The Advanced Placement Program and Educational Inequality

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Abstract

The Advanced Placement (AP) program is nearly ubiquitous in American high schools and is often touted as a way to close racial and socioeconomic gaps in educational outcomes. Using administrative data from Michigan, I exploit variation within high schools across time in AP course offerings to identify the causal effect of AP course availability on college choice and degree attainment. I find that higher income students, White and Asian students, and higher-achieving students are more likely to take advantage of additional AP courses when they are offered, thus widening existing gaps in course-taking. I find little evidence that additional AP availability improves college outcomes for any students. Expanding access to AP courses without additional incentives or support for disadvantaged students to succeed is unlikely to address educational inequality.

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1 Introduction

Since its introduction in 1952, the Advanced Placement (AP) program, which provides an opportunity for ambitious high school students to take college-level courses and possibly obtain college credit or placement out of introductory college courses, has grown dramatically and become nearly ubiquitous in American high schools. Despite the popularity of AP and the perception that participation improves college preparation, increases chances of admission to selective colleges, and accelerates degree attainment, there is little convincing causal evidence on how taking AP courses affects human capital investment and later outcomes.

In this paper, I investigate whether and how Advanced Placement courses affect the selectivity of college students attend, on-time college graduation, and overall college graduation. I use administrative data from the state of Michigan and exploit variation within high schools across time in how many AP courses are offered to identify the causal effect of AP course availability. Because there is reason to believe that AP affects different types of students differently—and because of policy interest in AP access as a driver of educational inequality—I also test for heterogeneous treatment effects by socioeconomic status, race, and prior academic preparation level.

I find very little evidence that a school offering an additional AP course improves students' outcomes. I find precisely estimated null effects of AP course availability on college selectivity and degree attainment, and no differential effects by family income or race. Students who enter high school with strong academic preparation (measured by performance on a standardized math test) are the only ones who experience positive benefits of additional course offerings. I estimate that for a student whose middle school math performance was a standard deviation above average, having an an additional AP course available increases their likelihood of earning a bachelor's degree in four years by 0.6 percentage points, and increases the six-year graduation rate by 0.3 percentage points. However, these effects are substantively small, and are not robust to two-way fixed effects bias corrections.

The lack of effect on student outcomes is partly driven by a weak first stage: when an additional AP course is offered at a high school, the average number of AP courses taken only increases by 0.032, which translates into fewer than 10 students in the typical high school. For one additional course offering, the average number of AP exams taken increases by a statistically insignificant 0.012. Taking the exam and receiving a sufficiently high score are required for college credit or placement, and selective colleges use AP scores to evaluate applicants. The small increases in AP enrollments and especially exams help explain the limited effect of expanding AP offerings.

Not only do expanded AP offerings serve few students, they serve students unequally. Higher-income students (those not eligible for subsidized school meals) take more AP courses and exams when they are offered, but low-income students do not. Likewise, only White and Asian students—and not Black or Hispanic ones—take advantage of expanded AP curricula.

Taken as a whole, the results suggest that at best, expanding AP programs will do little to improve outcomes or close achievement gaps. At worst, increasing AP course offerings may exacerbate socioeconomic and racial inequalities in access to advanced coursework.

The paper proceeds as follows: Section 2 provides history and background on the AP program; Section 3 reviews prior related work; Section 4 describes the methodological approach and data; Section 5 presents findings about the effect of AP courses on college outcomes, as well as on the first stage of AP course-taking; Section 6 discusses threats to identification and presents robustness checks; and Section 7 concludes.

2 Background

The Advanced Placement (AP) program traces its origins to just after World War II, when the Ford Foundation created the Fund for the Advancement of Education and concluded that better coordination between secondary and postsecondary schools would help increase the number of college entrants and graduates in the United States. A committee was formed "to develop high school course descriptions and assessments that colleges would find rigorous enough to use as a basis for granting credit" and a pilot program in 11 subject areas was launched in 1952 (College Board, 2003, p. 1). Since 1955, the AP program has been run by the College Board, the same non-profit organization responsible for the SAT college entrance exam.

Participation has grown dramatically since the program's inception, from 1,229 students at 104 schools nationwide in the 1955-56 academic year (the first year data are available from the College Board) to 2.5 million students and nearly 23,000 schools in 2021 (College Board, 2021a). In 2012 (the most recent year for which this figure is available), 74 percent of all public high schools offered AP courses (Malkus, 2016), and these schools serve even more students as a proportion of all public high school students (Theokas and Saaris, 2013). Trends for the state of Michigan—the setting for the current study—are similar.

A non-trivial amount of federal, state, and local public funds are dedicated to subsidizing AP teacher training, exam fees, and performance incentives (Klopfenstein, 2010). The U.S. Department of Education created Advanced Placement Incentive Program grants in the late 1990s to increase AP participation among low-income students and reduce achievement gaps; this program was expanded under No Child Left Behind in 2001 (Klopfenstein, 2010). In 2016, the Department of Education awarded over $28 million to subsidize exam fees for low-income students in 41 states (including $560,000 to Michigan) plus the District of Columbia (U.S. Department of Education, 2016). A number of large school districts, including New York City and Washington, D.C., have adopted policies mandating a minimum number of AP offerings per school (Tugend, 2017). Some schools in Washington, D.C. require all students to enroll in at least one AP course (Burnett and Burkander, 2021).

The AP program serves several ostensible purposes. The College Board describes it as a way to "[enable] willing and academically prepared students to pursue college-level studies—with the opportunity to earn college credit, advanced placement or both—while still in high school" (Rodriguez, McKillip, and Niu, 2013, p.1). The College Board also touts participation as beneficial to college admission and performance, saying that "Taking AP courses demonstrates to college admission officers that students have sought the most rigorous curriculum available to them, and research indicates that students who score a 3 or higher on an AP Exam typically experience greater academic success in college and are more likely to earn a college degree than non-AP students" (Rodriguez, McKillip, and Niu, 2013, p. 1). As summarized by Klopfenstein and Thomas (2010), "while the College Board generally makes no explicit statements that AP experience is a cause of college success, their promotional literature readily leads readers to such a conclusion" (p. 170).

As of 2023, the College Board offers 38 AP courses in six subject areas: science, math and computer science, history and social sciences, English, world languages and cultures, and arts. In 2021, the most popular subjects (by exams taken) were English Language and Composition, U.S. History, English Literature and Composition, World History, Psychology, and U.S. Government (College Board, 2021b).

3 Related Literature

The current study fits within a number of overlapping literatures. One way to conceptualize the AP program is as a high-ability track within a high school. There is a large literature on ability and achievement tracking that informs theory about the effects of AP participation, particularly differential effects by student type (see Betts, 2011, for a review). Theoretically, there is an efficiency-equity tradeoff in any tracking system; the empirical evidence is mixed. Others see AP as a type of or alternative to dual enrollment programs (see, for example, Klopfenstein and Lively, 2012), which have the explicit goal of reducing the financial and time cost of postsecondary education. There is also a broader literature on high school curriculum (e.g., Altonji, 1995).

There are several key mechanisms by which we might expect participation in

AP courses and exams to affect educational choices and outcomes. AP courses are generally considered more rigorous than standard high school classes, so that the experience of taking an AP course and preparing for the exam may directly increase students' knowledge, skills, and college readiness. AP participation can serve as a signal of student ability, motivation, and college readiness, as well as a signal of school quality, which are used by admissions committees at selective colleges in evaluating applicants. Students earning sufficiently high scores on AP exams (a three on a five-point scale, in most cases, though policies vary by institution) can earn college credit and/or placement out of introductory-level college courses, thus shortening time to graduation.

The predicted effects of AP participation are not unambiguously positive. Performance in AP courses and particularly on AP exams may serve as a signal to students that causes them to re-asses their own academic ability and potential for college success; depending on performance, students could revise their self-assessment upwards or downwards.[1] The effort required and stress induced by rigorous AP courses could crowd out effort in other academic and non-academic tasks, depending on the degree of complementarity between the various tasks. This is particularly important in considering policies that subsidize or incentivize AP participation in some way, as they may induce some students to take more than the socially optimal number of AP courses or exams.

Rigorous causal evidence on the effect of AP is fairly limited. Jackson (2010) evaluates the Advanced Placement Incentive Program in Texas, which paid students and teachers for passing AP exams and provided training to teachers. He exploits exogenous variation in when schools implemented the program and finds that it increased participation in AP courses and exams, the number of students scoring highly on the SAT or ACT, and college matriculation. In a longer-term follow up, Jackson (2014) shows positive effects on degree attainment and earnings.

[1] For evidence that grades and standardized test scores can lead to this type of belief updating, see Jacob and Wilder (2010); Goodman (2016); Gonzalez (2017); and Avery and Goodman (2022).

In the only experimental work to date, Conger et al. (2021) randomly assigned high school students into a treatment that included the option to enroll in newly introduced AP Biology or Chemistry course in their schools. Taking an AP science course resulted in a higher self-reported level of course rigor and a higher level of science skill. However, in a longer-term follow-up, Conger, Long, and McGhee (2023) find no effect on SAT or ACT performance, no change in students' self-reported portfolio of college applications, and no ultimate effect on selective college enrollment. They also find suggestive evidence that competitive college enrollment may have *decreased*. These somewhat discouraging findings point to the importance of considering who the marginal students are when expanding access. In a world where nearly all schools have AP courses available, the marginal student may be less prepared and unlikely to benefit.

A related series of studies exploit cutoffs in continuous AP exam scores that translate into the 1-5 integer scores reported to students and colleges. Smith, Hurwitz, and Avery (2017) find that receiving a credit-granting score (a three in most cases) on an AP exam positively affects on-time college graduation. Avery et al. (2018) use the same regression discontinuity design as Smith, Hurwitz, and Avery (2017) and find that receiving a higher score on an AP exam significantly increases the likelihood that a student will major in that subject in college; they argue that "a substantial portion of the overall effect is driven by behavioral responses to the positive signal of receiving a higher score" (p. 918). Gurantz (2021) uses a similar regression discontinuity strategy to examine college course-taking by subject, finding that women who earn credit from AP exams in STEM subjects take more STEM courses.

It is important to note that receiving credit or placement is contingent on taking and passing an AP exam. A significant proportion of students who take an AP course do not take the associated exam; Fazlul, Jones, and Smith (2021) find that 15 percent of AP course enrollments (in four metro Atlanta school districts) do not result in an exam. Even among those who take an exam, many do not receive a passing score. These numbers are likely even higher for schools and students on the margin of offering and taking AP. In the setting of Conger, Long, and McGhee (2023)—schools that had not previously offered AP science—40 percent of treated students opted out of the exams, and 85 percent of those who did take the exams did not pass.

