Computer Science for All? The Impact of High School Computer Science Courses on College Majors and Earnings

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This study provides the first causal analysis of the impact of expanding Computer Science (CS) education in U.S. K-12 schools on students' choice of college major and early career outcomes. Utilizing rich longitudinal data from Maryland, we exploit variation from the staggered rollout of CS course offerings across high schools. Our findings suggest that taking a CS course increases students' likelihood of declaring a CS major by 10 percentage points and receiving a CS BA degree by 5 percentage points. Additionally, access to CS coursework raises students' likelihood of being employed and early career earnings. Notably, students who are female, low socioeconomic status, or Black experience larger benefits in terms of CS degree attainment and earnings. However, the lower take-up rates of these groups in CS courses highlight a pressing need for targeted efforts to enhance their participation as policymakers continue to expand CS curricula in K-12 education.

1 Introduction

Over the last several decades, computing skills have become increasingly important in the workplace and, thus, in education and training. A recent report using data from the U.S. Department of Labor found that the U.S. economy is digitalizing at a rapid pace: while only 9 percent of occupations required high digital skills in 2002, this number increased to 26 percent by 2020 (Muro and Liu, 2023). Accordingly, employment in computer and information technology occupations is projected to grow by 10 percent from 2022 to 2032 (Bureau of Labor Statistics, 2023b). However, the supply of students with matched skills for computing positions is far from sufficient to meet the growing demand. In 2022, there were over 450,000 open computing jobs nationwide, but only 90,000 students who received a CS bachelor's degree (Code.org, 2022b). Filling these gaps will require a large increase in CS training overall, as well as particular attention to groups that historically have been underrepresented in the field and lacking opportunities to participate. For example, only 25 percent of all computing jobs are held by women (Ashcraft et al., 2016). Students from lower socioeconomic backgrounds, Black and Hispanic students, and students from urban and rural areas also have less access to foundational CS coursework compared to their counterparts in K-12 public schools (Code.org, 2022a).

To prepare students for a future of computing technologies and increasing the share of groups historically underrepresented in the field, many schools, districts, and states are working at a rapid pace to expand CS course offerings in K-12 classrooms. In the 2021-2022 school year, 53 percent of public high schools in the U.S. offered at least one CS course, a significant increase from the 35 percent of high schools that did so in 2017-2018. The nationwide trend is driven in part by ambitious "CS for All" policies in some states. Many states (e.g., Virginia and Washington) and school districts (e.g., New York City and Chicago) already have similar universal CS education initiatives in place. In Maryland, which is the context for our study, the state has rapidly expanded high school CS course offerings over the last decade. A 2018 law further requires all Maryland high schools to offer "high-quality" (hereafter, HQ) courses that are aligned with rigorous K-12 CS standards and labor market demands for high-skill digital training. Now virtually all Maryland high school students have access to HQ CS coursework.[1]

Despite heightened policy interest in making CS education widely available in K-12 classrooms across the U.S., little evidence exists on how the introduction of CS coursework affects students' postsecondary and labor market outcomes, and importantly, who benefits from such efforts. To our knowledge, this paper provides the first causal evidence on the effects of CS coursework on educational attainment, college major choice, and earnings. We use rich longitudinal data from the Maryland Longitudinal Data System (MLDS) Center, a partnership among several state-level governmental agencies that allows researchers to track students' trajectories from K-12 education through college and into the workforce. Our identification strategies exploit the staggered adoption of HQ CS course offerings across Maryland public high schools from 2015 to 2020 and combines elements of Difference-in-Differences (DID) and Instrumental Variables (IV) approaches. Specifically, we compare the change in outcomes between exposed and unexposed cohorts of students in schools newly offering HQ CS to the change in outcomes between cohorts in schools not-yet offering HQ CS. We further overcome selection bias by leveraging plausibly exogenous variation induced by *unexpected exposure* to HQ CS, an indicator for students who were concurrently enrolled in high school when HQ CS was first offered. As such, they were unexpectedly afforded an opportunity to take a HQ CS course. This unexpected exposure variable serves as our instrument that allows us to estimate the reduced-form effect of high schools offering HQ CS courses and the local average treatment effect (LATE) of students taking HQ CS coursework on student outcomes.

We find that unexpected HQ CS exposure increased the chance of students taking HQ CS by 6.2 percentage points, on average. Taking a HQ CS course did not change students'

[1] The push to increase access to CS coursework also aligns with a national trend for increasing career and technical education (CTE) pathways for students. Recent reauthorizations of the federal Carl D. Perkins Career and Technical Education Act, in 2006 and 2018, place particular attention on STEM and CS learning in CTE. Maryland, like many other states (e.g. Florida, Idaho, Indiana, Massachusetts, New Jersey, Washington), houses CS curricula, at least in part, within the state's CTE programs (Stanton et al., 2017).

likelihood of graduating high school, enrolling in college on-time, or receiving a Bachelor of Arts (BA) degree, but raised their probability of majoring in CS in their college freshmen year by 10.2 percentage points and earning a CS BA degree by 5.5 percentage points. Evidence also suggests that students induced to claim a CS major in their early college career are primarily from CS-adjacent STEM majors, but the increased BA degree attainment in CS are driven by students who would otherwise earn a BA degree in many potential fields of study if they did not have access to high school CS coursework, including other STEM fields, social sciences and humanities. We also find positive effects on early career labor market outcomes: unexpected HQ CS exposure raised students' likelihood of being employed by 2.6 percentage points and annual earnings by about 8 percent at age 24.

Average effects mask substantial heterogeneity across student subgroups. The take-up rates and the impact of CS course-taking on enrolling in a CS major during the freshmen and sophomore years are higher for students who are male, identify as White or Asian, and have higher baseline 8th grade mathematics test scores. Interestingly, the impact on CS degree receipt and earnings are similar or even stronger for less socioeconomically advantaged students and Black students, suggesting that expanded CS course offerings in high school may be particularly valuable for these students' long-term persistence in the field. We interpret these differences in light of the context of our study, where the set of schools that contribute identifying variation were later adopters of HQ CS courses and differ from the broader Maryland student population in several ways. Students in these schools are more likely to be from low-income backgrounds, are more likely to be Black, and have lower baseline test scores. Together, these findings suggest that exposing students to HQ CS coursework in high school can be an effective approach for increasing the supply of CS majors and professionals in the labor market, particularly for historically underrepresented groups. However, to increase the share of historically underrepresented groups in CS, their take-up of such courses would have to be significantly larger for these groups relative to groups with higher representation in CS.

Our study makes at least three main contributions to the extant literature. First, as noted above, this paper is among the first causal estimates on the impact of high school CS coursework on student outcomes. Economists' interests in how curricular availability in K-12 schools affects student outcomes can be dated back to the 1990s when scholars started to conceptualize and study curricular availability as an important aspect of school quality (Altonji, 1995; Levine and Zimmerman, 1995; Rose and Betts, 2004). Recent research advances this literature by identifying effects of course availability in particular subjects or at advanced levels, including mathematics (Goodman, 2019; Cortes et al., 2015; Joensen and Nielsen, 2009, 2016), science (De Philippis, 2021; Broecke, 2013), general STEM (Görlitz and Gravert, 2018; Darolia et al., 2020), career and technical education (Dougherty, 2018; Brunner et al., 2023), and advanced placement (AP) (Owen, 2023; Jackson, 2010, 2014). Given the growing importance of computing skills and the fast expansion of "CS for All" initiatives, this paper fills an important gap in this literature by focusing solely on CS.

Second, our identification strategy and detailed course-taking data allow us to provide credible causal estimates on the impact of students *taking* a CS course in addition to the impact of *having access to* CS courses in high school. While both margins are of great policy relevance, most research focuses on course access because of the inability to overcome student selection into particular courses or lack of data on course-taking. We adopt a similar identification strategy used by De Philippis (2021), which studies the implementation of a reform in the United Kingdom that aims to increase the availability of advanced science courses in secondary school. The key idea of this method is that, in addition to exploiting the staggered nature of course introduction across schools, certain cohorts in a given school are unexpectedly exposed to the newly introduced courses. We borrow from this identification strategy by using unexpected CS course offering as an instrument that overcomes *within* school selection into CS courses. In the U.S. context, our paper is most similar to Darolia et al. (2020), which primarily focuses on the reduced-form effect of STEM course availability on student major choice because their first-stage is too weak to identify LATE course-taking impacts. Our paper thus represents one of the first in a U.S. context to estimate LATE course-taking impacts by using an instrument that overcomes identification threats and has a strong enough first stage.

Lastly, this study also adds to a few papers that use credible quasi-experimental methods to estimate the labor market returns of high school curricula by focusing on CS, a subject for which the returns have not been assessed previously. While some earlier papers attempt to estimate earnings impacts of high school coursework, their identification strategies leave important endogeneity issues unaddressed (Altonji, 1995; Levine and Zimmerman, 1995; Rose and Betts, 2004). In contrast, several recent papers improve upon prior work with stronger methodology and better data, but they focus on impacts on postsecondary outcomes and are unable to follow students into the labor market (De Philippis, 2021; Darolia et al., 2020; Görlitz and Gravert, 2018; Owen, 2023). Jackson (2014), Joensen and Nielsen (2016), and Goodman (2019) each uses different kinds of quasi-experimental variation to estimate earnings impacts of AP courses, advanced mathematics, and foundational mathematics courses, respectively. Our study complements these papers by adding important causal evidence on how exposing students to rigorous CS coursework in high school affects both their postsecondary outcomes and early career earnings.

This paper proceeds as follows. Section 2 discusses institutional background on secondary CS education in Maryland. Section 3 describes the data, provides descriptive statistics of our sample and defines the treatment variable. In Section 4, we detail the identification strategy and empirical methods. Section 5 discusses results for educational attainment and college major outcomes (5.1) as well as model validity of our identifying strategy (5.2). In Section 6, we characterize compliers of the newly available HQ CS courses (6.1) and conduct a comprehensive heterogeneity analysis (6.2). After presenting effects on earnings (7), we probe the robustness of our findings in 8. Finally, Section 9 concludes with a discussion of hypothesized mechanisms that may explain the results, potential policy implications, and the broader role that supply-side decision-making in education plays in determining workforce skills and productivity.

2 Institutional Background

Maryland passed its CS Education for All legislation (HB 281) in 2018, requiring all Maryland school districts and all public high schools within them to offer at least one HQ CS course by the the 2021-2022 school year. This legislation also established the Maryland Center for Computing Education (MCCE), a centralized clearinghouse that coordinates CS educational efforts across Maryland. MCCE works closely with the Maryland State Department of Education (MSDE) to support the state's 24 county-based school districts in increasing access to computing education and building a pipeline of teachers equipped with the skills to teach CS (Maryland General Assembly, 2018). After establishment, MCCE convened a panel of experts from government, industry, and education to establish a streamlined definition of HQ CS courses across school districts based on the alignment of School Codes for the Exchange of Data (SCED)[2] to the Maryland Computer Science Standards.

Using the list of of HQ CS courses established by MCCE and detailed course-taking data from the MLDS Center, we are able to examine the history of the introduction of rigorous CS courses across Maryland public high schools, which started several years before the passage of the CS for All legislation. In Table 1, we present the list of the 18 HQ CS courses offered in Maryland high schools between the 2012-2013 school year, when our CS course data are first available, and the 2019-2020 school year, the most recent year these data are available. Although the statewide policy focused on implementation by the 2021-22 school year, course data beyond the 2019-2020 school year suffer from broader data collection constraints due to the Covid-19 pandemic. To help illustrate the content and goals of these courses, as well as their alignment to economic demand for high-skilled, digitally literate workers, we also classified the 18 HQ CS courses into subcategories based on guidance from MCCE staff and

[2]The National Center for Education Statistics uses SCED to classify secondary school courses (NCES, 2023).

the baseline mathematics scores of students taking these courses: Foundational CS, AP CS, and Programming & Cybersecurity. The overlap between HQ CS and AP CS with CTE are notable. Specifically, four out of the seven foundational CS courses are also CTE courses, while two of the five AP CS courses can be used to fulfil requirements of specific CTE programs.[3] The HQ CS courses are a subset of a broader set of CS courses (hereafter, Any CS), which were locally aligned to the Information Technology SCED codes by the MCCE appointed working group.[4] Some examples of these courses that are not classified as HQ CS include Computer Applications, Introduction to Computer Technology, Java Fundamentals, and Computer Graphics.

We focus our study only on HQ CS courses because of their confirmed alignment with the Maryland Computer Science Standards by the MCCE working group and that they are required by the Maryland CS for All legislation. From a practical perspective, most of the variation in CS course offerings across schools and over time comes from HQ CS courses, which is critical for identification. Figure 1 shows the percent of Maryland high schools and districts offering HQ CS courses by academic school year from 2012-2013 to 2019-2020. About 60 percent of high schools offered HQ CS in 2013 while over 90 percent did so in 2020. All of Maryland's school districts offered HQ CS as of 2013 except for the two least populous counties in the state. Aligned to the legislative mandate, as of 2020, all school districts in Maryland offered HQ CS, though a small share of schools still need to offer the course by 2021-2022 to fulfill the mandate.

We further explore how the distribution of HQ CS course-taking varies among all HQ CS course-takers. The top-left sub-figure in Figure 2 shows that for all HQ CS course-takers, about 85 percent of them only took one HQ CS course. Close to 10 percent of them took two, while very few students took three or more. Not surprisingly, for all HQ CS course-takers and those who took only one HQ CS course, the majority of the courses they took

[3]When MSDE develops specific CTE programs of study, it leverages AP offerings in high schools and infuses them into the program when the AP courses are in alignment with the intent of the CTE program(Advance CTE and College Board, 2018).

[4]A list of Any CS courses offered in Maryland high schools can be found in Appendix A Table A1

are Foundational CS while about 40 percent of the courses are AP CS. Less than 5 percent of the courses these students took are in Programming & Cybersecurity, which are more specialized courses. In comparison, for students who took two or more HQ CS courses, their likelihood of taking an AP CS or a Programming & Cybersecurity course is much higher. We provide a more in-depth analysis of the composition of students who took these different types of CS courses in Section 6.1.

3 Data

Most of the data elements from the MLDS Center cover school years from the 2007-08 school year through 2021-22. However, there are several caveats that directly impact our analyses. First, the course enrollment data that we use to construct our main treatment variable only start to be available in the 2012-13 school year, meaning that if a school already started offering HQ CS before that year, we simply cannot observe it. Second, the postsecondary data housed at the MLDS Center come from both the Maryland Higher Education Commission (MHEC) and the National Student Clearinghouse (NSC), which overlap to a large degree but not completely in terms of data elements. Specifically, while we can observe all students' college enrollment, persistence, completion status, and degree field, there is more missing data for college major choice in students' freshmen and sophomore years for out-of-state college attendees relative to in-state students because of incomplete information on intended majors for out-of-state students from NSC.[5] Lastly, the earnings data come from the State of Maryland's Division of Unemployment Insurance and do not include wages for federal employees, military employees, individuals who are self-employed, private contractors, or

[5]In our analytic sample, 84 percent of students attended college in-state conditional on enrolling in college on-time. Appendix A Table A2 shows the percent of students with non-missing major information at different levels of educational attainment and different types of institutions. For first-year enrollment, we have data on intended majors for 98 percent of in-state college enrollees and 65 percent of out-of-state enrollees. For students who persisted to the second-year of college, we have data on intended majors for over 99 percent of in-state persisters and 72 percent of out-of-state persisters. For students who earned a BA degree within four years of college enrollment, we have major information for all in-state graduates and 99 percent of out-of-state graduates.

individuals who reside and work outside Maryland.

Student Outcomes. Our main outcome variables are indicators for being a CS major at different levels of educational attainment, indicators for other broad major categories, and log earnings. To classify majors, we use the Classification of Instructional Programs (CIP) codes from NCES (NCES, 2020). We also use the Department of Homeland Security (DHS) classification of STEM designated-degree programs (DHS, 2023). Our primary outcomes measure whether students claimed a CS major in their first- or second-year of college and whether they earned a CS BA degree. We also create auxillary educational attainment measures regardless of field, including high school graduation, college enrollment, college persistence, and BA degree receipt. All measures are dichotomous. For all educational attainment measures, we focus on "on-time" educational attainment (i.e., no gaps in students' educational trajectories) to preserve power given our panel is not particularly long.[6]

In order to examine fields that students are being pulled from, we create similar outcomes for other broad major categories including: (i) other non-CS STEM major; (ii) other non-CS non-STEM major; (iii) engineering; (iv) health; (v) business; (vi) social sciences; (vii) humanities; and (viii) education.

