# High School Academic Acceleration and Early Postsecondary Outcomes: Evidence from Washington State

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## Abstract

Taking advanced courses in high school predicts positive later outcomes, yet many seemingly prepared students do not take such courses. Acceleration policies that automatically enroll high achieving high school students into more advanced courses have been shown to increase advanced coursetaking, but little is known about the longer-term impacts of these policies. We studied districts in Washington that implemented Academic Acceleration policies, which identified proficient 11th and 12th grade students for automatic enrollment into advanced and dual credit courses. Using a difference-in-differences design, we examined the effects of Academic Acceleration on college enrollment and early postsecondary coursetaking. We found no significant effects of the policy on students' college enrollment, first-year college coursetaking, or persistence to a second year of college.

## 1. Introduction

Academic acceleration policies that automatically enroll high achieving students into more advanced courses have become more widespread in recent years. Several states including Indiana, Kentucky, Maryland, Oklahoma, Texas, Virginia, and Washington have passed statewide policies that use universal screening (typically a state assessment) to review all students' readiness for advanced coursetaking (Kaput, 2021; Plucker et al., 2024; Stand for Children, 2019). Per the policies, students' performance on screeners is measured against an objective standard to default students who meet or exceed the standard into advanced courses, making the courses opt-out, rather than opt-in. These mechanisms are designed to reduce or remove barriers to advanced course participation that often leave out many students who are prepared for more advanced course material, and similar mechanisms have also been used to identify students for gifted and talented programs and for English learner reclassification (Austin et al., 2025; Bartlett et al., 2026; Card & Guiliano, 2016; Grissom & Redding, 2016, 2019).[1]

Washington state's Academic Acceleration policy was initially implemented district-by-district, and these district-level policies were among the first automatic enrollment policies in the U.S. Like other automatic enrollment policies, Academic Acceleration in Washington uses universal screening and opt-out mechanisms to increase participation in advanced courses. All high school students' scores on statewide standardized assessments are screened against the state standard in English language arts (ELA) and math, and students who meet or exceed the standard by scoring "proficient" or higher are identified as eligible for an advanced course in the relevant subject(s). Students with proficient or higher test scores are then automatically enrolled into the

[1] This targeted approach to acceleration contrasts with earlier policy reforms, such as those in Chicago and California, that sought to accelerate middle math course taking for all students and had generally negative effects on students' math achievement and subsequent course taking (Allensworth et al., 2009; Clotfelter, Ladd, & Vigdor, 2015; Nomi, 2012; Domina et al., 2015).

advanced course (e.g., dual credit English instead of regular English, AP Pre-Calculus instead of regular Pre-Calculus). Students receive information about their course assignment and the benefits of advanced courses and are given the opportunity to opt-out.

A growing set of causal studies have identified positive effects of the newer, targeted automatic enrollment policies, as well as interventions using similar mechanisms, on students' middle school math acceleration and their high school advanced course planning and enrollment (e.g., Austin et al., 2025, 2026; Dougherty et al., 2015, 2017; Vargas Castaño et al., 2025). In North Carolina, automatic acceleration of middle schoolers who were identified using universal screening as having at least a 70% probability of passing their Grade 8 standardized algebra assessment had positive effects on students' later math course taking (Dougherty et al., 2015). However, the number of students remaining on the accelerated track decreased substantially each year, with low-income students least likely to persist (Dougherty et al., 2017). This attrition is consistent with other findings on the long-term effects of algebra acceleration (Clotfelter et al., 2015; Liang et al., 2012). In Texas, an academic acceleration policy making advanced middle school math opt-out increased participation in eighth-grade algebra by 11.7 percentage points (Vargas Castaño et al., 2025).

Only one study to date has examined the effects of academic acceleration later in high school: in Washington state, Academic Acceleration policies for students entering Grades 11 and 12 significantly increased any advanced course taking, by about 5 percentage points among students with proficient test scores who were the focus of the policy (Austin et al., 2025). Increases in advanced coursetaking were larger for proficient students from racial/ethnic groups with disproportionately low participation in advanced courses (significant increases of 8 percentage points in any advanced course and 5 percentage points in advanced math enrollment), and students from low-income backgrounds (significant increases of 8 percentage points in any advanced course, 5 percentage points in math, and 6 percentage points in ELA or social studies enrollments) (Austin et al., 2025).

No studies have yet examined the effects of automatic enrollment on postsecondary outcomes. Building on prior work examining the effects of automatic enrollment for students in Grades 11 and 12 (Austin et al., 2025), we examine whether the policy's effects on advanced course participation in Grades 11 and 12 translated into effects on postsecondary enrollment and success. Academic Acceleration places students with proficient test scores into advanced courses that align with their high school and beyond plan and is intended to lead to completion of courses that provide dual credit for high school and college (Revised Code of Washington, 2019). A number of studies have demonstrated a positive association between taking advanced and dual credit later in high school and college outcomes (e.g., Attewell & Domina, 2008; Austin, 2020; Byun et al., 2014; Jackson, 2014; Long et al., 2012).

Most prior work generally suggests that effects on college outcomes are positive but smaller than effects on high school outcomes. For example, Jackson's (2014) study of the Advanced Placement (AP) Incentive Program found statistically significant effects of about 16 percentage points on students' high school AP coursetaking, but only marginally significant effects of 2 to 3 percentage points on college enrollment. Based on Jackson's findings, we might expect effects of up to 1 to 2 percentage points on college enrollment (i.e., one-eighth to one-fifth the effect on coursetaking), and consistent with these prior studies (e.g., Jackson, 2014; Conger, Kennedy, et al., 2021; Conger et al., 2023), would not expect postsecondary effects larger in magnitude than high school effects. However, some work has shown no association between AP coursetaking and first-year college grades or persistence (Klopfenstein & Thomas,

2009) and at least one recent study found some negative effects on confidence in college success (Conger et al., 2021). The policy's focus on preparing students for post-high school success and the need for more evidence makes it important to examine the extent to which the policy had effects on postsecondary outcomes.

