When Resources Meet Relationships: The Returns to Personalized Supports for Low-Income Students*

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Abstract

Children from low-income families face persistent educational and economic disadvantages that contribute to long-run earnings gaps. This paper studies Communities in Schools (CIS), a program that places coordinators in high-poverty schools to connect struggling students with individualized support. CIS is the largest program of its kind in the U.S., serving nearly 2 million students annually. We find that CIS improves test scores, attendance, and behavior for struggling students, and that these gains persist, leading to higher rates of high school graduation, college attendance, and adult earnings. These long-run effects can be closely forecast from changes in short-run outcomes, with non-cognitive measures playing a central role. CIS delivers returns that compare favorably to other major education interventions, such as class-size reductions. Our results suggest that programs like CIS—combining traditional school resources with mentorship and social capital—can produce lasting improvements in education and economic mobility for disadvantaged students.

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I Introduction

In the United States, individuals from low-income families earn significantly less than their peers from high-income families (Chetty et al., 2014; Solon, 1992). These disparities begin well before labor market entry: children from low-income backgrounds score lower on standardized tests, are less likely to graduate from high school, and attend college at lower rates (Black, Devereux and Salvanes, 2005; Chetty et al., 2020). Improving educational outcomes for children from low-income families is likely to be a promising way to reduce earnings disparities later in life, given the strong evidence on the returns to education (Bhuller, Mogstad and Salvanes, 2017; Card, 1999; Zimmerman, 2014).

One approach is to increase resources and programs in high-poverty schools. Initiatives to improve teacher quality, reduce class sizes, expand afterschool and tutoring programs, and equalize school funding have all shown important benefits (Aizer et al., 2016; Chetty, Friedman and Rockoff, 2014; Hyman, 2017; Jackson, Johnson and Persico, 2016; Nickow, Oreopoulos and Quan, 2020). Still, many low-income students encounter barriers that make it difficult to engage fully with these opportunities: they are more likely to miss school, face logistical or informational hurdles to participating in enrichment programs, and contend with unstable home environments that complicate classroom learning (Malkus, 2024; Robinson, Bisht and Loeb, 2022). Interventions that address these upstream challenges directly may help students make fuller use of existing resources and also improve outcomes in schools where new investments are scarce.

How can we design interventions that help students overcome these upstream barriers to learning in high-poverty schools? Evidence from recent randomized trials in other domains points to a promising answer: pairing traditional resources with personalized supports that help individuals navigate challenges and connect to services (e.g., Bergman et al., 2024; Weiss et al., 2019). These interventions rely on the presence of a trusted adult who can coordinate services and advocate for participants. While most existing implementations have focused on adults outside of schools, extending this approach to children in educational settings may both improve outcomes directly and amplify the returns to other school-based investments.

In this paper, we study Communities in Schools (CIS), a program that places a "site coordinator" in high-poverty schools to connect struggling students with both in-school supports (such as tutoring and mentoring) and out-of-school resources (including food assistance, housing support, and health services), tailored to their individual needs. CIS is the largest integrated student support program in the United States, serving nearly 2 million students across more than 3,000 schools each year. The goal is to ensure students' basic needs are met so they can succeed in the classroom. We evaluate whether the program improves students' short-run academic outcomes—test scores, absences, suspensions—and long-run outcomes—high school graduation,

college attendance, and earnings—using administrative data from the universe of federal tax records and from K–12 education records in Texas, where nearly half of CIS schools are located.[1]

Our analysis yields four main findings. First, using the staggered rollout of CIS in Texas, we show that the program raises test scores among students who struggle academically, have poor attendance, or face disciplinary problems. Second, these gains do not fade out: they translate into higher rates of high school graduation, two-year college enrollment, and ultimately higher adult earnings, as shown in linked Census and IRS records. Third, the long-run effects of CIS can be predicted from its short-run impacts on test scores, attendance, and discipline, with no evidence of additional channels. Noncognitive outcomes such as attendance and discipline are central to this result: only about half of the long-run impact on graduation is explained by short-run improvements in test scores. Finally, we show that CIS delivers gains per dollar spent that compare favorably to reducing class size, and that targeted expansions could meaningfully raise high school graduation rates among at-risk students. Together, these results show that providing individualized supports in high-poverty schools can both improve student achievement in the short run and translate into durable gains in education and earnings.

CIS operates primarily in high-poverty schools where student achievement is low, often in racially diverse communities with low adult educational attainment. Even within the same city or district, CIS programs tend to serve schools in neighborhoods with especially high poverty and limited economic opportunity. Once in a school, teachers and staff can refer students with academic, behavioral, attendance, basic needs, or social-emotional concerns to "case management," a process in which students meet regularly with the site coordinator to ensure they have the resources needed to succeed (Communities in Schools, 2023). In our Texas sample, about 12.5% of students receive case management in a given year of middle school and 18% receive it at some point during grades 6-8. Site coordinators connect students to supports both inside and outside the classroom, ranging from tutoring to help with food insecurity, housing instability, health care access, and social-emotional needs. These services may come from the school, government agencies, or community organizations. Case-managed students receive the most intensive CIS support, but other students may also access basic-needs services and benefit from school-wide CIS initiatives, such as efforts to improve attendance culture.

We begin by showing that case management assignments can be predicted with a relatively high degree of accuracy using data from before the CIS program begins. This allows us to construct a natural control group of students in non-CIS schools who are likely candidates for intensive CIS services, while avoiding bias that could arise from conditioning directly on case management (e.g., a parental divorce may both trigger case management and independently lower

[1] Texas is unique in providing substantial state funding for CIS. In 2022, the state allocated more than $38 million to CIS programs, drawing on both the state budget and federal Temporary Assistance for Needy Families funds.

test scores). Focusing on middle school CIS programs in Texas, we use information on student demographics (e.g., free and reduced-price lunch, or FRPL, status) and elementary school records on test scores, attendance, and suspensions. A gradient boosting algorithm produces strong out-of-sample predictions: 28.7% of students in the top quartile of predicted case management actually receive services, compared to 7.2% in the bottom quartile. Likely case managed students are more likely to be FRPL-eligible, have disciplinary records, be chronically absent, and score lower on standardized tests. Models restricted to a single variable, such as test scores, or to linear regression perform substantially worse. Throughout, we refer to students in the top and bottom quartiles of predicted case management as *high-risk* and *low-risk*, reflecting case management's stated goal of serving students at risk of dropping out in the future.

We next show that CIS improves standardized math and reading scores for high-risk students. In our baseline specification, we estimate an event study where each CIS school is matched to a "never treated" control school that is demographically and academically similar. The match is constructed using data from more than five years before CIS begins, which allows us to test whether outcomes in treated and control schools follow parallel trends prior to CIS entry. We estimate an improvement of 0.11 standard deviations (SD) for high-risk students. For low-risk students, we find relatively precise null effects, suggesting limited spillovers and indicating that CIS's academic benefits are concentrated among students with prior academic, attendance, or discipline challenges.

We use several strategies to test the robustness of our baseline findings on academic outcomes. Results are similar when we draw comparison groups from outside the matched control, including "later treated" CIS schools. We also relax the parallel trends assumption by allowing trends to vary at the school by race by sex level. As another check, we construct a control group of schools within the same commuting zone as the CIS school, ensuring that our estimates are not driven by city-wide changes coinciding with CIS entry. We find no evidence of changes in student demographics, teacher quality, or teacher tenure around the time CIS enters a school. We also show that results are robust to assigning children to a middle school based on their first elementary school, ruling out the possibility that higher-ability students sort into CIS schools after program entry. In addition, we show that within CIS schools, test score gaps between high- and low-risk students narrow immediately after the program begins, ruling out the possibility that our results reflect school-wide improvements coinciding with CIS entry. These findings align with conversations with CIS affiliate directors, who note that program entry is driven by persistent perceptions of need, while its timing is shaped largely by logistical or other idiosyncratic constraints rather than by shifts in leadership or other time-varying factors.

Having established that CIS improves test scores for high-risk students, we next turn to its impact on long-run outcomes: high school graduation, college attendance, and earnings. The mission of CIS is not only to raise in-school performance but also to provide students with the

resources they need to succeed beyond school. As the organization itself describes, its goal is to "surround students with a community of support, empowering them to stay in school and achieve in life" (Communities In Schools, Inc., 2011). Assessing whether the short-run gains we document persist or fade out in the long run is therefore critical for evaluating the overall effectiveness of CIS.

We find that CIS improves long-run outcomes for high-risk students. In our baseline matched-control design, three years of CIS exposure in middle school raises high school graduation by 3.4pp (5.2%), closing 13% of the initial gap between high- and low-risk students. It also increases college attendance by 2.96pp (9.1%), driven entirely by two-year college enrollment. For low-risk students, we find relatively precise null effects, consistent with our short-run results.

We complement our analysis of academic outcomes in Texas with linked Census and IRS records to estimate the effect of CIS on earnings (Goldman, Gracie and Porter, 2023). Because most Texas programs began after 2010, many exposed students are still too young to have reliable earnings measures. To address this, we use the staggered rollout of CIS programs outside Texas to estimate event studies and find that three years of exposure raises earnings at age 27 by 1.54 percentile ranks (4.3%). For Texas schools, where earnings can already be measured for younger cohorts, we obtain similar estimates. The gains come mainly from reducing the likelihood that students fall into the bottom quartile of the income distribution and increasing the likelihood that they move into the second quartile. Consistent with CIS's description as the nation's "leading dropout prevention organization," the evidence suggests that CIS helps students avoid adverse, or "left-tail," outcomes both in school and later in life.

In the final section, we test whether CIS's long-run effects can be anticipated from its short-run impacts on in-school outcomes. This exercise sheds light on the mechanisms driving CIS's effectiveness. If predicted impacts exceed actual long-run effects, it would suggest fade-out—perhaps because test-score gains reflect skills less relevant for lasting human capital, because attendance or discipline shifts stem from administrative changes rather than behavior, or because later constraints, such as limited college counseling, offset early gains. Conversely, if predicted impacts fall short of actual long-run effects, it would point to benefits not fully captured by short-run measures, such as mentoring, social capital, college-preparation programs, peer spillovers, or downstream complementarities like enrolling in advanced classes or seeking stronger teachers. If the predicted impacts align closely with the actual long-run effects, it suggests that our short-run outcomes serve as valid "statistical surrogates," allowing future CIS expansions to be evaluated more quickly without waiting years for follow-up data with long-run outcomes.

We find that the data best fit the third scenario: CIS's long-run effects are predicted with striking accuracy from its short-run impacts. Using math and reading test scores, quarterly attendance records, and disciplinary incidents, we construct predicted long-run outcomes and estimate CIS's effect on them in our baseline design. The predicted impacts closely match the

actual long-run effects: for example, CIS raises high school graduation by 3.1pp (SE=1.3), while the predicted effect based only on short-run data is 2.4pp (SE=0.8). This pattern holds for both high- and low-risk students and extends to college attendance and earnings, showing that standard in-school outcomes provide a reliable window into CIS's long-run effectiveness. Notably, if we had relied only on test scores, we would have substantially understated CIS's true impact. The results underscore that, beyond cognitive skills, CIS's lasting effects on engagement and behavior are central to improvements in graduation and later-life earnings.

Our paper contributes to the literature on improving outcomes in high-poverty schools by evaluating a coordinator-led model that connects struggling students to academic and non-academic supports. Prior studies have largely focused on expanding resources, improving curricula, or introducing new programs—examples include high-dosage tutoring, teacher quality initiatives, class-size reductions, "no excuses" charter schools, and school finance reforms (e.g., Guryan et al., 2023; Chetty, Friedman and Rockoff, 2014; Angrist et al., 2019; Dobbie and Fryer Jr, 2011; Jackson, Johnson and Persico, 2016). Yet the poorest students may struggle to fully benefit from these investments if they are frequently absent or face challenges at home that limit their engagement. We provide large-scale evidence that a comprehensive individualized support model—CIS—raises test scores, attendance, and behavior for high-risk students, with gains that persist through high school graduation, college enrollment, and early-career earnings. On a cost-adjusted basis, CIS compares favorably to other major interventions: each $1,000 invested raises and earnings by $406, compared with roughly and $40 for class-size reductions (Chetty et al., 2011). Taken together with evidence from similar interventions in other settings (Lavecchia, Oreopoulos and Brown, 2020), our results show that integrated student supports can deliver durable gains for poor students.

This study also contributes to a growing literature on the role of social connections in shaping economic opportunity. Social networks are an important determinant of economic mobility (Chetty et al., 2022; Fischer and Mattson, 2009; Putnam, 2016), shape how local shocks persist across generations (Chetty et al., 2023), and causally influence human capital investments (Cattan, Salvanes and Tominey, 2022). A distinctive feature of CIS is its reliance on site coordinators—caring adults who work directly with students to identify needs, connect them to supports, and ensure they remain engaged in school. This relational model produces lasting gains in academic achievement, educational attainment, and earnings. Our findings add to a growing body of experimental evidence showing that social ties not only promote mobility directly but also amplify the impact of traditional opportunity-enhancing investments—such as college access programs, emergency rent assistance, housing vouchers, and job training—when those investments are paired with mentorship and relational support (Bell and Petkova, 2024; Bergman et al., 2019; Caspi and Rafkin, 2023; Espinosa et al., 2024; Finkelstein and Notowidigdo, 2019; Katz et al.,

2022; Weiss et al., 2019). Unlike most prior studies, however, this paper examines these dynamics in the context of children and K–12 schools. This focus underscores that social connections are not only powerful for adults navigating the labor market and public programs, but can also be deliberately cultivated early in life to alter life trajectories from an early age.

Finally, we contribute to a growing literature that seeks to forecast the long-run returns of interventions focused on children using short- or medium-run academic outcomes. A common practice for school-based programs is to estimate returns on investment by projecting long-run effects from test score gains (e.g., Guryan et al., 2023; Hanushek, 2011; Kline and Walters, 2016; You, Fu and Chou, 2025).[2] We show that CIS's long-run impacts can indeed be forecasted from short-run outcomes, but not from test scores alone. Measures of engagement and behavior—such as attendance and discipline—are essential to capture the noncognitive dimensions through which CIS operates. This finding reinforces evidence that test scores can understate the lasting impacts of programs, especially those that build noncognitive skills (e.g., Anders, Barr and Smith, 2023; Chetty et al., 2011; Cortes, Goodman and Nomi, 2015; Gray-Lobe, Pathak and Walters, 2023). More broadly, our results underscore the promise of using richer sets of short-run outcomes as statistical surrogates to evaluate programs in real time, without waiting years or decades for long-run follow-up data (Athey et al., 2019; Prentice, 1989).

II Background on Communities in Schools

Communities in Schools (CIS) is the largest and most established provider of integrated student support (ISS) services in the United States. Founded in the 1970s in New York, today the program operates in more than 3,500 schools, serving 2 million students in 29 states and the District of Columbia (Communities in Schools, 2023). CIS aims to reduce dropout rates and improve academic outcomes for students by addressing both academic and non-academic barriers to learning. The key component of the CIS model is the "site coordinator"–or navigator–placed in the school, who works with school staff to identify students in need, assess the root causes of disengagement, and connect them to tailored supports. Importantly, the site-coordinator is a dedicated CIS employee, not a teacher or administrator, freeing up those members of the school to focus on their own roles.

Integrated student support programs, such as CIS, recognize that many students, especially those from disadvantaged backgrounds, face personal, familial, and community-level challenges such as food insecurity, unstable housing, mental health needs, or limited academic support outside of the classroom, that interfere with their ability to succeed in school. The job of the navigator is to

[2]The scaling factor from Chetty, Friedman and Rockoff (2014) is frequently used for this purpose. See Table A.IV in Kline and Walters (2016) for a review of observational and experimental estimates linking test scores to earnings.

systematically identify these barriers for a given student, connect the student to the necessary set of resources, and ensure that the student is able to fully engage with their education. The navigators act as bridges between students and existing school, community, or government resources.

Within a school, CIS operates at multiple tiers of intensity. At the most targeted level, students receive "case management" from the navigators. This involves creating an individualized student support plan and regular check-ins with the site-coordinator, who helps provide access to in-school and community-based services. For a lighter touch, some students also receive a middle tier of support – often targeted services for a group of student with a shared need, which also falls in the broad category of case management (Texas Education Agency, 2023). Beyond these two levels of intensity, CIS also implements school-wide initiatives such as attendance drives, behavioral support campaigns, or family engagement events intended to improve school climate and support all students (Tier I).

There is no strict rule for how students are referred to site coordinators for case management. In practice, students are often referred based on observable risk factors including chronic absenteeism, behavioral issues, low academic performance, or signs of unmet basic needs (Parise and Hossain, 2017). In many cases, teachers, counselors, or administrator may flag students for referral. In others, coordinators may proactively identify students using administrative records.

Although CIS is a national program, its presence is especially pronounced in Texas, where nearly half of all CIS schools are located. Figure I shows the share of students enrolled in a CIS school over time. The program expanded rapidly in recent decades: by 2019, 12.7% of Texas public school students attended a school with CIS. Unlike in other states, CIS is written into the state education charter and received public funding both from the Texas Education Agency (TEA) and through federal Temporary Assistance for Needy Families (TANF) funds. In FY 2022, state funding for CIS totaled $33.5 million plus $4.8 million of TANF funds. Because of this relationship, in Texas there are additional rules for CIS implementation. For example, students are eligible for case management only if they are defined as being "at-risk" by TEA, are eligible for free and reduced price lunch, are engaging in delinquent activities, or are determined to be experiencing family conflict or crisis (Texas Education Agency, 2021).

Once students are referred to the site coordinator, they undergo an individualized needs assessment to identify the academic, behavioral, and non-academic barriers that may be impeding their success. The coordinator then works with the student, their family, and school personnel to develop a personalized support plan and begin connecting the student to appropriate services. These supports span a wide range: academically, students might receive tutoring, credit recovery, test prep, or help with study habits. Outside the classroom, coordinators can provide or facilitate access to food pantries, transportation assistance, medical or mental health care, crisis housing, or family support services. Navigators may also offer mentoring, lead social-emotional

learning groups, or organize goal-setting and attendance check-ins. Supports may be delivered directly, through school-based staff, or via external providers and community partnerships. Site coordinators are trained to monitor progress, maintain regular contact with students, and revise plans as needs evolve—serving as a single point of coordination in an often fragmented support ecosystem. More than just a service broker, the navigator also provides students with a consistent, supportive relationship as a "caring adult"—a critical element for many students navigating instability at home or in school.

Given these goals, it is not surprising that CIS programs tend to be located in schools and neighborhoods with high poverty rates. Figure II Panel *A*, maps the locations of CIS schools in Texas as of 2019, with counties shaded by their poverty rates in the year 2000. CIS programs are heavily concentrated in the western and southern regions of the state—areas with the highest poverty levels—and are also clustered in major urban centers in the east.