For earnings, we compute several measures of logarithmic real annual earnings at different ages by summing quarterly earnings over the calendar year, converting to 2021 dollars using the CPI-U (Minneapolis Fed, 2023), and taking logarithmic transformations. Because our earliest partially treated cohort (i.e., enrolled in 9th grade in 2012) would have graduated high school in 2015 and received BAs in 2019, we are only able to observe earnings from ages 23-25 for some treated cohorts.[7]

Sample Construction. We focus our analysis on the 2008-09 to 2016-17 9th grade

[6] Appendix A Table A3 shows that we can observe high school graduation, college enrollment, and college persistence for all cohorts in our analytic sample, but we can only observe BA completion for seven of the nine cohorts in our sample.

[7] Table A3 shows that we can observe age 23 earnings for six of nine cohorts while we can only observe age 25 earnings for four of nine cohorts in our analytic sample. Age 25 is the latest age that we can observe earnings for the earliest partially treated cohort, which was the 2012 9th grade cohort.

cohorts who enrolled in regular public high schools in the state.[8] These include the earliest cohort for whom we can observe students' baseline information (including prior test scores) and the latest cohort for whom we can observe students' full course-taking history. This yields a sample of 635,771 students enrolled in 233 regular program high schools.

We then impose four additional restrictions to obtain an analytic sample that allows us to determine when treatment first occurred and which cohorts are unexposed or unexpectedly exposed to HQ CS courses. First, we drop students enrolled in high schools that were already offering Any CS courses (i.e., both HQ CS and the broader set of less rigorous CS courses) in 2013. Because our course data begin in 2013, we are not able to observe when these schools started to offer CS courses, and many schools may have done so prior to 2013. Thus, for these schools, we are unable to identify which cohorts were unexpectedly exposed to HQ CS. This is our most essential sample restriction for our research design, but also the most consequential because it reduces our sample size to 66,381 students in 59 high schools and produces an analytic sample that is relatively more socioeconomically disadvantaged and includes a larger share of students of color compared to the population (see additional discussion below).

Next, we drop students who are outgoing transfers (transfer from a Maryland public high school to a private or out-of-state high school) and incoming transfers (first observed grade of enrollment is 10th-12th grade). Because we are not able to observe these students' full course-taking history, omitting them reduces measurement error and attenuation bias. Lastly, we drop students in 9th grade cohorts who enrolled in high school after the school first began offering HQ CS. This restriction mitigates concerns about sorting motivated by the HQ CS offering. Our final analytic sample consists of 50,507 students in 58 high schools.[9] In our robustness analysis in Section 8, we test whether our main results are robust to the third and fourth restrictions by performing analysis on the sample prior to the imposition of these sample restrictions.

[8] We drop students enrolled in special education, alternative, and other special program schools.

[9] Our sequence of sample restrictions is also presented in Appendix A Table A4

Descriptive Statistics. Table 2 provides summary statistics for both the Maryland student population we are interested in and the analytic sample. Compared to the population data, students in our analytic sample are 18 percentage points more likely to be free and reduced-price meals system (FARMS) students and 15 percentage points more likely to be Black. Similarly, analytic sample students are also more likely to be enrolled in special education (SPED), have lower baseline test scores in 8th grade, and attend smaller schools.[10] For the variables central to our analysis, analytic sample students are less likely to be exposed to and take a HQ CS course, have lower levels of educational attainment, are less likely to earn a CS BA degree, and have lower levels of earnings. Overall, our study estimates effects for a student sample that is more disadvantaged compared to the average Maryland student along a variety of dimensions and mainly has implications for how traditionally under-served students access and benefit from CS courses.

Temporal Patterns of CS Exposure and Course-Taking. We define that a student was *partially exposed* to HQ CS courses when the first high school we observe a student enrolling in offered HQ CS at some point during the student's high school career. Most students in Maryland take four years to complete and graduate from high school. Therefore, we define exposure based on the four-year time frame that students were expected to be enrolled in high school. Similarly, because students can transfer between schools in an endogenous manner, we focus only on the first high school in which students enrolled. We also document the share of students who took a HQ CS course.

Figure 3 shows the percent of the population of Maryland high school students partially exposed to and enrolling in HQ CS courses by the year they enrolled in 9th grade. Because of the limited time span of our course enrollment data, the earliest 9th grade cohort that we can observe exposure to HQ CS courses is the 2010 cohort, for whom we can observe being

[10]For time-varying characteristics including FARMS, English language learner (ELL), and SPED, we are missing data for 10 percent of the sample. For these characteristics, we impute zeros in place of missing values. For 8th grade mathematics and English language arts (ELA) test scores, we are missing data for 13 percent of the sample. We impute the mean value for the analytic sample in place of missing values. In results not shown, we find little difference in our main results after dropping observations with imputed data.

exposed as 12th graders in 2013 when our course data begin. As expected, partial exposure and actual course-taking increase over time. Specifically, well over 70 percent of the 2010 9th grade cohort were partially exposed to HQ CS. There was a roughly 25 percentage point increase in partial exposure from when our data begin to the 2017 cohort, with about 95 percent of this cohort partially exposed to HQ CS. Correspondingly, there was also a large increase in HQ CS course-taking during this time, from about 5 percent in 2010 to over 20 percent in 2017.

Defining Unexpected Exposure. Central to our identification is the idea of students' *unexpected* exposure to HQ CS in high school. Among all students who were at least partially exposed to HQ CS, a student is considered *unexpectedly* exposed to HQ CS when their first high school was not previously offering HQ CS before they started high school. Focusing our analysis on students with unexpected exposure while excluding those with expected exposure who enrolled in high school after HQ CS was first offered addresses potential concerns that students may sort into schools based on knowledge of courses previously offered. We provide more details about the construction of this variable in the discussion of our empirical strategy in Section 4.

The bottom panel of Figure 3 shows trends for our analytic sample with regard to unexpected exposure and course-taking. About 40 percent of the 2012 cohort were unexpectedly partially exposed to HQ CS, compared to over 80 percent of the 2017 cohort.[11] The trends in HQ CS course-taking are similar to the population, with about 5 percent of the 2012 cohort taking HQ CS and about 20 percent of 2017 cohort doing so. Since our identification strategy leverages unexpected exposure to HQ CS, the variables measuring unexpected partial exposure and HQ CS course-taking in this figure are central to our research design.

We illustrate in Figure 4 an exposure timeline for a school first offering HQ CS in 2015. For this hypothetical school, the three 9th grade cohorts from 2008-09 to 2010-11 would

[11] Because our analytic sample excludes schools offering HQ CS courses in 2013, we infer partial exposure and course-taking to be zero from 2009 to 2011. The first high schools to begin offering HQ CS in our analytic sample did so in 2015, which implies that the 2012 9th grade cohort was the first cohort with some partially exposed students because these students were 12th graders in schools first offering HQ CS in 2015.

have been expected to graduate from high school before HQ CS was first offered and are thus considered unexposed. The 9th grade cohorts from 2011-12 to 2014-15 would have been concurrently enrolled in high school when HQ CS was first offered and are defined as unexpectedly exposed. Lastly, the 9th grade cohorts who started high school in 2015-16 and 2016-17 would have enrolled in high school after HQ CS was first offered. We thus exclude them from the analysis to mitigate concerns about students sorting into high schools on the basis of HQ CS course offerings.[12] In the following section, we detail our identification strategy.

4 Empirical Strategy

Similar to other studies that attempt to identify curricular effects, we need to tackle two layers of selection: (i) the selection of students into high schools based on whether HQ CS is offered and (ii) the selection of students into HQ CS within a given high school. Inspired by De Philippis (2021), our research design combines elements of generalized DID and IV, exploiting variation between schools that do and do not offer HQ CS, and across unexpectedly exposed and unexposed cohorts. We start by specifying a basic econometric model as follows:

$$Y_{isc} = \gamma_s + \pi_c + \alpha_1 X_{isc} + \alpha_2 HQCS_{isc} + \epsilon_{isc} \tag{1}$$

where Y_{isc} is the outcome for student i in high school s and cohort c. γ_s represents the fixed

[12] We perform a variety of balance tests using our treatment and exposure variables (see Appendix A for details). Having balanced characteristics is not necessary to establish the validity of our research design, but showing results of these tests does provide additional descriptive information about our sample. Appendix A Table A5 tests for balance between students in never-treated and treated schools, Table A6 tests for balance between unexposed and unexpectedly exposed students, and Table A7 tests for balance between early and late HQ CS adopters. Overall, students in treated schools (Table A5) and unexpectedly exposed students (Table A6) are more likely to have characteristics that are historically overrepresented in CS. Students in treated schools and unexpectedly exposed students are (1) less likely to be female, FARMS, or Black, (2) more likely to be White or Asian, (3) have higher 8th grade mathematics scores, (4) attend larger high schools, (5) are more likely to take HQ CS, and (6) have higher educational attainment, likelihood of being CS majors, and higher early-career earnings. There are far fewer significant differences between students attending schools that are early versus late adopters of HQ CS (Table A7). Students in late adopter schools are less likely to be Hispanic, multiracial, or ELL but more likely to be SPED.

effect for the first high school of enrollment; π_c represents the 9th grade cohort fixed effect; X_{isc} is a rich set of individual and school-level demographics and characteristics, including gender, race/ethnicity, gender-race/ethnicity interactions, FARMS, English language learner (ELL), SPED, lagged standardized mathematics and English language arts (ELA) test scores from 8th grade, total high school enrollment, and indicators for imputed values of these baseline controls; $HQCS_{isc}$ indicates whether student i took at least one HQ CS course when attending school s; and ϵ_{isc} is the error term, which is clustered at the first high school enrollment level. The coefficient α_2 measures the effect of taking at least one HQ CS in high school on student outcomes.

Aligned to a generalized DID framework with two-way fixed effects (TWFE), the set of school and cohort fixed effects are our first step for addressing potential endogeneity and selection concerns. The first high school fixed effect γ_s controls for any time-invariant school characteristics, such as the neighborhood a school serves and the overall teacher quality, which might correlate with students' HQ CS course-taking and their later outcomes. The cohort fixed effects π_c control for time-varying factors that are common to all units in a given cohort, such as state-level policies and changes in the business cycle that affect all students' educational opportunities and longer-run career prospects. However, there may still be selection bias from students sorting into HQ CS courses within schools, or the decision of schools to offer HQ CS courses at a specific point in time. For example, the timing of a high school's offering HQ CS may correlate with some time-varying school characteristics, such as cohort quality or students' demand for HQ CS courses, which would bias our results.

To address these concerns, we construct an indicator variable $PartialExpo_{sc}$ for whether a particular cohort of students c was *unexpectedly* exposed to HQ CS in the first high school s they attended and use it as an instrumental variable for $HQCS_{isc}$. As discussed above, we define unexpected exposure based on whether a cohort of students was concurrently enrolled in high school when at least one HQ CS course was first offered. Given that the unexpected partial exposure variable varies at the school-cohort level, this strategy helps address the students' selection into HQ CS courses within schools by using only school-level variation. Our analytic sample does not contain students who enrolled in high schools after those schools have adopted HQ CS. We are confident that our definition captures the unexpectedness for two reasons. First, because the students in our sample attended schools predominantly based on where they live,[13] mobility across schools is likely uncommon and it is even less likely that students sort into schools specifically on the basis of anticipated HQ CS offering. Second, we only measure unexpected exposure based on the first high school a student enrolled in to limit concerns about strategic mobility of students across schools. Our analysis thus compares the change in outcomes between unexpectedly exposed and unexposed cohorts between schools offering HQ CS (treatment group) and schools never or not-yet offering HQ CS (control group).

To estimate LATE effects, we implement our IV design through a two-stage least squares (2SLS) equation. Our first- and second-stage equations are as follows:

$$P(HQCS_{isc}) = \gamma_s + \pi_c + \beta_1 X_{isc} + \beta_2 PartialExpo_{sc} + u_{isc} \tag{2}$$

$$Y_{isc} = \gamma_s + \pi_c + \delta_1 X_{isc} + \delta_2 \widehat{HQCS}_{isc} + \nu_{isc} \tag{3}$$

where $HQCS_{isc}$ is an indicator for whether student i took at least one HQ CS course.[14] The coefficient β_2 measures the first-stage effect of unexpected partial exposure to HQ CS on the propensity of taking a HQ CS course. In the second-stage, δ_2 estimates the LATE of taking HQ CS on major choice, degree field, and earnings by leveraging the exogenous variation in unexpected exposure to HQ CS from the first stage. This estimates the LATE for the compliers: those whose decision to take HQ CS was induced by the unexpected exposure to HQ CS. In the next section, we first proceed by presenting our main results for educational

[13] In our analytic sample, 84 percent of students attend Maryland traditional public neighborhood schools.

[14] We use a binary indicator for taking at least one HQ CS course because about 85 percent of students only take one HQ CS course, as shown in the top-left sub-figure of Figure 2.

attainment and college major choice before discussing validity checks of our identification strategy.

5 Results

5.1 Main Results: Effects on Educational Attainment and College Majors

College major choice is our primary outcome of interest and is dependent on students' enrollment in college after high school graduation. Therefore, we start by estimating our 2SLS models on a series of educational attainment variables, including on-time high school graduation, college enrollment and persistence, and graduation with a BA degree. In Table 3, we report both reduced-form and LATE estimates for these outcomes. In Panel 1, we do not find any evidence that unexpected exposure to HQ CS changes these educational attainment measures. Panel 2 shows that our F-statistic for the first stage is above 16, except for BA graduation for which we have a slightly small sample. While the point estimates are positive for all the outcomes, none of them are statistically significant. Impacts on high school graduation and college enrollment are large but imprecisely estimated, while effects on college persistence and BA attainment are more modest but also imprecisely estimated. Together, these findings suggest that unexpected exposure to HQ CS courses is not inducing increased educational attainment overall and that any effects we may observe are likely to run through CS-specific outcomes.

The main results for the impact of HQ CS course-taking on CS major propensity are shown in Table 4. We present results from five main specifications, starting with simple OLS estimates (Column (1)) with high school fixed effects, cohort fixed effects, and controls for student- and school-level characteristics. In Column (2), we report the first stage of our IV estimator. We then report the reduced-form estimates in Column (3). Column (4) shows results from our IV estimator detailed in Section 4. Our last specification adds school-cohort linear time trends as controls to our IV estimator to further account for potential bias, and we show these results in Column (5).

Across all the models, our results consistently show that taking at least one HQ CS course has a strong positive impact on students' likelihood of choosing a CS major and obtaining a CS BA degree. Specifically, as shown in Panel 1, taking HQ CS results in about a 6.5 percentage point increase in the chance of being a CS major in our OLS estimation. Turning to our IV strategy, our first-stage estimates (Column (2)) indicate that HQ CS exposure results in an increase of about 6 percentage points in the likelihood of taking a HQ CS course. The first-stage F-statistic is 16.3 so we can be confident in the significance of this result according to the guidance in Stock and Yogo (2002).[15] Although we primarily focus on the impact of taking a HQ CS course in this paper, the reduced-form estimates shown in Column (3) are policy relevant and show the impact of unexpected HQ CS exposure. Across all the three outcomes, all coefficients are highly significant and positive, suggesting that access to HQ CS coursework can shift students' college major choice toward CS. Our preferred IV specification in Column (4) shows that taking HQ CS increases a student's likelihood of enrolling in a CS major in their freshmen year by about 10 percentage points, 56 percent bigger than the OLS estimates. Given that less than 2 percent of unexposed students are first-year CS majors, this 10 percentage point increase corresponds to over a sixfold increase in the chance of being a first-year CS major. Our estimate is slightly smaller and marginally significant when including the school-cohort linear time trends shown in

[15] Most of the IV literature has considered an F-statistic greater than 10 to be strong enough to perform inference using conventional t-ratios (Stock and Yogo, 2002). Most of the F-statistics in our analysis satisfy this criterion although we do have F-statistics that range from about 5 to 10 for some outcomes and subgroups due to smaller sample sizes. However, a more recent literature argues that t-ratios should be a continuous function of the first-stage F-statistic (Lee et al., 2022). With our preferred specification for our main results in Table 4, Column (4), our F-statistics range from 13.5 to 16.3. According to Lee et al. (2022), an F-stat of 13.048 implies that standard errors should be multiplied by a factor of about 1.5 to conduct inference at the 95 percent confidence level while an F-stat of 16.618 implies standard errors should be multiplied by a factor of about 1.4 at the 95 percent confidence level. Based on this more conservative approach, a simple rule of thumb for interpreting our results is that highly significant results ($p < 0.01$) should only be considered significant ($p < 0.05$) or marginally significant ($p < 0.1$), significant results should only be considered marginally significant ($p < 0.1$) or insignificant, and marginally significant results should be considered insignificant. We do not find this approach changes the main findings of this paper.

Column (5), although the F-statistic falls to 7.1. Across all three outcomes shown in Panels 1-3, controlling for time trends does not change the substantive interpretation of our findings but it does reduce the precision of our estimates. Thus, we focus on interpreting results from our preferred IV specification in Column (4).

