** (37.7)	49.0 (35.1)	50.4 (34.3)	69.5** (30.8)	61.1** (30.9)
Elite college enrollment	0.0032*** (0.0009)	0.0012 (0.0008)	0.0014* (0.0008)	0.0019*** (0.0007)	0.0020*** (0.0007)
In-state four-year college enrollment	-0.0021 (0.0013)	-0.0042*** (0.0012)	-0.0034*** (0.0012)	-0.0030*** (0.0011)	-0.0026** (0.0011)
Out-of-state four-year college enrollment	0.0034*** (0.0012)	0.0034*** (0.0012)	0.0031*** (0.0011)	0.0037*** (0.0009)	0.0034*** (0.0009)
Any four-year college enrollment	0.0013 (0.0019)	-0.0008 (0.0016)	-0.0003 (0.0016)	0.0007 (0.0013)	0.0008 (0.0013)
Any in-state bachelor's degree	-0.0030** (0.0013)	-0.0061*** (0.0013)	-0.0049*** (0.0012)	-0.0029*** (0.0011)	-0.0023** (0.0010)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>					
Average earnings	396.6*** (120.4)	176.1* (99.9)	140.8 (94.6)	284.7*** (94.4)	241.0*** (87.2)
Log earnings	0.0050* (0.0027)	-0.0006 (0.0023)	-0.0000 (0.0023)	0.0051** (0.0021)	0.0046** (0.0020)
School FE, cohort FE	✓	✓	✓	✓	✓
Student characteristics		✓	✓	✓	✓
Lagged school covariates			✓		✓
County-by-cohort FE				✓	✓

* Notes: This table reports estimates of the effect of one additional tutoring center for non-economically disadvantaged students under five different specifications. All columns include high school fixed effects and cohort fixed effects. Column 1 includes no additional controls. Columns 2 through 5 add student-level demographic controls, including eighth-grade test scores. Columns 3 and 5 add lagged school-level covariates. Columns 4 and 5 add county-by-cohort fixed effects. Column 5 corresponds to the main specification. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effects of Tutoring Centers on School-Level SAT and ACT Outcomes

	All	Non-econ. disadv.	Econ. disadv.	White	Asian	Hispanic	Black
<i>Panel A: SAT</i>							
SAT total	1.444 (1.246)	3.164** (1.345)	-0.007 (1.813)	3.660*** (0.978)	-0.835 (1.427)	0.618 (1.505)	1.032 (1.508)
SAT math	1.129* (0.677)	2.113*** (0.754)	-0.060 (1.007)	2.369*** (0.560)	0.235 (0.810)	0.708 (0.787)	0.504 (0.765)
SAT reading	0.296 (0.607)	1.049* (0.628)	0.054 (0.870)	1.290*** (0.482)	-1.069 (0.763)	-0.093 (0.798)	0.540 (0.781)
Observations	13,472	8,413	8,113	10,951	4,244	10,567	6,706
SAT participation	-0.010* (0.006)	-0.007 (0.006)	0.002 (0.008)	-0.008* (0.005)	-0.002 (0.003)	-0.001 (0.008)	0.009 (0.007)
Observations	15,975	12,372	12,444	15,502	10,559	15,967	13,975
<i>Panel B: ACT</i>							
ACT composite	0.114*** (0.042)	0.118*** (0.043)	0.094 (0.071)	0.088** (0.041)	0.058 (0.061)	0.090 (0.059)	0.130*** (0.045)
Observations	13,997	7,725	7,598	7,698	3,390	10,923	6,232
ACT participation	-0.008 (0.008)	0.002 (0.008)	-0.004 (0.011)	-0.021*** (0.007)	0.004 (0.010)	0.005 (0.010)	0.000 (0.010)
Observations	15,975	12,372	12,444	15,502	10,559	15,967	13,975

* Notes: This table reports estimates of the effect of the number of tutoring centers within two miles of a campus on school-level SAT and ACT outcomes among graduates of Texas public high schools in the 2011 to 2020 graduation cohorts, from data published by the Texas Education Agency. The regression specifications are similar to those in the main analysis, including school fixed effects, county-by-cohort fixed effects, and time-varying school covariates. Column 1 uses all graduates; columns 2 and 3 split by economic status; columns 4 through 7 restrict to the indicated racial or ethnic group. Participation rates range from zero to one. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effects of Private Tutoring Centers on Additional College Outcomes

	All	Non-economically disadvantaged
<i>Panel A: College application and enrollment</i>		
Any in-state four-year college application	-0.0054*** (0.0013)	-0.0046*** (0.0012)
Any in-state four-year college admission	-0.0042*** (0.0011)	-0.0038*** (0.0011)
Any in-state Top Ten Percent admission	-0.0015*** (0.0004)	-0.0020*** (0.0007)
Any in-state college enrollment	-0.0038*** (0.0011)	-0.0040*** (0.0012)
Any out-of-state college enrollment	0.0017*** (0.0006)	0.0033*** (0.0009)
<i>Panel B: Financial aid</i>		
Received any merit-based aid	0.0011 (0.0008)	-0.0002 (0.0008)
Received any need-based aid	-0.0003 (0.0009)	-0.0004 (0.0009)
<i>Panel C: Quality of degree obtained</i>		
<i>Conditional on getting an in-state college degree</i>		
Earned a STEM degree	0.0009 (0.0009)	0.0011 (0.0011)
Earned a degree from an in-state flagship university	0.0029** (0.0014)	0.0038** (0.0016)
Average earnings of graduates from the same institution	24.0 (24.4)	44.3* (23.5)
Average earnings of graduates from the same institution and major	92.5*** (34.9)	87.6** (41.8)

* Notes: This table reports estimates of the effect of one additional tutoring center within two miles of a student's ninth-grade campus on additional college outcomes, using the main specification. Panel C restricts the sample to students who earn a degree from an in-state institution. Flagship universities are the University of Texas at Austin and Texas A&M University. The earnings outcomes in Panel C assign each graduate the mean earnings of all graduates from the same institution, or from the same institution and major, measured 12 to 15 years after ninth-grade enrollment and deflated to real 2015 dollars. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effects of Private Tutoring Centers on Sandwich Earnings