The current study represents, to my knowledge, the first causal evidence on the long-term effects of AP course offerings on college outcomes. Although Conger, Long, and McGhee (2023) examine effects on enrollment, they do not yet have results on college persistence and graduation. Given the possibility that AP affects college readiness (in ways that may or may not be reflected in standardized test scores), long-term effects may emerge even in the absence of shorter-term ones. Furthermore, I use naturally occurring changes to AP course offerings across a number of subjects, whereas Conger, Long, and McGhee (2023) focus on AP science only. Although advanced science courses are an important part of the curriculum to study, my findings are relevant to a larger set of schools. Jackson (2014) studies college completion, but for a program that financially incentivizes students and teachers to pass exams, paired with teacher training. It is unclear whether his positive findings translate to a program with fewer resources and with less emphasis on exams. Smith, Hurwitz, and Avery (2017) look at on-time college graduation as an outcome, but only for students who take an AP exam and are close to passing. As mentioned above, this population is a small subset of the overall population of students on the margin of taking AP. While the small portion of students who take and pass an AP exam may benefit, a full accounting of the effects of AP must consider any effects (or lack thereof) on a larger population. Thus, the findings of the current paper will be useful to educators making the highly relevant decision of whether to offer an additional AP course or hire an additional AP teacher.

4 Method and Data

4.1 Identification

Simply comparing students or schools with different levels of AP courses or exams will give an upwardly biased estimate of the effect on educational outcomes, since students taking AP and schools offering AP tend to be higher-achieving to begin with. To estimate the causal impact of AP course availability on college outcomes, I exploit time variation in how many AP courses a high school offered each year. My strategy is similar to that of Darolia et al. (2020), who use what they argue is "plausibly exogenous variation in course offerings within high schools over time" (p. 22) to study the effect of STEM course availability on postsecondary STEM enrollment and degree attainment in Missouri. My identification strategy, like theirs, hinges on year-to-year differences in course offerings within a school being (conditionally) exogenous. This would be the case if the variation is due to things like unrelated changes in teaching staff (due to, e.g., retirement or parental leave) and rules governing class size.

I use panel data covering the graduating classes of 2005 through 2012 in a sample of Michigan public high schools. By controlling for school fixed effects, I compare a cohort of high school seniors to another cohort from the same school, where one cohort had a higher number of AP courses available to them. I also include year fixed effects to account for the general upward trend in both AP and college outcomes. School-specific time trends account for the possibility that schools on an especially steep trajectory in terms of outcomes differentially select into offering more APs.

My primary estimating equation is

$$Y_{ijt} = \beta_0 + \beta_1(\#\text{ AP courses available})_{jt,t-1} + \delta_j + \lambda_t + \tau_j t + \varepsilon_{ijt} \tag{1}$$

where Y_{ijt} is the outcome of interest for student i graduating from school j in year t. The three outcomes I measure are (1) whether a student enrolled at a college that is classified as competitive or higher by the Barron's selectivity index, (2) whether they earned a bachelor's degree within four years of graduating high school, and (3) whether they earned a bachelor's degree within six years. The treatment is the count variable (# AP courses available)$_{jt,t-1}$: the number of AP subjects available to cohort t at school j during their junior and senior year.[2] δ_j are school fixed effects; λ_t are year fixed effects; and τ_j are school-specific linear time trends. I estimate Equation 1 with a linear probability model and cluster standard errors at the school level.

To test for heterogeneity by socioeconomic status, I subset the data and estimate Equation 1 separately for students who are and are not eligible for free or reduced-price lunch (FRPL) in 12th grade.[3] To test for heterogeneity by race, I estimate separate regressions for underrepresented minority (URM) students (i.e., Black, Hispanic, or Native) and non-URM (White or Asian). To test for heterogeneity by academic preparation, I add an interaction term between the number of AP courses and standardized score on the Michigan standardized math test in middle school:[4]

$$Y_{ijt} = \eta_0 + \eta_1 AP_{jt,t-1} + \eta_2 Math_i + \eta_3 AP_{jt,t-1} \cdot Math_i + \delta_j + \lambda_t + \tau_j t + \varepsilon_{ijt} \quad (2)$$

In Equation 2, I've abbreviated the treatment variable—number of AP courses available—to $AP_{jt,t-1}$. Here, η_1 is the effect for a student with an average middle school math score, and $\eta_1 + \eta_3$ is the effect for a student with a math score one standard deviation above the mean.[5]

Equations 1 and 2 represent the reduced form or intent-to-treat effect of AP course availability, which is a policy-relevant parameter for schools and districts considering introducing or expanding an AP program. I also estimate first stage effects

[2]As an example, if school j offered AP Biology and U.S. History in 2006 and Biology and U.S. Government in 2007, $AP_{j,2007,2006}$ would equal 3. This variable can take values between 0 and 26 AP subjects. I collapsed a number of subjects that the transcript data didn't allow me to distinguish between. For example, microeconomics and macroeconomics are two distinct subjects, but many schools just listed "AP economics." Appendix Figure A1 summarizes these decisions.

[3]In Michigan, the threshold for subsidized lunch is family income up to 185 percent of the federal poverty line. In 2019, this was equivalent to $47,638 for a family of four.

[4]The grades in which the state of Michigan tests students by subject have changed over time. I use a student's eighth grade test score if it is available, and their seventh grade score if not. I use math scores because the other subject tests were not offered in the relevant years for the full sample.

[5]Test scores are not available for all students; they would be missing if the student attended middle school in a different state or at a private school, or if they were exempt from the test. For the heterogeneity analysis by test score, students missing test scores are omitted.

of course availability on AP course- and exam- taking:

$$\begin{aligned}(\text{\# AP courses taken })_{ijt,t-1} = \alpha_0 + \alpha_1(\text{\# AP courses available})_{jt,t-1} \\ + \delta_j + \lambda_t + \tau_j t + \varepsilon_{jt}\end{aligned} \tag{3}$$

I test for heterogeneity in first stage effects in the same way I do for the reduced form: with separate regressions by income and race, and with an interaction term with test score. The first stage results provide important evidence about how increasing AP offerings increases access, and for whom.

Another relevant treatment effect parameter would be the effect of an additional AP course for the students who actually take the course (the treatment effect on the treated). This suggests an instrumental variables strategy using course availability as an instrument for course-taking. However, the validity of the IV estimates relies on the exclusion restriction that the presence of AP courses at a school affects students only so far as it encourages them to take more AP courses and exams. This would be violated in the presence of within-school spillovers, such as positive spillovers of AP content and a more college-oriented culture, or negative spillovers due to diversion of resources. The direction of the bias here is theoretically ambiguous. For this reason, I consider the reduced form effects more internally valid. Furthermore, as I show below, the first stage is sufficiently weak that two-stage least squares estimates would be invalid. Therefore, I present only reduced form and first stage results.

4.2 Data

The data I use are provided by the Michigan Department of Education and accessed through the Michigan Educational Data Center (MEDC). My first data source is the Michigan Transcript Study, which collected longitudinal transcript data from a random sample of Michigan public high schools. This dataset includes, for each school in the sample, every course taken by students at that school in a given year. The collected course data are sufficiently clean for 87 of the schools.

In order to measure the treatment I am interested in—AP courses available by school and AP courses taken by student—I systematically identified which courses were AP based on course title. The way in which schools list courses is not standardized across schools. Flagging courses as AP was an iterative process that started with more obvious course titles (e.g. "AP Calculus" or "Advanced Placement Biology") and continued by searching for other phrases associated with AP and with one of the recognized AP subjects (e.g. "AP CMP GOV" for comparative government and politics). While some courses were obviously AP, others were more ambiguous. If I wasn't reasonably sure a course was AP, I erred on the more conservative side and did not classify it as AP. I assign course availability at the school level and course-taking at the student level, counting by number of subjects. For a subset of the students for whom I have course-taking data, I can also observe how many AP exams they took. MEDC has access to all AP exams taken by Michigan students between 2006 and 2013. Since most students take AP in their junior and senior years, I can count AP exams for the classes of 2007 onward.[6]

To identify cohorts of high school seniors by school, I use demographic and enrollment data from the Michigan Student Data System. This student-by-year panel dataset contains demographic information (including race and free and reduced lunch eligibility) as well as the school and district each student attends each year. Information on college outcomes comes from the National Student Clearinghouse (NSC). The NSC provides information on college enrollment at any four- or two-year school in the country (with a few exceptions), by date of enrollment and institution. For heterogeneity analysis by prior student achievement, I use K-12 student assessment data containing standardized test scores.

My final sample includes 174,469 students at who were seniors at 87 public Michigan high schools between 2005 and 2012.

[6]As is standard in the education literature, years refer to the spring of the academic year. For example, 2006 refers to the 2005-2006 school year.

5 Results

5.1 Descriptive Results

I begin with descriptive statistics about the students and schools in the sample, summarized in Table 1. Roughly half of the students are female. The majority, 74 percent, are White, four percent are Asian, 17 percent are Black, three percent are Hispanic, and fewer than one percent are Native (a category which includes American Indian, Alaska Native, Native Hawaiian, and Pacific Islander students). Given the small number of Asian, Hispanic, and Native students, for analyses by race and ethnicity I collapse the categories into underrepresented minority students (Black, Hispanic, and Native) and non-URM (White and Asian). Around a quarter of students in the sample are eligible for free or reduced-price lunch, which I use as a proxy for family income. These means all closely resemble the full population of Michigan seniors during this time. At the school-cohort level, the average school in the panel enrolls around 1,400 students, has a student-to-teacher ratio of 21, spends $6,300 per student, and has a local unemployment rate of 9 percent.[7]

The average student in the sample has just under 10 AP courses available to them during their junior and senior year, takes 0.78 courses, and takes 0.73 exams. The average school offers 8.56 courses to a cohort. I provide more detail on the variation in AP course offerings by school and across time, as well as AP course- and exam-taking, in Appendix Table A1 and Appendix Figures A2 through A10. Over time, the most common AP course offerings are English, Calculus, U.S. History, Biology, and Chemistry (see Appendix Table A1). The most common courses taken are English, Calculus, U.S. Government, Biology, and Psychology; and the most popular exams are English, Calculus, U.S. History, U.S. Government, and Biology.[8]

[7]The minimum values for the school enrollment and student-to-teacher ratio variables are zero. These characteristics are measured two years prior to the current year, so zeros reflect the small number of schools that were recently established.

[8]Recall that English and Calculus are each actually two separate courses: English Literature and Composition and English Language and Composition, and Calculus AB and BC. (See Appendix Figure A1.) Still, the hierarchy in Table A1 corresponds to national and Michigan AP exam data from the College Board.