A notable pattern across panels is that our IV estimates are about 1.5 to 2 times as large as our OLS estimates. There may be a two main explanations for this discrepancy. One explanation is that our modest first stages might bias our IV estimate upward. We provide comprehensive robustness checks in Section 8. A second explanation is that the LATE is estimated for the compliers who are more likely to have higher baseline mathematics achievement and higher socioeconomic status. If the effect of HQ CS course-taking on CS major likelihood is larger for more advantaged students who are more likely to be compliers, then this may explain the larger LATE estimates. High-achieving students are also more likely to take more advanced CS courses. If these courses have larger effects on CS majors than less advanced courses, then this may also explain why our estimated LATE for compliers is larger than OLS estimates. We explore this possibility in Section 6.

The sign, magnitude, significance, and pattern of results for second-year CS major shown in Panel 2 are very similar to and statistically indistinguishable from those in Panel 1. The pattern of results for CS BA receipt in Panel 3 is similar as well, though the estimates are about half the size compared to effects on major choices in the freshmen or sophomore year. Specifically, the LATE estimate (Column (4)) shows that HQ CS course-taking results in a large, highly significant 5.5 percentage point increase in the likelihood of earning a CS BA. Given that earning a CS BA degree is a rare outcome for unexposed students (0.4 percent), the effect corresponds to over a twelve-fold increase in CS BA propensity.

If taking HQ CS drives more students to enroll in a CS major and earn a CS BA, which majors are they drawn from? To answer this question, we use our preferred IV specification from Table 4, Column (4) to estimate the LATE of HQ CS on other broad categories of major outcomes in Table 5. For first- and second-year majors, the results provide strong evidence that taking HQ CS primarily changes major choices for students who would otherwise choose other STEM, non-CS majors. Specifically, taking HQ CS reduces students' likelihood of choosing other STEM majors by 10 to 13 percentage points and engineering by 5 to 9 percentage points. We do not observe evidence that HQ CS converts students from disciplines such as social sciences or humanities. At the same time, effects on degree receipt in these other fields shows a less-consistent pattern. Most of the point estimates for these outcomes are insignificantly negative, with a marginally significant positive effect of about 5 percentage points for health majors. Given the large number of tests run in Table 5, as well as the exploratory nature of this supplemental analysis, we do not infer too much from the one statistically significant effect on health BA receipt that is similar in magnitude to effects on intended health major (that are not statistically significant). Overall, our findings here provide suggestive evidence that HQ CS course-taking might convert some students who would otherwise choose a non-CS STEM major in college into a CS major.

5.2 Validity Checks

Parallel Trends. The validity of our identification strategy partially depends on the staggered parallel trends assumption as we exploit variation between high schools and across student cohorts. In our context, this assumption states that in the absence of schools offering HQ CS, trends in CS majors between high schools offering HQ CS and high schools never or not-yet offering HQ CS would have evolved in parallel on average for all combinations of treated and never or not-yet treated groups (Roth et al., 2023). We follow the literature and test for parallel pre-trends with a nonparametric event study specification:

$$Y_{isc} = \gamma_s + \pi_c + \phi_n \sum_{n=-3}^{4} \mathbb{1}(EventTime_{sc} = n) + v_{isc} \tag{4}$$

where $EventTime_{sc}$ is a continuous variable centered at zero for the 12th grade cohort of students in a high school the year before HQ CS was adopted, so negative and zero values represent pre-adoption cohorts and positive values represent post-adoption cohorts with increasing levels of exposure.[16] ϕ_n is the effect of unexpected HQ CS exposure n cohorts before or after schools begin offering HQ CS. These parameters represent reduced-form effects. In addition to this nonparametric TWFE specification, we also use two new DID estimators in our event study analysis to test for robustness (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

Our event study estimates provide strong evidence in favor of the staggered parallel trends assumption. Figure 5 shows the event study estimates for the first-stage impact of HQ CS exposure on the probability of taking a HQ CS course as well as our three main CS major outcomes. For the likelihood of taking at least one HQ CS course, the point estimates for the pre-trend coefficients are precise zeros for the Callaway and Sant'Anna (2021) (CS 2021) estimator and the Sun and Abraham (2021) (SA 2021) estimator, while they are somewhat less precise but indistinguishable from zero for TWFE. After the adoption of HQ CS, there is a sharp increase in the chance of taking a HQ CS course of about 5 to 10 percentage points for students who have one year of exposure. This likelihood of taking HQ CS increases for longer exposure and all of the coefficients are statistically significant. We observe similar patterns for the three outcomes for CS majors. Although many of the estimates are imprecise, there is a clear pattern of positive post-event coefficients. The post-event coefficients for CS major in first- and second-year of college also demonstrate increased magnitudes with longer HQ CS exposure. For all three outcomes, at least some of the post-event coefficients are significant. The figure looks qualitatively similar if we include demographics controls in the model (Figure A1) or excluding never treated schools or closed schools (Figure A2). Overall, our event study estimates suggest that the parallel trends assumption likely holds and such finding is also robust to alternative DID estimators designed for staggered treatment timing.

Validity of IV. A major threat to the validity of our IV estimator is that a school's

[16] 12th graders for whom their schools started offering HQ CS during their senior year only have one year of exposure (coded as a 1 for $EventTime_{sc}$ for the first year of HQ CS offering). Students who are 11th graders in the starting year of HQ CS offering thus have two years of exposure (coded as 2), and so on.

decision to offer HQ CS is endogenously related to other factors that may affect the outcomes. We directly test this possibility by using a range of student demographic variables as outcomes in our reduced-form specification and test how student compositions vary across unexposed and unexpectedly exposed cohorts between schools that offer HQ CS and schools never or not-yet offering HQ CS. Any significant coefficients should be thought of as indicative of differential trends in student demographics that are correlated with the timing of HQ CS offering rather than as changes that are the direct consequence of or only occurring after HQ CS offering. These results are shown in Appendix A Table A8. Most of the coefficients are insignificant, with some evidence of a decrease in female and special education and an increase in English learner over time in the treatment group compared to the control group. However, estimated effects for changes in key characteristics such as FARMS and baseline test scores are indistinguishable from zero. Thus, we believe that the concern that schools offered HQ CS as a response to student composition should have little to small impact on our estimates at best. Regardless, we include a rich set of demographic controls and probe for robustness to the inclusion of school by cohort linear time trends in our analysis.

Finally, we discuss the monotoncity or "no defiers" assumption for LATE. Violations of this assumption are unlikely to be of major concern. Balance tests between unexposed and unexpectedly exposed students show that it is rare for students in the unexposed group to take HQ CS: only 0.6 percent of students in this group took HQ CS (see Appendix A Table A6). The only way for an unexposed student to take HQ CS is to transfer schools, which is relatively uncommon.[17] Although it is plausible that there are always-takers in our data (students who take HQ CS regardless of exposure), it seems unlikely that there are defiers who do not take HQ CS when their schools offer it but transfer to a different school to take HQ CS when their schools do not offer it. Empirical evidence, our identification strategy, and the institutional setting suggest that the the IV identifying assumptions likely hold, which allows us to identify the LATE.

[17] Only 4 percent of students in our analytic sample transferred to another Maryland public school during their high school enrollment

6 Complier Characterization and Heterogeneity

6.1 Complier Analysis

Our 2SLS approach estimates causal effects of HQ CS for compliers, or those who take HQ CS after their high school unexpectedly offers a course. We start to characterize the compliers by estimating first-stage coefficients for subgroups based on gender, socioeconomic status (FARMS), race/ethnicity, and quartiles of baseline mathematics achievement. The results are reported in Table 6. We then compare these coefficients to the first-stage coefficients for the full sample shown in Table 4, Column (2), Panel 1 to compute the relative likelihood of being a complier.

We find that HQ CS course take-up is concentrated amongst students traditionally over-represented in CS fields. Compliers are more likely to be male, not eligible for FARMS, Asian, and in the top quartile of baseline mathematics scores. Students in these subgroups consistently have larger first-stage coefficients relative to the full sample and are thus more likely to take HQ CS conditional on unexpected exposure. In particular, unexpected HQ CS exposure raises the likelihood of taking HQ CS by 13.2 percentage points for Asian students and by 10.5 percentage points for top quartile mathematics students. These estimates are 114 and 70 percent larger, respectively, than the full sample estimate.

We further describe compliers based on their course-taking behaviors. Specifically, we plot the distributions of HQ CS course types taken by quartiles of mathematics achievement in Figure 6. For students in the bottom (1st) quartile of mathematics achievement, about 90 percent of the courses taken are Foundational CS courses. In contrast, higher achieving students are more likely to take an AP or a Programming & Cybersecurity course, which may be driven by taking a second or third HQ CS course during their high school career.[18] Over 60 percent of the courses top (4th) quartile students take are AP CS courses.[19] Together,

[18] Appendix A Table A9 shows that while only 7 percent of bottom (1st) quartile compliers take more than one HQ CS course, 15 percent of top (4th) quartile compliers take more than one course.

[19] We also characterize the compliers by showing summary statistics for demographic characteristics and 8th grade mathematics test scores for different groups of HQ CS course-takers in Appendix A Table A10.

these findings point to heterogeneity in the intensity and type of HQ CS course take-up among student subgroups, which has two implications. First, this take-up heterogeneity may suggest differential impacts on CS major and earnings outcomes by student subgroup. Second, given the higher take-up among more advantaged students, this may raise some concerns that these students are the primary beneficiaries of efforts aimed at increasing access to CS courses.

6.2 Heterogeneity

In this section, we estimate the LATE of taking HQ CS on our three main CS major outcomes by gender, FARMS status, racial/ethnic subgroups, and prior mathematics achievement. Results are shown in Table 7.[20] Given large standard errors for many of the coefficients, these results are merely suggestive since we are unable to make definitive claims about significant differences between subgroups. We find that results are not totally consistent when comparing first- and second-year CS major outcomes and BA degree attainment in CS. First, when considering major choice in the first two years of college, there is stronger evidence of positive, large, and significant effects for students who are traditionally overrepresented in CS and higher-achieving. Specifically, we observe larger and statistically significant coefficients for males relative to females and for White students compared to Black and Hispanic students.[21] Even in cases in which the coefficients are not uniformly larger, we observe stronger evidence of positive, large, and significant effects on CS majors for non-FARMS students and top quartile mathematics scores students.[22] The clearest contrast in estimates is for gender, with an increased likelihood of majoring in CS in their freshmen year of 17 percentage points for males and an imprecise zero for females. When we consider these results in combination with our complier analysis, we find that subgroups with higher HQ CS take-up (males and

[20] We also estimate reduced-form effects of HQ CS exposure on CS majors across subgroups. These estimates are shown in Appendix A Table A11 and reveal similar patterns to the IV results.

[21] The coefficients for Asian students are large but imprecisely estimated.

[22] The smaller standard errors for the more advantaged subgroups are likely driven in part by the stronger first-stage relationship for these groups.

high achievers) are also more likely to be the beneficiaries of taking HQ CS in terms of their intent to major in CS.

Interestingly, the story is somewhat different when considering CS BA degree attainment. Although there is stronger evidence of large and significant effects in the first- and second-year of college for overrepresented subgroups and high-achievers, there is stronger evidence that underrepresented subgroups and relatively lower-achieving students benefit more in terms of CS BA receipt. For example, the estimated impact for female and male students is similar in magnitude although only the coefficient for males is statistically significant. Notably, the effects of taking HQ CS on the likelihood of receiving a CS BA for FARMS students are more than three times as large relative to non-FARMS students, with the coefficient for the latter group no longer statistically significant. The estimated impact is also almost 30 percent bigger for Black students compared to White students and both estimates are statistically significant. Since sample sizes for Hispanic and Asian students are small, corresponding estimates are imprecise although Asian students have the largest coefficient among all four racial/ethnic groups.[23]

In terms of achievement, although students in the bottom quartile of prior mathematics achievement exhibit the smallest estimated effect on CS BA across all subgroups of students, students from the second quartile experience the largest and only significant (marginally) effect on their CS BA receipt across the achievement distribution. The effect for third quartile students has a similar magnitude to the effect for the second quartile while the impact for the fourth quartile is smaller. Because pursuing a CS BA degree requires some mathematics proficiency, it is not surprising that the lowest mathematics achievers do not benefit much from taking HQ CS for this particular outcome. Overall, the positive and stronger impact of taking HQ CS on CS BA receipt for some historically underrepresented groups and relatively low achieving students is encouraging and suggests that the expansion

[23]We also estimated results for multiracial students, which are not shown in Table 7, with the statistically insignificant coefficients as follows: 0.0628 for first-year CS major, 0.3100 for second-year CS major, and 0.0465 for receiving a CS BA degree.

of HQ CS in high school has the potential to reduce gender, socioeconomic, and racial/ethnic disparities in the field.

7 Effects on Employment and Earnings

Given our findings on HQ CS course-taking impacts on college major choice, we may also see effects on earnings, given the wage premium for CS degrees and digital skills more broadly. Although compliers are mainly on the margin of being CS versus other STEM majors in the first- and second-year of college, these students are on the margin of CS versus a variety of major categories including other STEM, social sciences, and humanities at the time of BA receipt. Given the similarity in earnings between CS and other STEM majors but the discrepancy in earnings between CS majors and all other college majors,[24] these findings imply we should likely expect to find a positive earnings effect of HQ CS course-taking that we may not have expected had the first- and second-year results persisted to BA receipt.

Table 8 shows the impact of taking HQ CS on the likelihood of employment (i.e., have non-zero earnings in our data) and log earnings at age 24.[25] We present both reduced-form and IV estimates for all students and also for different subgroups consistent with our discussion in Section 6.2. Given we need to track students for at least 9 years to have their earnings data, our sample sizes shrink substantially in this analysis, resulting in large standard errors for both estimators. The weak first stage yields especially large standard errors for our IV estimates, so we focus our interpretation on the reduced-form results.

First, we do not see any effect on employment at age 23, which may be driven by the fact that many students take more than four years to earn a BA degree.[26] At the same

[24] In 2022, the median annual wage was $100,530 for computer and information technology occupations, $97,980 for all STEM occupations, and $44,670 for non-STEM occupations (Bureau of Labor Statistics, 2023c,a).

[25] Table A12 shows that using the inverse hyperbolic sine transformation yields estimates that are very similar to our main results.

[26] In our analytic sample, 10 percent of students complete a BA within four years, 16 percent complete within five years, and 18 percent complete within six years. Conditional on enrolling in college on-time, 21 percent of students complete a BA within four years, 31 percent complete within five years, and 35 percent

time, there is evidence that unexpected exposure to HQ CS increases students' likelihood of employment at the ages of 24 and 25. Specifically, unexpected exposure to HQ CS in high school raises the likelihood of being employed at age 24 by 2.6 percentage points and at age 25 by 3.0 percentage points. Conditional on employment at age 24, unexpected exposure to HQ CS also increases student yearly earnings by approximately 8 percent.[27]

An interesting pattern emerges when disaggregating our earnings results by student subgroups: the positive effects of HQ CS exposure on log earnings at age 24 are primarily concentrated among students who are females, FARMS, Black, and in the top quartile of the mathematics achievement distribution. All of these effects are at least marginally significant and are in the range of 10 to 14 percent. The point estimates for males and other mathematics achievement quartiles are also positive and large but imprecisely estimated. These findings are relatively consistent with our findings on CS BA receipt reported in Table 7, confirming that students who earn a CS BA are also more employable and earn significantly more given the high market demand for CS workers. We also estimate the effect of taking HQ CS on log earnings for ages 19 to 22 as shown in Table A13 and find little effect. This provides further evidence that the long-run effects of taking HQ CS are likely driven by altering postsecondary course-taking and majors as well as the occupations and industries course-takers enter after the completion of postsecondary education.[28]

complete within six years.

[27]We choose to only report effects on earnings at age 24 to balance the benefits of a larger sample size with the fact that there my be a delay in observed effects for labor market outcomes as students transition from postsecondary education to the workforce. The 2SLS results all have positive signs on the coefficients, but they are all imprecisely estimated due to the loss of statistical power.

[28]Our measure of earnings in Table 8 is conditional on having observed earnings data, so the positive effect of HQ CS exposure on employment suggests that using an earnings measure that incorporates this extensive margin change in labor supply should yield larger estimates. In Appendix A Table A14, we impute zero for individuals with missing earnings and use log of this measure of earnings plus one. Using this earnings measure increases our reduced-form and IV estimates more than threefold and both estimates are at least marginally significant.

8 Robustness

In this section, we probe the robustness of our main results and event study estimates for CS majors, further consider our earnings results, and discuss potential concerns regarding sample selection bias due to sample attrition. Appendix A Table A15 shows that our reduced-form and IV results on CS major choice and CS BA receipt are robust to several alternative sample restrictions: 1) excluding the students in never-treated schools from the sample so that the relevant comparison is between early-adopting treated schools and late-adopting not-yet treated schools (Columns (1) and (2)); 2) including incoming transfers and students with expected exposure (Columns (3) and (4)); and 3) using only students for whom we can observe on-time BA receipt (Columns (5) and (6)). In all three of these robustness checks, our estimates are statistically indistinguishable from those in our main results.