	All	Non-economically disadvantaged
<i>Full sample</i>		
Has positive earnings	0.0004 (0.0015)	0.0014 (0.0018)
Imputed earnings	53.9 (47.5)	137.7** (67.8)
Observations	3,470,504	1,537,186
<i>Conditional on positive earnings</i>		
Average earnings	123.7* (68.3)	236.5*** (86.4)
Log earnings	-0.0003 (0.0015)	0.0027 (0.0017)
Observations	2,438,354	1,080,708

* Notes: This table replicates Table 6 using sandwich earnings, which keep only quarters of positive earnings that are immediately preceded and followed by quarters with positive earnings. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Alternative Treatment Definition: Ninth-Grade Exposure

	All	Non-econ. disadv.	Econ. disadv.
<i>Panel A: High school outcomes</i>			
State standardized test: math	-0.0024 (0.0036)	-0.0005 (0.0032)	-0.0038 (0.0039)
State standardized test: reading	-0.0001 (0.0025)	0.0042 (0.0030)	-0.0029 (0.0024)
High school graduation (in 4 years)	-0.0008 (0.0007)	-0.0001 (0.0007)	0.0011 (0.0009)
<i>Panel B: College outcomes</i>			
SAT score	0.64* (0.37)	1.13*** (0.42)	0.17 (0.48)
ACT score	0.05*** (0.01)	0.04*** (0.01)	0.00 (0.02)
Average earnings of enrollees	36.2** (18.2)	57.5** (23.2)	-1.5 (19.7)
Elite college enrollment	0.0008 (0.0007)	0.0018* (0.0009)	-0.0005 (0.0007)
In-state four-year college enrollment	-0.0012* (0.0006)	-0.0013* (0.0008)	-0.0001 (0.0007)
Out-of-state four-year college enrollment	0.0008** (0.0004)	0.0017*** (0.0006)	-0.0001 (0.0002)
Any four-year college enrollment	-0.0003 (0.0007)	0.0004 (0.0010)	-0.0003 (0.0007)
Any in-state bachelor's degree	-0.0007 (0.0006)	-0.0007 (0.0009)	-0.0000 (0.0006)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>			
Average earnings	78.2* (42.3)	150.0*** (53.9)	-38.3 (43.8)
Log earnings	0.0004 (0.0012)	0.0023 (0.0014)	-0.0018 (0.0019)

* Notes: This table replicates Tables 4 through 6 using an alternative treatment definition: the number of tutoring centers within two miles of a student's ninth-grade campus in the ninth-grade year alone. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Sorting of Private Tutoring Centers and Pet Care Services on Student Characteristics

	Private tutoring centers	Pet care services
White	0.0002 (0.0001)	-0.0001* (0.0001)
Asian	0.0147*** (0.0001)	0.0044*** (0.0000)
Hispanic	-0.0230*** (0.0001)	-0.0068*** (0.0001)
Black	0.0063*** (0.0001)	0.0019*** (0.0001)
Economically disadvantaged	-0.0379*** (0.0001)	-0.0138*** (0.0001)
At-risk	-0.0202*** (0.0001)	-0.0080*** (0.0001)
Gifted	0.0132*** (0.0001)	0.0054*** (0.0001)
Eighth-grade reading score	0.0638*** (0.0003)	0.0225*** (0.0002)
Eighth-grade math score	0.0684*** (0.0004)	0.0220*** (0.0002)
Observations	5,474,264	5,474,264
Mean establishments within two miles	0.8	2.7

* Notes: This table reports estimates from regressing student characteristics on the number of establishments within two miles of a student's ninth-grade campus. Each cell reports the coefficient from a separate regression. Column 1 uses private tutoring centers and column 2 uses pet care establishments. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Placebo Tests: Effects of Pet Care Exposure

	Pet care alone		Pet care and tutoring jointly			
	All	Non-econ. disadv.	All		Non-econ. disadv.	
	Pet care	Pet care	Tutoring	Pet care	Tutoring	Pet care
<i>Panel A: High school outcomes</i>						
State standardized test: math	-0.0043* (0.0023)	-0.0041 (0.0027)	-0.0028 (0.0040)	-0.0042* (0.0023)	0.0006 (0.0036)	-0.0042 (0.0028)
State standardized test: reading	-0.0028 (0.0018)	-0.0053** (0.0023)	-0.0001 (0.0030)	-0.0028 (0.0018)	0.0042 (0.0039)	-0.0055** (0.0023)
High school graduation (in 4 years)	-0.0006 (0.0007)	-0.0010** (0.0005)	-0.0011 (0.0012)	-0.0004 (0.0007)	0.0001 (0.0010)	-0.0009** (0.0005)
<i>Panel B: College outcomes</i>						
SAT score	-0.14 (0.31)	0.29 (0.35)	0.67 (0.47)	-0.19 (0.31)	1.64*** (0.51)	0.16 (0.33)
ACT score	0.03** (0.01)	0.03*** (0.01)	0.07*** (0.02)	0.02** (0.01)	0.07*** (0.02)	0.02** (0.01)
Average earnings of enrollees	12.9 (17.6)	26.7 (21.2)	34.7 (24.3)	10.2 (17.3)	57.4* (30.7)	21.3 (20.9)
Elite college enrollment	-0.0002 (0.0003)	0.0001 (0.0005)	0.0011* (0.0006)	-0.0002 (0.0003)	0.0020*** (0.0007)	0.0000 (0.0005)
In-state four-year college enrollment	0.0000 (0.0005)	-0.0003 (0.0007)	-0.0020** (0.0008)	0.0002 (0.0005)	-0.0026** (0.0011)	-0.0000 (0.0007)
Out-of-state four-year college enrollment	0.0002 (0.0004)	0.0001 (0.0006)	0.0019*** (0.0006)	0.0001 (0.0004)	0.0035*** (0.0009)	-0.0002 (0.0006)
Any four-year college enrollment	0.0003 (0.0006)	-0.0002 (0.0009)	-0.0002 (0.0008)	0.0003 (0.0006)	0.0009 (0.0013)	-0.0003 (0.0009)
Any in-state bachelor's degree	-0.0000 (0.0004)	-0.0002 (0.0007)	-0.0015** (0.0007)	0.0001 (0.0004)	-0.0023** (0.0010)	-0.0000 (0.0007)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>						
Average earnings	19.0 (29.3)	37.7 (43.7)	121.0* (68.1)	14.6 (28.6)	237.7*** (86.8)	27.6 (41.7)
Log earnings	-0.0001 (0.0009)	-0.0002 (0.0012)	0.0004 (0.0018)	-0.0001 (0.0009)	0.0047** (0.0020)	-0.0004 (0.0012)