To examine postsecondary effects of Academic Acceleration, we used student-level administrative data from Washington state that included demographic characteristics, standardized test scores, and postsecondary outcome measures. We implemented a staggered difference-in-differences (DID) design to estimate postsecondary enrollment, first-year coursetaking outcomes, and persistence to a second year of college for students in 72 districts that began implementing an Academic Acceleration policy in one of three years: 2014–15, 2015–16, and 2016–17. We examined each outcome for students who achieved a proficient or higher test score in either subject. We also examined each outcome for proficient students who were members of groups with disproportionately low participation in advanced courses: students from low-income backgrounds, measured by their eligibility for free or reduced-price lunch (hereafter, FRL), and students who identify as Black, Hispanic or Latino, Native American/Alaska Native, Native Hawaiian/Pacific Islander, or two or more races (hereafter, URM for underrepresented minoritized students).

We found that Academic Acceleration had no statistically significant effects on proficient students' probability of college enrollment or their probability of persistence to a second year. The only significant effect of the policy was a 2 percentage point decline in taking at least one upper-level (200-level or higher courses typically taken in the second year of college or later) course in the first year of college. We also found some evidence of a shift away from 2-year college enrollment toward enrollment in 4-year colleges for proficient students eligible for FRL relative to proficient students not eligible for FRL.

## 2. Washington Context

Between 2013–14 and 2015–16, 72 of Washington's 263 districts with at least one high school were early adopters of an Academic Acceleration policy, encouraged by a 2013 state law that provided supports for doing so.[2] In 2019, the state legislature updated its policy to require all remaining districts adopt Academic Acceleration by the 2021–22 school year.[3]

In the early adopting districts, students who met the state standard (described below) on both the reading and writing state standardized assessments were eligible for automatic enrollment into advanced ELA and/or social studies courses. Students who met the standard on the Algebra I and Geometry end-of-course assessments were eligible for automatic enrollment into advanced math courses.[4] To meet the state standard, students had to achieve scale scores that represented either Level 3 ("proficient") or Level 4 ("advanced") achievement.[5] Students typically took the relevant assessments in 10th or 11th grade, so students with a proficient test score were automatically enrolled in an advanced course beginning in 11th or 12th grade.[6,7] In

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[2] To our knowledge, policy adoption decisions were made by school boards in each district.
[3] The 2019 policy update also allowed for students to qualify for automatic enrollment in advanced courses based on either their eighth grade or their high school assessments, rather than only their high school assessments.
[4] The revised 2019 law expanded automatic enrollment to also include advanced science courses for students who met the standard on the state science assessment.
[5] High school students who achieved a Level 3 (Level 4) score on statewide assessments met (exceeded) state expectations for college-level knowledge and skills, indicating students were on track to engage in college level courses without remediation. The minimum scale score for proficiency (Level 3) was slightly higher than the minimum scale score for meeting state graduation requirements.
[6] Before 2014–15, Washington required students to take the High School Proficiency Examinations for reading and writing in 10th grade. In 2014–15, the state transitioned to having students take the Smarter Balanced Assessment (SBA) in ELA. From 2014–15 to 2016–17, students took the ELA SBA in 11th grade; beginning in 2017–18, students took the ELA SBA starting in 10th grade.
[7] Before 2014–15, Washington required students to take two end-of-course (EOC) examinations for algebra and geometry upon completion of the corresponding courses, typically between ninth and 11th grade. In 2014–15, the state transitioned to having students take the SBA in math. From 2014–15 to

2018–2019, 66 percent of all 11th- and 12th-grade students met the state standard in ELA, and 40 percent met the state standard in math.

## 3. Data, Sample, and Measures

We used individual-level administrative data obtained through a data-sharing agreement with the Washington Office of Superintendent of Public Instruction that included 11th- and 12th-grade students in Washington public schools from 2010–11 through 2018–19 and higher education records from Washington's Education Research Data Center (ERDC).[8,9] The data included student-level information on students' demographic characteristics; scale scores on all standardized assessments in math and ELA; high school course enrollment history; and postsecondary outcomes, including postsecondary enrollment, first-year college coursetaking, and persistence to a second year of college. Three demographic characteristics, measured at baseline, served as covariates: sex, special education status (whether students have an individualized education plan [IEP]), and whether the student spoke English as a primary language. Two demographic characteristics were used to examine whether outcomes differed for students who were members of groups with disproportionately low participation in advanced courses: students eligible for FRL, and URM students.

Our sample consisted of students enrolled in 240 public-school districts in Washington state. These districts represented 91% of the districts with a high school during the study

2016–17, students took the math SBA in 11th grade; beginning in 2018, students took the math SBA starting in 10th grade. Students still had the opportunity to take one or both EOC examinations through 2017–18.

[8] Both the K12 and higher education data obtained were obtained through ERDC. ERDC's data system is a statewide longitudinal data system that includes de-identified data about people's preschool, educational, and workforce experiences.

[9] Postsecondary outcomes were not reliably available for Grade 12 students in the 2010–11 cohort, so these students were excluded from our analyses.

period.[10] To be eligible for Academic Acceleration, students must have met or exceeded state standards on their reading/writing and/or math standardized assessment by earning a score of "proficient" or higher. Students who took an assessment but did not earn a score that met standards or who had not yet taken the assessment(s) were classified as not having a proficient test score. We limited our sample to students in Grades 11 and 12 who scored "proficient" or higher in our analysis for two reasons. First, as the policy was written, only students who had a score of proficient or higher were identified for automatic enrollment. Second, prior research found that policy effects on high school advanced coursetaking were in fact concentrated among proficient students, so we expect any effects on postsecondary to occur for these students (Austin et al., 2025). We conducted additional analyses using all students in grades 11 and 12, and the results were quite similar to those reported below.

Our indicator of treatment is based on policy adoption year. Our treatment group comprised students enrolled in the 72 districts that were early implementers of the policy—those adopting between 2013–14 and 2015–16 and implementing in 2014–15 (45 districts), 2015–16 (12 districts), and 2016–17 (15 districts). Our comparison group included the remaining 168 districts that did not adopt Academic Acceleration before the legislative update that made policy adoption mandatory by 2022. We defined pre-policy as the time period(s) before policy implementation and post-policy as the time period(s) for each district beginning with the first full academic year following the date on which the district adopted its policy. For example, if a district adopted an Academic Acceleration policy on January 31, 2014, we defined 2013–14 as

[10] We excluded a small number of districts that began implementing the policy before statewide guidance was available or that did not have any high school enrollment data prior to 2015; these early implementing districts lacked sufficient pre-policy data to examine outcomes.

the district's last pre-policy year and 2014–15 as the district's first post-policy year; this district would be part of the 2015 implementation cohort.