We further explore our earnings results by discussing event study estimates for these outcomes. Our event study estimates for log earnings at ages 23-25 shown in Figure A3 test for parallel pre-trends and HQ CS exposure impacts after the courses are offered. For log earnings at age 23, the TWFE pre-event coefficients are insignificantly different from zero. However, there appears to be a slight positive pre-event time trend. All of the post-event estimates are positive and both the effect magnitude and significance increase with exposure. For log earnings at age 24, none of the pre-event effects are significant and there is little evidence of a time trend. For cohorts exposed to HQ CS, there is evidence of a positive, significant increase in earnings at age 24. At age 25, treatment group students have about 10 percent lower earnings compared to control group students prior to HQ CS offering, but for the cohort with one year of HQ CS exposure this estimated difference is about zero percent. Given that treatment students have earnings in the pre-event period that are about 10 percent lower, this is suggestive of about a 10 percent impact of one year of HQ CS exposure. This pattern is consistent with the ages 23-24 event study estimates for one year of exposure.

Finally, we address concerns about sample attrition with regard college major choice.

One concern for our major outcomes is that we have more missingness for students who enroll out-of-state. If missingness is systematically related to HQ CS course-taking and CS major propensity, then this could lead to bias in our estimates. However, it is likely the case that out-of-state enrollees are more advantaged and higher ability students who are also more likely to both take HQ CS and major in CS. So, if anything, this missingness will likely bias our estimates towards zero. Regardless, our reduced-form and IV estimates for HQ CS exposure and course-taking effects on out-of-state enrollment shown in Table A16 are insignificant.

9 Conclusion

As computing has become an essential skill in both school and the workplace, many states have significantly expanded secondary CS course offerings in recent years. Maryland has emerged as a national leader in CS education, particularly with the state's 2018 Computer Science Education for All policy that required all Maryland public high schools to offer at least one HQ CS course. This paper leverages the staggered expansion of HQ CS course offerings in Maryland to study the impact of HQ CS access for all students in the state. Although recent research has studied the impacts of secondary science, general STEM, mathematics, career and technical education, and AP coursework on a wide range of outcomes, our study is the first that focuses on the impact of high school CS coursework on postsecondary fields of study and earnings (De Philippis, 2021; Broecke, 2013; Darolia et al., 2020; Görlitz and Gravert, 2018; Goodman, 2019; Cortes et al., 2015; Joensen and Nielsen, 2016; Jackson, 2014; Owen, 2023; Dougherty, 2018; Brunner et al., 2023). This is an important area of exploration to better understand how to prepare students with the skills to succeed in CS occupations, which are projected to grow rapidly in the coming years (Bureau of Labor Statistics, 2023b).

Our results provide consistent evidence that HQ CS course-taking increases students' likelihood of enrolling in a CS degree in their freshmen year by 10.2 percentage points and receiving a CS BA degree by about 5.5 percentage points. Because receiving a CS BA is a rare event for unexposed students, this effect represents a twelve-fold increase. Due to a much smaller sample size and the resulting weaker first stages, our 2SLS results for early career employment and earnings are not precise enough to draw strong inferences. However, reduced-form estimates suggest that unexpected exposure to HQ CS increases students' employment likelihood and earnings at age 24. The reduced-form estimate is relevant for policy, which to date has only mandated that at least one HQ CS course is *offered* in all Maryland high schools and not that all Maryland high school students *take* a HQ CS course in order to graduate.[29] Overall, these findings support the often claimed benefits (e.g. expanding digital skills training, boosting CS degree receipt, and increasing the supply of CS workers) of expanding CS coursework in K-12 schools. We also interpret them as particularly encouraging given that our analytic sample focuses on students enrolled in schools that serve a relatively higher share of students from lower socioeconomic backgrounds and students of color, who are traditionally underrepresented in CS and STEM fields more broadly.

Additionally, our findings also have strong implications for efforts aimed at improving the supply of underrepresented groups in the CS field. In addition to the sample focused on a unique subset of schools serving students traditionally underrepresented in CS and STEM fields, we provide evidence that HQ CS course expansion in Maryland benefited CS BA degree receipt and earnings of students from lower socioeconomic backgrounds and Black students. This finding counters some prior research that finds positive effects of advanced science coursework on STEM majors only among already overrepresented groups including males (De Philippis, 2021). In contrast, our complier analysis suggests that the take-up of such course opportunities are much lower for these same students. In other words, underrepresented students who do comply benefit with increased CS BA receipt and earnings even though underrepresented students are less likely to comply. One major goal of "For All" policies is to ensure that all students, regardless of their background, have

[29] Arkansas, Nebraska, Tennessee, South Carolina and Nevada have all passed legislation requiring CS for graduation in recent years (Spearman and Roberts, 2022).

access to educational resources provided in public school settings. But equality of access does not always guarantee the same outcomes, and sometimes can even reproduce cycles of disadvantage. Together, our findings point to the importance of boosting HQ CS course take-up among historically underrepresented students to reduce demographic gaps among CS BA recipients and in the CS workforce. A growing literature has already pointed out a few promising strategies to increase underrepresented groups' participation in CS or general STEM courses, such as culturally-relevant pedagogy (Madkins et al., 2019) and having same gender teachers (Bottia et al., 2015).

The question of which academic fields students are being pulled from is another policy-relevant finding that extends prior academic literature. Early in their college careers, impacted students are already on the margin of CS and other STEM fields, suggesting that the CS for All initiative in Maryland may only be moving students across academically similar fields of study when they start their college career. However, at the time of BA receipt, impacted students seem to, instead, be on the margin of CS versus other STEM fields, social sciences, and humanities. If the early college major results were to persist to BA receipt, it is unlikely that taking HQ CS would have much impact on earnings given the similarity in earnings between CS and other STEM fields (Bureau of Labor Statistics, 2023c,a). However, given the much more pronounced differences on the labor market returns between CS versus social sciences and humanities for BA receipt(The Hamilton Project, 2020), there is a large implied earnings effect which we confirm in our reduced-form estimates. This suggests that the welfare implications of this policy are very different considering impacts on different educational and career stages.

While we do not have data to directly test the mechanisms that can explain how exposure to high school CS coursework impact student college major choice, there may be a few possibilities. First, exposure to CS content through coursework may increase students' interest in the subject as a potential choice of major, particularly for those who had little prior exposure. For this mechanism, we would likely expect to see effects mostly for first- and second-year majors when students may be taking mostly general education courses. Second, taking a CS course may help students develop the knowledge and skills they need to be more prepared for rigorous coursework that a CS major involves in college. This may be particularly true for HQ CS courses with more rigorous content. For this mechanism, we may expect to see effects for persistence in the CS major and likely expect to see effects mostly for CS BA receipt. Given that we find larger effects in the first- and second-year, but also still find positive effects for persistence in the CS major as well as for CS BA receipt, it seems likely that both of these mechanisms may be playing a role. Third, peer effects may also be relevant since having classmates interested in similar fields of study may raise the chances of choosing a major in that field later on. Other mechanisms that are likely important will be the subject of future research, including number of HQ CS courses taken, HQ CS course type or course quality, teacher quality, and complementarities with or substitution between other courses.

Although our paper documents large impacts of HQ CS course offerings on HQ CS course-taking, CS major choice, and subsequent earnings, this finding may not be unique to CS. Other changes in course offerings may also move students towards particular majors, occupations, and potential earnings. For example, one study finds that the courses students are enrolled in at the time when they choose their majors has a large influence on the major field chosen (Patterson et al., 2021). Another study finds that providing students with labor market information about a particular major field made them significantly more likely to declare a major in that field (Conlon, 2021). In both K-12 and postsecondary education, there is likely a large role for educational institutions to play in their supply-side decision-making about what courses are offered and when that could have large impacts on educational attainment, major choices, and career paths. These choices may have profound, direct impacts on students' earnings and overall welfare. Beyond impacts on students, supply-side decisions in education also have consequences for building a skilled workforce, growing the STEM pipeline, and promoting innovation to spur economic growth.

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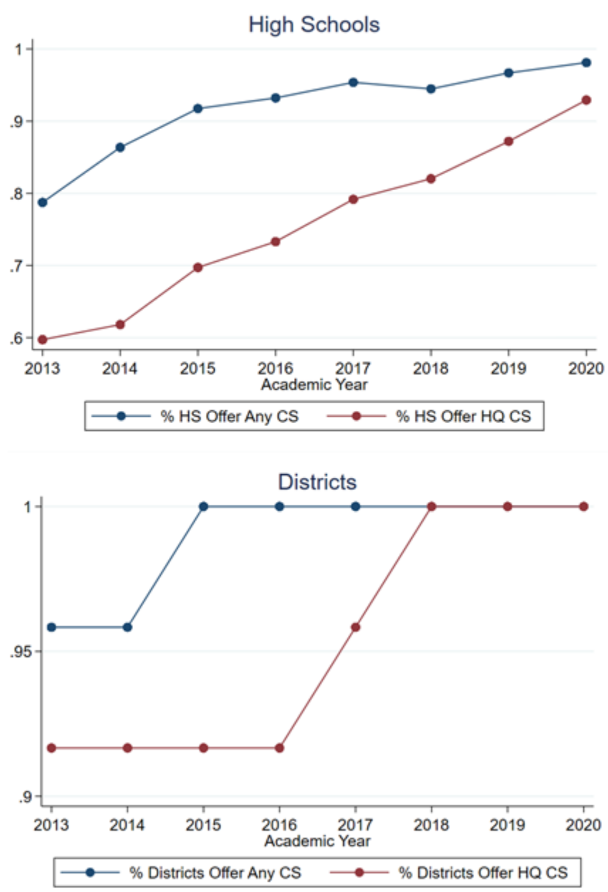
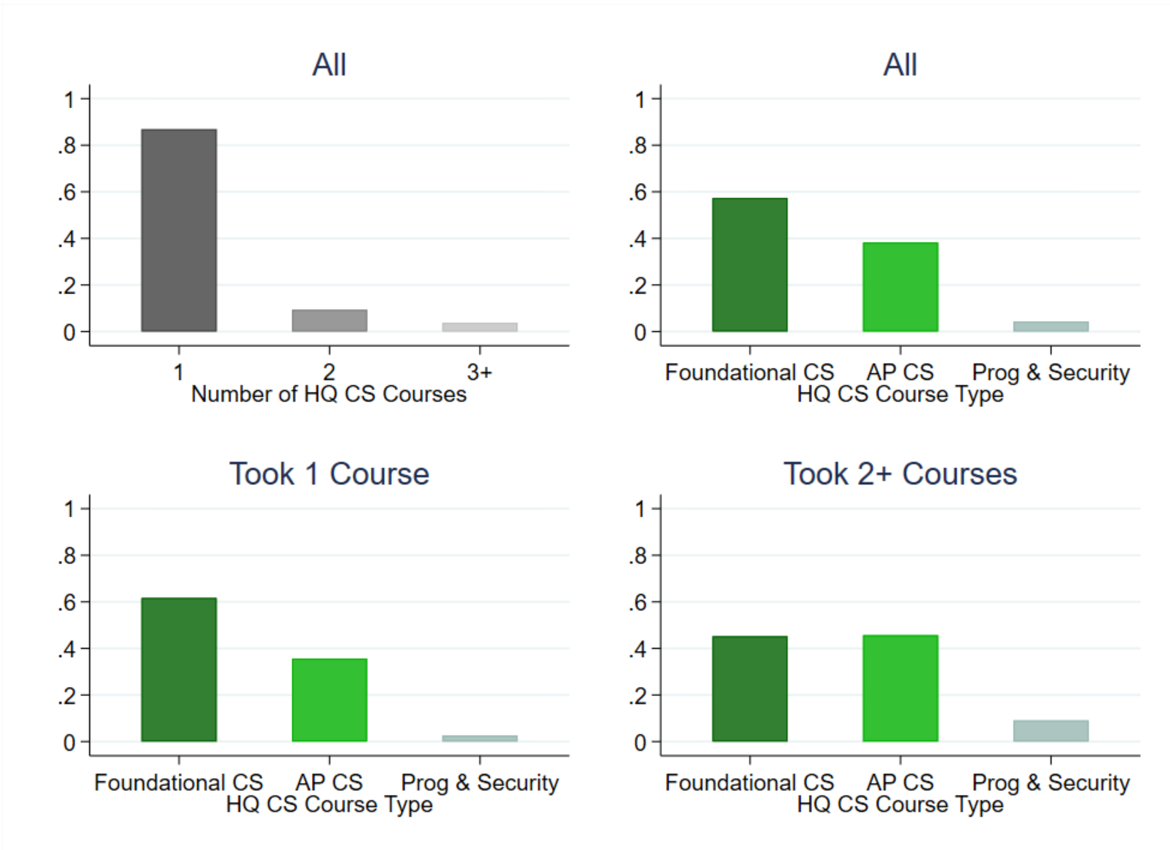
Figures and Tables

Figure 1: Trends in Maryland High Schools and Districts Offering Computer Science



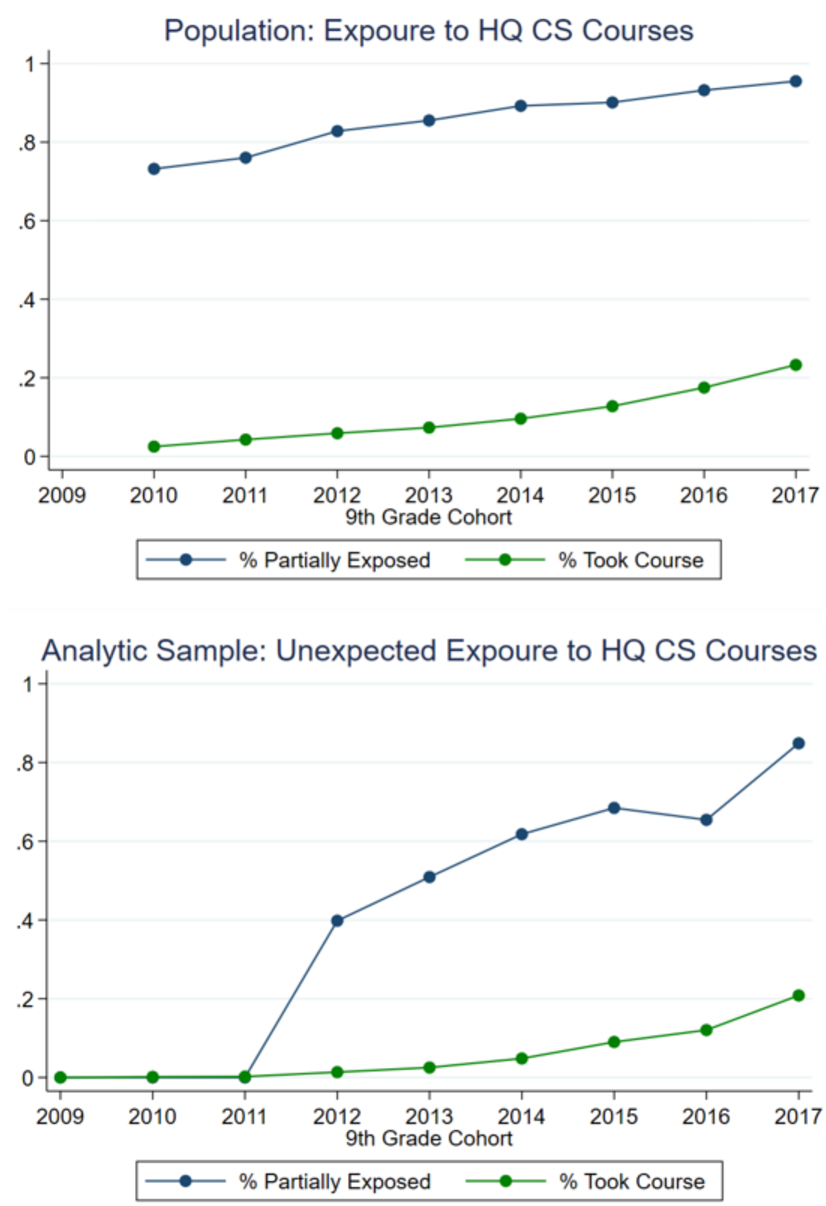
Notes: This figure shows the percent of Maryland high schools and districts offering at least one any computer science (Any CS) course and at least one high-quality computer science (HQ CS) course by academic year. Any CS refers to any CS course offered while HQ CS refers to a subset of CS courses that are considered high-quality. We define a school or district as offering a course if there is at least one student observed as enrolled in a given course in that school year. The sample of high schools consists of Maryland traditional, CTE, and charter schools from 2013 to 2020. SPED, alternative, other program, and the Maryland SEED School are excluded. District data are obtained by aggregating school-level data to the district-level. The sample of school districts includes 24 of Maryland's Local Education Agencies (LEAs) operated at the county-level. The Maryland SEED School, which is also considered an LEA, is excluded.