* Notes: This table reports placebo estimates using an identically constructed measure of exposure to pet care establishments (NAICS code 812910). Columns 1 and 2 replace tutoring exposure in the main specification with pet care exposure. Columns 3 through 6 add pet care exposure to the main specification; within each sample, the two columns report the coefficients on tutoring exposure and pet care exposure from a single regression, so each is estimated conditional on the other. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Heterogeneity: Effects by Student Subgroup

	Gender		Race/ethnicity				Other status			
	Female	Male	White	Asian	Hispanic	Black	Not gifted	Gifted	Not at-risk	At-risk
<i>Panel A: High school outcomes</i>										
State standardized test: math	-0.0038 (0.0039)	-0.0022 (0.0043)	-0.0014 (0.0038)	0.0093 (0.0058)	-0.0079 (0.0049)	0.0002 (0.0041)	-0.0020 (0.0038)	-0.0065 (0.0051)	-0.0011 (0.0038)	-0.0034 (0.0039)
State standardized test: reading	-0.0016 (0.0033)	0.0012 (0.0031)	0.0020 (0.0038)	0.0069 (0.0056)	-0.0019 (0.0033)	-0.0035 (0.0039)	-0.0002 (0.0030)	0.0009 (0.0052)	0.0035 (0.0040)	-0.0012 (0.0028)
High school graduation (in 4 years)	-0.0020* (0.0010)	-0.0007 (0.0012)	-0.0010 (0.0009)	0.0038* (0.0022)	0.0010 (0.0014)	0.0008 (0.0019)	-0.0015 (0.0011)	-0.0001 (0.0012)	-0.0005 (0.0010)	0.0002 (0.0013)
<i>Panel B: College outcomes</i>										
SAT score	0.48 (0.52)	0.81 (0.51)	1.69*** (0.53)	0.14 (0.83)	0.30 (0.72)	-1.13 (0.80)	0.52 (0.50)	1.10 (0.73)	1.58*** (0.51)	0.68 (0.59)
ACT score	0.08*** (0.02)	0.07*** (0.02)	0.06*** (0.02)	0.06* (0.03)	0.03 (0.03)	0.02 (0.02)	0.08*** (0.02)	0.06** (0.02)	0.08*** (0.02)	0.05** (0.02)
Average earnings of enrollees	29.4 (26.6)	43.6 (27.6)	65.0* (34.7)	-46.0 (51.4)	36.3 (30.5)	-49.2 (38.9)	49.0** (24.0)	24.1 (37.3)	58.3* (32.2)	46.6* (26.1)
Elite college enrollment	0.0007 (0.0007)	0.0014** (0.0007)	0.0021** (0.0008)	0.0016 (0.0017)	0.0002 (0.0007)	-0.0010 (0.0006)	0.0007 (0.0005)	0.0026** (0.0013)	0.0022*** (0.0008)	0.0003 (0.0005)
In-state four-year college enrollment	-0.0035*** (0.0010)	-0.0006 (0.0008)	-0.0025** (0.0012)	-0.0008 (0.0021)	-0.0003 (0.0009)	-0.0003 (0.0014)	-0.0015** (0.0008)	-0.0045*** (0.0015)	-0.0031*** (0.0012)	0.0000 (0.0007)
Out-of-state four-year college enrollment	0.0024*** (0.0007)	0.0014** (0.0006)	0.0042*** (0.0010)	0.0001 (0.0010)	0.0007** (0.0003)	-0.0005 (0.0007)	0.0020*** (0.0006)	0.0017* (0.0010)	0.0028*** (0.0009)	0.0012*** (0.0004)
Any four-year college enrollment	-0.0010 (0.0010)	0.0008 (0.0009)	0.0017 (0.0014)	-0.0007 (0.0022)	0.0005 (0.0010)	-0.0008 (0.0015)	0.0004 (0.0009)	-0.0028* (0.0016)	-0.0003 (0.0014)	0.0013 (0.0009)
Any in-state bachelor's degree	-0.0037*** (0.0010)	0.0007 (0.0006)	-0.0016* (0.0010)	-0.0011 (0.0019)	0.0013 (0.0009)	-0.0022** (0.0011)	-0.0008 (0.0007)	-0.0045*** (0.0016)	-0.0029*** (0.0011)	0.0010 (0.0007)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>										
Average earnings	55.2 (70.7)	184.6** (85.9)	197.4** (98.9)	-37.9 (205.3)	60.3 (66.7)	70.2 (90.3)	101.6 (69.8)	153.1 (134.3)	304.1*** (100.9)	55.0 (60.9)
Log earnings	-0.0023 (0.0023)	0.0031 (0.0022)	0.0028 (0.0024)	0.0063 (0.0067)	-0.0012 (0.0023)	0.0005 (0.0046)	-0.0003 (0.0019)	0.0042 (0.0034)	0.0059** (0.0024)	-0.0016 (0.0025)

* Notes: This table reports the estimates from the main specification by student subgroup. Subgroups are defined by gender, race and ethnicity, gifted program participation, and at-risk of dropping out status, all measured before ninth-grade enrollment. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Heterogeneity: Effects by School-Level Economic Composition

