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New Advanced Placement course designed to broaden access promotes participation and demographic diversity in computer science education

Daniela Ganelin[a,1] and Thomas S. Dee[a]

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Advanced Placement (AP) provides college-level courses to over 1 million US secondary students annually. Black, Hispanic, and female students have historically been underrepresented in AP Computer Science (CS). A new, broadly focused course—AP CS Principles—launched nationally in 2016–17 with the goal of increasing student participation and diversity. We examine its effects on AP CS participation. Combining publicly available sources, we assemble a panel dataset of annual AP exam-taking and course offerings from 2006–07 to 2020–21 at Massachusetts high schools. Using synthetic difference-in-differences, we estimate that offering the new course led to 16 additional yearly AP Computer Science exams per school, more than tripling baseline exam counts for the average adopting school. Exam counts among female and Black or Hispanic students more than quadrupled. The new exams were concentrated in AP Computer Science Principles, with no statistically significant reduction in exam counts for the preexisting AP CS course. We also estimate that offering the new course increased schools' probability of having any AP CS exam participation by 29 percentage points, with larger gains for female and Black or Hispanic students. We find some evidence of positive spillover effects on several other AP courses. The results suggest the promise of course design and availability in promoting engagement and diversity in advanced STEM education.

STEM education | computer science | education policy | educational equity | causal inference

Significance

We investigate the launch of a nationwide Advanced Placement (AP) course which was specifically designed to broaden and diversify participation in computer science (CS) education. When schools introduced the new course, they tripled their exam counts in AP Computer Science, quadrupled exam counts among female and Black or Hispanic students, and doubled their probability of having any participation among female students or among Black or Hispanic students. The results suggest the new course supports an ongoing expansion of the STEM education and careers pipeline. More generally, these findings underscore the promise of intentional course design and availability in achieving prominent education policy goals related to participation and diversity at scale.

Computer science (CS) plays an increasingly essential role in the economy, the labor market, and daily life (1, 2). Recent evidence has linked secondary CS coursework to later increases in employment, income, and computer science degree attainment (3). However, many students do not have opportunities to learn CS in school, despite educators' and policymakers' calls for "Computer Science for All" (2, 4–6). In the United States, 40% of public high schools do not offer a foundational CS course, due in part to a lack of qualified teachers, and only 6% of high school students are enrolled in foundational CS in a given year (5, 7). Among students who do take part in CS education, female, Black, and Hispanic learners are underrepresented at the secondary and postsecondary levels, mirroring disparities in the computing workforce (1, 8, 9). These differences arise in part due to structural disparities: Schools serving high numbers of Black or Hispanic students are less likely to offer CS courses (6, 9). Evidence suggests that other contributing factors include stereotypes held by students and adults, a lack of belonging within the CS classroom, and differences in prior academic experiences (9–15).

Historically, these disparities have been visible in the Advanced Placement (AP) CS program. AP is a nationwide program in which over a million students annually take college-level courses at their high schools, with accompanying final exams administered by the College Board (16). In addition to exposure to advanced material and new subjects, highly scoring students often earn college credit or placement at their eventual colleges and universities (17, 18). Between 2009 and 2016, when the Java-programming-focused AP Computer Science A (CS A) was the only AP CS course, fewer than 24% of exam-takers each year were female, and fewer than 16% were Black or Hispanic (19, 20).

With the goal of broadening access and attracting more students from underrepresented groups, the College Board launched a new AP Computer Science Principles course in the 2016–2017 school year (21, 22). The new course features a broader framing of CS, including topics such as societal impact; a new assessment structure involving a creative project as well as a traditional exam; and flexibility for teachers in choosing programming languages (23). Its large-scale national launch (i.e., over 43,000 exams in 2,500 schools the first year) involved NSF funding, curriculum development by several providers, teacher training initiatives, and marketing toward underrepresented groups (20, 22, 24, 25). CS Principles does serve a more diverse group of students than CS A (26, 27). However, it is

Author affiliations: [a]Graduate School of Education, Stanford University, Stanford, CA 94305

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[1]To whom correspondence may be addressed. Email: dganelin@stanford.edu.

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Table 1. Estimated effects of AP CS Principles on AP Computer Science exam counts

Student group	Estimated effect by subject: AP CS Principles	AP CS A	Any AP CS	Baseline treated mean: Any AP CS
All students	16.090***	−0.226	15.860***	7.034
	(1.088)	(0.677)	(1.290)	
Female	4.834***	−0.209	4.621***	1.364
	(0.461)	(0.377)	(0.589)	
Male	11.250***	0.073	11.320***	5.670
	(0.807)	(0.497)	(1.090)	
Black or Hispanic	2.493***	−0.000	2.492***	0.659
	(0.486)	(0.124)	(0.486)	
White	10.140***	−0.251	9.886***	4.409
	(0.806)	(0.595)	(0.921)	

Notes: The dependent variable is the count of exams among students in the given subgroup in the given subject. The last column shows the mean exam count for any AP Computer Science in 2015–16, the last year prior to the introduction of AP CS Principles, among adopting schools. Estimates are constructed based on a balanced panel of 287 schools observed over 15 y (4,305 observations) using a synthetic difference-in-differences estimator (see text for details). SE (in parentheses) are constructed through school-level block bootstrapping (*P < 0.1, **P < 0.05, ***P < 0.01).

unclear whether the introduction of CS Principles in fact expands access and participation at the school level. Conceivably, CS Principles could simply displace schools' AP CS A offerings or even differentially draw underrepresented student groups away from CS A (24, 28).

In this study, we present quasiexperimental evidence on the effects of introducing AP CS Principles at the school level on AP participation in CS and other AP courses. We focus on Massachusetts public schools because, unlike other states, the Massachusetts Department of Elementary and Secondary Education publicly provides uniquely detailed data on AP participation disaggregated by subject, school, and student subgroup. Supplementing with publicly available College Board data, we construct a panel dataset of AP exam participation and course offerings for secondary schools in Massachusetts observed annually from 2006–2007 to 2020–2021 (*SI Appendix*, Table S1). Of the 287 schools in our sample, 176 began offering CS Principles during the last five years of the study period. Using a synthetic difference-in-differences (SDID) approach, we estimate the effects of introducing AP CS Principles on participation in AP Computer Science and other subjects, both overall and for various student subgroups.

We find that offering CS Principles has a large effect in expanding AP Computer Science by bringing in new students and schools to AP CS Principles, with very little impact on participation in the preexisting AP CS A (Table 1 and Figs. 1–3). Specifically, when a school introduces CS Principles, its AP CS exam count jumps by an estimated 16 exams, more than tripling participation for the average adopting school relative to a 2015–16 baseline of 7 exams. The new exams are concentrated in CS Principles, with no statistically significant drop in CS A. The marginal exam-takers brought in by CS Principles are more diverse than previous CS exam-takers, at 29% female (compared to 19% at baseline) and 16% Black or Hispanic (compared to 9% at baseline). Relative to baseline, exam counts more than quadruple for female students as well as Black and Hispanic students. Additionally, we find evidence that introducing CS Principles has a positive spillover effect on participation in several other AP subjects (*SI Appendix*, Table S5).

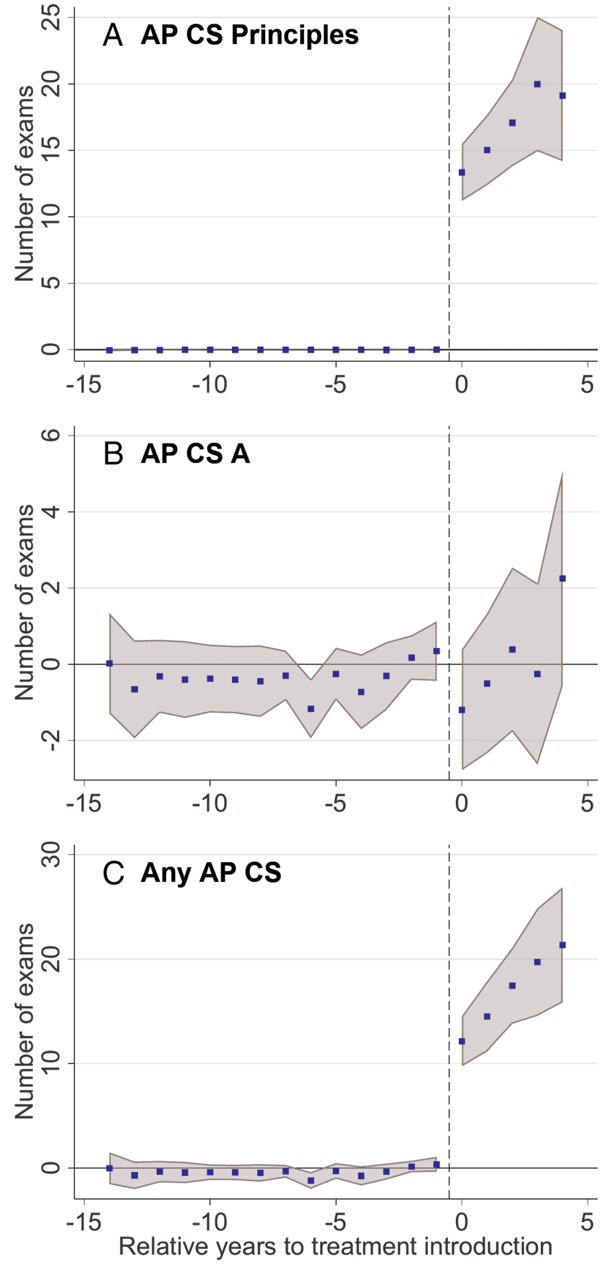

Fig. 1. Synthetic difference-in-differences event studies for the effects of AP CS Principles on AP Computer Science exam counts, by subject. Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam counts in (*A*) AP Computer Science Principles, (*B*) AP Computer Science A, and (*C*) any AP Computer Science. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% CI. Estimates are based on a balanced panel of 287 schools observed over 15 y. See Table 1 for details.

In addition to increasing AP CS exam counts, CS Principles has a large effect in bringing in AP CS participation at schools that previously had none (Table 2 and *SI Appendix*, Figs. S1 and S2). After introducing CS Principles, a school's probability of having any AP CS exam-takers increases by 29 percentage points. Furthermore, its probability of having any female exam-takers grows by 38 percentage points, and its probability of having any Black or Hispanic exam-takers increases by 36 percentage points. These effects are particularly large compared to the low baseline CS participation rates among these groups: In 2015–16, only 36% of later-treated schools had any female exam-takers and 22% of schools had any Black or Hispanic exam-takers. We observe a

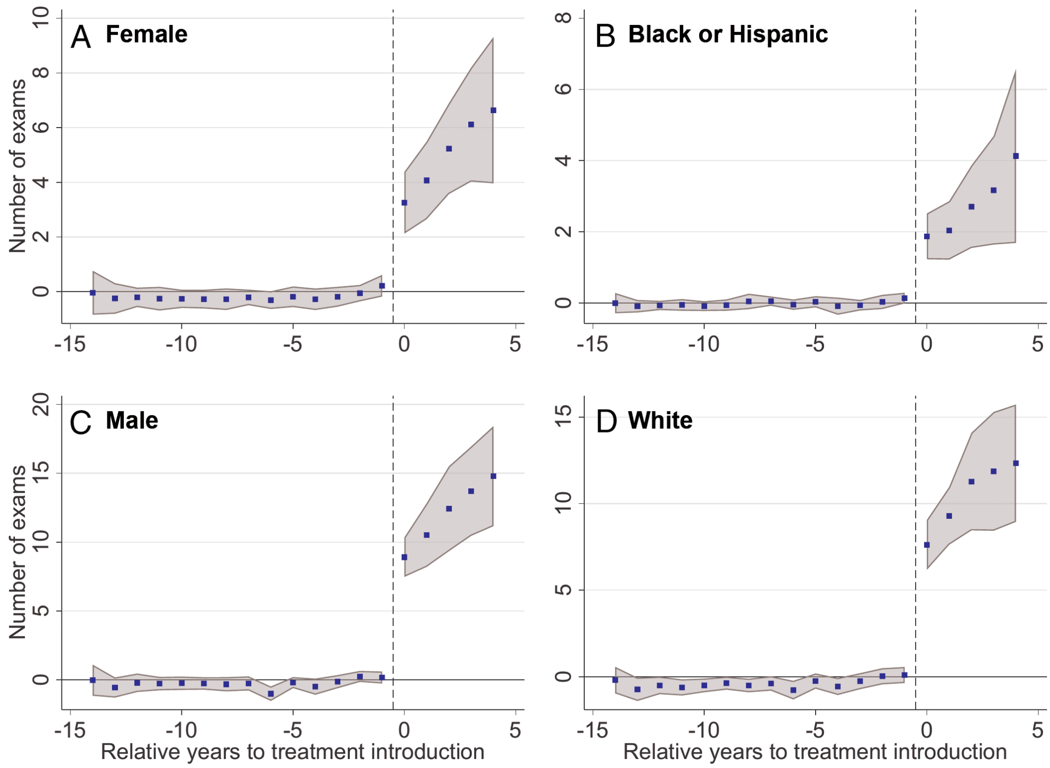

Fig. 2. Synthetic difference-in-differences event studies for the effects of AP CS Principles on AP Computer Science exam counts, by subgroup. Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam counts in combined AP Computer Science, for each student subgroup: (*A*) female students, (*B*) Black or Hispanic students, (*C*) male students, and (*D*) White students. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% CI. Estimates are based on a balanced panel of 287 schools observed over 15 y. See Table 1 for details.

weakly significant substitution effect on having any CS A exam participation. Event studies show that this effect is concentrated only in the first year, in which there is a 13 percentage-point decrease in the probability of having any CS A participation.