While the vast majority of schools offered at least one AP course to their juniors and seniors over the entire period, there is considerable variation in the number offered. The number of AP courses varies both across and within schools over time (see Appendix Figures A3, A4, and A5), and the changes go in both directions. My identifying variation comes from within-school increases and decreases in AP course offerings. These changes are driven by particular courses. The most common subjects to be introduced are World History, Economics, Psychology, Biology, and Statistics; the most likely to be taken away are Psychology, Computer Science, European History, World History, and Economics. The most marginal subjects—meaning those that experience the most changes in both directions—are Psychology, World History, and Computer Science.

5.2 Reduced Form Effect of AP Course Availability on College Selectivity and Graduation

To identify the causal effect of course availability on the probability of enrolling in a competitive college, graduating within four years of leaving high school, and graduating within six years, I estimate Equation 1 on the sample of seniors in Michigan public high schools. The reduced form estimates appear in Table 2. On average, there is no effect of an additional AP course offering on any of the outcomes; all of the treatment coefficients are close to zero, none are significant, and they are estimated precisely. For example, the effect on enrolling in a competitive college is 0.2 percentage points, with a standard error of 0.2 percentage points. The effects on four-year and six-year BA attainment are a statistically insignificant 0.1 percentage points. For all three outcomes, I can rule out (with 95 percent confidence) effects greater in magnitude than a single percentage point.

There is reason to believe that the effects of AP vary by student type. In particular, more academically prepared students might be more likely to reap the benefits of a college-level curriculum. Less prepared students might have more to gain from more rigorous courses; on the other hand, they could fall further behind their peers if they're pushed into courses beyond their preparation level, or might be harmed by the diversion of resources towards AP students and teachers. Structural inequities may prevent low-income and underrepresented minority students from accessing advanced classes, as well as contribute to their lower levels of academic preparation. To test this, I estimate reduced form effects of AP course availability, this time estimating separate effects by a proxy for low family income (free or reduced price lunch eligibility) and student race (an indicator for underrepresented minority), and with an interaction with middle school math test score.

Table 3 displays the reduced form effect of AP availability by family income, which I proxy by eligibility for subsidized meals. The control means (in square brackets for each group) indicate existing gaps, with lower income students half as likely to attend a selective college and a third as likely to earn a BA in six years. There is no evidence that additional AP course offering improve college choice or graduation for low- or higher-income students. All of the effects are close to zero, precisely estimated, and statistically insignificant. Even if we took the point estimates at face value, they would translate into not even one additional low- or higher-income student per school enrolling in a competitive college. (The average cohort has 72 low-income students and 215 non-low-income students in its senior class.) Examining heterogeneity by race (Table 4) suggests a similar story. All estimated effects are small (less than half of a percentage point) and statistically insignificant.

I also examine heterogeneity by prior academic achievement, which I measure using standardized scores on the state middle school math test. Table 5 summarizes the reduced form effect of AP course availability by prior test scores. Here, there is some evidence of positive effects for the most academically prepared students. While a student with average middle school test scores sees no improvement in outcomes from an additional course offering (main effect, first row), higher-achieving students do. The interaction terms with test score for the two graduation outcomes are positive and significant at the $\alpha = 0.01$ level. A one standard deviation increase in test score increases the size of the AP course effect on four-year graduation by 0.7 percentage points and on six-year graduation by 0.3 percentage points. This implies that for a student with a math score one standard deviation above the mean, the additional available AP increases their chance of on-time college graduation by 0.6 percentage points.

The reduced form analyses imply that expanding AP course availability has little discernible effect on student outcomes for most students. The exception is a small but significant effect on graduation outcomes for students who enter high school with the strongest academic preparation; though there may be small positive effects for these students, it would come at the expense of widening existing achievement gaps.

5.3 First Stage Results on AP Course- and Exam-Taking

The generally null effects of expanding AP course offerings could be due to two (not mutually exclusive) explanations. First, they may have little effect because few students (or only particular types of students) take additional courses when they become available. Second, they may be ineffective even for the students who do take them. Examining the first stage effect of AP course offerings—that is, the effect on the number of AP courses and exams that students take—will shed light on these two channels.

Table 6 shows the first stage effect of AP course availability on the number of AP courses and exams that students take. The point estimates suggest that an additional AP course offering increases the number of AP courses the average student takes by 0.032, and has no detectable effect on the number of exams. To put these magnitudes in context, the average student in my sample takes 0.78 AP courses, so this represents an increase in course-taking of 4 percent. Put differently, the average senior class has around 300 students, so these numbers translate into between 9 and 10 additional students taking an AP course. This helps explain why the reduced form effects in Table 2 are close to zero and not statistically significant: even if AP courses improve outcomes for the students who take them, very few students take an additional AP course when it becomes available. Importantly, there is no effect on taking AP exams, which are necessary to receive college credit or placement. Note that the first-stage F statistics are small and below conventional thresholds, suggesting the instrument of course availability is not sufficiently strong to estimate effects using an IV model. For this reason (along with threats to the exclusion restriction), I do not report IV estimates.

Tables 7, 8, and 9 show first stage results estimated by family income, race, and prior academic achievement. The first stage estimates in Table 7 suggest that for higher-income students, an additional course offering leads to higher rates of course- and exam-taking (an additional 0.037 courses and 0.019 exams); however, for lower-income students, the first stage is essentially zero. Looking at the effects by race in Table 8, White and Asian students take 0.35 more AP courses and 0.14 more AP exams, on average. There is no first-stage effect on course-taking for Black, Hispanic, and Native students. Finally, Table 9 indicates that higher-achieving students are more likely to take advantage of additional AP courses. While an average student increases their course-taking by 0.022 courses, a student with a test score one standard deviation above average increases their course-taking by 0.046 courses more, or 0.069 courses. In terms of exam taking, there is no first stage effect on exams for average-performing students, but a significant effect for high-performing ones. I estimate that a student with a middle school math score one standard deviation above average increases their number of AP exams by 0.064—nearly the same increase as in course-taking.

Again, the first stage F statistics are small, so I do not report IV estimates for these subgroups. However, the first stage results are telling. First, they suggest that it is the already advantaged students who take additional AP courses when they become available, widening gaps in advanced course-taking. Furthermore, the largely null reduced-form results for nearly all subgroups and outcomes suggest that even those who do take newly available AP courses see little payoff, with the possible exception of the highest achievers.

6 Threats to Identification and Robustness Checks

Because I am not able to randomly assign schools to offer AP courses, I have to worry about whether my results are picking up a true causal effect or are driven by some spurious correlation. There are several main threats to identification. Perhaps both AP participation and gaps in college enrollment are growing over time, but the former is not causing the latter. My empirical strategy addresses this in several ways: first, I include year fixed effects to allow for a time trend in college outcomes. Second, as I show in Appendix Figure A5, while there is an upward trend in number of AP courses at most schools, it is by no means strictly monotonic, meaning I am identifying off of changes in AP offerings in both directions. A related issue of confounding endogeneity is that it is possible that longer-term, systematic changes to the student population and the demand for AP courses are occurring, and that these are correlated with student outcomes. This would be the case if, for example, schools offer more AP courses in order to attract higher-achieving students. I test for this type of endogeneity in two ways.

First, I re-estimate all reduced form and first-stage effects with additional controls for student- and school-level characteristics. Student characteristics include sex, race (indicators for White, Asian, Black, Hispanic, and Native), free or reduced-price lunch eligibility in 12th grade, and standardized score on the middle school math test. I also add time-varying school characteristics: average middle school math test score, school size, student-to-teacher ratio, per-student spending, and local unemployment; these are measured in the student's sophomore year so that they are unaffected by the treatment. The sample means of all additional control variables are reported in Table 1. Versions of Tables 2 through 9 estimated with additional student- and school-level controls are included as Appendix Tables A2 through A9. The results are nearly identical.

As a second robustness check, I directly test for positive selection of students into schools with more AP courses by estimating a version of Equation 1 where the left-hand-side variable is the average middle school test score of the senior class:

$$\text{(Average middle school test score)}_{jt} = \alpha + \sum_{k=-2}^{2} \beta_k AP_{j,t+k} + \delta_j + \lambda_t + \tau_j t + \varepsilon_{jt} \quad (4)$$

Note this is done at the school-year level. Positive β_k's, particularly for $k \leq 0$, wolud suggest that a stronger AP curriculum attracts higher-achieving students, and would cause me to worry that my findings are driven by students with better outcomes coming into schools with more AP rather than more AP causing improved outcomes. Figure 1 graphically depicts the estimated β_k coefficients. There is no evidence that higher-achieving students are positively selecting into schools with more AP courses.

6.1 Corrections for Two-Way Fixed Effects Estimates

Several recent papers have highlighted potential issues with linear regressions that estimate policy treatment effects using time and group fixed effects (two-way fixed effects, or TWFE), such as Equation 1 above (see de Chaisemartin and D'Haultfoeuille (forthcoming) and Roth et al. (2022) for reviews). TWFE approaches may, unless researchers are willing to make implausibly strong assumptions, produce estimates that are misleading or hard to interpret. The key problem in extending the canonical two-period difference-in-difference design with a binary treatment to an equation more like Equation 1 comes from what both sets of authors refer to as "forbidden comparisons." A treatment parameter from a TWFE model is a weighted average of all possible comparisons of groups experiencing different changes to the treatment, including comparing groups whose treatment (e.g., number of AP courses available) change more relative to those who change less. If the lower-treated group has a larger per-unit treatment effect, such comparisons can result in a negative effect, even if the effect of the treatment is positive for both groups.

de Chaisemartin and d'Haultfoeuille (2020) propose an alternative estimator, which they call DID_M, which eliminates "forbidden comparisons" and averages, across groups and time, all comparisons of groups whose treatment changes to groups whose treatment doesn't change. I re-estimate all reduced form effects and first stage effects of AP course availability (the treatment) using the DID_M approach, implemented with the did_multiplegt Stata command (de Chaisemartin et al., 2019).

The results of this alternative approach are included as Appendix Tables A10 through A13 (reduced form) and A14 through A17 (first stage). Because they leverage fewer comparisons, the DID_M estimates are substantially noisier than the TWFE estimates. However, the magnitudes lead to similar conclusions.

The estimates of DID_M provide no evidence that additional AP courses improve outcomes overall or for specific subgroups. The only significant positive result from the TWFE estimates—a small effect on college graduation for higher-achieving students—reverses sign and is not statistically significant when using the DID_M estimator. However, the DID_M parameter is less precisely estimated. Furthermore, the estimates in Table 5 and A13 are not directly comparable, since Table 5 comes from estimating a regression with an interaction term with continuous math test score and Table A13 comes from subsetting the sample by test score range.

