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Essays on the Economics of Higher Education

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To my family

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Abstract

Community colleges have been hailed as a "cornerstone of American higher education," and act as an important access point to post-secondary education for a wide range of students, particularly socioeconomically disadvantaged students and underrepresented minorities. However, community colleges have also been criticized for poor completion outcomes, leading to the implementation of policies meant to bolster academic success. This dissertation focuses on how various policies, either directly or indirectly, affect community college student success, and whether these policies achieved its intended goals.

Chapter 1 studies how the removal of remedial education policies affects academic outcomes for students along a continuous range of college readiness. In this chapter, I merge two rich, detailed administrative datasets to show that remedial education, a policy meant to support underprepared students, actually impeded community college students' academic progress. In fact, I find that many students who would have been placed into remedial education are able to pass transfer-level courses after the remedial education policies were removed. In addition, students attempt more transfer-level coursework, which is necessary to accomplish long-run goals, such as transfer or degree receipt. These positive effects decline, but are still present, even for students who were expected to have benefited most from remedial education.

Chapter 2 examines the indirect effects of changes in the minimum wage in California on students' decisions to enroll in postsecondary institutions. In this chapter, my co-authors and I find that increases in minimum wages above the state minimum wage in different cities have small, positive effects on overall college enrollment. However, underlying this effect is a significant shift in student composition across college sector and quality. Lower performing, economically disadvantaged, and traditionally underrepresented students increase enrollment at community colleges. Meanwhile, higher performing students substitute their enrollment from community colleges into four year public institutions. Results examining unit accumulation for community college students provide evidence that increases in minimum wage alleviate financial constraints.

Finally, Chapter 3 investigates the effects of financial aid tied to academic conditions, such as full-time enrollment, on community college student outcomes. In this chapter, my co-authors and

I find that community college students do not change their course-taking behavior to take advantage of generous increases in financial aid. Students do not seem to learn about these conditions even after receiving the grant, as there are null effects of grant receipt in the following semester, suggesting a deeper lack of awareness regarding the details of their financial aid package. These results have important implications regarding how to structure financial aid policies efficiently.

CHAPTER 1

Remedial Education Reform in California and Community College Student Outcomes

1.1. Introduction

Community college is a vital and substantial component of the higher education system in the United States today. In 2021, community colleges enrolled 41% of all undergraduates and 39% of all first-time college students.[1] In particular, the California Community College system is the largest system of higher education in the United States, enrolling 25% of all community college students in the nation.[2] Community colleges have been touted as a cheaper alternative and gateway to four-year colleges,[3] with the potential to help reduce inequities in income and wealth.[4] However, the reality has not been so rosy. Out of the California community college students who stated an intent to transfer or graduate with a degree, only 48% were able to do so in 6 years, despite these programs and degrees being meant to be accomplished in 2 years (PPIC, 2019).

Projections have suggested that the state of California will face a shortage of 1 million college-educated workers by 2030 (Johnson et al., 2015), stemming from low graduation rates. One reason cited for these low success rates is remedial education, which has been observed to function more as a roadblock for many students, rather than providing help and support as initially intended (MDRC, 2013). Remedial education consists of courses that reteach and reinforce previously taught skills to help improve student outcomes in future college-level coursework (Jiminez et al., 2016). Proponents of remedial education argue that remedial education can give struggling students individualized attention, and build confidence for later college-level courses.[5] Remedial education is extremely widespread, with 80% of students enrolling at least once throughout their college journey, and disproportionately affects underrepresented minorities and socioeconomically disadvantaged students (Cuellar Meija et al., 2016b).

However, despite the objective to support entering community college students considered underprepared, descriptive studies have shown that remedial education sequences have had unintended

[1]Fast Facts 2021. American Association of Community Colleges. https://www.aacc.nche.edu/wp-content/uploads/2021/03/AACC_2021_FastFacts.pdf. Accessed 12 Oct. 2021.

[2]Key Facts. California Community Colleges. https://www.cccco.edu/About-Us/Key-Facts. Accessed 12 Oct. 2021.

[3]Barrington, Kate. "Community Colleges are More Important than Ever." Community College Review. 07 July 2020. Accessed 12 Oct. 2021.

[4]Mintz, Steven. "Community Collges and the Future of Higher Education." 9 Mar. 2019. https://www.insidehighered.com/blogs/higher-ed-gamma/community-colleges-and-future-higher-education. Accessed 12 Oct. 2021.

[5]Barrington, Kate. "How Community College are Changing Remedial Education." Community College Review. 16 June 2021. https://www.communitycollegereview.com/blog/how-community-colleges-are-changing-remedial-education. Accessed 8 Nov. 2021.

negative consequences, such as lengthening time to degree and encouraging overall attrition,[6] with many students never advancing to a transfer-level course. For example, English remediation sequences require an average of 1.9 terms for completion, and math remediation sequences require an average of 2.5 terms for completion. Attrition is extremely high, with only 44% of remedial math students and 60% of remedial English students completing the sequence (Bailey et al., 2010; Cuellar Meija et al., 2016b). This is particularly troublesome, as students will not be able to access certain college-level courses without finishing their remediation sequence, in particular introductory English and math courses that are required for graduation or transfer. Only 27% of students who take a remedial math course eventually complete a transfer-level math course with a C or better, while 44% of remedial English students go on to complete a transfer-level English course (Cuellar Meija et al., 2016b).

These worrisome statistics have motivated the state of California to pass various reforms with the hopes of encouraging and bolstering student success. In 2013, California first passed remedial education placement reforms. Following concerns that these policies were not working as intensely as hoped, California then instituted one of the most sweeping changes to the remedial education system, effectively removing mandatory remedial education requirements altogether in 2017. I use the introduction of these policies as a source of quasi-exogenous variation, to study how remedial education reforms, as well as the complete removal of remedial education, affect community college student success outcomes, particularly with respect to course selection and overall units accumulated.

I take advantage of two rich and extensive administrative datasets which span from 2008-2020: 1) transcript data on all students enrolled in the now 116 community colleges in the California Community College (CCC) system from the California Community Colleges Chancellor's Office, and 2) data on the universe of public high school students in the state of California from the California Department of Education (CDE), which includes standardized test score data. I use high school demographic and academic ability variables to predict the likelihood that student

[6] Bautsch, Brenda. "Hot Topics in Higher Education: Reforming Remedial Education." National Conference of State Legislatures. February 2013. https://www.ncsl.org/documents/educ/REMEDIALEDUCATION_2013.pdf. Accessed 28 July, 2021.

would have been enrolled in remedial education before any reforms were made, which also acts as a proxy for perceived college readiness.

This is one of the only papers that can study the effect of the removal of remediation requirements on students along a continuous measure of college readiness. My paper has the advantage of being able to use rarely available student-level data on college course selection linked to student-level high school data including a rich set of controls, and information on parent education and English proficiency, along with more commonly observed controls such as gender, race, and socioeconomic status. Finally, it is one of few causal papers that can study the effect of increasing direct access to transfer-level courses, joining a collection of papers which study a similar policy change in Florida beginning in 2013, and the first to do so with respect to California.

I find that the effective removal of remedial education through Assembly Bill (AB) 705 had larger effects on course selection than the combination of remedial education placement reform policies implemented from 2013-2017. Earlier reforms to remedial education placement through multiple measure did not have large effects on the proportion of students enrolled in remedial English or math. In comparison, there were large reductions in the proportion of students enrolled in remedial courses after AB 705 was passed, particularly concentrated among students with the lowest levels of academic preparation.

In addition, AB 705 had comparatively larger effects on both math and English transfer-level course participation, with students after remedial reform implementation passing both subjects with a C or better at similar or even higher rates compared to students enrolled in community college before any course selection reform. This result holds for students across all levels of academic preparation, except for those deemed least prepared for college. These results are consistent with the motivation for AB 705 legislation, which was to completely eliminate the use of remediation unless deemed necessary.[7]

Both remedial education placement reforms and the effective removal of remedial education affected students' overall course loads, with mixed results regarding course completion rates. During the time period of remedial education placement reforms, students attempted more transfer-level courses and completed them at higher rates than students before any policy reform. In contrast,

[7]California State Assembly Bill. Assembly Bill 705: Seymour-Campbell Student Success Act of 2012. SEC 2.78213d(1)(A).

after AB 705 was implemented, although students did attempt and earn more transfer-level units, they did so at a lower completion rate than did students before any policy change. These findings suggest that once given the option to take transfer-level courses, there is potential for students to take more transfer-level courses too quickly.

I find that the students who benefit the most from this increase in access to transfer-level courses are students on the margin of being placed into remedial education, and that these beneficial effects decrease but are still positive, as students are deemed to be less and less college ready. These results provide support for the previous literature, which has found that students who are most negatively affected by remedial education are those at the margin.[8]

This paper has many policy implications, particularly regarding the future of remedial education. Considering that I find that many students, who would have been placed into remedial education before any reform was passed, were capable of passing transfer-level English or math, suggests that remedial education might not have imparted substantial benefits to those students. Since remedial education is a widespread, but costly intervention, it is important for colleges to understand what sort of benefits or costs are accruing as a result of this policy. As colleges nationwide move to restructure and reform remedial education, it is also necessary to understand how these policy changes affect all students across a range of academic needs, and not just at the margin.

1.2. Literature Review and Policy Background

Causal studies regarding the efficacy of remedial education have not come to a consensus, with studies finding a mix of negative or null effects of remedial education on a large range of student outcomes. Recent papers focusing on four-year college students similarly find mixed results on a myriad of outcomes, including credit accumulation, persistence, degree completion, and even labor market outcomes (Bettinger and Long, 2008; Boatman and Long, 2018; Calcagno and Long, 2008; Martorell and McFarlin, 2011). An earlier study by Bettinger and Long (2005) finds positive effects of math remediation on math credits completed and the probability of transfer for community college students, but no effect of English remediation on any measure of success, also suggesting the importance of studying effects separately by subject.

[8] There are some papers who find that remedial education actually has the largest negative effect for students at the lowest levels of academic preparation (Calcagno and Long, 2008).

My paper relates specifically to a strand of literature focused on remedial courses and its effect on college outcomes, and more broadly on how college readiness affects college success. Many papers dedicated to understanding the effect of enrolling in remedial education utilize a regression discontinuity strategy (Calcagno and Long, 2008; Duchini, 2017; Martorell and McFarlin, 2011), which provides great internal validity, and focuses on students at the margin, who are potentially the students who would least benefit from remedial education.

A few papers regarding the efficacy of remedial education, such as Scott-Clayton and Rodriguez (2015). Xu (2016), and Boatman and Long (2018), are able to study its effects on students over a range of academic needs. Boatman and Long (2018), like other papers, use a regression discontinuity design; however, they are able to analyze effects of remediation on students who are assigned to different quantities of remedial courses, considered as a proxy for college readiness. Their results suggest that the benefits of remedial courses on students' academic success are dependent on the level of student preparation. For example, students who only required one remedial course faced the largest negative effects, and were less likely to complete a college degree and accumulated fewer college credits over time. However, students required to take two remedial courses faced less negative effects, and in some cases, were even more likely to persist than similar students who were required to take only one remedial course.

In contrast, Xu (2016) finds that students who required the most remediation faced the largest negative effects. Following a similar strategy as Boatman and Long (2018), using regression discontinuity to study students on the margins of requiring different levels of remedial courses, Xu finds that students who required the lowest level of remedial education were more likely to drop out of college and, consequently, less likely to ever enroll in a transfer-level English course. Similarly, Clotfelter et al. (2015), using an instrumental variables strategy relying on variation of placement policies and geographic proximity of various community colleges, find that students at the bottom of the 8th-grade achievement distribution are the most adversely affected by remediation.

A potential reason for these mixed outcomes stems from how students are defined as under-prepared and placed into remedial education. A large proportion of community colleges across the nation relies solely on placement exam score cutoffs to place students into remedial education,[9] and this was largely true in California community colleges, in particular (Cuellar Meija et al., 2016a). However, studies have shown that standardized testing routinely underplaces students into remedial education at an overwhelming rate (Belfield and Crosta, 2012), and can be a worse predictor of future academic success than overall high school performance (Allensworth and Clark, 2020; Scott-Clayton, 2012; Scott-Clayton et al., 2014).[10]

Before AB 705 was passed, placement of students into remedial education varied widely across the 114 community colleges in California. Although the vast majority of colleges relied mostly on assessment test scores taken by incoming first-time students, there was substantial variation in the cutoff score used to place students into remedial education, and even the exam administered was not consistent across colleges (Cuellar Meija et al., 2016b).

These studies prompted the State legislature of California to pass a mandate in 2013 requiring community colleges to use multiple measures,[11] such as high school courses taken, or high school GPA, to place students into remedial education, instead of relying so heavily on entrance exam scores.[12] This mandate could affect students on the margin of requiring remedial education, diverting them from mandatory remedial education, and increasing their direct access to transfer-level courses. However, studies have shown that despite the mandate, multiple measures were being inconsistently applied across colleges, and that this uneven implementation resulted in slow-moving changes in remedial education participation.(Cuellar Meija et al., 2016a)

Colleges also offered students automatic exemptions from the remedial education assessment, through the submission of other test scores from college admission exams (SAT and ACT), college-level proficiency exams (Advanced Placement, International Baccalaureate), college-level course completion at another college (Cuellar Meija et al., 2016a), or scores on the high school 11th

[9] Rutschow, Elizabeth Zachary, et al. "The Changing Landscape of Developmental Education Practices: Findings from a National Survey and Interviews with Postsecondary Institutions." Center for the Analysis of Posesecondary Readiness. Nov. 2019. https://postsecondaryreadiness.org/dev/wp-content/uploads/2019/11/changing-landscape-developmental-education-practices.pdf. Accessed 28 Apr. 2021.

[10] However, there are some papers which suggest that standardized exams are just as good at predicting academic success as high school measures, or that standardized exams in conjunction with high school measures are better at predicting academic success.

[11] 5 CA ADC § 55522

[12] Other measures include grade in the last math/English course, high school GPA, the Early Assessment Program (EAP) or counselor recommendation.

grade assessments through the Early Assessment Program.[13] Furthermore, students were allowed to retake the exam again, although retake policies varied across colleges as well.

However, even with potential test retakes and exemptions, a large proportion of students were still affected by and enrolled in remedial education. Roughly 31% of students took a remedial education course during their first semester of enrollment.[14] Thus, remedial education was widespread at California community colleges before 2017.

Concurrently, there was a related push in the California Community College system encouraging students to increase their transfer-level course participation, and thus encourage long-run student success.[15] Together, these changes indicate that transfer-level course participation should increase, and that remedial education enrollment should decrease during 2013-2017.

However, descriptive studies indicated that these policies were not working quickly enough. To help further address these issues, California implemented one of the most sweeping changes to the remedial education placement process. In October 2017, Assembly Bill (AB) 705 was passed,[16] to address the well-documented problems regarding remedial education, and to change how and the ease with which colleges could place students into remedial education, with mandatory implementation by Fall 2019.[17]

AB 705 again reiterated that colleges more consistently use high school transcript data to place students into remedial education, as research has shown standardized tests are poor indicators of future college success, and other measures, such as high school GPA, grades, and courses, can be better predictors of academic success (Allensworth and Clark, 2020; Scott-Clayton, 2012; Scott-Clayton et al., 2014). Furthermore, colleges had to "maximize the probability that a student will enter and complete transfer-level coursework in English and math within a one-year timeframe,"[18]

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[13] Early Assessment Program. California Department of Education. *https://www.cde.ca.gov/ci/gs/hs/eapindex.asp*, Accessed 07 Jan. 2022.

[14] Author's calculations.

[15] The California Community Colleges Chancellor's Office has made a concentrated push to improve student outcomes, specifically through cosing achievement gaps, increasing degree attainment and transfers to four-year universities, and reducing unnecessary credit accumulation. *See,* Vision for Success. California Community Colleges. *https://www.cccco.edu/About-Us/Vision-for-Success.* Accessed 07 Jan. 2022.

[16] What is AB 705? California Community Colleges,`https://assessment.cccco.edu/ab-705-implementation`. Accessed 24 March 2021.

[17] AB 705 Resources. Academic Senate for California Community Colleges. `https://asccc.org/ab-705-resources`. Accessed 24 March 2021.

[18] California State Assembly Bill. Assembly Bill 705: Seymour-Campbell Student Success Act of 2012. SEC 2.78213d(1)(A).

suggesting that enrollment in remedial education would no longer be the default for entering students. Community colleges now have the burden of proof to show that a particular student would most likely not be able to pass a college-level course before placing them in remedial education. These factors together suggest that it will be difficult for colleges to deny most students entry to transfer-level courses. AB 705 mandated that these changes be implemented systemwide by Fall 2019, although some colleges chose to pilot these changes earlier in 2018.[19]

A priori, it is not certain what effects these policy changes will have on student outcomes. Increasing direct access to transfer-level courses necessary for degree attainment or transferring to a 4-year college could decrease time to degree by allowing students to take the necessary classes more quickly. On the other hand, if some students are actually underprepared and require remedial education, then allowing direct access to transfer-level courses could result in more attrition and lower pass rates than before the policy change.

Few papers assess the impact of increasing open access to transfer-level courses, with the notable exception of recent studies focused on Florida. In 2014, Florida passed a similar bill to AB 705, drastically restructuring remedial education in the Florida College System, and no longer requiring students take the remedial education placement exam.[20] Park-Gaghan et al. (2020) find that the effective removal of remedial education helped narrow achievement gaps in gateway course passing for underrepresented minorities. In a closely related paper focused on Florida's policy change, Park-Gaghan et al. (2021) find positive effects on course pass rates for all students across different levels of college preparedness, as defined by general high school course taking, with the largest effects for students deemed the least prepared. However, they are unable to fully account for any linear pre-trends in their analysis, and cannot disentangle effects of policies that potentially affect students at all levels of academic preparation similarly.

I add to the literature on increased direct access to transfer-level courses by studying students along a continuous measure of college readiness, instead of focusing on students at the margin. Furthermore, I have a rich set of rarely available controls to account for student ability, through standardized tests in both English and math taken in high school.

[19] The RP Group (2019). "Access, Enrollment, and Success in Transfer-Level English and Math in the California Community College System." https://mk0edsource0y23p672y.kinstacdn.com/wp-content/uploads/2019/09/AccessEnrollmentSuccess_FINAL.pdf. Accessed May 5, 2021.

[20] SB 1720. https://www.flsenate.gov/Committees/billsummaries/2013/html/501.

I exploit the different timing of these policies, defining three separate time periods – before any of these remedial education reform policies have been implemented, "during" to encompass policies starting in 2013 that were considered to have somewhat less "bite," and "after," including the passing of AB 705 as an additional policy heavily reforming remedial education.

1.3. Data

For this analysis, I use administrative data on the California Community College (CCC) system, which encompasses 116 colleges and represents the largest public higher-education system in the United States, serving over 2.1 million students.[21] This administrative data from the California Community Colleges Chancellor's Office (CCCCO) includes the population of students who enrolled in a community college from 2000-2020, although I focus only on college enrollment from 2011-2020 due to the timing of the policy change and other data restrictions.

The CCCCO data include information on the individual student's demographics, such as gender and race, as well as comprehensive transcript data. The transcript data are at the student-term level, and includes information on all courses taken by an individual student, including the grade earned in each course, the total number of units attempted, units earned, as well as longer-run outcomes, including certificates, awards earned, and transfer status. The CCCCO data also include granular data on each course, including remediation status and subject, as well as transfer status.

I complement the CCCCO data by matching at the student level to data on the entire universe of public high school students in California. This data from the California Department of Education (CDE) cover 5.7 million students from 2008 - 2020, with an average of 475,000 students per cohort. In addition, the CDE data include demographic information on the student's gender, race, socioeconomic status, birthday, and high school attended.

1.4. Empirical Strategy

I use the introduction of various remedial education reforms in 2013, as well as the removal of mandatory remedial education in 2017, as sources of quasi-exogenous variation to study how changes

[21] Facts and Figures. Foundation for California Community Colleges. https://foundationccc.org/About-Us/About-the-Colleges/Facts-and-Figures. Accessed May 5, 2021.

in access to transfer-level courses affect students' academic success at California community colleges, measured by course selection, pass rates in transfer-level courses, and overall course load.

I compare college outcomes of students, before, during, and after the policy changes. I define the "before" period to be from Fall 2011 up to and including Spring 2013, the "during" period to start from Fall 2014 up to and including Fall 2017, and the "after" period to be from Spring 2017 to Spring 2020.

Importantly, I do not observe whether students are recommended to enroll remedial education, only if they actually enroll in a remedial education course. Thus, I am not able to observe which students may have initially been recommended to take remedial courses, but did not actually take those courses due to exam retakes, or dropped out of school before taking remedial courses. Furthermore, as students no longer have to take the entrance exam that places students into remedial education after the implementation of AB 705, it's difficult to say which students might be affected by these remedial education reforms.