We assess the reliability of our quasiexperimental design in several ways. We find that event-study evidence is consistent with the identifying assumptions of the SDID design. Our main results are also similar across changes in estimation method, treatment definition, outcome measurement, and inclusion of covariates (*SI Appendix*, Tables S2–S4 and Figs. S3 and S4).

Our study suggests that AP CS Principles is an encouraging proof point on how course design and availability can improve participation and diversity in STEM education. The design intent of CS Principles was to make advanced STEM content appealing and accessible across student groups. Accompanying teacher training and curriculum development initiatives also made it feasible for thousands of schools to introduce the course. Our results show that, when schools introduce CS Principles, they do in fact increase participation in CS education across a broad population of secondary students, especially for historically underserved groups.

Materials and Methods

Data. The main analytical sample in this study consists of a balanced school–year panel of n = 287 unique traditional public schools in Massachusetts consistently serving secondary students, observed annually from 2006–07 to 2020–21 (for a total of 4,305 school-by-year observations). This sample draws together matched data on AP exam participation from the Massachusetts Department of Elementary and Secondary Education (DESE) and on officially audited AP course offerings from the College Board's AP Course Ledger. All data are compiled from publicly available sources; Supporting Information provides details on data construction, summary statistics (*SI Appendix*, Table S1), missingness checks, and analyses.

Outcome Variables. We estimate the effects of introducing AP CS Principles on school-by-year measures of AP participation. Our focal outcome variables measure participation in AP Computer Science. We consider participation in AP CS Principles (to estimate take-up of the new course), in AP CS A (to estimate any substitution effect on the preexisting course), and in any AP CS, combining both courses. We exclude the former AP Computer Science AB, which was discontinued after 2008–09. We measure participation based on AP exam-taking, which is instrumentally relevant to college credit and placement. Our data generally do not include alternative measures such as course enrollment or exam scores.

Our primary results measure the intensive margin of participation: the count of exams. A secondary set of results measures the extensive margin of participation, as an indicator of access: having at least 1 exam-taker from a given subgroup in a given school and year. We estimate effects for all students and also for key underrepresented subgroups: female students, along with male students for comparison, and Black and Hispanic/Latino students, along with White students for comparison. Finally, we estimate the effects on participation in other AP subjects.

Treatment. We are interested in the effects of a school decision to adopt AP CS Principles, which we conceptualize as a teacher or virtual provider leading a class section for a group of students. Our focal treatment definition identifies whether a school ever offered the new course, between its introduction in the 2016–17 school year and the end of our study period in 2020–2021. This definition effectively conceptualizes the limited number of treatment reversals (i.e., subsequently ceasing to offer the course) as possibly following from an initial adoption. As a complement to our evidence based on this ever-treated definition, we also find similar results using estimators that accommodate treatment reversals (*SI Appendix*, Table S3).

In our dataset, 172 of 287 schools report at least one officially audited AP CS Principles course offering. However, this may undercount actual offerings: A school can offer a class designed around the AP without filing paperwork to have the offering audited by the College Board (29). Accordingly, we aim to construct a measure of treatment that encompasses both audited and unaudited course offerings, while excluding individual students who self-register for an exam in a course not offered at their school.

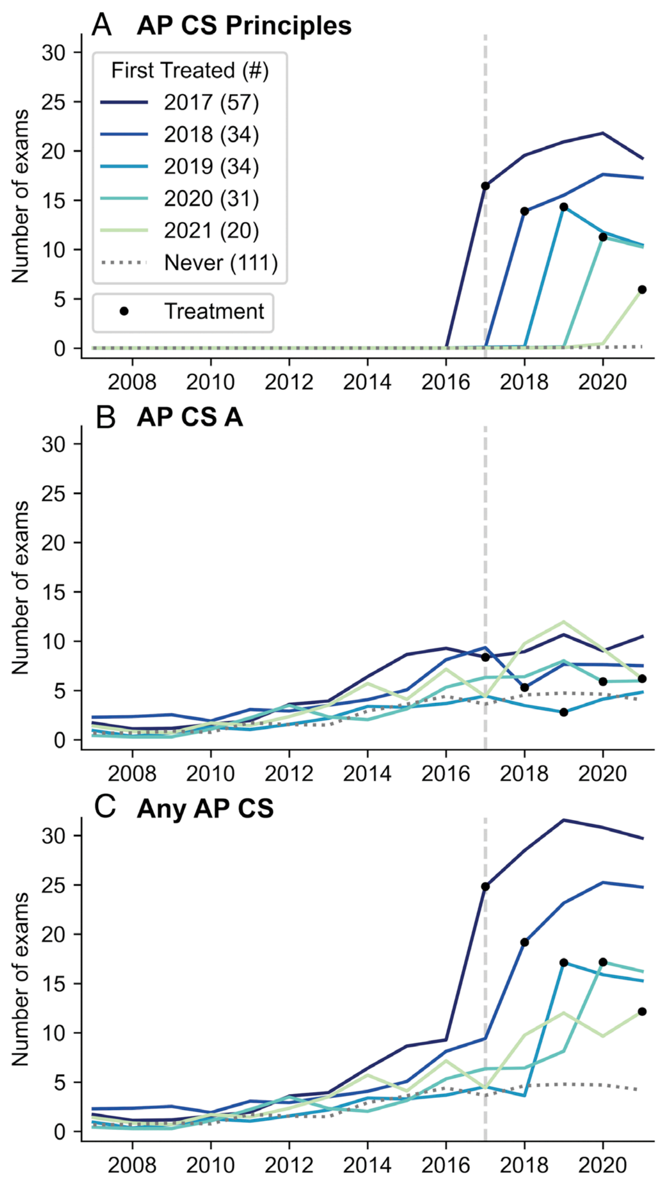

Fig. 3. Mean AP Computer Science exam counts, by treatment cohort and year. Mean number of exams in each treatment cohort in (*A*) AP Computer Science Principles, (*B*) AP Computer Science A, and (*C*) any AP Computer Science. Each colored line represents a group of schools which introduced CS Principles in the same school year. A black dot indicates each cohort's treatment year, indexed by spring exam date (e.g., 2021 represents the 2020–2021 academic year). The legend shows the number of schools in each cohort, for a total of 287 schools. The dotted gray line represents those schools that were never treated.

In our preferred treatment definition, we consider a school to be treated if it has ever met at least one of these conditions: 1) the school has an officially audited AP CS Principles offering in the College Board AP Ledger, and/or 2) the school reports at least 5 exams in AP CS Principles in the same year. By this definition, 176 of 287 schools are treated at some point. In *SI Appendix*, Table S4, we show that our main results are similar under several alternative treatment definitions.

Estimation. Our quasiexperimental research design fundamentally relies on comparing how key outcomes change in schools that take up AP CS Principles relative to the contemporaneous outcome changes in schools that do not. The key identifying assumption of this general difference-in-differences (DID) approach is that the changes observed within untreated, comparison schools over time constitute a valid counterfactual for what would have happened in treatment schools in the absence of treatment (i.e., the "parallel-trends" assumption). DID estimates that accommodate the variation in treatment timing and possible treatment-effect heterogeneity (CSDID) suggest that this key assumption is not clearly met in our data (*SI Appendix*, Fig. S3) (30). Given this implied internal validity threat, our primary analysis focuses instead on synthetic difference-in-differences (SDID) estimates, which address the potential influence of time-varying, school-specific confounds (31).

Table 2. Estimated effects of AP CS Principles on having any AP Computer Science exam-taking

Student group	Estimated effect by subject: AP CS Principles	AP CS A	Any AP CS	Baseline treated mean: Any AP CS
All students	0.782***	−0.049*	0.289***	0.597
	(0.027)	(0.029)	(0.028)	
Female	0.699***	−0.036	0.382***	0.364
	(0.027)	(0.034)	(0.036)	
Male	0.773***	−0.022	0.316***	0.580
	(0.023)	(0.036)	(0.034)	
Black or Hispanic	0.503***	0.006	0.358***	0.216
	(0.028)	(0.030)	(0.032)	
White	0.771***	−0.057	0.297***	0.568
	(0.032)	(0.040)	(0.037)	

Notes: The dependent variable is 1 if a school-year reports at least one AP exam in the given subject by a student in the given subgroup. The last column shows the mean value for any AP Computer Science in 2015–16, the last year prior to the introduction of AP CS Principles, among adopting schools. Estimates are constructed based on a balanced panel of 287 schools observed over 15 y (4,305 observations) using a synthetic difference-in-differences estimator (see text for details). SE (in parentheses) are constructed through school-level block bootstrapping (*$P < 0.1$, **$P < 0.05$, ***$P < 0.01$).

The SDID estimator combines core features of the traditional DID and synthetic-control designs in a least-squares framework. Specifically, like DID, it accommodates additive school-specific fixed effects. Like synthetic control, it reinforces the key parallel-trends assumption by using school-specific weights which optimally align pretreatment trends across the treated and comparison schools. The SDID approach also introduces time-specific weights that place more emphasis on pretreatment periods that resemble the posttreatment period. The simultaneous application of both school and time-specific weights implies a statistical "double robustness" property that enhances the reliability of the resulting estimates (31–33).

We examine the robustness of our results through evidence on their alignment with key assumptions and sensitivity to specification choices. One check is to examine event-study results based on the SDID design (34). This approach provides evidence on whether treated and comparison units appear to have similar trends prior to the onset of treatment. We also present results that explore the sensitivity of our findings to additional covariates and alternative estimation approaches. These alternative designs include CSDID and interactive fixed-effect counterfactual estimators (IFEct). IFEct estimates a treatment effect by imputing counterfactual untreated outcomes for treated cases using a factor-augmented model (33). Under this approach, school–year observations in the treated state are initially considered as missing. The existing untreated observations are then used to train a model that explicitly estimates the counterfactual untreated outcome for each treated observation. Specifically, the model estimates school and year fixed effects and also latent factors for school–year interactions, allowing the imputed counterfactual estimates to flexibly accommodate time-varying, school-specific confounds. The IFEct estimator then calculates a treatment effect by subtracting untreated counterfactuals from the corresponding observed treated outcomes. The IFEct approach also allows us to explore the sensitivity of our main findings both to using an unbalanced school–year panel and to using a definition of treatment that accommodates treatment switching (i.e., adopting and later dropping AP CS Principles). We report the key results of these robustness checks in *SI Appendix*.