	Below-median disadv. share			Above-median disadv. share		
	All	Non-econ. disadv.	Econ. disadv.	All	Non-econ. disadv.	Econ. disadv.
<i>Panel A: High school outcomes</i>						
State standardized test: math	0.0021 (0.0039)	0.0009 (0.0038)	0.0029 (0.0039)	-0.0083 (0.0080)	-0.0049 (0.0084)	-0.0075 (0.0078)
State standardized test: reading	0.0010 (0.0038)	0.0016 (0.0044)	-0.0010 (0.0036)	-0.0010 (0.0054)	0.0064 (0.0069)	-0.0012 (0.0052)
High school graduation (in 4 years)	0.0005 (0.0013)	0.0007 (0.0011)	0.0036** (0.0017)	0.0006 (0.0017)	0.0006 (0.0030)	0.0010 (0.0017)
<i>Panel B: College outcomes</i>						
SAT score	1.72*** (0.52)	1.94*** (0.58)	1.79*** (0.66)	-1.76 (1.07)	-0.40 (1.46)	-1.50 (1.07)
ACT score	0.06*** (0.02)	0.05*** (0.02)	0.02 (0.02)	0.02 (0.03)	0.10* (0.06)	-0.04 (0.03)
Average earnings of enrollees	32.7 (28.2)	57.9* (32.9)	-23.4 (35.4)	-26.1 (47.9)	-58.9 (88.3)	-41.9 (43.8)
Elite college enrollment	0.0013** (0.0007)	0.0022*** (0.0007)	-0.0008 (0.0007)	0.0000 (0.0011)	-0.0012 (0.0023)	0.0000 (0.0009)
In-state four-year college enrollment	-0.0025** (0.0010)	-0.0019 (0.0012)	-0.0005 (0.0010)	-0.0010 (0.0014)	-0.0050* (0.0027)	-0.0006 (0.0013)
Out-of-state four-year college enrollment	0.0022*** (0.0007)	0.0032*** (0.0010)	0.0001 (0.0004)	0.0002 (0.0004)	0.0020** (0.0009)	-0.0001 (0.0004)
Any four-year college enrollment	-0.0002 (0.0011)	0.0013 (0.0014)	-0.0004 (0.0011)	-0.0008 (0.0014)	-0.0030 (0.0028)	-0.0007 (0.0014)
Any in-state bachelor's degree	-0.0016* (0.0009)	-0.0021* (0.0011)	0.0011 (0.0008)	-0.0006 (0.0013)	-0.0004 (0.0021)	-0.0005 (0.0013)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>						
Average earnings	184.5** (80.9)	284.9*** (90.3)	-51.8 (86.4)	-138.4* (79.8)	-169.8 (185.8)	-174.1** (77.9)
Log earnings	0.0023 (0.0021)	0.0058*** (0.0020)	-0.0034 (0.0038)	-0.0061* (0.0031)	-0.0041 (0.0064)	-0.0092** (0.0036)

* Notes: This table reports estimates from the main specification, splitting schools at the median share of economically disadvantaged students, measured over the earliest observed cohorts and held fixed. Columns 1 through 3 restrict to schools below the median; columns 4 through 6 restrict to schools above the median. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Heterogeneity: Effects by Initial Tutoring Center Presence

	No initial center		At least one initial center	
	All	Non-econ. disadv.	All	Non-econ. disadv.
<i>Panel A: High school outcomes</i>				
State standardized test: math	0.0017 (0.0076)	0.0047 (0.0088)	-0.0040 (0.0044)	-0.0002 (0.0036)
State standardized test: reading	0.0001 (0.0051)	0.0055 (0.0068)	-0.0018 (0.0033)	0.0017 (0.0042)
High school graduation (in 4 years)	-0.0039*** (0.0015)	-0.0024** (0.0012)	-0.0011 (0.0013)	-0.0003 (0.0012)
<i>Panel B: College outcomes</i>				
SAT score	-0.31 (0.98)	1.49 (0.96)	0.85 (0.54)	1.58*** (0.57)
ACT score	0.06* (0.03)	0.09*** (0.03)	0.07*** (0.02)	0.06*** (0.02)
Average earnings of enrollees	51.1 (50.7)	54.8 (47.4)	8.3 (28.1)	41.1 (37.9)
Elite college enrollment	-0.0007 (0.0014)	0.0006 (0.0014)	0.0012** (0.0006)	0.0025*** (0.0008)
In-state four-year college enrollment	-0.0010 (0.0016)	-0.0034* (0.0018)	-0.0025*** (0.0009)	-0.0025** (0.0012)
Out-of-state four-year college enrollment	0.0007 (0.0011)	0.0012 (0.0015)	0.0022*** (0.0007)	0.0034*** (0.0009)
Any four-year college enrollment	-0.0003 (0.0014)	-0.0023 (0.0020)	-0.0003 (0.0009)	0.0009 (0.0013)
Any in-state bachelor's degree	-0.0025* (0.0015)	-0.0033 (0.0021)	-0.0016** (0.0008)	-0.0026** (0.0011)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>				
Average earnings	-47.1 (115.1)	70.1 (144.0)	147.8* (84.7)	284.4*** (107.6)
Log earnings	-0.0052 (0.0038)	-0.0005 (0.0044)	0.0011 (0.0021)	0.0061*** (0.0023)

* Notes: This table reports estimates from the main specification, splitting schools by the number of tutoring centers within two miles at the beginning of the sample period. Columns 1 and 2 restrict to schools with no tutoring center; columns 3 and 4 restrict to schools with at least one. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Heterogeneity: Effects by Type of Tutoring Business

	All		Non-econ. disadv.	
	Chain	Independent	Chain	Independent
<i>Panel A: High school outcomes</i>				
State standardized test: math	-0.0077 (0.0061)	0.0002 (0.0060)	0.0023 (0.0059)	-0.0014 (0.0051)
State standardized test: reading	-0.0032 (0.0049)	0.0018 (0.0042)	0.0037 (0.0054)	0.0039 (0.0055)
High school graduation (in 4 years)	-0.0033** (0.0013)	0.0009 (0.0019)	-0.0025** (0.0011)	0.0026 (0.0021)
<i>Panel B: College outcomes</i>				
SAT score	0.53 (0.80)	0.78 (0.74)	2.16*** (0.80)	1.03 (0.79)
ACT score	0.10*** (0.03)	0.05* (0.03)	0.09*** (0.03)	0.04 (0.03)
Average earnings of enrollees	81.7** (35.9)	-18.5 (35.5)	102.3** (42.2)	3.0 (50.8)
Elite college enrollment	0.0015* (0.0009)	0.0006 (0.0008)	0.0030** (0.0012)	0.0008 (0.0011)
In-state four-year college enrollment	-0.0021* (0.0013)	-0.0019 (0.0012)	-0.0026 (0.0016)	-0.0026 (0.0018)
Out-of-state four-year college enrollment	0.0017* (0.0009)	0.0021*** (0.0007)	0.0029** (0.0013)	0.0041*** (0.0015)
Any four-year college enrollment	-0.0004 (0.0014)	0.0002 (0.0013)	0.0004 (0.0019)	0.0015 (0.0021)
Any in-state bachelor's degree	-0.0021* (0.0012)	-0.0010 (0.0010)	-0.0026 (0.0016)	-0.0019 (0.0015)
<i>Panel C: Early-career earnings (conditional on positive earnings)</i>				
Average earnings	235.1** (94.6)	48.0 (88.2)	263.6** (110.2)	222.8* (118.3)
Log earnings	0.0046* (0.0027)	-0.0023 (0.0026)	0.0042 (0.0029)	0.0050* (0.0028)