Instead, I use a rich variety of variables on demographics and ability chosen through a data-driven process to *predict* treatment intensity – a continuous variable representing the predicted probability a student takes remedial English (and separately for math) within the first semester of enrollment. Specifically, I focus on the first semester within the first year of enrollment conditional on the student being enrolled in credit-bearing courses. This restriction allows me to avoid any biases regarding students persisting into the spring semester, or students whose first semester is in the spring rather than the fall.[22]

I use the predicted probability of enrolling in a remediation course as a proxy for counterfactual treatment intensity had remedial education reforms not been passed to estimate the following equation:

$$\begin{aligned} Y_{ihcts} = {} & \alpha + \beta_1[\text{during}_t] + \beta_2[\text{after}_t] + \beta_3[\hat{\mathrm{T}}_{ihc(s=math)}] + \beta_4[\hat{\mathrm{T}}_{ihc(s=Eng)}] \\ & + X_{ihc} + \lambda_c + \lambda_h + \epsilon_{ichts} \end{aligned} \tag{1.1}$$

[22]23% of students first enroll in community college in the spring semester rather than the fall semester.

where each observation is unique at the student i-semester t level, and Y_{ihcts} represents both continuous and binary outcomes, such as the total number of units taken in a semester or whether or not a student passed a transfer-level course for subject s (English or math). The variable $during_t$ is an indicator variable which is 1 if the student is enrolled in community college during the initial reform period, when the course selection reforms focused on students with higher levels of academic preparation, during Fall 2014 to Fall 2016, inclusive. The variable $after_t$ is an indicator variable which is 1 if the student is enrolled in community college after the passing of AB 705, during Spring 2017-Spring 2020, inclusive. Finally, $\hat{\mathrm{T}}_{ihcs}$ is the predicted treatment intensity, and is a continuous measure ranging from 0 to 1. The larger $\hat{\mathrm{T}}_{ihcs}$, the more likely a student is predicted to have enrolled in a remedial education course in subject s within the first semester of enrollment. I control for both the predicted treatment intensity for English and math.

The coefficient of interests are β_1 and β_2, and fixed effects λ_c and λ_h are estimated at the college and high school level. X_{ict} is a vector of controls, including a linear time trend, and student controls for gender, race, age (in months), socioeconomic disadvantage status, and 6th grade standardized test scores in both ELA and math.

1.4.1. Model Selection and Predicted Treatment Intensity. To model predicted treatment intensity, I fit a lasso-logistic model to identify the best factors to predict the probability that a student would have enrolled in remedial education before the passing of remedial education reforms without overfitting the model. I use characteristics chosen from the CDE dataset to estimate a logit model calculating the probability a student would have been placed in remedial education, had these reforms not been passed.[23]

Important variables included in the lasso choice set are standardized test scores in both English and math; however, because California switched from the CST standardized test to the SBAC standardized test in 2014, and the two tests are not comparable over time, I use 6th grade test scores, which are the most recent test scores such that all students in the sample take the same version (CST).

For this analysis, I focus specifically on students who decide to enroll in community college immediately after high school. This sample restriction is also partially due to data limitations

[23]The complete list of the CDE variable choice set is in Appendix A.

after the implementation of AB 705 in 2017.[24] This constraint ensures that students in the later cohorts have an equal opportunity to enroll in community college as students who graduated high school earlier. Furthermore, depending on the timing of enrollment, these remedial education reforms might be more or less salient, depending on the student's goals. For example, "traditional" students' goals lean more towards 4-year transfer and degree receipt compared to students who might be enrolling in community college after spending time in the labor force thus making AB 705 more salient for their course selection. Findings from Calcagno and Long (2008) support the idea that remediation might have positive effects for nontraditional students, in contrast to the somewhat negative effects of remediation on traditional students.

To find the predicted treatment intensity variables, I regress actual remedial education status on a host of characteristics chosen using the lasso logit methodology, a purely data-driven process that does not rely on a theoretical basis for choosing variables for prediction. This allows me to be agnostic as to why certain variables should or should not predict remedial education status.

The lasso logit methodology is a method of choosing variables to improve the prediction accuracy of a model, and in particular works to minimize the following equation:

$$L + \lambda(\sum |\beta_1| + |\beta_2|) + |\beta_3|) + ... \tag{1.2}$$

where L represents the log likelihood function, but the parameter of importance is λ, the penalization parameter. Various methods can be used to choose this parameter, but I use adaptive lasso, which is typically used when the goal is model selection. This particular method typically yields fewer variables than other methods.

I fit separate models for both predicted English and math remediation during the first semester of enrollment. I use only students who enroll in community college during 2011-2013, before any reforms to remedial education or course selection occurred, to create my prediction model. Furthermore, I focus on students who enroll in at least one credit-bearing course.[25]

[24] This restriction is also due to being in between two different standardized test regimes.

[25] Both transfer-level and remedial courses are credit-bearing, but remedial courses do not count towards a degree

To predict whether a student would have enrolled in a remedial course within the first semester of enrollment, I estimate separate binary logit models using the model chosen by the adaptive lasso method for each subject. These predictions are then included in Equation 1.1 as $\hat{\mathrm{T}}_{ihcs}$, as a continuous measure of treatment intensity, and representing the perceived college readiness of the student had the student been enrolled in community college during the period before any policy change.[26]

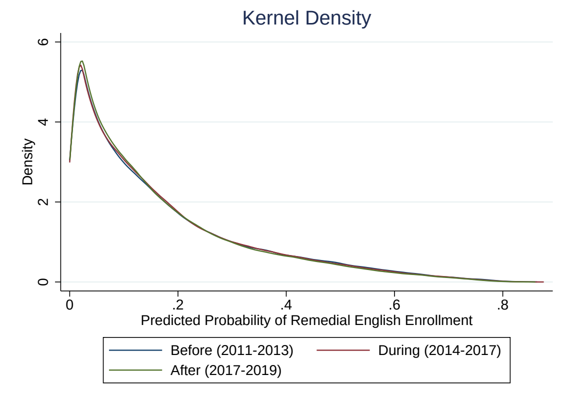


FIGURE 1.1. Distribution of Predicted Probability of Remedial English Enrollment

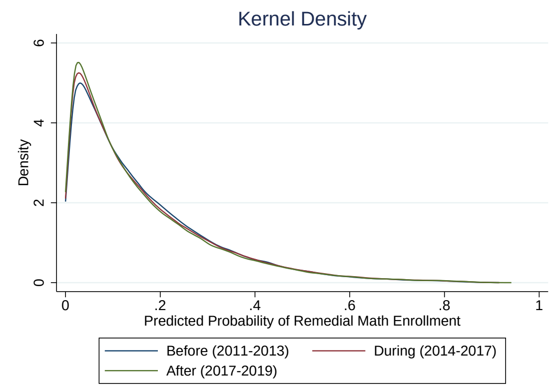


FIGURE 1.2. Distribution of Predicted Probability of Remedial Math Enrollment

I plot the distributions of the predicted probability of being enrolled in remedial English in the first semester of enrollment for students before, during, and after remedial education reforms in Figure 1.1. Comparing the kernel densities of predicted probability of enrolling in remedial English for students in community college before, during, and after remedial education policy changes, there are fewer students with lower predicted probabilities of remedial English enrollment in community college during the "before" period (Fall 2011 - Spring 2013), compared to students in the "during" (Fall 2014 - Fall 2017) and "after" (Spring 2017 - Spring 2020) period. Similarly, there are slightly fewer students with lower predicted probabilities of remedial English enrollment in the during period compared to the after period. However, with respect to students with higher predicted probabilities of remedial English enrollment, the densities across time periods seem similar. The distribution of

[26]Table A1 in Appendix A displays the selected model and associated coefficients for predicting English remedial participation and math remedial participation for students during the first semester a student is enrolled, respectively.

the predicted probability of remedial English enrollment is statistically significantly different across time periods.

Figure 1.2 shows that the distributions of the predicted probability of enrolling in a remedial math course, within the first semester of community college enrollment. There is a larger difference in the distribution of the predicted probabilities of math remedial enrollment compared to the distribution of the predicted probabilities of English remedial enrollment. Again, students in the period after the policy change are more likely to have a lower predicted probability of enrolling in remedial math than students in the period before and during remedial education policy changes.

1.5. Summary Statistics and Descriptive Trends

To understand how these policies may have affected community college students' course selection, I first graph course participation trends, separately by English and math course participation, and conditional on being in my sample of recent high school graduates. Figure 1.3 graphs the proportion of students enrolling in each type of English course. I define "on-time" to be students who enrolled in community college during the first year after high school graduation. I focus on the student's first semester of attendance during this first year. Although the focus of this paper is on transfer-level and remedial course participation, I include participation in the non-transferable, degree-credit courses, such that the graph represents all English course takers.[27]

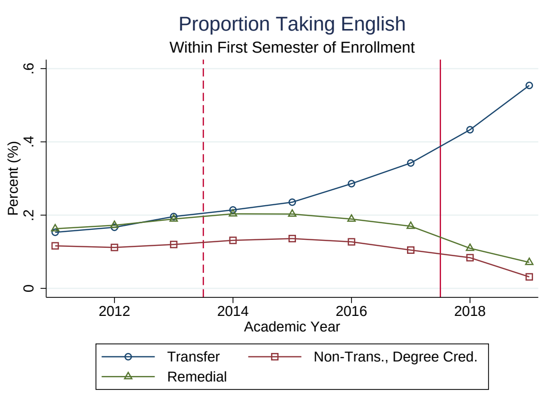


FIGURE 1.3. English Course Taking

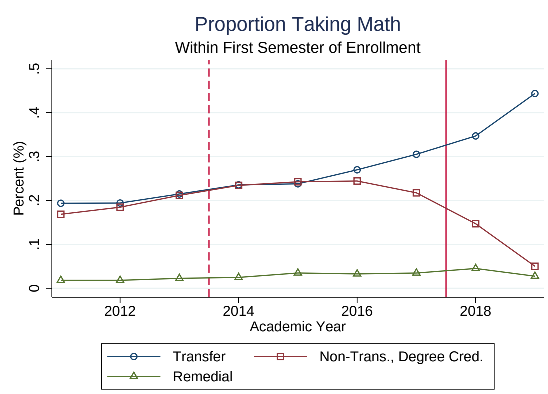


FIGURE 1.4. Math Course Taking

[27]There are also non-degree credit, non-remedial courses, but the proportion of students enrolled in those courses are relatively stable and close to 0 across all years. Graphs including those trends are included in Appendix A.

As seen in Figure 1.3, course participation rates in both transfer-level and remedial English are relatively flat from 2011-2013. After the implementation of the multiple measures mandate in 2013, represented by the red dashed line, there begins a steady increase in transfer-level course taking, as well as a slight decrease in remedial English course taking. By 2017, after AB 705, as represented by the solid red line, there are more precipitous increases in transfer-level English course participation, and decreases in remedial English course taking. There are similar, although somewhat muted, patterns regarding math course participation rates, as shown in Figure 1.4.

1.5.1. Composition Changes. It is possible that the changes in course participation rates as seen in Figure 1.3 and Figure 1.4 are not a result of policy changes, but rather changes in composition of the students enrolling in community college during each of the separate time periods. For example, if more students with higher abilities who could directly enroll in transfer-level courses regardless of any policy reforms decided to attend community college, then this could also explain the observed increases in the proportion of students taking transfer-level courses over time.

I first present summary statistics on the average demographic characteristics over the entire period of analysis, along with summary statistics within each time period, before, during, and after the policy changes of interest, in Table 1.1.

On average, there have been changes regarding the composition of students enrolling in community college over time. For example, the proportion of male, Asian, Black, and White students, along with average math standardized test score, have decreased over time, while the proportion of disabled, Hispanic, "Other Race," and socioeconomically disadvantaged students have increased. Furthermore, the predicted probability of remedial English for students enrolled in community college before any remedial education reform, is similar to the predicted probability of remedial English for students enrolled during remedial education placement reforms, but declines slightly by 0.005 percentage points on average for students enrolled after remedial education requirements were removed altogether. A similar pattern regarding the predicted probability of remedial math enrollment is seen across these three time periods as well, with students enrolled in community college after AB 705 having, on average, 0.004 percentage points lower predicted probabilities of math remedial enrollment. This could be concerning if patterns regarding changes in ability as proxied by standardized test scores follow a similar pattern regarding the outcomes of interest.

Table 1.1. Student-Level Summary Statistics - Demographics

	All (F2011-SP2020) Mean	Before (F2011-SP2013) Mean	During (F2014-F2017) Mean	After (SP2017-SP2020) Mean
Male	0.498	0.501	0.499	0.491
	(0.500)	(0.500)	(0.500)	(0.500)
Disabled	0.0701	0.0602	0.0664	0.0914
	(0.255)	(0.238)	(0.249)	(0.288)
Asian	0.123	0.129	0.120	0.121
	(0.329)	(0.335)	(0.325)	(0.326)
Hispanic	0.509	0.464	0.521	0.549
	(0.500)	(0.499)	(0.500)	(0.498)
Black	0.0592	0.0652	0.0585	0.0522
	(0.236)	(0.247)	(0.235)	(0.222)
Other Race	0.0317	0.0290	0.0308	0.0374
	(0.175)	(0.168)	(0.173)	(0.190)
White	0.285	0.325	0.277	0.248
	(0.451)	(0.468)	(0.448)	(0.432)
Age (in months)	142.5	142.5	142.6	142.5
	(4.913)	(4.928)	(4.954)	(4.810)
Economic Disadvantage	0.535	0.492	0.550	0.566
	(0.499)	(0.500)	(0.497)	(0.496)
CST ELA Z-Score	-0.0221	0.00381	-0.0380	-0.0274
	(0.851)	(0.837)	(0.853)	(0.865)
CST Math Z-Score	-0.0590	-0.0368	-0.0622	-0.0806
	(0.904)	(0.976)	(0.888)	(0.839)
Pr(Remed. Eng.)	0.174	0.176	0.175	0.170
	(0.163)	(0.166)	(0.163)	(0.160)
Pr(Remed. Math)	0.163	0.165	0.164	0.160
	(0.150)	(0.148)	(0.151)	(0.150)
Observations	1213138	387780	549131	275857

Notes: Standard deviations are stated in parentheses.

Looking at average outcomes of interest across time for each time period, Table 1.2 shows that there is suggestive evidence that, while remedial education placement reforms might not have resulted in the course placement improvements intended, this was not the case with AB 705. Remedial course enrollment in both math and English actually increased slightly by 1.6 and 2.4 percentage points, respectively during the period of remedial education placement reform. However, after AB 705 was implemented, from 2017-2019, remedial English enrollment decreased by almost 10 percentage points, and remedial math enrollment decreased by 8.7 percentage points.

There are also large increases in transfer-level enrollment, for both English and math, and for both the intermediate period during remedial educational placement reforms, and after AB 705 was

Table 1.2. Student-Level Summary Statistics - Outcomes

	All (F2011-SP2020) Mean	Before (F2011-SP2013) Mean	During (F2014-F2017) Mean	After (SP2017-SP2020) Mean
Remed. Eng. Enrollment	0.163	0.173	0.190	0.0955
	(0.369)	(0.378)	(0.392)	(0.294)
Remed. Math Enrollment	0.166	0.170	0.194	0.107
	(0.373)	(0.376)	(0.395)	(0.309)
Transfer Eng. Enrollment	0.293	0.180	0.275	0.488
	(0.455)	(0.384)	(0.447)	(0.500)
Transfer Math Enrollment	0.158	0.102	0.142	0.267
	(0.364)	(0.302)	(0.349)	(0.443)
Pass Transfer-Level English	0.236	0.139	0.223	0.399
	(0.425)	(0.346)	(0.416)	(0.490)
Pass Transfer-Level Math	0.112	0.0707	0.103	0.186
	(0.315)	(0.256)	(0.304)	(0.389)
Non-Transfer, Degree-Credit Eng.	0.105	0.119	0.124	0.0470
	(0.306)	(0.323)	(0.330)	(0.212)
Non-Transfer, Degree-Credit Math	0.185	0.181	0.215	0.130
	(0.388)	(0.385)	(0.411)	(0.337)
Total Units Attempted	10.94	10.37	11.04	11.53
	(4.128)	(4.287)	(4.002)	(4.046)
Total Units Earned	7.857	7.618	7.955	8.002
	(5.190)	(5.073)	(5.177)	(5.365)
Transfer Units Attempted	9.084	8.090	9.087	10.47
	(4.334)	(4.315)	(4.206)	(4.224)
Transfer Units Earned	6.825	6.077	6.918	7.690
	(4.816)	(4.591)	(4.737)	(5.112)
Observations	1213138	387780	549131	275857

Notes: Standard deviations are stated in parentheses.

passed. However, it is also arguable that there are larger increases in transfer-level participation after AB 705 was passed, relative to the increases during the intermediate period after remedial education placement reforms in 2013. For example, there was a 52.7% increase in transfer English participation, and a roughly 40% increase in transfer math participation, from before any remedial reforms were implemented to the intermediate period during remedial education placement reforms. However, there were much larger increases in transfer-level participation when comparing the "before" period to the "after" period. Both English and math transfer-level participation experienced an over 100% increase, when comparing before any remedial education reforms to after the institution of remedial education was discouraged altogether.

I also observe encouraging increases in transfer-level course pass rates, as well as increases in the number of units attempted and earned, both overall and specifically transfer-level units.

Next, I disaggregate these summary statistics to focus on how these demographic characteristics trend over time, and whether these trends move smoothly over time. Again it might be concerning if there are large discrete changes in student composition at the same time as the policy changes, which could potentially be the true driver of effects observed, instead of the policy changes.

Figure 1.5 graphs these trends over time, plotting for each year the proportion of students enrolled for each demographic characteristic. The proportion of Asian, Black, and male students enrolled in community college are relatively stable over time, across all policy periods of interest. Although the proportion of Hispanic, white, and socioeconomically disadvantaged students are increasing over time, these changes are smooth across the vertical lines representing the year of policy reform, suggesting that these changes are not driving any changes in outcomes observed.

Demographic Trends over Time

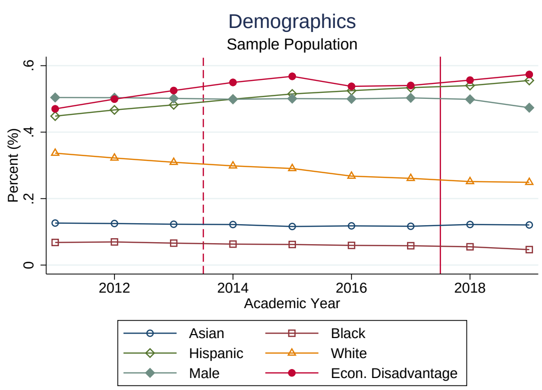


FIGURE 1.5. Analytic Sample

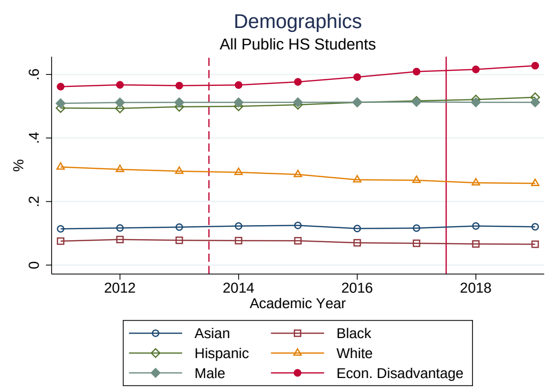
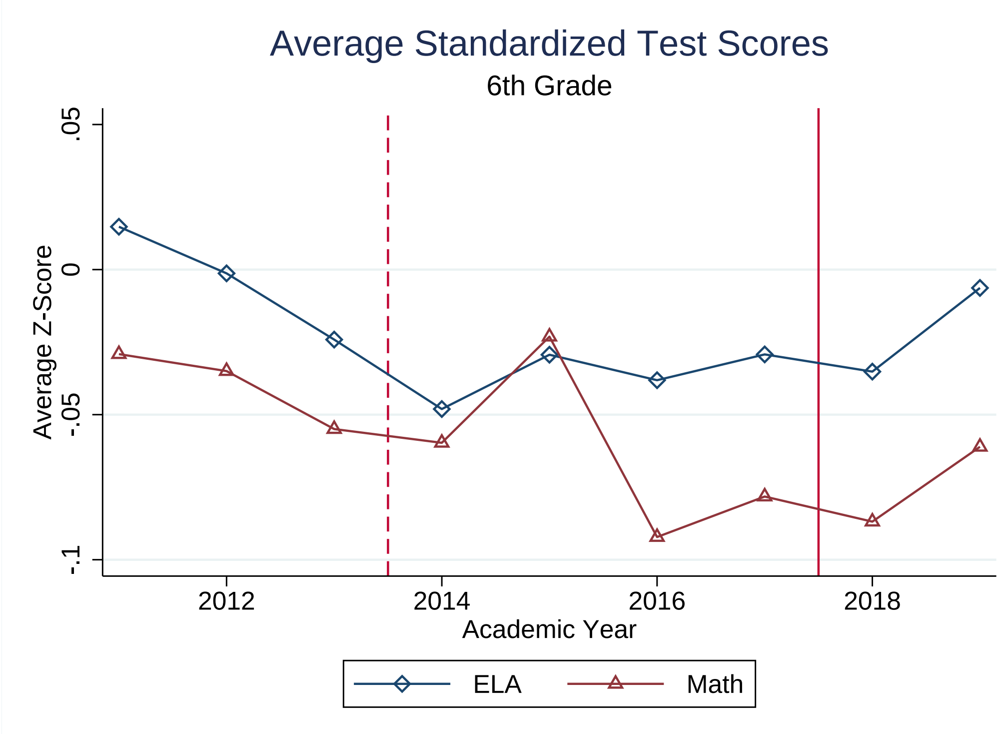


FIGURE 1.6. All Public High School Students

Furthermore, to show that these changes are merely reflective of overall demographic shifts in the state of California, I graph the demographic trends over time for all public high school students in California over this time, instead of only students who enroll in community college. Figure 1.6 displays very similar trends in demographic composition across time as students enrolled in community college, with slight decreases in the proportion of white students, and slight increases in the proportion of Hispanic and socioeconomically disadvantaged students. This suggests that any shifts in demographic composition are not driven by changes in selection by students enrolling in community college.