County-level poverty rates do not tell the full story. In Figure II Panel *B*, we compare the characteristics of neighborhoods directly served by CIS schools to those without CIS, using school catchment zones to map neighborhoods to schools. Students in CIS neighborhoods are substantially less likely to be white and more likely to qualify for free and reduced-price lunch. These neighborhoods also tend to be more urban—denser in population—and have higher poverty rates and lower levels of educational attainment. One of the most striking differences is in rates of economic mobility: children from low-income families in CIS neighborhoods experience rates of upward mobility that are, on average, 0.35 standard deviations lower than those in non-CIS neighborhoods.

It is not simply that CIS schools are located in cities with higher poverty or greater diversity; rather, within a given commuting zone, CIS programs tend to serve the neighborhoods with the greatest need. These neighborhoods are more racially diverse, have lower incomes, and exhibit lower rates of economic mobility than other neighborhoods in the same labor market. In Figure II Panel *B*, we show that these differences persist even when comparing neighborhoods within the same commuting zone: the within-CZ estimates are very similar to the raw differences, indicating that CIS programs are systematically targeted toward the more disadvantaged communities in each area, consistent with the program's mission.

Although CIS programs are more common in high-poverty neighborhoods and overall coverage is substantial, most neighborhoods—including many high-poverty ones—do not have a CIS school. In Figure II Panel *C*, we plot the relationship between poverty rates and the coverage of CIS programs in those neighborhoods. Among the tracts with the lowest poverty rates, only 10-15% of tracts are served by a CIS program. In tracts with the highest poverty rates, this proportion rises to around 30%. This underscores both the targeted nature of CIS and the substantial scope for expansion, something we return to in the Conclusion. In the next section, we

describe the data we use to estimate the impact of CIS on student outcomes.

III Data

We leverage three main sources of data for our analysis. First, we use administrative education records from Texas that track student outcomes in both the short and long run. Second, we incorporate administrative data from Communities in Schools (CIS) in Texas, which provide detailed information on student-level interactions with CIS navigators. Third, we use Census and IRS records to estimate the long-run effects of CIS on earnings outcomes outside of Texas. In this section, we describe each of these data sources in more detail.

III.A Texas Education Research Center

Our primary data come from administrative academic, postsecondary, and labor market records maintained by the Texas Education Research Center (ERC) at the University of Texas at Austin. These data link individual-level records from the Texas Education Agency (TEA), the Texas Higher Education Coordinating Board (THECB), and the Texas Workforce Commission (TWC). Critically, the ability to track students across these datasets allows us to follow a single individual from elementary school through high school, into college, and into the labor market.

We begin with student-level data from TEA on all students enrolled in Texas public schools between 1995 and 2019. The dataset includes more than 109 million student-year observations for approximately 16 million unique students. For each student, we observe time-invariant characteristics—such as birth cohort, race/ethnicity, and sex—as well as time-varying information including campus attended, grade level, free and reduced-price lunch (FRPL) status, and indicators for being classified as "at-risk" under TEA guidelines.[3]

We also identify whether each student ever attends a school served by the CIS program. To do this, we worked directly with Communities In Schools to obtain a list of all schools with a CIS program as of the 2019–2020 school year, including the year the program began at each school. Using this information, we construct an indicator for whether a student ever attends a CIS school and track the timing of CIS entry at each site. Of the 16 million students in our dataset, approximately 3 million attend a school that has adopted CIS by 2019.

We construct a range of in-school outcomes for each student. Our primary cognitive outcome is performance on standardized state math assessments, administered annually in grades 3-8.[4]

[3]The at risk indicator flag students who are at risk of dropping out of school, according to TEA. There are 13 criteria, including poor academic performance, interactions with the criminal justice system, parenthood/pregnancy, and experiencing homelessness. See Texas Education Agency (2013).

[4]The specific assessments vary over time (e.g., TAAS (1990-2002), TAKS (2003-2012), STAAR (2012-), so we

We also examine behavioral outcomes, including attendance and discipline. For attendance, we observe the number of days present in each of the six, six-week grading periods of the school year. For discipline, we construct indicators for whether a student was ever suspended (in-school or out-of-school) during the year, as well as a continuous measure of disciplinary incidents based on severity and frequency.

We follow students over time to measure long-run educational outcomes. For each student, we observe whether they graduate from high school by 2022. We also track in-state college enrollment using records from the Texas Higher Education Coordinating Board (THECB) through 2019.[5] We define indicators for whether a student ever attends any post-secondary institution between ages 18 and 22, and separately for two-year and four-year colleges.

We measure labor market outcomes using earnings data from the Texas Unemployment Insurance (UI) system. These records capture quarterly earnings for workers in UI-covered employment but exclude self-employed individuals, military personnel, informal workers, and independent contractors (Stevens, 2007). We cannot distinguish between individuals who are missing from the UI data because they are not working, are in a non-covered job, or have left the state. We restrict our earnings analysis to individuals who ever report positive earnings between ages 18 and 25, and impute zero earnings in quarters and years in which a person has no record. Overall, 82% of our sample appears in the UI data at least once during this age range.[6] After imputing the zeroes, we construct income percentile ranks at each age from 18-27, ranking relative to other students in the same birth cohort. Because we are limited in the set of cohorts for which we can observe adult earnings outcomes, we also use separate data from the Census Bureau and IRS to measure earnings effects, which we describe in Section III.C.

Table I provides summary statistics from the ERC data for three groups: all students statewide, students who ever attend a school with a CIS program as of 2019, and students who attend a middle school with a CIS program in 2019. Our data include more than 16 million students across Texas, with approximately 3.3 million attending a school that ever implements CIS.

Students in CIS schools are on average somewhat more disadvantaged than the typical Texas student, but not dramatically so. They are less likely to be white (26.2% vs. 36.0%) and more likely to qualify for free or reduced-price lunch (52% vs. 47%). On average, they score about 0.13 standard deviations lower on standardized math tests. Long-run outcomes are broadly similar: high school graduation rates are nearly identical, and average earnings at age 27 differ by only $1,000

standardize scores within grade and year.

[5]The ERC also maintains National Student Clearinghouse records, which capture out-of-state enrollment, but only for more recent cohorts. We focus on in-state enrollment using THECB data to maximize coverage across the full panel.

[6]This is consistent with Sprung-Keyser et al. (2022) who find that among people who were living in Texas at age 16, 88% were still in Texas at age 26. Our fraction is somewhat lower, but we do not condition on being in Texas as late as 16.

($23,800 vs. $24,800 statewide).

However, these average differences mask substantial heterogeneity in student need within schools, which we explore in Section IV. In particular, students who are case-managed are more likely to come from lower-income backgrounds and have markedly worse outcomes than their peers in the same school.

III.B CIS Mircodata

We use student-level microdata from Communities in Schools of Texas, available beginning in 2014, and link them to Texas ERC administrative records using student identifiers. These data allow us to observe individual students' participation in CIS and the specific services they receive. There are two key components: (1) intake records for students who are formally case-managed by CIS, which include baseline needs determinations, and (2) logs of each interaction between students and CIS navigators, detailing the date, duration, and type of service provided.

The participant data contain one row per student-year for students who were formally case-managed by CIS. For each student, we observe the date that parental consent was received, indicating permission for the student to participate in CIS programming and receive case management. We also observe the broad category of need that the students were flagged for. These include any combination of academics, attendance, behavior, and social services. In addition, we have more detailed information on the types of needs that navigators anticipate targeting. These include domains such as "academic readiness", "social skill", "self esteem", "classroom conduct", and "family conflict".

The interaction data contain one row per student-interaction with the navigator. For each interaction, we observe the date, the type of service provided, the organizational provider (whether the service was delivered by CIS staff or an external partner), and the duration of the interaction. For example, a navigator might spend 10 minutes providing food assistance through a backpack program, or one hour leading a self-esteem workshop with a small group of students.

Combining these two data sources with the broader ERC administrative data, we can characterize both the types of students who receive case management from CIS and the nature of their interactions with navigators. Case-managed students are somewhat more likely to be female than male (46.6% male), are overwhelmingly from racial and ethnic minority groups (only 12.8% are white), and the vast majority qualify for free or reduced-price lunch (71%). Academically, these students tend to struggle: even at the 75th percentile, their standardized math scores fall below the statewide mean, and the average score is more than half a standard deviation below the state average. Case-managed students also face higher disciplinary rates. In sixth grade, 26.6% of them are suspended—more than twice the statewide average of 12.9% (see Appendix Table A.1

for details).

Consistent with these student characteristics, academics and behavior are the most common reasons for referral to CIS case management: 83% of students are referred for academic concerns and 65% for behavioral issues (not mutually exclusive). This focus is reflected in the specific needs that navigators aim to address. The most frequently targeted need is "test readiness," which appears in 41% of case-managed student-years. Behavioral supports are also prevalent: 36.6% of students are targeted for social skills, 32% for self-esteem, and 15% for issues related to classroom conduct. Appendix Table A.2 provides a full list of targeted needs.

These data also confirm that CIS provides services targeting both cognitive and non-cognitive domains. Analyzing the ten most common services provided in each year from 2015 to 2017, we find that time spent is roughly evenly split between cognitive and non-cognitive supports (see Appendix Figure A.1). The most common services include academic and social skills development, tutoring, character building, and attendance interventions. Notably, food assistance is also among the most frequently provided services, potentially influencing both academic performance and behavioral engagement.

III.C Census-IRS Data

In Section IVC, we estimate the impact of CIS on long-run earnings outcomes using linked Census and IRS data. Our analysis relies on a combination of the 2000 and 2010 Census short forms and data from federal income tax returns in 1979, 1984, 1989, 1994, 1995, and 1998-2019.

We follow Chetty et al. (2023) to construct our analysis sample.[7] We focus on children born in the 1978–1992 birth cohorts who appear in the 2020 Numident file and are successfully linked to a parent via dependent claiming on 1040 tax forms.[8]

We measure both parent and child income using IRS tax records. Parent income is defined as the average of total household income between child ages 13–17, imputing zeros for any year without a tax filing.[9] We rank parents relative to others with children in the same birth cohort to construct percentile ranks. For children, we measure household income at age 27—including spousal income, if present—and convert it to percentile ranks relative to other children in the same cohort.

Although we do not observe school enrollment in these data, we infer neighborhood of residence based on the Census tract from which a parent first files a 1040 tax return claiming the child. We assign each child to this invariant neighborhood and crosswalk Census tracts to

[7] See Section II of Chetty et al. (2023) for additional details on data sources and sample construction.

[8] Valid parents are adults who appear in the 2020 Numident and would have been between ages 15–50 at the time of the child's birth. The first such individual(s) is (are) assigned as the child's invariant parent(s).

[9] We exclude families with zero or negative mean income over the entire 5-year window.

school catchment zones using the procedure described in Section IV.C.

Our final sample includes 689,200 children who grew up in neighborhoods that ever received a CIS program during our analysis window. Of those, approximately 392,000 children would have been exposed to the program, based on their age at the time of implementation in their tract.

We return to these data after detailing our empirical methodology and estimating the effect of CIS on short-run student outcomes in the Texas data.

IV Empirical Methodology

We now turn to developing a research design to estimate the effects of CIS on student outcomes in the short- and long-run. If CIS programs had been randomly assigned, we could identify causal effects by simply comparing outcomes in schools that did and did not receive the program. In practice, program adoption was not randomized. Instead, we implement a difference-in-differences approach, comparing changes in outcomes within CIS schools following program entry to changes over time in a comparison group of similar schools that have not or have not yet adopted the program.

While CIS programs operate at the school level, the most intensive supports–such as case management–are provided to a subset of students, as described in Section II. To better understand the effects of CIS on the students most likely to be served by the program, we construct a proxy for case-managed status at the student level using pre-treatment student characteristics. While we have actual case-management status in later years, we do not use this label directly as this would risk conditioning on negative, contemporaneous shocks that both lead students to be case-managed and directly impact student outcomes.

As a starting point, we assess how well individual pre-treatment variables predict case management status using simple, univariate comparisons. For each student who attends a CIS middle school between 2014 and 2019, we construct an indicator for ever receiving case management during middle school, using the CIS microdata. In Figure III Panel *A*, we show that 19.7% of students in the bottom 5% of the standardized math score distribution are ever case managed, compared to 4.5% of students in the top 5% of the distribution. Similarly, students with the highest number of elementary school absences are nearly twice as likely to be case-managed, compared to students with near-perfect attendance. These univariate predictors show clear targeting patterns, but explain only a modest share of the variation in case management status on their own: the R^2 remains low in each case.

To improve predictive accuracy, we train a gradient boosting model using the full set of elementary predictors that we observe (Ke et al., 2017). Inputs include standardized math scores, attendance, disciplinary history, grade retention, and demographic variables such as race/ethnicity,

sex, and FRPL status. The model is trained using five-fold cross-validation and evaluated on a held-out test set. In Figure III Panel *C*, we show the out-of-sample accuracy of the model. For each percentile of the prediction distribution, we plot the fraction of students in the hold out data who were ever case-managed. The model demonstrates substantially greater predictive power than any individual variable: among the 5% of students with the lowest predictions, only 1.1% are case managed, while in the top 5% of predictions, 37.3% are. Figure III, Panel *D*, shows the relative importance of each predictor based on model "gain," a measure of the improvement in model fit attributed to splits on that variable. Test scores are among the most important predictors, but other features—including attendance, grade retention, and prior disciplinary incidents—also contribute meaningfully. The out-of-sample R^2 of the full model is 3.8 times larger than when using test scores alone.

Because our goal is to estimate the effect of CIS on students most likely to be served by the program, we apply the trained prediction model to the full panel of students—across all years and all schools—regardless of whether the school has implemented CIS. This allows us to construct a measure of predicted case management risk for every student in the data, even in years and schools where CIS was not active. Throughout the paper, we categorize students in the top quartile of the prediction distribution as "high-risk" and students in the bottom quartile as "low-risk" (as shown in Figure III Panel *C*). In treated schools, this enables us to identify high-risk students before the program begins. In control schools, it allows us to identify observationally similar high-risk students who were not exposed to CIS.

Using these predictions, we report summary statistics for high- and low-risk students in both CIS and non-CIS schools. Table II presents these results. We find large differences between high- and low-risk students across both groups of schools. For example, 76.3% (76.6%) of high-risk students are eligible for FRPL in a given year, compared to 10.0% of low-risk students in CIS (non-CIS) schools. Disparities in outcomes are similarly large: high-risk students score 1.5 standard deviations lower than low-risk students in 6th grade and earn half as much at age 27 ($17,000 vs. $35,000). Although outcomes for these groups are similar in CIS and non-CIS schools, the concentration of high-risk students is substantially higher in CIS schools. High-risk students outnumber low-risk students by a factor of 1.67 in CIS schools, compared to 0.87 in non-CIS schools.

Having constructed a measure of predicted treatment intensity for each student, we now turn to estimating the effect of CIS on student outcomes. We implement an event study framework that compares outcomes in treated schools in the year before and after CIS adoption to a set of control schools that never receive CIS. We measure outcomes at the school-by-year-by-race-by-sex level and define event time relative to the first year of program implementation. We estimate treatment effects in the eleven year window centered around the program beginning. Because we

use elementary school characteristics to predict case management risk, we focus our analysis on programs beginning in middle schools.

We select a matched comparison school for each treated school based on pre-treatment characteristics. For each treated school, we measure covariates six years prior to the start of its CIS program (i.e., at event time $t = -6$), ensuring that the match is based on data outside the event study window. For potential control schools, we compute the same covariates at $t = -6$ relative to a hypothetical start year, aligning the match in calendar time. To operationalize the match, we estimate a linear probability model of CIS adoption as a function of school-level demographics and outcomes, using data from the donor pool of non-treated schools. We then compute the predicted probability of treatment for each school and match each treated school to the control school with the closest predicted value.[10] Because a single control school may serve as a match for multiple treated schools—either within the same cohort or across cohorts—we cluster all standard errors at the school level in our baseline estimates.[11]

After matching, we stack the treated and control pairs across implementation cohorts and estimate the following event study specification:

$$y_{s,T(s),t,r,m} = \sum_{\tau \in [-5,5]; \tau \neq -1} \beta_\tau \cdot (\mathbf{1}[t - T(s) = \tau] \cdot CIS_s) + \lambda_{s,T(s),r,m} + \gamma_{t,T(s)} + \varepsilon_{s,T(s),t,r,m}, \quad (1)$$

where $y_{s,T(s),t,r,m}$ is an outcome in school s, year t, and implementation cohort $T(s)$ (i.e. the year in which school s adopts CIS, or the pseudo-year for the never treated control group), for students of race/ethnicity r and sex m. The coefficients β_τ capture the difference in outcomes in treatment and control schools at relative time τ, with $\tau = -1$ as a reference period. The fixed effects $\lambda_{s,T(s),r,m}$ absorb all level differences across schools by implementation cohort, race, and sex. The $\gamma_{t,T(s)}$ fixed effects control for year-specific shocks within each implementation cohort, capturing the evolution of outcomes in the control schools. In our main results we estimate Equation 1 separately for high- and low-risk students and cluster standard errors at the campus level.

In many cases, we also report results from a more parsimonious difference-in-differences

[10] The full set of predictors includes the share of students eligible for FRPL; the share identified as at risk of dropping out (per TEA); the racial/ethnic composition (White and Hispanic shares); the fraction of students in the top and bottom quartiles of the predicted case management distribution; the share of students chronically absent (missing more than 10% of the school year); in-school suspension rates; standardized math scores; and the predicted high school graduation rate, described in Section VI

[11] In robustness checks, we find that our results are similar when expanding the set of control schools to the nearest 5 matches or restricting to unique control matches.

specification:

$$\begin{aligned} y_{s,T(s),t,r,m} = &\beta_{post,1-2}(\mathbf{1}[1 \leq \tau \leq 2] \cdot CIS_s) \\ &+ \beta_{post,3-5}(\mathbf{1}[3 \leq \tau \leq 5] \cdot CIS_s) \\ &+ \tilde{\lambda}_{s,T(s),r,m} + \tilde{\gamma}_{t,T(s)} + \varepsilon_{s,T(s),t,r,m}. \end{aligned} \tag{2}$$

This specification includes the same school and year fixed effects as Equation 1, but instead of estimating separate coefficients for each relative year, it averages the treatment effect over two post-treatment periods: years 1–2 and years 3–5. We focus primarily on $\beta_{post,3-5}$, which captures the average effect several years after implementation.

The key identifying assumption in this design is that, absent the introduction of CIS, outcomes in the treated schools would have followed the same path as those in the matched control schools. This assumption may be violated if the timing of CIS coincides with other reforms or resource shifts at the school (e.g., leadership changes, increases in funding) that also impact student outcomes. It could also be threatened if the composition of students changes at the time of CIS adoption, for example due to differential sorting.