Figure 2: Distribution of Number and Types of HQ CS Courses Taken



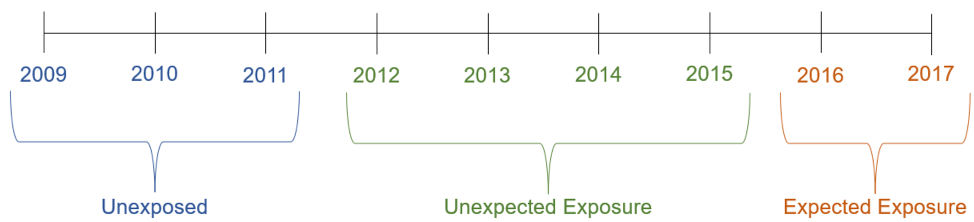
Notes: This figure shows the distributions of the number of HQ CS courses taken for all HQ CS course-takers as well as the types of HQ CS courses taken for all HQ CS course-takers, students who only took one HQ CS course, and students who took two or more HQ CS courses. The classification of HQ CS course titles are shown in Table 1. The data used to generate this figure consist of the HQ CS course-takers in the analytic sample. For the top left sub-figure, data are unique at the student level. For the other sub-figures, data are unique at the student by course level.

Figure 3: Trends in Maryland High School Student HQ CS Exposure and Course-Taking



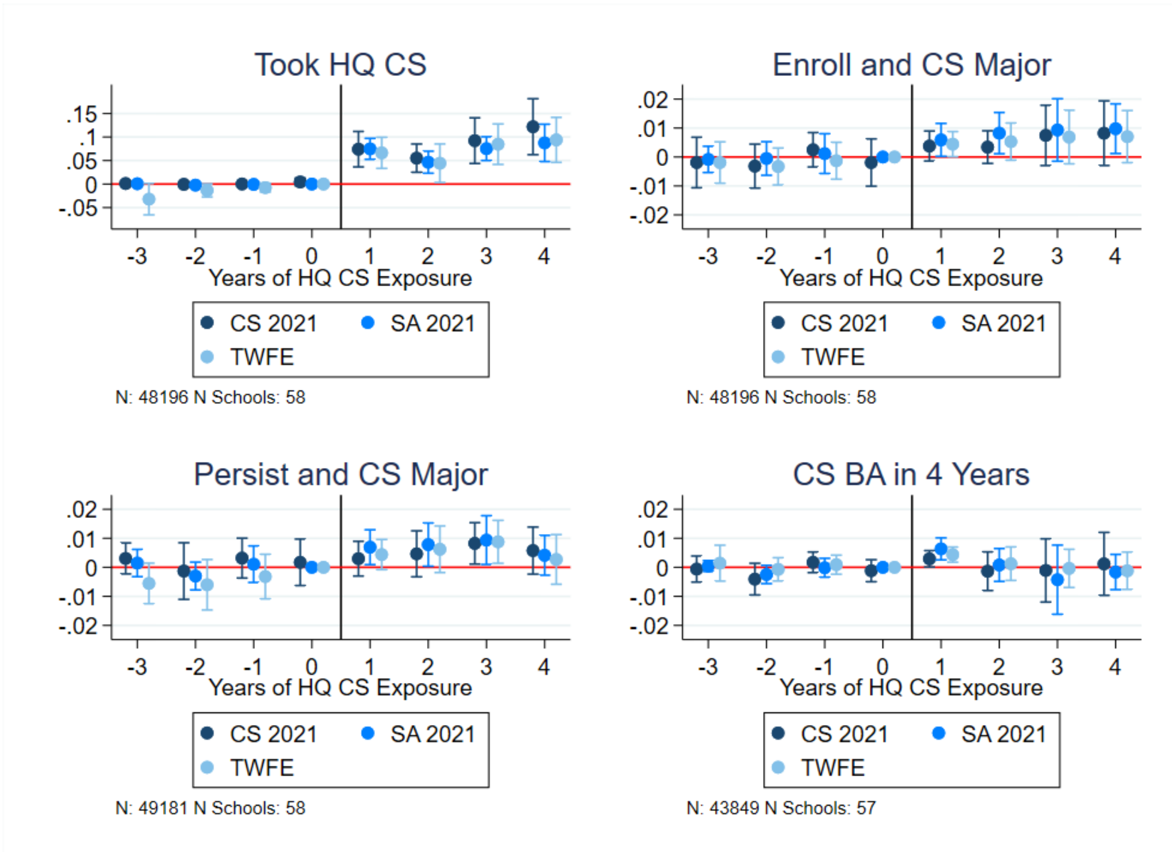
Notes: This figure shows the percent of Maryland high school students in the population and analytic sample who are exposed to and took HQ CS by 9th grade cohort. For descriptive purposes in the population, exposure is defined based on both unexpected and expected exposure, which implies that both students enrolled in high school concurrently when HQ CS is first offered and students who enroll in high school after HQ CS is first offered are considered exposed. Students whose high school enrollment ends before the introduction of HQ CS are unexposed. For the analytic sample, exposure is defined based on unexpected exposure, which implies that only students concurrently enrolled in high school when HQ CS is first offered are considered exposed while students who enroll after are excluded from the sample. Students whose high school enrollment ends before the introduction of HQ CS are unexposed. Partial exposure for the population (sample) implies being enrolled in high school during or after (during) the introduction of HQ CS. The sub-figure with our analytic sample shows the trends in the variables central to our research design.

Figure 4: Exposure Timeline for School First Offering HQ CS in 2015



Notes: The figure shows 9th grade cohort exposure for a high school that first offers HQ CS in 2015. The 2009-2011 9th grade cohorts were unexposed since they would have been expected to graduate from high school before HQ CS was first offered. The 2012-2015 9th grade cohorts were unexpectedly exposed to HQ CS since the course was first offered at some point in time while these cohorts were already concurrently enrolled in high school. The 2012 9th grade cohort had just one year of exposure while there would be an additional year of exposure for each of the subsequent three cohorts. The 2016-2017 9th grade cohorts were expectedly exposed to HQ CS since these cohorts first enrolled in high school after the course was first offered. We exclude those with expected exposure from our analytic sample.

Figure 5: Event Study Estimates of HQ CS Exposure Impacts on HQ CS Course-Taking and CS Majors



Notes: This figure reports event study point estimates and confidence intervals from regression specifications that include lead and lag indicators for HQ CS exposure as well as high school and cohort fixed effects. CS major outcomes are as described in Section 3. Statistics are computed by comparing the change in outcomes between unexpectedly exposed and unexposed cohorts between schools offering HQ CS and schools not yet or never offering HQ CS. Results are shown using the Callaway and Sant'Anna (2021) (CS 2021) estimator, Sun and Abraham (2021) (SA 2021) estimator, and the two-way-fixed effects (TWFE) estimator. The event time variable on the x-axis is a continuous variable centered at zero for the senior cohort of students in a high school the year before HQ CS was adopted, so zero and negative values represent pre-adoption cohorts and positive values represent post-adoption cohorts with increasing levels of exposure. The analytic sample is as described in Section 3 and obtained by imposing the sample restrictions shown in Appendix A Table A4. Robust standard errors used to compute confidence intervals are clustered as the high school level.

Figure 6: Distribution of HQ CS Course Types by Mathematics Quartiles

Notes: This figure shows the distribution of the types of HQ CS courses taken for students by 8th grade mathematics test score quartile. The classification of HQ CS course titles are shown in Table 1. The first quartile represents at the bottom of the achievement distribution while the fourth quartile represents students at the top of the achievement distribution. The data used to generate this figure consist of the HQ CS course-takers in the analytic sample. Data are unique at the student by course level.

Table 1: Distribution of High-Quality Computer Science Course-Taking in Analytic Sample

SCED Code	SCED Course Title	CS Course Type	N	Percent
10971	Computer Science Essentials-CTE	Foundational CS	726	27.76%
10970	Foundations of Computer Science-CTE	Foundational CS	293	11.20%
10201	Web Page Design	Foundational CS	261	9.98%
10012	Exploring Computer Science	Foundational CS	136	5.20%
10171	Information Technology-Other	Foundational CS	75	2.87%
10952	Advanced Computing Concepts and Information Technologies-CTE	Foundational CS	-	-
10951	Introduction to Programming and Applications-CTE	Foundational CS	-	-
10972	AP Computer Science Principles-CTE	AP CS	597	22.83%
10973	AP Computer Science A-CTE	AP CS	182	6.96%
10157	AP Computer Science A	AP CS	104	3.98%
10011	Computer Science Principles	AP CS	85	3.25%
10019	AP Computer Science Principles	AP CS	33	1.26%
10155	Java Programming	Programming & Cybersecurity	51	1.95%
10152	Computer Programming	Programming & Cybersecurity	40	1.53%
10154	C++ Programming	Programming & Cybersecurity	-	-
10153	Computer Programming - Other Language	Programming & Cybersecurity	-	-
10108	Network Security	Programming & Cybersecurity	-	-
10020	Cybersecurity	Programming & Cybersecurity	-	-

Notes: This table shows the distribution of high-quality computer science (HQ CS) course-taking in the analytic sample. HQ CS refers to a specific subset of CS courses that are offered in Maryland high schools. The data used to generate this table are unique at the student by course level. Courses are classified into three broad CS course type categories with guidance from the Maryland Center for Computing Education and based on baseline 8th grade mathematics scores. The Computer Science Principles course comprises the same content as AP Computer Science Principles without the AP test requirement. Data for cells with 10 or fewer observations are suppressed.

Table 2: Summary Statistics

	(1) Population	(2) Analytic Sample
Female	0.490	0.502
Free/Reduced Lunch	0.338	0.523
Black	0.348	0.498
Hispanic	0.126	0.118
White	0.410	0.312
Asian	0.060	0.025
Multiracial	0.052	0.042
English Learner	0.035	0.037
Special Education	0.123	0.162
Math Score	-0.056	-0.435
ELA Score	0.005	-0.318
Science Score	0.009	-0.385
School Total Enrollment	1,505	990
HQ CS Exposure	0.758	0.373
Took HQ CS	0.101	0.044
Any CS Exposure	0.859	0.594
Took Any CS	0.177	0.100
HS Grad in 4 Years	0.875	0.785
Enroll in College	0.631	0.509
Enroll and CS Major	0.034	0.020
Persist in College	0.545	0.408
Persist and CS Major	0.038	0.021
BA in 4 Years	0.190	0.104
CS BA in 4 Years	0.014	0.006
Earnings Age 23	\$21,277	\$20,198
Earnings Age 24	\$24,885	\$22,857
Earnings Age 25	\$27,353	\$24,699
N	635,771	50,507
N Schools	233	58

Notes: Baseline demographic characteristics and test scores are measured in 8th grade. Test scores have been standardized to have a mean of zero and standard deviation of one within each school year. School total enrollment is measured as the total enrollment in the high school in the the first year in which a student is observed as being enrolled in high school. HQ CS is a more rigorous subset of Any CS while Any CS includes all CS courses offered in Maryland high schools. In the population, CS exposure includes unexpected and expected exposure, while in the analytic sample, exposure only includes unexpected exposure by construction. All educational attainment measures are on-time rates and assume no gaps in students' educational trajectories. Earnings are measured in 2021 dollars.

Table 3: HQ CS Effects on Educational Attainment

	(1) HS Grad	(2) Enroll	(3) Persist	(4) BA Grad
Panel 1: RF Estimates				
Z	0.0108	0.0034	0.0006	0.0007
	(0.0095)	(0.0095)	(0.0085)	(0.0049)
Unexpo Mean	[.7675]	[.4933]	[.3918]	[.0991]
% Change	{1.41%}	{.70%}	{.15%}	{.71%}
N	50,507	50,507	50,507	43,871
N Schools	58	58	58	57
Panel 2: IV Estimates				
HQ CS	0.1751	0.0555	0.0097	0.0128
	(0.1738)	(0.1527)	(0.1362)	(0.0864)
Unexpo Mean	[.7675]	[.4933]	[.3918]	[.0991]
% Change	{22.81%}	{11.25%}	{2.48%}	{12.87%}
F-stat	16.5679	16.5679	16.5679	13.5183
N	50,507	50,507	50,507	43,871
N Schools	58	58	58	57

Notes: This table reports results for the impact of HQ CS exposure and course-taking on educational attainment. Outcomes in columns (1)-(4) include high school graduation, college enrollment, college persistence, and bachelor's degree (BA) receipt, which are measured as on-time rates and assume no gaps in students' educational trajectories. The table shows reduced-form (RF) results in Panel 1 and instrumental variables (IV) results in Panel 2. Reduced-form estimates show the effect of HQ CS exposure while IV estimates show the impact of HQ CS course-taking. The analytic sample is as described in Section 3 and obtained by imposing the sample restrictions shown in Appendix A Table A4. The specification is two-way fixed effects (TWFE) with high school and cohort fixed effects as well as demographic controls. For the IV estimates, unexpected exposure to HQ CS instruments for taking HQ CS. The demographic controls are as described in Section 4: gender, race, gender-race interactions, FARMS, ELL, SPED, math and ELA test scores, total high school enrollment, and indicators for imputed values. The percent change is the coefficient divided by the mean for the unexposed. Robust standard errors are clustered at the high school level.
*** p<0.01, ** p<0.05, * p<0.10

Table 4: HQ CS Effects on CS Majors

	(1) OLS CS Maj	(2) FS HQ CS	(3) RF CS Maj	(4) IV CS Maj	(5) IV CS Maj
Panel 1: Enroll and CS Major					
HQ CS	0.0654***			0.1019***	0.0738*
	(0.0130)			(0.0364)	(0.0434)
Z		0.0617***	0.0063***		
		(0.0153)	(0.0021)		
Unexpo Mean	[.0161]	[.0068]	[.0161]	[.0161]	[.0161]
% Change	{405.27%}	{911.22%}	{38.91%}	{630.99%}	{456.74%}
F-stat				16.3157	7.0743
N	48,196	48,196	48,196	48,196	48,196
N Schools	58	58	58	58	58
Panel 2: Persist and CS Major					
HQ CS	0.0704***			0.1200***	0.1253*
	(0.0139)			(0.0408)	(0.0693)
Z		0.0618***	0.0074***		
		(0.0154)	(0.0027)		
Unexpo Mean	[.0165]	[.0066]	[.0165]	[.0165]	[.0165]
% Change	{428.01%}	{944.08%}	{45.09%}	{729.13%}	{761.01%}
F-stat				16.2328	6.8163
N	49,181	49,181	49,181	49,181	49,181
N Schools	58	58	58	58	58
Panel 3: CS BA in 4 Years					
HQ CS	0.0338***			0.0547***	0.0753
	(0.0099)			(0.0185)	(0.0535)
Z		0.0551***	0.0030***		
		(0.0150)	(0.0010)		
Unexpo Mean	[.0043]	[.0039]	[.0043]	[.0043]	[.0043]
% Change	{783.12%}	{1410.15%}	{69.92%}	{1267.92%}	{1747.3%}
F-stat				13.5259	8.4135
N	43,849	43,849	43,849	43,849	43,849
N Schools	57	57	57	57	57
Trends					X

Notes: This table reports results for HQ CS exposure and course-taking impacts on CS major outcomes. Column (1) shows ordinary least squares (OLS) estimates, column (2) shows first-stage (FS) estimates, column (3) shows reduced-form (RF) estimates, and columns (4) and (5) show instrumental variables (IV) estimates. The Panel 1 outcome is enrolling in college on-time and being a first-year CS major, Panel 2 is persisting to the second-year of college on-time and being a CS major, and Panel 3 is earning a CS BA on-time. Demographic controls are as defined in Section 4 and Table 3. Column (4) shows our preferred IV specification while column (5) includes high school by cohort linear time trends. Robust standard errors are clustered at the high school level.
*** p<0.01, ** p<0.05, * p<0.10