Figure 1.7. Average Standardized Test Scores over Time



I also graph the trends in average standardized test scores in both English (ELA) and math across time for each cohort in Figure 1.7. As previously mentioned, I have to use 6th grade exam scores as a result of data constraints. California switched from the California Standards Test (CST) exam to the Smarter Balance Academic Consortium (SBAC) exam in 2014. These tests are not comparable to each other. Consequently, to ensure that all students in my sample are taking the same exam during the same grade, I must use students' 6th grade exam scores. In addition, to compare these exam scores across cohorts, I standardize each cohort's test scores by finding their z-score.[28]

To further investigate whether the observed changes in course participation could stem from a change in the combination of demographic and ability variables, I predict the probability of enrolling in a transfer-level English or math course. I use various demographic variables, such as

[28] $z = \frac{x-\bar{x}}{\sigma_x}$, where σ is the standard deviation, and $\bar{x}$ is the mean.

race, gender, socioeconomic status, and ELA and math standardized test scores to predict transfer-level course taking in either English and math for students enrolled in community college in the period before any policy change. I then use that prediction model to estimate the proportion of students likely to take transfer-level courses based on these variables alone. This exercise is to show that, if changes in these demographic and ability variables are actually the reason behind the observed course selection changes, then these predictions should be able to project a similar trend as the observed course selection changes.

Predicted Transfer-Level Course Taking

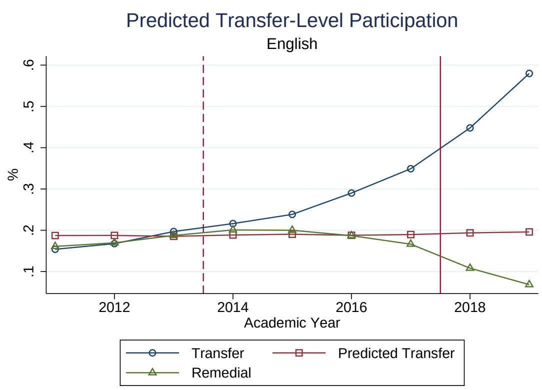


FIGURE 1.8. English

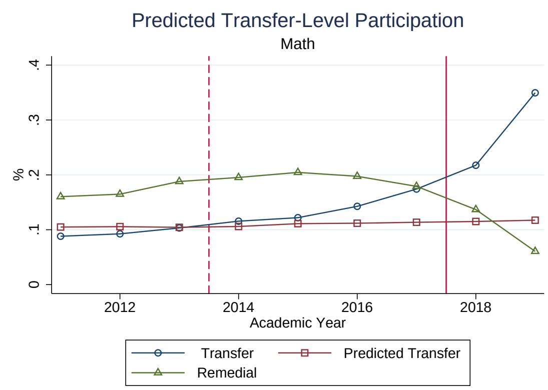


FIGURE 1.9. Math

I graph the average likelihood of taking a transfer-level course by year, along with the actual proportion of students taking a transfer-level course. As Figure 1.8 shows, the predicted proportion of students enrolled in transfer-level English based on demographic and ability characteristics alone is very stable and flat across all time periods, relative to the actual proportion of students enrolled in transfer-level English.

Similarly, Figure 1.9 shows that the predicted proportion of students enrolled in transfer-level math based on demographic and ability characteristics is incredibly flat across all time periods, especially when comparing to the actual transfer-level course participation observed over time. These graphs provide evidence that changes in transfer-level course participation in either English or math do not stem from composition changes in the students deciding to enroll in community college across time, and instead are likely driven by changes in policy.

1.6. Results

I investigate how students are affected by the two sets of policy changes. The first policy change consisted of the multiple measures mandate implemented in 2013. As hypothesized earlier, this measure is likely to affect students who had higher levels of academic preparation. In contrast, AB 705 with its effective removal of remedial education requirements, is likely to affect most the students with the lowest level of academic preparation.

1.6.1. Overall. I first examine how these two bundles of policies affected all students on average. Table 1.3 displays the average treatment effects of each policy period. The "Pass with a C" outcome is a binary measure, and equals 1 if a student received a C or better in a transfer-level course in English or math respectively, and 0 if otherwise. As many students might not elect to enroll in transfer-level courses, I assign those students a 0 as well for this variable, and thus capture the intent-to-treat (ITT) effect of the policies.

"During" represents the initial period of policy reform from Fall 2014 - Fall 2017, covering the multiple measures mandate. Following those two reforms, remedial English enrollment increases 1.6 percentage points, while transfer-level English enrollment increases 11 percentage points, and the probability of passing transfer-level English with a C or better increases by 9.7 percentage points. The treatment-on-the-treated effect (TOT) is 88.2% (0.97/0.110), suggesting that, conditional on enrolling in an English transfer-level course, 88.2% of those students passed with a C or better. This is considerably higher than the transfer-level English pass rate during the period before any policy was implemented, at 77.22% (0.139/0.180).

Next studying students enrolled in the "after" period (i.e. after the implementation of AB 705 in Fall 2017), there are significantly larger effects on course selection. There is, a large decline in the proportion of students enrolling in remedial English after AB 705, of 7.4 percentage points, suggesting that the policy did have the intended effect. There is a comparatively larger effect on transfer-level English enrollment, at a 31 percentage points, with a corresponding increase in the probability of passing transfer-English of 26.5 percentage points. This translates to a TOT effect of 85.48% (0.265/0.310), which again is higher than the transfer-level English pass rate of 77.22% before any course selection policy was implemented. This suggests that, if we assume the influx of

TABLE 1.3. Changes in Course-Taking

	Any	Remedial	Transfer-Level	Pass C
English Course Taking	(1)	(2)	(3)	(4)
During	0.127***	0.016**	0.110***	0.097***
	(0.010)	(0.007)	(0.007)	(0.006)
After	0.156***	-0.074***	0.310***	0.265***
	(0.014)	(0.012)	(0.014)	(0.010)
Average	0.472	0.173	0.180	0.139
Observations	951506	951506	951506	951506
Student Controls	X	X	X	X
High School FE	X	X	X	X
College FE	X	X	X	X
Predicted Treatment Intensity	X	X	X	X
Math Course Taking	(5)	(6)	(7)	(7)
During	0.100***	0.024***	0.044***	0.036***
	(0.009)	(0.005)	(0.004)	(0.003)
After	0.043***	-0.057***	0.161***	0.112***
	(0.012)	(0.010)	(0.010)	(0.007)
Average	0.455	0.170	0.102	0.071
Observations	951506	951506	951506	951506
Student Controls	X	X	X	X
High School FE	X	X	X	X
College FE	X	X	X	X
Predicted Treatment Intensity	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

students enrolling in transfer-level English were indeed all students who would have been placed in remedial education in the regime before AB 705 was implemented, then 85% of them could have passed transfer-level English had they not been recommended to take remedial English.

Next, I conduct a similar analysis on math course enrollment. During the intermediate period during which the multiple measures mandate was passed, students were more likely to be enrolled in remedial math by 2.4 percentage points, more likely to be enrolled in transfer-level math by 4.4 percentage points, and also more likely to pass transfer-level math by 3.6 percentage points, than students who were enrolled in comunity college before any remedial education reform was passed. With respect to the effects observed during the same period for English course taking, the results for math course taking are comparatively smaller. The TOT effect for students passing transfer-level math is 81.81% (0.036/0.044), which is much higher compared to the pre-policy period pass rate of 69% (0.071/0.102).

There are also comparatively larger effects after AB 705 passed in 2017 relative to the "during" period. There is a decrease in the probability of enrolling in a remedial math course of 5.7 percentage points, an increase in the proportion of the students taking a transfer-level math course of 16.1 percentage points, and a corresponding increase of passing transfer-level math with a C or better of 11.2 percentage points. The TOT effect is 69.5% (0.112/0.161), which is comparable to the pre-policy transfer-level math course pass rate of 69%.

Finally, I look at how these policies affected overall course load for students. I find that the number of both overall units, which include remedial courses in its count, and transfer-level units earned and attempted increased across both time periods. That the increase in overall units is smaller than the increase in transfer-level units, but still positive, suggests that although some students fully substituted remedial courses for transfer-level courses, some might have attempted other additional transfer-level courses, and thus attempting more units overall.

I find that that overall course completion rates were larger in the "during" period (51%) than the "after" period (28.7%), but both overall course completion rates were lower than the before policy overall course completion rate of 73.5%. However, these overall course completion rate comparisons might be somewhat misleading, as students could be substituting remedial education courses for transfer-level courses in a multitude of ways. In contrast, the transfer-level course completion rate during the intermediate period of policy reform (Fall 2014 - Fall 2017) of 84.1% (0.872/1.037) is actually higher than the analogous completion rate of 75% before any policy change (Fall 2011 - Spring 2013), as well as the transfer-level completion rate of students after AB 705 passed at 66.5%. This result suggests that after AB 705 was passed, students might have been attempting more transfer-level units than they could handle.

Taking all of these results together, there are two noticeable patterns. First, that relative to English course-taking results, there are much smaller effects on math course-taking. Anecdotal evidence indicates that students are more hesitant to take transfer-level math courses[29] and that

[29] "Overcoming Math Anxiety." Mission College Santa Clara. *https://missioncollege.edu/depts/math/math-anxiety.html.* Accessed 07 Jan 2022.

advisors are likely to suggest below transfer-level math placement for students with lower levels of college readiness (Cuellar Meija et al., 2021)

Second, another interesting pattern is that TOT effects on transfer-level pass rates, as well as transfer-level course completion rates, tend to be higher during the intermediate period of reform (Fall 2014 to Fall 2017) when the multiples measure mandate was implemented, compared to the TOT effects for students after AB 705 was passed. This points to suggestive evidence that each set of policies affect students at opposite ends of the college readiness scale. In other words, the remedial education placement reforms affects students on the margin of requiring remedial education, or with higher levels of academic preparation, while AB 705 affects students with lower levels of academic preparation.

In order to test this hypothesis rigorously, I next conduct a heterogeneity analysis, grouping students by their predicted probability of enrolling in a remedial course, a proxy for perceived college readiness.

1.6.2. Heterogeneity Analysis. To conduct the heterogeneity analysis, I split the sample into four quartiles, based on students' predicted probability of enrolling in remedial education (predicted treatment intensity), separately for English and math. Students in the first quartile are those who are deemed the most academically prepared, and students in the fourth quartile are those who are deemed the least academically prepared, as under the old remedial education system before any policy change, which focused on standardized exams.

1.6.2.1. *Course Taking.* I first graph the proportion of students within each predicted probability quartile enrolled in remedial English and math, respectively, in Figure 1.10 and Figure 1.11. The red dashed line represents when the multiple measures mandate was implemented in 2013, and the red solid line represents when AB 705 was implemented in 2017. Although there are moderate increases in both the proportion of students taking remedial English and remedial math before any policy change for students in 3rd and 4th quartile, the proportion of students enrolled in remedial English and math is relatively stable for students in the 1st and 2nd quartiles, or the students who are deemed most academically prepared. There is a slight decrease in the proportion of students in the 4th quartile taking remedial English and math in the intermediate period, and slight increases in the proportion of students in the 3rd and 2nd quartile taking remedial English and math, while the proportion of students in the 1st quartile stays relatively steady and close to zero.

Remedial Course Taking

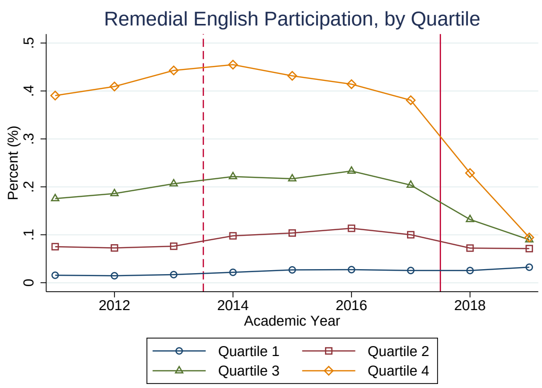


FIGURE 1.10. English

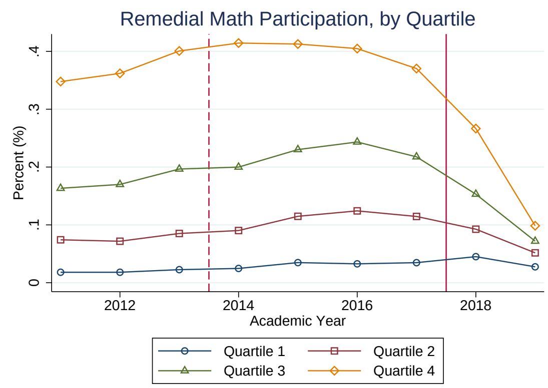


FIGURE 1.11. Math

However, the most interesting trends are observed in the period after AB 705 was passed, with large declines in remedial participation concentrated among students in the 3rd and 4th quartiles, or the quartiles of students deemed the least college ready.

Transfer-Level Course Taking, by Quartile

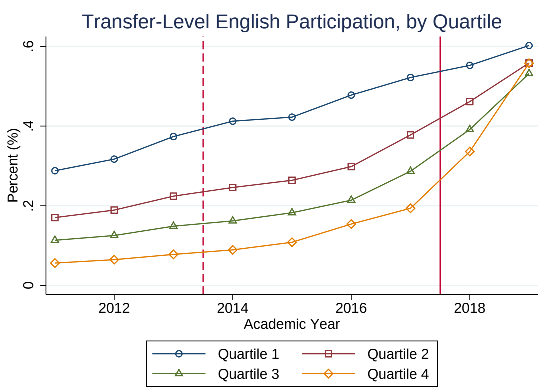


FIGURE 1.12. English

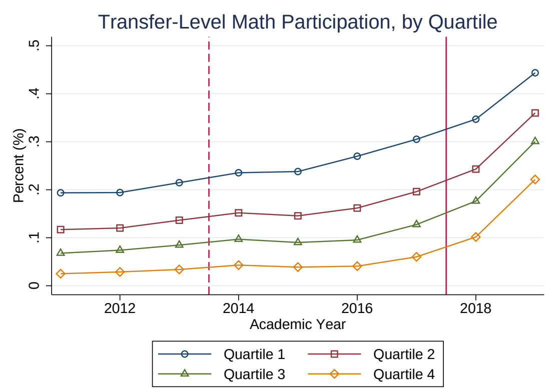


FIGURE 1.13. Math

Similarly, Figure 1.12 and Figure 1.13 graph transfer-level course participation in both English and math by quartile, respectively. Again, there is a gradual positive trend in English transfer-level participation among students in the 1st quartile, although for the other quartiles, transfer-level

English participation is relatively flat before any policy change, from 2011-2013. With respect to math transfer-level participation trends, the proportion of students enrolled remains relatively stable for all quartiles. However, for both English and math, there is a steady increase in transfer-level course participation during the intermediate period of policy change. In contrast, after AB 705 was passed, while there are increases in transfer-level participation at steeper rates for students in the 2nd and 3rd quartile than for students in the first quartile, the sharpest increase observed is for students in the 4th quartile, or students deemed least prepared for college.[30] There is not, however, a similar pattern observed for math transfer-level participation by quartile; although there are increases in transfer-level participation across all quartiles, the largest participation rate increase is not concentrated among students in the 4th quartile.[31]

Transfer-Level Course Pass Rates, by Quartile

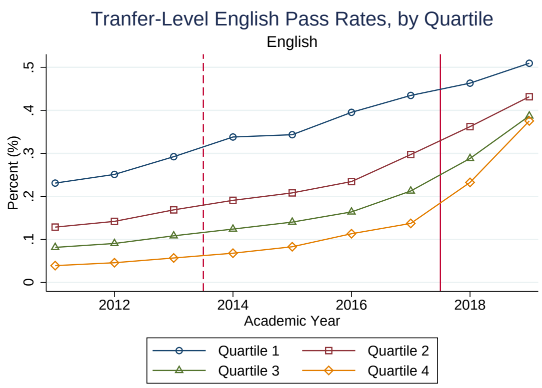


FIGURE 1.14. English

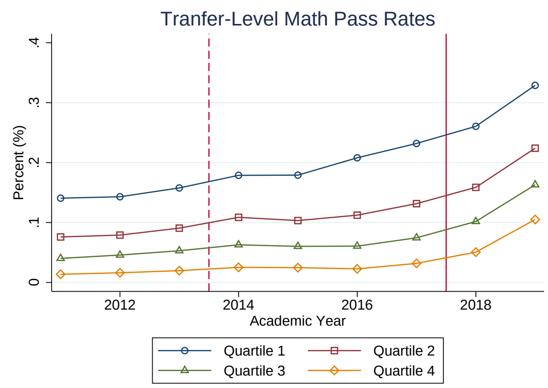


FIGURE 1.15. Math

Figure 1.14 and Figure 1.15 show that overall trends in transfer-level course pass rates follow similar patterns as the trends in transfer-level course participation.

Table 1.4 shows how each quartile of students were affected by the policy changes. Again, as noted earlier, students in the 1st, 2nd, and 3rd quartiles experienced a slight increase in remedial English participation, from 0.9 - 2.8 percentage points, during the intermediate period of policy change from Fall 2014 - Fall 2017. However, after AB 705 was implemented, the overall decline

[30] The observed increase in transfer-level English course participation rates after the implementation of AB 705 were 0.040, 0.098, 0.122, and 0.221, for quartiles 1, 2, 3, and 4, respectively.

[31] In contrast, the observed increase in transfer-level math course participation rates after the implementation of AB 705 were 0.07, 0.116, 0.125, and 0.08, for quartiles 1, 2, 3, and 4, respectively.

in remedial English participation as observed in Table 1.3 is driven by large declines in English remedial participation by students in the top two quartiles, or the students who are deemed the least academically prepared. Furthermore, it is students in the 4th quartile, who are 23 percentage points less likely to be enrolled in English remediation.

Table 1.4. English Course Taking, By Quartile

	Overall	1st Qrt	2nd Qrt	3rd Qrt	4th Qrt
Remedial English	(1)	(2)	(3)	(4)	(5)
During	0.016**	0.009**	0.028***	0.028***	0.005
	(0.007)	(0.004)	(0.009)	(0.010)	(0.015)
After	-0.073***	0.012***	-0.004	-0.076***	-0.230***
	(0.012)	(0.004)	(0.009)	(0.012)	(0.021)
Average	0.173	0.016	0.076	0.194	0.420
Transfer-Level English					
During	0.110***	0.139***	0.108***	0.093***	0.080***
	(0.007)	(0.014)	(0.010)	(0.009)	(0.009)
After	0.310***	0.237***	0.293***	0.319***	0.370***
	(0.014)	(0.021)	(0.018)	(0.017)	(0.022)
Average	0.180	0.333	0.201	0.134	0.069
Observations	951506	239698	238324	237718	234091
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

Looking at English transfer-level course participation in Table 1.4, there are large increases in transfer-level English course participation across all quartiles, for both time periods. Comparatively, the increases in participation are larger for students enrolled in the "after" period relative to students enrolled in the "during" period. Furthermore, there are additional patterns that support the hypothesis that the multiple measures mandated implemented during the intermediate policy period affected more students who were more academically prepared, and AB 705 affected more strongly students who were deemed less academically prepared. The increase in transfer-level participation increases at a decreasing rate across quartiles, for students enrolled in the "during" period, while the opposite pattern is observed for transfer-level participation during the period AB

705 was passed, with students in the 4th quartile experiencing the largest increase in transfer-level English participation.

Finally, Table 1.5 displays the effects of each policy period on whether or not students pass transfer-level English with a C or better. For the intermediate policy period, the increase in the probability of passing transfer-level English is positive across all four quartiles of students, but declines moving from students with the highest level of academic preparation in the 1st quartile to students with the lowest level of academic preparation in the 4th quartile. In contrast, after AB 705 was passed, the observed pattern is reversed, with students in the 4th quartile experiencing the largest increase in the probability of passing transfer-level English with a C or better, at 28.4 percentage points.

Table 1.5. English Pass Rates, By Quartile

Pass Rates	Overall (1)	1st Qrt (2)	2nd Qrt (3)	3rd Qrt (4)	4th Qrt (5)
During	0.097***	0.129***	0.095***	0.081***	0.068***
	(0.006)	(0.011)	(0.008)	(0.007)	(0.007)
After	0.265***	0.229***	0.258***	0.269***	0.284***
	(0.010)	(0.015)	(0.011)	(0.012)	(0.014)
Conditional Pass Rates					
Before	0.772	0.793	0.774	0.758	0.806
During	0.882	0.928	0.880	0.871	0.850
After	0.855	0.966	0.881	0.843	0.768
Average	0.139	0.265	0.154	0.100	0.054
Observations	951506	239698	238324	237718	234091
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

When looking across all four quartiles, the pass rate conditional on actually taking a transfer-level English course (TOT effect) is higher for students enrolled in the "during" period than the "before" period. This pattern is similar for students enrolled in the period after AB 705 was passed, except for students in the fourth quartile, or students deemed the least college prepared, with respect to the old remedial education placement system. This finding suggests that some of the students in the fourth quartile who enrolled in transfer-level English might not yet have been prepared to take that course.

I repeat the same analysis for math course taking. Overall, there are similar, though more muted patterns observed for math course taking. For example, Table 1.6 shows that, similar to English remedial course taking, there are small increases in math remedial participation across all quartiles during the intermediate policy change. In addition, after AB 705, there is again, an overall decrease in remedial math participation, driven by decreases in participation by students in the 3rd and 4th quartile particularly. For example, students in the 4th quartile after AB 705 was passed were 17.8 percentage points less likely to enroll in remedial math compared to students in the 4th quartile enrolled before any remedial education reform.