In the next section, after presenting our baseline results, we assess the robustness of these findings to a range of alternative specifications designed to address these concerns. We begin by estimating the effect of CIS on student outcomes in the matched sample and then test whether results are sensitive to alternative designs, including control groups using later-treated school, comparisons within commuting zones, and within schools.

V Effect of CIS on Student Outcomes

We begin by documenting the first stage of our analysis: the impact of CIS adoption on students' exposure to the program. For each student-year, we calculate cumulative years of exposure to a CIS program during middle school. We estimate Equation 1 separately for high- and low-risk students, comparing changes in exposure in treated versus matched control schools. Figure IV presents the event study results. Before program adoption, exposure is near zero in both groups, as expected. Following CIS entry, exposure rises rapidly and then levels off, consistent with students progressing through middle school grades in CIS schools. In the absence of student mobility across schools or grade retention, we would expect exposure to equal one year at $\tau = 0$, increase to approximately 1.67 at $\tau = 1$, and level off at 2.0 in subsequent years.[12] The observed first stage

[12] If students never move and there are an equal number of students in 6-8th grade in a school, then in any given year at $\tau >= 3$, 6th graders will have one year of exposure, 7th graders 2, and 8th graders 3, averaging to 2 years of average exposure.

closely tracks this pattern. At $\tau = 1$, students average 1.46 (high-risk) and 1.55 (low-risk) years of exposure, roughly 90% of the anticipated exposure level.

In the years three to five after adoption—the main period we use for estimating more parsimonious DID effects throughout our analysis—students average 1.70 years of exposure (high-risk) and 1.75 years (low-risk). These estimates are statistically indistinguishable, confirming that high- and low-risk students experience similar exposure to CIS at a school-level once the program is in place.

V.A Impacts on Test Scores

Having established that CIS adoption does increase program exposure, we now turn to measuring the impact of that exposure on students' standardized test scores in middle schools. We start by estimating Equation 1 separately for high-risk students, with results shown in Figure V Panel *A*. The overall patterns mirrors the first stage. In the pre-period, coefficients are tightly centered around zero, supporting the parallel trends assumption. Recall that control schools were matched on covariates measured at $\tau = -6$, outside the event-study window. Thus, the pre-period serves as an out-of-sample test of parallel trends between treated schools and the matched control schools.

Following CIS adoption, test scores begin to improve in treated schools relative to their matched controls, before stabilizing in later years. We estimate a difference-in-differences effect of 0.11 standard deviations (S.E. = 0.04) for high-risk students. When we scale the effect using an instrumental variables (IV) specification based on years of exposure, we find that three years of CIS exposure increases test scores by 0.18 standard deviations (S.E. = 0.07). For context, this magnitude is similar to the long-run impact of well-known education interventions. For example, prior work on the Tennessee STAR experiment has found that assignment to a small class in third grade led to gains of roughly 0.1 standard deviations on later standardized tests (Athey, Chetty and Imbens, 2025).

Off-the-shelf standard errors in our setting do not account for the fact that we estimate students' risk status using a predictive model, which introduces an additional source of uncertainty into our second-stage estimates. In Figure V Panel *A*, we find a t-statistic of 2.75, implying that standard errors would need to be approximately 40% larger in order to fail to reject a positive treatment effect at the 5% level. To assess the extent to which this uncertainty might affect inference, we conduct a clustered bootstrap procedure in which we sample schools with replacement and, in each iteration, re-estimate both the risk model and the treatment effect. The distribution of the bootstrapped treatment effects is shown in Appendix Figure A.3. The resulting 95% confidence interval is 10.4% wider than that implied by the analytic standard errors.[13] While

[13] For computational reasons, we do not re-estimate the full GBM model and instead use a somewhat simpler model,

this adjustment modestly widens the interval, it does not change the overall conclusion: the estimated impact of CIS on test scores for high-risk students remains statistically significant.

How do the meaningful effects of CIS on test scores for high-risk students compare to the effects for their lower-risk peers? In Figure V Panel *B*, we repeat we repeat the analysis for low-risk students. As with high-risk students, we observe coefficients close to zero in the pre-period, supporting the parallel trends assumption. However, following CIS adoption, we find no evidence of improvement in test scores for low-risk students; the estimated treatment effect is 0.01 standard deviations (S.E. = 0.04). To assess whether the treatment effects decline gradually across the risk distribution—or are concentrated entirely among high-risk students—we repeat this analysis for the middle two quartiles of the predicted risk distribution. As shown in Appendix Figure A.2, we find a monotonic pattern: treatment effects are largest for high-risk students, somewhat smaller for students in the third quartile, smaller still for those in the second quartile, and near-zero for low-risk students. This gradient suggests that CIS has the greatest impact on the students most likely to be served by the program.

While high-risk students are more likely to receive case management, we caution against interpreting the difference in treatment effects across groups as the causal impact of case management itself. One possibility is that low-risk students see no gains because they are unlikely to be directly served by the navigator. However, it may also be that CIS services—whether case management or broader supports—are less impactful for students with fewer needs. These estimates should therefore not be read as evidence that case management has no effect for the marginal low-risk recipient. We do not rescale the reduced-form estimates by case management take-up, and instead emphasize the central finding: CIS meaningfully improves outcomes for high-risk students.

We perform several exercises to test the robustness of this result. First, we allow for a more flexible set of controls. We augment Equation 1 so that the year-specific shocks in γ can also flexibly vary by race/ethnicity and sex. This allows for the possibility that different demographic groups experienced distinct trends in test scores in the control schools and ensures tighter, "within-group" comparisons between treatment and control. The results are shown in Figure VI Panels *A* (high-risk) and *B* (low-risk). We again find clear, positive treatment effects for high-risk students and no effects for low-risk students.

Next, we implement an alternative empirical strategy that compares earlier-treated schools to later-treated schools, rather than to matched control schools. For each implementation cohort $T(s)$ and relative time period τ, we define the set of control schools as those with later implementation cohorts ($T(s') > T(s) + \tau$).[14] We estimate a reduced form regression analogous to our baseline

which we describe in the notes of Appendix Figure A.3. However, we keep this internally consistent for comparison between the bootstrap and analytic standard errors.

[14]Because control schools eventually receive treatment, the panel is not balanced: control schools drop out of the

specification, using the same λ and γ fixed effects as in the matched design.

The results are shown in Figure VI, Panels *C* (high-risk) and *D* (low-risk). This approach yields similar treatment effects: for high-risk students, we estimate a 0.09 standard deviation increase in test scores (S.E. = 0.03), compared to 0.11 (0.04) in the baseline; for low-risk students, the estimate is –0.01 (0.04), similar to the baseline effect of 0.01 (0.04). Unlike our baseline design, which uses never-treated schools as controls, this approach exploits only the quasi-random timing of CIS adoption across eventually treated schools.

We also refine the later-treated design by restricting to comparisons within the same commuting zone. Specifically, we interact the γ year fixed effects with commuting zone indicators and compare earlier- and later-treated schools within the same city over time. Doing so ensures that both treatment and control schools are subject to the same local policies, funding environments, and district-level initiatives. We continue to define the control group dynamically as schools with later implementation dates, as in the previous specification. Figure VI, Panels *E* and *F*, show that the results remain stable: high-risk students see meaningful improvements in test scores, while low-risk students continue to show no gains. This within-city design provides additional support for the claim that the effects we observe are driven by the CIS program rather than differential local trends that coincide with the timing of CIS entry.

A related concern is that changes in student or teacher composition—rather than the CIS program itself—drive the improvements in outcomes we observe. To assess this possibility, we test for discontinuous shifts in observable school-wide characteristics at the time of CIS adoption. Figure VII presents the results. In Panel *A*, we plot an event study of the fraction of students who are White, finding no evidence of any changes in racial composition around program entry. In Panel *B*, we extend this analysis to a broader set of student and teacher characteristics, reporting difference-in-differences estimates in standard deviation units for comparability. Across nearly all characteristics—including student demographics, teacher experience, and staff size—the estimated effects are small and statistically indistinguishable from zero. The lone exception is average teacher pay, which we estimate to fall by 0.12 standard deviations (approximately $800 in average pay) in treatment schools. Importantly, this shift runs counter to concerns that improvements in teacher quality, rather than CIS, drive the gains in student outcomes for high-risk students.

Although we show that observable student characteristics do not change when CIS is introduced, a remaining concern is that higher-ability, high-risk students may selectively sort into CIS schools at program entry. To address this, we construct an alternative assignment measure that predicts middle school enrollment based on students' elementary school rather than their actual middle school.[15] We then re-estimate treatment effects using this predicted school assignment,

comparison group in a given relative year once they begin treatment themselves.

[15]We construct a transition matrix from grade g to grade $g+1$ separately by birth cohort and assign students to

holding treatment and control matches fixed. As shown in Figure VI Panels *G* and *H*, the results are highly consistent: we continue to find gains for high-risk students and no effects for low-risk students. Since the first stage is somewhat weaker in this exercise driven by moves during childhood, we also report IV estimates based on three years of exposure for both actual and anticipated school assignments (Appendix Figure A.4). The similarity of the IV estimates across approaches helps alleviate concerns about endogenous sorting into CIS schools.

Finally, we show that when CIS programs begin, the within-school gap in test scores between high- and low-risk students narrows. To do this, we estimate an alternative specification that compares outcomes for high-risk versus low-risk students within treated schools. Concretely, we estimate:

$$y_{s,T(s),t,r,m,grp} = \sum_{\tau \in [-5,5]; \tau \neq -1} \beta_\tau \cdot (\mathbf{1}[t - T(s) = \tau] \cdot \mathbf{1}(grp = HighRisk) + \lambda_{s,T(s),r,m,grp} + \gamma_{t,T(s)} + \varepsilon_{s,T(s),t,r,m,grp}, \quad (3)$$

where grp indexes high- and low-risk students. We include the same set of year fixed effects as in the matched control design and interact the school fixed effects with an indicator for being high-risk.

The results are shown in Figure VIII, Panel *A*. In the pre-period, the gap between the two groups was flat or slightly widening. After CIS adoption, this trend reverses: high-risk students begin to catch up, and by $\tau = 5$, the within-school gap has closed by 0.09 standard deviations—about 6% of the gap at $\tau = -1$. This shrinking within-school gap strengthens our interpretation that CIS drives improvements for high-risk students, rather than broader school-level changes affecting all students. In contrast, when we repeat this analysis for control schools (Panel *B*), we observe the opposite pattern: gaps between high- and low-risk students continue to widen over time. This suggests that the narrowing of test score gaps in treated schools does not reflect a broader trend toward reduced inequality, but rather a specific effect of CIS program adoption.

Taken together, these results provide consistent evidence that CIS improves academic outcomes for high-risk students. Across all specifications, we find positive effects for high-students and no gains for their low-risk peers. These finding suggest that CIS delivers targeted academic benefits to the students most likely to be served the program. Moreover, the narrowing of achievement gaps within schools following CIS adoption reinforces the interpretation that the program drives these improvements, rather than broader shifts in school composition or environment. In the next section, we examine whether these short-run gains in test scores translate

schools based on modal transitions from their first observed elementary school.

into improvements in long-run educational outcomes.

V.B Impacts on Long-Run Outcomes in Texas

In addition to improving academic performance, CIS aims to increase high school graduation and economic opportunity, particularly for students at risk of dropping out. In this section, we examine whether the gains in test scores for high-risk students translate into improvements in students' long-run outcomes. We focus on high school graduation as a key intermediate milestone, followed by post-secondary enrollment, and labor market outcomes.

Unlike test scores and other contemporaneous outcomes, long-run outcomes are only observed once per student. As a result, our empirical design must shift from capturing year-specific exposure to capturing a student's cumulative exposure to the CIS program. Rather than defining a single implementation cohort for each school, $T(s)$, as in Section IV A, we define a grade-specific cohort, $T(s,g)$, as the first year in which students in school s and grade g are ever exposed to CIS during any part of middle school.

As an example, consider a middle school that adopts a CIS program in 2005. The students who were in 8th grade in 2004 complete middle school before the program begins and are therefore never treated. In contrast, the 7th graders in 2004 are exposed to CIS during their 8th grade year, and the 6th graders in 2003 (and 2004) are exposed in 7th (and 8th) grade. In this case, we define $T(s,8) = 2005$, $T(s,7) = 2004$, and $T(s,6) = 2003$. This cohort-specific adjustment allows us to estimate the following grade-specific version of Equation 1 that accounts for cumulative exposure to CIS during middle school:

$$y_{s,T(s),t,g,r,m} = \sum_{\tau \in [-5,5]; \tau \neq -1} \beta_\tau \cdot (\mathbf{1}[t - T(s,g) = \tau] \cdot CIS_s) + \lambda_{s,T(s),g,r,m} + \gamma_{t,T(s),g} + \varepsilon_{s,T(s),g,t,r,m}. \quad (4)$$

This approach is analogous to estimating treatment effects using a single-grade sample (e.g., restricting to students observed in 8th grade), and stacking separate single-grade specifications into a single, pooled regression framework. Importantly, our results are robust to restricting the sample to 8th graders only.

We begin by estimating an adapted version of the first stage, measuring cumulative exposure to CIS in elementary, middle, and high school, as well as total exposure across grades. As our identifying variation is driven by programs that begin in middle schools, it is reassuring that differences in exposure are concentrated at that level. In later years ($\tau > 3$), students in treatment schools receive only modest additional CIS exposure in elementary school—on average, 0.15 more years, or just 6% of the treatment–control difference in middle school exposure—largely due to nearby elementary schools adopting CIS in subsequent years. Importantly, there is no difference in

high school exposure between treatment and control students. These patterns, shown in Appendix Figure A.5, support the interpretation that our estimated effects are primarily driven by exposure during middle school. To account for the small differences in elementary exposure, we report IV estimates using total cumulative exposure across all grades.

Next, we estimate the effect of CIS on high school graduation, separately for high- and low-risk students. Results for high-risk students are shown in Figure IX Panel *A*. As with test scores, we find positive treatment effects: pre-period coefficients are near zero, and graduation rates rise following CIS adoption. We estimate a difference-in-differences effect of 3.1 percentage points (S.E.=1.3) Rescaling for 3 years of total exposure, we find that CIS increases graduation rates by 3.4pp. This represents a 5.2% increase over the high-risk baseline graduation rate of 65%, and closes 13% of the pre-treatment gap in graduation rates between high- and low-risk students.

As with test scores, we find no corresponding increase in graduation rates for low-risk students. Figure IX Panel *B*, shows slightly negative pre-trends that persist in the post-period. We estimate a small decline of 1.4 percentage points (S.E. = 0.9pp) off a high baseline graduation rate of 90.8%. While we cannot reject a null or small positive effect for low-risk students, we can reject impacts as large as those observed for high-risk students. When we repeat the analysis across the full risk distribution, we again find a monotonic pattern: treatment effects are largest for high-risk students and decline steadily across quartiles.

Together, these results show that CIS improves high school graduation rates—delivering on one of the program's central goals. The effects are concentrated among the students most likely to be served by the program, mirroring the pattern observed for test scores. By providing high-risk students with targeted support during middle school, CIS meaningfully alters their educational trajectory, leading to lasting improvements even after students leave the program's direct reach.

We next test whether the gains in graduation rates translate into increased post-secondary enrollment in Texas. We focus on 2-year college attendance, since students on the margin of graduating high school are more likely to attend 2-year rather than 4-year institutions. Figure IX Panel *C* shows the results for high-risk students. The pattern resembles that of test scores and graduation: coefficients are flat in the pre-period and rise modestly after CIS adoption, though the estimates are noisier. We estimate a 2.6 percentage point increase in the share of high-risk students who ever enroll in a 2-year college between ages 18 and 22, but the standard error is large (2.5pp), and the effect is not statistically significant. In contrast, we find no increase for low-risk students and instead find a modest, noisy decrease (Panel *D*).

Consistent with 2-year college being the primary margin of response, we find little evidence of effects on other types of postsecondary enrollment. For high-risk students, the estimated effect on attending any institution in the Texas Higher Education Coordinating Board (THECB) data is nearly identical to the effect on 2-year enrollment (2.67pp vs. 2.55pp). Only 3.1% of high-risk

students who attend any postsecondary institution fail to attend a 2-year college, reinforcing that 2-year colleges capture the vast majority of postsecondary participation for this group. By contrast, only 8.9% of high-risk students attend a 4-year college in the pre-period, and we find no impact of CIS on this outcome. Low-risk students attend 4-year colleges at much higher rates (45%) but also show no treatment effect (DID = –0.07pp).

We have shown that CIS increases educational attainment for high-risk students, both by raising high school graduation rates and through suggestive increases in 2-year college enrollment. To assess whether these educational gains translate into improved economic outcomes, we next examine impacts on earnings in early adulthood. Estimating earnings effects in our current Texas-based design requires a long panel: we must observe students in elementary school to predict risk and follow them for multiple decades to measure earnings at a meaningful age. For example, a student in first grade in 1995—the first year of our education data—turns 27 in 2016, near the end of our available earnings records. To overcome this limitation, we turn to a second design based on CIS programs outside of Texas, using linked Census and IRS data to measure labor market outcomes.

V.C Impacts on Earnings National Census/IRS Data

We estimate the impact of CIS on earnings using national Census and IRS data. This shift in data and setting requires several changes to our empirical approach. First, we focus on CIS programs outside of Texas, which allows us to test whether the program's impacts generalize beyond the Texas context. Second, we adopt a staggered-adoption design that compares cohorts within the same neighborhood who differ in their exposure to CIS, rather than comparing treated and matched control schools. Finally, because the national data do not contain detailed early-life measures, we do not distinguish between high- and low-risk students in this analysis, though we explore heterogeneity by parent income.

A key challenge in the Census-IRS data is that we do not observe which schools children attend. Instead, we rely on addresses to map between neighborhoods and school catchment zones. We assign children to neighborhoods based on the first Census tract in which they are claimed by their parents.[16] We then map each tract to the set of schools that contain the tract in their catchment zones following the approach in Chetty et al. (2018).[17]

Unlike in Texas, we do not focus on CIS programs in middle schools. Instead, we use programs at all levels of school and map CIS exposure to birth cohorts. For each tract ever served

[16]This is analogous to the approach we take in Appendix Figure A.4, assigning students to schools based on their first elementary school, and helps to limit concerns of endogenous sorting into schools and neighborhoods with CIS programs.

[17]We use the same crosswalk between school catchment zones and Census tracts as in Chetty et al. (2018), provided by Peter Bergman and originally from Maponics (2017).

by CIS, we identify the first birth cohort exposed to CIS, based on the timing of the program's introduction. For example, if a tract contains both an elementary school that adopted CIS in 2002 and a middle school that adopted in 2000, we define the first treated cohort as the 8th graders enrolled in the middle school in 2000—primarily students born in 1987. We denote this first exposed cohort in tract n as the tract's implementation cohort, $C(n)$, analogous to our approach in Section IV.