* Notes: This table reports estimates from regressions that decompose the treatment into separate counts of chain and independent tutoring centers. Chains are defined as firms whose names appear more than five times in a single year. Within each sample, the two columns report the coefficients on chain and independent exposure from a single regression, so each is estimated conditional on the other. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Private Tutoring Participation and Spending from the Survey: High School Students

	(1) All	(2) Non-econ. disadv.	(3) Econ. disadv.	(4) <i>p</i> -value (2)–(3)
<i>Panel A: Participation</i>				
Paid private tutoring	0.24	0.28	0.12	0.002
Paid one-on-one tutoring	0.18	0.21	0.11	0.034
Paid tutoring at a tutoring center	0.09	0.11	0.03	0.018
Observations	373	229	144	
<i>Panel B: Spending, conditional on participation</i>				
Annual spending (\$)	2,939	3,266	846	<0.001
Hourly rate (\$)	52.5	57.2	24.3	<0.001
Hours per week	3.5	3.4	3.9	0.395
Observations	72	58	14	

Notes: This table replicates Table 9 for children in grades 9 through 12 and reports weighted means. Participation refers to paid private tutoring for which the household pays the full cost, and the participation categories are not mutually exclusive. Panel B restricts the sample to children who currently participate in paid private tutoring. Spending and hourly rates are computed from bracketed responses using bracket midpoints. Non-econ. disadv. and econ. disadv. divide households by economic disadvantage as defined in Table 8. Column (4) reports *p*-values from weighted regressions of each outcome on an indicator for economic disadvantage.

Table A15: Local Supply of Tutoring Centers and Tutoring Participation: 5-Mile Radius

	All		Non-econ. disadv.		Econ. disadv.	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>All students</i>						
Paid tutoring at a tutoring center	0.007*** (0.002)	0.005*** (0.002)	0.006*** (0.002)	0.006*** (0.002)	0.002 (0.001)	0.001 (0.001)
Paid one-on-one tutoring	0.000 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.004** (0.002)	0.005 (0.005)	0.005 (0.004)
Observations	1,142	1,142	674	674	468	468
<i>High school students</i>						
Paid tutoring at a tutoring center	0.007** (0.003)	0.006** (0.003)	0.007* (0.003)	0.007** (0.003)	0.004 (0.004)	0.004 (0.004)
Paid one-on-one tutoring	-0.004 (0.004)	-0.007 (0.005)	-0.006 (0.004)	-0.008 (0.005)	0.001 (0.005)	0.003 (0.006)
Observations	372	372	229	229	143	143
Demographic controls		✓		✓		✓
County FE	✓	✓	✓	✓	✓	✓

Notes: This table reports weighted estimates from regressions of tutoring participation on the number of tutoring centers within five miles of the centroid of the ZIP code of the child's school. Each row within a panel reports a separate regression. In the first row of each panel, the dependent variable is an indicator for currently receiving paid tutoring at a tutoring center. In the second row, it is an indicator for currently receiving paid one-on-one tutoring. The top panel uses the full sample, and the bottom panel restricts the sample to students in grades 9 through 12. Demographic controls include those reported in Table 8. Columns (3) through (6) divide the sample by economic disadvantage. Standard errors are clustered at the school ZIP code level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Data Appendix

The Texas ERC data are available between 1997 and 2024. The 1997 cohort is dropped because I include eighth-grade standardized test scores as a control variable in all regressions on post-ninth-grade outcomes. I further restrict the sample to cohorts projected to apply to college before the COVID-19 pandemic, which disrupted both the college application process and the SAT/ACT testing infrastructure.

Analysis sample.— The main analysis sample consists of Texas public high school students enrolled in ninth grade between 1998 and 2016. To construct this sample, I begin with the population of ninth-grade students in each cohort with valid student identifiers, covariates, and high school locations. For students observed in ninth grade in multiple years, I keep the first ninth-grade record. I then drop students who transfer out of the Texas public school system during high school, whether to a private school or to an out-of-state institution. For each outcome, I use the largest set of cohorts for which the outcome is fully observed. Below, I discuss the sample and definition for each outcome in more detail.

High school state standardized test scores.— High school standardized test scores come from the Texas Assessment of Knowledge and Skills (TAKS), administered from 2003 to 2011, which offers grade-level exams in ninth through eleventh grade. TAKS is the only statewide assessment regime in Texas that offers grade-level tests to all high school students. Its predecessor, the Texas Assessment of Academic Skills (TAAS), was administered in high school only as an exit exam, and its successor, the State of Texas Assessments of Academic Readiness (STAAR), consists of end-of-course exams that students take in varying grades depending on course enrollment. Roughly 30 percent of students, for example, take the STAAR Algebra I exam in eighth grade.

I use math and reading scores from the general administration of the test, excluding formats designed for students with disabilities. When a student retakes an exam, I keep the score from the first administration. Scores are standardized to have mean zero and unit variance within each subject, grade, and test year, so that estimates are interpretable relative to the statewide test-taking population in each cell.

High school graduation.— I construct two measures of high school completion: an indicator for graduating within four years of ninth-grade enrollment, and an indicator for graduating within five years, which captures the small additional share of students who complete high school after a brief delay. Students who do not appear in the TEA graduation files within the relevant window are coded as non-graduates.

College admission exam scores.— College admission exam scores come from the college application records that Texas public universities report to the THECB, which are available for ninth-grade cohorts from 1998 onward. The sample is restricted to students who submit at least one application to a bachelor's degree program at a Texas public four-year university in the application cycle exactly four years after ninth-grade enrollment, which corresponds to on-time application for students who progress through high school without delay. For each student, I take the maximum SAT score and the maximum ACT score across all applications submitted in that cy-

cle, retaining scores within the valid range of each exam, 400 to 1600 for the SAT and 1 to 36 for the ACT. For students with both, I convert the ACT score to the SAT scale using the official 2018 ACT/SAT concordance tables ([ACT, Inc. and The College Board, 2018](#)) and keep the higher of the two, so each student carries a single score, either SAT or ACT. The THECB data file records the SAT and ACT scores used in each admission decision rather than the scores from a single test sitting. Taking the maximum across applications yields the highest score the student was willing to send to any Texas public four-year university. The resulting measure can therefore be interpreted as an applicant's maximum college admission exam score.