TABLE 1.6. Math Course Taking, By Quartile

	Overall	1st Qrt	2nd Qrt	3rd Qrt	4th Qrt
Remedial Math	(1)	(2)	(3)	(4)	(5)
During	0.024***	0.012***	0.033***	0.039***	0.017*
	(0.005)	(0.003)	(0.007)	(0.009)	(0.010)
After	-0.056***	0.020**	-0.001	-0.062***	-0.178***
	(0.010)	(0.009)	(0.008)	(0.009)	(0.018)
Average	0.170	0.020	0.080	0.182	0.379
Transfer-Level Math					
During	0.045***	0.058***	0.042***	0.033***	0.024***
	(0.004)	(0.009)	(0.006)	(0.005)	(0.003)
After	0.161***	0.158***	0.160***	0.157***	0.134***
	(0.010)	(0.016)	(0.011)	(0.011)	(0.012)
Average	0.102	0.205	0.129	0.077	0.030
Observations	951506	228026	241467	242209	238062
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

With respect to math transfer-level course taking, I observe similar patterns in Table 1.6 to that of English transfer-level course taking in Table 1.4. For example, there are increases in transfer-level math participation across all quartiles, and for both time periods. Furthermore, participation increases at a decreasing rate from students in the first quartile, who are deemed most college ready, to students in the fourth quarter, who are deemed the least college ready, in the intermediate policy period. However, with respect to transfer-level participation after AB 705 was passed, the increase in participation rates is no longer increasing across quartile. Although the changes in enrollment are positive across all quartiles, the changes are relatively similar across the 1st, 2nd, and 3rd quartiles, with a slightly smaller increase for students in the 4th quartile. In fact, the increase in transfer-level math enrollment for students in the 4th quartile does not offset the decrease in remedial math participation, suggesting that despite open access, students who are deemed least ready for college math are the most hesitant to enroll in transfer-level math.

Table 1.7. Math Pass Rates, By Quartile

Pass Rates	Overall (1)	1st Qrt (2)	2nd Qrt (3)	3rd Qrt (4)	4th Qrt (5)
During	0.036***	0.049***	0.036***	0.024***	0.016***
	(0.003)	(0.007)	(0.005)	(0.003)	(0.002)
After	0.112***	0.126***	0.112***	0.100***	0.079***
	(0.007)	(0.012)	(0.009)	(0.007)	(0.007)
Conditional Pass Rates					
Before	0.696	0.741	0.672	0.636	0.667
During	0.800	0.845	0.857	0.727	0.667
After	0.696	0.797	0.700	0.637	0.590
Average	0.071	0.152	0.086	0.049	0.020
Observations	951506	228026	241467	242209	238062
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

Table 1.7 next shows how students fared in transfer-level math courses after the implementation of the varying course-selection policies. Again, there are increases in the proportion of students passing transfer-level math with a C better across all quartiles and in both time periods. Similar to changes in English transfer-level pass rates, during the period of intermediate policy change, there are increases in math transfer-level pass rates at a decreasing rate, as students are less and less academically prepared (moving from the 1st quartile to the 4th). Unlike English transfer-level pass rates in the period after AB 705 was passed, math transfer-level pass rates follow a similar pattern as math transfer-level pass rates for students enrolled in the "during" period.

1.6.2.2. *Course Load.* Another outcome of interest is overall course load. Although course selection policies directly affected remedial and transfer-level English and math courses, it is possible that students shifted their other course taking in addition. For example, students might opt to take fewer courses overall as a response to taking more time-intensive, difficult transfer-level courses. On the other hand, students who no longer have to enroll in remedial courses might substitute transfer-level courses for those remedial courses instead, and might even be inclined to take additional transfer-level courses.

Focusing on changes in overall transfer-level units taken by English readiness quartiles,[32] I find that there are increases in both transfer-level units attempted and earned across both time periods, and across all quartiles, as seen in Table 1.8.

However, it is uncertain if these results are merely reflective of the increases in English transfer-level course taking. Considering increases in transfer-level English and math and decreases in remedial English and math were observed, it should be expected to see increases in overall transfer-level course taking. Roughly, if students in the 4th quartile experienced a 37 percentage point increase in the probability of enrolling in transfer-level English, then a 1.11 unit increase in overall attempted transfer-level course taking is expected for students in the 4th quartile.[33] In contrast, students are, on average, enrolling in an additional 2.749 transfer-level units, suggesting that, in addition to enrolling in transfer-level English, students in the 4th quartile are also attempting other transfer-level courses.

Potentially more informative are the transfer-level course completion rates, which I calculate by dividing the increase in transfer-level units earned by the increase in transfer-level units attempted. The transfer-level course completion rate by students enrolled during the intermediate policy period are, across all quartiles and overall, higher than that of students enrolled before the course-selection policies of interest. In contrast, there are much lower transfer-level course completion rates for all quartiles of students enrolled in community college after the implementation of AB 705, compared

[32] I conduct a similar analysis for math transfer-level units. However, results are largely similar, due to the fact that students in a particular quartile based on predicted English remedial enrollment is likely in the same quartile based on predicted math remedial enrollment.

[33] On average, a course is 3 units. 0.37 * 3 = 1.11

Table 1.8. Transfer Units, By English Quartile

	Overall	1st Qrt	2nd Qrt	3rd Qrt	4th Qrt.
Transfer Units Attempted	(1)	(2)	(3)	(4)	(5)
During	1.037***	0.826***	0.925***	1.022***	1.218***
	(0.054)	(0.095)	(0.071)	(0.066)	(0.063)
After	2.361***	1.841***	2.191***	2.448***	2.749***
	(0.081)	(0.133)	(0.096)	(0.094)	(0.113)
Average	9.09	9.52	8.36	7.67	7.12
Transfer Units Earned					
During	0.872***	0.719***	0.787***	0.823***	1.013***
	(0.039)	(0.066)	(0.055)	(0.051)	(0.061)
After	1.571***	1.403***	1.473***	1.514***	1.665***
	(0.062)	(0.101)	(0.080)	(0.067)	(0.091)
Average	6.92	7.71	6.35	5.67	5.09
Observations	951506	239698	238324	237718	234091
Completion Rate					
Before	0.761	0.810	0.760	0.739	0.715
During	0.841	0.870	0.851	0.805	0.832
After	0.665	0.762	0.672	0.618	0.606
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

to students enrolled in community college during the intermediate period of policy reform, and students enrolled before any reform. These results again lend support that the policies implemented during the intermediate period of policy change affected students of the highest academic readiness the most, and AB 705 affected students deemed least college ready the most.

That the coefficients for overall units (not just transfer-level) in Table 1.9 are positive, but not as large as increases in transfer-level units taken suggest that while some students are merely substituting their remedial course for a transfer-level course, some students are also taking *additional* transfer-level courses. That is, if all students were only substituting remedial courses and transfer-level courses one to one, then the expectation would be that there are no changes in overall course load. Together, these results taken together provide suggestive evidence that the two policy periods affected students at different levels of college readiness. The multiple measures mandate

TABLE 1.9. Overall Units, By English Quartile

	Overall	1st Qrt	2nd Qrt	3rd Qrt	4th Qrt.
Total Units Attempted	(1)	(2)	(3)	(4)	(5)
During	0.696***	0.641***	0.733***	0.684***	0.707***
	(0.069)	(0.096)	(0.086)	(0.073)	(0.097)
After	1.130***	1.069***	1.125***	1.157***	1.155***
	(0.091)	(0.115)	(0.104)	(0.098)	(0.145)
Average	11.04	10.93	10.52	10.24	10.16
Total Units Earned					
During	0.355***	0.470***	0.394***	0.253***	0.293***
	(0.044)	(0.072)	(0.060)	(0.052)	(0.059)
After	0.324***	0.680***	0.380***	0.173**	0.035
	(0.064)	(0.081)	(0.085)	(0.077)	(0.101)
Average	7.95	8.84	7.88	7.43	7.125
Observations	951506	239698	238324	237718	234091
Completion Rate					
Before	0.720	0.809	0.749	0.726	0.701
During	0.510	0.733	0.538	0.370	0.414
After	0.287	0.636	0.338	0.150	0.030
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

affected students who were deemed more prepared for college work, while AB 705 most affected students at lower levels of college readiness.

I find that, overall, there were increases in the proportion of students across all levels of college readiness who enrolled and passed transfer-level English and math with a C or better, after the introduction of AB 705, despite mostly affecting students deemed less academically prepared for college. When examining the TOT effect, I find that almost all students across a range of college readiness were able to pass transfer-level English (and math) courses at similar, or higher, rates than students enrolled in community college before either set of policy changes. This does not hold, however, for students in the 4th quartile of both the predicted probability of enrolling in English and math, respectively. This result suggests that there are some students who are among the least prepared that are enrolling in transfer-level English or math that are not ready to do so.

1.6.3. By Treatment Intensity. Finally, I use the predicted probability of enrolling in remedial English (math) as a continuous treatment variable, and interact it with the "during" and "after" policy variables, estimating the following equation:

$$\begin{aligned} Y_{ihcts} &= \alpha + \beta_1[\text{during}_t * \hat{\text{T}}_{ihcs}] + \beta_2[\text{after}_t * \hat{\text{T}}_{ihcs}] \\ &\quad + \beta_3[\text{during}_t] + \beta_4[\text{after}_t] + \beta_5[\hat{\text{T}}_{ihcs}] \\ &\quad + \lambda_c + \lambda_h + X_{ihc} + \epsilon_{ichts} \end{aligned} \tag{1.3}$$

where each observation is unique at the student i-semester t level, and Y_{icts} represents both continuous and binary outcomes, such as the total number of units taken in a semester or whether or not a student passed a transfer-level course for subject s (English or math). The variable $during_t$ is an indicator variable which is 1 if the student is enrolled in community college during the initial remedial education reform period, where reforms centered on the method of remedial education placement, during 2013-2017. The variable $after_t$ is an indicator variable which is 1 if the student is enrolled in community college after the passing of AB 705, during 2018-2019. Finally, $\hat{\text{T}}_{ics}$ is the predicted treatment intensity, and is a continuous measure ranging from 0 to 1. The larger $\hat{\text{T}}_{ics}$, the more likely a student is predicted to have enrolled in a remedial education course in subject s within the first year of enrollment.

The coefficients of interests are β_1 and β_2, and fixed effects λ_c and λ_h are estimated at the college and high school level. X_{ihc} is a vector of controls, including a linear time trend, and student controls for gender, race, age (in months), socioeconomic disadvantage status, and 6th grade standardized test scores in both ELA and math.

The purpose of this exercise is to try and isolate the effects of AB 705 as a policy on its own. Considering the suggestive evidence that AB 705 had disparate effects on students with the highest predicted probability of enrolling in remedial education, and should have not affected students with the highest academic preparation (or the lowest predicted probability of remedial education enrollment) as much. Thus, the expectation would be to see effects increase as the predicted probability of remedial enrollment increased.

Table 1.10 looks at these effects treating college readiness as a continuous variable. Looking at effects during the intermediate policy period, there is no longer a statistically significant change in

TABLE 1.10. Course Taking, By Treatment Intensity

English	Any (1)	Remedial (2)	Transfer-Level (3)	Pass C (4)
During × Pr(Remed. Eng.)	-0.108**	-0.029	-0.112***	-0.111***
	(0.044)	(0.040)	(0.033)	(0.027)
1 S.D. Effect	[-1.7%]	[-0.4%]	[-1.8%]	[-1.7%]
After × Pr(Remed. Eng.)	-0.143**	-0.624***	0.291***	0.123***
	(0.058)	(0.052)	(0.070)	(0.045)
1 S.D. Effect	[-2.3%]	[-9.9%]	[4.7%]	[2.0%]
Average	0.472	0.173	0.180	0.139
Observations	951506	951506	951506	951506
Student Controls	X	X	X	X
High School FE	X	X	X	X
College FE	X	X	X	X
Math				
During × Pr(Remed. Math)	-0.146***	-0.027	-0.055***	-0.055***
	(0.035)	(0.034)	(0.017)	(0.014)
1 S.D. Effect	[-2.3%]	[-0.4%]	[-0.3%]	[-0.8%]
After × Pr(Remed. Math)	-0.323***	-0.549***	-0.057	-0.100***
	(0.047)	(0.072)	(0.049)	(0.030)
1 S.D. Effect	[-5.2%]	[-8.8%]	[-0.9%]	[-1.6%]
Average	0.455	0.170	0.102	0.071
Observations	951506	951506	951506	951506
Student Controls	X	X	X	X
High School FE	X	X	X	X
College FE	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

the proportion of students taking remedial English as the predicted treatment intensity increases.[34] There are statistically significant decreases in both transfer-level English course taking and the corresponding pass rate, corresponding to that in Table 1.4, all quartiles of students experience large increases in English transfer-level participation, but at a decreasing rate. It is reassuring that the decrease in passing with a C or better is almost exactly the same as the decrease in transfer-level English participation, suggesting that the decrease in pass rate is a result of the decrease in course participation.

[34]In Table 1.4 the increases in remedial English participation are not increasing when moving from the 1st quartile to the 4th quartile.

Focusing on students after AB 705 was passed, there are large reductions in enrolling in remedial English and math, that increase as the predicted probability of remedial enrollment increases. For example, a one standard deviation increase of 16% in the predicted probability of enrolling in remedial English leads to a 9.9% decrease in the probability of actually enrolling in remedial English, and a 4.7% increase in the probability of enrolling in transfer-level English. Furthermore, the one-standard-deviation ITT effect on the transfer-level English pass rate is 2%. That the increase in transfer-level English participation is smaller than the decrease in remedial English participation suggests that as the predicted probability of remedial English enrollment increases, students are somewhat more hesitant to take transfer-level English.

Again, there are similar patterns for math course taking, although all effects are smaller than that for English course taking. During the intermediate period of policy reform, there is no longer a statistically significant effect on remedial math enrollment. Although there are statistically significant reductions in both the transfer-level math enrollment and corresponding pass rate as the predicted probability of math remedial enrollment increases, the declines are exactly the same, suggesting that the decrease in pass rate is driven only by decreases in transfer-level math enrollment.

After AB 705, a one standard deviation increase in the predicted probability of enrolling in remedial math of 16% leads to a 8.8% decline in the probability of actually enrolling in remedial math. However, interesting to note is that there is no longer a statistically significant increase in the proportion of students enrolling in transfer-level math, *as the predicted probability of enrolling in remedial math increases.* Note in Table 1.6 that while there were positive effects of AB 705 on the proportion of students enrolling in transfer-level math for students in all quartiles, those increases slightly decreased moving from the 1st quartile to the 4th. Initially disappointing is seeing that the intent-to-treat effect of AB 705 is negative for passing transfer-level math with a C or better. However, in light of the quartile analysis in Table 1.7, this negative coefficient only indicates that there are still increases in the probability of passing transfer-level math, but that these increases are decreasing as the probability of remedial math (or predicted treatment intensity) increases.

These results make intuitive sense – students who are deemed less prepared are more hesitant to enroll in transfer-level courses despite open access to them. This can be seen from the fact that the decrease in remedial course enrollment was not completely offset by the increase in transfer-level enrollment. Furthermore, the declines in math transfer-level pass rates indicate that pass rates declined as students who were deemed less college prepared enrolled in transfer-level math.

It is important to note, however, that these results do not contradict the heterogeneity analysis by quartile. This treatment intensity analysis hypothesizes that effects from these policies change at an increasing rate along a continuous measure of college readiness. That is, it is not enough for there to be a level shift across all students, but that students with the highest predicted probability also experience the largest change.

TABLE 1.11. Overall Units, By Treatment Intensity

	Overall		Transfer-Level	
	Units Attempted	Units Earned	Units Attempted	Units Earned
English	(1)	(2)	(3)	(4)
During × Pr(Remed. Eng.)	0.081	-0.274	0.975***	0.853***
	(0.291)	(0.212)	(0.235)	(0.209)
1 S.D. Effect	[0.01]	[-0.44]	[0.16]	[0.14]
After × Pr(Remed. Eng.)	0.218	-1.394***	1.983***	0.573*
	(0.425)	(0.331)	(0.373)	(0.300)
1 S.D. Effect	[0.03]	[-0.22]	[0.32]	[0.09]
Average	10.37	7.62	8.09	6.08
Observations	951506	951506	951506	951506
Student Controls	X	X	X	X
High School FE	X	X	X	X
College FE	X	X	X	X
Math				
During × Pr(Remed. Math)	-0.279	-0.806***	1.230***	0.982***
	(0.237)	(0.208)	(0.276)	(0.262)
1 S.D. Effect	[-0.04]	[-0.13]	[0.20]	[0.16]
After × Pr(Remed. Math)	-0.300	-1.972***	2.277***	0.742**
	(0.366)	(0.324)	(0.469)	(0.371)
1 S.D. Effect	[0.05]	[-0.32]	[0.36]	[0.12]
Average	10.37	7.62	8.09	6.08
Observations	951506	951506	951506	951506
Student Controls	X	X	X	X
High School FE	X	X	X	X
College FE	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

Next, Table 1.11 shows how overall course load changed during the different policy periods, for students as they are deemed less and less prepared for college. Although overall units attempted did not increase during either policy, the number of units earned does decline after AB 705 was introduced, with the number of units earned declining as students are deemed less and less prepared for college. However, these seemingly negative effects hide changes in the *types* of courses being taken.

For example, after AB 705 was passed, a one standard deviation increase in the predicted probability of enrolling in remedial English of 16% leads to a 0.32 increase in the number of transfer-level credits attempted, and a 0.09 increase in the number of transfer-level credits earned.[35] These results together suggest that students who were most affected by AB 705, that is students who were deemed the least prepared for college, were likely substituting some of their remedial courses with transfer-level courses, but not all, leading to an overall decline in units taken, but an increase in transfer-level courses taken.

Studying effects of the policies by treatment intensity provide suggestive evidence that, as students' college readiness declines, the positive effects of the policies decline slightly as well. However, when considering these treatment intensity results in conjunction with the quartile results, it indicates that while students at all levels of college readiness experience positive results as a result of the reforms, these benefits decline as students' academic preparation decreases.

1.7. Conclusion

Remedial education is a costly practice implemented across the United States to address the flagging academic success of students deemed underprepared for college work. However, despite the widespread use of remedial education, previous empirical studies have found mixed results regarding its effect on students' academic success, as measured by persistence, units earned, and a multitude of other benchmarks. Thus, it is uncertain how a policy in California increasing open access to transfer-level courses for all students might affect the academic success of students. For example, if remedial education has positive effects on students' academic achievements, then allowing open access to transfer-level courses could result in lower course pass rates, lower course grades, and

[35] Results are very similar using the likelihood of requiring remedial math.

longer time to degree for students who would have benefited from remedial education. On the other hand, if remedial education had a null or negative effect on students' academic success by underplacing students, then allowing these students to have direct access to college-level courses could increase long-run student success outcomes, such as transferring to a 4-year college or earning an associate's degree.

I add to this literature by being the first to study the effects of remedial education reform policies for students on a continuous measure of college readiness, and for the state of California. In particular, I find that the multiple measures mandate passed in 2013 had smaller effects on course selection than did AB 705 passed in 2017. However, while the multiple measures mandate targeted students with more academic preparation, AB 705 influenced more students at lower levels of college readiness.

I find that, after AB 705, there are large reductions in the proportion of students taking remedial English and math, driven by students with the lowest estimated levels of academic preparation. However, during both policy periods, there are large increases in transfer-level participation, and pass rates, in both English and math, suggesting that many students who would have been placed in remedial courses could have passed transfer-level courses at similar or higher rates than students enrolled before any policy was passed. This result is true for students across all quartiles in both subjects, except for students in the 4th quartile, or students with the lowest level of academic preparation. Although these students are still experiencing increases in the probability of passing transfer-level English and math, the conditional pass rates are lower than the pass rates before any course selection policies were passed. This suggests that some students with lower academic preparation are taking transfer-level courses before they are adequately prepared.

Furthermore, as college readiness declines, I show that students are more hesitant to take transfer-level courses, despite open access, particularly in math. Anecdotal evidence has suggested that could be driven in part by counselors' hesitance in encouraging students to take transfer-level math. This finding might be somewhat concerning, particularly in light of the importance of "gateway momentum," and evidence suggesting that students students who are able to take and complete transfer-level, or "gateway" math and English courses, within their first year of enrollment are more likely to graduate with college credential.[36] My results suggest that if a major goal of these remedial education reforms were to help encourage student success, there may be other barriers, besides remedial education, in place.

I also find that there are increases in the number transfer-level units attempted and earned, during both periods and across all quartiles. However, the course completion rate of these courses is higher for students enrolled during the intermediate period, and lower for students enrolled after AB 705 was implemented, compared to students enrolled in the period before any policy change. These results suggest that allowing open access for students to enroll directly into transfer-level courses encourages students to take more transfer-level courses, although the additional courses might be more than some students can handle.

Overall, I find that these policies have a positive effect on transfer-level course participation and pass rates on students across all ranges of academic preparation, with the largest positive effects for students on the margin of requiring remedial education. A natural question to ask is, given these positive short-run effects, how might these policies affect long-run outcomes, such as transfer to a 4-year college, or receiving an associate degree, and the timeframe in which they complete these goals.

[36] Jenkins, Davis and Thomas Bailey. "Early Momentum Metrics: Why They Matter for College Improvement." CCRC. Feb. 2017. https://files.eric.ed.gov/fulltext/ED572783.pdf.