Table 5: HQ CS Course-Taking Effects on Other Majors

	(1) CS	(2) Other STEM	(3) Non-STEM	(4) Engi	(5) Health	(6) Bus	(7) Soc Sci	(8) Hum	(9) Edu
Panel 1: Enroll and Major									
HQ CS	0.1019***	-0.1301**	0.0860	-0.0876**	0.0366	0.0067	-0.0177	0.2444	-0.0489
	(0.0364)	(0.0589)	(0.1426)	(0.0347)	(0.0622)	(0.0663)	(0.0381)	(0.1703)	(0.0453)
Unexpo Mean	[.0161]	[.0434]	[.4029]	[.0166]	[.046]	[.0399]	[.0189]	[.2665]	[.0213]
% Change	{631%}	{-300%}	{21%}	{-526%}	{80%}	{17%}	{-93%}	{92%}	{-229%}
F-stat	16.3157	16.3157	16.3157	16.3157	16.3157	16.3157	16.3157	16.3157	16.3157
N	48,196	48,196	48,196	48,196	48,196	48,196	48,196	48,196	48,196
N Schools	58	58	58	58	58	58	58	58	58
Panel 2: Persist and Major									
HQ CS	0.1200***	-0.1005*	0.0239	-0.0585**	0.0512	-0.0278	-0.0502	0.1065	-0.0337
	(0.0408)	(0.0536)	(0.1242)	(0.0245)	(0.0518)	(0.0532)	(0.0381)	(0.1178)	(0.0416)
Unexpo Mean	[.0165]	[.043]	[.3128]	[.0144]	[.0399]	[.041]	[.0256]	[.1759]	[.0222]
% Change	{729%}	{-234%}	{8%}	{-405%}	{128%}	{-68%}	{-196%}	{61%}	{-152%}
F-stat	16.2328	16.2328	16.2328	16.2328	16.2328	16.2328	16.2328	16.2328	16.2328
N	49,181	49,181	49,181	49,181	49,181	49,181	49,181	49,181	49,181
N Schools	58	58	58	58	58	58	58	58	58
Panel 3: BA Grad in 4 Years and Major									
HQ CS	0.0547***	-0.0494	0.0206	-0.0115	0.0516*	0.0227	-0.0656	-0.0517	-0.0140
	(0.0185)	(0.0461)	(0.0871)	(0.0253)	(0.0303)	(0.0296)	(0.0428)	(0.0395)	(0.0229)
Unexpo Mean	[.0043]	[.0154]	[.0792]	[.0038]	[.008]	[.0117]	[.024]	[.0175]	[.0066]
% Change	{1268%}	{-321%}	{26%}	{-302%}	{646%}	{194%}	{-274%}	{-296%}	{-211%}
F-stat	13.5259	13.5259	13.5259	13.5259	13.5259	13.5259	13.5259	13.5259	13.5259
N	43,849	43,849	43,849	43,849	43,849	43,849	43,849	43,849	43,849
N Schools	57	57	57	57	57	57	57	57	57

Notes: This table reports IV results for HQ CS course-taking impacts on different categories of major outcomes shown in each column. Panels show results at different levels of educational attainment. Column (1) shows results for CS majors for comparison and the specification is the same as our preferred specification shown in Table 4 Column (4). The Column (2) outcome Other STEM includes all non-CS STEM majors. The Column (3) outcome Non-STEM is all other non-CS non-STEM majors. The Column (4) outcome is engineering. The demographic controls are the same as those described in Section 4 and Table 3. Robust standard errors are clustered at the high school level.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 6: Characterizing Compliers with First-Stage Coefficients

	(1)	(2)	(3)	(4)
Panel 1: Gender and Socioeconomic Status				
	Females	Males	FARMS	Not FARMS
Z	0.0502***	0.0726***	0.0532***	0.0638***
	(0.0128)	(0.0191)	(0.0126)	(0.0208)
Ratio wrt Full FS	.8137	1.1765	.8617	1.0341
N	24,035	24,161	25,583	22,613
Panel 2: Race				
	Black	Hispanic	White	Asian
Z	0.0620***	0.0389**	0.0582***	0.1320**
	(0.0144)	(0.0182)	(0.0193)	(0.0653)
Ratio wrt Full FS	1.0041	.6303	.9432	2.1393
N	24,100	5,795	14,861	1,175
Panel 3: Quartiles of Math Achievement				
	1st Q	2nd Q	3rd Q	4th Q
Z	0.0396***	0.0496***	0.0323***	0.1052***
	(0.0126)	(0.0132)	(0.0111)	(0.0303)
Ratio wrt Full FS	.6419	.8036	.5243	1.7045
N	12,081	10,727	13,332	12,056

Notes: This table reports first-stage results for the following subgroups: gender, socioeconomic status, race, and quartiles of mathematics achievement. The dependent variable is an indicator for taking HQ CS. The specification is TWFE and includes high school and cohort fixed effects. The instrument Z is an indicator for unexpected exposure to HQ CS. The relative likelihood of being a complier is computed as the ratio of the subgroup-specific first-stage coefficient with respect to the first-stage coefficient for the full sample shown in Table 4, Column (2), Panel 1. Robust standard errors are clustered at the high school level. For multiracial students, the coefficient is 0.0704 and the ratio is 1.1403.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 7: Heterogeneity Analysis for CS Majors

	(1)	(2)	(3)	(4)
Panel 1: Gender and Socioeconomic Status				
	Females	Males	FARMS	Not FARMS
Enroll and CS	-0.0035	0.1723***	0.1030	0.0948***
	(0.0425)	(0.0568)	(0.0641)	(0.0368)
F-stat	15.0754	13.8651	17.6795	8.6655
N	24,035	24,161	25,583	22,613
Persist and CS	0.0940*	0.1321**	0.1167*	0.1004*
	(0.0539)	(0.0665)	(0.0617)	(0.0552)
F-stat	15.4441	13.5628	17.8775	8.5547
N	24,557	24,624	25,995	23,186
CS BA in 4 Years	0.0475	0.0570**	0.0784**	0.0228
	(0.0377)	(0.0223)	(0.0390)	(0.0217)
F-stat	12.5842	11.7861	13.665	9.9109
N	21,916	21,933	22,621	21,228
Panel 2: Race				
	Black	Hispanic	White	Asian
Enroll and CS	0.0908	0.0264	0.1260**	0.3118
	(0.0607)	(0.1310)	(0.0510)	(0.2685)
F-stat	15.9496	5.3779	8.8700	4.1392
N	24,100	5,795	14,861	1,175
Persist and CS	0.0821	0.0165	0.1651***	0.4532
	(0.0619)	(0.2139)	(0.0466)	(0.3076)
F-stat	16.2913	5.7607	8.4297	4.1839
N	24,623	5,854	15,183	1,205
CS BA in 4 Years	0.0709**	0.0532	0.0557*	0.1172
	(0.0315)	(0.0713)	(0.0324)	(0.1237)
F-stat	11.9692	9.0688	10.1801	5.8367
N	21,677	5,276	13,648	1,108
Panel 3: Quartiles of Math Achievement				
	1st Q	2nd Q	3rd Q	4th Q
Enroll and CS	0.0794	0.0951	0.0211	0.1097***
	(0.0822)	(0.1154)	(0.1175)	(0.0412)
F-stat	7.8699	13.0812	8.3321	11.8759
N	12,081	10,727	13,332	12,056
Persist and CS	0.0215	0.1790	0.2189	0.1101*
	(0.0748)	(0.1138)	(0.1619)	(0.0598)
F-stat	8.1939	13.6098	8.7908	11.6384
N	12,319	10,852	13,707	12,303
CS BA in 4 Years	0.0097	0.0798*	0.0747	0.0382
	(0.0261)	(0.0433)	(0.0693)	(0.0325)
F-stat	7.0264	8.1983	15.1543	12.0658
N	10,955	9,654	12,278	10,962

Notes: This table reports IV results for HQ CS course-taking impacts on CS major outcomes across subgroups. Robust standard errors are clustered at the high school level. For multiracial students, the coefficients are insignificantly positive including: 0.0628 for Enroll CS, 0.31 for Persist CS, and 0.0465 for CS BA.
*** p<0.01, ** p<0.05, * p<0.10

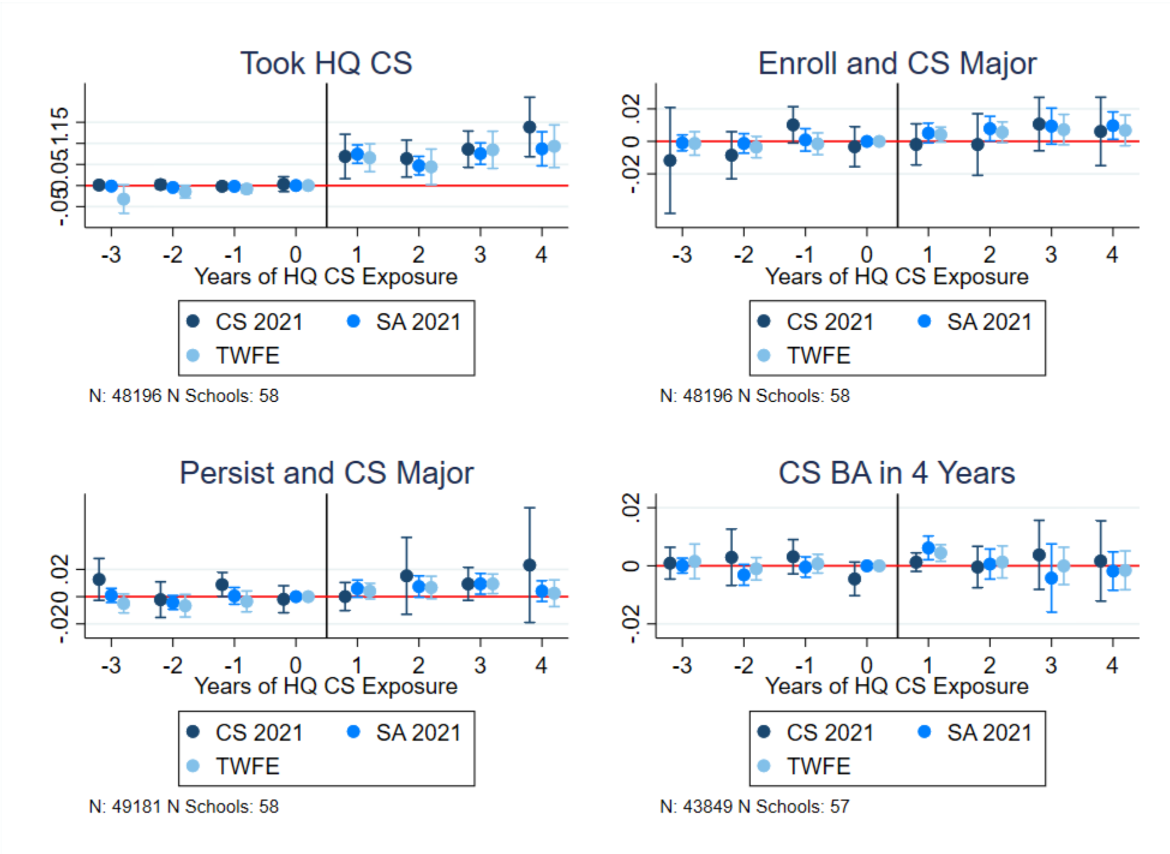
Table 8: HQ CS Effects on Employment and Log Earnings

	(1)	(2)	(3)	(4)
Panel 1: Employment and Log Earnings for Full Sample				
	Employed at 23	Employed at 24	Employed at 25	Earnings at 24
RF	0.0001	0.0263**	0.0295**	0.0802**
	(0.0103)	(0.0100)	(0.0117)	(0.0365)
IV	0.0021	0.9603	1.1505	3.1215
	(0.2337)	(0.5844)	(0.7879)	(2.0772)
F-stat	9.8193	8.7282	4.5333	6.5259
Panel 2: Log Earnings at 24 by Gender and Socioeconomic Status				
	Females	Males	FARMS	Not FARMS
RF	0.0999*	0.0580	0.1409***	-0.0079
	(0.0580)	(0.0510)	(0.0508)	(0.0455)
IV	5.0463	1.8333	5.0519	-0.3526
	(4.3223)	(1.7939)	(3.2892)	(1.9501)
F-stat	3.5764	7.5234	4.242	6.8144
N	10,777	9,475	10,532	9,720
Panel 3: Log Earnings at 24 by Race				
	Black	Hispanic	White	Asian
RF	0.1203**	-0.0957	0.0115	-0.1147
	(0.0501)	(0.1097)	(0.0505)	(0.2222)
IV	3.4022	-43.6498	0.5863	-1.9591
	(2.3983)	(103.4302)	(2.5920)	(3.6836)
F-stat	4.2231	.1907	3.5624	1.6394
N	10,449	1,572	6,742	414
Panel 4: Log Earnings at 24 by Quartiles of Math Achievement				
	1st Q	2nd Q	3rd Q	4th Q
RF	0.0514	0.0809	0.0605	0.1184*
	(0.0734)	(0.0614)	(0.0714)	(0.0660)
IV	2.0740	3.2961	4.3554	3.5933
	(3.5059)	(2.8401)	(6.1913)	(2.4497)
F-stat	2.3709	4.1064	3.2505	5.6907
N	5,072	5,072	5,045	5,063

Notes: This table reports reduced-form (RF) and IV results for the impact of HQ CS exposure and course-taking on employment and log earnings. Employment is measured using indicators for non-missing earnings at ages 23-25. We measure real annual earnings at age 24 in 2021 dollars and use the logarithmic transformation of real annual earnings. Panel 1 shows results for the full sample, Panel 2 shows results across gender and socioeconomic status, Panel 3 shows results across races, and Panel 4 shows results across quartiles of math achievement. The reduced-form and IV specifications are the same as those shown in Table 4, Columns (3) and (4), respectively. Robust standard errors are clustered at the high school level. For multiracial students, the coefficients are insignificant including 0.1657 for the reduced-form estimate and 4.5502 for the IV estimate.
*** p<0.01, ** p<0.05, * p<0.10

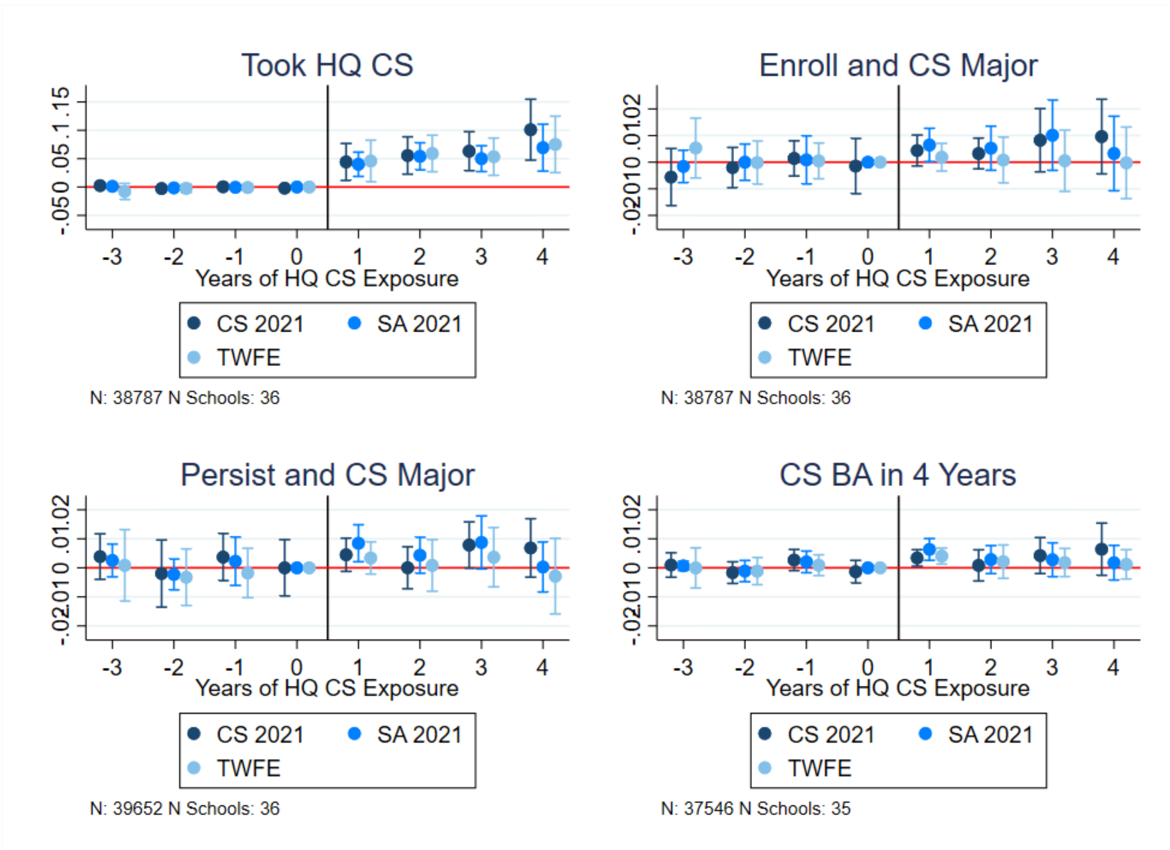
A Appendix

Figure A1: HQ CS Exposure Impacts on HQ CS Course-Taking and CS Majors with Demographic Controls



Notes: This figure replicates the first-stage and CS major event study results shown in Figure 5, but this specification includes demographic controls as described in Section 4. CS major outcomes are as described in Section 3. Statistics are computed by comparing the change in outcomes between unexpectedly exposed and unexposed cohorts between schools offering HQ CS and schools not yet or never offering HQ CS. Results are shown using the Callaway and Sant'Anna (2021) (CS 2021) estimator, Sun and Abraham (2021) (SA 2021) estimator, and the two-way-fixed effects (TWFE) estimator. The event time variable on the x-axis is a continuous variable centered at zero for the senior cohort of students in a high school the year before HQ CS was adopted, so zero and negative values represent pre-adoption cohorts and positive values represent post-adoption cohorts with increasing levels of exposure. Robust standard errors used to compute confidence intervals are clustered as the high school level.