We implement a staggered adoption design, comparing outcomes for children in tracts exposed to CIS earlier versus later. For each implementation cohort $C(n)$, we define the control group as the set of tracts where CIS is introduced more than five birth cohorts later—that is, all n' such that $C(n') > C(n) + 5$.[18] For these later-treated control tracts, we assign a pseudo-implementation cohort $\tilde{C}(n') = C(n)$, matching the implementation cohort of the treated group they are compared to. We then stack the data across implementation cohorts following the approach of Sun and Abraham (2021), fully interacting all specifications with indicators for the implementation cohort. This structure allows later-treated tracts to serve as controls for multiple earlier implementation cohorts. We cluster standard errors at the high school catchment level.

Concretely, we estimate:

$$y_{i,\tilde{C}(n(i))} = \sum_{\tau \in [-5,5]; \tau \neq -1} \beta_\tau \cdot (\mathbf{1}[c(i) - \tilde{C}(n(i)) = \tau] \cdot CIS_{n(i)}) + \phi_{n(i),r(i),p(i),tildeC(n(i))} + \delta_{c(i),r(i),p(i),tildeC(n(i))} + \varepsilon_{s,T(s),t,r,m}, \quad (5)$$

where y_i is an adult outcome for individual i, who grew up in Census tract $n(i)$, was born in cohort $c(i)$, belongs to race/ethnicity group $r(i)$, and whose parents are in income quartile $p(i)$. The term $\tilde{C}(n(i))$ denotes the implementation cohort for the treated tract and the pseudo-implementation cohort for the matched control tract.

This structure closely parallels our baseline specification in Equation 1, adapted to a cross-cohort design. The fixed effects ϕ absorb neighborhood-by-demographic variation within each implementation cohort, analogous to the school-by-demographic fixed effects in Texas. The δ terms control for cohort-by-demographic shocks within implementation cohorts, analogous to year fixed effects. The coefficients β_τ trace the path of estimated treatment effects from five cohorts before to five cohorts after the CIS program begins in treated tracts.

We begin by estimating the first stage in this setting. Specifically, we estimate Equation 5 using the total number of years of exposure to CIS as the dependent variable. The resulting event study coefficients are shown in Figure X, Panel *A*. The pattern is similar to the one shown in

[18] This restriction ensures that the sample remains balanced across all relative time periods in the event study window. In Section A and Figure VI, we do not impose this restriction. However, applying it here does not drive the results: the unbalanced panel yields similar conclusions.

Figure IV: exposure begins to rise after the program is introduced and then levels off.

To interpret the magnitudes, consider the coefficient at event time $\tau = 0$, corresponding to the first cohort exposed to CIS. If no students moved away from their initial Census tract, this coefficient would mechanically equal 1.[19] Instead, we estimate a coefficient of 0.51, reflecting migration during childhood. Importantly, the maximum potential years of exposure in this design is not capped at three years, as in our Texas middle school setting, but can be somewhat higher depending on the number and levels (elementary, middle, high school) of schools in the neighborhood that adopt CIS. In practice, we estimate that children born 3–5 years after the implementation cohort—i.e., cohorts with the longest potential exposure—experience an average of 2 additional years of CIS exposure relative to children in later-treated control tracts.

Does this exposure translate into improved outcomes later in life? Figure X Panel *B* plots the effect of CIS on adult household income rank measured at age 27. The estimated coefficients in the pre-period are close to zero and statistically insignificant, suggesting that treatment and control tracts were on parallel trajectories before CIS was introduced. In the years following implementation, children in the treated tracts begin to earn more than their peers in later-treated control tracts. Estimating the difference-in-differences analog of Equation 5, we find that CIS improves adult income ranks by 1.14 ranks (S.E. = 0.37) for students born 3-5 years after the implementation cohort, corresponding to a 3.2% increase in income—about $850 annually—relative to a pre-period average of $26,270. Rescaling by the first stage, we estimate that 3 years of exposure to CIS programs increases earnings at age 27 by 1.54 percentile ranks (S.E. = 0.45), equivalent to a $1,140 increase (4.3%) in annual earnings.

Although the earnings results are estimated using CIS programs outside of Texas, we do not believe these impacts are unique to that setting. As described in Section B, our Texas panel is not long enough to observe earnings ranks at age 27 directly. However, we can predict later-life earnings using earlier observed outcomes. We estimate a machine learning model—similar to the one used to predict case management in Section IV—to forecast income rank at age 27 based on earnings ranks and college attendance between ages 18 and 22 (Athey et al., 2019; Chetty, Deming and Friedman, 2023). See Appendix Figure A.8 for details. Using these predictions as the dependent variable, we then estimate Equation 4. The results closely mirror those from Figure X (see Appendix Figure A.7 for a direct comparison), suggesting that the earnings gains from CIS are not specific to one location or sample.

In the Texas sample, we found large differences in treatment effects for children who more versus less likely to be intensely served by CIS. Although we cannot construct the same predictions

[19] We measure exposure assuming that students attend their local schools, though this is not always the case. Chetty et al. (2018) use the same tract-to-school crosswalk and show in North Carolina that it reliably predicts actual school attendance. Deviations from local attendance would attenuate the first stage, making our estimates an upper bound for exposure and any IV estimates a lower bound for the treatment effect.

of high- and low-risk in the national sample, we examine heterogeneity by parent income as a proxy for student need. Specifically, we estimate difference-in-differences treatment effects separately for children whose parents were in the top quartile (lower need) and bottom quartile (higher need) of the national income distribution. The broadly results mirror the patterns observed in Texas: we find a 1.14-rank increase in adult income (S.E. = 0.51) for children from low-income families, and no detectable effect for students from high-income families (–0.02 ranks, S.E. = 0.80), though the estimates are imprecise.[20] Mirroring our earlier findings, these results suggest that CIS exposure during childhood continues to benefit children with greater needs well into adulthood.

We find that increases in earnings for low-income children are driven primarily by greater employment in adulthood, rather than increases in earnings conditional on working. To investigate this, we construct four binary outcome variables indicating whether each child is in a given adult income quartile at age 27. We estimate difference-in-differences treatment effects on the probability of falling into each quartile. As shown in Figure XI, exposure to CIS reduces the likelihood of being in the bottom quartile by 2.3 percentage points (S.E. = 1.1pp) and increases the likelihood of being in the second quartile by 1.7 percentage points (S.E. = 1.0pp). We find no significant effects on the probability of being in the top half of the adult income distribution. The bottom quartile comprises children earning less than $15,000 annually, but 80% of these children are not working. We cannot reject that the entire effect on bottom-quartile incidence is driven by changes in employment, proxied by the presence of W-2 earnings.

These patterns are consistent with the education effects documented in the Texas sample. CIS increases high school graduation rates for students in need and modestly boosts 2-year college attendance, but does not shift students into four-year college pathways. The earnings effects we document of $1,140 are approximately 17% of the returns to a four-year degree (Zimmerman, 2014), yet they remain substantial. Following the frame work in Hendren and Sprung-Keyser (2020), we project that three years of exposure to CIS during childhood increases lifetime earnings more than $75,000, expected to generate an increase in future federal tax revenue of approximately $7,100 (present day value) per student.

The long-run earnings gains we estimate are sizable, but such impacts take decades to materialize—and by the time they are measurable, it is often too late to guide program design or policy decisions. This raises a natural question: could we have forecast these long-run effects using outcomes observed earlier in life? In the next section, we return to the Texas data to examine the extent to which effects on short-run outcomes explain the long-run effects we document.

[20] Unlike in Texas, where treatment effects decreased monotonically across risk groups, we observe somewhat larger effects among children from middle-income families, but we have limited power to compare effects across subgroups.

VI Mechanisms: Short-Run Outcomes as Statistical Surrogates

In the prior section, we showed that exposure to CIS improves outcomes across the course of a student's life. Test scores rise soon after the program begins, high school graduation rates increase, and those same students go on to earn more in adulthood—often a decade or more after their initial exposure. But how much of this long-run impact can be attributed to CIS's effect on short-run outcomes? In other words, to what extent do improvements in academic performance, attendance, or behavior mediate the gains we observe later in life?

It is common practice to forecast long-run impacts by scaling up short-run effects on test scores (e.g., Guryan et al., 2023; Hanushek, 2011; Kline and Walters, 2016; You, Fu and Chou, 2025). For example, one could take the estimated effect of CIS on test scores from Section IV.A and extrapolate the corresponding impact on adult earnings using established relationships (e.g., Chetty, Friedman and Rockoff, 2014). This approach implicitly assumes that test scores are a valid *statistical surrogate* for long-run outcomes—that is, that the full effect of CIS operates through its impact on test scores (Athey et al., 2019). However, the projections will not be accurate if the surrogacy assumption is violated.

On one hand, projections based on test scores could overstate long-run effects. A large literature documents fade-out, where early gains in academic outcomes diminish over time (see Cascio and Staiger, 2012, for a review). For example, improvements in test scores might reflect increased teaching to the test or changes in classroom practices that boost scores without meaningfully improving long-term skills. Even if gains are genuine, they may be offset by later constraints—such as limited access to college counseling or labor market networks—that CIS does not fully address. In these cases, rescaling based on short-run test score impacts would predict larger long-run effects than are ultimately realized.

On the other hand, projections based solely on test scores may understate the true long-run effects of CIS. This pattern has been documented in the context of several educational interventions, where modest impacts on test scores masked much larger improvements in adult outcomes (e.g., Anders, Barr and Smith, 2023; Chetty et al., 2011; Cortes, Goodman and Nomi, 2015; Gray-Lobe, Pathak and Walters, 2023). In such cases, test scores do not fully capture the dimensions of student development affected by the intervention. For example, long-run gains could additionally reflect changes in non-cognitive skills, engagement, motivation, or access to support systems that are not reflected in improved test scores. If CIS improves outcomes through these or other channels, then test scores alone may provide an incomplete picture of its true long-term impact.

Can we accurately predict the long-run effect of CIS using short-run outcomes? We investigate this question in the context of high school graduation. To do so, we construct predicted

graduation outcomes using students' contemporaneous academic and behavioral indicators, and then compare the treatment effect of CIS on these predicted outcomes to the actual long-run effect.

We train a gradient boosting algorithm separately in grades 6–8, using data on all students, not just those in schools with CIS programs. The model includes a set of year-specific covariates: student demographics, standardized math scores, attendance, and disciplinary outcomes (including in- and out-of-school suspensions and the number of infractions). These predictors capture both academic outcomes (e.g., test scores) and behavioral outcomes (e.g., attendance and discipline), which may reflect non-cognitive traits relevant for long-run success. All predictors are measured contemporaneously, so the model output can be viewed as a composite measure of academic and behavioral performance in each year, with weights determined by how strongly each variable predicts high school graduation in the broader student population. See Appendix Figure A.9 for details.

We then estimate the impact of CIS on predicted high school graduation rates for high-risk students using our standard event study framework. The results are shown in Figure XII Panel *A*, alongside the actual effect on high school graduation for comparison. The patterns are remarkably similar: the increase in predicted graduation rates following CIS implementation closely mirrors the observed increase in true graduation rates.

In Figure XII Panel *B*, we present the difference-in-differences for these two outcomes. We estimate a true effect of CIS on high school graduation of 3.1 percentage points (S.E. = 1.2 pp), compared to a predicted effect of 2.4 percentage points (S.E. = 0.8 pp). In other words, nearly 80% of the true impact on high school graduation can be explained by the effect on observable, contemporaneous outcomes.

Had we only used test scores, we would have substantially understated the effect of CIS on high school graduation. The results of two procedures are shown on the right hand side of Figure XII Panel *B*. In the first, we use the observational relationship between test scores and high school graduation rates. We fit a regression of graduation rates on test scores in each grade using statewide data and use that to predict graduation. In the second, we allow for a more flexible, possibly non-linear relationship by training a simplified gradient boosting model that uses only test scores as inputs. In both cases, the predicted impacts are substantially smaller: the predicted impacts from the GBM model are only 43% as large as the true effect.

More broadly, our findings highlight the potential of using rich, short-run administrative data to forecast long-run program impacts in real time. Test scores—and the cognitive skills they reflect—explain a meaningful share of CIS's long-run effects, but alone they understate its full impact. Behavioral measures like attendance and discipline also play a critical role. By combining these outcomes using only existing administrative records, we are able to predict long-run effects with a high degree of accuracy. This approach shows promise for evaluating interventions without

waiting years or decades for follow-up data (Prentice, 1989; Athey et al., 2019).

VII Conclusion

Students from low-income families face persistent disadvantages in school and beyond (Chetty et al., 2014; Solon, 1992; Black, Devereux and Salvanes, 2005; Chetty et al., 2020). We show that providing personalized, coordinated supports in high-poverty schools can meaningfully improve outcomes– both in the classroom and over the long run. Communities in Schools (CIS) the largest integrated student support program in the country, raises test scores, boosts high school graduation, increases college enrollment, and improves early-career earnings for students most at risk of falling behind. These impacts are not driven by a single academic channel, but by improvements in both cognitive and non-cognitive outcomes.

Our results suggest that social connection—through a dedicated navigator—can significantly improve children's outcomes, echoing findings from programs targeting adults in other domains (Bell and Petkova, 2024; Bergman et al., 2019; Caspi and Rafkin, 2023; Espinosa et al., 2024; Finkelstein and Notowidigdo, 2019; Katz et al., 2022; Weiss et al., 2019). In schools, this relational model not only students' outcomes, but does so in a relatively cost effective way. We estimate that each $1,000 invested in CIS raises adult earnings by approximately $406, compared to an estimated $40 per $1,000 from class-size reductions (Chetty et al., 2011).[21]

Although CIS now serves over 2 million students nationwide, our findings suggest there is still considerable scope for expansion—even in a state like Texas, where coverage is already high. We find that the benefits of CIS are concentrated among the most disadvantaged students, while effects are minimal for their more advantaged peers. This pattern provides a natural framework for guiding future expansions. In Figure XIII, we simulate a targeted rollout strategy that prioritizes schools with the largest concentrations of high-risk students. While CIS programs currently reach about 20% of middle school students in Texas, we estimate that the current allocation captures only 60% of the potential impact achievable at that coverage level. Expanding to 40% coverage—focusing on the next-highest-need schools—could achieve 92% of the maximum attainable impact on high-risk students' graduation rates. Our findings point to the potential for substantial additional gains from targeting intensive supports and personalized assistance to the students who need them most.

[21] Chetty et al. (2011) estimate that 2.14 years of smaller class sizes raise earnings in adulthood by $368 and cost $9,355 per student. We estimate that 3 years of CIS raises earnings by $1,140. Although we do not observe costs directly, we cite Texas Legislative Budget Board (2016), which estimates conservatively that CIS costs $936 per case-managed student. Assuming that is equal to the cost per student per year, we estimate that $1,000 spent on CIS increases adult earnings by $406.

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TABLE I: Summary Statistics

	All Students (1)	Ever in School with CIS Program (2)	Ever in MS with CIS Program (3)
A. Student Characterisitcs			
Fraction Male	0.520	0.514	0.495
Fraction White	0.360	0.262	0.239
Fraction Hispanic	0.454	0.551	0.591
Fraction Black	0.142	0.157	0.143
Fraction Other Race/Ethnicity	0.044	0.030	0.028
Fraction Student-Years FRPL	0.470	0.522	0.515
Fraction Stydent-Years Dropout-Risk	0.450	0.497	0.511
B. Student Outcomes			
Standardized Math Score in 6th Grade	0.004	-0.133	-0.156
Absence Rate in 6th Grade	0.037	0.039	0.040
Fraction Ever Receive In-School Suspension in 6th Grade	0.129	0.169	0.169
Ever Ever Receive Out-of-School Suspension in 6th Grade	0.061	0.087	0.090
Fraction Ever Graduate from HS	0.738	0.732	0.727
Fraction Ever Enroll in TX College Ages 18-22	0.501	0.477	0.479
Fraction Appear in Earnings Data	0.820	0.846	0.833
Average Earnings at Age 27 ($)	24812	23818	23959
Employment Rate at Age 27	0.605	0.624	0.609
Average Individual Earnings Rank at Age 27	0.497	0.490	0.488
C. School Characteristics			
Average Number of Students	991	1368	903
Average Number of Teachers	65	91	61
Fraction Teachers Male	0.229	0.309	0.312
Fraction Teachers White	0.670	0.610	0.582
Fraction Teachers Hispanic	0.217	0.294	0.306
Fraction Teachers with Advanced Degree	0.266	0.280	0.258
Average Teacher Tenure (yrs)	7.643	7.584	7.428
Average Teacher Pay ($)	51586	52761	52027
Number of Student-Years	109172	27222	31094
Number of Students	16165	3332	1639
Number of Students in HS Grad Sample	9714	2248	2220
Number of Students in College Sample	8311	1907	1822
Number of Students in Earnings Sample	5960	1351	1228
Number of Schools	10682	613	416
Number of School-Years	189748	12941	8730

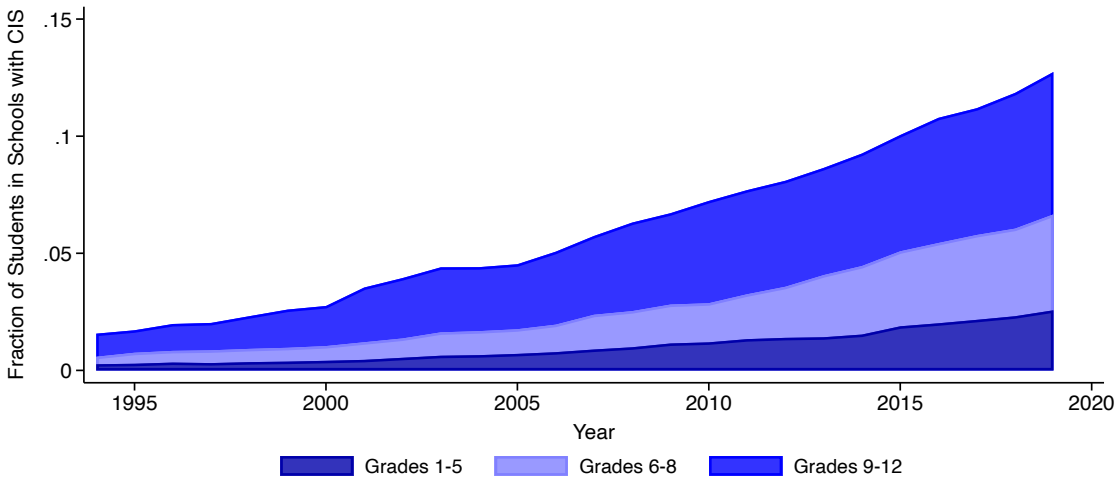
Notes: This table reports summary statistics on three groups: (1) all students in our data, (2) students who ever attend a school that has a CIS program in 2019, and (3) students who ever attend a middle school that has a CIS program in 2019. All statistics are constructed using the maximum available data from the years 1994 to 2019. Panel A provides statistics on student demographic characteristics. Panel B provides statistics on student outcomes. High school graduation is estimated among students who are 19 or older in 2019. College enrollment is estimated among students who are 22 or older in 2019. The fraction of students in the earnings data is the fraction of students with non-zero earnings at some point between ages 18 and 25. The employment rate is the fraction of students with non-zero earnings at 27, conditional on being in the earnings data. All earnings statistics are calculated among students who are 27 or older in 2019. Panel C provides statistics on school characteristics.