CHAPTER 2

Minimum Wage and Higher Education

"With Amendment 2 being passed, ... I'll still be able to help my mom and be able to (return to school and) pay for my classes." – Sade Andrews, in response to Florida passing a $15 minimum wage.[1]

2.1. Introduction

Minimum wage continues to be one of the most highly debated policies in the US. President Joe Biden campaigned on raising the federal minimum wage to $15 an hour, which would bind for over 27 million workers.[2] In just the past six years, 29 states have increased their minimum wage above the federal minimum wage. Moreover, over 50 localities (with over 30 from California) have adopted minimum wages greater than their state minimum wage.[3] Many of these policies have come with substantial raises in the minimum wage as well; for example Seattle adopted a $15 minimum wage in 2018, over double the current federal minimum wage of $7.25.[4]

In this study, we investigate how minimum wages influence student enrollment decisions into higher education. Minimum wage has a theoretically ambiguous effect on educational attainment. In a simple reservation wage model, a higher minimum wage may induce decreases in human capital attainment since the opportunity cost of forgoing employment is higher. However, as found in numerous settings, higher minimum wages can reduce equilibrium employment (Choi et al., 2020; Clemens and Wither, 2019; Jardim et al., 2017; Lordan and Neumark, 2018; Meer and West, 2016; Neumark et al., 2014, 2004; Sabia, 2009),[5] subsequently making it more difficult for job-seekers to find employment. Under sufficiently slack labor market conditions, individuals increase their enrollment into postsecondary education (Betts and McFarland, 1995; Boffy-Ramirez, 2017; Foote and Grosz, 2020; Ost et al., 2018).[6] Additionally, a higher minimum wage could help alleviate

[1] CNBC. "College students say raising the minimum wage to $15 an hour would be a game changer." https://www.cnbc.com/2020/11/25/college-students-say-a-15-minimum-wage-would-be-a-game-changer.html. Accessed 23 Mar. 2021.

[2] Business Insider. https://www.businessinsider.com/joe-biden-15-minimum-wage-election-economy%2Dpaychecks-low-workers-2020-10. Accessed 19 Nov. 2020.

[3] The Economic Policy Institute, https://www.epi.org/minimum-wage-tracker/. Accessed 19 Nov. 2020.

[4] Vox, https://www.vox.com/the-highlight/2019/7/13/20690266/seattle-minimum-wage-15-dollars. Accessed 19 Nov. 2020.

[5] The literature on employment overall is still highly contentious, with several other studies finding no impact of minimum wages on employment, including Card and Krueger (1994), Dube et al. (2010), and Addison et al. (2012), among others.

[6] Related work from Clemens et al. (2021) finds increases in educational requirements in online job postings in response to increases in minimum wage.

financial constraints for students pursuing a post-secondary education. Approximately 80% of community college students face unmet needs *despite* receiving financial aid, while over 80% of all college-goers receive some type of financial aid.[7] Further highlighting potential financial constraints, over 40% of enrolled college students work over 30 hours a week.[8] With in-state public university tuition being relatively cheap, even a small increase in minimum wage could represent a large shift in a student's budget constraint. For instance, the average annual in-state tuition for a community college in California in 2022 is $1,997.[9] For a 32-week school year, a year of tuition can be covered by simultaneously working 3.5 hours a week at $15 an hour. Indeed, many proponents of increased minimum wage have made a direct link to college tuition costs (Miao, 2020; Tempera, 2013; Watson, 2019).

Though an ample literature has investigated the employment effects of minimum wage, there is surprisingly little evidence regarding the role of minimum wage on educational attainment, particularly at the post-secondary level. This is perhaps due to limited comprehensive data availability on student outcomes and historically scant variation in minimum wage. Chaplin et al. (2003) find evidence that state minimum wages induce high school drop out in states without mandatory schooling age requirements and studies from Neumark and Wascher (1995a,b, 2003) also suggest increases in minimum wage results in decreases in years of schooling. To our knowledge, the only other study to explicitly investigate post-secondary enrollment comes from Lee (2020), who uses cross-border variation in state minimum wages to find decreases in community college enrollment in states with higher minimum wages.[10]

This study uses administrative student-level data from California paired with variation in city and county minimum wages to investigate the impact of minimum wages during a student's senior year in high school on post-secondary educational outcomes, including on-time enrollment and

[7]Institute for College Access & Success, 2009, `https://ticas.org/wp-content/uploads/legacy-files/legacy/files/pub/cc_fact_sheet.pdf`. Accessed 19 Nov. 2020.

[8]Georgetown University Center on Education and the Workforce, `https://cew.georgetown.edu/cew-reports/workinglearners/`. Accessed 19 Nov. 2020.

[9]Community College Review, 2022. `https://www.communitycollegereview.com/tuition-stats/california`. Accessed 18 May 2022.

[10]Evidence from New Zealand found that the introduction of a minimum wage led to increases in educational enrollment, while subsequent raises in the minimum wage decreased enrollment (Pacheco and Cruickshank, 2007). A recent working paper from Alessandrini and Milla (2021) uses Canadian survey data to find increases in community college enrollment and decreases in university enrollment in response to minimum wage.

units attempted. The data include detailed records on the universe of California high school students merged to their post-secondary outcomes using data from both the National Student Clearinghouse and transcript records from the California Community College (CCC) system.[11] Variation in minimum wage comes from both the timing of adoption and magnitude of minimum wage policies, which have occurred across over 30 California localities since 2015. This source of variation has also been used in recent studies to identify labor market impacts of minimum wage (Dube and Lindner, 2021; Even and Macpherson, 2019; Luca and Luca, 2019; Neumark and Yen, 2020).[12]

Our results show that changes in local minimum wage laws significantly affect post-graduation plans of high school seniors in California. We first find a statistically significant increase in postsecondary enrollment among high school graduates in response to an increase in local minimum wage. The size of the increase is, however, relatively small: A student's probability of postsecondary enrollment increases by less than one percent for a $1 increase in minimum wage. This effect is driven almost entirely by enrollment into the cheaper, less selective university system (CSUs), with a smaller effect on enrollment into the University of California (UC) system, and virtually no effect on either in-state private or out-of-state enrollment. We also find no change in overall enrollment in the state's two year community colleges.

Importantly, we next show that underlying this small increase in overall enrollment in response to increased minimum wage are significant shifts in the composition of college-going students across higher-education sectors. First, we find the minimum wage effect is largely driven by students from disadvantaged socioeconomic backgrounds and women. Assuming a marginal dollar is more impactful for lower income workers, and given that female workers are twice as likely as men to work at minimum wage,[13] this evidence suggests that higher minimum wage may alleviate financial

[11]To date, these data have been utilized by only a handful of studies, including Howell et al. (2010), Kurlaender et al. (2016), Naven (2019), Reed et al. (2019), and Kurlaender et al. (2020).

[12]Even and Macpherson (2019) find slower growth among county-industry pairs in counties with a higher share of low wage workers in response to minimum wage. Luca and Luca (2019) focus on the Bay Area to find that lower quality restaurants on Yelp shut down after their city increases minimum wage. Neumark and Yen (2020) provide some preliminary results from a pre-analysis plan which suggest some potential negative employment effects. Evidence from Dube and Lindner (2021) suggests that the effects from city-level minimum wage changes are broadly consistent with prior evidence of small but adverse labor market outcomes.

[13]Statista. "Share of workers by gender 1980-2020." `https://www.statista.com/statistics/185536/share-of-workers-paid-at-the-minimum-wage-by-gender/`. Accessed 22 Mar. 2021.

constraints for college-aspiring students who otherwise would not have pursued a college education. We also find that relatively stronger students (as proxied by high school standardized test scores) significantly increase their enrollment into the California's higher-tier public university system (UC) in response to a higher minimum wage, while relatively weaker students significantly increase their enrollment into the community college system.

Finally, we use our matched high school data to college transcripts to explore how first-year student outcomes at the California Community Colleges (CCCs) were impacted by local minimum wages. Results show that CCC-attending students significantly increase their enrollment units in response to a more generous local minimum wage. Given the marginally induced CCC-attending student comes from a more disadvantaged socioeconomic background, this result once again suggests that minimum wage may help alleviate financial constraints for college-going students.

Our results have several important implications for minimum wage policy. First, they suggest that overall there is little concern about a distortionary effect on postsecondary enrollment. Instead, we find increases in enrollment among ethnic minority and female students from disadvantaged backgrounds. Unit analyses further suggest that minimum wage likely helps alleviate financial constraints among college-going students. The subsequent welfare consequences of minimum wage policies hinges on the returns to higher education, and whether the college premium differs across student type. The literature largely suggests that students underinvest in higher education (Avery and Turner, 2012; Barrow and Malamud, 2015; Oreopoulos and Petronijevic, 2013), due to both pecuniary and non-pecuniary returns (Oreopoulos and Salvanes, 2011). For marginal students, the returns to higher education are likely even higher than for the average student (Hoekstra, 2009; Zimmerman, 2014). Thus, even absent effects on alleviating financial constraints, it is possible that any adverse labor market impacts of minimum wage could be welfare improving by nudging (underinvested) high school graduates toward a college degree. Additionally, to the extent that minimum wages do alleviate financial constraints, our study relates to the broader literature on how income and financial aid greatly impact college-going (Bound et al., 2010; Bulman et al., 2021; Castleman and Page, 2016; Manoli and Turner, 2014; Marx and Turner, 2019; Scott-Clayton, 2015). Lastly, and perhaps most importantly, our paper contributes to the greater debate on expanding minimum wage – our results suggest that the discussion on the potential merits of a more generous minimum wage should not be constrained to only the effects on labor market outcomes.

2.2. Data

Our primary data source covers the universe of public high school students in the state of California and comes from California Standards Test (CST). During the time frame of our study (2010-2019), high school students are required to take an English language arts (ELA) standardized test during their eleventh grade.[14] In addition to test scores, the data include information on student gender, race, economic disadvantage status, and birth date, as well as their zip code of residency and a high school identifier. Each cohort consists of approximately 475,000 students.[15]

Our second data source covers the California Community Colleges (CCC) system, with data provided by the California Community College Chancellor's Office. Each year, over two million students enroll across the 114 CCC campuses, making it the largest public postsecondary system in the US. These data include course-level enrollments, units attempted, and units completed, as well as the zip code of the student's residence.

The CST data are then matched to the National Student Clearinghouse (NSC) in order to identify postsecondary enrollment at any US university. We also match the CST scores to the CCC administrative records, allowing us to identify whether minimum wage during a student's senior year of high school impacts their enrolled course units.

2.2.1. Summary Statistics. Table 2.1 presents summary statistics for our sample of California high school students, split by whether the student attended a high school in a city that never adopted a minimum wage versus one that (eventually) adopted a minimum wage. Our total sample includes over 3.6 million high school students, with nearly 2.5 million residing in a city that never adopts a minimum wage through the sample. At baseline, cities that adopt a minimum wage tend

[14]Studies suggest the ELA test scores are indicative of skills that are broadly applicable to longer run outcomes (Master et al., 2017; Naven, 2019). Though students are also required to take a standardized math test, students can choose from one of multiple math exams, complicating comparing various math scores within grades and across years. Thus our analyses do not include math exam scores.

[15]Our analyses for coding minimum wage assumes that a student's home zip code and high school of enrollment are the same between the eleventh and twelfth grades. So long as students are not actively transferring to a new high school for their senior year in response to minimum wage, this measurement error should attenuate estimates toward zero. Later, we provide evidence of little compositional differences in high school cohorts in response to minimum wage.

Table 2.1. Summary Statistics

	All Cities Mean	Non-Adopter Cities Mean	Adopter Cities Mean
Student Covariates			
Male	0.50	0.50	0.50
Disabled	0.06	0.06	0.07
Black	0.06	0.05	0.08
Asian	0.14	0.11	0.20
Hispanic	0.49	0.47	0.53
Other Race	0.03	0.03	0.02
Age (months)	199.90	199.96	199.76
Econ. Disadvantage	0.52	0.50	0.57
Std. ELA Test Score	0.06	0.07	0.05
Took Std. Math Test	0.93	0.93	0.94
Enrollment Outcomes			
Any College	0.612	0.611	0.617
2-year Institution	0.346	0.358	0.317
4-year Institution	0.283	0.269	0.316
CA State University	0.122	0.116	0.135
University of CA	0.066	0.057	0.085
In-state Private	0.033	0.032	0.035
Out-of-state	0.065	0.065	0.063
Observations	3607986	2481481	1126505

Notes: Data include the census of California high school students from 2010 to 2019. Enrollment outcomes are binary indicator variables that are 1 if a student attends that type of college and 0 if otherwise.

to be more ethnically diverse, having more Asian, Black, and Hispanic students. They also tend to have more students from socioeconomically disadvantaged backgrounds. Student test scores are similar across both city types. Turning to our college-going outcomes, we see that roughly three in five high school graduates immediately enroll in some college after graduating, slightly lower than the national average of 66%.[16] One in three students enroll in community college, and over one in four students enroll in a four year university. While overall college-going rates are similar in minimum wage vs. non-minimum wage cities, minimum wage cities tend to send more students to four year universities. Though cities that adopt a minimum wage tend to have differing types of students at baseline, our identification strategy will effectively compare changes in student outcomes within adopter cities by utilizing variation in the timing and magnitude of minimum wage laws as described in the next section.

[16]Education Data, https://educationdata.org/high-school-graduates-who-go-to-college. Accessed 6 Apr. 2021.

2.3. Econometric Specification

From 2008 to mid 2014, California minimum wage was \$8 an hour. Since then, California has gradually increased its minimum wage at the turn of each new year. Moreover, since 2015, over 30 California cities and counties have adopted their own binding minimum wages (higher than the state minimum wage). Notable examples include San Francisco, Los Angeles County, and San Jose, who have increased their minimum wage annually since 2014, 2015, and 2016, respectively. Various cities across southern California and the greater Bay Area have also adopted various minimum wage laws. Importantly, much like studies from Even and Macpherson (2019), Luca and Luca (2019), Neumark and Yen (2020), and Dube and Lindner (2020), we utilize this plausibly exogenous variation in the timing, size, and location of minimum wage laws to isolate the causal effect of minimum wage on outcomes. Detailed descriptions of each locality minimum wage law in the US are provided by the UC Berkeley Labor Center.[17]

Our primary specifications estimate the following equation:

$$Y_{ihct} = \alpha + \beta[\text{MinimumWage}]_{ct} + \lambda_h + \phi_t + X_{ict} + \epsilon_{ihct} \tag{2.1}$$

where each observation is unique at the student i level. Each student is a senior attending high school h in city c in year t. Our primary outcome for Y_{ihct} is an indicator for whether the student enrolled in (a particular) university. $[\text{MinimumWage}]_{ct}$ captures the minimum wage (in dollars) in city c in year t. We code $[\text{MinimumWage}]_{ct}$ based on the minimum wage in January of the student's senior year; later analysis tests the sensitivity of our results to coding minimum wage based on varying months leading to and through the student's (potential) post-secondary enrollment. Fixed effects λ_h and ϕ_t are estimated at the high school and year level. Though cross-sectional variation in minimum wage comes at the city level, high school fixed effects effectively control for any time-invariant differences in students across cities (since high school fixed effects absorb city fixed effects). X_{ict} is a vector of controls, including city time trends and student controls for gender, race, age (in months), economic disadvantage status, and standardized test score. The

[17]UC Berkeley Labor Center. "Inventory of US City and County Minimum Wage Ordinances." https://laborcenter.berkeley.edu/inventory-of-us-city-and-county-minimum-wage-ordinances/. Accessed 17 May 2022.

coefficient β can be interpreted as the predicted change in the outcome variable in response to a \$1 increase in local minimum wage. This two-way fixed effect model identifies β using variation in the timing and magnitude of minimum wages across graduating cohorts from the same high school while controlling for any statewide time trends with year fixed effects.

2.3.1. Balance Test. Given our twoway fixed effects specification at the high school and year level paired with controlling for student controls and linear city time trends, the primary threat to our identification strategy stems from high schools experiencing unobserved differential non-linear trends that are correlated with both student likelihood of postsecondary enrollment and minimum wage laws. For example, if more (less) affluent families move into cities after the city adopts a minimum wage law, and affluency is positively (negatively) related with college enrollment, estimates for β would be positively (negatively) biased. Hence, any shifts in the composition of high school seniors in response to changes in minimum wage laws would invalidate the identification strategy.

To test for this possibility, we consider two approaches. First, in column (1) of Table 2.2 we regress local minimum wage on our full model of student covariates to see if student covariates can jointly predict local minimum wages. With a p-value of 0.27, we fail to reject the null hypothesis that all the student covariates are jointly unrelated to the student's local minimum wage.

For our second approach, we first estimate (in two separate OLS regressions) our full model with 4-year college-going and 2-year college-going as the outcome variables, but without minimum wage as a covariate. These models serves as general prediction models for college-going. We then take the predicted values from these two regressions and regress them on minimum wage in columns (2) and (3). This allows us to directly test for whether students of differing propensity to attend a university differentially sorted across cities by minimum wage. We find no evidence that more generous minimum wages led to changes in the composition of student-types as related to likelihood of attending college: A \$1 increase in minimum wage is associated with a 0.05 percentage point increase in a student's likelihood of attending a 4-year university and a 0.20 percentage point *drop* in attending a 2-year college. Not only are these estimates statistically insignificant, the standard errors from these models are also fairly small; we can rule out increases in a student's inherent

Table 2.2. Balance Test

	Individual Level			High-School Level		
	Joint Sig	Balance test		Joint Sig	Balance test	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	MW	$\widehat{Pr(4\text{-}year)}$	$\widehat{Pr(2\text{-}year)}$	MW	$\widehat{Pr(4\text{-}year)}$	$\widehat{Pr(2\text{-}year)}$
Regressor(s):						
-Minimum Wage (MW)		0.049	-0.204		0.072	0.024
		(0.035)	(0.165)		(0.058)	(0.061)
-Male	0.000			0.004		
	(0.000)			(0.009)		
-Disabled	-0.003*			-0.005		
	(0.001)			(0.022)		
-Black	0.001			0.041		
	(0.001)			(0.035)		
-Asian	0.001			0.177		
	(0.001)			(0.121)		
-Hispanic	-0.000			0.023		
	(0.001)			(0.026)		
-Other	-0.003*			-0.011		
	(0.002)			(0.018)		
-Age (months)	-0.000			0.001		
	(0.000)			(0.001)		
-Econ. Disadvantage	-0.000			-0.043		
	(0.002)			(0.039)		
-Std. ELA Test Score	-0.001**			-0.002		
	(0.000)			(0.006)		
-Took Std. Math Test	-0.004			-0.043*		
	(0.003)			(0.022)		
-Cohort Size				0.000		
				(0.000)		
F-Stat	1.22			0.88		
P-Value	0.274			0.551		
Observations	3607986	3607986	3607986	17449	17449	17449
High School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
City-Time Trend	X	X	X	X	X	X
Student controls	X			X		

Notes: MW = Minimum wage (in $). The first column regresses local minimum wage on the full set of student covariates. The second column regresses a dummy variable for going to any college on student covariates. The third column takes the predicted values from the second column and regresses them on minimum wage. The fourth through sixth columns repeat this exercise but with the data collapsed to the high school level (and averaging across covariates). Standard errors are clustered at the city level.

probability of attending a 4-year (2-year) institution larger than 0.12 (0.13) percentage points in response to a $1 increase in minimum wage with 95% confidence.

Finally, in columns (4) through (6), we replicate these exercises but with the data collapsed to the high school level (and averaging across student covariates), while also including a control for high school cohort size, to confirm our previous findings: Changes in the timing and magnitude of minimum wage are uncorrelated with changes in observable student characteristics. Notably, our null results for cohort size provides some assurance that changes in minimum wage laws are largely uncorrelated with high school dropout.

2.4. Results

2.4.1. Increases in Overall Enrollment into Higher Education. Table 2.3 presents our results from equation (1) to look at whether minimum wage during a student's senior year of high school influences their likelihood of pursuing a postsecondary education. We first find that a more generous minimum wage induced more college-going among high school graduates. While the effect is statistically significant at the 10% level, the point estimate is relatively small: A \$1 increase in local minimum wage leads to a 0.40 percentage point increase in likelihood of attending any college. Since approximately 60% the sample enrolls in college, a \$3 increase in minimum wage translates to roughly a 2% (1.2 ppts) increase in college-going. In the next two columns, we decompose this effect by two vs. four year enrollment. Results show the effect is entirely driven by enrollment changes into four year institutions. The magnitude of the four year effect is meaningful and the point estimate is statistically significant at the 1% level. A \$3 increase in minimum page is associated with a 6.1% (1.7 ppts) increase in four year college enrollment.

In the second panel of Table 2.3, we further decompose the four year effect into categories by institution type: the California State Universities (CSU), the Universities of California (UC), in-state private universities, and out-of-state universities. Results from this exercise show the minimum wage effect on enrollment is most pronounced at the in-state public universities, particularly CSUs, which overall have the lowest tuition costs and are the least selective for admission. Additionally, enrollment into the more selective UCs also appears to have a stronger reaction to changes in minimum wage compared to more expensive in-state private and out-of-state universities. These results provide suggestive evidence that minimum wage increases may help alleviate budget constraints for lower income students attending in-state public institutions. We explore this further in the next section.