Although all of the first stage results using the approach of de Chaisemartin and d'Haultfoeuille (2020) (Appendix Tables A14 through A17) are noisier than the TWFE estiamtes, the magnitudes are similar and again suggest that higher income, non-underrepresented minority, and higher-achieving students are more likely to take advantage of newly offered AP courses.

7 Conclusion

Using administrative data from the state of Michigan and exploiting within-school, across-time variation in AP course offerings, I have shown that introducing more AP courses fails to close gaps in access and outcomes. The most disadvantaged students do not take additional AP courses or exams when they become available; rather, their higher-income, higher-achieving, and White and Asian peers are the ones who take advantage.

This finding is consistent with work by historians, sociologists, and education researchers arguing that the Advanced Placement program, like many other examples of educational resources, benefits already privileged students and systematically excludes the already marginalized, thus perpetuating inequities (e.g., Schneider, 2009). For example, Rodriguez and McGuire (2019) use cross-sectional national data and instrument for AP availability with per-pupil school expenditures and find that when schools introduce additional AP courses, the Black-White gap in AP course-taking widens. They argue that their results imply opportunity hoarding by White students and families. Similarly, Solorzano and Ornelas (2002) show that Chicana and Latina students in one California district are underrepresented in AP courses, even in schools with strong AP programs. These studies, as well as the current analysis, suggest that without a concerted effort to ensure equal access for all students, expanding AP offerings will most likely only worsen educational inequality.

Even if students were granted truly equal access to AP courses, it is not obvious that college outcome gaps would close. I find very limited evidence that access to additional advanced courses improves college quality or post-secondary attainment. Although my primary results suggest that the most academically prepared students may benefit from AP, this finding doesn't hold under alternative estimation approaches. Even if there is a benefit for high-achieving students, it appears to be small, and would only serve to widen existing gaps.

Despite a push by some policymakers to use AP courses as a tool for combating inequality and improving college readiness, the current study complements recent research (Conger, Long, and McGhee, 2023) showing that expanding AP access is unlikely to do so, at least not without additional incentives or supports. In both my setting—Michigan schools making year-to-year adjustments in AP offerings—and that of Conger, Long, and McGhee (2023)—a national set of schools that had never offered AP science adding it to the curriculum—the program failed to improve the outcomes its proponents espouse.

The causal evidence on the AP program is not universally negative. Jackson (2010, 2014) found positive achievement and college completion effects of paying students to pass AP exams; Smith, Hurwitz, and Avery (2017) found that passing an AP exam improved on-time college graduation. However, the positive effects in these cases are tied to students taking and passing exams, not simply enrolling in AP courses. In settings where a school or student on the margin of offering or taking AP is relatively disadvantaged, most students are unlikely to benefit without an additional push to take and succeed on the exams.

The policy implications from the current as well as previous work are similar: putting financial and legal resources towards expanding AP access is, by itself, unlikely to achieve the goal of closing gaps in educational outcomes. If educators and policymakers strive to address educational inequality, additional resources focused on AP exams are likely necessary, and may be best targeted more explicitly at disadvantaged students.

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Table 1: Sample Descriptive Statistics

	Mean	Std dev	Min	Max	N non-missing
Student level characteristics					
Female	0.51	0.50	0.00	1.00	174,395
White	0.74	0.44	0.00	1.00	174,469
Asian	0.04	0.20	0.00	1.00	174,469
Black	0.17	0.38	0.00	1.00	174,469
Hispanic	0.03	0.18	0.00	1.00	174,469
Native	0.01	0.09	0.00	1.00	174,469
Eligible for free or reduced-price lunch	0.24	0.43	0.00	1.00	174,469
Middle school math test score (std.)	0.26	1.01	-6.36	7.19	155,377
AP courses available junior & senior year	9.79	4.58	0.00	20.00	174,469
AP courses taken junior & senior year	0.78	1.37	0.00	11.00	174,469
AP tests taken	0.73	1.59	0.00	23.00	136,285
School-cohort level characteristics					
Average middle school math test score	0.08	0.43	-1.04	1.15	687
School enrollment	1377	500	0	2519	689
Pupil-to-teacher ratio	21.41	10.97	0.00	245.50	686
Per pupil instructional spending	6360	1783	3477	43080	686
Local unemployment rate	8.87	4.61	1.74	25.50	689
AP courses available year t and $t-1$	8.56	4.64	0.00	20.00	689

Notes: "Native" includes American Indian, Alaska Native, Native Hawaiian, and other Pacific Islander students. Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. School-year characteristics are all measured in year $t-2$, except for AP course availability. AP course availability is measured as the number of unique AP subjects offered over two years; if a subject is offered both years, it is counted once.

Table 2: Reduced Form Effect of AP Course Availability on College Outcomes

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
# of AP courses available at school in junior and senior year	0.002	0.001	0.001
	(0.002)	(0.001)	(0.002)
Mean of outcome variable	[0.391]	[0.156]	[0.318]
Observations	174,469	174,469	174,469
Cohorts	2005-2012	2005-2012	2005-2012

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Table reports estimate of Equation 1. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses.

Table 3: Reduced Form Effect of AP Course Availability on College Outcomes, by Family Income

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
Effect of one additional available AP course for:			
Low-income students	0.002	0.000	0.000
	(0.003)	(0.001)	(0.002)
	[0.225]	[0.045]	[0.130]
Observations		42,628	
Non-low-income students	0.001	0.001	0.000
	(0.002)	(0.001)	(0.002)
	[0.444]	[0.193]	[0.379]
Observations		131,841	

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Low-income status is proxied by eligibility for free or reduced-price lunch (FRPL). Effects by income are estimated with separate estimations of Equation 1 by FRPL status. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses. Means of the outcome variables are in brackets.

Table 4: Reduced Form Effect of AP Course Availability on College Outcomes, by Race and Ethnicity

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
Effect of one additional available AP course for:			
Black, Hispanic, & Native students	0.005	0.001	0.004
	(0.004)	(0.002)	(0.003)
	[0.281]	[0.054]	[0.145]
Observations		37,514	
White & Asian students	0.001	0.001	-0.001
	(0.002)	(0.001)	(0.002)
	[0.421]	[0.185]	[0.365]
Observations		136,955	

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Effects by race are estimated with separate estimations of Equation 1 by underrepresented minority status. Underrepresented minority includes students who identify as Black, Hispanic, American Indian, Native Hawaiian, or Pacific Islander. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses. Means of the outcome variables are in brackets.

Table 5: Reduced Form Effect of AP Course Availability on College Outcomes, by Academic Preparation

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
# AP courses available at school	0.003	-0.001	-0.000
in junior & senior year	(0.002)	(0.001)	(0.001)
Middle school math test score	0.190***	0.047***	0.133***
	(0.009)	(0.007)	(0.011)
# of AP courses available × math score	-0.001	0.007***	0.003***
	(0.001)	(0.001)	(0.001)
Observations	155,377	155,377	155,377
Cohorts	2005-2012	2005-2012	2005-2012
Effect for students with average math score	0.003	-0.001	-0.000
Outcome mean, math score in (-0.25, 0.25)	[0.344]	[0.095]	[0.262]
Effect for students 1 sd above average	0.002	0.006***	0.003*
Outcome mean, math score in (0.75, 1.25)	[0.588]	[0.257]	[0.491]

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. Students missing a test score are not included in this analysis. Effects by academic preparation are estimated using a single equation (Equation 2), where course availability is interacted with the continuous measure of test score. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses.

Table 6: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
# of AP courses available at school	0.032***	0.012
in junior and senior year	(0.010)	(0.008)
Mean of outcome variable	[0.780]	[0.731]
Kleibergen-Paap Wald F statistic	10.99	2.39
Observations	174,469	136,285
Cohorts	2005-2012	2007-2012

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Table reports estimate of Equation 3. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses.

Table 7: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Family Income

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Low-income students	0.010	-0.008
	(0.007)	(0.007)
Group mean	[0.359]	[0.292]
Observations	42,628	35,904
Non-low-income students	0.037***	0.019**
	(0.011)	(0.009)
Group mean	[0.916]	[0.888]
Observations	131,841	100,381
Cohorts	2005-2012	2007-2012

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Low-income status is proxied by eligibility for free or reduced-price lunch (FRPL). Effects by income are estimated with separate estimations of Equation 3 by FRPL status. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses. Means of the course- and exam-taking variables are in brackets.

Table 8: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Race and Ethnicity

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Black, Hispanic, & Native students	0.013	0.003
	(0.010)	(0.008)
Group mean	[0.357]	[0.245]
Observations	37,514	29,414
White & Asian students	0.035***	0.014*
	(0.011)	(0.009)
Group mean	[0.896]	[0.865]
Observations	136,955	106,871
Cohorts	2005-2012	2007-2012

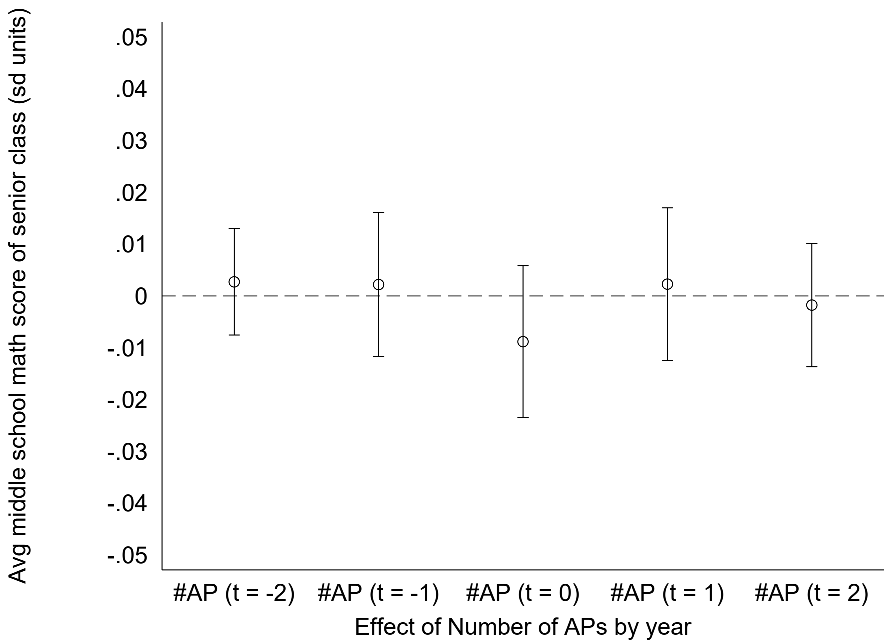
Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Effects by race are estimated with separate estimations of Equation 3 by underrepresented minority status. Underrepresented minority includes students who identify as Black, Hispanic, American Indian, Native Hawaiian, or Pacific Islander. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses. Means of the course- and exam-taking variables are in brackets.