Results

Table 1 shows the estimated effects of introducing AP CS Principles on AP Computer Science exam counts, by student group and specific subject. Introducing AP CS Principles is estimated to lead to an extra 16 annual AP CS exams. The new exams are concentrated in CS Principles: estimated effects on CS Principles exam counts are about equal to those on AP CS overall. This overall growth represents a 230% increase compared to the mean of 7 exams in the adopting schools in the 2015–16 school year, the year before CS Principles was first introduced. Among the 16 new AP CS exams, 5 are taken by female students (i.e., a 340% increase relative to the baseline of 1.3) and 11 by male students (i.e., a 200% increase relative to the baseline of 6). The number of AP CS exams taken by Black or Hispanic students grew by 2.5, representing a 380% increase from the baseline of 0.7, while the number taken by White students grew by 10, a 220% increase from the baseline of 4. At 29% female and 16% Black or Hispanic, the demographic breakdown of the new participation does not perfectly mirror that of the student body, but it is substantially more representative than baseline AP CS participation, which was 19% female and 9% Black or Hispanic. The estimated effect of offering CS Principles on the number of CS A exams is small and statistically insignificant, both overall and for each subgroup.

Figs. 1 and 2 present corresponding event-study estimates for these outcome measures. These SDID-based estimates illustrate how AP exam counts in treated schools change relative to comparison schools in each school year prior to and following the adoption of CS Principles. The results consistently indicate that, prior to treatment, the number of AP CS exams trends similarly across treatment and weighted comparison schools, a pattern aligned with the key identifying assumption of this research design. These event-study estimates also provide a visual illustration of the results in Table 1 and evidence on the dynamic character of these estimated effects. For example, Fig. 1 shows that initial increases in CS Principles and combined Computer Science exam counts are followed by further increases over time, while there is not a clear impact on CS A exam counts.

Fig. 2 shows similar event-study patterns by demographic subgroup for combined AP CS participation. Specifically, these results indicate a sharp jump in exam-taking for each group after the introduction of AP CS Principles, followed by further growth over time. This is consistent with the hypothesis that one or more factors supporting student engagement (e.g., staff capacity to offer the course, its quality, and awareness among students) grows following the initial course adoption. One notable caveat to this evidence of growing impact is that the longer-run estimates are only defined for early-adopting schools, which may have relevant forms of treatment heterogeneity. For instance, Fig. 3 shows that early adopters experience greater first-year changes in mean Computer Science and CS Principles exam counts.

Table 2 examines the estimated effects of CS Principles on the extensive margin (i.e., whether a school has *any* AP CS exam participation). *SI Appendix*, Figs. S1 and S2 show the corresponding SDID-based event studies and cohort means. The estimated effect on the probability of having any CS Principles exam-taking is 87 percentage points in the initial treatment year, decreasing to 74 percentage points in the fifth year. This indicates that most treating schools maintain CS Principles participation over time. The results in Table 2 also suggest that adopting CS Principles leads to a small and weakly significant reduction in having any AP CS A exam-taking (i.e., 5 percentage points, $P < 0.10$). However, the corresponding event-study evidence (*SI Appendix*, Fig. S1) shows that there is a statistically significant negative effect on CS A only in the initial treatment year. Event studies using alternative methods show similar patterns (*SI Appendix*, Figs. S3 and S4). These results suggest that some schools offer CS Principles as a substitute for CS A in the first year but later offer both courses.

The results in Table 2 also indicate that introducing AP CS Principles increases a school's probability of having any AP CS exam-taking by 29 percentage points. This estimated impact is a 50% increase relative to the 2015–16 treated school mean of 60%. These estimated extensive-margin effects are substantially larger for subgroups that are historically underrepresented in computing. Specifically, these estimates indicate that offering CS Principles doubles the probability that a school has at least one female student take an AP CS exam (i.e., a 38 percentage-point increase relative to a 36% baseline). Similarly, the probability of having at least one Black or Hispanic student take an AP CS exam more than doubles (i.e., a 36 percentage-point increase relative to a 22% baseline, implying 160% growth).

Supporting Information provides additional results tables and figures. *SI Appendix*, Figs. S5–S9 show additional SDID event-study figures featuring results for each AP CS measure defined for each subgroup.

SI Appendix, Table S2 shows that our main results are similar when the outcome measure is an enrollment-based rate rather than a count-based measure. *SI Appendix*, Tables S3 and S4 report that the main results are also broadly similar under alternative estimation methods, covariate inclusion, and treatment definitions. Additionally, *SI Appendix*, Table S4 shows the robustness of key findings when excluding observations for years after the onset of the COVID-19 pandemic.

SI Appendix, Table S5 reports the estimated effects of introducing AP CS Principles on exams in non-CS AP subjects. These potential spillover effects are theoretically ambiguous. CS Principles could crowd out schools' capacity to offer other AP courses or replace other AP courses in students' schedules (35). However, CS Principles could also increase student awareness and enthusiasm for these courses and encourage future AP course-taking (36). We find evidence of positive spillover effects on overall AP participation (*SI Appendix*, Table S5). Specifically, adopting CS Principles increases the overall annual AP exam count by an estimated 33 exams, which more than doubles the focal estimate for any AP CS of 16. This represents a 9% increase relative to a baseline mean of 358 exams. These gains appear to be concentrated in AP science courses, particularly Environmental Science, and in AP English courses, particularly English Language & Composition. Event-study evidence is consistent with the internal validity of these results (*SI Appendix*, Fig. S10). This specific heterogeneity is aligned with secondary-student participation patterns for Environmental Science and English Language: like CS Principles, these courses often serve as introductory courses in their respective domains. Specifically, program-wide grade-level data show that all three courses have a greater share of younger students than the other AP science, English, and CS courses, respectively (16).

Discussion

As part of building the STEM pipeline, policymakers and educators frequently underscore a national imperative to increase the participation and diversity of students learning rigorous computer science. These concerns motivated the recent design and large-scale adoption of the new Advanced Placement CS Principles course for secondary students. In this study, we provide credibly causal evidence on the impact of offering this new course. By and large, the evidence presented here indicates that CS Principles succeeded in its proximate goals. In our 15-y study frame, Massachusetts schools that offered AP CS Principles experienced a large increase in overall AP Computer Science participation, tripling their exam counts compared to a 2016 baseline. These gains were particularly large for female and Black or Hispanic students, for whom exam counts more than quadrupled. Additionally, schools more than doubled their probability of having at least one female student or at least one Black or Hispanic student participate in AP Computer Science. Our results show that CS Principles expanded participation both by increasing exam counts across groups and by expanding access (i.e., schools beginning to offer AP Computer Science for the first time). In parallel, national trend data also indicate that AP Computer Science participation has boomed since the introduction of the new course. Between the introduction of CS Principles in 2016 and 2022, the number of AP CS exams has more than tripled, growing by 147,000 (20). Generalizing this study's findings to the 6,050 US schools that offered CS Principles in 2022 implies that the availability of this new course explains 65 percent of the sharp growth in exams. Female, Black, and Hispanic participation have also continued to grow nationally.

At least three caveats to these findings merit emphasis. First, our quantitative data do not allow us to distinguish the specific mediators that shaped the implementation and estimated effects of CS Principles. The course's design, adoption, and large-scale implementation likely required multiple forms of coherent and effective action at the national, state, and local levels. Whether initiatives like this can be successfully replicated is necessarily uncertain.

A second and related caveat is that the data in this study pertain to Massachusetts, one of the states with the highest AP participation rates. For example, 59% of public high schools nationally offered a STEM AP course in 2020–21, while 88% did in Massachusetts (16). This unusually high AP participation may raise questions about the generalizability of our findings. In particular, the challenges of offering and taking up CS Principles may be greater in other states where barriers to AP participation are more salient. Alternatively, because other settings begin with lower AP participation, the marginal impact of offering this course could instead be correspondingly greater. We note that the overall growth in AP CS participation has been similar in Massachusetts and nationally (20).

A third and important caveat is that, while our study provides evidence on the changed participation of high-school students in advanced CS courses, it does not provide direct evidence on other conventional learner outcomes. This would be important if the broader focus of AP CS Principles makes it less beneficial for students, compared to CS A or other courses that students would otherwise take (37). However, four factors are inconsistent with this concern. First, the CS Principles course does appear to be broadly recognized as valid for college credit. For example, all University of Massachusetts campuses grant credit or placement for either AP Computer Science exam (38). Similarly, the University of California, Berkeley grants credit for CS Principles but indeed not CS A (39). Second, recent descriptive findings show that CS Principles exam-takers—especially Hispanic and female ones—disproportionately enroll in CS and other STEM majors in college (21). Other research shows that even low-scoring AP participants have better college outcomes than matched academically similar peers who do not participate in AP (40). Third, we present quasiexperimental results that offering CS Principles enhances the take-up of other AP courses, particularly in science and English, providing direct evidence consistent with its academic benefit. Fourth, our evidence shows that negative effects on participation in AP CS A, a potential substitute for CS Principles, are small, time-limited, and statistically insignificant overall.

More broadly, these results provide a potentially compelling template for actionable at-scale strategies to support student excellence and representation in any subject. Intentional course design that integrates advanced academic content with elements that have broad appeal to students, alongside steps that make the course practically available to schools and students, may form a potent and perhaps underutilized strategy for engaging learners and supporting their potential.

Data, Materials, and Software Availability. .csv file, code, and documentation data have been deposited in Stanford Digital Repository (https://doi.org/10.25740/fg838wx4213). Previously published data were used for this work (41).

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Supporting Information for
New Advanced Placement Course Designed to Broaden Access Promotes Participation and Demographic Diversity in Computer Science Education

Daniela Ganelin, Thomas Dee

*Daniela Ganelin
Email: dganelin@stanford.edu

This PDF file includes:

Supporting Information Text
Figures S1 to S10
Tables S1 to S5
SI References

Supporting Information Text

Methods and Data. Below, we provide additional details on our data and methods.

Methods

Our data and code are available in the Stanford Digital Repository (1). We use Python 3.8 for compiling data and figures, and Stata 15.1 for causal inference analyses using the csdid, sdid, sdid_event, and fect packages. The respective methods are described by the authors (2–7). We do not use covariates when generating the figures shown in the paper. Table S3 reports the estimated effects of CS Principles, conditional on covariates, for each method.

The results presented in the main body of the paper are based on synthetic difference-in-difference estimators (SDID). We construct standard errors through block bootstrapping clustered at the school level and use default options for parameters such as regularization.

We present results using other methods in Table S3. For CSDID (Callaway and Sant'Anna's difference-in-difference estimators), we use asymptotic normal standard errors. The comparison group includes both never-treated and not-yet-treated schools. We note that our CSDID results generally fail the pretrend test, suggesting a parallel-trends violation (details in Table S3).

We also present results using interactive fixed-effects counterfactual estimators (IFEct), which require a researcher-specified parameter, r (6). This parameter is the number of singular values in the underlying model's matrix decomposition – conceptually, the number of latent factors allowed to vary for each unit and time period. We select this parameter using the placebo test, which estimates a "placebo effect" under the temporary assumption that the treatment started earlier than it actually did. A failure of identifying assumptions is likely to manifest as a nonzero placebo effect. We present the results that pass the placebo test with $p > 0.1$: using $r = 2$ for exam counts, and both $r = 1$ and $r = 2$ for any exam-taking. In the latter case, $r = 1$ always passes the placebo test with a higher p-value than $r = 2$. We do not present fixed-effects counterfactual estimator results using matrix completion or two-way fixed effects, as no such configurations pass the placebo test. We construct standard errors through nonparametric block bootstrapping clustered at the school level.

Data

The main analytical sample in this study consists of a balanced panel of n = 287 unique traditional public schools in Massachusetts consistently serving grade 9-12 students, observed annually during the 15 school years from 2006-07 to 2020-21 (for a total of 4,305 school-by-year observations). This sample draws together matched, publicly available data web-scraped from the Massachusetts Department of Elementary and Secondary Education (DESE) and the College Board websites (8–12).