We examined several postsecondary outcomes. For all students in our sample, we examined any college enrollment in the fall after high school graduation and type of college enrollment (2-year or 4-year), as well as persistence to a second year of college. We are able to track in-state, out-of-state, and private college enrollments for all students using National Student Clearinghouse data (which is obtained by ERDC). In our plausibly causal models, type of college enrollment and persistence are not conditional on enrolling in college since these are outcomes that might be influenced by the policy.

For students who enrolled in public higher education institutions in Washington, we also examined the following course taking outcomes in their first year: the total number of credits attempted and earned; whether students passed all courses they attempted; enrolling in and passing at least one upper-level class; and, for students with a proficient math score, enrolling in and passing at least one upper-level math course. Because these outcomes are only available for students who attended in-state public colleges or universities, they are by necessity conditional on enrollment in one of these institutions.

In Table 1, we present information on the study sample in the pre-policy period (2010–11 to 2013–14). The table includes the demographic characteristics, course enrollments, and proportion of students who met the proficiency standard on the state standardized assessments. Average annual Grades 11 and 12 enrollment was similar in the two sets of districts. Policy implementing districts had larger shares of URM students (29 percent compared with 24 percent) and FRL students (47 percent compared with 37 percent) than comparison districts. Policy implementing and comparison districts had similar shares of students proficient in ELA and/or math (74 percent and 77 percent, respectively); advanced coursetaking rates were 5 percentage points higher in comparison districts than treatment districts for both ELA/Social Studies (32 percent compared to 27 percent) and math (17 percent compared with 12 percent). In both treatment and comparison districts, URM and FRL students were enrolled in advanced courses at lower rates than all students.

In Table 2, we present the proportion of proficient students, overall and by demographic group, experiencing each outcome in comparison and early implementing districts across the pre-policy and post-policy years. Although there were differences in average outcomes between students in comparison and early implementing districts in the pre-policy years, the average standardized mean differences (SMDs) between groups (i.e., all pre-implementation years together) were all smaller than 0.25. The magnitude of these differences meet the What Works Clearinghouse (2022) standard for baseline equivalence with the use of statistical adjustment, which we use in our models. In the pre-policy years, we see the smallest SMDs for 2-year college enrollment and the coursetaking outcomes. We see larger differences between groups in college persistence and credits attempted and earned. After policy implementation, we see differences similar to those observed pre-policy between the two groups in college enrollment and persistence outcomes. However, we see larger differences between groups in credits attempted and earned and some coursetaking outcomes.

## 4. Difference-in-Differences Design

To estimate the impact of Academic Acceleration on postsecondary outcomes, we used a difference-in-differences (DID) model that compared changes in postsecondary enrollment, coursetaking and credit outcomes, and persistence in early implementing districts to changes in each outcome in comparison districts. We estimated the following two-way fixed effects model, pooling across the three early implementation cohorts (2014–15, 2015–16, and 2016–17):[11]

$$Y_{idt} = \gamma_d + \lambda_t + \delta_1 Policy_{dt} + \theta \mathbf{X}_{idt} + u_{idt} \qquad \text{Eq.(1)}$$

where $Y_{idt}$ is a given postsecondary outcome for proficient student *i* in district *d* in year *t*, $\gamma_d$ and $\lambda_t$ are district and year fixed effects, and $Policy_{dt}$ is a time-varying indicator that equals one for early implementing districts in the post-policy period and zero otherwise. The coefficient of interest is $\delta_1$, which represents the change in outcome for students with a proficient score in early implementing districts, relative to proficient students in comparison districts. $\mathbf{X}_{idt}$ is a vector of student characteristics, including URM status, FRL status, IEP status, sex, and whether the student spoke English as a primary language. For binary outcomes, we fit linear probability models. In all models, standard errors are clustered at the district level, since it is the level of policy variation.

Because we might expect the policy to differently affect students who are members of particular demographic groups, as was the case for policy effects on high school outcomes, we also estimated two models with two-way interaction terms for each demographic characteristic to test whether postsecondary changes differed for students who are members of a URM group and who are eligible for FRL, relative to students who do not have these characteristics, as in the following equation:

$$Y_{idt} = \gamma_d + \lambda_t + \delta_1 Policy_{dt} + \delta_2 Grp_{idt} + \delta_3 Policy_{dt} * Grp_{idt} \qquad \text{Eq.(2)}$$
$$+ \theta \boldsymbol{X}_{idt} + u_{idt}$$

where $\boldsymbol{Grp}_{idt}$ is a dummy variable for whether student *i* is a member of a URM group or is eligible for FRL, depending on the model, and all other terms are defined as described above.

[11] Results by cohort were largely similar. Results available from the authors upon request.

### *4.1 Assumption of Parallel Trends*

A DID design identifies plausibly causal effects of the policy under the assumption that outcomes in policy implementing districts would have followed the same trend as in comparison districts in the absence of the intervention. We plotted pre-policy trends in each outcome of interest, which we present in Figures 1 and 2, to assess the plausibility of this assumption. We also formally tested the assumption (de Chaisemartin & d'Haultfoeuille, 2020) and found that it held for most outcomes, including enrollment in any college ($p$=0.45), enrollment in a 2-year college ($p$=0.95), enrollment in a 4-year college or university ($p$=0.17), credits earned in the first year ($p$=0.26), taking at least one upper-level course in the first year ($p$=0.19), taking at least one upper-level math course in the first year ($p$=0.20), passing at least one upper-level math course in the first year ($p$=0.39), and persisting to Year 2 ($p$=0.31). The null hypothesis of parallel trends was rejected for two outcomes: passing all courses attempted in the first year ($p$ = 0.00) and passing at least one upper-level course in the first year ($p$ = 0.03). We therefore have confidence that findings for most outcomes can be ascribed to policy implementation.