TABLE II: Characteristics of Low- and High-Risk Students

	Low-Risk in CIS School (1)	High-Risk in CIS School (2)	Low-Risk in Non-CIS School (3)	High-Risk in Non-CIS School (4)
A. Student Characterisitcs				
Fraction Male	0.584	0.363	0.584	0.362
Fraction White	0.549	0.130	0.685	0.226
Fraction Hispanic	0.353	0.603	0.175	0.509
Fraction Black	0.019	0.252	0.029	0.255
Fraction Other Race/Ethnicity	0.079	0.015	0.110	0.010
Fraction Student-Years FRPL	0.100	0.763	0.068	0.766
Fraction Student-Years Dropout-Risk	0.159	0.713	0.116	0.675
B. Student Outcomes				
Standardized Math Score in 6th Grade	0.717	-0.806	0.786	-0.646
Absence Rate in 6th Grade	0.028	0.051	0.027	0.044
Fraction Ever Receive In-School Suspension in 6th Grade	0.057	0.242	0.045	0.197
Ever Ever Receive Out-of-School Suspension in 6th Grade	0.028	0.145	0.015	0.091
Fraction Ever Graduate from HS	0.864	0.557	0.858	0.609
Fraction Ever Enroll in TX College Ages 18-22	0.669	0.333	0.757	0.328
Fraction Appear in Earnings Data	0.802	0.786	0.854	0.814
Average Earnings at Age 27 ($)	35828	17503	35168	15290
Employment Rate at Age 27	0.604	0.572	0.655	0.599
Average Individual Earnings Rank at Age 27	0.573	0.436	0.581	0.417
Number of Student-Years (1,000s)	3645	6077	19977	17475
Number of Students in HS Grad Sample	199	298	1124	1014
Number of Students in College Sample	151	249	915	787
Number of Students in Earnings Sample	91	145	563	431

Notes: This table reproduces the summary statistics in Table I, separately for students who are high- versus low-risk and who attend CIS versus non-CIS schools. High-risk students are those in the top quartile of the case management prediction model described in Section IV. See the notes to Table I for additional details on the construction of these statistics.

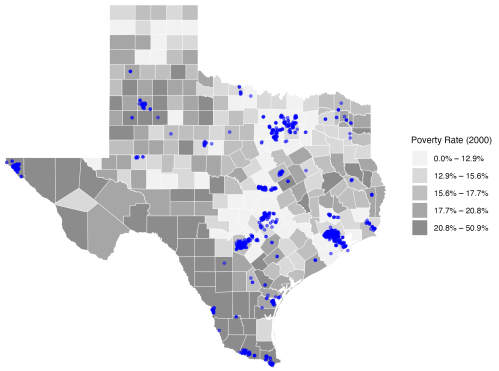
FIGURE I: CIS Program Rollout Across Texas



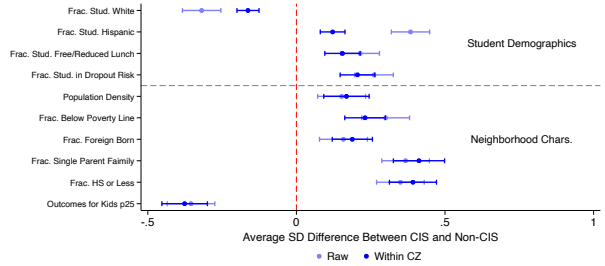
Notes: This figure plots the share of all Texas students enrolled in schools with a CIS program from 1994 to 2019. For each year, we identify the schools with an active CIS program and calculate the number of students enrolled in those schools divided by the total statewide student enrollment. We decompose this total by school level—elementary (grades 1–5), middle (grades 6–8), and high school (grades 9–12)—based on each school's predominant grade span. The shaded regions show the fraction of all Texas students enrolled in CIS elementary, middle, and high schools, respectively, so that the vertical height of each band reflects the share of the statewide student population served by CIS at that level.

FIGURE II: CIS Location Characteristics

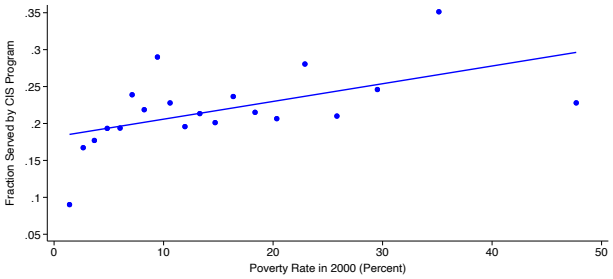
A: Map of CIS Schools in Texas and County-Level Poverty Rates



B: School and Neighborhood Correlates of CIS Programs



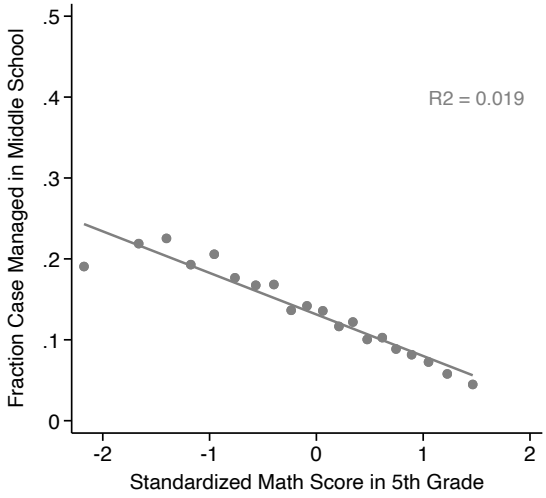
C: Relationship Between CIS Presences and Census Tract-Level Poverty Rates



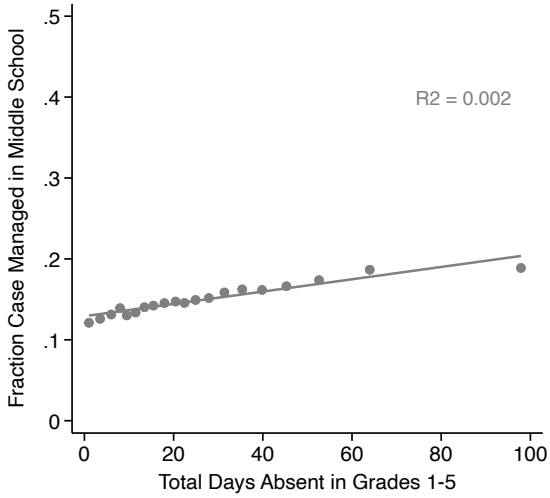
Notes: This figure presents descriptive evidence on the locations and characteristics of CIS programs in Texas. Panel A overlays the locations of CIS programs as of 2019 onto a county-level map of Texas, with counties shaded according to poverty rates from the 2000 Census. Panel B plots differences in school and neighborhood characteristics between schools with and without CIS programs. The light blue series shows raw differences in standard deviation units, while the dark blue series restricts comparisons to schools within the same commuting zone. Student demographic characteristics are measured in the using TEA administrative data described in Section IVA. Neighborhood characteristics are drawn from the 2000 Census and assigned to schools using school catchment zones following Chetty et al. (2018). Panel C presents a binned scatterplot of the relationship between 2000 Census tract poverty rates and CIS program coverage in 2019. To construct the figure, we divide Census tracts into 20 equal-sized bins by poverty rate and plot the fraction of tracts in each bin that are served by a CIS program against the average poverty rate in the bin, along with a line of best fit.

FIGURE III: Predicting CIS Case Management

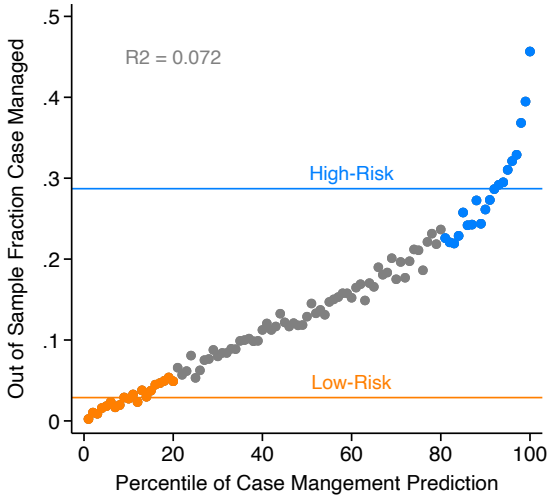
A: Relationship with Test Scores



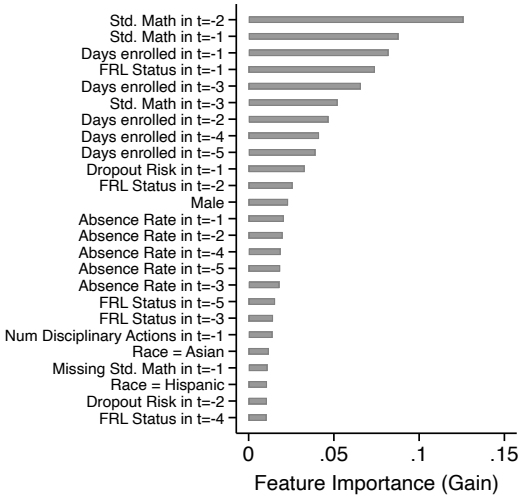
B: Relationship with Attendance



C: Out-of-Sample Accuracy of Case Management Prediction

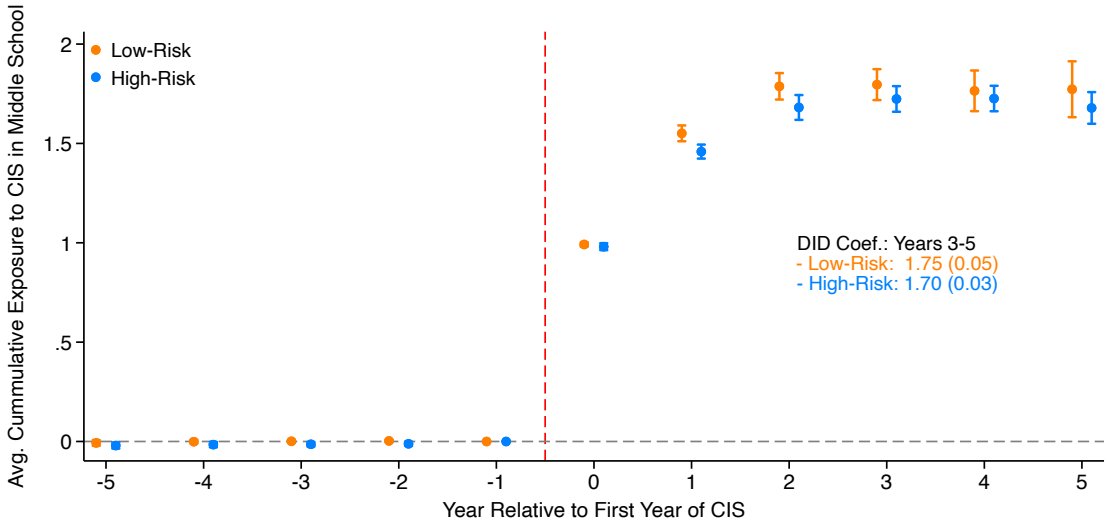


D: Feature Importance – Model Gain



Notes: This figure summarizes the empirical prediction of CIS case management in middle school using pre-treatment student characteristics measured in elementary school. The plots are constructed using a 20% hold-out test set not included in model training, as described in Section IV. Panel A shows a binned scatterplot of the relationship between standardized 5th grade math scores and the probability of being case managed. See the notes to Figure II for details on the construction of the binned scatter plots. Panel B shows the analogous relationship using cumulative absences in grades 1–5. Panel C reports a binned scatterplot of out-of-sample rates of case management by predicted percentile of predicted case management status, based on a gradient boosting model trained on student elementary school outcomes. We classify the top quartile of the predicted distribution as high-risk and the bottom as low-risk and refer to these groups throughout. Panel D presents the top 25 predictors in the GBM ranked by model gain. Predictor variables are measured in years relative to when students first enroll in middle school, ranging from one year prior ($t = -1$) to five years prior ($t = -5$). The model is estimated using data from middle schools with active CIS programs between 2014 and 2019. The dependent variable is an indicator for ever being case managed, as defined in Section B, and predictors include student demographics, test scores, attendance, and disciplinary history. See Section IV for additional details on the prediction model.

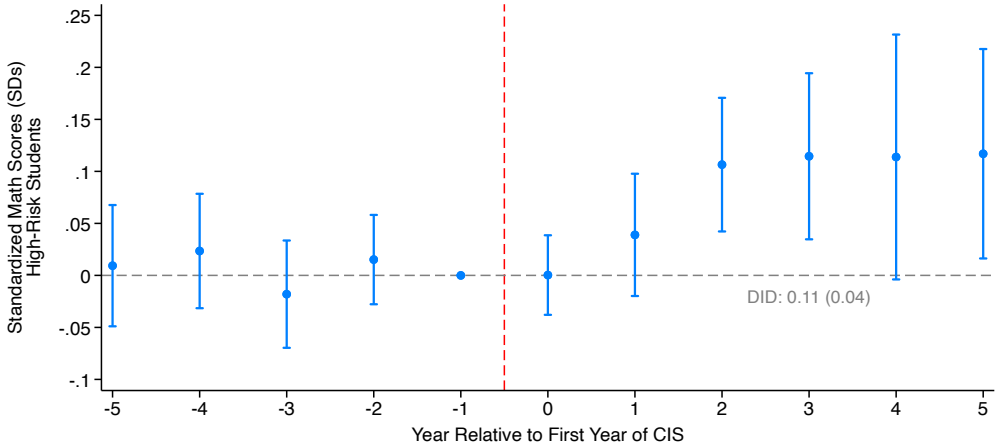
FIGURE IV: First Stage: Exposure to CIS in Middle School



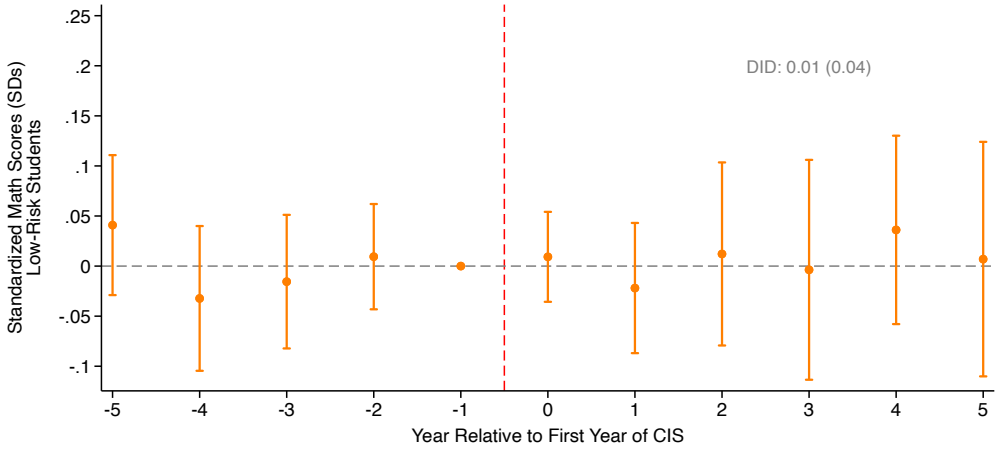
Notes: This figure plots the first stage relationship between treatment timing and cumulative exposure to CIS in middle school, separately for high-risk (blue) and low-risk (orange) students. See Section IV and the notes to Figure III for details on the assignment to groups. For each group, we estimate Equation 1, comparing treated schools to matched control schools, where the dependent variable is the number of years a student has been exposed to CIS in middle school as of year t. The figure presents event study coefficients estimated using data from 1998 to 2016. In addition to the event study, we estimate a simplified difference-in-differences specification (Equation 2) and report the average difference in CIS exposure between treated and control students over relative years 3 to 5, compared to the pre-treatment period ($\beta_{post,3-5}$). Standard errors are clustered at the school level.

FIGURE V: Reduced Form Effects of CIS on Standardized Math Scores

A: High-Risk Students



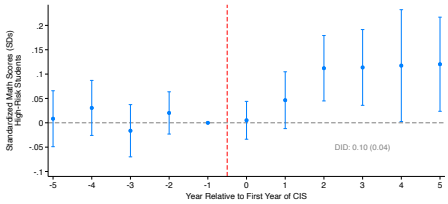
B: Low-Risk Students



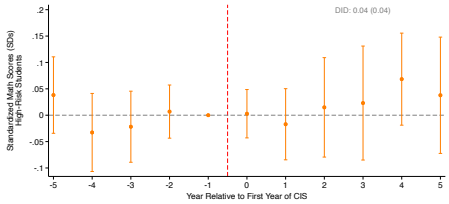
Notes: This figure presents reduced-form estimates of the effect of CIS on standardized math scores in middle school, separately for high-risk (Panel A) and low-risk (Panel B) students. See Section IV and the notes to Figure III for details on the assignment to groups. For each group, we estimate Equation 1, comparing treated schools to matched control schools, where the outcome is the standardized math score of students in year t, standardized within grade and calendar year. The figure shows event study coefficients estimated using data from 1998 to 2016. In addition to the event study, we estimate a simplified difference-in-differences specification (Equation 2) and report the average effect on test scores over relative years 3 to 5, compared to the pre-treatment period ($\beta_{post,3-5}$). Difference-in-difference coefficients for the other risk quartiles (medium-high and medium-low) as well as students with missing predictions are reported in Appendix Figure A.2. Standard errors are clustered at the school level.