First, we identified a universe of schools using data from the DESE Statewide Reports. From these reports, we obtained data on school enrollment by grade and student subgroup (e.g. by gender, race, and economic status) and AP exam participation by exam and student subgroup. Second, we used additional DESE data sources (School and District Profiles, Charter School Directory, Enrollment Data, and Alternative Schools list[1]) to obtain schools' profile information, including status as charter or alternative schools. Third, we used the College Board's AP Audit Course Ledger to identify authorized AP course offerings at each school from the 2007-08 to 2020-21 school years.

We began with the universe of 2,178 K-12 Massachusetts schools with unique school codes that reported enrollment during the study period. We excluded 1,698 schools that never reported a grade 9-12 enrollment, then 57 charter schools, then 3 virtual schools, then 40 schools from DESE's 2015-16 Alternative Schools list, as well as 13 additional schools that were not on the list but

[1] The Alternative Schools list was obtained by e-mail request from DESE.

similarly targeted at-risk or special-needs youth according to the schools' websites, local newspapers, or school staff.

At this stage, our sample included 367 units with distinct school codes. In some cases, we were able to identify that distinct school codes in fact represented related entities: for example, sub-units of the same school or schools that underwent a merger or division. In order to track schools for longer periods of time and create a more balanced panel, we constructed 9 "composite schools" that combine enrollment and AP offerings for multiple school codes. These 9 composite schools replaced 25 of the original school codes, for a new sample size of 351 schools. The next subsection contains details on each composite school.

Next, we filtered for schools that consistently served grade 9-12 students prior to the advent of AP CS Principles in 2016-17, which we operationalized as having grade 9-12 enrollment in each of the three school years between 2013-14 and 2015-16. This excluded 57 schools: 17 that first had enrollment after 2013-14, 33 that no longer had enrollment by 2015-2016, and 7 others, all called Middle Schools, that had only lower-grade enrollment in some of these three years.

This left a study sample of 294 traditional public schools which consistently served secondary students prior to the introduction of AP CS Principles. This was the unbalanced panel, with 4,381 school-year observations, used for the IFEct and CSDID results in Table S3. We matched these schools to College Board data using school names and addresses.

To construct our final balanced panel, we used the 287 schools that reported enrollment during each year of the study period. From the sample of 294 schools, we eliminated 4 schools that first reported enrollment during the study period, and then 3 schools that closed during the study period – all due to old or unsafe buildings, according to news articles (13, 14). This formed the final balanced panel of 287 schools and 4,305 observations that we used for our main SDID results. Of these, 286 schools reported at least one AP exam according to DESE data on AP exam participation, and 284 schools officially offered at least one AP course according to College Board data.

As a check for differential attrition, we defined an indicator in the unbalanced panel of 294 schools for school-year observations where enrollment was present. This created a rectangularized balanced panel of 4,410 school-year observations. Regressing enrollment status on ever-treated status with by-school and by-year fixed effects, we found no statistically significant effect of treatment status on missingness. Four of the seven eliminated schools were treated.

Constructing Composite Schools

In constructing composite schools, our goal is to track a comparable cross-section of students over time. For example, the composite Southbridge school captures grades 6-12 over the entire study period; the composite Somerset-Berkley school captures high-school students from the towns of Somerset and Berkley over time. When multiple school codes are joined to form a composite school within a single school year, we add enrollment and exam counts, and we consider the school to have an audited AP offering for a particular course if any of the school codes have an audited offering. In order to capture consistent grade spans, we temporarily reintroduce and incorporate three middle schools that we had previously excluded. In the explanations below, all years refer to the spring of the academic year: e.g., 2019 refers to the 2018-19 school year.

Of the nine composite schools described below, seven (all but Southbridge and Carver) were treated according to our preferred treatment definition. Treatment always took place in the school's final reporting configuration (e.g., not prior to the mergers or divisions described below.)

In Southbridge, (a) Southbridge Junior High and (b) Southbridge High School shared an address, then merged to become (c) Southbridge Middle/High School in 2013 when they both moved to a new building (15). From 2017 onwards, the combined school's records were reported separately for (d) Southbridge Middle School and (e) Southbridge High School, both at the new address. We construct a composite Southbridge school that combines all five school codes.

In Medford, (a) Medford High and the smaller (b) Medford Vocational Technical High reported enrollment separately at the same address through 2017; afterwards, only Medford High reported enrollment, in a larger quantity that corresponds to the previous sum of both schools. As the school's website continues to advertise a Voc-Tech program, we infer that the two units now report enrollment together, and we construct a composite Medford school that combines both school codes (16).

In Holyoke, (a) Holyoke High School and (b) William J. Dean Vocational Technical High School underwent an administrative merger under a common principal in 2019, although they retained separate physical locations (17). After the merger, enrollment is reported only for Holyoke High. We construct a composite Holyoke school that combines both school codes.

In Barre, at the same address as the long-established (a) Quabbin Regional High School, (b) IB School of Quabbin reported a small enrollment from 2014 to 2018. As Quabbin's website continues to advertise an IB program, we infer that the two units now report enrollment together, and we construct a composite Quabbin school that combines both school codes (18).

In Winchendon, enrollment is reported at (a) Murdock Middle/High School through 2013, and then separately at (b) Murdock Middle and (c) Murdock High at the same address. We construct a composite Murdock school that combines all three school codes.

In Somerset, (a) Somerset High School, which had also served the students of the neighboring town of Berkley under a tuition agreement, last reported enrollment in 2011. Afterwards, enrollment was reported at the new (b) Somerset Berkley Regional High School building on the same property, built together by the two towns under a regionalization agreement (19, 20). We construct a composite Somerset-Berkley school that combines both school codes.

In Carver, enrollment was reported separately at (a) Carver Middle School and (b) Carver High School until 2009, and then together at (c) Carver Middle/High School, all located in the same complex. In 2009, AP data are reported at the combined school even though enrollment is reported for the separate schools. We construct a composite Carver school that combines all three school codes.

In Leominster, enrollment is reported separately at (a) Leominster High School and (b) Center for Technical Education Innovation, located in a wing of the same building, except for 2016 - where enrollment is reported only at Leominster High, in a quantity similar to the sum of both schools in other years, although AP data are reported for the Center. We construct a composite Leominster school that combines both school codes.

In Lawrence, a new high school comprising six themed sub-schools at the same address opened in 2008, and until 2016 enrollment was reported separately at the six schools: (a) Performing & Fine Arts, (b) Business Management & Finance, (c) Health & Human Services, (d) Humanities & Leadership Development, (e) Math, Science & Technology, and (f) International (21). Afterward, combined enrollment was reported at (g) Lawrence High School at the same address (with an overlap of 9th grade enrollment spread across all seven schools in 2016). We construct a composite Lawrence school that combines all seven school codes. Because the school opened after the 2006-07 school year, this school was eliminated in the 287-school balanced panel.

Other Data Notes
Massachusetts' measurement of economic disadvantage changed in 2015. When using economic disadvantage as a covariate, we use the within-year ranking as our measure (22). A single school, Massachusetts Academy of Science and Math, is missing an economic disadvantage

measurement for 2007, the first year of the sample. We impute 0, which is the measurement for the next 7 years.

When using school 9-12 enrollment as a covariate (in Table S3), we use the logarithm of the enrollment summarized in Table S1.

Enrollment data are reported as a schoolwide percentage for some categories (including race) and a raw count for others (including economic disadvantage measures). In the former case, we calculate an approximate count using total enrollment to allow addition in the case of composite schools. After creating composite schools, we convert counts back to percentages, using the new total enrollment, for use as covariates. For Table S2 only, we use state-provided grade-level race and gender data, although we note that gender counts do not sum exactly to the total in 8% of school-by-year observations.

The last two years of our study period overlap with the COVID-19 pandemic. Due to the pandemic, AP administration was modified in spring 2020: the CS A exam was administered online at home in a shortened format, while the CS Principles exam was cancelled and scores assigned based solely on project components (23). In spring 2021, at the end of a school year when many schools operated in a hybrid format, both exams were available in both the traditional paper format and an online at-home format (24, **25**). Table S4 shows that our main results are similar when excluding pandemic-year observations (i.e., spring 2020 and later). We observe that 2020-21, the last school year in our study frame, differed in several ways from previous treatment years: the 2021 treatment cohort included the fewest schools, experienced the smallest first-year changes in mean Computer Science and CS Principles participation (Figures 3 and S2), and had the smallest estimated first-year treatment effects in these subjects. Additionally, the 2020-21 school year featured the most treatment reversals (i.e., 28 previously treated schools did not offer CS Principles this year, compared to at most 9 in any previous year).

When referring to “any AP CS” participation, we include AP CS Principles and AP CS A. We exclude the former AP Computer Science AB, which was discontinued after spring 2009 (26). In the course’s last administration, Massachusetts had fewer than 100 total CS AB exams.

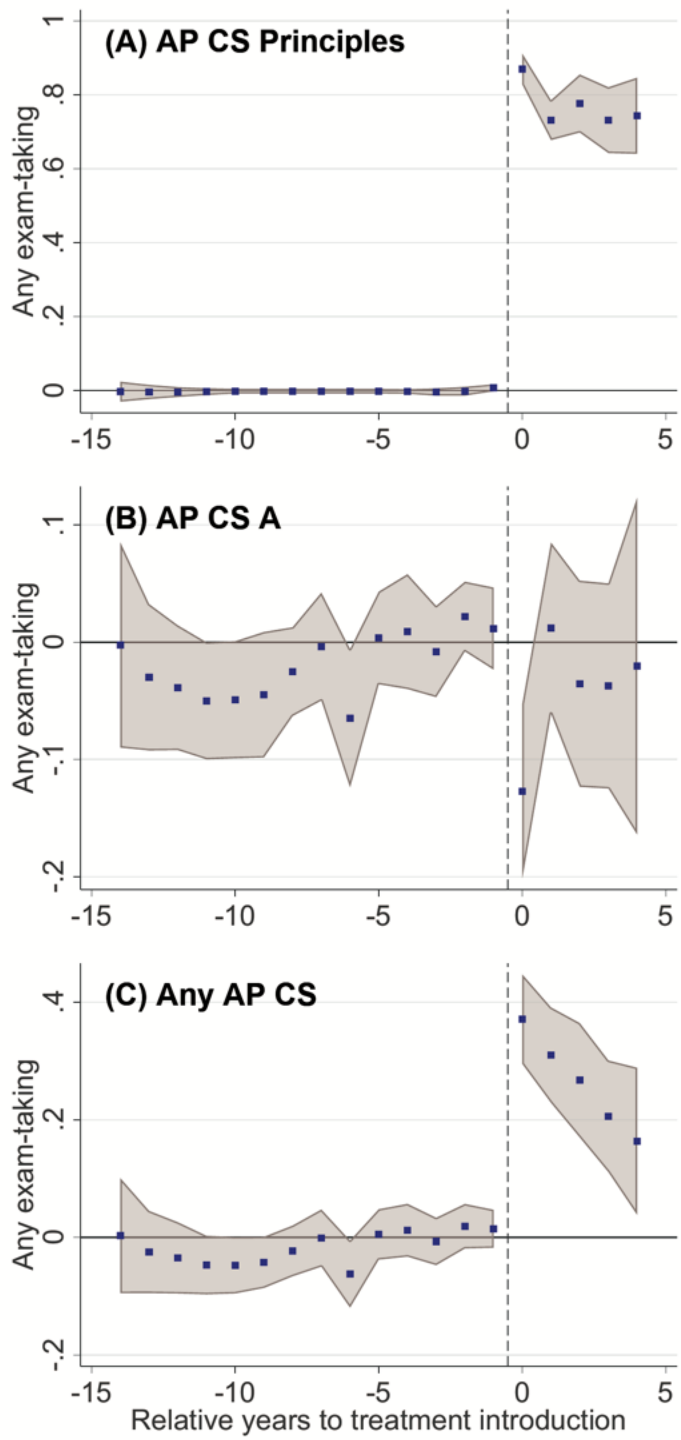

Figure S1. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on Having Any AP Computer Science Exam Participation, by Subject.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam-taking in (A) AP Computer Science Principles, (B) AP Computer Science A, and (C) any AP Computer Science. The dependent variable is 1 if a school reports at least one AP exam in the given subject in a given year. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. See Table 2 for details.