As recent literature shows, DID estimates that satisfy parallel trends assumptions can still be biased when intervention timing varies. Several recent papers provide procedures to check for bias and help ensure the reliability of two-way fixed effects (TWFE) estimates in these cases (Callaway and Sant'Anna, 2021; de Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). We found similar estimates using both Goodman-Bacon (2021) and de Chaisemartin and d'Haultfoeuille (2020). This is because nearly all of the weight from the TWFE regressions were obtained from the ''safe'' comparisons of new policy implementers to nonpolicy implementers. We therefore elected to focus on the standard TWFE estimates, both for simplicity and because some procedures (e.g., Goodman-Bacon) require collapsing the data down to the district-year level, making them poorly suited for the interaction terms that are a key focus.

## 5. Results

In Table 3, we present our main results for all proficient students, as well as interaction coefficients from models including the interaction between policy and proficient URM students, and between policy and proficient FRL students. For the most part, we do not observe statistically significant effects of Academic Acceleration on students' postsecondary outcomes. The only significant main effect is a negative effect on taking at least one upper-level course in the first year of college (2 percentage points). We find little evidence of effects of the policy on college enrollment (any, 2-year, or 4-year) or persistence to a second year; the estimated coefficients are all close to zero. For any college enrollment, 4-year college enrollment, and persistence, we can rule out effects larger than an increase of 2 percentage points and smaller than declines of 2 percentage points. In other words, we are able to rule out the possibility that effects are as large in magnitude as the effects of automatic enrollment on high school coursetaking outcomes found in prior work (Austin et al., 2025). We find small negative effect estimates on credits earned and attempted (declines of approximately one credit), but these are not significant. In additional analyses, we explored whether the policy effects varied by number of years post-adoption using event study analyses. We did not find evidence to support differential effects over time.[12]

[12] We also examined conditional results by college enrollment pathway. This produced four sets of results: impacts for those who took an advanced course (akin to a "treatment on the treated") by 2-year/4-year enrollment and impacts for those who did not take and advanced course by 2-year/4-year college enrollment. Consistent with the main results, we found few to no meaningful or statistically significant results. Results available from the authors.

The policy is estimated to have decreased 2-year college enrollment by 2 percentage points more for proficient students eligible for FRL than proficient students not eligible for FRL (Table 3). The policy had a more positive effect on the number of credits attempted in the first year for proficient URM students than for other proficient students (0.60 more credits). For both groups, nearly all the other coefficients are substantively small and not statistically significant, indicating that Academic Acceleration did not have differential effects based on student characteristics.

## 6. Discussion and Conclusion

This study is the first to produce rigorous estimates of changes in postsecondary educational outcomes following the adoption of targeted automatic enrollment policies. We extend the acceleration literature to include postsecondary outcomes for students who were exposed to Academic Acceleration toward the end of their high school careers.

We found that Academic Acceleration in Grades 11 and 12 largely did not lead to changes in postsecondary enrollment, coursetaking in students' first year of college, or persistence to a second year of college. The exception is the probability of taking at least one upper-level course in the first year, which decreased 2 percentage points as a result of the policy (the probability of passing at least one upper-level course in the first year similarly decreased, but the coefficient was not statistically significant). We found negative, but not significant, effects of the policy on credits attempted and earned in the first year which could suggest that students were able to take fewer credits because they could apply college credits earned from their advanced high school courses.

We also found few differential effects for students from URM groups or students eligible for FRL. The primary exception is that the policy significantly decreased 2-year college enrollment by 2 percentage points for proficient students eligible for FRL compared to proficient students not eligible for FRL. There is suggestive evidence that the policy shifted FRL students from 2-year to 4-year college enrollment, as the interaction coefficient for 4-year enrollment is a positive 2 percentage points, although the coefficient is not statistically significant. Finally, while overall there was suggestive evidence the policy may have had a negative effect on credits attempted and earned, this effect was less negative for the number of credits attempted in the first year for URM students.

The preponderance of null findings is perhaps unsurprising, for multiple reasons. First, the policy's effects on Grades 11 and 12 advanced coursetaking were relatively small when compared to the number of students eligible to be automatically enrolled: enrollments in any advanced course increased 5 percentage points overall, and 5–8 percentage points for students with disproportionately low participation in advanced courses, but could have increased much more, as students' overall post-policy probability of advanced course enrollment was only 52% (Austin et al., 2025). Therefore, even if all increases in advanced coursetaking translated into increases in college enrollment and success, we would expect effects no larger than 5–8 percentage points on these postsecondary outcomes. In fact, based on our estimated coefficients and standard errors, we can rule out effects larger than 2 percentage points.

Second, accelerating students in the last two years of high school may potentially be too late to substantially change students' postsecondary trajectories. This may particularly be the case for college enrollment. Long et al. (2012) found that taking a more rigorous course in 9th or 10th grade had larger positive effects on college enrollment than did taking a more rigorous course in 11th or 12th grade. By Grades 11 and 12, many students' 4-year college expectations are set, their GPAs are established, and their coursetaking trajectories are unlikely to change much beyond the course or two into which Academic Acceleration automatically placed them

(Desjardins et al., 2019; Domina et al., 2019; Easton et al., 2017; Jacob & Wilder, 2010). Moreover, findings are mixed on whether AP courses affect students' college admissions test scores (Conger et al, 2023; Jackson, 2014). Consistent with this, we find little evidence of effects related to college enrollment and small changes related to college coursetaking and credits attempted and earned.

In contrast, accelerating students earlier may have the potential for longer-run impacts. For example, accelerating students into eighth grade algebra often involves accelerated middle school courses beginning in sixth or seventh grade and the accelerated trajectory can persist through high school—ultimately changing the level, and associated rigor, of seven or more math classes for a student (e.g., Dougherty et al., 2015, 2017). This has effects on their competitiveness for college admissions and enrollment as well as their readiness for college coursework (Asim et al., 2019; Byun et al., 2014; Long et al., 2012). Washington's statewide Academic Acceleration policy, which required all districts to begin implementing by 2021–22, extended the screening criteria to Grade 8 standardized tests, making acceleration possible early in high school. Future research should compare the trajectories of students exposed to the updated policy to those of students exposed to acceleration only late in high school, to examine how earlier acceleration affects both high school and postsecondary outcomes.