Table 9: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Prior Achievement

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
# AP courses available at school	0.022*	-0.005
in junior & senior year	(0.011)	(0.009)
Middle school math test score	0.220***	0.049
	(0.055)	(0.069)
# of AP courses available * math score	0.046***	0.070***
	(0.005)	(0.007)
Observations	155,377	123,669
Cohorts	2005-2012	2007-2012
Effect for students with average math score	0.022*	-0.005
Outcome mean, math score in (-0.25, 0.25)	[0.424]	[0.303]
Effect for students 1 sd above average	0.069***	0.064***
Outcome mean, math score in (0.75, 1.25)	[1.239]	[1.129]

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. Students missing a test score are not included in this analysis. Effects by academic preparation are estimated using a single equation, where course availability is interacted with the continuous measure of test score. Regressions include school fixed effects, year fixed effects, and school-specific linear time trends. Robust standard errors clustered at the school level in parentheses. Means of the course- and exam-taking variables are in brackets.

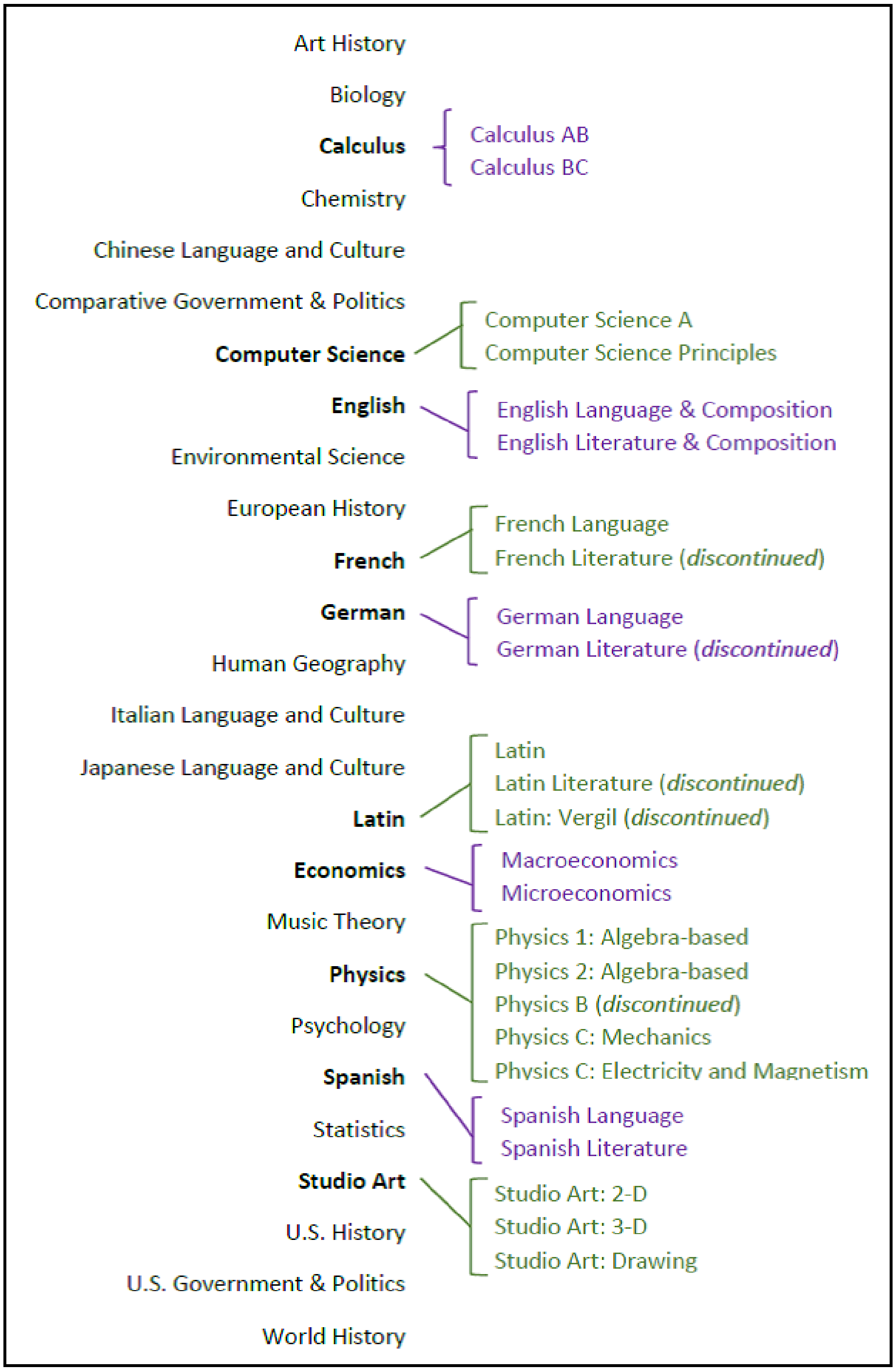
Figure 1: Test for Selection: Effect of Number of AP Courses Available on Average Middle School Math Test Scores of Senior Class



Notes: Figure shows estimated coefficients and 95 percent confidence intervals for the β_k's in Equation 4, which is a school-year-level regression of average standardized middle school math test score of the high school's senior class on the number of AP courses offered every year. Regressions control for school and year fixed effects and school-specific linear time trends.

Appendix Tables and Figures

Figure A1: Coding of AP Courses by Subject



Notes: Subjects in bold have been collapsed from multiple AP subjects, corresponding to the bracketed courses, due to data limitations. This was done because in many cases it was impossible to distinguish, e.g., English Literature and Composition from English Language and Composition (because the school would list the course as "AP English.")

Table A1: Most Popular AP Courses Offered, Courses Taken, and Exams Taken, by Cohort

	Top 5 AP courses offered (school level)	Top 5 AP courses taken (student level)	Top 5 AP exams taken (student level)
2005	English	English	
	U.S. History	Calculus	
	Calculus	U.S. Government	
	Biology	Biology	
	Chemistry	U.S. History	
2006	English	English	
	Calculus	Calculus	
	U.S. History	U.S. Government	
	Chemistry	Biology	
	Biology	U.S. History	
2007	English	English	English
	Calculus	Calculus	Calculus
	U.S. History	U.S. Government	U.S. Government
	Chemistry	Biology	Biology
	Biology	Psychology	Psychology
2008	English	English	English
	Calculus	Calculus	Calculus
	U.S. History	U.S. Government	U.S. History
	Biology	Biology	Biology
	U.S. Government	Psychology	U.S. Government
2009	English	English	English
	Calculus	Calculus	Calculus
	U.S. History	U.S. Government	U.S. History
	Biology	Biology	Biology
	U.S. Government	Psychology	U.S. Government
2010	English	English	English
	Calculus	Calculus	Calculus
	U.S. History	U.S. Government	U.S. History
	Biology	Biology	U.S. Government
	U.S. Government	Psychology	Biology
2011	English	English	English
	Calculus	Calculus	Calculus
	U.S. History	U.S. Government	U.S. History
	Biology	Psychology	U.S. Government
	U.S. Government	Biology	Biology
2012	English	English	English
	Calculus	Calculus	Calculus
	Biology	Psychology	Psychology
	U.S. History	U.S. Government	U.S. History
	Chemistry	Biology	U.S. Government