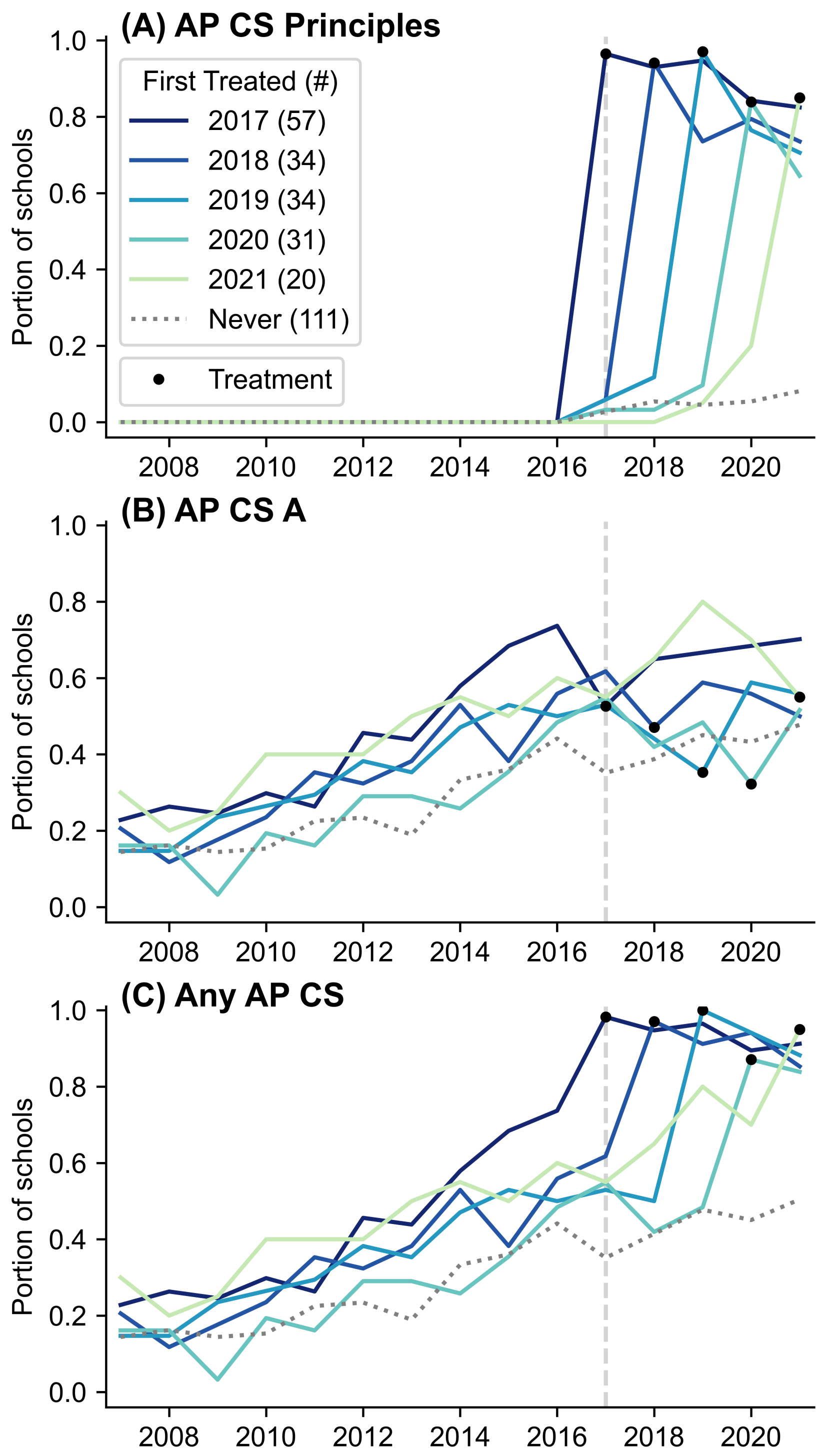

Figure S2. Portion of Schools with Any AP Computer Science Exam Participation, by Treatment Cohort and Year.

Portion of schools in each treatment cohort with any exam-taking in (A) AP Computer Science Principles, (B) AP Computer Science A, and (C) any AP Computer Science. Each colored line represents a group of schools which introduced CS Principles in the same school year. A black dot indicates each cohort's treatment year, indexed by spring exam date (e.g., 2021 represents the 2020-2021 academic year). The legend shows the number of schools in each cohort, for a total of 287 schools. The dotted gray line represents those schools that were never treated.

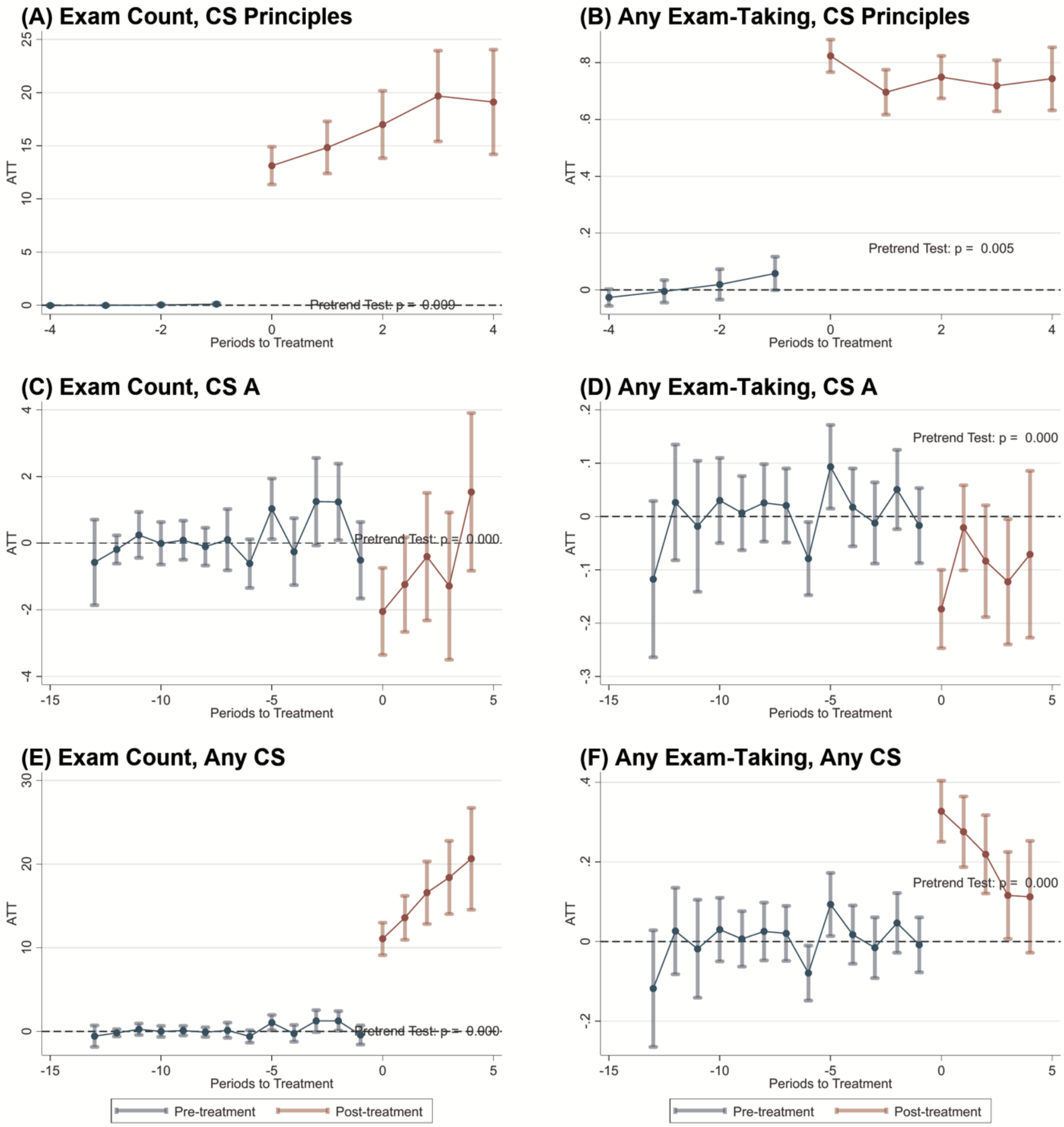

Figure S3. CSDID Event Studies for the Effects of AP CS Principles on AP Computer Science Participation.

Estimates are based on the estimator described in Callaway and Sant'Anna's "Difference-in-Differences with multiple time periods." Point estimates show estimated pre-treatment effects and post-treatment effects, by year, on outcomes in AP Computer Science Principles (A and B), AP Computer Science A (C and D), and any AP Computer Science (E and F). On the left, the dependent variable is the number of exams; on the right, the dependent variable is 1 if a school reports at least one AP exam in the given subject in the given year. In all panels, a pretend test rejects the joint hypothesis that the pre-treatment effects are all equal to 0, indicating a failure of the parallel-trends assumption. The error bars show 95% confidence intervals. Estimates are constructed based on an unbalanced panel of 294 schools observed over 15 years (4,381 observations). See Table S3 and the SI text for details.

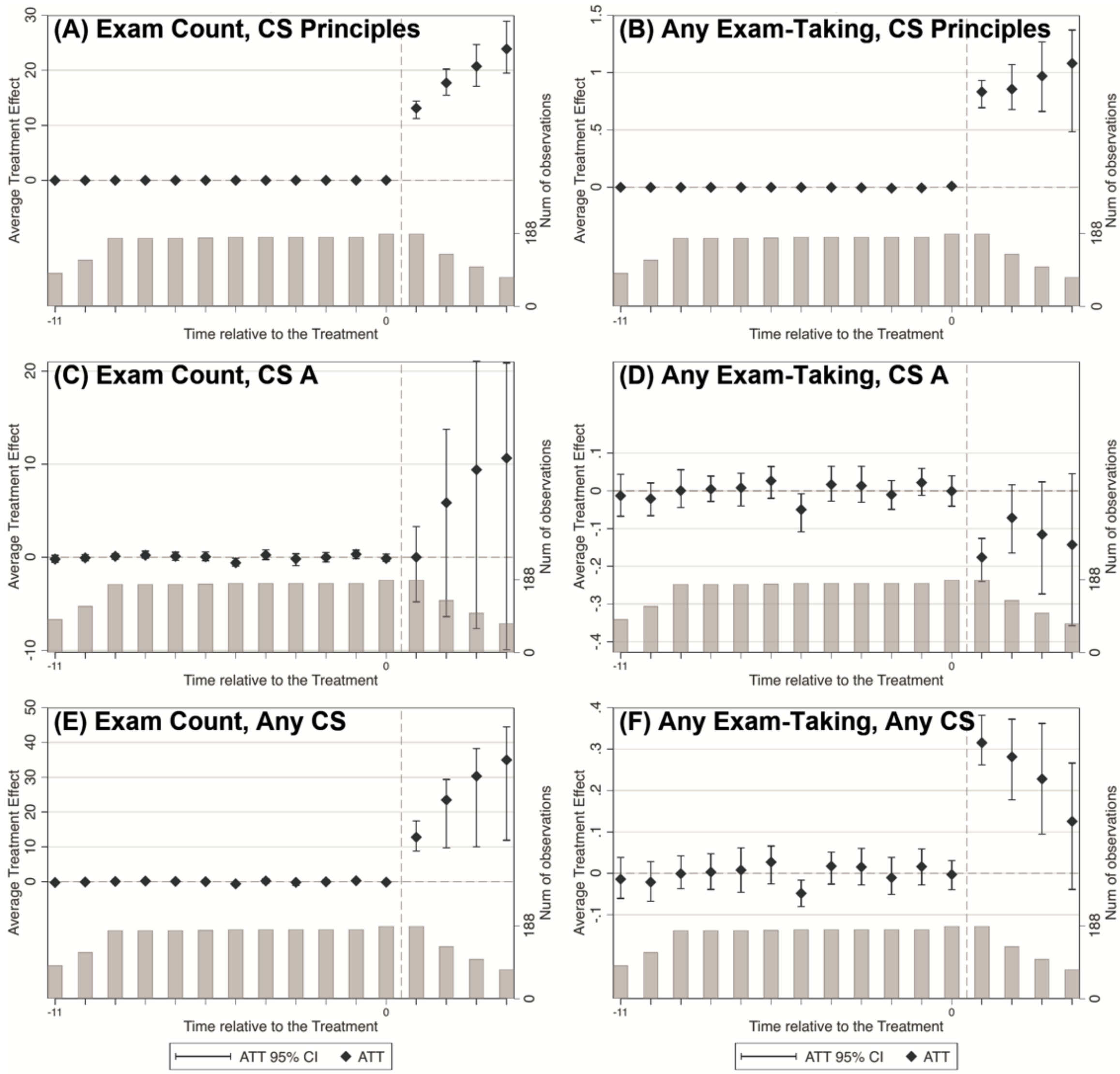

Figure S4. IFEct Dynamic Treatment Effects of AP CS Principles on AP Computer Science Participation.