Although we found consistently null effects of Academic Acceleration on postsecondary outcomes for Washington students, the findings can inform the increasing number of district, state, and federal policymakers considering or beginning to implement similar policies across the United States and should motivate policymakers to begin universal screening and automatic enrollment earlier in students' academic careers.

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## Tables and Figures

***Table 1. District Characteristics for Grades 11 and 12 by Policy Implementation Status, 2010–11 through 2013–14***

| | All districts | | | Policy implementing districts | | | Comparison districts | | |
|---|---|---|---|---|---|---|---|---|---|
| Number of districts | | 240 | | | 72 | | | 168 | |
| Average annual Grades 11 and 12 enrollment | | 587 | | | 569 | | | 594 | |
| **Student category** | **All** | **URM** | **FRL** | **All** | **URM** | **FRL** | **All** | **URM** | **FRL** |
| Total number of students | 561,528 | 143,959 | 223,443 | 163,882 | 48,079 | 77,213 | 397,646 | 95,880 | 146,230 |
| Proportion of all students | | 0.26 | 0.40 | | 0.29 | 0.47 | | 0.24 | 0.37 |
| Proportion of total students designated as proficient (met state standards in either ELA or math as of prior year) | 0.76 | 0.64 | 0.66 | 0.74 | 0.62 | 0.65 | 0.77 | 0.65 | 0.67 |
| Total number of proficient students | 428,580 | 91,978 | 147,922 | 120,881 | 29,871 | 49,839 | 307,699 | 62,107 | 98,083 |
| Proportion of all proficient students | | 0.21 | 0.35 | | 0.25 | 0.41 | | 0.20 | 0.32 |
| Average number of advanced courses taken by proficient students | 0.83 | 0.55 | 0.52 | 0.68 | 0.49 | 0.46 | 0.89 | 0.58 | 0.56 |
| **Proficiency** | | | | | | | | | |
| Proportion of proficient students who met standards in ELA | 0.94 | 0.93 | 0.92 | 0.94 | 0.92 | 0.92 | 0.95 | 0.93 | 0.92 |
| Proportion of proficient students who met standards in math | 0.60 | 0.52 | 0.55 | 0.59 | 0.51 | 0.54 | 0.60 | 0.53 | 0.55 |
| **ELA or SS** | | | | | | | | | |
| Proportion of proficient students enrolled in any ELA or SS | 0.88 | 0.90 | 0.90 | 0.87 | 0.89 | 0.89 | 0.88 | 0.90 | 0.91 |
| Proportion of proficient students enrolled in advanced ELA or SS | 0.30 | 0.23 | 0.21 | 0.27 | 0.21 | 0.20 | 0.32 | 0.24 | 0.22 |
| **Math** | | | | | | | | | |
| Proportion of proficient students enrolled in any math course | 0.80 | 0.83 | 0.82 | 0.80 | 0.84 | 0.82 | 0.80 | 0.83 | 0.82 |
| Proportion of proficient students enrolled in advanced math | 0.16 | 0.09 | 0.09 | 0.12 | 0.07 | 0.07 | 0.17 | 0.10 | 0.10 |

*Note.* This table presents district summary statistics for 11th- and 12th-grade students by policy implementation status, aggregated from 2010–11 to 2013–14. All values are weighted at the individual level except for average enrollment, which is averaged across districts. All data except proficient designation (which is measured as of the prior school year) are aggregated across the pre-policy years for all four cohorts (2010/11–2013/14). ELA = English Language Arts, SS = Social Studies, URM = underrepresented minoritized students, FRL = students eligible for free or reduced-price lunch.