Figure A2: Proportion of Schools Offering Any AP Courses, by Cohort

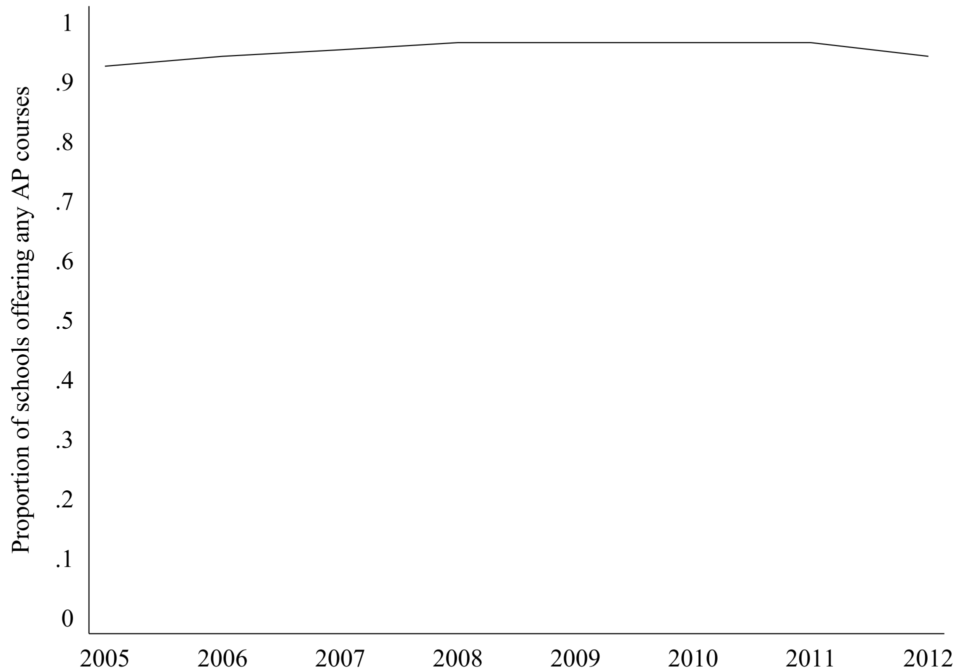


Figure A3: Average Number of AP Courses Offered by School, by Cohort

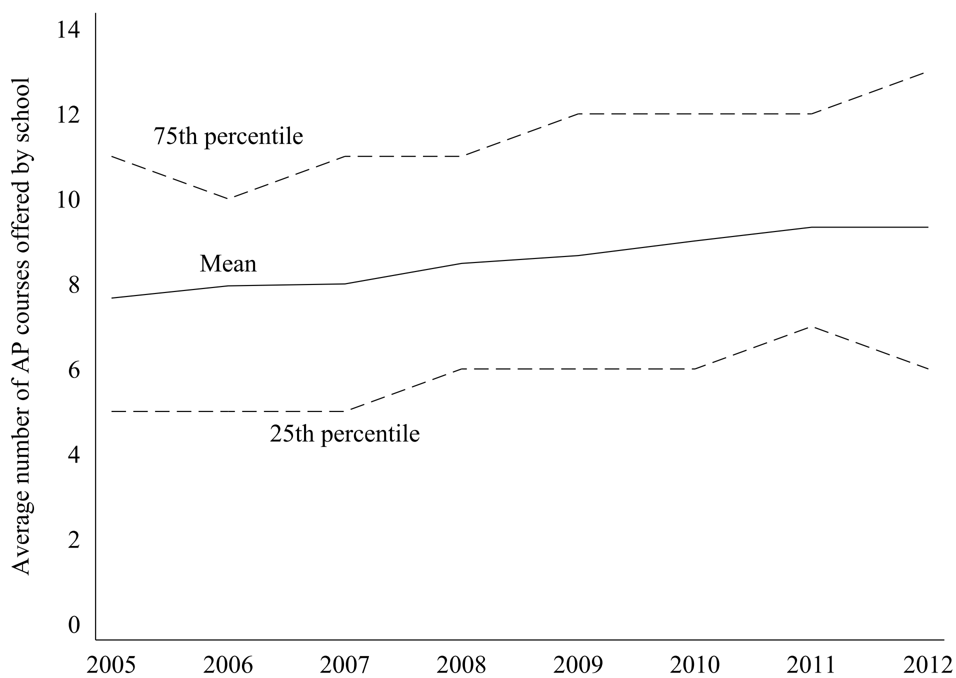


Figure A4: Distribution of Number of AP Courses Offered at a School, by Cohort

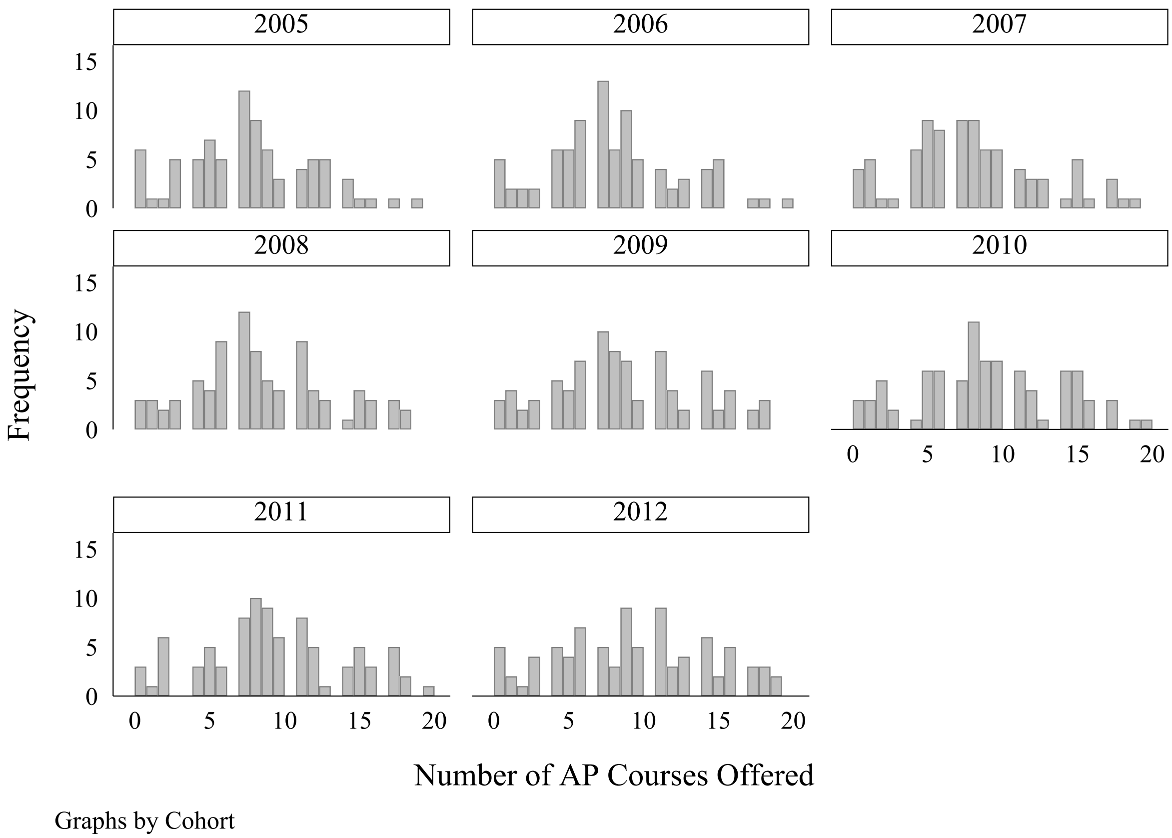


Figure A5: School-by-Cohort Variation in Number of AP Courses Offered

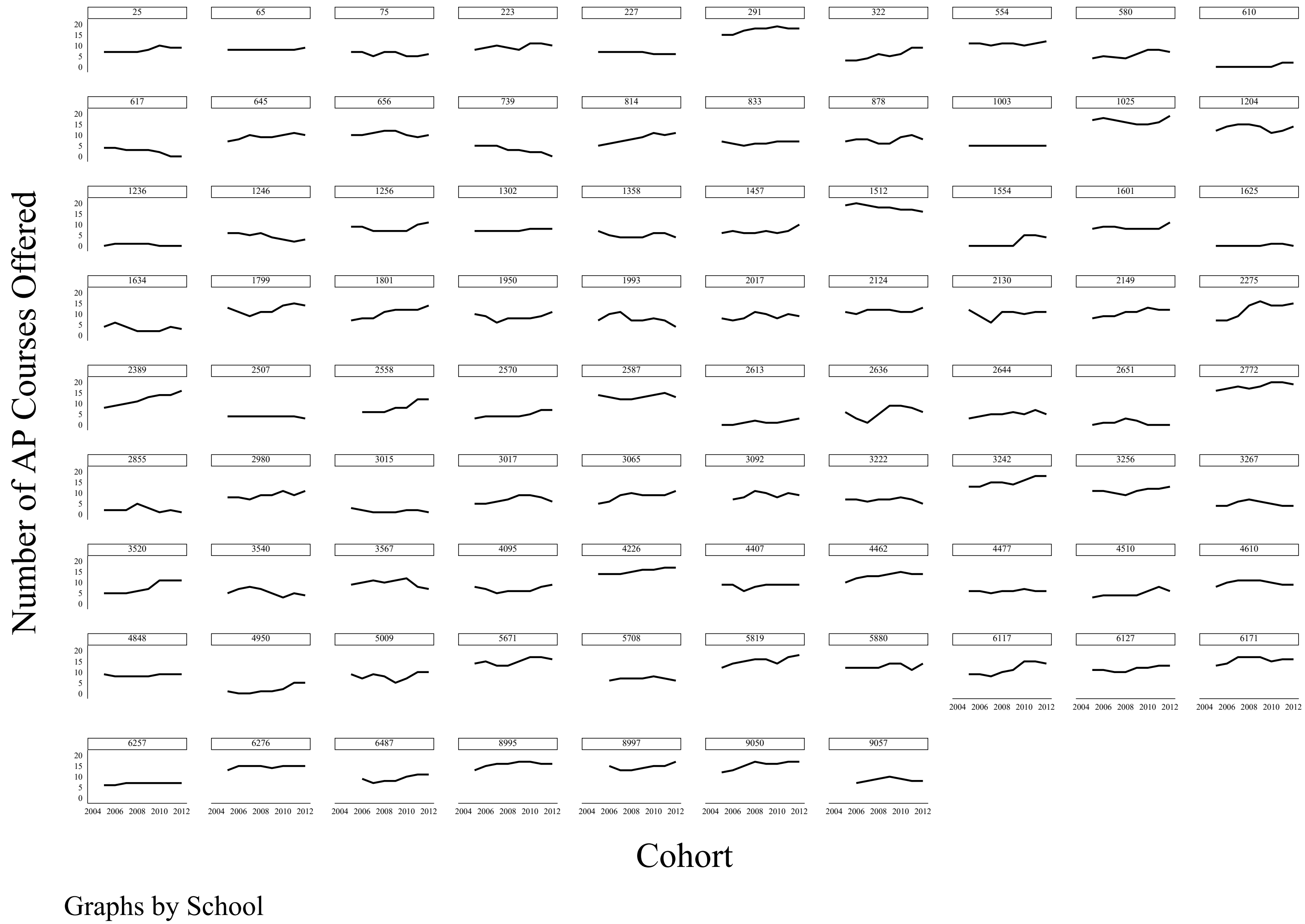


Figure A6: Proportion of Students Taking Any AP Courses, by Cohort and Family Income

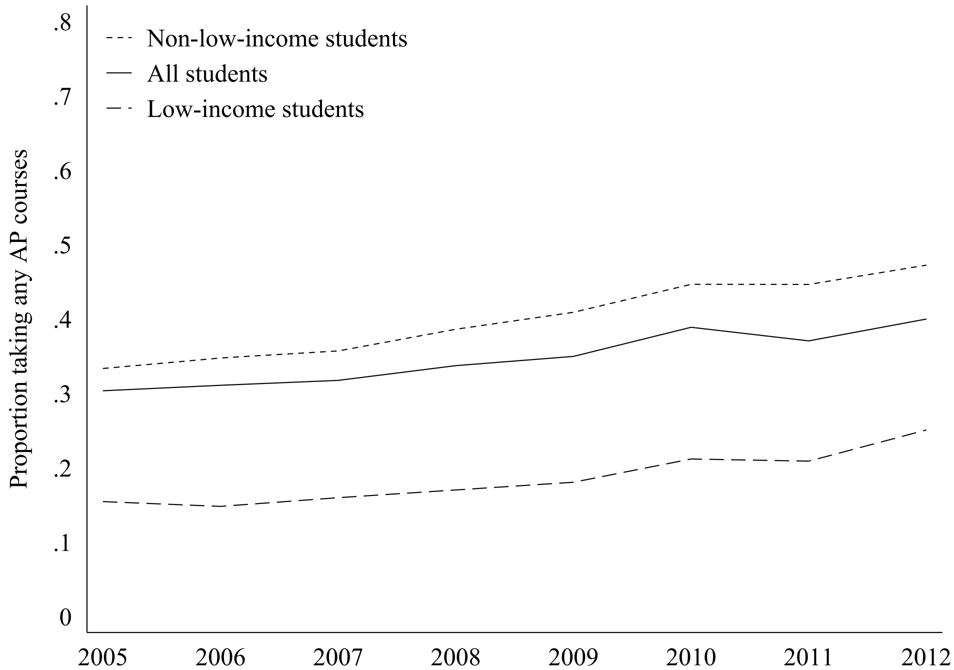


Figure A7: Average Number of AP Courses Taken by Students, Conditional on Taking Any AP, by Cohort and Family Income