Estimated effects of consecutive years of offering AP CS Principles on schools having any participation in AP Computer Science. Point estimates show estimated effects, by year, on outcomes in AP Computer Science Principles (Panels A and B), AP Computer Science A (C and D), and any AP Computer Science (E and F). On the left, the dependent variable is the number of exams; on the right, the dependent variable is 1 if a school-year reports at least one AP exam in the given subject. School-year observations are arranged on the horizontal axis according to the number of consecutive years that a school has been treated, with treatment reversal allowed. Bar graphs show the number of treated schools. Point estimates are based on the difference between the observed outcome and the imputed counterfactual. The error bars show 95% confidence intervals. We use interactive fixed effects counterfactual estimators with $r = 2$ for exam counts and $r = 1$ for any exam-taking, based on placebo test results. Estimates are constructed based on an unbalanced panel of 294 schools observed over 15 years (4,381 observations). See Table S3 and the SI text for details.

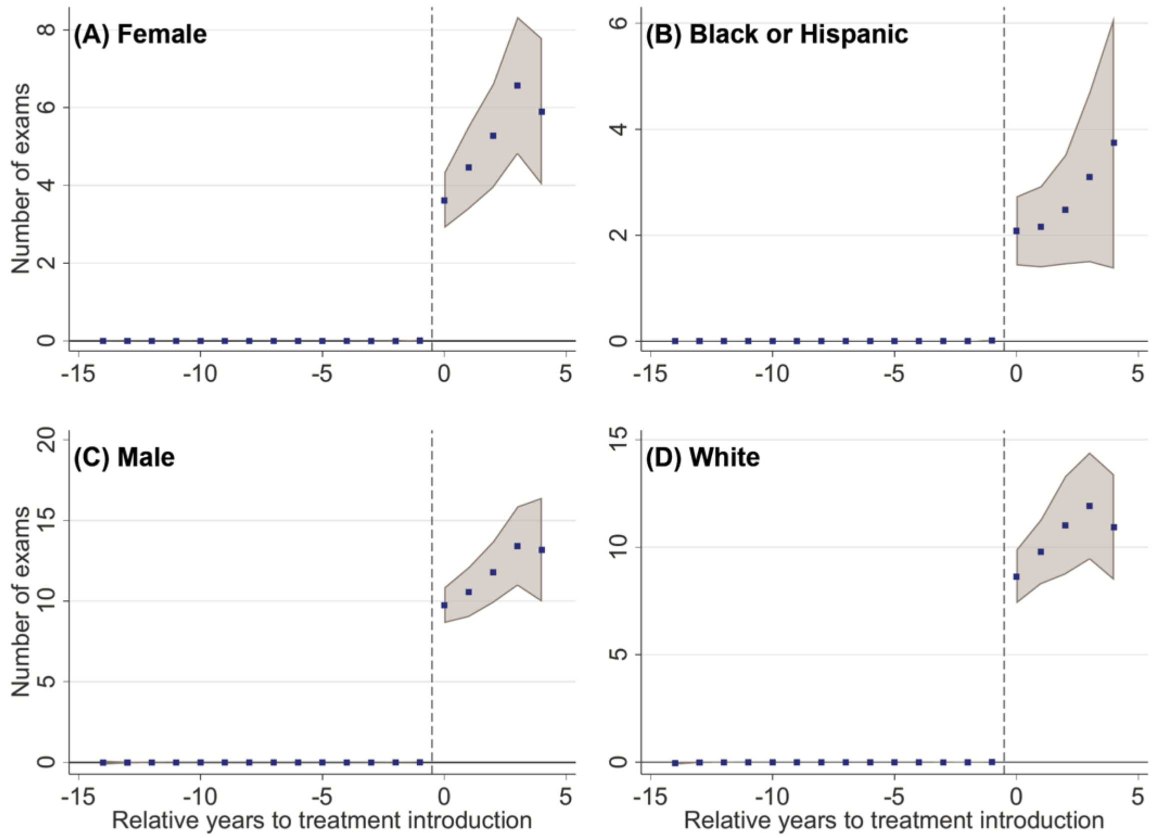

Figure S5. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on AP CS Principles Exam Counts, by Subgroup.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam counts in AP Computer Science Principles, for each student subgroup: (A) female students, (B) Black or Hispanic students, (C), male students, and (D) White students. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years. See Table 1 for details.

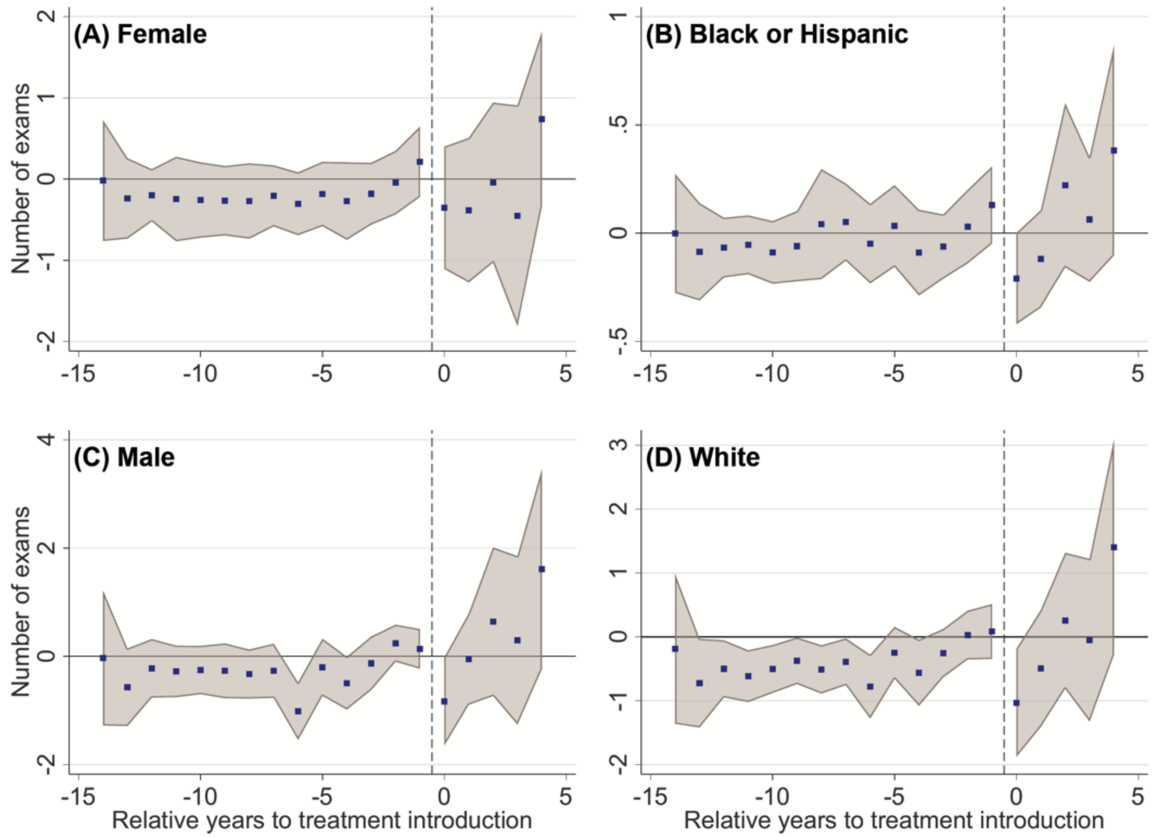

Figure S6. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on AP CS A Exam Counts, by Subgroup.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam counts in AP Computer Science A, for each student subgroup: (A) female students, (B) Black or Hispanic students, (C), male students, and (D) White students. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years. See Table 1 for details.

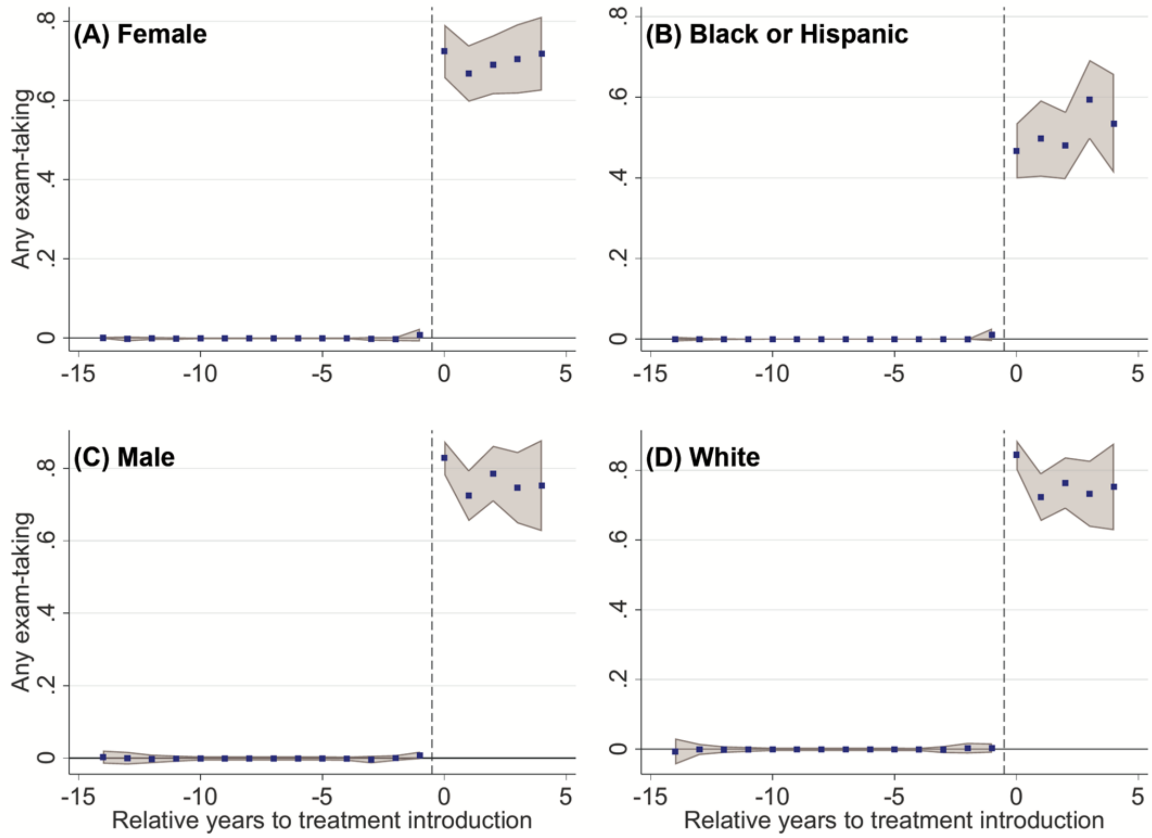

Figure S7. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on Having Any AP CS Principles Exam Participation, by Subgroup.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam-taking in AP CS Principles, for each student subgroup: (A) female students, (B) Black or Hispanic students, (C), male students, and (D) White students. The dependent variable is 1 if a school reports at least one AP exam in the given subject in the given year by a student in the given subgroup. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years. See Table 2 for details.