College applications and admissions.— For the full eligible student sample, college application and admission outcomes measure applications to, and offers of admission from, Texas public four-year universities. I exclude transfer applications and drop applications for summer terms. All outcomes are measured in the application cycle exactly four years after ninth-grade enrollment. I construct various variables for analysis, including an indicator for submitting at least one application to any Texas public four-year university, an indicator for being admitted to at least one such institution, and an indicator for being admitted through the Texas Top Ten Percent admission policy, which guarantees admission to any Texas public university for students graduating in the top ten percent of their high school class. The threshold has been lowered for UT Austin in some years due to enrollment caps. I treat any student admitted under the prevailing automatic admission rule as a top-percent admit.

College enrollment.— I combine in-state enrollment records from the THECB with out-of-state records from the NSC, which covers Texas public high school students in the 2008 through 2019 graduation cohorts; the enrollment sample is therefore restricted to ninth-grade cohorts whose projected graduation falls within this window. I drop enrollment records dated before high school graduation and records for summer terms. The primary college enrollment outcome is an indicator for enrolling in any college in the fall semester four years after ninth-grade enrollment. I anchor the enrollment window to ninth-grade enrollment rather than to high school graduation because high school graduation is itself an endogenous outcome. When a student holds multiple enrollment records in that semester, I keep the record at the larger institution, measured by the number of students in my analysis sample enrolled at the institution in the relevant school year. I also construct separate indicators for two-year and four-year enrollment and for in-state and out-of-state enrollment.

In addition, I construct two measures of the quality of the enrolled college. The first is an indicator for enrolling at an elite institution, defined as those rated “most competitive” (Tier 1) by the 2009 Barron's selectivity index ([Barron's Educational Series, 2009](#)), together with the University of Texas at Austin and Texas A&M University, the two public flagship universities in Texas. The second is the average earnings of a college's enrollees. I construct this measure at the institution level using the cohorts for which I observe both college enrollment and earnings 12 to 15 years after ninth-grade enrollment, with earnings processed as described under *Earnings* below, assigning each institution the average earnings of the sampled students who enroll there. In the regression

analysis, I use a leave-one-out version of this measure, defining the earnings of each student's college using all sampled enrollees other than the student, so that a student's own earnings do not enter the outcome assigned to them.

Financial aid.— Financial aid outcomes come from the Financial Aid Database (FAD) reports that Texas institutions of higher education submit annually to the THECB. The analysis sample includes the 2006 to 2016 ninth-grade cohorts. The reporting universe consists of students who were enrolled at a reporting institution and completed a Free Application for Federal Student Aid (FAFSA) or Texas Application for State Financial Aid (TASFA), whether or not they received aid, together with enrolled students who received aid, such as merit scholarships or tuition exemptions, without completing either application.

Reporting is required of all Texas public institutions and of private institutions participating in state aid programs. Because the FAD covers only Texas institutions, students who enroll out of state do not appear in the file, and both financial aid outcomes carry the interpretation of enrolling in state and receiving the relevant form of aid. Both outcomes are measured in the academic year beginning four years after ninth-grade enrollment, which corresponds to the first year of college for students who progress through high school and enter college without delay, and are defined over the full ninth-grade analysis sample, so that students who do not appear in the FAD are coded as zeros rather than excluded.

The first outcome is an indicator for receiving any need-based grant aid, defined as a positive sum across the federal Pell Grant, the Supplemental Educational Opportunity Grant, the Texas Public Educational Grant, the TEXAS Grant, the Texas Educational Opportunity Grant, the Tuition Equalization Grant, the federal-state partnership grants, and other need-based grants and scholarships. The federal-state partnership programs were renamed twice over the sample period, from the State Student Incentive Grant to the Leveraging Educational Assistance Partnership and its supplemental counterpart; I sum across the corresponding fields, which are populated in non-overlapping years. Program-level fields associated with discontinued legacy programs that are never positive during the sample period are excluded.

The second is an indicator for receiving any merit-based aid, defined as a positive sum of merit-based gift aid funded by private donations to the institution, merit-based gift aid funded by the institution, and the federal Byrd Honors Scholarship. Merit-based gift aid in the FAD comprises awards made on the basis of academic performance, talent, or athletics rather than financial need. Tuition exemptions and waivers, which are category-based entitlements such as veterans' benefits, and categorical aid, which consists of outside funds not packaged by the institution's financial aid office, are excluded from both measures because neither can be assigned to a need or merit basis.

College degree completion.— I construct indicators for completing any college degree, a bachelor's degree, and an associate degree; degrees above the bachelor's level are excluded. A degree is counted if completed within six years of projected high school graduation, that is, within ten years of ninth-grade enrollment. I anchor the window to projected rather than actual graduation because graduation timing is itself an endogenous outcome. When a student completes multiple

degrees within the window, I keep one record per student, prioritizing the bachelor's degree over the associate degree, so the associate indicator identifies students whose highest degree within the window is an associate degree.

I classify college majors as STEM or non-STEM using the 2024 STEM Designated Degree Program List published by the U.S. Department of Homeland Security, which enumerates the fields of study that qualify for the STEM extension of the Optional Practical Training program. The list identifies STEM fields at the six-digit level of the Classification of Instructional Programs (CIP), the taxonomy in which the THECB records report each student's program of study.

Earnings.— I measure earnings using TWC quarterly wage records. For each individual, I sum within-quarter earnings across employers, deflate by the quarterly U.S. consumer price index to real 2015 dollars, and winsorize at the 99th percentile. The main earnings outcome averages the non-missing quarters within person over the period 12 to 15 calendar years after ninth-grade enrollment, which corresponds to ages 26 to 29 for students who enter ninth grade at typical ages. The TWC data do not distinguish between non-employment in Texas and employment outside Texas, so a missing earnings record may reflect either.

For this outcome, I restrict the sample to cohorts with complete earnings records over the full window. I keep the full ninth-grade analysis sample regardless of educational attainment, so that the earnings estimates capture the combined effect of tutoring center exposure on labor market outcomes through any pathway, including direct labor force entry, two-year college, and four-year college.