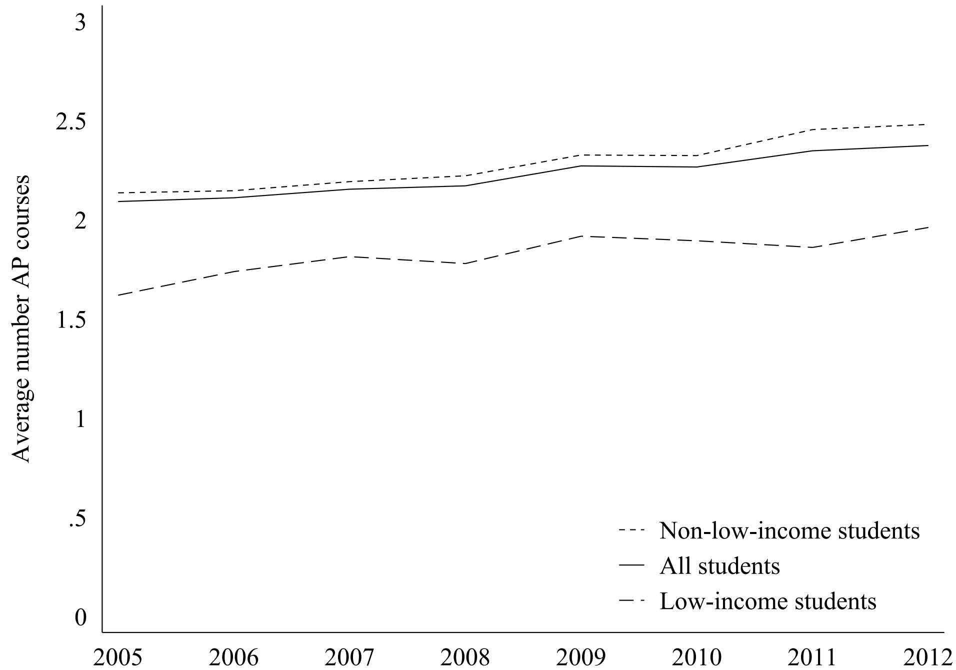


Figure A8: Proportion of Students Taking Any AP Exams, by Cohort and Family Income

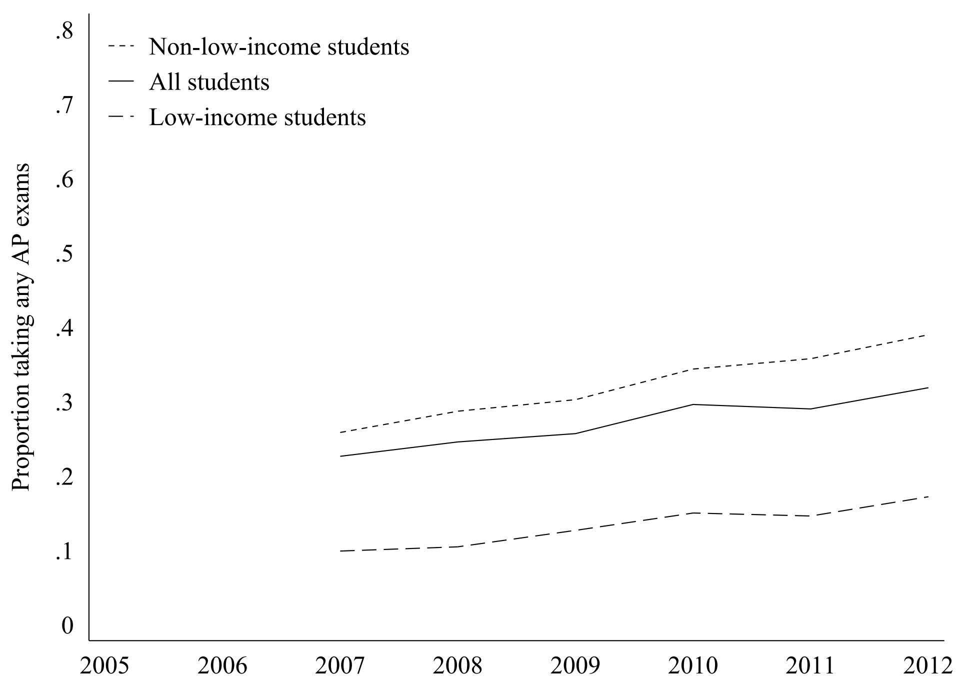


Figure A9: Proportion of Students Taking Any AP Exams, Conditional on Taking Any AP Course, by Cohort and Family Income

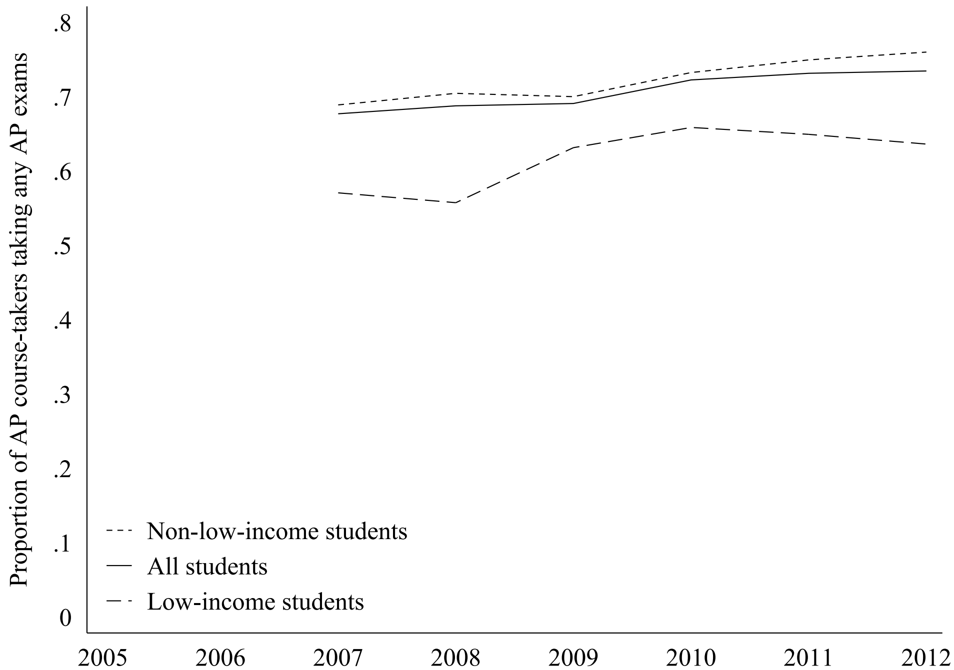


Figure A10: Average Number of AP Exams Taken by Students, Conditional on Taking Any AP Course, by Cohort and Family Income

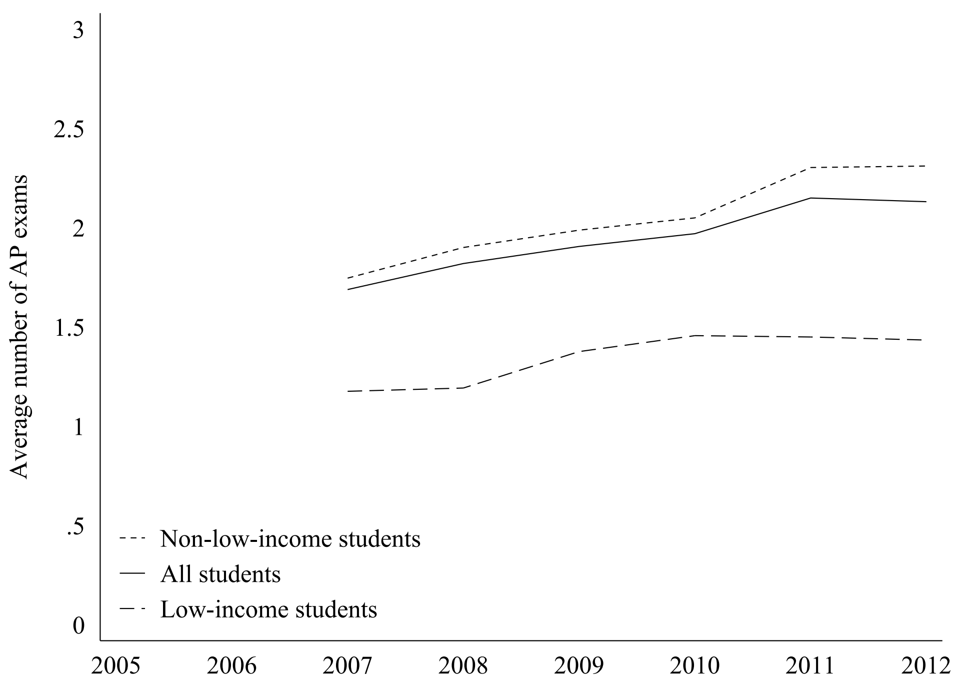


Table A2: Reduced Form Effect of AP Course Availability on College Outcomes, with Additional Student and School Controls

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
# of AP courses available at school in junior and senior year	0.002 (0.002)	0.001 (0.001)	0.000 (0.001)
Mean of outcome variable	[0.391]	[0.156]	[0.318]
Observations	174,469	174,469	174,469
Cohorts	2005-2012	2005-2012	2005-2012

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Table reports estimate of Equation 1. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (race, gender, free or reduced price lunch status, and middle school standardized math test score), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses.

Table A3: Reduced Form Effect of AP Course Availability on College Outcomes, by Family Income, with Additional Student and School Controls

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
Effect of one additional available AP course for:			
Low-income students	0.001	0.000	-0.000
	(0.003)	(0.001)	(0.002)
	[0.225]	[0.045]	[0.130]
Observations		42,628	
Non-low-income students	0.002	0.001	-0.000
	(0.002)	(0.001)	(0.002)
	[0.444]	[0.193]	[0.379]
Observations		131,841	

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Low-income status is proxied by eligibility for free or reduced-price lunch (FRPL). Effects by income are estimated with separate estimations of Equation 1 by FRPL status. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (race, gender, and middle school standardized math test score), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses. Means of the outcome variables are in brackets.