***Table 2. Pre-policy and Post-policy Probabilities of Postsecondary Outcomes for Proficient Students***

| | | Pre-policy (2011–2014) | | | Post-policy (2017–2019) | | |
|---|---|---|---|---|---|---|---|
| **Enrollment Outcomes** | | **Comparison (n=349,146)** | **Policy (n=144,936)** | **SMD** | **Comparison (n=322,629)** | **Policy (n=142,681)** | **SMD** |
| Proportion enrolled in any college (Total n = 940,444) | All Proficient | 0.46 | 0.43 | 0.06 | 0.43 | 0.41 | 0.05 |
| | Not URM | 0.47 | 0.44 | 0.06 | 0.44 | 0.42 | 0.04 |
| | URM | 0.42 | 0.40 | 0.04 | 0.41 | 0.38 | 0.07 |
| | Not FRL | 0.49 | 0.48 | 0.03 | 0.46 | 0.45 | 0.02 |
| | FRL | 0.39 | 0.36 | 0.05 | 0.37 | 0.33 | 0.07 |
| Proportion enrolled in a two-year college (Total n = 940,444) | All Proficient | 0.19 | 0.20 | -0.01 | 0.15 | 0.15 | 0.01 |
| | Not URM | 0.18 | 0.19 | -0.02 | 0.14 | 0.14 | 0.02 |
| | URM | 0.24 | 0.23 | 0.03 | 0.20 | 0.18 | 0.04 |
| | Not FRL | 0.18 | 0.19 | -0.03 | 0.14 | 0.15 | 0.03 |
| | FRL | 0.22 | 0.21 | 0.03 | 0.18 | 0.16 | 0.04 |
| Proportion enrolled in a four-year college (Total n = 940,444) | All Proficient | 0.26 | 0.23 | 0.08 | 0.28 | 0.25 | 0.06 |
| | Not URM | 0.29 | 0.25 | 0.08 | 0.30 | 0.27 | 0.06 |
| | URM | 0.18 | 0.17 | 0.02 | 0.21 | 0.20 | 0.04 |
| | Not FRL | 0.31 | 0.28 | 0.06 | 0.32 | 0.30 | 0.04 |
| | FRL | 0.17 | 0.15 | 0.04 | 0.19 | 0.17 | 0.05 |
| Proportion persisting to Y2 (Total n=940,444) | All Proficient | 0.82 | 0.78 | 0.08 | 0.81 | 0.78 | 0.07 |
| | Not URM | 0.82 | 0.79 | 0.08 | 0.83 | 0.79 | 0.07 |
| | URM | 0.78 | 0.75 | 0.06 | 0.76 | 0.73 | 0.07 |
| | Not FRL | 0.84 | 0.82 | 0.04 | 0.83 | 0.81 | 0.04 |
| | FRL | 0.76 | 0.71 | 0.08 | 0.74 | 0.70 | 0.10 |
| **Credit and Course Taking Outcomes** | | **Comparison (n=102,080)** | **Policy (n=39,527)** | **SMD** | **Comparison (83,424)** | **Policy (34,340)** | **SMD** |
| Average credits attempted in Y1 (Total n=298,025) | All Proficient | 31.42 | 30.51 | 0.06 | 31.31 | 29.54 | 0.12 |
| | Not URM | 32.41 | 31.63 | 0.06 | 32.29 | 30.77 | 0.10 |
| | URM | 27.73 | 27.26 | 0.03 | 28.67 | 26.85 | 0.12 |
| | Not FRL | 32.62 | 32.09 | 0.04 | 32.10 | 30.90 | 0.08 |
| | FRL | 28.85 | 28.05 | 0.05 | 29.43 | 26.90 | 0.16 |
| Average credits earned in Y1 (Total n=298,025) | All Proficient | 29.13 | 27.80 | 0.09 | 28.86 | 26.80 | 0.13 |
| | Not URM | 30.28 | 29.10 | 0.08 | 30.12 | 28.30 | 0.11 |
| | URM | 24.82 | 24.05 | 0.05 | 25.43 | 23.52 | 0.12 |
| | Not FRL | 30.53 | 29.74 | 0.05 | 29.89 | 28.46 | 0.08 |
| | FRL | 26.09 | 24.80 | 0.08 | 26.38 | 23.57 | 0.17 |
| Proportion who took any upper-level course in any subject (Total n=298,025) | All Proficient | 0.68 | 0.69 | -0.01 | 0.73 | 0.72 | 0.03 |
| | Not URM | 0.70 | 0.70 | -0.01 | 0.75 | 0.73 | 0.04 |
| | URM | 0.63 | 0.65 | -0.04 | 0.68 | 0.69 | -0.00 |
| | Not FRL | 0.70 | 0.71 | -0.02 | 0.75 | 0.74 | 0.03 |
| | FRL | 0.63 | 0.64 | -0.01 | 0.69 | 0.68 | 0.02 |
| Proportion who passed any upper-level course | All Proficient | 0.63 | 0.63 | 0.01 | 0.64 | 0.60 | 0.07 |
| | Not URM | 0.65 | 0.65 | 0.01 | 0.66 | 0.63 | 0.08 |
| | URM | 0.57 | 0.58 | -0.02 | 0.57 | 0.56 | 0.03 |

| | | Pre-policy (2011–2014) | | | Post-policy (2017–2019) | | |
|---|---|---|---|---|---|---|---|
| (Total n=298,025) | Not FRL | 0.66 | 0.66 | -0.01 | 0.66 | 0.64 | 0.06 |
| | FRL | 0.58 | 0.57 | 0.01 | 0.58 | 0.54 | 0.08 |
| Proportion who took any upper-level math course (Total n=218,422) | All Proficient | 0.07 | 0.06 | 0.03 | 0.1 | 0.09 | 0.04 |
| | Not URM | 0.08 | 0.07 | 0.04 | 0.12 | 0.11 | 0.04 |
| | URM | 0.04 | 0.04 | -0.02 | 0.05 | 0.06 | -0.02 |
| | Not FRL | 0.08 | 0.07 | 0.03 | 0.12 | 0.11 | 0.02 |
| | FRL | 0.05 | 0.04 | 0.02 | 0.07 | 0.05 | 0.05 |
| Proportion who passed any upper-level math course (Total n=218,422) | All Proficient | 0.07 | 0.06 | 0.03 | 0.09 | 0.08 | 0.05 |
| | Not URM | 0.07 | 0.06 | 0.04 | 0.11 | 0.09 | 0.05 |
| | URM | 0.03 | 0.04 | -0.04 | 0.05 | 0.05 | -0.01 |
| | Not FRL | 0.07 | 0.07 | 0.02 | 0.11 | 0.10 | 0.03 |
| | FRL | 0.04 | 0.04 | 0.03 | 0.06 | 0.04 | 0.06 |

*Note.* Summary statistics and standardized mean differences (SMDs) are shown for 11th- and 12th-grade students who met the proficiency threshold for Academic Acceleration, by district policy implementation status and student demographic characteristics. Total n = total sample of eligible students in all cohorts (2011–2019) used for DID analyses. Table reports pre-policy statistics aggregated across pre-implementation years shared by all cohorts (n=494,082) and post-policy statistics aggregated across all shared post-implementation years (n=333,406); students from 2015 and 2016 cohorts are not included in this comparison due to overlapping implementation statuses. General course and credit outcomes are limited to students who enrolled in an in-state public institution who met the proficiency threshold in either math or English Language Arts (n=298,025), and upper-level math course outcomes are limited to students who enrolled in an in-state public institution who met the proficiency threshold in math (n=218,422). FRL = students eligible for free or reduced-price lunch; URM = underrepresented minoritized students.