To supplement the main measure, I construct “sandwich” earnings, a measure common in analyses of quarterly state earnings data ([Sorkin, 2018](#); [Card, Rothstein and Yi, 2025](#); [Mountjoy, 2026](#)), which retains person-quarters with positive earnings that are immediately preceded and followed by quarters with positive earnings. This restriction discards the high-variance transition quarters between employment spells, so the measure approximates a wage rate under continuous labor market attachment rather than a mixture of wages and employment gaps.

To trace the dynamics of the earnings effects, I also estimate effects on earnings separately for each year after on-time high school graduation. For these year-by-year estimates, I use the largest set of cohorts for which earnings are observed in the relevant year, so the sample is larger at short horizons than at long ones. Effects two years after on-time graduation, for example, draw on more cohorts than effects at ten years.

C Survey Questions

Screener

1. In which state do you currently live?
 - ☐ Texas
 - ☐ Another U.S. state
 - ☐ Outside the United States
2. Are you the parent or guardian of (at least) a child who currently lives with you?
 - ☐ Yes
 - ☐ No
3. How many children in your household are currently in school in kindergarten through 12th grade (K–12)?
 - ☐ 0
 - ☐ 1
 - ☐ 2
 - ☐ 3
 - ☐ 4 or more

If you have more than one child currently enrolled in K–12, please answer the remaining questions about the child who had a birthday most recently. By this we mean the child whose last birthday fell nearest to today, which is not the same as your youngest child. We refer to this child as the “selected child” throughout the survey.

Child’s Education

1. What is your relationship to the selected child?
 - ☐ Mother
 - ☐ Father
 - ☐ Grandparent
 - ☐ Other relative
 - ☐ Other guardian or caretaker
2. What is the selected child’s biological sex?
 - ☐ Female
 - ☐ Male
3. What is the selected child’s race or ethnicity? [Select all that apply.]

- ☐ White
 - ☐ Black or African American
 - ☐ Hispanic, Latino, or Spanish origin
 - ☐ Asian
 - ☐ American Indian or Alaska Native
 - ☐ Native Hawaiian or Other Pacific Islander
 - ☐ Some other race or ethnicity
4. What grade is the selected child currently (2025-2026) in?
- ☐ Kindergarten
 - ☐ 1st grade
 - ☐ 2nd grade
 - ☐ 3rd grade
 - ☐ 4th grade
 - ☐ 5th grade
 - ☐ 6th grade
 - ☐ 7th grade
 - ☐ 8th grade
 - ☐ 9th grade
 - ☐ 10th grade
 - ☐ 11th grade
 - ☐ 12th grade
5. What type of school does the selected child currently attend?
- ☐ Traditional public school
 - ☐ Public charter school
 - ☐ Private school
 - ☐ Homeschool
 - ☐ Other
6. During the current school year (2025-2026), is the selected child eligible for free or reduced-price school meals?
- ☐ Yes
 - ☐ No
7. During the current school year (2025-2026), does the selected child receive or participate in any of the following programs or services? [Select all that apply.]

- ☐ Special education
- ☐ Gifted and talented program
- ☐ Bilingual or English learner
- ☐ Dyslexia services
- ☐ At-risk of dropping out
- ☐ None of the above

Private Tutoring Participation

The next questions ask about private tutoring the selected child may have received during the current school year. By “private tutoring,” we mean academic instruction provided outside of regular school hours by a private company, a tutoring center, or an individual tutor hired by your household. Please do not include free tutoring offered by the selected child’s school or district, and please do not include homework help provided by family members.

1. During the current school year (2025–26), has the selected child received any private tutoring outside of regular school hours?
 - ☐ Yes
 - ☐ No
2. For each of the following, please indicate whether the selected child received any private tutoring. If the selected child was too young to be enrolled in school during any of these periods, mark “Not applicable.”

	Yes	No	Not applicable
Previous school year (2024–25)	☐	☐	☐
Two school years ago (2023–24)	☐	☐	☐
Three school years ago (2022–23)	☐	☐	☐
Ever in an earlier school year before 2022–23	☐	☐	☐

3. What type(s) of private tutoring does the selected child currently receive? [Select all that apply.]
 - ☐ Tutoring at a tutoring center or company location (e.g., Kumon, Sylvan, Mathnasium, Huntington, or a similar business)
 - ☐ One-on-one tutoring from an individual tutor hired by your household
 - ☐ Small-group tutoring
 - ☐ Online tutoring through a commercial platform or website
 - ☐ Other

4. Who pays for the selected child's private tutoring? [Select all that apply.]

- ☐ Our household pays the full cost
- ☐ Our household pays part of the cost, with help from a scholarship, discount, or subsidy
- ☐ The tutoring is fully covered by a scholarship, voucher, school program, or other source at no cost to us
- ☐ Not sure

5. In which subject(s) does the selected child currently receive private tutoring? [Select all that apply.]

- ☐ Mathematics
- ☐ Science
- ☐ English language arts or reading
- ☐ Social studies or history
- ☐ Writing
- ☐ Foreign language
- ☐ General academic enrichment (not subject-specific)
- ☐ Other

6. Does the selected child currently receive private tutoring specifically for preparation for any of the following? [Select all that apply.]

- ☐ STAAR or other state standardized tests
- ☐ SAT or ACT
- ☐ PSAT or NMSQT
- ☐ AP or IB
- ☐ College preparation or counseling services
- ☐ None of the above

7. In which grade did the selected child first begin receiving any form of private tutoring?

- ☐ Before kindergarten
 - ☐ Kindergarten
 - ☐ 1st grade
 - ☐ 2nd grade
 - ☐ 3rd grade
 - ☐ 4th grade
 - ☐ 5th grade
 - ☐ 6th grade
 - ☐ 7th grade
 - ☐ 8th grade
 - ☐ 9th grade
 - ☐ 10th grade
 - ☐ 11th grade
 - ☐ 12th grade
 - ☐ Not sure
8. On average, how many hours per week does the selected child currently spend in private tutoring?
- ☐ Less than 1 hour per week
 - ☐ 1–2 hours per week
 - ☐ 3–4 hours per week
 - ☐ 5–6 hours per week
 - ☐ More than 6 hours per week
9. Which of the following reasons explain why you enrolled the selected child in private tutoring? [Select all that apply.]
- ☐ The selected child was struggling in one or more academic subjects
 - ☐ To prepare the selected child for a specific standardized test (e.g., STAAR, SAT, AP)
 - ☐ To strengthen the selected child's chances of admission to a competitive college
 - ☐ A teacher or school staff member recommended it
 - ☐ Other parents or families in our community enroll their children in tutoring
 - ☐ A convenient tutoring provider opened or became available in our area
 - ☐ The academic support at the selected child's school was not sufficient
 - ☐ Other
10. During the current school year, approximately how much has your household spent in total on private tutoring for the selected child?