Table A4: Reduced Form Effect of AP Course Availability on College Outcomes, by Race and Ethnicity, with Additional Student and School Controls

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
Effect of one additional available AP course for:			
Black, Hispanic, & Native students	0.003	0.001	0.003
	(0.003)	(0.001)	(0.002)
	[0.281]	[0.054]	[0.145]
Observations		37,514	
White & Asian students	0.001	0.001	-0.001
	(0.002)	(0.001)	(0.002)
	[0.421]	[0.185]	[0.365]
Observations		136,955	

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Effects by race are estimated with separate estimations of Equation 1 by underrepresented minority status. Underrepresented minority includes students who identify as Black, Hispanic, American Indian, Native Hawaiian, or Pacific Islander. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (gender, free or reduced price lunch status, and middle school standardized math test score), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses. Means of the outcome variables are in brackets.

Table A5: Reduced Form Effect of AP Course Availability on College Outcomes, by Academic Preparation, with Additional Student and School Controls

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
# AP courses available at school in junior & senior year	0.002	-0.001	-0.001
	(0.002)	(0.001)	(0.001)
Middle school math test score	0.189***	0.047***	0.130***
	(0.009)	(0.008)	(0.011)
# of AP courses available * math score	-0.001	0.006***	0.002***
	(0.001)	(0.001)	(0.001)
Observations	155,377	155,377	155,377
Cohorts	2005-2012	2005-2012	2005-2012
Effect for students with average math score	0.002	-0.001	-0.001
Outcome mean, math score in (-0.25, 0.25)	[0.344]	[0.095]	[0.262]
Effect for students 1 sd above average	0.001	0.005***	0.002
Outcome mean, math score in (0.75, 1.25)	[0.588]	[0.257]	[0.491]

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. Students missing a test score are not included in this analysis. Effects by academic preparation are estimated using a single equation (Equation 2), where course availability is interacted with the continuous measure of test score. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (race, gender, and free or reduced price lunch status), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses.

Table A6: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, with Additional Student and School Controls

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
# of AP courses available at school in junior and senior year	0.031***	0.013
	(0.010)	(0.008)
Mean of outcome variable	[0.780]	[0.731]
Kleibergen-Paap Wald F statistic	9.28	2.59
Observations	174,469	136,285
Cohorts	2005-2012	2007-2012

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Table reports estimate of Equation 3. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (race, gender, free or reduced price lunch status, and middle school standardized math test score), and time-varying school-level controls (average middle school test score, urbanicity, size of senior class, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses.

Table A7: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Family Income, with Additional Student and School Controls

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Low-income students	0.010	-0.009
	(0.007)	(0.008)
Group mean	[0.359]	[0.292]
Observations	42,628	35,904
Non-low-income students	0.038***	0.021**
	(0.012)	(0.009)
Group mean	[0.916]	[0.888]
Observations	131,841	100,381
Cohorts	2005-2012	2007-2012

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Low-income status is proxied by eligibility for free or reduced-price lunch (FRPL). Effects by income are estimated with separate estimations of Equation 3 by FRPL status. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (race, gender, and middle school standardized math test score), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses. Means of the course- and exam-taking variables are in brackets.

Table A8: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Race and Ethnicity, with Additional Student and School Controls

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Black, Hispanic, & Native students	0.011	-0.003
	(0.009)	(0.006)
Group mean	[0.357]	[0.245]
Observations	37,514	29,414
White & Asian students	0.035***	0.018**
	(0.012)	(0.009)
Group mean	[0.896]	[0.865]
Observations	136,955	106,871
Cohorts	2005-2012	2007-2012

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Effects by race are estimated with separate estimations of Equation 3 by underrepresented minority status. Underrepresented minority includes students who identify as Black, Hispanic, American Indian, Native Hawaiian, or Pacific Islander. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (gender, free or reduced price lunch status, and middle school standardized math test score), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses. Means of the course- and exam-taking variables are in brackets.

Table A9: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Prior Achievement, with Additional Student and School Controls

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
# AP courses available at school	0.022*	-0.005
in junior & senior year	(0.011)	(0.008)
Middle school math test score	0.240***	0.078
	(0.055)	(0.067)
# of AP courses available * math score	0.043***	0.065***
	(0.005)	(0.006)
Observations	155,377	123,669
Cohorts	2005-2012	2007-2012
Effect for students with average math score	0.022*	-0.005
Outcome mean, math score in (-0.25, 0.25)	[0.424]	[0.303]
Effect for students 1 sd above average	0.065***	0.060***
Outcome mean, math score in (0.75, 1.25)	[1.239]	[1.129]

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. Students missing a test score are not included in this analysis. Effects by academic preparation are estimated using a single equation, where course availability is interacted with the continuous measure of test score. Regressions include school fixed effects, year fixed effects, school-specific linear time trends, student-level controls (race, gender, and free or reduced price lunch status), and time-varying school-level controls (average middle school test score, school enrollment, pupil:teacher ratio, per student instructional spending, and local unemployment, all measured in the student's sophomore year). Robust standard errors clustered at the school level in parentheses. Means of the course- and exam-taking variables are in brackets.

Table A10: Reduced Form Effect of AP Course Availability on College Outcomes, DID_M Alternative to TWFE Estimates

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
# of AP courses available at school in junior and senior year	0.001	-0.003	-0.002
	(0.007)	(0.003)	(0.004)
Mean of outcome variable	[0.391]	[0.156]	[0.318]
Observations	125,957	125,957	125,957
Cohorts		2005-2012	

Notes: Table reports estimates of DID_M based on de Chaisemartin and d'Haultfoeuille (2020). Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses.

Table A11: Reduced Form Effect of AP Course Availability on College Outcomes, by Family Income, DID_M Alternative to TWFE Estimates

Effect of one additional available AP course for:			
Low-income students	0.013	-0.003	-0.007
	(0.012)	(0.004)	(0.006)
	[0.225]	[0.045]	[0.130]
Observations		32,999	
Non-low-income students	-0.001	-0.004	-0.003
	(0.007)	(0.003)	(0.005)
	[0.444]	[0.193]	[0.379]
Observations		90,681	

Notes: Table reports estimates of DID_M, separately by income status, based on de Chaisemartin and d'Haultfoeuille (2020). Low-income status is proxied by eligibility for free or reduced-price lunch (FRPL). Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses. Group mean of outcome variable in square brackets.

Table A12: Reduced Form Effect of AP Course Availability on College Outcomes, by Race and Ethnicity, DID_M Alternative to TWFE Estimates

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
Effect of one additional available AP course for:			
Black, Hispanic, & Native students	-0.015	0.003	0.003
	(0.019)	(0.006)	(0.011)
	[0.281]	[0.054]	[0.145]
Observations		26,694	
White & Asian students	0.001	-0.003	-0.002
	(0.008)	(0.004)	(0.006)
	[0.421]	[0.185]	[0.365]
Observations		97,966	

Notes: Table reports estimates of DID_M, separately by underrepresented minority status, based on de Chaisemartin and d'Haultfoeuille (2020). Underrepresented minority includes students who identify as Black, Hispanic, American Indian, Native Hawaiian, or Pacific Islander. Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses. Group mean of outcome variable in square brackets.

Table A13: Reduced Form Effect of AP Course Availability on College Outcomes, by Academic Preparation, DID_M Alternative to TWFE Estimates

	(1) Enrolled in competitive+ college	(2) Earned BA degree in 4 years	(3) Earned BA degree in 6 years
Effect of one additional available AP course for:			
Students with math score in (-0.25, 0.25)	-0.001	0.002	-0.003
	(0.008)	(0.004)	(0.008)
	[0.344]	[0.095]	[0.262]
Observations		22,724	
Students with math score in (0.75, 1.25)	0.012	-0.005	-0.006
	(0.010)	(0.009)	(0.011)
	[0.588]	[0.257]	[0.491]
Observations		13,994	

Notes: Table reports estimates of DID_M, separately by test score range, based on de Chaisemartin and d'Haultfoeuille (2020). Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. Students missing a test score are not included in this analysis. Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses. Group mean of outcome variable in square brackets.

Table A14: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, DID_M Alternative to TWFE Estimates

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
# of AP courses available at school in junior and senior year	0.021	0.004
	(0.016)	(0.016)
Mean of outcome variable	[0.780]	[0.731]
Observations	125,957	91,693
Cohorts	2005-2012	2007-2012

Notes: Table reports estimates of DID_M based on de Chaisemartin and d'Haultfoeuille (2020). Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses.

Table A15: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Family Income, DID_M Alternative to TWFE Estimates

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Low-income students	0.006	-0.008
	(0.017)	(0.017)
Group mean	[0.359]	[0.292]
Observations	32,999	26,210
Non-low-income students	0.029	0.012
	(0.020)	(0.023)
Group mean	[0.916]	[0.888]
Observations	90,681	65,481
Cohorts	2005-2012	2007-2012

Notes: Table reports estimates of DID_M, separately by income status, based on de Chaisemartin and d'Haultfoeuille (2020). Low-income status is proxied by eligibility for free or reduced-price lunch (FRPL). Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses. Group mean of outcome variable in square brackets.

Table A16: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Race and Ethnicity, DID_M Alternative to TWFE Estimates

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Black, Hispanic, & Native students	-0.011	0.002
	(0.043)	(0.023)
Group mean	[0.357]	[0.245]
Observations	26,694	20,019
White & Asian students	0.034	0.007
	(0.024)	(0.023)
Group mean	[0.896]	[0.865]
Observations	97,966	71,481
Cohorts	2005-2012	2007-2012

Notes: Table reports estimates of DID_M, separately by underrepresented minority status, based on de Chaisemartin and d'Haultfoeuille (2020). Underrepresented minority includes students who identify as Black, Hispanic, American Indian, Native Hawaiian, or Pacific Islander. Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses. Group mean of outcome variable in square brackets.

Table A17: First Stage Effect of AP Course Availability on AP Course- and Exam-Taking, by Academic Preparation, DID_M Alternative to TWFE Estimates

	(1) First stage: # AP courses taken	(2) First stage: # AP exams taken
Effect of one additional available AP course for:		
Students with math score in (-0.25, 0.25)	0.009	0.004
	(0.019)	(0.017)
Group mean	[0.424]	[0.303]
Observations	22,724	17,296
Students with math score in (0.75, 1.25)	0.052	0.031
	(0.043)	(0.043)
Group mean	[1.239]	[1.129]
Observations	13,994	10,712
Cohorts	2005-2012	2007-2012

Notes: Table reports estimates of DID_M, separately by test score range, based on de Chaisemartin and d'Haultfoeuille (2020). Middle school math test score is measured as a standardized scale score. I use eighth grade test score if available and seventh grade score if not. Students missing a test score are not included in this analysis. Estimates include school fixed effects, year fixed effects, and school-specific linear time trends. Bootstrapped standard errors clustered at the school level in parentheses. Group mean of outcome variable in square brackets.