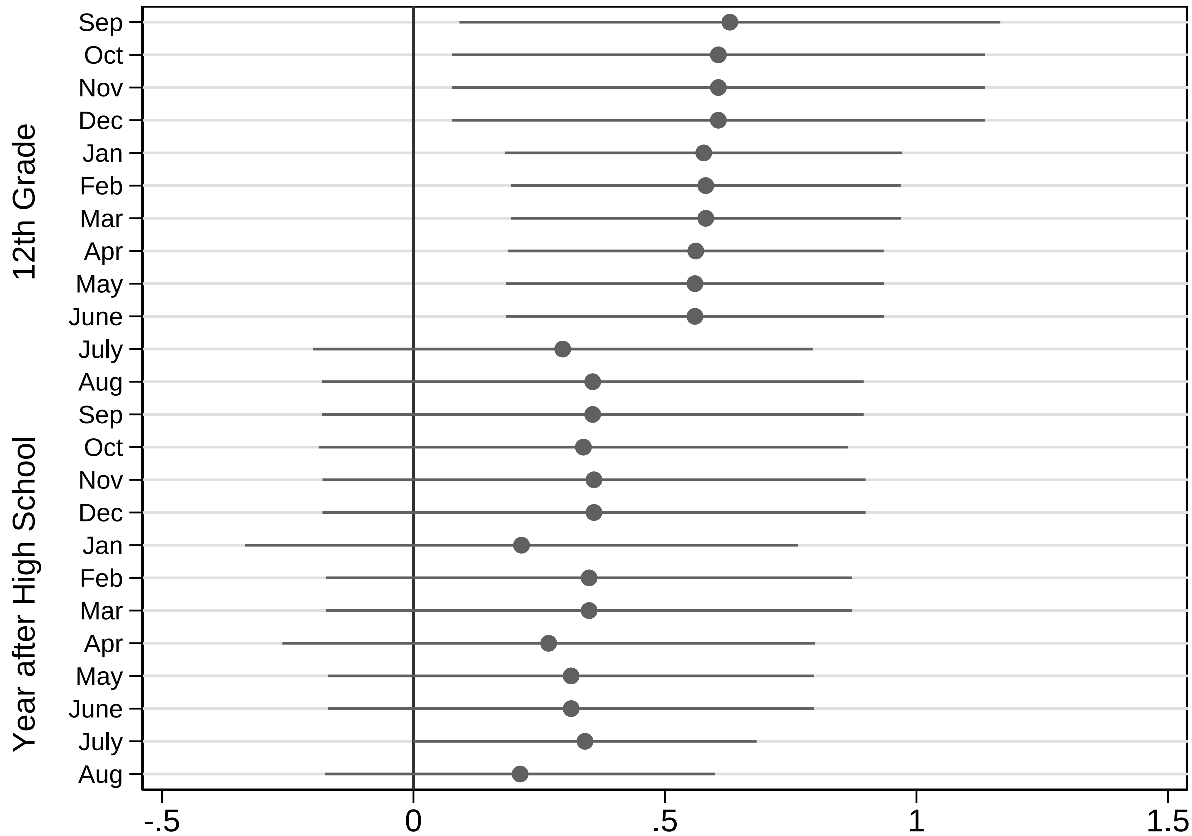
Table 2.3. Impact of Minimum Wage on College-going

	Any College	2-year Institution	4-year Institution	
	(1)	(2)	(3)	
Minimum Wage	0.401*	-0.033	0.577***	
	(0.220)	(0.256)	(0.201)	
Observations	3607986	3607986	3607986	
High School FE	X	X	X	
Year FE	X	X	X	
Student Controls	X	X	X	
City-time Trend	X	X	X	
Type of 4-year:	CA State Univ.	Univ. of CA	In-state Private	Out-of-state
	(4)	(5)	(6)	(7)
Minimum Wage	0.355*	0.163*	0.030	0.026
	(0.215)	(0.094)	(0.045)	(0.113)
Observations	3607986	3607986	3607986	3607986
High School FE	X	X	X	X
Year FE	X	X	X	X
Student Controls	X	X	X	X
City-Time Trend	X	X	X	X

Notes: Minimum wage measured in dollars. Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. Standard errors are clustered at the city level.

For additional robustness and exploration, in Figure 2.1 we test the sensitivity of the on-time four year enrollment result by the month of the student's local minimum wage. We consider the 24 months of the high school city's minimum wage starting with the September of the student's senior year. The first roughly 12 months cover all the treatment months prior to any potential on-time enrollment. The subsequent 12 months reflect "future" local (high school city) minimum wages on a student's decision to enroll on-time - these months largely serve as a placebo test assuming that students typically attend a four year college in a different city from their residence during high school. Nevertheless, future local minimum wages may influence present college-enrolment decisions if high school graduates forecast future local minimum wages and if they attend a university in the same city as their high school. The results from this exercise first show that the four year effect is robust to each month of the student's high school tenure, from September through June. By July, the magnitude of the effect drops and becomes statistically insignificant. Still, for each of the 14 months starting with July (post-graduation), the point estimates remain positive, perhaps highlighting potential enrollment into institutions in the same city as the student's high school.

Figure 2.1. Impact of High School City's Minimum Wage by Month on On-time Enrollment at a 4-year Institution



Notes: Each coefficient and 95% confidence interval come from a separate regression of an indicator for the student enrolling at a 4-year institution within a year of graduating high school on the minimum wage in the city of the student's high school by month. The first 12 coefficients cover the minimum wage the student faces prior to any on-time college enrollment, while the latter 12 reflect "future" local (high school) minimum wage impacts on on-time 4-year enrollment decisions.

In Table B1, Table B2, and Table B3,[18] we replicate Table 2.3 but test the sensitivity of our results to replacing high school fixed effects with city effects, city time trends with high school time trends, and to removing time trends altogether, respectively. Across all three considerations, our results become even more precisely estimated, with even larger point estimates. This indicates that our primary model is taking a relatively conservative approach, and that the "true" positive effect of minimum wage on college enrollment may be larger than our primary estimates suggest.

[18]Tables in Appendix B.

2.4.2. Differential Responses by Student Type and University. In Table 2.4, we consider a variant of specification (1) where we interact the minimum wage variable with individual student covariates of interest. Results show that minimum wage has virtually no effect on male student enrolment, with the entire average effect driven by female students: A \$1 minimum wage increase is associated with a 0.71 percentage point increase (about 1.2% increase) in college-going for women. We also find that the minimum wage effect for socioeconomically disadvantaged students is nearly four times the magnitude of that for their advantaged counterparts (0.72pp vs 0.18pp). Likewise, the effects are also particularly pronounced for traditionally underrepresented students. In the remaining panels of Table 2.4, we investigate these heterogeneities across the different college sectors. The interactions on socioeconomic disadvantage and minority race also reveal that the positive effects for these groups are driven by the CCCs and the CSUs (the less selective four-year universities), whereas the effects for the UCs are reversed – the student composition attending the UCs skews away from economically disadvantaged and traditionally underrepresented students.

We also find that overall enrollment effects are stronger for students with higher standardized test scores: a one standard deviation increase predicts an additional 0.22 percentage point increase in the probability of going to college in response to a \$1 increase in minimum wage. Interestingly, we find a significant shift in the type of university a student attends in relation to their standardized test score. The probability that an average student (Z=0) attends a two year community college is unresponsive to minimum wage; however, those with weaker (stronger) test scores are relatively more (less) likely to attend a community college in response to an increase in minimum wage. Importantly, the movement away from community colleges among higher performing students is driven by a movement into four year institutions, and in particular, the UCs – a one standard deviation increase in a student's standardized test score is associated with an additional 0.66 percentage point increase in likelihood of attending a UC in response to a \$1 increase in minimum wage.

Altogether, our results paint a picture of small increases in overall enrollment associated with changes in minimum wages. However, underlying this are are relatively large responses by student and university type. First, overall enrollment into community colleges is unresponsive to increases in minimum wage, while enrollment into four year universities significantly increases. Lower performing, economically disadvantaged, and traditionally underrepresented students are more likely

Table 2.4. Differential Responses to Minimum Wage by Types of Students and Universities

	Male	Socioeconomic Disadvantage	Minority Race	Std. ELA Test Score
	(1)	(2)	(3)	(4)
Any College				
Minimum Wage	0.709***	0.184	0.052	0.348
	(0.220)	(0.234)	(0.227)	(0.216)
x Interaction	-0.602***	0.534***	1.002***	0.222**
	(0.092)	(0.102)	(0.137)	(0.092)
2-year Institution				
Minimum Wage	0.041	-0.538*	-0.639**	0.089
	(0.253)	(0.274)	(0.290)	(0.283)
x Interaction	-0.144*	1.243***	1.673***	-0.502***
	(0.081)	(0.130)	(0.127)	(0.089)
4-year Institution				
Minimum Wage	0.830***	0.895***	0.831***	0.415**
	(0.204)	(0.226)	(0.198)	(0.203)
x Interaction	-0.495***	-0.782***	-0.660***	0.684***
	(0.063)	(0.096)	(0.085)	(0.057)
CA State University				
Minimum Wage	0.392*	0.300	0.212	0.328
	(0.222)	(0.214)	(0.205)	(0.208)
x Interaction	-0.072	0.136**	0.398***	0.113*
	(0.053)	(0.056)	(0.054)	(0.066)
University of CA				
Minimum Wage	0.297***	0.297***	0.352***	0.004
	(0.096)	(0.101)	(0.108)	(0.117)
x Interaction	-0.262***	-0.331***	-0.523***	0.661***
	(0.037)	(0.041)	(0.055)	(0.040)
In-state Private				
Minimum Wage	0.016	0.060	0.051	0.049
	(0.044)	(0.043)	(0.047)	(0.045)
x Interaction	0.028	-0.074***	-0.053*	-0.082***
	(0.021)	(0.026)	(0.027)	(0.020)
Out-of-state				
Minimum Wage	0.107	0.233**	0.214*	0.040
	(0.115)	(0.103)	(0.116)	(0.113)
x Interaction	-0.159***	-0.510***	-0.483***	-0.060**
	(0.026)	(0.054)	(0.047)	(0.024)
Observations	3607986	3607986	3607986	3607986
High School FE	X	X	X	X
Year FE	X	X	X	X
Student Controls	X	X	X	X
City-time Trend	X	X	X	X

Notes: Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. "Interaction" is an interaction between minimum wage (in $) and the variable labeled in the column header. Standard errors are clustered at the city level.

to enroll in a community college in response to an increase in minimum wage. Likewise, higher performing students substitute their enrollment from community colleges into four year institutions (given the overall CCC enrollment effect is zero). Decomposing by type of four year institution, we find that the higher performing students are enrolling into UCs (the more selective system), while economically disadvantaged and traditionally underrepresented students increase enrollment into the CSUs. Thus, evidence suggests minimum wage may have a "lifting" effect for all student types – lower performing students who otherwise would not have attended college are now attending community colleges, while higher performing students increase their enrollment into the more selective four year university system.

2.4.3. Early College Enrollment Outcomes. In this section, we utilize the CCC records in order to investigate early college outcomes. To do so, we match the sample of California high school students from the previous section to their CCC records (if they attended). We focus strictly on "on-time" enrollment – students who attended a CCC within a year of graduating high school. Our models replace high school fixed effects with college fixed effects (since we now focus strictly on college-going students), while maintaining the remaining covariates from our primary econometric model. We also consider multiple variants of minimum wage: (1) the minimum wage at the beginning of the term in the city of the college, (2) the minimum wage at the beginning of the term in the city of the student's residence, and (3) the student's minimum wage from high school (January of senior year). These models should perhaps be interpreted cautiously given potential endogenous selection of college and city of residence (e.g. students move to cities with more generous minimum wage to attend college), and given results from the prior section where different types of students shift their postsecondary enrollment based on local minimum wage. Multiple channels may also be at play in these models - a more generous minimum wage may increase units attempted via relieving financial constraints, but also stronger employment opportunities may crowd out units attempted.

The results from this exercise are presented in Table 2.5. Since CCC students pay per unit enrolled, we consider units earned as our primary outcome variable. Across both fall and spring

Table 2.5. Impact of Minimum Wage on Units Earned at CCC

	Fall Semester	Spring Semester
Units Earned		
City of College MW		
Beginning of Term MW	1.116***	0.889***
	(0.052)	(0.041)
Observations	3589757	3648126
City of Residence MW		
Beginning of Term MW	1.487***	1.309***
	(0.084)	(0.077)
Observations	3565929	3630987
High School City MW		
MW During High School	1.088***	0.967***
	(0.037)	(0.033)
Observations	3581291	3638621
University FE	X	X
Year FE	X	X
City-time Trend	X	X
Student controls	X	X

Notes: The sample consists of California community college students, conditional on being matched to the CST high school dataset. MW = Minimum wage (in $). CCC = California Community College. Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. Standard errors are clustered at the city level.

semesters, and across all three minimum wage considerations, we find that a more generous minimum wage induces greater unit accumulation among CCC students. The magnitudes of the coefficients suggest that students enroll in one more class (3-4 units) per semester for a $3 increase in minimum wage. Given we found in the previous section that the composition of CCC-attending students shifted toward economically disadvantaged students, these results provide evidence that increases in minimum wage may help alleviate financial constraints for CCC-attending students.

2.5. Conclusion

Minimum wage remains one of the most hotly discussed policies in the US today. One component of the minimum wage debate centers on its potential impacts on human capital accumulation. On one hand, all else equal, a more generous minimum wage may entice high school graduates to delay or forego a higher education. On the other, if minimum wage generates greater unemployment, then high school graduates adversely affected by the labor market may opt to pursue a postsecondary education. Moreover, many minimum wage proponents point to rising college costs and how a higher education is unaffordable at current minimum wage levels.

In this study, we investigate how enrollment into college is affected by local minimum wage by utilizing administrative data on the census of California high school graduates linked with postsecondary outcomes paired with variation in the timing and magnitude of minimum wage laws across California municipalities. We precisely estimate a small but positive effect of minimum wage on overall college-going. The effects are particularly driven by increased enrollment into the lower tier four year public university system. Further evidence suggests that minimum wage serves to alleviate financial constraints for high school graduates. For instance, the effects are driven entirely by students from socioeconomically disadvantaged backgrounds; these students are mostly selecting into community colleges and the lower tier public university system. Meanwhile, higher performing students significantly shift their enrollment into the more selective four year public schooling system. Though overall enrollments at community colleges are unchanged, conditional on enrolling, students complete more units in response to a more generous minimum wage.

Our results highlight how the discussion on the impacts of minimum wage should not be limited to its labor market impacts, and to the extent that students are underinvesting in their higher education, these results suggest a positive overall impact of minimum wage.

CHAPTER 3

Evaluating Incentives for Full-time Enrollment at California Community Colleges

3.1. Introduction

Despite overall increases in college enrollment, low-income students, even after adjusting for academic achievement, are less likely to enroll in college and complete a degree than their more affluent peers (Bailey and Dynarski, 2011; Keane, 2002). The past several decades have been witness to steep increases in college tuition and fees, as well as room and board (College Board, 2015). These patterns suggest that financial constraints remain an important barrier to educational attainment for many students. Consequently, reducing the cost students face for attending college remains an important policy tool to improve college participation and postsecondary degree attainment, particularly for low-income students.

As college tuition continues to rise, especially at public state flagship institutions, many low-income students turn to community colleges for their postsecondary schooling. These open-access institutions have captured the attention of national and state policymakers concerned with improving postsecondary degree production, workforce shortages, and the overall economic health of the nation; however, community colleges have been scrutinized for their poor completion outcomes. As one of the key drivers in the push to increase the stock of college graduates in the U.S., especially among individuals from disadvantaged backgrounds, it is critical to understand what can support improvement in community college degree outcomes (both sub-baccalaureate degree/certificate completion and transfer to four-year colleges). To this end, policymakers have begun to focus on reducing community college costs, with calls to make community college "as free and universal as high school." Recently, several states (Minnesota, Rhode Island, Tennessee, and Oregon) have established "Promise Programs" that offer some version of free community college. Although most financial aid programs fall short of "free community college," there is nonetheless an important need to target aid to students enrolled at these broad access institutions.

The empirical support for financial aid programs comes from a large body of research showing that college costs affect college enrollment decisions, persistence, and completion (Dynarski and Scott-Clayton, 2013; Page and Scott-Clayton, 2016; Scott-Clayton, 2017). However, much of these findings are based on financial aid programs that mainly target four-year universities. The research on the impacts of costs and financial aid for community college students is decidedly thinner, and focuses on changing the "sticker price" students face (Denning, 2017) or the impact of losing need-based aid due to poor academic performance (Schudde and Scott-Clayton, 2016). Thus, very little is known about how providing new community college students financial assistance affects their outcomes.

In light of previous research, this paper examines the impact of changes in a large need-based state financial aid program in California that specifically serves community college students.Our research is especially timely given changes in the structure of the award specifically targeting community college students. There were multiple changes to the Cal Grant B award, tied to full-time enrollment. Starting in 2014-2015, a bonus of \$600 for full-time enrollment (defined as enrolling in 12 credits per semester) at a California community college was awarded. Starting in the 2017-2018 academic year, this bonus increased for full-time enrollment increased to \$1,000 per year. Even more significantly, community college students enrolling in 15 credits per semester also received the new College Completion Grant, which awarded an additional \$1,500 per year. Finally, in the 2018-2019 academic year, the Student Success Grant was introduced, essentially combining the Full-time Success grant and the Completion Grant, and yet again increasing aid generosity to \$1,250 a year for enrolling in at least 12 credits a semester and \$4,000 a year for enrolling in at least 15 credits a semester.

This study rigorously evaluates the efficacy of the changes in the Cal Grant B program targeting community college students. We answer the following research questions:

(1) How do changes in Cal Grant B financial incentives affect college choice for first-time students? Specifically, what is the impact of eligibility for the Cal Grant B program among recent California high school graduates on the choice to attend a California Community College (versus no college or a four-year college)?

(2) How do changes in Cal Grant B financial incentives affect medium-run outcomes of recent California high school graduates enrolled at California Community Colleges (e.g. enrolling in 12 or 15 credits, first year credit attainment, and persistence by term)?

We answer these questions using a regression discontinuity design based on income-eligibility cutoffs. This design enables us to evaluate the causal impact of the Cal Grant B—a fully developed financial aid intervention in the state of California—on student outcomes. We employ a unique dataset that links individual-level data from multiple state administrative data sets: 1) California Community Colleges Chancellor's Office data on college behavior; 2) California's K-12 schools for pre-collegiate characteristics; and 3) California Student Aid Commission data on financial aid information. Additionally, we supplement state administrative data with information from the National Student Clearinghouse, which has national coverage and includes private universities as well as the California State University and University of California systems. Together, these data allow us to include both unprecedented controls for pre-collegiate characteristics, as well as track the impact of the program on college enrollment decisions and college attainment beyond the community college.

Our evaluation is uniquely positioned to make several important contributions to the field. First, the California community college system is the largest in the country; one-fourth of all community college students in the nation are enrolled at a California Community College. The scale of the system offers important individual student (over 1.8 million students served) and institutional-level (117 colleges) diversity across the state. Second, to our knowledge, this is the first study examining a statewide need-based financial aid program that specifically targets community college students. This is especially compelling given that the increase in the maximum Cal Grant B award we plan to examine is so large. Third, the design of recent reforms to the aid program offers an important opportunity to examine the effect of an additional incentive to enroll in 15 credits per semester, which, unlike the traditional 12-unit threshold for full-time enrollment, would be sufficient for normative-time Associate degree completion (i.e., within two years). Fourth, the financial aid program we are evaluating is means tested, and as such our results will provide policymakers with important information on the impact of aid that targets low-income students enrolled at community colleges, a population which historically has had much lower completion rates than their more affluent peers.

It is also important to note that, while these bonuses are intended to encourage timely progress toward a degree, they could also divert students away from four-year colleges and toward community college. Research on a scholarship awarded in Massachusetts has found merit aid redirects students to less-selective universities and results in lower completion rates (Cohodes and Goodman, 2014).

Our study allows us to examine whether or not similar effects operate for need-based aid at the two-year versus four-year college margin.

We find, however, that being eligible for the Cal Grant B does not induce students to enroll in a California community college over other alternatives (including entering the labor force immediately after high school or enrolling in a four-year institution). In addition, we find that, despite generous incentives to take at least 12 or at least 15 credits, community college students are not altering their course-taking behavior to take advantage of these increases in financial aid. Finally, we find that the supplemental Cal Grant B awards do not have any effects on persistence to the next term.

One explanation for these null results is a potential lack of awareness of the details regarding each part of their financial aid package by the student. Many students do not know from which source some of their financial aid comes, suggesting that students are not aware that enrolling as a full-time student would provide more generous benefits. Survey responses regarding Cal Grant B take-up and student awareness of different grant receipt provide suggestive evidence supporting this hypothesis.

Our findings, though disappointing, have many policy implications. As financial aid is an important policy lever, it is worthwhile understanding why students are not changing their behavior in response to increased generosity. Our results suggest that students are more unaware of the composition and details of their financial aid packages than previously assumed. This might be particularly true in the California community college context, as other various financial aid fully covering tuition for low-income students may result in students never looking more closely at any line-item bills detailing college costs, and thus never learning about the various details of their financial aid.

3.2. Literature Review

A variety of different aid programs exist for reducing the cost of college attendance. Need-based financial aid provides money for tuition or living expenses of lower-income students. Many need-based aid programs also have some academic standards requirements and are not (re)awarded solely on the basis of financial need. By far the largest such program is the federal Pell Grant program, which in the past year provided over $30 billion in aid to over 9 million students (Scott-Clayton, 2017; U.S. Dept. of Education, 2016).Many states also have additional need-based aid programs, which in recent years provided, on average, about $700 in grant aid per full-time-equivalent student per year (College Board, 2015). In contrast, state merit aid programs are awarded without consideration for financial need and target students who show academic promise in high school (for example through a sufficiently high GPA or test scores). While smaller in magnitude than need-based aid, these have become increasingly important as a share of total state aid programs (College Board, 2015). States also provide universal tuition assistance in the form of in-kind tuition subsidies that lower tuition below the cost of attendance (Denning, 2017; Long, 2004).[1]

The rationale behind all of these programs is that by lowering the price of college that students face, demand for college ought to increase. A very large body of literature has emerged to assess whether or not this is true. Overall, this research suggests that "costs matter." Early work suggests that a $1,000 decrease in the net price of college is associated with a 3 to 5 percentage point increase in college enrollment (Leslie and Brinkman, 1988), and estimates from more recent research are of similar magnitude (Deming and Dynarski, 2010; Page and Scott-Clayton, 2016). However, effects of aid programs can vary as a result of type of aid, institution, requirements for eligibility, and timing of the award (Deming and Dynarski, 2010; Dynarski and Scott-Clayton, 2013). A key challenge in this area of research is teasing out the effect of changes in the cost of college from the many other, observed or unobserved, determinants of college enrollment. Recent research has attempted to address this challenge by taking advantage of exogenous changes in financial aid policy.