- ☐ Less than \$500
 - ☐ \$500 to \$999
 - ☐ \$1,000 to \$2,499
 - ☐ \$2,500 to \$4,999
 - ☐ \$5,000 to \$9,999
 - ☐ \$10,000 or more
 - ☐ Not sure
11. During the current school year, approximately what is the hourly rate your household pays for private tutoring?
- ☐ Less than \$25 per hour
 - ☐ \$25 to \$49 per hour
 - ☐ \$50 to \$74 per hour
 - ☐ \$75 to \$99 per hour
 - ☐ \$100 or more per hour
 - ☐ Not sure
12. Approximately how long does it take for the selected child to travel from home to the tutoring location (one way)?
- ☐ Less than 10 minutes
 - ☐ 10–20 minutes
 - ☐ 20–30 minutes
 - ☐ 30–45 minutes
 - ☐ More than 45 minutes
 - ☐ Not applicable (online tutoring only)
13. [If the selected child does not currently receive private tutoring] Which of the following reasons explain why the selected child is not currently receiving private tutoring? [Select all that apply.]
- ☐ The selected child does not need tutoring
 - ☐ The cost of private tutoring is too high
 - ☐ My household's schedule does not allow time for tutoring
 - ☐ Transportation to a tutoring provider is a barrier
 - ☐ I am not aware of tutoring options near my home or the selected child's school
 - ☐ I provide academic help at home
 - ☐ The selected child receives sufficient academic support at school
 - ☐ Other
14. To the best of your knowledge, approximately how many private tutoring centers are currently located within a 10-minute drive of the selected child's school?

- ☐ None
- ☐ 1
- ☐ 2
- ☐ 3
- ☐ 4
- ☐ 5 or more
- ☐ Not sure

15. How much do you agree or disagree with the following statement? Since the selected child began receiving private tutoring, the selected child's academic performance has improved.

- ☐ Strongly agree
- ☐ Somewhat agree
- ☐ Neither agree nor disagree
- ☐ Somewhat disagree
- ☐ Strongly disagree

Household Characteristics

1. What is the highest degree or level of schooling that you completed?

- ☐ Less than high school diploma
- ☐ High school diploma or equivalent (e.g., GED)
- ☐ Some college, no degree
- ☐ Associate degree
- ☐ Bachelor's degree
- ☐ Post-baccalaureate degree (e.g., Master's, JD, MD, PhD)

2. How would you describe the area where you currently live?

- ☐ Urban (in a large city)
- ☐ Suburban (outside a large city but not rural)
- ☐ Rural (in the countryside or a small town)

3. What was the total income of your household during the past 12 months, before taxes? Include income from all sources and all household members.

- ☐ Less than \$15,000
- ☐ \$15,000 to \$24,999
- ☐ \$25,000 to \$34,999
- ☐ \$35,000 to \$49,999
- ☐ \$50,000 to \$74,999
- ☐ \$75,000 to \$99,999
- ☐ \$100,000 to \$149,999
- ☐ \$150,000 to \$199,999
- ☐ \$200,000 or more
- ☐ Prefer not to answer

4. What is the ZIP code of your home address?

ZIP code: _____

5. What is the zip code of the school the selected child currently attends?

ZIP code: _____

6. Approximately how far is your home from the selected child's school?

- ☐ Within 1 mile
- ☐ Between 1 and 2 miles
- ☐ Between 2 and 5 miles
- ☐ Between 5 and 10 miles
- ☐ Over 10 miles

You are invited to participate in a research study conducted by Marty Haoyuan Chen at the University of Maryland, College Park.

Purpose. This research study examines how families in Texas make decisions for their school-age children. The findings will contribute to academic research on educational opportunity and household investment in children's schooling.

Procedures. If you agree to participate, you will complete a single online survey lasting approximately 5-10 minutes. The survey asks about your child's academic activities, and your child's participation in off-campus learning. There are no follow-up surveys, interviews, or additional tasks. No audio or video recording is involved.

Voluntary participation. Your participation is entirely voluntary. You are not expected to skip any question. You may end your participation at any time without penalty or loss of benefits to which you are otherwise entitled.

Risks and benefits. There are no known risks associated with participation in this study, and there are no direct benefits to you from participating.

Confidentiality. No personally identifying information will be collected. If we write a report or article about this research project, your identity will be protected to the maximum extent possible. Your information may be shared with representatives of the University of Maryland, College Park or governmental authorities if you or someone else is in danger or if we are required to do so by law.

Questions. If you have questions about this study, please contact Marty Haoyuan Chen at hchen915@umd.edu. If you have questions about your rights as a research participant, please contact the Institutional Review Board at the University of Maryland, College Park at irb@umd.edu or 301-405-0678. Please reference IRB Package KIRB-0831-1 when contacting the IRB about this study.

Statement of consent. By clicking "Next page", you indicate that you are at least 18 years of age; you have read this consent form or have had it read to you; your questions have been answered to your satisfaction and you voluntarily agree to participate in this research study. You may print, download, or save a copy of this consent form for your records before proceeding.

Next page >

Figure C1: Screenshot: Consent Form



Study title. Survey on Children's Education and Off-campus Learning in Texas

Study description. You are invited to participate in a research study conducted by Marty Haoyuan Chen at the University of Maryland, College Park. This is a research study examining how families in Texas make decisions about private tutoring for their school-age children.

Eligibility. To participate, you must be (i) at least 18 years of age, (ii) currently residing in the state of Texas, and (iii) the parent or legal guardian of at least one child who is presently enrolled in a Texas school in kindergarten through 12th grade.

Time commitment. The survey takes approximately 5-10 minutes to complete.

Recording. No audio or video recording is involved in this study. All responses are collected as written text through the online survey platform.



Figure C2: Screenshot: Instructions