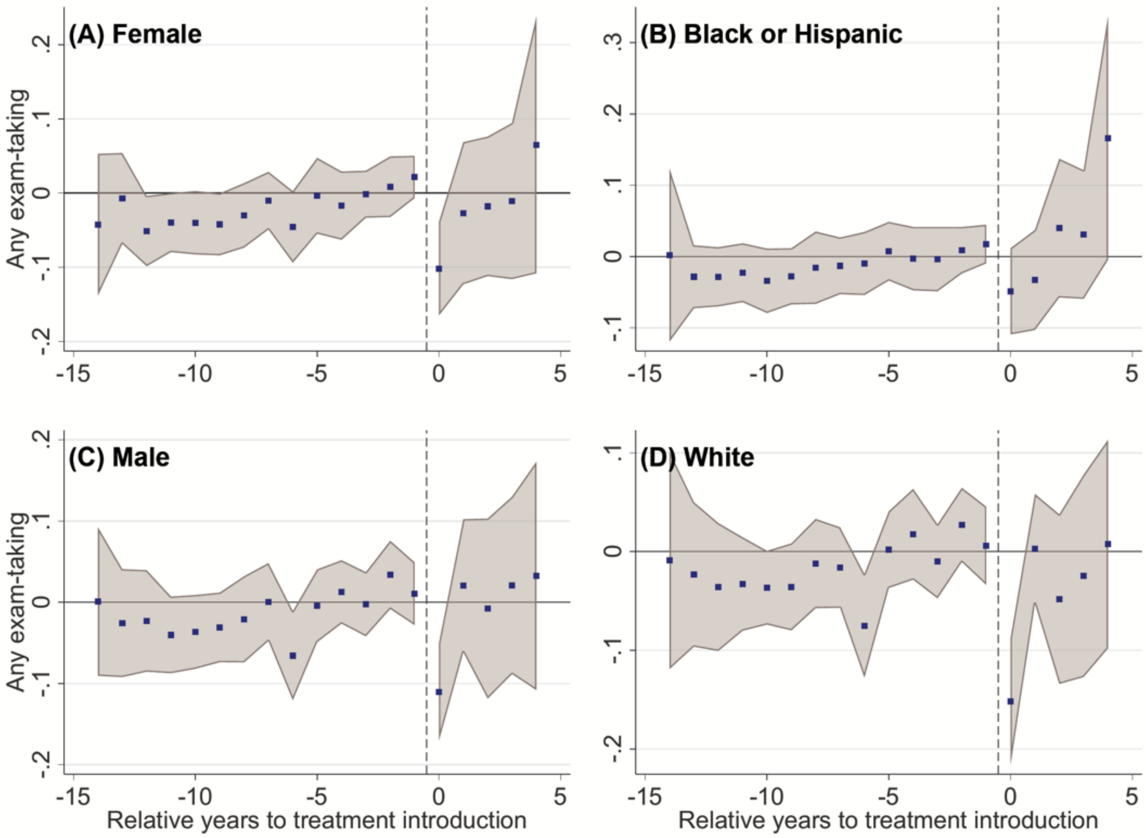

Figure S8. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on Having Any AP CS A Exam Participation, by Subgroup.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam-taking in AP CS A, for each student subgroup: (A) female students, (B) Black or Hispanic students, (C), male students, and (D) White students. The dependent variable is 1 if a school reports at least one AP exam in the given subject in the given year by a student in the given subgroup. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years. See Table 2 for details.

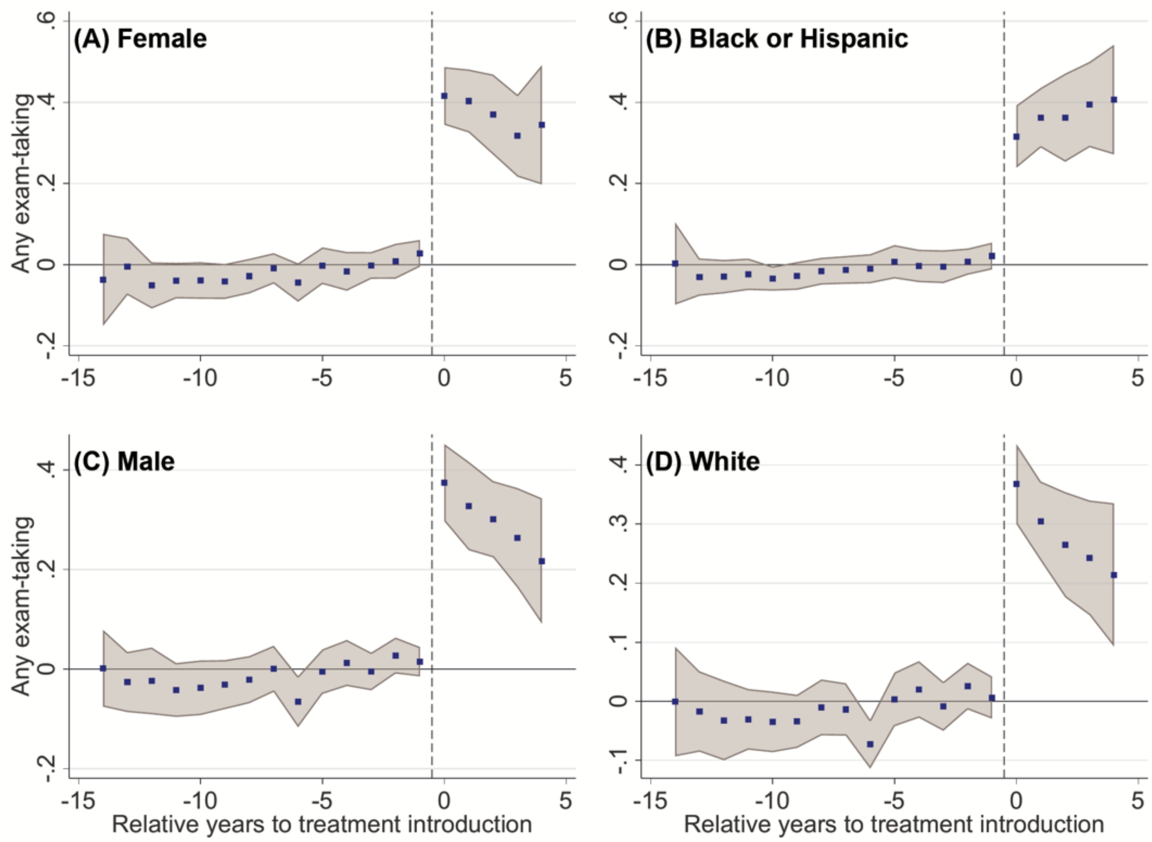

Figure S9. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on Having Any AP Computer Science Exam Participation, by Subgroup.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam-taking in any AP Computer Science, for each student subgroup: (A) female students, (B) Black or Hispanic students, (C), male students, and (D) White students. The dependent variable is 1 if a school reports at least one AP exam in the given subject in the given year by a student in the given subgroup. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years. See Table 2 for details.

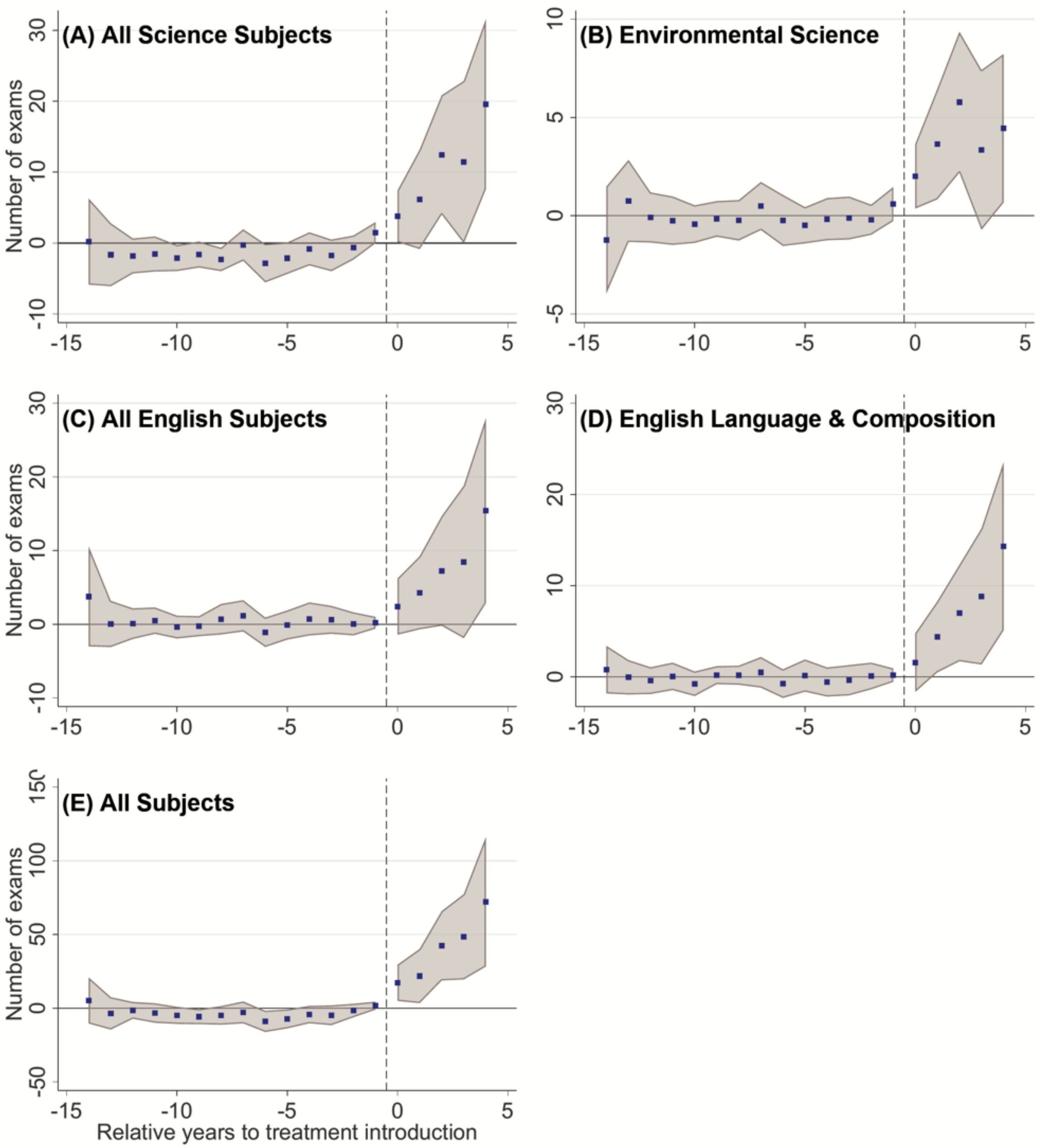

Figure S10. Synthetic Difference-in-Differences Event Studies for the Effects of AP CS Principles on Exam Counts in Other AP Subjects.

Point estimates show the estimated treatment effect for each year since schools' first AP CS Principles introduction on their exam counts in (A) all Science subjects, (B) Environmental Science only, (C) both English subjects, (D) English Language & Composition only, and (E) all AP subjects, including computer science. First treatment occurs in year 0. The shaded gray area shows bootstrapped 95% confidence intervals. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years. See Table S5 for details.