Figure A2: HQ CS Exposure Impacts on HQ CS Course-Taking and CS Majors Excluding Never-Treated



Notes: This figure replicates the first-stage and CS major event study results shown in Figure 5 with a sample that excludes students in never-treated and closed high schools. CS major outcomes are as described in Section 3. Statistics are computed by comparing the change in outcomes between unexpectedly exposed and unexposed cohorts between schools offering HQ CS and schools not yet offering HQ CS. Results are shown using the Callaway and Sant'Anna (2021) (CS 2021) estimator, Sun and Abraham (2021) (SA 2021) estimator, and the two-way-fixed effects (TWFE) estimator. The event time variable on the x-axis is a continuous variable centered at zero for the senior cohort of students in a high school the year before HQ CS was adopted, so zero and negative values represent pre-adoption cohorts and positive values represent post-adoption cohorts with increasing levels of exposure. Robust standard errors used to compute confidence intervals are clustered as the high school level.

Figure A3: Event Study Estimates of HQ CS Exposure Impacts on Log Earnings

Notes: This figure reports event study point estimates and confidence intervals from regression specifications that include lead and lag indicators for HQ CS exposure as well as high school and cohort fixed effects. We measure real annual earnings in 2021 dollars and use the logarithmic transformation of real annual earnings. Statistics are computed by comparing the change in outcomes between unexpectedly exposed and unexposed cohorts between schools offering HQ CS and schools not yet or never offering HQ CS. Results are shown using the the two-way-fixed effects (TWFE) estimator. The event time variable on the x-axis is a continuous variable centered at zero for the senior cohort of students in a high school the year before HQ CS was adopted, so zero and negative values represent pre-adoption cohorts and positive values represent post-adoption cohorts with increasing levels of exposure. The analytic sample is as described in Section 3 and obtained by imposing the sample restrictions shown in Appendix A Table A4. Robust standard errors used to compute confidence intervals are clustered as the high school level.

Table A1: Any CS Courses Offered in Maryland High Schools

SCED Code	SCED Course Title	N Districts	N Schools	% of Schools
10972	AP Computer Science Principles-CTE	17	112	55%
10970	Foundations Of Computer Science-CTE	13	92	45%
10973	AP Computer Science A-CTE	15	92	45%
10911	Principles Of Arts, Media and Communication-CTE	16	57	28%
10012	Exploring Computer Science	7	54	26%
10912	Interactive Media and Design Level I-CTE	13	40	20%
10157	AP Computer Science A	5	38	19%
10921	CCNA I - Intro To Networks-CTE	11	38	19%
10971	Computer Science Essentials-CTE	4	37	18%
10913	Interactive Media and Design Level II-CTE	12	36	18%
10922	CCNA II - Routing and Switching Essentials-CTE	10	35	17%
10004	Computer Applications	5	31	15%
10952	Advanced Computing Concepts and Information Technologies-CTE	5	31	15%
10001	Introduction To Computer Technology	7	29	14%
10914	Interactive Media Portfolio Capstone-CTE	9	28	14%
10943	IT Dual Enrollment-CTE	3	26	13%
10002	Computing Systems	2	24	12%
10201	Web Page Design	5	24	12%
10961	Java Fundamentals-CTE	4	24	12%
10908	Digital Imaging-CTE	1	23	11%
10947	Computer and Information Sciences Capstone-CTE	4	21	10%
10920	IT Essentials-CTE	7	20	10%
10948	IT Apprenticeship-CTE	4	19	9%
10202	Computer Graphics	3	18	9%
10011	Computer Science Principles	2	17	8%
10931	Cybersecurity Essentials-CTE	5	16	8%
10953	Specialized Topics In Computer and Information Sciences-CTE	2	14	7%
10152	Computer Programming	6	13	6%

Notes: These data were obtained from the Maryland Computer Science Dashboard created by the Maryland Center for Computing Education using MLDS data. Data are based on CS courses offered and student enrollment in the 2019-2020 school year (MCCE, 2022).

Table A1 (Continued): Any CS Courses Offered in Maryland High Schools

SCED Code	SCED Course Title	N Districts	N Schools	% of Schools
10019	AP Computer Science Principles	4	11	5%
10934	NDG Linux Essentials-CTE	3	10	5%
10995	Information Technology—Aide	2	10	5%
10998	Information Technology—Workplace Experience	1	10	5%
10999	Information Technology—Other	3	9	4%
10933	Cyber Ops-CTE	2	8	4%
10102	Networking Systems	2	7	3%
10159	IB Computing Studies	4	7	3%
10949	IT General WBL-CTE	1	7	3%
10951	Introduction To Programming and Applications-CTE	2	7	3%
10101	Network Technology	3	6	3%
10962	Database Foundations-CTE	3	6	3%
10932	CCNA Security-CTE	2	5	2%
10008	Particular Topics In Computer Literacy	1	4	2%
10251	Computer Technology	2	4	2%
10256	Particular Topics In Information Support and Services	1	4	2%
10965	Java Foundations-CTE	1	4	2%
10974	Cybersecurity-CTE	2	4	2%
10049	Computer Literacy—Other	1	3	1%
10051	Information Management	2	3	1%
10108	Network Security	1	3	1%
10154	C++ Programming	1	3	1%
10199	Computer Programming—Other	3	3	1%
10254	IT Essentials: PC Hardware and Software	1	3	1%
10966	Java Programming-CTE	1	3	1%
10055	Particular Topics In Management Information Systems	1	2	1%
10111	Particular Topics In Networking Systems	1	2	1%

Table A1 (Continued): Any CS Courses Offered in Maryland High Schools

SCED Code	SCED Course Title	N Districts	N Schools	% of Schools
10155	Java Programming	2	2	1%
10156	Computer Programming—Other Language	1	2	1%
10160	Particular Topics In Computer Programming	1	2	1%
10923	CCNA III - Scaling Networks-CTE	2	2	1%
10924	CCNA IV - Connecting Networks-CTE	2	2	1%
10997	Information Technology—Independent Study	1	2	1%
10003	Computer and Information Technology	1	1	0%
10005	Business Computer Applications	1	1	0%
10007	IB Information Technology In A Global Society	1	1	0%
10016	PLTW Cybersecurity	1	1	0%
10052	Database Management and Data Warehousing	1	1	0%
10099	Management Information Systems—Other	1	1	0%
10109	Essentials Of Network Operating Systems	1	1	0%
10147	Networking Systems—Independent Study	1	1	0%
10148	Networking Systems—Workplace Experience	1	1	0%
10203	Interactive Media	1	1	0%
10205	Computer Gaming and Design	1	1	0%
10297	Information Support and Services—Independent Study	1	1	0%
10301	Computer Forensics	1	1	0%
10302	Cyber Crime	1	1	0%
10919	Introduction To The Internet Of Things (IOT)-CTE	1	1	0%
10935	Network Essentials-CTE	1	1	0%
10937	Network Operations 1 A/B-CTE	1	1	0%
10939	Introduction To Operating Systems-CTE	1	1	0%

Table A2: Descriptive Statistics on Non-Missing Majors

	(1) Analytic Sample
Non-Missing Enroll Major if In-State	0.980
Non-Missing Enroll Major if Out-of-State	0.651
Non-Missing Enroll Major if Public	0.969
Non-Missing Enroll Major if Private	0.733
Non-Missing Persist Major if In-State	0.996
Non-Missing Persist Major if Out-of-State	0.724
Non-Missing Persist Major if Public	0.983
Non-Missing Persist Major if Private	0.808
Non-Missing BA Major if In-State	1.000
Non-Missing BA Major if Out-of-State	0.989
Non-Missing BA Major if Public	1.000
Non-Missing BA Major if Private	0.989
N	50,507

Notes: This table shows the percent of students with non-missing college major data at different levels of educational attainment and different institution types in the analytic sample. The levels of educational attainment include first-year college enrollment, second-year college persistence, and BA degree receipt. The different types of institutions include in-state, out-of-state, public, and private.

Table A3: Analytic Sample Cohorts and Observed Outcomes

	9th Grade Cohort Year								
School Year	2009	2010	2011	2012	2013	2014	2015	2016	2017
2008-09	HSY1								
2009-10	HSY2	HSY1							
2010-11	HSY3	HSY2	HSY1						
2011-12	HSY4	HSY3	HSY2	HSY1					
2012-13	PSY1	HSY4	HSY3	HSY2	HSY1				
2013-14	PSY2	PSY1	HSY4	HSY3	HSY2	HSY1			
2014-15	PSY3	PSY2	PSY1	HSY4	HSY3	HSY2	HSY1		
2015-16	PSY4	PSY3	PSY2	PSY1	HSY4	HSY3	HSY2	HSY1	
2016-17	Earn Age 23	PSY4	PSY3	PSY2	PSY1	HSY4	HSY3	HSY2	HSY1
2017-18	Earn Age 24	Earn Age 23	PSY4	PSY3	PSY2	PSY1	HSY4	HSY3	HSY2
2018-19	Earn Age 25	Earn Age 24	Earn Age 23	PSY4	PSY3	PSY2	PSY1	HSY4	HSY3
2019-20		Earn Age 25	Earn Age 24	Earn Age 23	PSY4	PSY3	PSY2	PSY1	HSY4
2020-21			Earn Age 25	Earn Age 24	Earn Age 23	PSY4	PSY3	PSY2	PSY1
2021-22				Earn Age 25	Earn Age 24	Earn Age 23	PSY4	PSY3	PSY2

Notes: This table shows the school years that we can observe outcomes for each 9th grade cohort assuming no gaps in students' educational trajectories and progression to the workforce. HSY stands for "high school year" and PSY stands for "post-secondary year". Earnings are measured through age 25 since that is the latest age that we can observe earnings for the 2012 9th grade cohort, which is the first cohort with some students exposed to HQ CS course offering in our analytic sample.

Table A4: Sample Restrictions

	N	N Schools
Total Population	**1,072,356**	**364**
Drop Specialized Schools	1,059,976	283
Drop 2005-2008, 2018-2020 Cohorts	637,678	278
Drop K-8 Schools (Yields Target Population)	**635,771**	**233**
Drop Any CS 2013	66,381	59
Drop Outgoing Transfers	60,984	59
Drop Incoming Transfers	57,133	58
Drop Expected Exposure (Yields Analytic Sample)	**50,507**	**58**

Notes: This table shows the number of observations and schools that remain after imposing additional sample restrictions. The full population of Maryland high school students is shown in bold in the top row. The target population, which is obtained after making a standard set of restrictions, is shown in bold in the fourth row. This includes students in the 2009-2017 9th grade cohorts enrolled in Maryland public traditional, CTE, and charter high schools. A few additional sample restrictions are necessary in order to implement our research design. The analytic sample after the final restriction is made is shown in bold in the bottom row.

Table A5: Balance Tests Between High Schools Offering and Not Offering HQ CS

	(1) Never-Treated	(2) Treated	(3) Difference	(4) P-Value
Female	0.533	0.496	-0.036	0.241
Free/Reduced Lunch	0.662	0.499	-0.162	0.049
Black	0.782	0.451	-0.332	0.001
Hispanic	0.044	0.130	0.086	0.098
White	0.127	0.343	0.216	0.024
Asian	0.017	0.026	0.009	0.411
Multiracial	0.024	0.045	0.021	0.041
English Learner	0.010	0.042	0.032	0.082
Special Education	0.196	0.156	-0.040	0.229
Math Score	-0.647	-0.399	0.248	0.222
ELA Score	-0.486	-0.290	0.195	0.339
Science Score	-0.658	-0.340	0.318	0.101
School Total Enrollment	415	1,087	672	0.000
Took HQ CS	0.010	0.050	0.040	0.000
Unexp Expo Any CS	0.228	0.582	0.354	0.000
Took Any CS	0.157	0.090	-0.066	0.339
HS Grad in 4 Years	0.676	0.804	0.127	0.069
Enroll in College	0.422	0.523	0.102	0.284
Enroll and CS Major	0.010	0.022	0.012	0.001
Persist in College	0.336	0.419	0.083	0.402
Persist and CS Major	0.009	0.024	0.015	0.000
BA in 4 Years	0.101	0.105	0.004	0.939
CS BA in 4 Years	0.005	0.006	0.001	0.783
Earnings Age 23	$15,114	$21,085	$5,971	0.000
Earnings Age 24	$16,923	$23,953	$7,029	0.000
Earnings Age 25	$18,274	$25,962	$7,688	0.000
N	7,267	43,240	50,507	
N Schools	21	37	58	

Notes: Balance tests are performed to assess differences in observable characteristics between students enrolled in high schools never offering HQ CS (never-treated) and students enrolled in high schools offering HQ CS (treated) in the analytic sample. All educational attainment measures are on-time rates and assume no gaps in students' educational trajectories. Earnings are measured in 2021 dollars. P-values are based on robust standard errors clustered at the high school level.

Table A6: Balance Tests Between Unexposed and Unexpectedly Exposed Students

	(1) Unexposed	(2) Unexpectedly Exposed	(3) Difference	(4) P-Value
Female	0.510	0.487	-0.023	0.097
Free/Reduced Lunch	0.544	0.487	-0.057	0.066
Black	0.548	0.415	-0.133	0.010
Hispanic	0.077	0.187	0.110	0.027
White	0.311	0.315	0.004	0.932
Asian	0.022	0.030	0.008	0.040
Multiracial	0.037	0.049	0.011	0.004
English Learner	0.025	0.058	0.033	0.028
Special Education	0.167	0.152	-0.015	0.199
Math Score	-0.489	-0.344	0.145	0.052
ELA Score	-0.348	-0.269	0.079	0.192
Science Score	-0.424	-0.321	0.103	0.200
School Total Enrollment	896	1,149	253	0.029
Took HQ CS	0.006	0.107	0.101	0.000
Unexp Expo Any CS	0.308	1.000	0.692	0.000
Took Any CS	0.054	0.177	0.123	0.000
HS Grad in 4 Years	0.768	0.816	0.048	0.077
Enroll in College	0.493	0.535	0.041	0.237
Enroll and CS Major	0.016	0.027	0.011	0.000
Persist in College	0.392	0.434	0.042	0.198
Persist and CS Major	0.016	0.030	0.013	0.000
BA in 4 Years	0.099	0.116	0.017	0.381
CS BA in 4 Years	0.004	0.009	0.005	0.010
Earnings Age 23	$19,985	$20,910	$925	0.328
Earnings Age 24	$22,591	$24,225	$1,634	0.214
Earnings Age 25	$24,535	$26,411	$1,877	0.228
N	31,669	18,838	50,507	
N Schools	54	36	58	

Notes: Balance tests are performed to assess differences in observable characteristics between unexposed and unexpectedly exposed students in the analytic sample. We test for balance on observable characteristics using the instrument Z, which is the indicator for unexpected exposure to HQ CS. All educational attainment measures are on-time rates and assume no gaps in students' educational trajectories. Earnings are measured in 2021 dollars. P-values are based on robust standard errors clustered at the high school level.

Table A7: Balance Tests Between High Schools that Adopt HQ CS Early and Late

	(1) Early Adopter	(2) Late Adopter	(3) Difference	(4) P-Value
Female	0.484	0.511	0.028	0.320
Free/Reduced Lunch	0.466	0.538	0.072	0.484
Black	0.438	0.466	0.028	0.841
Hispanic	0.199	0.050	-0.149	0.070
White	0.270	0.429	0.159	0.220
Asian	0.033	0.017	-0.016	0.119
Multiracial	0.054	0.034	-0.020	0.071
English Learner	0.068	0.012	-0.056	0.053
Special Education	0.138	0.177	0.039	0.096
Math Score	-0.412	-0.383	0.029	0.878
ELA Score	-0.289	-0.292	-0.003	0.984
Science Score	-0.356	-0.320	0.036	0.839
School Total Enrollment	1,242	903	-339	0.194
Took HQ CS	0.045	0.055	0.010	0.524
Unexp Expo Any CS	0.611	0.540	-0.071	0.143
Took Any CS	0.095	0.084	-0.011	0.590
HS Grad in 4 Years	0.789	0.821	0.032	0.547
Enroll in College	0.523	0.524	0.002	0.981
Enroll and CS Major	0.025	0.019	-0.006	0.222
Persist in College	0.425	0.413	-0.011	0.886
Persist and CS Major	0.026	0.020	-0.006	0.280
BA in 4 Years	0.101	0.111	0.010	0.801
CS BA in 4 Years	0.006	0.006	-0.001	0.706
Earnings Age 23	$20,400	$21,950	$1,549	0.335
Earnings Age 24	$23,158	$24,948	$1,790	0.391
Earnings Age 25	$25,109	$27,044	$1,934	0.423
N	23,376	19,864	43,240	
N Schools	17	20	37	

Notes: Balance tests are performed to assess differences in observable characteristics between students enrolled in high schools that offer HQ CS early (early adopter) and students enrolled in high schools that offer HQ CS late (late adopter). This sample only includes high schools that offer HQ CS (excludes never-treated schools). We test for balance on observable characteristics using an indicator for being a "late" adopter, which includes all schools that first offer HQ CS from 2017 to 2020. The "early" adopters begin offering HQ CS in 2015 or 2016. This classification of early- and late-adopters is only used for this particular table; elsewhere in the analysis early- and late-adopters has a more general meaning consistent with recent event study literature (Roth et al., 2023). All educational attainment measures are on-time rates and assume no gaps in students' educational trajectories. Earnings are measured in 2021 dollars. P-values are based on robust standard errors clustered at the high school level.