FIGURE VI: Robustness of Baseline Treatment Effect

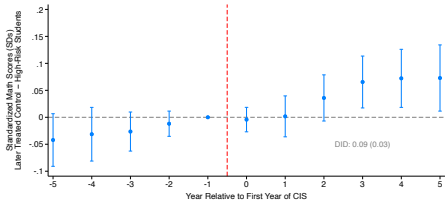
A: Flexible Time Controls by Race and Sex
High-Risk



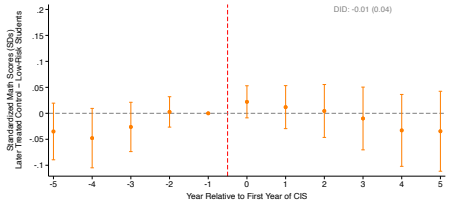
B: Flexible Time Controls by Race and Sex
Low-Risk



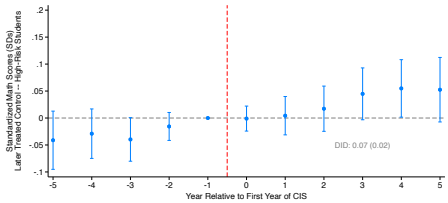
C: Alternate Design: Later Treated Schools
High-Risk



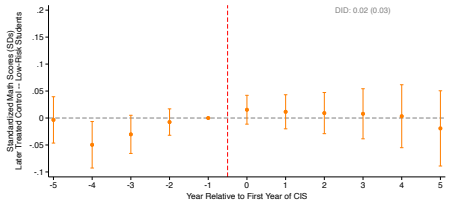
D: Alternate Design: Later Treated Schools
Low-Risk



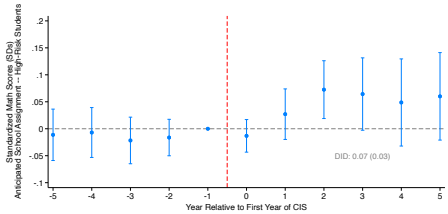
E: Within-CZ Comparison
High-Risk



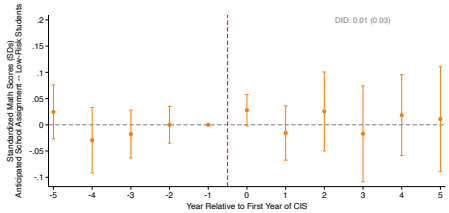
F: Within-CZ Comparison
Low-Risk



G: Assign to First School
High-Risk



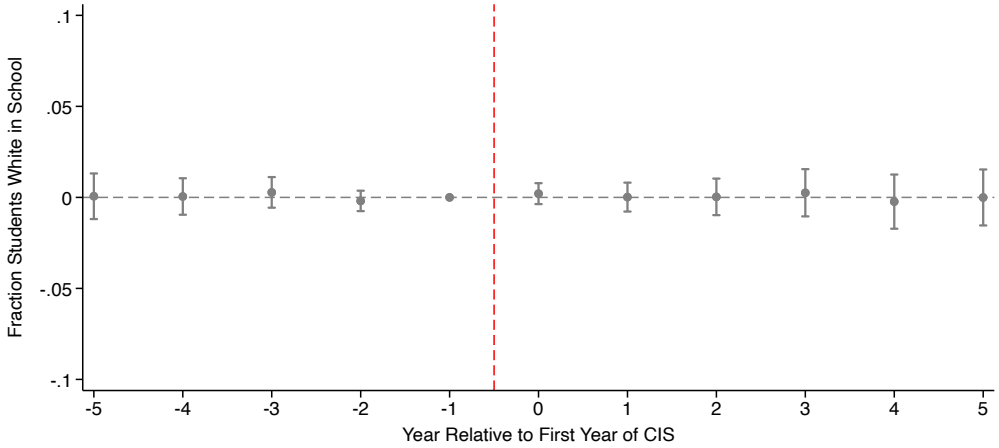
H: Assign to First School
Low-Risk



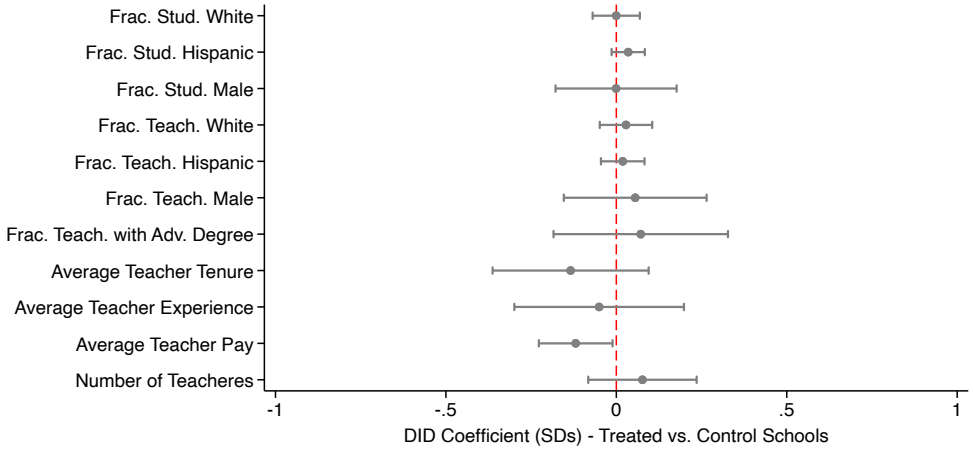
Notes: This figure presents a series of robustness checks for the baseline results shown in Figure V. Panels A and B modify the specification in Equation 1 to include year fixed effects interacted with race/ethnicity and sex, separately for high- and low-risk students. Panels C (high-risk) and D (low-risk) adopt a staggered adoption design in which later-treated CIS schools serve as the control group for earlier-treated schools. For each implementation cohort $T(s)$ and relative time τ, the control group is defined as the set of schools s' such that $T(s') > T(s) + \tau$. See Section V A for further details. Panels E (high-risk) and F (low-risk) extend this staggered design by interacting the year fixed effects with commuting zone fixed effects, ensuring comparisons occur within commuting zones. Panels G (high-risk) and H (low-risk) return to the matched-control design but assign students to a predicted middle school based on their first elementary school enrollment. To do so, we construct a school-to-school transition matrix by grade and birth cohort and assign students to the modal transition path. We then re-estimate Equation 1 among students predicted to appear in the treatment and control schools. See Section IV A for additional details. In all panels, standard errors are clustered at the school level.

FIGURE VII: Change in Covariates After CIS Program Begins

A: Fraction of Students White



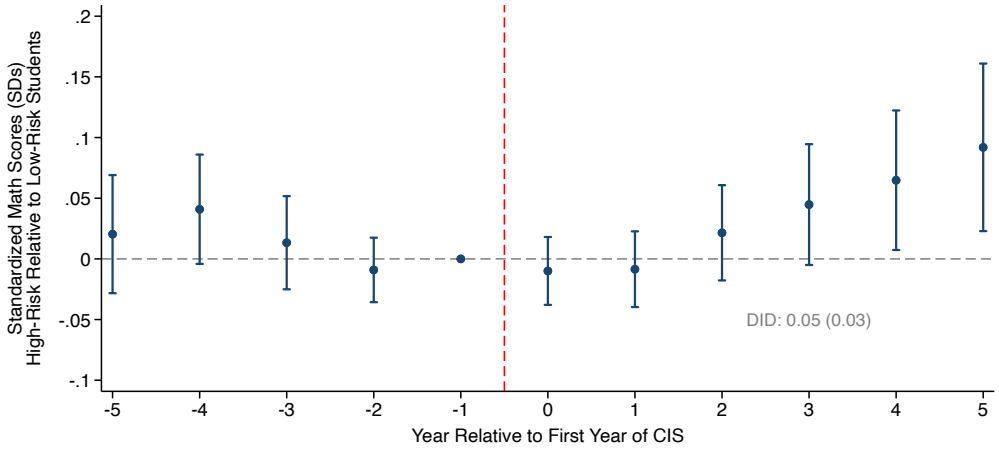
B: DID Coefficients for Student and Teacher Characteristics



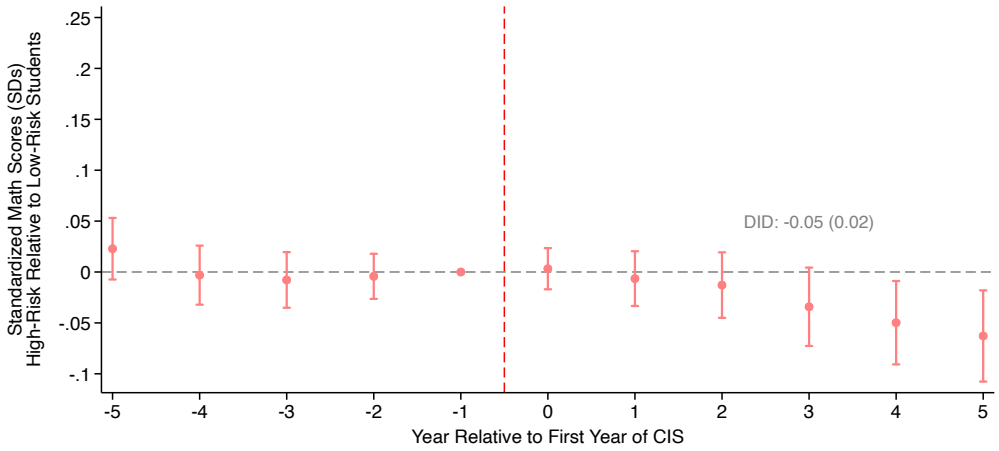
Notes:This figure assesses whether student and teacher characteristics change in treatment schools relative to matched control schools following the start of a CIS program. Panel A estimates an event study version of Equation 1, omitting race/ethnicity and sex interactions since the dependent variable—the fraction of students who are white—is not group-specific. Panel B presents results from a difference-in-differences specification (Equation 2), again excluding demographic interactions, applied to a range of student and teacher covariates. Coefficients are scaled in standard deviation units for comparability, and we report estimates of $\beta_{post,3-5}$, representing average post-treatment differences in years 3 to 5. Standard errors are clustered at the school level.

FIGURE VIII: Within-School Effects of CIS on Standardized Test Scores: High-Risk Relative to Low-Risk Students

A: Treated Schools



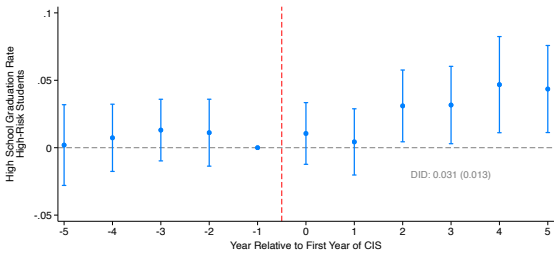
B: Control Schools



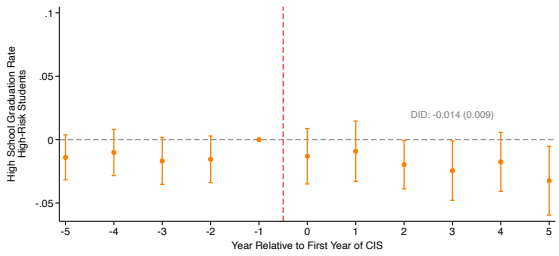
Notes: This figure presents event study coefficients estimating the relative effect of CIS on standardized test scores for high-risk students compared to their low-risk peers within the same school, estimated using Equation 3. Panel A restricts to treated schools, comparing outcomes for high- versus low-risk students as the CIS program begins. Panel B repeats the same analysis in matched control schools, where no program is implemented. The coefficients reflect differences in test score trajectories between high- and low-risk students before and after program rollout, netting out school-by-year shocks.

FIGURE IX: Effect of CIS on Long-Run Educational Outcomes

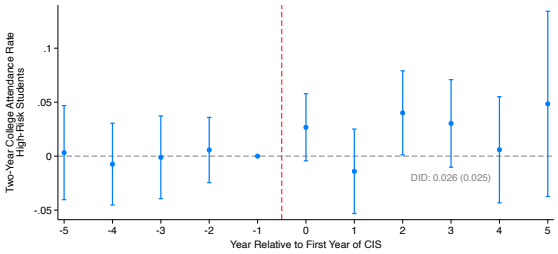
A: High School Graduation
High-Risk



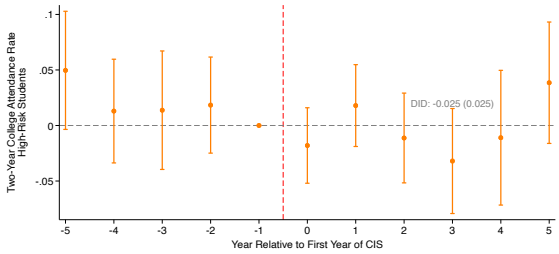
B: High School Graduation
Low-Risk



C: Two-Year College Attendance
High-Risk



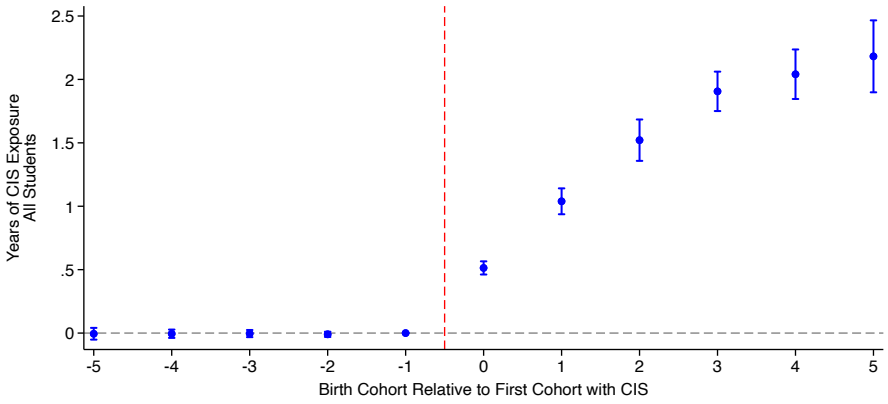
D: Two-Year College Attendance
Low-Risk



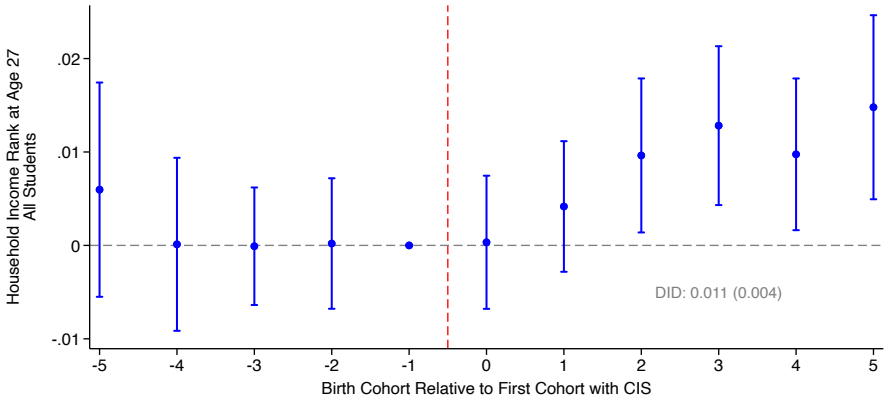
Notes: This figure presents event study coefficients estimating the effect of CIS on long-run student outcomes. Panels A and B show effects on high school graduation rates for high- and low-risk students, respectively, using data from 1998 to 2016, and estimated using Equation 4. High school graduation is defined as having graduated by 2022. Panels C and D estimate effects on 2-year college attendance, again separately by risk group. We define college attendance as an indicator for ever enrolling in a 2-year college (as recorded in THECB data) between ages 18 and 22. Standard errors are clustered at the school level.

FIGURE X: Effect of CIS Household Income Rank at Age 27
Census-IRS Sample: All Children

A: First Stage

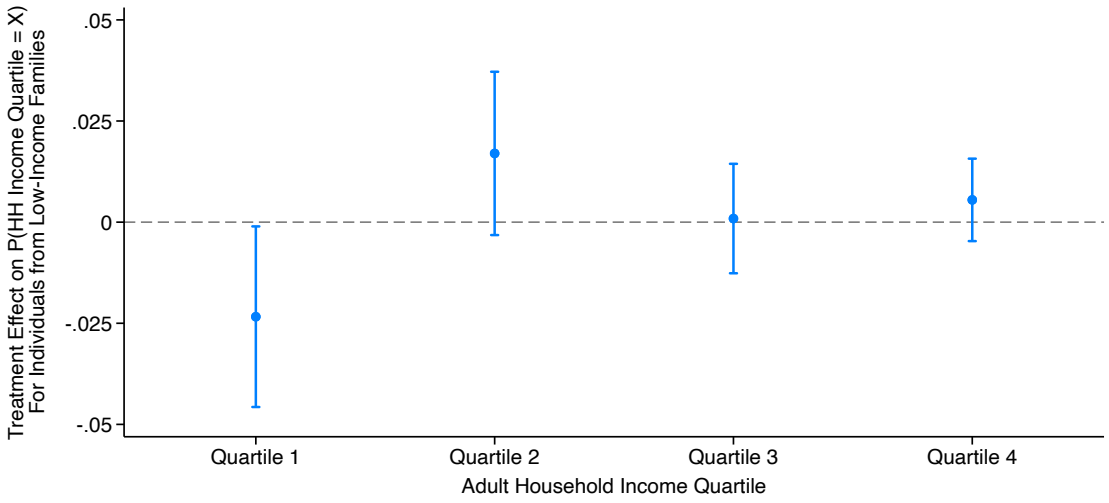


B: Reduced Form



Notes: This Figure presents the first stage and reduced form effects of CIS using the Census-IRS data described in Section III C. In both panels, we plot event study coefficients estimated from Equation 5. In Panel A, the dependent variable is the average number of years of exposure to a CIS program. In Panel B, the dependent variable is adult household income rank measured at age 27. Standard errors are clustered at the high school catchment zone boundary. Figure III from Goldman, Gracie and Porter (2023). CBDRB-FY23-CES014-028.