Numerous studies have examined the impact of need-based aid. The research on the effects of federal need-based aid such as Pell Grants have produced mixed results, with some studies finding no effects of college enrollment or attainment (Hansen, 1983; Kane, 1994; Marx and Turner, 2015), while others find positive effects on college outcomes (Bettinger, 2004; Denning, 2019; Lovenheim and Owens, 2014; Seftor and Turner, 2002). Goldrick-Rab et al. (2016) find that a randomly-awarded scholarship to Pell-eligible students attending the University of Wisconsin system had small effects on persistence to the second year but no effects beyond that. In contrast, Angrist et al. (2016) find that awarding prospective college students very large privately funded scholarships to

[1]Other forms of financial aid include subsidized student loans, federal work-study programs, and veterans' educational benefits, such as the G.I. Bill (Barr, 2015, 2019)

attend college in the University of Nebraska system increased college enrollment and persistence, and diverted students away from community college toward a four-year college.

There are also several studies that explore the impacts of merit aid programs. Dynarski (2000, 2004) finds large positive effects of the Georgia HOPE scholarship program on college enrollment, for relatively well-off students. Sjoquist and Winters (2013) demonstrate that the HOPE scholarship had little effect on college retention. Other work on merit-based programs has positive effects on both enrollment and degree completion (Bettinger et al., 2019). In contrast, Goodman (2008) finds that a Massachusetts merit aid program had no effect on overall college enrollment, but diverted students away from more selective private universities. Subsequent research found that this diversion reduced BA attainment (Cohodes and Goodman, 2014). Leveraging the variation in the introduction of merit aid programs in fifteen states over time, Fitzpatrick and Jones (2016) show that eligibility for state merit programs slightly increase the likelihood that natives stay in state (the main intent of these programs); however, the magnitude of the effects on enrollment are quite small overall (less than 3 percent of a cohort).

Much of the research on college costs has focused on programs that target four-year colleges. However, despite the lower cost of attendance, community college students are also sensitive to college costs given that they tend to come from lower-income backgrounds and are more likely to confront challenges associated with the continued need to work, food or housing insecurity, childcare responsibilities, and transportation difficulties (Goldrick-Rab et al., 2016, 2017). These financial burdens leave community college students far more vulnerable to interrupted enrollment, and to part-time enrollment (Crosta, 2014).

Although not as large as the literature on the effects of four-year college costs, a body of research does exist on the impact of community college cost. A recent study by Denning (2017) examines the impact of changes in the community college "sticker price" tuition in Texas and finds that a $1,000 decrease in tuition increases community college attendance right after high school graduation by 5.1 percentage points. The effect is primarily driven by enrollment of students who would have normally not enrolled in college, as there was little evidence of diversion away from four-year institutions or reductions in BA attainment. There was, however, some significant downward sorting for African American students (Denning, 2017). Angrist et al. (2016) find that, among scholarship applicants in Nebraska who indicated they planned to attend a community college, a randomized offer of financial aid had no effect on college enrollment or college sector. Polson and Weisburst (2014) also observe a positive effect of federal work-study on second-year persistence and on transferring to a four-year institution in Texas community colleges. Finally, Scott-Clayton (2011a) finds that the federal work-study program had no effect on student outcomes at community colleges.

The Cal Grant B program that we examine offers financial assistance to low-income students; however, it is not entirely a need-based grant. In order to receive the aid, students must not only qualify by being low-income, but must also satisfy additional requirements for initial receipt and for renewal, specifically be enrolled in a minimum number of credits and maintain a certain grade point average. As such, it has also been described by some (i.e., Bettinger et al. (2019)) as a merit-based grant program. Importantly, for our analysis, this means that the Cal Grant B is a means-tested aid program with "strings" for receipt, a facet of the financial aid literature of more recent exploration.

Like the Cal Grant B, many financial aid programs impose some additional requirements—above and beyond financial need or pre-college academic merit—for initial receipt of aid or for renewal. For instance, renewal of the Pell Grant in subsequent years requires students to maintain Satisfactory Academic Progress (SAP). The loss of financial aid due to not meeting academic standards provides another source of variation in college costs as well as an interesting test of whether students respond to the financial incentives embedded in these requirements. Schudde and Scott-Clayton (2016) find that large numbers of community college students lose Pell eligibility due to missing SAP. Losing Pell eligibility reduces year-to-year persistence by nearly 4 percentage points. In a related paper, Scott-Clayton (2017) observe that students just below the GPA threshold are less likely to persist, but note positive effects on grades among those that do persist to year two; by year three, however, missing the GPA threshold results in lower overall persistence rates. Interestingly, Scott-Clayton (2011b) finds that the effects of a merit aid program in West Virginia disappear once students become seniors and no longer face minimum GPA and credit completion requirements to renew their merit aid. These findings suggest that academic requirements drive the positive effects of merit aid on degree progress, beyond the financial award itself Scott-Clayton (2011b).

Another study examined the impact of the Opening Doors Initiative, which provided a scholarship with academic requirements and academic supports for low-income community college students in six states. The interventions varied in program design, requirements, and effects. Opening Doors Louisiana was implemented at three New Orleans community colleges and offered \$250 for enrolling at least half-time, \$250 for staying enrolled half-time and earning at least a C average at mid-semester, and \$500 for finishing the semester with at least a C average. This program had positive effects on short-term outcomes including credit attainment, first-semester GPA, and persistence for one and two additional semesters (Barrow et al., 2014).

These results motivated the creation of the Performance-Based Scholarship (PBS) Demonstration in six states (Arizona, California, Florida, New Mexico, New York, and Ohio). The program designed financial aid to cover 15-25 percent of the difference between cost of attendance and other financial aid received. The targeted populations and interventions varied across campuses, but all offered students multiple payments throughout the semester if they met specific academic benchmarks. An RCT evaluation of the PBS Demonstration had modest positive effects on academic outcomes, including 2.1 additional credits and a 3.3 percentage point increase in degree completion, but no significant effects on persistence (Mayer et al., 2015).

California had the largest PBS intervention and offered students a portable scholarship that could be used at any accredited institution in the nation. It targeted low-income high school seniors aged 16-19, and individuals were randomly assigned to one of six scholarship amounts varying between \$1,000-4,000 for up to two years. Students were also required to complete six or more credits with a C average or better. The California intervention increased college enrollment by five percentage points, driven by increases in community college attendance, but had no significant effect on persistence . The scholarships also increased the share of students achieving the academic benchmarks (Richburg-Hayes et al., 2015).

3.3. State Financial Aid Context and the Cal Grant Program

California has historically been a low tuition, low aid state. The Great Recession led to large tuition increases across the state's public colleges and universities. Today, California ranks in the top 10 for highest tuition among state public flagship campuses (\$13,490 at the University of California) and in the middle of the pack for tuition at public four-year institutions ($9,350 at the California State University campuses, relative to the national average of $9,650); however, California remains the state with the lowest tuition at public two-year institutions ($1,430, less than half of the national average at $3,520) (College Board, 2016). Overall, increases in tuition and a reduction in state appropriation for higher education were met with increases in need-based grant aid. Between 2004-05 and 2014-15, California's need-based grant aid increased by over 150% (NASSGAP, 2016). Relative to the rest of the nation, California's average state grant is well over the national average of $710, at just under $1,000; moreover, nearly all of state grant aid in California is need-based (College Board, 2015).

The size of California alone makes it an important state to investigate; the state accounts for nearly a quarter of all need-based undergraduate aid (NASSGAP, 2016) and enrolls one-sixth of the nation's full-time public two-year students (College Board, 2016). It is also important because of its unique postsecondary structure, serving over 2 million students through community colleges, an additional 475,000 through public non-selective four-year universities (CSUs), and another 210,000 at the State's selective flagship campuses (UCs). Moreover, the state grant programs under investigation are both typical of other need-based entitlement programs and unique in their focus on full-time enrollment.

In addition to federal aid programs such as the Pell Grant, low-income students in California are eligible for a number of financial aid programs. The focus of this study is on the Cal Grant program. This program was initially established as a competitive merit-based scholarship. The Cal Grant A targets low- and middle-income students that show academic promise and can be used at four-year universities. Legislation in the 1960s and 70s expanded the program to offer competitive aid targeted at low-income, minority, and community college students, and was the origin of today's Cal Grant B program. Kane (2003) and Bettinger et al. (2019) examined the impacts of the Cal Grant A program under this regime and found that it generated increases in enrollment, BA degree completion, and earnings. In contrast, the impacts of the Cal Grant B, which targets low-income students and can be used at a community college, have yet to be examined.

In 2001, the Cal Grant programs were converted from competitive programs to entitlement programs. This vastly expanded the level of financial aid available for students in California. The transition to entitlement involved setting income and asset ceilings, GPA requirements, and award levels as opposed to the ex post cut-offs that were merely a result of the competitive nature of the award system prior to 2001. For the Cal Grant B, the focus of this study, students had to have assets and income levels below specified cutoffs and have a high school GPA of at least 2.0. Eligible students can then receive the Cal Grant B if enrolled at least half-time in a program of study to earn an associate degree, career technical education certificate, or other community college certificates, or meet university transfer requirements. To renew the grant, students have to maintain a 2.0 GPA. In 2016-17, nearly 73,000 recent high school graduates received the Cal Grant B to attend a California Community College.

There have been three significant changes to the structure of the Cal Grant program. The first, enacted in the 2015-2016 academic year, the Full-Time Student Success Grant (FTSSG), increased the Cal Grant B awards for students taking 12 credits or more per semester with an additional $600 per year. The second reform, which occurred for the 2017-2018 academic year, was the College Completion Grant Program (CCG), which now awards Cal Grant B students an additional $1,500 annually for enrollment in 15 or more credit credits per semester (or the equivalent number of credits per quarter). The motivation for the CCG was to encourage students to be on track to obtain an associate degree or to transfer to a four-year university within two academic years. Finally, starting in 2018-2019, the Student Success Completion Grant (SSCG) combined the previous two changes and again increased the generosity. Students who enrolled in 12-14.9 credits a semester now receive an additional $1,298 a year, and a maximum of $4,000 a year if they enroll in at least 15 credits.

Another financial aid program with important implications for our study is the California College Promise Grant (CCPG), formerly the Board of Governors Fee Waiver. This program provides tuition and fee waivers for low-income community college students. Students receiving CCPG fee waivers who also receive Cal Grant aid can use all of the money from the Cal Grant to pay for living expenses. 95% of income-eligible Cal Grant B students in our study sample receive CCPG fee waivers if they attend community college. Thus, we are effectively be examining the effect of providing grant aid in a context where students face essentially zero tuition cost of attending community college.

The Cal Grant is the largest source of state-funded student financial aid in California, and was introduced in 2000. There are many types of Cal Grants, each targeted towards a different subset of students with different goals. To qualify for a Cal Grant, students need to meet both need-based requirements (such as income and asset thresholds), as well as merit-based requirements.

Our paper today focuses on the Cal Grant B, which is targeted towards low-income students, whose high school GPA is at least 2.0 or higher. This is a sizable, widely used program; during the 2019-2020 academic year Cal Grant B distributed over $2 billion in aid across all of California's college sectors. Within California's community college sector, in the same year there were over 116,000 recipients of the Cal Grant B, with nearly $196 million in aid distributed. Students are also required to submit their financial aid application by March 2nd. Cal Grant B can be used as a living allowance and tuition assistance, and although it can be used at four-year institutions, is targeted towards students enrolling in community college.

If a student is a attending California community college, the base Cal Grant B award is $1,656 annually, and essentially acts as a cash transfer, as tuition is waived for all Cal Grant B recipients at the community college.[2] Beginning in 2015, supplemental awards were introduced to incentivize full-time enrollment. From 2015-16 through 2017-18, the Full-time Student Success Grant (FTSSG) awarded students an additional $600 for students taking at least 12 credits each semester. In 2017-18, the College Completion Grant awarded $1,500 if students enrolled in at least 15 credits a semester. Finally, the Student Success Completion grant combined the two programs for the 2018-19 school year, awarding students $1,250 annually for enrolling in 12-14 credits per quarter, and $4,000 for enrolling in at least 15 credits per quarter.

As a result of the following structure of the Cal Grant B and its supplemental awards, we are able to answer both whether financial aid generally can help improve student outcomes, but whether financial aid tied to course-taking behavior can further improve student success and shorten time to degree.

We focus on and compare four different time periods, marked by changes in the supplemental Cal Grant B awards being offered. The first period ("No Supplemental Grant") spans 2010-2014, where there is no additional aid being offered for students taking a particular number of credits.

[2] This amount represents the full amount a student could receive if they were enrolled as a full-time student. Part-time students receive an amount proportional to the number of credits taken.

The second period spans 2015-16 and the 2016-17 academic years, when the Full-Time Student Success Grant was introduced. The third period covers the 2017-2018 academic year, when the College Completion Grant was introduced, and finally the fourth period spans 2018-19 and 2019-20, when the Student Success Completion Grant ("SSCG") was in place.

3.4. Data

To answer our research questions, we use a quasi-experimental regression discontinuity (RD) research design that exploits the strict income cutoff used to determine eligibility for Cal Grant B financial aid. Comparing the outcomes of those just above and below the cutoff allows us to isolate the impact of Cal Grant B eligibility in a credible and transparent way.

Crucially, our data span the change in the Cal Grant B program that incentivized timely progress toward a degree by awarding students with large financial bonuses for enrolling in 12 or 15 credits. We assess the impact of these incentives by comparing the effects of Cal Grant B eligibility before and after the incentives were in place. Because we have numerous cohorts, we will be able to see if the Cal Grant B effects changed exactly in the year the new incentives began, which will help distinguish a true impact of the incentives from spurious time trends in Cal Grant B impacts.

We implement this approach using a student-level dataset assembled from records from several administrative databases. This dataset will track students longitudinally from the time they first apply for financial aid through their postsecondary schooling experiences. Due to California's large population, we have very large sample sizes and are able to generate statistically precise impact estimates.

3.4.1. California Student Aid Commission. The Cal Grant program and other state financial aid programs are administered by the California Student Aid Commission (CSAC). To apply for state financial aid, students must submit information to CSAC. In particular, applicants for the Cal Grant program need to submit the FAFSA and a verified GPA to CSAC. Thus, CSAC contains information on the universe of applicants for California financial aid, including information on payments for Cal Grant aid as well as other state-funded financial aid programs. We have CSAC data going back to applicants for financial in the 2007-08 school year.

3.4.2. California Community Colleges Chancellor's Office. Data from the California Community Colleges Chancellor's Office (CCCCO) has information on enrollment, academic performance (GPA by term), academic major, credit accumulation, and degree completion for all students enrolled in a public community college in California. Crucially, the CCCCO data includes detailed information on financial aid eligibility and awards. This includes the information from the FAFSA that is used to determine eligibility for the Cal Grant program, Cal Grant awards, CCPG fee waivers, and federal financial aid (e.g., student loans and Pell grants). This will make it possible to conduct analyses specific to the community college sector, including those students who may not have submitted a financial aid application to CSAC. This information is available starting in the 1999-2000 school year.

3.4.3. National Student Clearinghouse. We supplement information from the CCCCO by linking our individual data to college enrollment data from the National Student Clearinghouse (NSC). This is important because some financial applicants near the Cal Grant B eligibility thresholds may decide to attend college outside of the CCC. Furthermore, students who initially enroll in a CCC campus may transfer to colleges outside of these systems. By linking data from the California state agencies to the NSC database, we will be able to observe initial enrollment, transfer, and graduation from colleges outside of the CCC systems. This data spans from 2014-2015 to 2018-2019.

3.4.4. California Department of Education. We also use records from the California Department of Education (CDE) to generate covariates for the analysis. Most importantly are math and reading scores from the assessments administered to all 11th graders attending a California public high school, as well as tests as early as grades 3-8. This information is available for students beginning in the 2001-02 school year. Thus, for students who are matched to the CDE data, 11th grade and at least some middle school scores will be available for all cohorts of financial aid applicants to be used in this study.[3] In addition to test scores, the CDE data also contain information

[3]One note is that for the 2013-2014 academic year, California switched standarized tests from the SBAC to the CST. To help make the test scores more comparable over time, we: 1) find the standardized z-score of each exam score using that year's mean and standard deviation; 2) average a student's 8th grade and 11th grade math and ELA standardized exam score, respectively. For students who we don't have either an 8th grade or an 11th grade test score, we use only the score available.

on participation in English Language Learner programs, receipt of special education services, and other pre-collegiate demographic information.

3.4.5. Sample. The primary sample is formed from CSAC records of financial aid applicants who submit FAFSA and verified high school GPA information. The sample consists of individuals who submit their information to CSAC the year of their high school graduation, and who enroll in the community college the following year. We limit the sample in this way because the Cal Grant entitlement award is only available to high school seniors and those who graduated in the prior year, and to minimize any selection issues for students choosing to enroll in community college at different times.

Four additional sample restrictions are motivated by our research design based on income-eligibility cutoffs, and to reduce concerns about selection. First, the sample is limited to students with high school GPAs above 2.0. Students with GPAs below 2.0 are not eligible for Cal Grant awards and the income cutoff has no bearing on their Cal Grant eligibility. Second, we exclude students who are ineligible for Cal Grants based on other pre-college criteria, such as having assets above the eligibility threshold or not being a California resident. Third, we must be able to match the student to the CDE data, to take advantage of important pre-college information. Last, we constrain our sample to students enrolling in 2010 and beyond, as that is the first we have National Student Clearinghouse data to understand students' postsecondary choices as a result of Cal Grant B eligibility.

Following these restrictions, we form a sample of students organized by academic cohort. The first cohort consists of students who graduated in 2010, and who could first use the Cal Grant B award during the 2010-11 academic year, and our last cohort consists of students who graduated in 2019, and who could first use the Cal Grant B award in 2019-2020. Finally, depending on the outcomes of interest, we further limit the sample to include only students who enroll in at least one credit-bearing course in a California community college.

3.4.6. Summary Statistics. Table 3.1 compares the average characteristics of students who are income-eligible and ineligible for Cal Grant B across the entire sample, not restricted to those who actually enroll in community college. The only difference between these two sets of students are that one set of students are income ineligible for Cal Grant B, while the other set are. However, both groups of students are eligible for Cal Grant B in all other aspects (GPA, assets, etc).

Table 3.1. Summary Statistics - Full Eligible Sample

	Overall (2010-2019)	Income Eligible	Income Ineligible
Asian	0.144	0.147	0.140
Black	0.0624	0.0724	0.0475
White	0.236	0.174	0.328
Latinx	0.531	0.584	0.452
"Other" Race	0.0299	0.0260	0.0358
Average ELA Score	0.0751	-0.0559	0.269
Average Math Score	0.0237	-0.0597	0.147
Male	0.446	0.434	0.465
Econ. Disadvantage	0.604	0.787	0.331
High School GPA	2.842	2.785	2.928
Income	50917.0	21812.5	94321.6
Assets	1620.3	296.5	3594.6
Received CGB (Fall, CSAC)	0.299	0.492	0.0120
Received CGB (Fall, CCC)	0.275	0.451	0.0110
Received Pell (Fall)	0.523	0.734	0.208
Received CCGP (Fall)	0.771	0.875	0.615
Amount Pell Received (Fall)	1151.9	1741.6	272.5
Amount CGB Base Received (Fall)	203.6	334.7	8.069
Amount FTSSG Received (Fall)	28.36	46.73	0.951
Amount Comp. Grant Received (Fall)	4.819	7.995	0.0823
Amount SSCG Received (Fall)	50.27	83.18	1.187
Received CGB (Spring, CSAC)	0.284	0.467	0.0115
Received CGB (Spring, CCC)	0.259	0.426	0.0106
Received Pell (Spring)	0.500	0.701	0.201
Received CCPG (Spring)	0.743	0.845	0.591
Amount Pell Received (Spring)	1091.3	1646.2	263.7
Amount CGB Base Received (Spring)	193.0	317.2	7.813
Amount FTSSG Received (Spring)	25.69	42.32	0.879
Amount Comp. Grant Received (Spring)	3.855	6.112	0.488
Amount SSCG Received (Spring)	45.44	75.15	1.126
Enrolled 2010-2014	0.412	0.420	0.399
Enrolled 2015-2016	0.205	0.206	0.205
Enrolled 2017	0.115	0.114	0.115
Enrolled 2018-2019	0.268	0.260	0.282
Observations	564449	337884	226565

Notes: This sample consists of high school seniors who submitted a financial aid application for the academic year following high school graduation for the first time. These students are also Cal Grant B asset eligible, GPA eligible (GPA $\geq$ 2.0), are a California resident, not in default, and are in the selective service, if male.

We see that Cal Grant B income eligible students are more likely to be Black, Hispanic, or Asian, with lower high school GPAs and lower average ELA and math standardized test scores. Income eligible students are also more likely to have enrolled in community college.