Table A8: Balance Tests for Changes in Observable Characteristics

	(1) TWFE	(2) CS 2021	(3) SA 2021
Female	-0.0118	-0.0243	-0.0300**
	(0.0103)	(0.0162)	(0.0138)
Free/Reduced Lunch	-0.0092	0.0283	0.0051
	(0.0129)	(0.0210)	(0.0212)
Black	-0.0144	0.0255	-0.0160
	(0.0140)	(0.0305)	(0.0206)
Hispanic	0.0180	0.0205	0.0259
	(0.0154)	(0.0217)	(0.0196)
White	-0.0020	-0.0220	0.0057
	(0.0100)	(0.0223)	(0.0152)
Asian	-0.0001	-0.0061	-0.0018
	(0.0033)	(0.0054)	(0.0039)
Multiracial	0.0017	-0.0098	-0.0062
	(0.0038)	(0.0074)	(0.0070)
English Learner	0.0092**	0.0060	0.0090**
	(0.0044)	(0.0051)	(0.0039)
Special Education	-0.0204**	-0.0140	-0.0284**
	(0.0097)	(0.0152)	(0.0141)
Math Score	0.0126	-0.0598	0.0363
	(0.0398)	(0.0615)	(0.0430)
ELA Score	0.0150	-0.0663	0.0240
	(0.0246)	(0.0572)	(0.0336)
Science Score	0.0604	-0.0661	0.0256
	(0.0385)	(0.0504)	(0.0477)
Joint Test P-Value	.1216		
N	50,506	50,506	50,506
N Schools	57	57	57

Notes: We test for changes in observable characteristics before and after HQ CS offering between schools offering and schools not yet or never offering HQ CS using the instrument Z, which is the indicator for unexpected exposure to HQ CS. Column (1) uses the two-way fixed effects (TWFE) estimator, Column (2) uses the Callaway and Sant'Anna (2021) (CS 2021) estimator, and Column (3) uses the Sun and Abraham (2021) (SA 2021) estimator. Robust standard errors are clustered at the high school level. The joint test regresses the exposure instrument Z on school and cohort fixed effects as well as demographic characteristics and tests whether the coefficients for the demographic characteristics are jointly zero.
*** p<0.01, ** p<0.05, * p<0.10

Table A9: Distribution of the Number of HQ CS Courses Taken by Subgroup

	(1)	(2)	(3)	(4)
Panel 1: Gender and Socioeconomic Status				
	Females	Males	FARMS	Not FARMS
1	0.922	0.833	0.850	0.887
2	0.061	0.115	0.110	0.077
3+	0.017	0.052	0.039	0.036
N	887	1,337	1,096	1,128
Panel 2: Race				
	Black	Hispanic	White	Asian
1	0.872	0.797	0.907	0.756
2	0.088	0.127	0.074	0.185
3+	0.039	0.075	0.020	0.059
N	1,191	212	612	119
Panel 3: Quartiles of Math Achievement				
	1st Q	2nd Q	3rd Q	4th Q
1	0.929	0.872	0.844	0.848
2	0.062	0.089	0.109	0.104
3+	0.009	0.039	0.048	0.048
N	467	437	505	815
Panel 4: Years of Exposure				
	1	2	3	4
1	0.993	0.945	0.886	0.720
2	0.006	0.052	0.094	0.178
3+	0.002	0.003	0.019	0.103
N	534	328	466	692

Notes: This table shows the distribution of the number of HQ CS courses taken for the following subgroups: gender, socioeconomic status, race, quartiles of mathematics achievement, and years of exposure. The data used to produce this table consist of the HQ CS course-takers in the analytic sample. Data are unique at the student level. For multiracial students who take HQ CS, 88.2 percent of students take one course, 9.4 percent of students take two courses, and 2.4 percent of students take three or more courses.

Table A10: Characterizing Compliers with Summary Statistics

	(1) HQ CS	(2) Foundational CS	(3) AP CS	(4) Prog & Security
Female	0.399	0.436	0.314	0.226
Free/Reduced Lunch	0.507	0.629	0.307	0.368
Black	0.536	0.729	0.241	0.425
Hispanic	0.095	0.117	0.078	0.075
White	0.275	0.097	0.524	0.377
Asian	0.054	0.029	0.101	0.047
Multiracial	0.038	0.027	0.053	0.075
Math Score	-0.184	-0.597	0.475	0.189
N	2,224	1,422	900	106

Notes: This table reports mean values for demographic characteristics and 8th grade mathematics test scores for different groups of HQ CS course-takers. Column (1) shows means for all HQ CS course-takers, Column (2) shows means for Foundational CS course-takers, Column (3) shows means for AP CS course-takers, and Column (4) shows means for Programming & Cybersecurity course-takers. The data used to produce this table consist of HQ CS course-takers in the analytic sample. Data are unique at the student level.

Table A11: Heterogeneity Analysis of HQ CS Exposure Effects on CS Majors

	(1)	(2)	(3)	(4)
Panel 1: Gender and Socioeconomic Status				
	Females	Males	FARMS	Not FARMS
Enroll and CS	-0.0002	0.0129***	0.0055*	0.0060**
	(0.0021)	(0.0041)	(0.0030)	(0.0026)
N	24,035	24,161	25,583	22,613
Persist and CS	0.0046**	0.0099*	0.0062**	0.0064
	(0.0022)	(0.0055)	(0.0028)	(0.0039)
N	24,557	24,624	25,995	23,186
CS BA in 4 Years	0.0018	0.0041**	0.0035***	0.0015
	(0.0013)	(0.0020)	(0.0013)	(0.0015)
N	21,916	21,933	22,621	21,228
Panel 2: Race				
	Black	Hispanic	White	Asian
Enroll and CS	0.0056	0.0011	0.0075*	0.0450*
	(0.0035)	(0.0054)	(0.0042)	(0.0258)
N	24,100	5,795	14,861	1,175
Persist and CS	0.0051	0.0007	0.0098**	0.0633**
	(0.0038)	(0.0090)	(0.0047)	(0.0241)
N	24,623	5,854	15,183	1,205
CS BA in 4 Years	0.0038***	0.0018	0.0035	0.0187
	(0.0013)	(0.0021)	(0.0028)	(0.0224)
N	21,677	5,276	13,648	1,108
Panel 3: Quartiles of Math Achievement				
	1st Q	2nd Q	3rd Q	4th Q
Enroll and CS	0.0030	0.0045	0.0007	0.0116**
	(0.0031)	(0.0051)	(0.0039)	(0.0044)
N	12,081	10,727	13,332	12,056
Persist and CS	0.0008	0.0086*	0.0072	0.0118**
	(0.0029)	(0.0049)	(0.0053)	(0.0056)
N	12,319	10,852	13,707	12,303
CS BA in 4 Years	0.0003	0.0033**	0.0022	0.0041
	(0.0008)	(0.0015)	(0.0020)	(0.0034)
N	10,955	9,654	12,278	10,962

Notes: This table reports reduced-form results for HQ CS exposure impacts on CS major outcomes across the following subgroups: gender, socioeconomic status, race, and quartiles of mathematics achievement. Robust standard errors are clustered at the high school level. For multiracial students, coefficients are insignificantly positive including 0.0046 for Enroll CS, 0.022 for Persist CS, and 0.0033 for CS BA.
*** p<0.01, ** p<0.05, * p<0.10

Table A12: HQ CS Effects on Other Earnings Measures

	(1) RF	(2) IV	(3) RF	(4) IV
Panel 1: Earnings Age 23				
	IHS		*Real*	
Z	0.0532*		532	
	(0.0317)		(366)	
HQ CS		1.2463		12,457
		(0.8874)		(9,821)
F-stat		9.933		9.933
N	24,757	24,757	24,757	24,757
N Schools	53	53	53	53
Panel 2: Earnings Age 24				
	IHS		*Real*	
Z	0.0791**		-83	
	(0.0365)		(549)	
HQ CS		3.0771		-3,232
		(2.0633)		(20,982)
F-stat		6.5259		6.5259
N	20,252	20,252	20,252	20,252
N Schools	52	52	52	52
Panel 3: Earnings Age 25				
	IHS		*Real*	
Z	0.0414		499	
	(0.0491)		(835)	
HQ CS		1.6031		19,299
		(1.9593)		(31,416)
F-stat		3.4052		3.4052
N	15,843	15,843	15,843	15,843
N Schools	51	51	51	51

Notes: This table reports reduced-form (RF) and IV results for HQ CS exposure and course-taking impacts on the inverse hyperbolic sine (IHS) of earnings in Columns (1)-(2) and untransformed real annual earnings in Columns (3)-(4). Panels 1-3 show estimates for ages 23-25. Earnings measures are in 2021 dollars. The specifications are the same as those in Table 4, Columns (3) and (4), respectively. Robust standard errors are clustered at the high school level.
*** p<0.01, ** p<0.05, * p<0.10

Table A13: HQ CS Effects on Log Earnings at Ages 19-22

	(1) RF	(2) IV
Panel 1: Log Earnings Age 19		
Z	-0.0618	
	(0.0389)	
HQ CS		-1.0355
		(0.6510)
F-stat		18.9058
N	32,533	32,533
N Schools	56	56
Panel 2: Log Earnings Age 20		
Z	-0.0072	
	(0.0340)	
HQ CS		-0.1144
		(0.5298)
F-stat		16.9857
N	33,890	33,890
N Schools	57	57
Panel 3: Log Earnings Age 21		
Z	-0.0263	
	(0.0310)	
HQ CS		-0.5502
		(0.6107)
F-stat		11.9216
N	32,041	32,041
N Schools	56	56
Panel 4: Log Earnings Age 22		
Z	-0.0165	
	(0.0284)	
HQ CS		-0.3072
		(0.5125)
F-stat		13.1109
N	29,431	29,431
N Schools	55	55

Notes: This table reports reduced-form (RF) and IV results for HQ CS exposure and course-taking impacts on log earnings from ages 19-22 in Panels 1-4. Log earnings are measured in 2021 dollars. The specifications are the same as those in Table 4, Columns (3) and (4), respectively. Robust standard errors are clustered at the high school level. *** p<0.01, ** p<0.05, * p<0.10

Table A14: HQ CS Effects on Earnings Measures with Imputations

	(1) Log	(2) IHS	(3) Real
Panel 1: Earnings Age 23			
RF	0.0412	0.0414	430
	(0.0963)	(0.1032)	(326)
IV	0.9465	0.9496	9,860
	(2.3142)	(2.4703)	(8,961)
F-stat	9.8193	9.8193	9.8193
N	37,117	37,117	37,117
Panel 2: Earnings Age 24			
RF	0.2992***	0.3175***	473
	(0.0969)	(0.1037)	(407)
IV	10.9449*	11.6150*	17,307
	(6.1739)	(6.5751)	(17,151)
F-stat	8.7282	8.7282	8.7282
N	30,930	30,930	30,930
Panel 3: Earnings Age 25			
RF	0.3014**	0.3219**	870
	(0.1246)	(0.1326)	(637)
IV	11.7623	12.5635	33,931
	(8.0265)	(8.5684)	(29,458)
F-stat	4.5333	4.5333	4.5333
N	24,805	24,805	24,805

Notes: This table reports reduced-form (RF) and IV results for HQ CS exposure and course-taking impacts on earnings measures with imputations. For individuals who are missing earnings data, we impute zero as their earnings value. Column (1) shows results for log earnings, which are measured as log of earnings plus one. Column (2) shows results for the inverse hyperbolic sine (IHS) of earnings. Column (3) shows results for untransformed real annual earnings. Panels 1-3 show estimates for ages 23-25. Earnings are measured in 2021 dollars. The specifications are the same as those in Table 4, Columns (3) and (4), respectively. Robust standard errors are clustered at the high school level.
*** p<0.01, ** p<0.05, * p<0.10

Table A15: HQ CS Effects on CS Majors with Different Sample Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Exc Never		No SR		NM CS BA	
	RF	IV	RF	IV	RF	IV
Panel 1: Enroll and CS Major						
Z	0.0041*		0.0061***		0.0070***	
	(0.0022)		(0.0021)		(0.0022)	
HQ CS		0.0821*		0.0911***		0.1275***
		(0.0432)		(0.0352)		(0.0476)
Unexpo Mean	[.0181]	[.0181]	[.0164]	[.0164]	[.0161]	[.0161]
% Change	{22.58%}	{454.41%}	{37.16%}	{555.39%}	{43.26%}	{790.57%}
F-stat		10.7356		14.9199		13.6345
N	38,787	38,787	58,274	58,274	41,658	41,658
N Schools	36	36	59	59	57	57
Panel 2: Persist and CS Major						
Z	0.0057*		0.0073***		0.0086***	
	(0.0029)		(0.0024)		(0.0028)	
HQ CS		0.1128**		0.1086***		0.1550***
		(0.0527)		(0.0382)		(0.0510)
Unexpo Mean	[.0188]	[.0188]	[.0169]	[.0169]	[.0166]	[.0166]
% Change	{30.06%}	{599.16%}	{42.92%}	{642.46%}	{51.54%}	{933.24%}
F-stat		10.6902		14.9474		13.4914
N	39,652	39,652	59,426	59,426	42,625	42,625
N Schools	36	36	59	59	57	57
Panel 3: CS BA in 4 Years						
Z	0.0032***		0.0031***		0.0030***	
	(0.0011)		(0.0010)		(0.0010)	
HQ CS		0.0558***		0.0561***		0.0547***
		(0.0187)		(0.0208)		(0.0185)
Unexpo Mean	[.0041]	[.0041]	[.0041]	[.0041]	[.0043]	[.0043]
% Change	{77.63%}	{1344.1%}	{74.61%}	{1353.79%}	{69.92%}	{1267.92%}
F-stat		14.3126		13.8217		13.5259
N	37,546	37,546	46,931	46,931	43,849	43,849
N Schools	35	35	58	58	57	57

Notes: This table replicates the reduced-form (RF) and IV results shown in Table 4 using different sample restrictions. Columns (1)-(2) show results for a sample that excludes never-treated or closed schools ("Exc Never"). Columns (3)-(4) show results for a sample that includes students who are incoming transfers or expectedly exposed students, who enrolled in high school after their school began offering HQ CS ("No SR" for "No Sample Restrictions"). Columns (5)-(6) show results for a sample that includes only observations from the sample in Table 4, Panel 3 ("NM CS BA" for "Non-Missing CS BA"). The sample in Columns (3)-(4) is the sample shown in the sixth row of Table A4 (after dropping outgoing transfers, but before dropping incoming transfers). The sample in Columns (5)-(6) contains fewer observations because on-time BA receipt can only be observed for a subset of our analytic sample (see Table A3). Robust standard errors are clustered at the high school level.
*** p<0.01, ** p<0.05, * p<0.10

Table A16: HQ CS Effects on Out-of-State and Private College Enrollment

	(1) Enroll Out-of-State	(2) Enroll Private
Panel 1: RF Estimates		
Z	-0.0081	-0.0062
	(0.0052)	(0.0048)
Unexpo Mean	[.0775]	[.065]
% Change	{-10.46%}	{-9.6%}
N	50,481	50,480
N Schools	58	58
Panel 2: IV Estimates		
HQ CS	-0.1310	-0.1007
	(0.0879)	(0.0771)
Unexpo Mean	[.0775]	[.065]
% Change	{-168.93%}	{-154.97%}
F-stat	16.6113	16.6113
N	50,481	50,480
N Schools	58	58

Notes: This table reports results for the impact of HQ CS exposure and course-taking on on-time out-of-state and private college enrollment. The table shows reduced-form (RF) results in Panel 1 and instrumental variables (IV) results in Panel 2. The specifications are the same as those in Table 3, Panels 1 and 2, respectively. For the IV estimates, unexpected exposure to HQ CS instruments for taking HQ CS. The percent change is the coefficient divided by the mean for the unexposed. Robust standard errors are clustered at the high school level.

*** p<0.01, ** p<0.05, * p<0.10