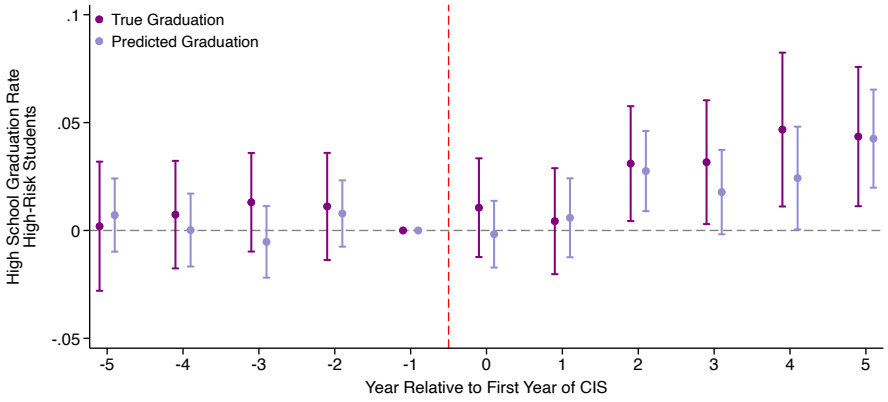
FIGURE XI: Effect on Adult Income Quartiles
Census-IRS Sample: Low-Income Students



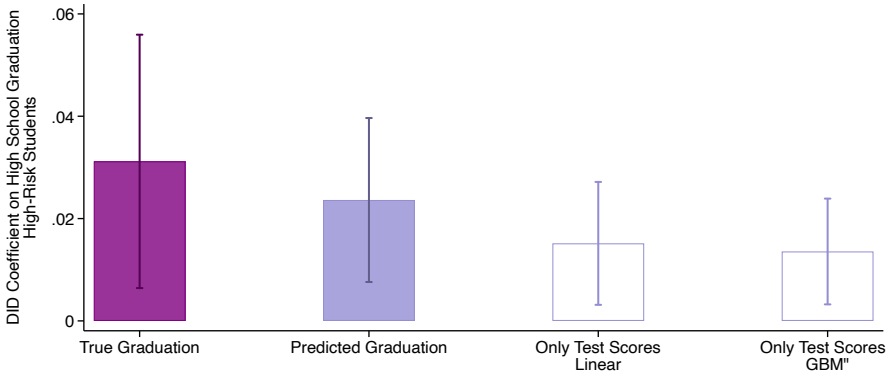
Notes: This figure shows the effect of CIS on the probability that low-income students fall into different quartiles of the income distribution at age 27. It is estimated using Census-IRS data, described in Section III C. We show difference in difference coefficients from four separate regressions, where in each the dependent variable is an indicator equal to 1 if a student ends up in quartile X at age 27. Coefficients capture average effects for students born 3–5 years after the implementation cohort, estimated using the difference-in-differences analog of Equation 5. Standard errors are clustered at the high school catchment zone level. Figure IVb from Goldman, Gracie and Porter (2023). CBDRB-FY23-CES014-028.

FIGURE XII: Comparing True vs. Predicted Long-Run Outcomes

A: High School Graduation
High-Risk Students



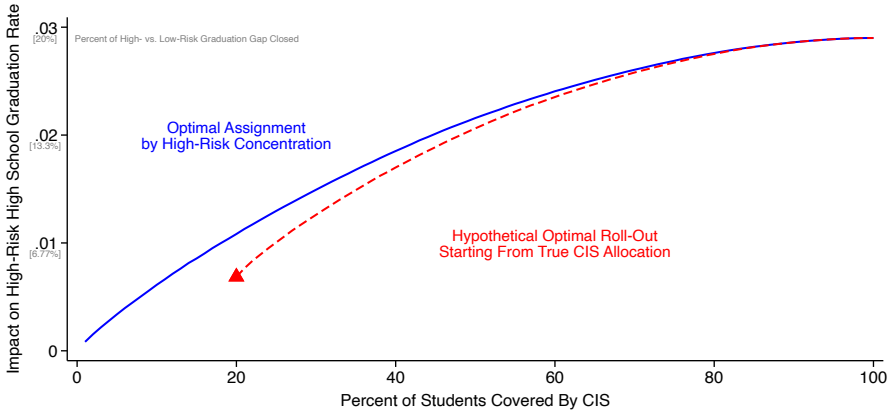
B: DID High School Graduation



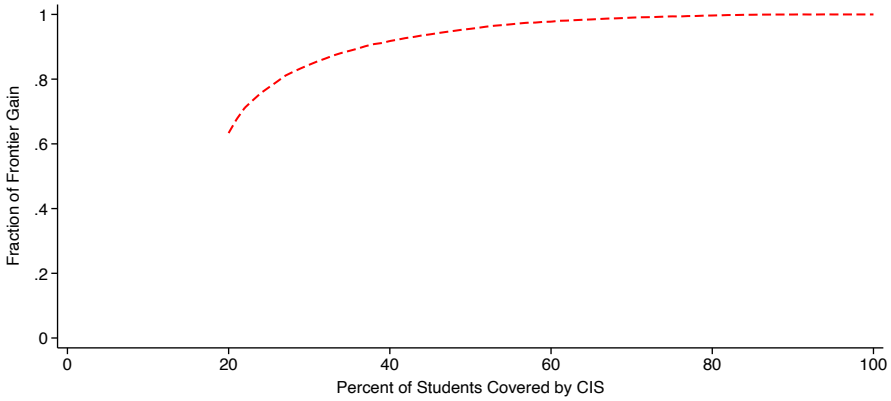
Notes: This figure compares the true effect of CIS on high school graduation for high-risk students to predicted effects based on contemporaneous outcomes. In Panel A, we present event study coefficients estimated using the matched control design. The dark purple series replicates the result shown in Figure IX Panel *A*. The lighter series shows coefficients from Equation 1, where the dependent variable is predicted high school graduation based on a gradient boosting model trained on contemporaneous academic and behavioral outcomes. See Appendix Figure A.9 for details on the prediction model. Panel B reports difference-in-differences estimates from each specification. Hollow bars show two alternative predictions based only on test scores. In the first, we use a linear regression of graduation on middle school test scores, estimated using statewide data from 1998–2004. In the second, we fit a flexible gradient boosting model on the same data to capture possible nonlinear relationships. Standard errors are clustered at the school level.

FIGURE XIII: CIS Expansion Counterfactual

A: Frontier Allocation vs. Current CIS Allocation



B: Hypothetical CIS Roll-Out: Fraction of Frontier Gain



Notes: This figure presents the results of a simulation estimating the effects of a hypothetical expansion of CIS programs. Motivated by the results in Figure IX, where gains are concentrated among high-risk students, the blue series in Panel A simulates a counterfactual rollout in which CIS is expanded to schools in descending order of high-risk student concentration. The horizontal axis plots the share of all middle school students covered by CIS; the vertical axis shows the corresponding increase in the statewide high-risk high school graduation rate (equal to the treatment effect of CIS times the fraction of high-risk students served by the program. The triangle marks the actual 2019 CIS allocation, which covers approximately 20% of students. The vertical gap between the actual allocation and the blue frontier curve reflects that CIS programs are not strictly assigned to the highest-need schools. The red dashed series shows a hypothetical expansion beginning from the existing CIS allocation and continuing to schools with the next-highest high-risk shares. Panel B shows the fraction of the frontier gain (from Panel A) achieved under this sequential rollout. The high versus low-risk gap in Panel A is based on a 15 percentage point gap in graduation rates today among schools that have never had a CIS program.

Supplementary Appendix

TABLE A.1: Characteristics of Case-Managed Students

	Mean (1)	p25 (2)	p50 (3)	p75 (4)
A. Student Characterisitcs				
Fraction Male	0.466			
Fraction White	0.128			
Fraction Hispanic	0.667			
Fraction Black	0.179			
Fraction Other Race/Ethnicity	0.026			
Fraction FRPL	0.710			
Fraction Dropout-Risk	0.762			
B. Student Outcomes				
Standardized Math Score in 6th Grade	-0.541	-1.186	-0.708	-0.022
Absence Rate in 6th Grade	0.046	0.012	0.034	0.062
Fraction Ever Receive In-School Suspension in 6th Grade	0.265			
Ever Ever Receive Out-of-School Suspension in 6th Grade	0.146			
C. Reason For Referral				
Referral includes				
Academics	0.834			
Attendance	0.223			
Behavior	0.655			
Social Services	0.347			
D. Information on Interactions				
Days After August 1st Parent Consent Received	77	35	64	126
Number of Interactions in Year	38	18	29	48
Number of Unique Services Received	14	9	13	18
Fraction of Services Individual	0.319	0.019	0.091	0.640
Fraction of Services from External Organization	0.148	0.000	0.000	0.000
Number of Student-Years	170495			
Number of Students	131956			

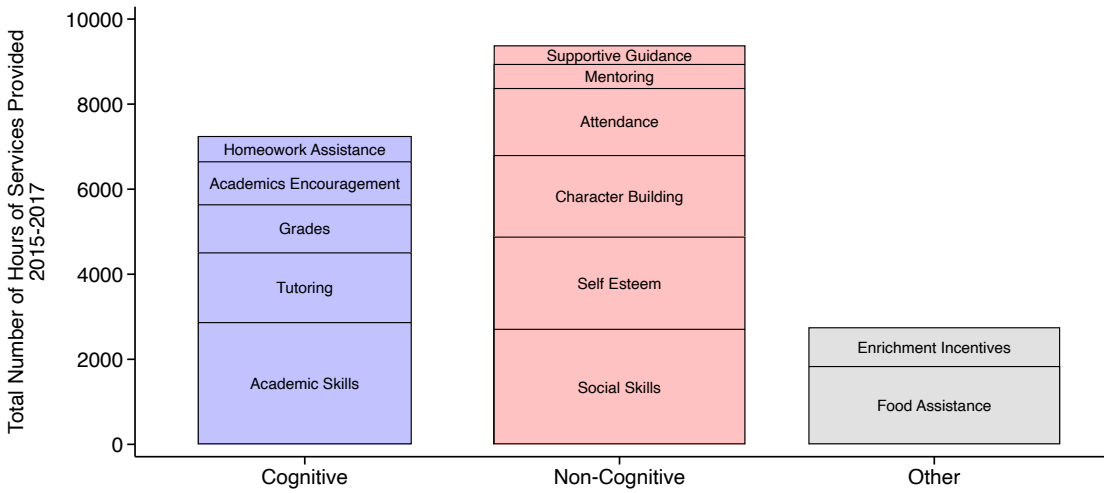
Notes: This table presents summary statistics for students who were case managed by CIS in a middle school between 2014 and 2019. Panel A reports student demographic characteristics, and Panel B summarizes student outcomes. Panel C shows the fraction of students referred for each broad category of need (not mutually exclusive). Panel D provides statistics on students' interactions with CIS navigators. A unique service refers to a distinct service code in the CIS microdata. Services are categorized as individual or group-based and are classified by whether they were delivered directly by TEA/CIS staff or by an external provider.

TABLE A.2: Needs Addressed by CIS

	Number of Student-Years (1)	Fraction of Case-Managed Students-Years (2)
Targeted Need Includes		
Test Readiness	70387	0.410
Grades	70278	0.409
Academic Readiness	69553	0.405
Social Skills	61917	0.360
Self Esteem	55514	0.323
Basic Needs	53079	0.309
College Readiness	36941	0.215
Homework Completion	35445	0.206
Absences	30469	0.177
Classroom Conduct	26273	0.153
Language Development	24481	0.142
Tardies	18653	0.109
Classroom Participation	17902	0.104
Family Conflict	13239	0.077
Mental Health and Wellness	12312	0.072
Life Skills	10428	0.061
Career/Employment	9210	0.054
Delinquent Conduct	5816	0.034
Health	3279	0.019
Grief/Loss	1924	0.011
Housing	1417	< 0.01
Suspected Substance Use	965	< 0.01
Violence	947	< 0.01
Gang Involvement	584	< 0.01

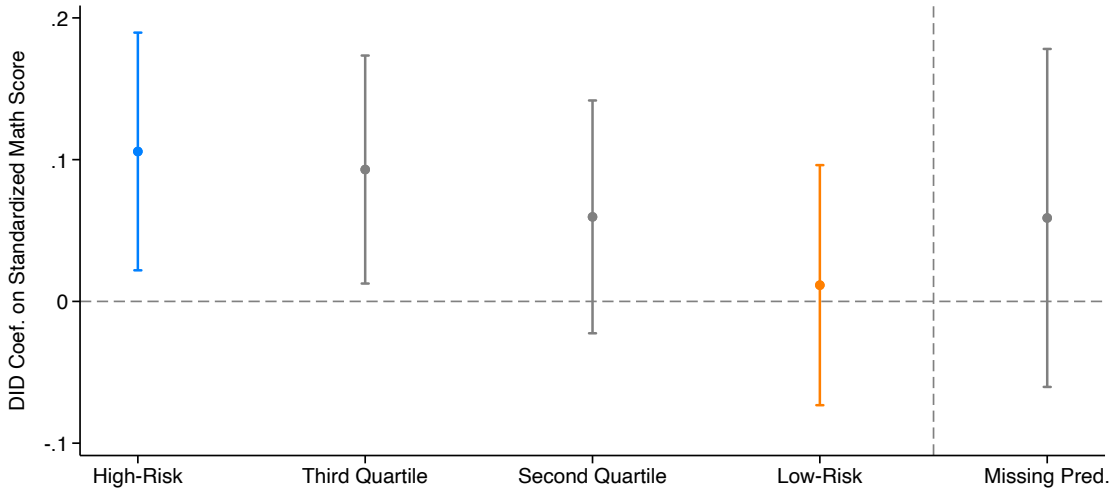
Notes: This table provides information on the detailed needs of students who are case managed by CIS in middle schools from 2014-2019, using the CIS microdata described in Section III B. For each need category, we provide the fraction of students-year observations who are flagged as having this need (not mutually exclusive).

FIGURE A.1: Top 10 Services Provided by CIS 2015-2017



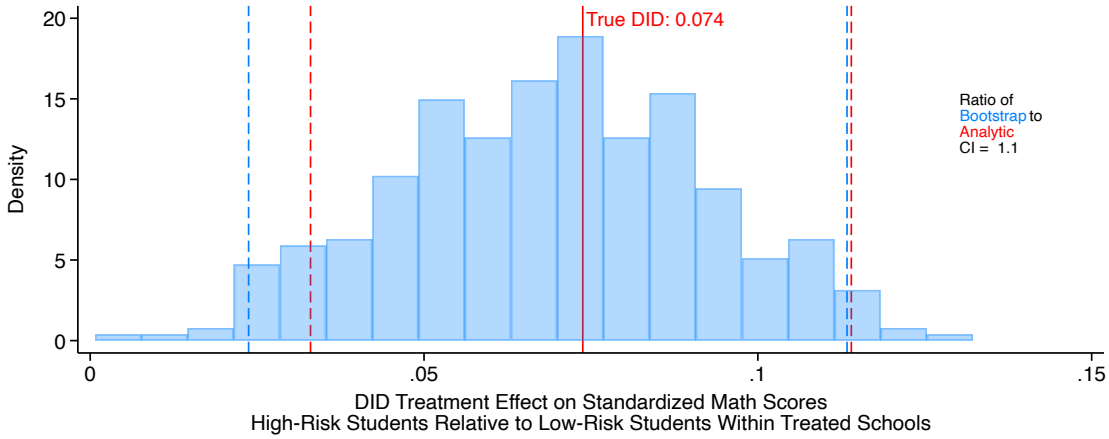
Notes: This figure summarizes the most common services provided by CIS navigators, measured in total time. Using the CIS microdata described in Section III B, we identify the top 10 most frequently provided services in each year from 2015 to 2017. We classify these services into three broad categories based on our own determination: cognitive, non-cognitive, and other. The height of each bar reflects the total time spent delivering that service across all three years.

FIGURE A.2: Heterogeneity: Effect of CIS on Test Scores
By Risk Group



Notes: This figure reports difference-in-differences coefficients estimating the effect of CIS on standardized test scores, separately by student risk status. For each quartile of the risk distribution—as well as for students without a predicted risk score—we report the coefficient $\beta_{post,3-5}$ from Equation 2, comparing treated schools to matched control schools. The estimates for high- and low-risk students correspond to those shown in the event studies in Figure V. Students are missing a risk score if they lack elementary school data. Standard errors are clustered at the school level.

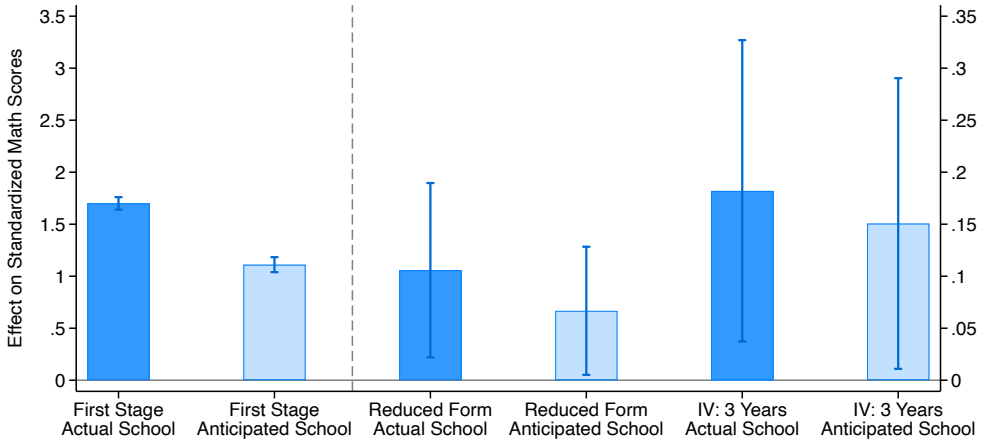
FIGURE A.3: Clustered Bootstrap: Within-School Effects on Test Scores High-Risk Students



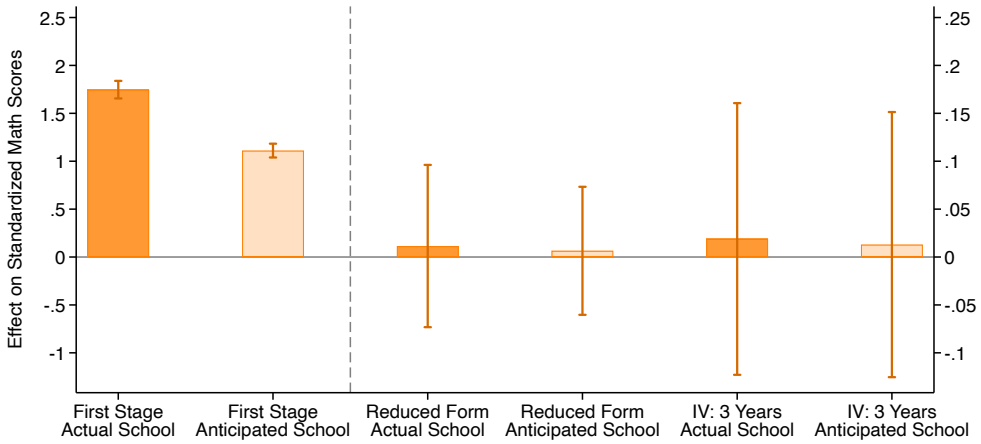
Notes: This figure shows the distribution of treatment effects from a clustered bootstrap procedure. For computational reasons, we do not re-estimate the full prediction model from Figure III or the corresponding treatment effects from Figure V. Instead, we take a simplified approach based on the within-school design shown in Figure VIII, Panel A. Rather than using the original gradient boosting algorithm to assign risk scores, we estimate a logistic regression on the same set of predictors (see Section IV) to classify students into high- and low-risk groups.The bootstrap procedure proceeds as follows: for each iteration, we first resample schools with replacement from the training sample and re-estimate the logistic regression model. We then resample schools with replacement from the analysis sample, use the estimated logistic model to assign risk scores to students in the resampled data, and estimate Equation 3. We repeat this process across bootstrap iterations.The figure shows the resulting distribution of treatment effect estimates. For reference, we also report the treatment effect from the actual data using this simplified design, as well as the ratio of the conventional 95% confidence interval (based on school-clustered standard errors) to the 95% confidence interval implied by the bootstrap distribution.