***Table 3. Results of Difference-In-Differences Models Overall and with Two-Way Interactions for Proficient Students***

| | Model 1 (policy) | | Model 2 (policy*URM) | | Model 3 (policy*FRL) | |
|---|---|---|---|---|---|---|
| | **Coef.** | **SE** | **Coef.** | **SE** | **Coef.** | **SE** |
| ***Attended any college or university*** | | | | | | |
| Policy | –0.00 | (0.01) | –0.00 | (0.01) | –0.00 | (0.01) |
| Policy*URM | | | –0.03 | (0.01) | | |
| Policy*FRL | | | | | –0.01 | (0.01) |
| ***Attended 2-year college*** | | | | | | |
| Policy | –0.00 | (0.00) | 0.00 | (0.00) | 0.00 | (0.01) |
| Policy*URM | | | –0.01 | (0.01) | | |
| Policy*FRL | | | | | –0.02** | (0.01) |
| ***Attended 4-year college or university*** | | | | | | |
| Policy | 0.00 | (0.01) | –0.00 | (0.01) | –0.01 | (0.01) |
| Policy*URM | | | 0.01 | (0.01) | | |
| Policy*FRL | | | | | 0.02 | (0.01) |
| ***Credits attempted in the first year*** | | | | | | |
| Policy | –0.93 | (0.73) | –1.12 | (0.81) | –0.95 | (0.73) |
| Policy*URM | | | 0.60* | (0.43) | | |
| Policy*FRL | | | | | 0.06 | (0.30) |
| ***Credits earned in the first year*** | | | | | | |
| Policy | –0.84 | (0.67) | –1.03 | (0.73) | –0.86 | (0.66) |
| Policy*URM | | | 0.64 | (0.44) | | |
| Policy*FRL | | | | | 0.06 | (0.33) |
| ***Passed all courses attempted in the first year*** | | | | | | |
| Policy | 0.01 | (0.01) | 0.01 | (0.01) | 0.01 | (0.01) |
| Policy*URM | | | 0.00 | (0.01) | | |
| Policy*FRL | | | | | 0.01 | (0.01) |
| ***Took at least one upper-level course in the first year*** | | | | | | |
| Policy | –0.02* | (0.01) | –0.02* | (0.01) | –0.02* | (0.01) |
| Policy*URM | | | 0.01 | (0.01) | | |
| Policy*FRL | | | | | 0.01 | (0.01) |
| ***Passed at least one upper-level course in the first year*** | | | | | | |
| Policy | –0.03 | (0.02) | –0.03 | (0.02) | –0.03 | (0.02) |
| Policy*URM | | | 0.01 | (0.01) | | |
| Policy*FRL | | | | | 0.00 | (0.01) |
| ***Took at least one upper-level math course in the first year*** | | | | | | |
| Policy | –0.00 | (0.00) | –0.00 | (0.01) | 0.00 | (0.01) |

| | Model 1 (policy) | | Model 2 (policy*URM) | | Model 3 (policy*FRL) | |
|---|---|---|---|---|---|---|
| | **Coef.** | **SE** | **Coef.** | **SE** | **Coef.** | **SE** |
| Policy*URM | | | –0.00 | (0.01) | | |
| Policy*FRL | | | | | –0.01 | (0.01) |
| ***Passed at least one upper-level math course in the first year*** | | | | | | |
| Policy | –0.01 | (0.01) | –0.01 | (0.01) | –0.00 | (0.01) |
| Policy*URM | | | –0.00 | (0.01) | | |
| Policy *FRL | | | | | –0.00 | (0.00) |
| ***Persisted to Year 2*** | | | | | | |
| Policy | –0.00 | (0.01) | –0.00 | (0.01) | –0.00 | (0.01) |
| Policy*URM | | | 0.00 | (0.01) | | |
| Policy*FRL | | | | | –0.00 | (0.01) |

*Note.* This table presents estimates of the overall policy effect (Model 1) as well as models with two-way interactions between policy and the two subgroups of interest for 11th- and 12th-grade students who met the proficiency threshold for Academic Acceleration. N for college enrollment and persistence outcomes = 940,444. N for general course outcomes = 298,025. N for math course outcomes = 218,422. *Policy* equals 1 in policy implementing districts in post-policy years and 0 otherwise. Coefficients on the two-way interaction terms *Policy*URM* and *Policy*FRL* represent the percentage point changes in decimal in the probability of postsecondary enrollment, coursetaking, and persistence for students who are part of a URM group relative to students who are not, and for students who are eligible for FRL, relative to students who are not. URM equals 1 if a student is a member of a racial/ethnic group typically underrepresented in advanced courses and 0 otherwise; FRL equals 1 if a student is eligible for FRL and 0 otherwise. All models include district and year fixed effects. Standard errors are in parentheses and are clustered at the district level. *$p < 0.05$. **$p < 0.01$. ***$p < 0.001$. ; Coef. = coefficient; FRL = free or reduced-price lunch URM = underrepresented minoritized students.

***Figure 1. Parallel Trends Plots for College Enrollment Outcomes (Panel A) and Credits Attempted and Earned in the First Year of College (Panel B)***