Table S1. Descriptive Statistics

Variable	Mean	SD	Minimum	Median	Maximum
Outcomes					
Exam Count, AP CS Principles	2.29	8.47	0	0	101
Exam Count, AP CS A	3.77	9.22	0	0	121
Exam Count, Any AP CS	6.07	14.05	0	0	186
Any AP CS Principles Exam-Taking	0.13	0.34	0	0	1
Any AP CS A Exam-Taking	0.37	0.48	0	0	1
Any AP CS Exam-Taking	0.43	0.49	0	0	1
Treatment					
AP CS Principles, Ever Prior	0.14	0.35	0	0	1
School-Year Covariates					
Enrollment, Grades 9-12	937	549	89	844	4328
% White	0.73	0.27	0.01	0.85	1.00
% Hispanic/Latino	0.12	0.17	0.00	0.05	0.89
% Black	0.07	0.12	0.00	0.03	0.79
% Asian	0.05	0.07	0.00	0.02	0.58
% First Language Not English	0.13	0.18	0.00	0.05	1.00
% Students with Disabilities	0.16	0.06	0.00	0.15	0.51
% Economically Disadvantaged	0.27	0.21	0.00	0.20	0.92

Notes: The main analytical sample in this study consists of a balanced panel of n = 287 unique traditional public schools in Massachusetts consistently serving grade 9-12 students, observed annually during the 15 school years from 2006-07 to 2020-2021 (n = 4,305 school-by-year observations). 176 schools introduced CS Principles over five treatment cohorts, as shown in Figure 3. The descriptive statistics shown are calculated over all school-by-year observations. Mean all-grades school enrollment is 984. "Any AP CS" includes both AP CS Principles and AP CS A. Treatment refers to our preferred treatment definition, as in line 1 of Table S4.

Table S2. Estimated Effects of AP Computer Science Principles on AP Computer Science Participation Rate Among Grade 9-12 Students

	Estimated Effect by Subject			Baseline Treated
Student Group	AP CS Principles	AP CS A	Any AP CS	Mean (Any AP CS)
All Students	0.0167***	-0.0031*	0.0137***	0.0060
	(0.0009)	(0.0018)	(0.0022)	
Female	0.0098***	-0.0033*	0.0064***	0.0022
	(0.0008)	(0.0019)	(0.0020)	
Male	(0.0237***	-0.0027	0.0210***	0.0098
	(0.0015)	(0.0018)	(0.0020)	
Black or Hispanic	0.0098***	-0.0014	0.0085***	0.0037
	(0.0010)	(0.0010)	(0.0015)	
White	0.0165***	-0.0021	0.0144***	0.0054
	(0.0012)	(0.0015)	(0.0015)	

Notes: The dependent variable is the number of exams among students in the given subgroup, divided by the number of grade 9-12 students in that subgroup. We note that racial/ethnic segregation across schools implies some sparseness in the denominator of this measure (e.g., the number of Black or Hispanic students or the number of White students is below 10 in 6% of school-year observations). In four school-year observations, the number of Black or Hispanic students is zero; we impute zero for the dependent variable. In 8% of school-year observations, state-provided counts of students by gender do not sum exactly to the grade total. The baseline column shows the mean value for any AP Computer Science in 2015-16, the last year prior to the introduction of AP CS Principles, among adopting schools. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years (4,305 observations) using a synthetic difference-in-differences estimator (see text for details). Standard errors (in parentheses) are constructed through school-level block bootstrapping. (*p<0.1, **p<0.05, ***p<0.01)

Table S3. Estimated Effects of AP Computer Science Principles on AP Computer Science Participation, with Alternative Estimation Methods and Covariates

Dependent Variable	Method	AP CS Principles: No covariates	AP CS Principles: Yes covariates	AP CS A: No covariates	AP CS A: Yes covariates	Any AP CS: No covariates	Any AP CS: Yes covariates	Notes
Exam counts	Synthetic Difference-in-Differences	16.090*** (1.088)	16.100*** (1.264)	-0.226 (0.677)	0.098 (0.673)	15.860*** (1.290)	16.190*** (1.497)	Balanced panel; ever-treated definition
	CSDID (Difference-in-Differences with Multiple Time Periods)	15.900*** (1.187)	15.900*** (1.185)	-1.055 (0.711)	-1.329 (0.808)	14.850*** (1.325)	14.570*** (1.301)	Unbalanced panel; ever-treated definition
	Interactive Fixed Effects Counterfactual Estimator (r = 2)	17.995*** (1.153)	18.005*** (1.357)	5.697 (4.446)	6.229 (5.252)	23.703*** (4.600)	24.284*** (5.536)	Unbalanced panel; treatment reversal allowed
Any exam-taking	Synthetic Difference-in-Differences	0.782*** (0.027)	0.786*** (0.022)	-0.049* (0.029)	-0.044 (0.033)	0.289*** (0.028)	0.295*** (0.038)	Balanced panel; ever-treated definition
	CSDID (Difference-in-Differences with Multiple Time Periods)	0.752*** (0.028)	0.746*** (0.029)	-0.099** (0.038)	-0.074 (0.040)	0.240*** (0.042)	0.264*** (0.044)	Unbalanced panel; ever-treated definition
	Interactive Fixed Effects Counterfactual Estimator (r = 1)	0.902*** (0.073)	0.903*** (0.073)	-0.128*** (0.047)	-0.127** (0.052)	0.247*** (0.047)	0.247*** (0.056)	Unbalanced panel; treatment reversal allowed
	Interactive Fixed Effects Counterfactual Estimator (r = 2)	0.815*** (0.074)	0.813*** (0.065)	-0.004 (0.144)	0.000 (0.137)	0.350*** (0.125)	0.352*** (0.119)	Unbalanced panel; treatment reversal allowed

Notes: Synthetic Difference-in-Differences estimates, which repeat earlier results, are constructed based on a balanced panel of 287 schools observed over 15 years (4,305 observations). Other methods use the slightly larger full, unbalanced panel of 294 schools (4,381 school-year observations). Counterfactual estimators, which allow for treatment reversal, measure treatment in each school-year observation; other methods define a school-year observation to be treated if there has been treatment in this school ever prior. Counterfactual estimator results shown all pass the placebo test, with $p > 0.1$. Results with $r = 1$ are not shown for exam counts because all six results fail the placebo test. CSDID results all fail the pretrend test ($p < .01$), except for the two CS Principles results that use covariates. Columns with covariates use the school-year covariates listed in Table S1. For "exam counts", the dependent variable is the count of exams among students in the given subgroup in the given subject. For "any exam-taking", the dependent variable is 1 if a school-year reports at least one AP exam in the given subject by a student in the given subgroup. Standard errors (in parentheses) are constructed through school-level block bootstrapping, except for CSDID which uses asymptotic normal standard errors. (*p<0.1, **p<0.05, ***p<0.01)

Table S4. Estimated Effects of AP Computer Science Principles on AP Computer Science Participation, with Alternative Treatment Definitions

				Estimated Effect by Subject		
Treatment Definition	Data Source Used	% Treated	Dependent Variable	AP CS Principles	AP CS A	Any AP CS
(1) Official CS Principles offering and/or 5+ CS Principles exams, ever prior	College Board Ledger and DESE Exam Data	14.05	*Exam Counts*	16.090***	-0.226	15.860***
				(1.088)	(0.677)	(1.290)
			Any Exam-Taking	0.782***	-0.049*	0.289***
				(0.027)	(0.029)	(0.028)
(2) Official CS Principles offering, ever prior	College Board Ledger only	13.87	*Exam Counts*	16.150***	-0.150	16.000***
				(1.020)	(0.657)	(1.446)
			Any Exam-Taking	0.765***	-0.035	0.296***
				(0.029)	(0.031)	(0.037)
(3) Official CS Principles offering (virtual-only excluded), ever prior	College Board Ledger only	13.08	*Exam Counts*	17.100***	-0.152	16.950***
				(1.220)	(0.702)	(1.359)
			Any Exam-Taking	0.785***	-0.030	0.308***
				(0.025)	(0.033)	(0.038)
(4) 5+ CS Principles exams, ever prior	DESE Exam Data only	12.31	*Exam Counts*	18.160***	-0.151	17.950***
				(1.450)	(0.843)	(1.355)
			Any Exam-Taking	0.806***	-0.035	0.307***
				(0.024)	(0.025)	(0.037)
(5) Pre-COVID data only: official CS Principles offering and/or 5+ CS Principles exams, ever prior	College Board Ledger and DESE Exam Data	7.32	*Exam Counts*	17.270***	-1.457	15.800***
				(1.203)	(0.984)	(1.489)
			Any Exam-Taking	0.875***	-0.073*	0.315***
				(0.019)	(0.042)	(0.038)

Notes: The dependent variable is the count of exams among students in the given subgroup in the given subject. The % Treated column shows the portion of school-year observations that were treated according to each treatment definition. Definition (1) is the preferred definition used elsewhere in the paper. Estimates are constructed using a synthetic difference-in-differences estimator (see text for details). Estimates in rows (1) - (4) are based on a balanced panel of 287 schools observed over 15 years (4,305 observations). Row (5) uses only 13 years of data (3,731 observations), excluding spring 2020 and spring 2021 exams. Standard errors (in parentheses) are constructed through school-level block bootstrapping. (*p<0.1, **p<0.05, ***p<0.01)

Table S5. Estimated Effects of AP Computer Science Principles on Participation in Other AP Subjects

	Number of Exams		Any Exam-Taking	
Subject	Estimated Effect	Baseline Treated Mean	Estimated Effect	Baseline Treated Mean
All	33.460***	358.483	0.013	0.960
	(9.662)		(0.018)	
Computer Science	15.860***	7.034	0.289***	0.597
	(1.290)		(0.028)	
CS Principles	16.090***	0.000	0.782***	0.000
	(1.088)		(0.027)	
CS A	-0.226	7.034	-0.049*	0.597
	(0.677)		(0.029)	
Science	8.821***	80.455	0.029	0.920
	(2.943)		(0.021)	
Biology	2.118	26.818	-0.003	0.847
	(1.305)		(0.023)	
Chemistry	1.450	16.761	0.050*	0.750
	(1.078)		(0.026)	
Physics: E&M	0.448	2.568	0.009	0.239
	(0.375)		(0.026)	
Physics: Mechanics	0.251	6.778	0.020	0.381
	(0.657)		(0.028)	
Physics 1	2.205*	13.864	0.011	0.568
	(1.286)		(0.035)	
Physics 2	-0.388	2.688	-0.015	0.188
	(0.313)		(0.026)	
Environmental Science	3.641***	10.977	0.031	0.551
	(1.050)		(0.040)	
English	6.015**	69.097	0.018	0.943
	(2.993)		(0.017)	
Language & Composition	5.701***	37.398	0.061***	0.835
	(2.107)		(0.023)	
Literature	0.223	31.699	0.040*	0.932
	(1.111)		(0.020)	
History & Social Sciences	5.551	113.159	-0.012	0.915
	(3.664)		(0.019)	
Math	1.354	63.756	-0.019	0.932
	(1.982)		(0.015)	
Foreign Languages	0.071	19.528	-0.043	0.699
	(0.829)		(0.027)	
Arts	0.390	5.085	-0.015	0.540
	(0.316)		(0.033)	
Capstone	0.612	0.369	0.012	0.023
	(0.646)		(0.020)	

Notes: Subject groups are listed in bold; when the overall effect is significant, we also list the individual constituent subjects in italics. Computer Science AB and Physics B, which were discontinued prior to the introduction of CS Principles, are not shown. On the left, the dependent variable is the count of exams in the given subject or subject group. On the right, the dependent variable is 1 if a school reports at least one AP exam in the given subject or subject group in the given year. The last column shows the mean value in 2015-16, the last year prior to the introduction of AP CS Principles, among adopting schools. Estimates are constructed based on a balanced panel of 287 schools observed over 15 years (4,305 observations) using a synthetic difference-in-differences estimator (see text for details). Standard errors (in parentheses) are constructed through school-level block bootstrapping. (*p<0.1, **p<0.05, ***p<0.01)

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