FIGURE A.4: Effects of CIS on Standardized Test Scores: Actual School Assignment vs. First School Assignment

A: High-Risk Students



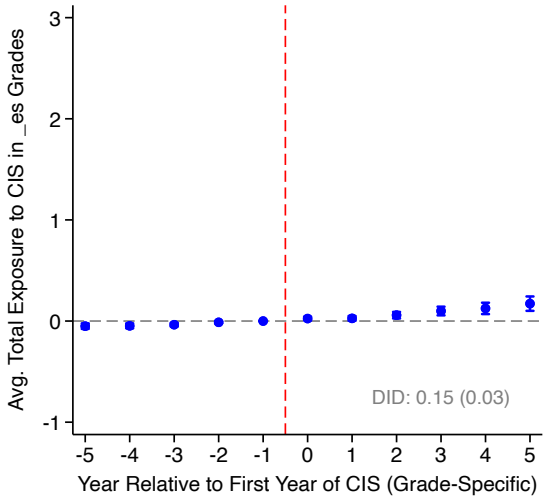
B: Low-Risk Students



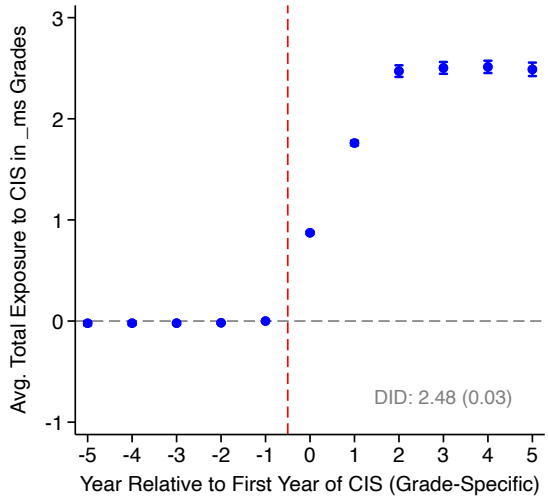
Notes: This figure compares the first stage, reduced form, and IV effects of CIS on standardized test scores using students' actual versus anticipated middle school assignments. In Panel A, we restrict to high-risk students. The first stage and reduced form estimates using actual school assignment replicate those reported in Figures IV and V, respectively. Anticipated school assignment follows the approach used in Figure VI: rather than using students' actual enrolled middle school, we assign them to an anticipated school based on the first elementary school they attend (see notes to Figure VI for details). The reduced form estimate under anticipated assignment is the same as that shown in Figure VI Panel G. The IV estimates rescale the reduced form by the corresponding first stage to yield an effect per three years of CIS exposure. Panel B presents the analogous comparisons for low-risk students.

FIGURE A.5: Detailed First Stage by School Type

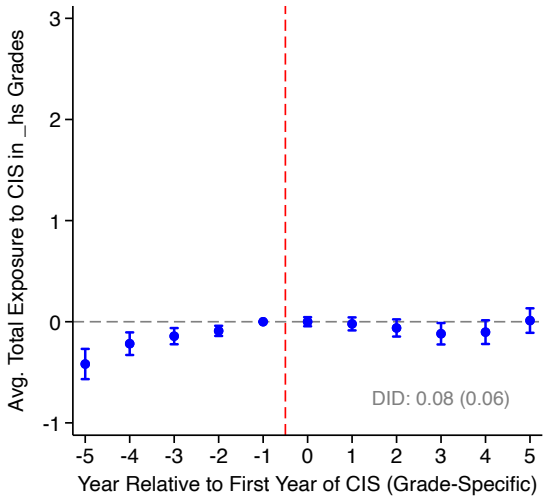
A: Total Exposure to CIS in Elementary School



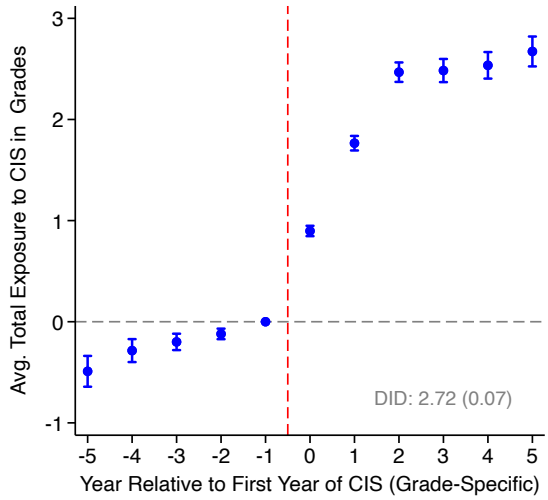
B: Total Exposure to CIS in Middle School



C: Total Exposure to CIS in High School

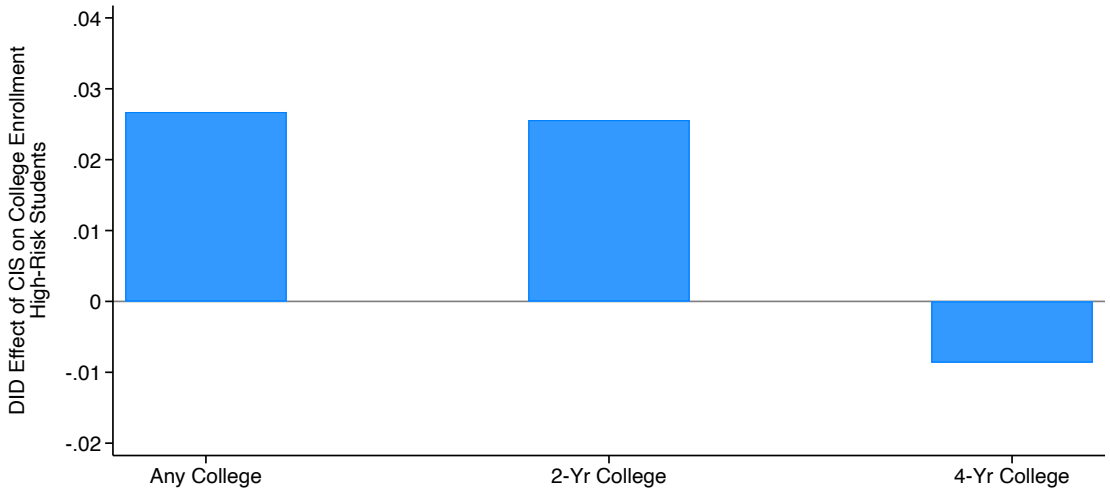


D: Total Exposure to CIS in All Grades



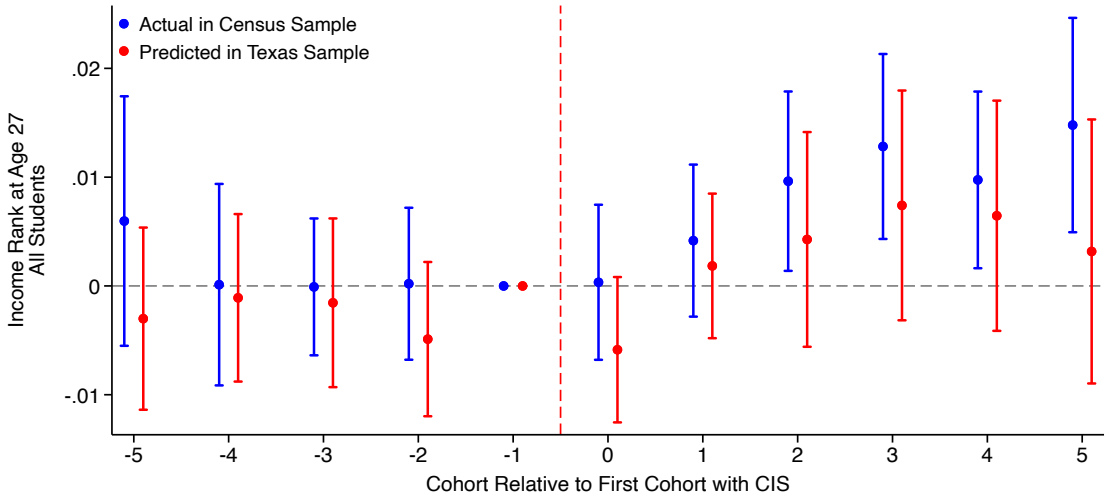
Notes: This figure presents event study estimates of CIS exposure by school level. In each panel, we estimate Equation 4, where the dependent variable is cumulative years of exposure to CIS during elementary school (Panel A), middle school (Panel B), high school (Panel C), and across all grades (Panel D). All specifications use the matched control design and leverage variation from the rollout of CIS to middle schools, as described in Section IV. These estimates assess whether variation in middle school exposure is correlated with exposure at other grade levels.

FIGURE A.6: Effect of CIS on College Enrollment by College Type
High-Risk Students



Notes: This figure presents reduced form, DID coefficients from three separate regressions showing the effect of CIS on college attendance for high-risk students. The middle bar is the same as that shown in Figure IX Panel *C* and restricts two two-year college attendance, see notes for additional details. The bar on the left captures college attendance at all levels and the bar on the right captures enrollment only at 4-year colleges. In each case, we define enrollment as ever enrolling between the ages of 18-22.

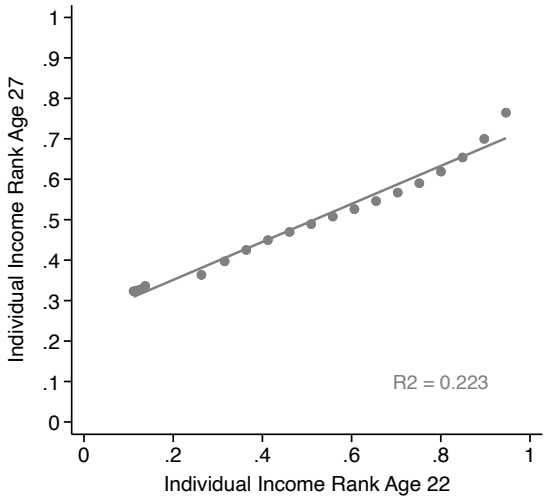
FIGURE A.7: Sample Comparison: Effect of CIS Income Rank at Age 27
All Students



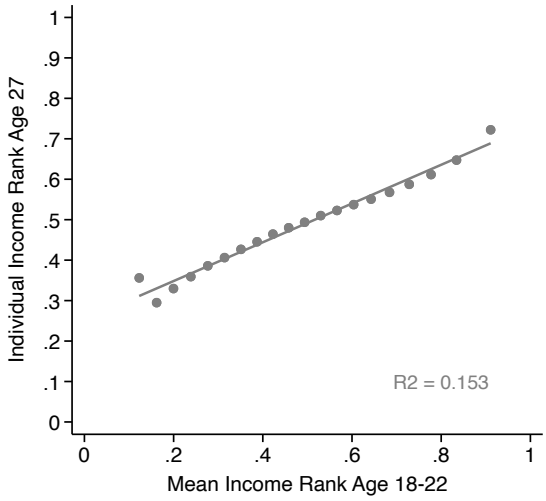
Notes: This figure compares the effect of CIS on adult earnings rank as measured in the national Census-IRS data to the effect on predicted earnings in the Texas sample. The Census series replicates the estimates from Figure X; see accompanying notes for additional details. The Texas series presents coefficients from Equation 5, where the dependent variable is predicted earnings rank at age 27. These predictions are based on observed earnings and college attendance between ages 18 and 22. See Appendix Figure A.8 for further details on the earnings prediction model.

FIGURE A.8: Predicting Individual Income Rank at Age 27

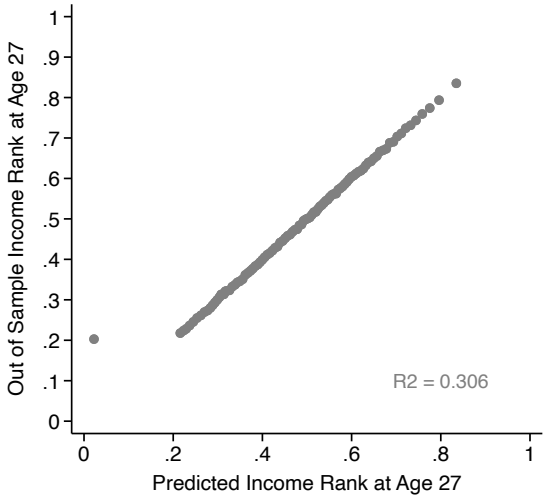
A: Relationship with Age 22 Rank



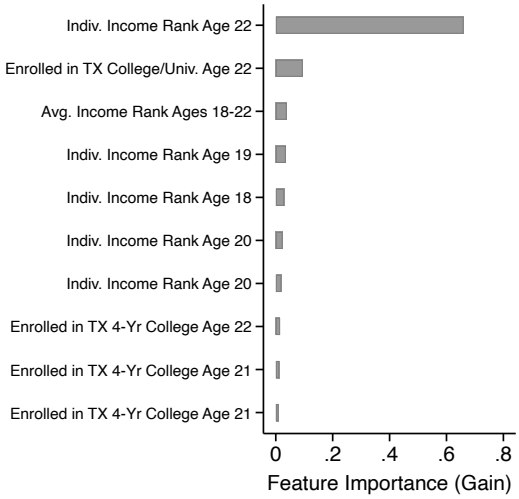
B: Relationship with Mean Rank Ages 18-22



C: Out-of-Sample Accuracy of HS Graduation Prediction



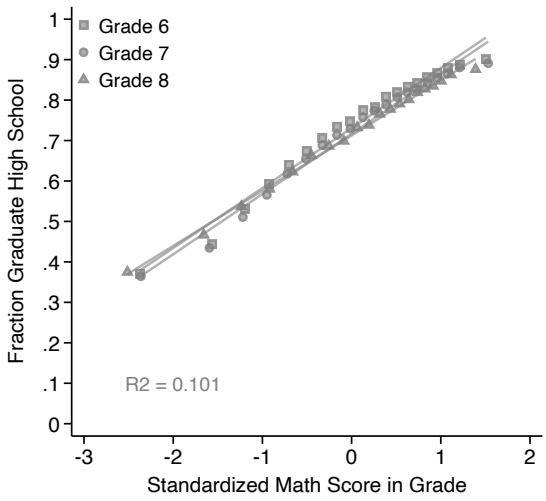
D: Feature Importance – Model Gain



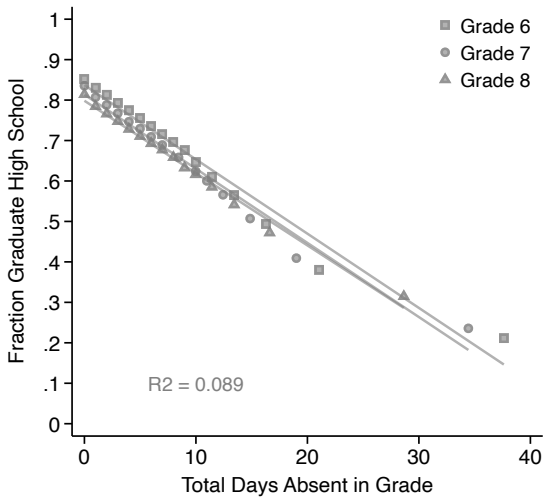
Notes: This figure summarizes the empirical prediction of household income rank at age 27 using earnings and college attendance data from ages 18 to 22. All plots are based on a 20% hold-out test set not used in model training, using statewide data students born between 1978-1994. Panel A shows a binned scatterplot of the relationship between income rank at age 22 and income rank at age 27. Panel B presents the analogous relationship using mean income rank across ages 18 to 22. Panel C shows a binned scatterplot of observed income ranks against predicted values from a gradient boosting model trained on earlier outcomes. Predictors include income rank at each age (18–22), average rank, change in rank over time, and indicators for 2-year and 4-year college attendance at each age. Panel D reports the top 10 predictors by average model gain.

FIGURE A.9: Predicting High School Graduation

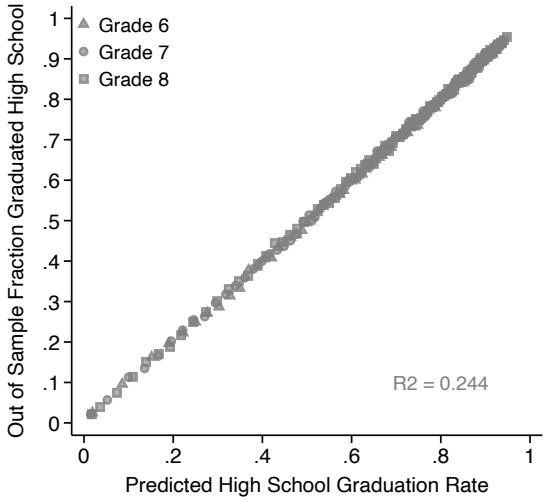
A: Relationship with Test Scores



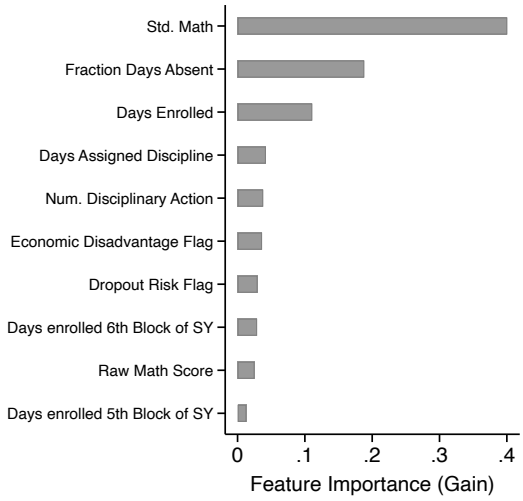
B: Relationship with Attendance



C: Out-of-Sample Accuracy of HS Graduation Prediction



D: Feature Importance – Model Gain



Notes: This figure summarizes the empirical prediction of high school graduation using academic and behavioral outcomes observed in middle school. All plots are based on a 20% hold-out test set not used in model training, using statewide data on middle school students from 1998–2004. Panel A shows a binned scatterplot of the relationship between standardized math scores and high school graduation, separately for grades 6–8. Panel B presents the analogous relationship using days absent in each grade. Panel C shows a binned scatterplot of observed graduation rates against predicted graduation probabilities from grade-specific gradient boosting models. These models are trained on student characteristics and outcomes measured in grade g, including demographics (race, sex, FRPL, dropout-risk flag, economic disadvantage flag), standardized math scores, attendance (at six-week intervals and overall), and disciplinary records (in-/out-of-school suspensions, days assigned, number of infractions). Panel D reports the top 10 predictors by average model gain across the grade-specific models.