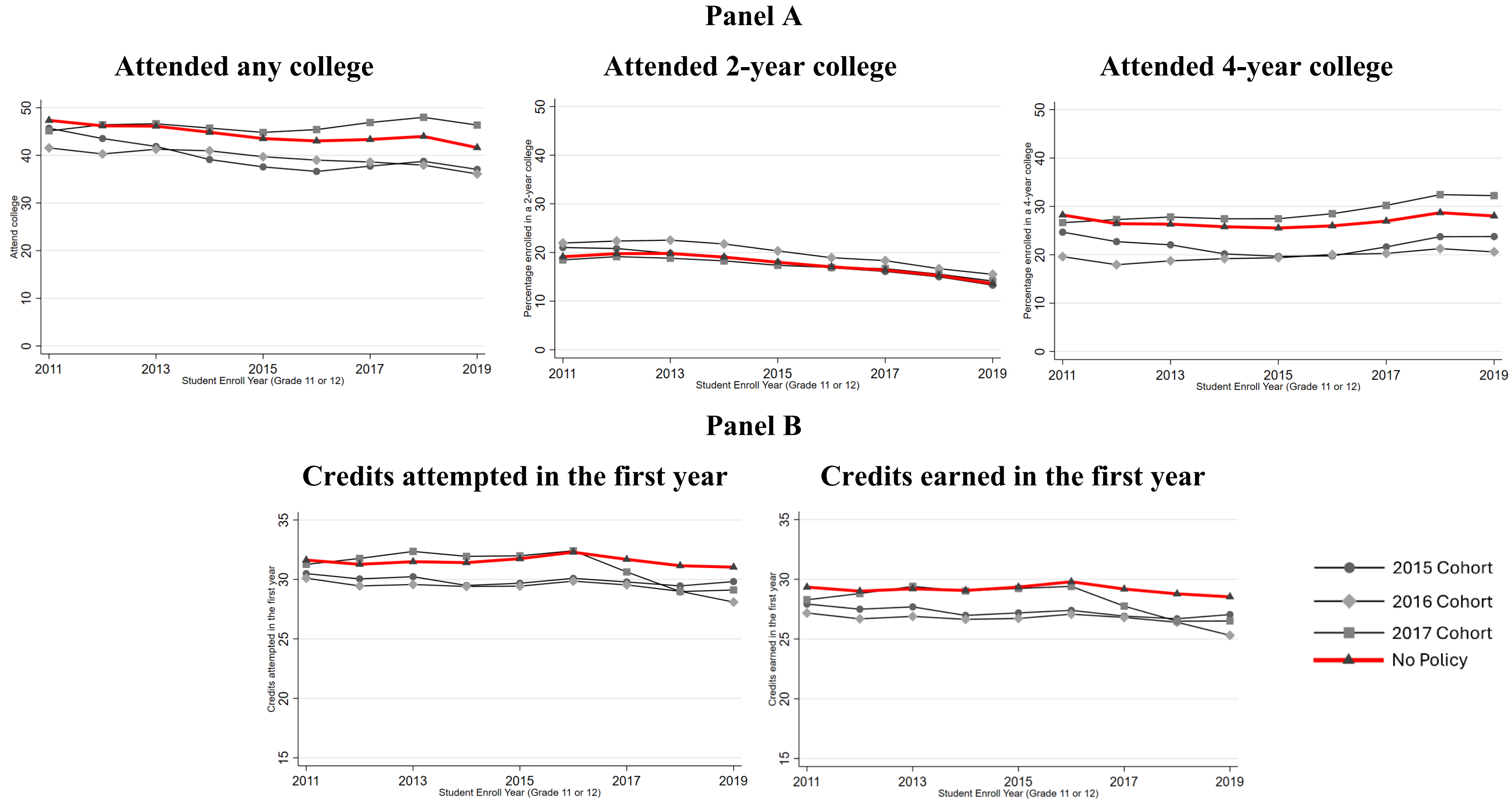
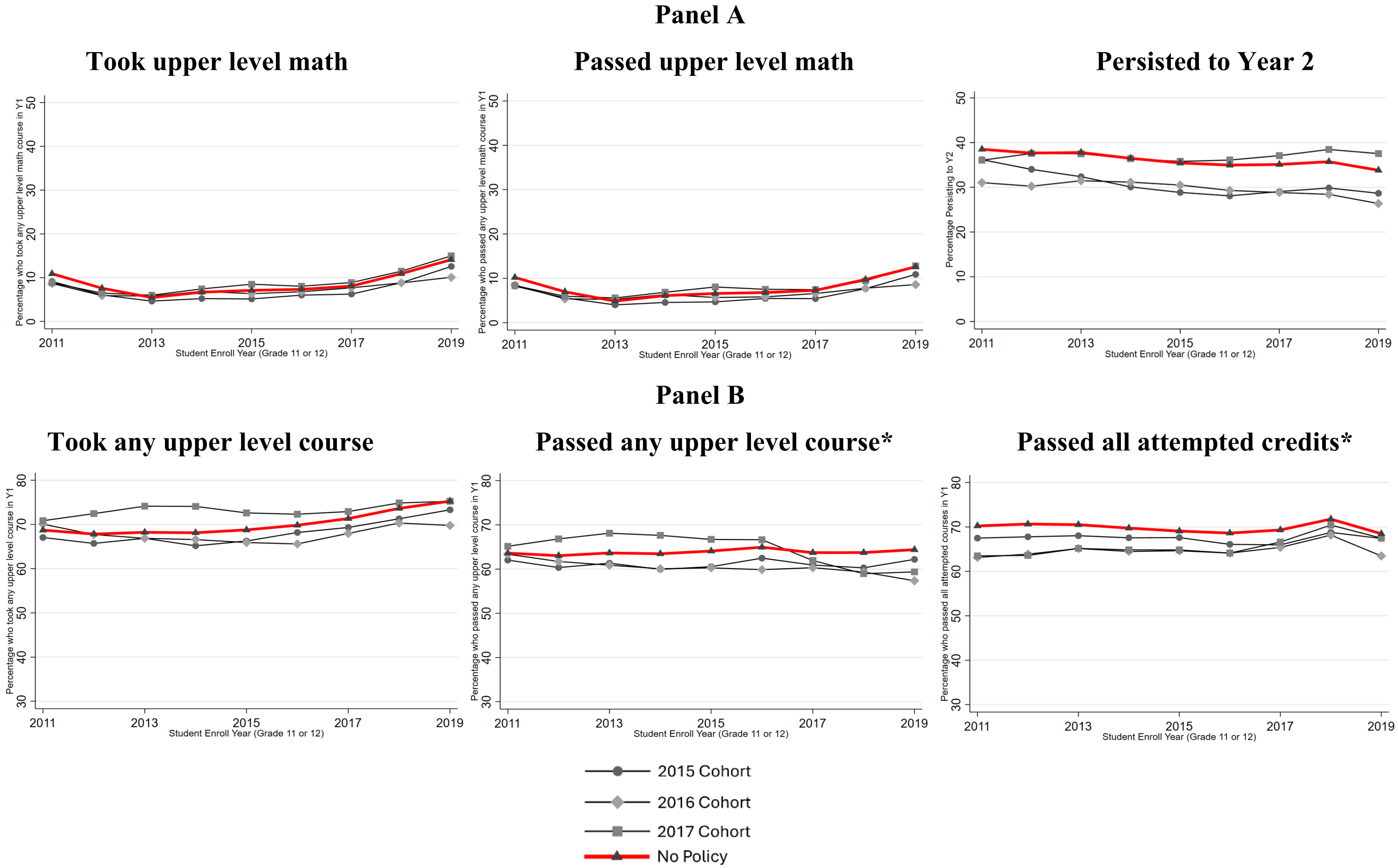


***Figure 2. Parallel Trends Plots for Upper Level Math and Persistence Outcomes (Panel A) and for General Upper Level Coursetaking and Passing All Attempted Credits (Panel B)***




*Note.* *indicates violation of parallel trends assumption