Table 3.2. Summary Statistics - Eligible Sample Enrolled in CCC

	Overall (2010-2019)	Income Eligible	Income Ineligible
Asian	0.145	0.148	0.141
Black	0.0611	0.0707	0.0469
White	0.237	0.175	0.327
Hispanic	0.530	0.582	0.453
“Other” Race	0.0298	0.0259	0.0356
Average ELA Score	0.0694	-0.0608	0.261
Average Math Score	0.0216	-0.0607	0.142
Male	0.449	0.436	0.468
Econ. Disadvantage	0.602	0.786	0.330
High School GPA	2.837	2.781	2.921
Income	51096.5	21949.5	94186.4
Assets	1623.6	301.0	3579.0
Receiving CGB (Fall, CSAC)	0.313	0.517	0.0124
Receiving CGB (Fall, CCC)	0.298	0.491	0.0119
Receiving Pell (Fall)	0.566	0.797	0.224
Receiving CCPG (Fall)	0.834	0.950	0.662
Amount Pell Received (Fall)	1248.9	1894.9	293.9
Amount CGB Base Received (Fall)	220.8	364.2	8.708
Amount FTSG Received (Fall)	30.75	50.86	1.026
Amount Comp. Grant Received (Fall)	5.227	8.702	0.0888
Amount SSG Received (Fall)	54.53	90.55	1.281
Receiving CGB (Spring, CSAC)	0.297	0.489	0.0119
Receiving CGB (Spring, CCC)	0.270	0.445	0.0109
Receiving Pell (Spring)	0.507	0.711	0.205
Receiving CCPG (Spring)	0.746	0.844	0.600
Amount Pell Received (Spring)	1112.5	1682.2	270.2
Amount CGB Base Received (Spring)	201.7	332.7	8.097
Amount FTSG Received (Spring)	27.14	44.89	0.909
Amount Comp. Grant Received (Spring)	4.139	6.582	0.527
Amount SSG Received (Spring)	48.59	80.64	1.203
Enrolled 2010-2014	0.405	0.413	0.393
Enrolled 2015-2016	0.206	0.206	0.205
Enrolled 2017	0.115	0.116	0.115
Enrolled 2018-2019	0.274	0.266	0.286
Observations	520295	310360	209935

Notes: Notes: This sample consists of high school seniors who submitted a financial aid application for the academic year following high school graduation for the first time. These students are also Cal Grant B asset eligible, GPA eligible (GPA $\geq$ 2.0), are a California resident, not in default, and are in the selective service, if male. Furthermore, these students have enrolled in at least one credit-bearing course in a California community college.

Next, conditioning on students who enroll in a California community college, we see in Table 3.2 patterns similar as to the full sample of students. Cal Grant B eligible students are again more likely to be Black, Latinx, or Asian, with lower average ELA and math test scores and high school GPA. There are roughly 100,000 more Cal Grant B income eligible students (or around 20%)

more students enrolled in community college. Although these differences, on average, look large, suggesting that income ineligible and income eligible students are not comparable, our econometric specification relies comparing students on the *margin* of being Cal Grant B income eligible.

3.5. Econometric Specification

The ideal way to answer our research questions would be to conduct a randomized control trial (RCT) with three experimental conditions: (1) eligible for Cal Grant aid with no added incentives for enrolling in 12 or 15 credits, (2) eligible for Cal Grant aid with added incentives for enrolling in 12 or 15 credits, (3) control group. Unfortunately, this type of experiment is unrealistic given that the Cal Grant is an established longstanding statewide entitlement program.

In lieu of an RCT, our research design leverages quasi-experimental variation in eligibility for Cal Grant B awards along with multiple policy changes that generated strong financial incentives for enrolling in 12, and especially 15 credits per semester. We first estimate the causal impacts of Cal Grant B eligibility and award receipt using a regression discontinuity design based on income-eligibility cutoffs. Then, to answer the question concerning the impact of the additional incentives, we assess whether the impact estimates changed once these increased incentives were offered, paying particular attention to whether the changes occur precisely when introduced in 2015-16, 2017-18, and 2018-19.

To understand the intuition behind the RD design based on income-eligibility cutoffs, recall that students with incomes above a cutoff specified by the state are not eligible for the Cal Grant B entitlement award. Conversely, students who meet the other pre-college eligibility criteria (e.g., a high school GPA of 2.0 or higher) can receive Cal Grant B aid if their income is below the cutoff and they enroll in a Cal Grant eligible college for at least 6 credits (half time).

The income cutoff used for Cal Grant B lends itself to a regression discontinuity analysis of the impact of eligibility for the program. The basic idea is to use the sample of students meeting all other pre-college eligibility criteria to compare students whose incomes are just above or just below the income-eligibility cutoff. As Table 3.2 showed, on average, financial aid applicants with incomes below this cutoff are likely to differ in many ways from those whose incomes are above it; however, these differences are likely to be much smaller among individuals whose incomes are close to the cutoff. In fact, under plausible assumptions described below, students with incomes just above and below the income cutoff will be similar in all other dimensions (e.g., college readiness). This reasoning implies that comparisons of students just above and below the income cutoff will isolate the causal effect of eligibility for the Cal Grant B entitlement program. This treatment effect is analogous to the effect of randomly offering students the Cal Grant B entitlement award in the ideal experiment described above.

Not all eligible students receive Cal Grant B aid. This is because some eligible students will not enroll in college (or enroll in a college that is not eligible for Cal Grant funding), not enroll in enough credits, or may turn down the Cal Grant to preserve semesters of lifetime eligibility in the case they decide to transfer to a four-year college. Conversely, some students who are ineligible for the Cal Grant entitlement award receive Cal Grant aid through the competitive Cal Grant program. The competitive Cal Grant awards are intended for "non-traditional" students who have been out of high school for longer than one year, so we do not expect to see many competitive Cal Grant recipients in our dataset of recent high school graduates. Nonetheless, the competitive Cal Grants introduce another potential source of slippage between income eligibility and award receipt, which we can account for.

To isolate the effect of receiving a Cal Grant award, we use fuzzy regression discontinuity methods (Hahn et al., 2001; Imbens and Lemieux, 2008). The idea is to isolate the variation in the receipt of Cal Grant awards generated by the income-eligibility cutoffs to estimate the impact of award receipt on student outcomes. This is analogous to using randomization status as an instrumental variable to estimate the "Local Average Treatment Effect" in an RCT with imperfect compliance.

3.5.1. Regression Discontinuity Model. To estimate the effect of eligibility for the Cal Grant B entitlement award we estimate the following model:

$$Y_i = \theta E_i + f(Income_i) + u_i \tag{3.1}$$

where Y_i represents an outcome of interest (e.g., earning an Associate Degree) for student i, E_i is a dummy variable for having income fall below the eligibility cutoff for the entitlement grant,

$Income_i$ denotes the income used to determine Cal Grant B eligibility, and u_i is a residual. The term $f(Income_i)$ is a flexible function that describes the relationship between the $Income_i$ and the outcome away from the cutoff. The parameter θ represents the causal effect of being income-eligible for the Cal Grant B entitlement grant.

The validity of this approach rests on the assumption that students with incomes that fall just above or below the cutoff are similar in all dimensions related to Y_i. Formally, this condition is that E_i and ϵ_i are uncorrelated. If this is true, then income eligibility is "as good as" randomly assigned in a narrow region around the income-eligibility cutoff. In this sense, our research design closely mimics an RCT, at least close to the cutoff. Below we discuss why we think this is a reasonable assumption in this case.

Continuing with the analogy to an RCT, the parameter θ captures the "intent-to-treat" effect. This combines the effect of being offered the financial aid, as well as incentives to enroll in a greater number of credits to either receive the Cal Grant B (which requires at least half-time enrollment) or to receive the bonuses for enrolling in 12 or 15 credits. This is an important parameter in itself since policymakers want to know what effect an offer of financial aid and these incentives have on student outcomes.

The effect of actually receiving the Cal Grant B is also of interest. To estimate this quantity, we use fuzzy regression discontinuity methods to estimate the following model:

$$Y_i = \beta Award_i + g(Income_i) + \epsilon_i \tag{3.2}$$

$$Award_i = \pi E_i + h(Income_i) + v_i \tag{3.3}$$

where $Award_i$ is an indicator for whether someone received a Cal Grant B award, and the functions g and f capture the relationship between income and the outcome Y_i or award receipt, respectively. In this setup, eligibility E_i serves as the instrumental variable for award receipt. The parameter π represents the "first-stage" effect of eligibility on award receipt, and the parameter β represents the effect of receiving the award.

The assumptions required for the fuzzy RD approach include the standard RD assumptions needed for the "sharp" RD model (i.e., the intent-to-treat effect) along with two other assumptions. The first is that $\pi \neq 0$, which implies that falling above or below the income-eligibility threshold affects Cal Grant B receipt. The second is the "exclusion restriction" (Imbens and Angrist, 1994), which in this case means that falling above or below the income-eligibility threshold only affects student outcomes by affecting the probability of receiving a Cal Grant B.

The empirical models laid out above specify both a constant intent-to-treat effect and effect of award receipt even though both are likely to vary across students. Our research design captures the effect for the subset of students whose incomes are close to the income-eligibility cutoff. This is a standard limitation of the RD design. However, the effect near the income cutoff is highly policy relevant; for instance, it would be useful for understanding changes in the cutoff that were caused by changes to the program's budget.

The estimated impacts of Cal Grant B receipt from the fuzzy RD models are also "local" in the sense that they only pertain to students whose Cal Grant B receipt is determined by whether or not they fall below the income-eligibility cutoff. This means that our estimated impacts might differ from the impacts for groups of students such as those who decide not to enroll in college or enroll in fewer than 6 credits in a semester. Nonetheless, because the Cal Grant B program is means-tested, policymakers are keenly interested in the effects of the program for students whose usage of the aid is determined by their financial need. This is precisely the subgroup of students for whom the fuzzy RD estimates will be relevant.

3.5.2. Validity Checks. The validity of the RD estimates would be undermined if students "sort" around the income cutoff in a way that generates systematic differences between those just above and below the cutoff. This type of sorting is possible in theory; families may be aware of the cutoffs and either report lower income to fall below the threshold or not apply for financial aid if their income is above the threshold.

We empirically test the assumptions underlying our approach. First, we implement the procedure proposed by McCrary (2008) to test whether the density of income is continuous at the relevant cutoffs as it would almost certainly be in the absence of systematic sorting of students onto one side of the cutoff.

McCrary Test

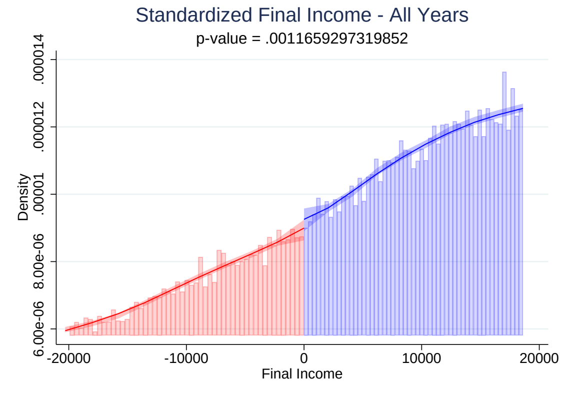


FIGURE 3.1. Final Income Stated

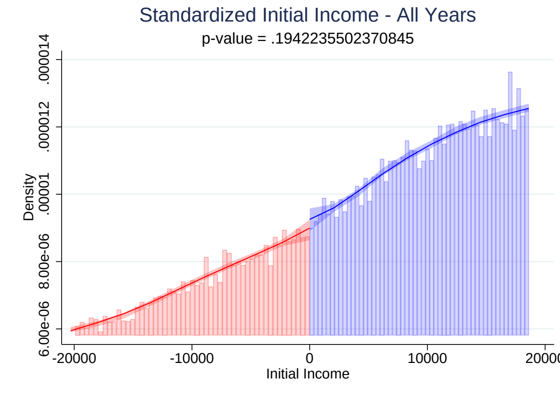


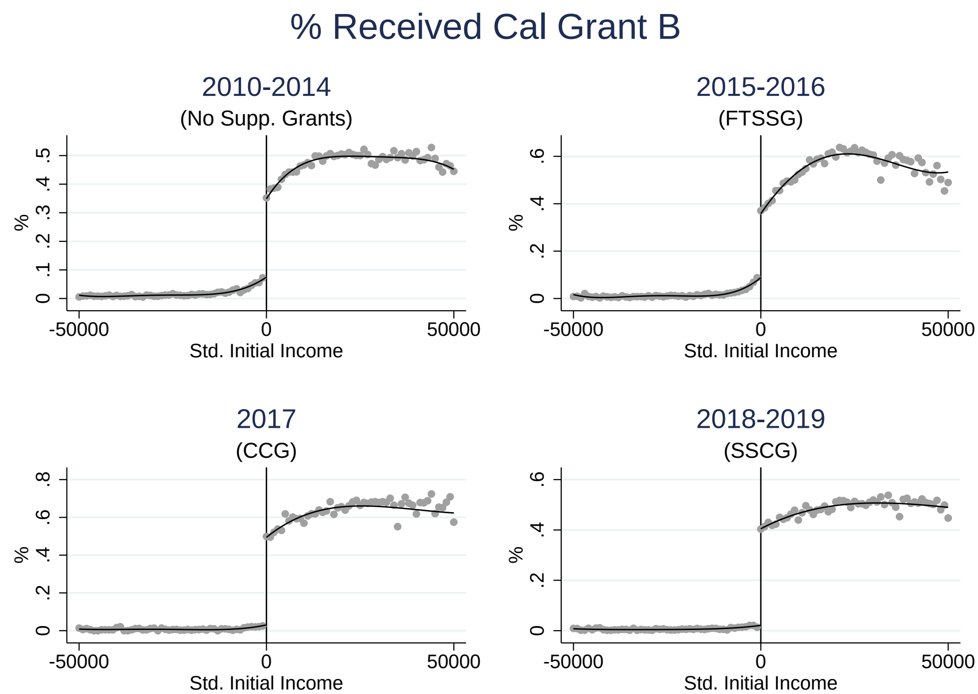
FIGURE 3.2. Initial Income Stated

Because Cal Grant B income and asset requirements change every year based on a student's family size and dependency status, we standardize each student's initial income to make results across years comparable, so that for every cohort, "$0" acts as the income threshold. We use the inverse of this standardized variable so that reading the graph from left to right shows the effect of becoming just eligible for Cal Grant B on various outcomes. That is, observations to the left of the line are students who are income ineligible for Cal Grant B, while students on the right (and on top of the solid vertical line) are income eligible for Cal Grant B.

We find that there *is* evidence of sorting around the income threshold when conducting the McCrary test on the final stated income on a student's financial aid application, as evident in Figure 3.1. The test shows that there is a statistically significant difference in the density of students just below and just above the income threshold.

However, students are allowed to "update" their income, and the final income stated is used to establish whether or not students are eligible for Cal Grant B throughout the financial aid process. We use instead the initial income a student states on their financial aid application as the running variable, and again standardize the initial income using the same process as outlined above. Figure 3.2 shows that, using the initial income, the McCrary test shows there is not a statistically significant change in the density of students at the relevant income cutoff.

FIGURE 3.3. First Stage - Cal Grant B Receipt (CCC)



[Figure 3.3](#) shows the change in likelihood to receive Cal Grant B as students become just income-eligible, across all four time periods of interest. There is a large discrete jump in the probability of receiving Cal Grant B for students to the right of the solid line, or students who are just eligible. There are some students who receive Cal Grant B despite being ineligible based on their initial income stated on their financial aid application, although their final income qualifies them for Cal Grant B.

The sample of students included in Figure 3.3 focuses on first-time students enrolling in at least one credit-bearing course in community college right after graduating high school. We further restrict this sample to include students who we could match to CDE data and who are eligible for Cal Grant B in all other respects (asset eligible, GPA eligible, are California residents who are not in default). Figure 3.3 shows that roughly 35-40% of eligible students received Cal Grant B from

2010-2016 and 2018-2019, with a slightly higher proportion of 50% of eligible students receiving Cal Grant B in 2017.[4]

Table 3.3. First Stage Results - Cal Grant B Receipt

	Overall	No Supp. Grant	FTSSG	CCG	SSCG
Period	2010-2019	2010-2014	2015-2016	2017	2018-2019
	(1)	(2)	(3)	(4)	(5)
CGB Receipt (Yearly, CCC)	0.306***	0.249***	0.262***	0.458***	0.383***
	(0.00672)	(0.0105)	(0.0146)	(0.0155)	(0.00884)
CGB Receipt (Fall, CCC)	0.297***	0.239***	0.258***	0.442***	0.373***
	(0.00660)	(0.0103)	(0.0144)	(0.0155)	(0.00891)
CGB Receipt (Spring, CCC)	0.271***	0.223***	0.235***	0.395***	0.339***
	(0.00647)	(0.0102)	(0.0142)	(0.0157)	(0.00876)
CGB Receipt (Yearly, CSAC)	0.309***	0.250***	0.265***	0.475***	0.383***
	(0.00674)	(0.0106)	(0.0145)	(0.0152)	(0.00895)
CGB Receipt (Fall, CSAC)	0.309***	0.250***	0.265***	0.475***	0.383***
	(0.00673)	(0.0106)	(0.0145)	(0.0152)	(0.00895)
CGB Receipt (Spring, CSAC)	0.294***	0.241***	0.249***	0.443***	0.370***
	(0.00663)	(0.0103)	(0.0146)	(0.0155)	(0.00889)
Observations	520,295	210,596	107,124	60,086	142,489

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, are enrolled in at least one credit-bearing course in a California community college, and are eligible for a Cal Grant B in all aspects, *except* income eligibility.

More formally, we show that there are large, statistically significant, discrete increases in the proportion of just-income-eligible students receiving Cal Grant B compared to just-income-ineligible students in Table 3.3. It is interesting to note that students enrolling during the Completion Grant and Student Success Grant period (2017-2019) are more likely to receive Cal Grant B than students in the previous years.

Following this, we seek to better understand a student's financial aid package more generally. It is also important to check that there are no large changes in eligibility for other sources of financial aid at the same income threshold as the Cal Grant B. If there were, we would be unable to discern which grant receipt caused any observed changes in outcomes.

[4]We are fortunate to have two measures of Cal Grant B receipt from two different datasets - both the CSAC dataset as well as the California Community College dataset. This allows us to cross-check against two different sources the patterns of CGB eligibility. Although we show in our main results using the CCC data, for consistency, as much of our outcomes stem from the community college data, we include results using CSAC data as a robustness check in Appendix C. The results are very similar.

Table 3.4. Financial Aid Outcomes - Receipt

Period	Overall 2010-2019 (1)	No Supp. Grant 2010-2014 (2)	FTSSG 2015-2016 (3)	CCG 2017 (4)	SSCG 2018-2019 (5)
CGB Receipt (Yearly, CCC)	0.306***	0.249***	0.262***	0.458***	0.383***
	(0.00672)	(0.0105)	(0.0146)	(0.0155)	(0.00884)
CGB Receipt (Fall, CCC)	0.297***	0.239***	0.258***	0.442***	0.373***
	(0.00660)	(0.0103)	(0.0144)	(0.0155)	(0.00891)
CGB Receipt (Spring, CCC)	0.271***	0.223***	0.235***	0.395***	0.339***
	(0.00647)	(0.0102)	(0.0142)	(0.0157)	(0.00876)
CGB Receipt (Yearly, CSAC)	0.309***	0.250***	0.265***	0.475***	0.383***
	(0.00674)	(0.0106)	(0.0145)	(0.0152)	(0.00895)
CGB Receipt (Fall, CSAC)	0.309***	0.250***	0.265***	0.475***	0.383***
	(0.00673)	(0.0106)	(0.0145)	(0.0152)	(0.00895)
CGB Receipt (Spring, CSAC)	0.294***	0.241***	0.249***	0.443***	0.370***
	(0.00663)	(0.0103)	(0.0146)	(0.0155)	(0.00889)
FTSG Receipt (Yearly)	0.0872***		0.209***	0.356***	0.0115***
	(0.00344)		(0.0131)	(0.0154)	(0.00167)
FTSG Receipt (Fall)	0.0753***		0.185***	0.307***	0.00897***
	(0.00323)		(0.0120)	(0.0149)	(0.00153)
FTSG Receipt (Spring)	0.0697***		0.171***	0.283***	0.00813***
	(0.00299)		(0.0116)	(0.0139)	(0.00139)
Completion Grant Receipt (Yearly)	0.00690***			0.0445***	0.00912***
	(0.000779)			(0.00594)	(0.00206)
Completion Grant Receipt (Fall)	0.00591***			0.0347***	0.00969***
	(0.000676)			(0.00524)	(0.00176)
Completion Grant Receipt (Spring)	0.00469***			0.0279***	0.00640***
	(0.000610)			(0.00431)	(0.00181)
SSG Receipt (Year)	0.0704***				0.293***
	(0.00259)				(0.00812)
SSG Receipt (Fall)	0.0629***				0.260***
	(0.00242)				(0.00786)
SSG Receipt (Spring)	0.0536***				0.221***
	(0.00221)				(0.00754)
Received CCPG (Year)	0.00145	0.00282	-0.0124*	0.00974	0.0115**
	(0.00331)	(0.00478)	(0.00710)	(0.00988)	(0.00516)
Received CCPG (Fall)	0.000998	0.00107	-0.0125*	0.00817	0.0111**
	(0.00341)	(0.00547)	(0.00663)	(0.0100)	(0.00531)
Received CCPG (Spring)	0.00414	-0.00270	0.00687	0.00313	0.00653
	(0.00456)	(0.00887)	(0.0111)	(0.0174)	(0.00889)
Received Pell (Year)	0.0174**	0.0278**	0.0257*	0.000963	0.00551
	(0.00817)	(0.0121)	(0.0146)	(0.0209)	(0.0128)
Received Pell (Fall)	0.0199**	0.0348***	0.0226	-0.0235	0.00566
	(0.00821)	(0.0123)	(0.0141)	(0.0178)	(0.0130)
Received Pell (Spring)	0.0248***	0.0352***	0.0209	-0.0537***	0.00817
	(0.00835)	(0.0128)	(0.0135)	(0.0181)	(0.0132)
Observations	520,295	210,596	107,124	60,086	142,489

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, are enrolled in at least one credit-bearing course in a California community college, and are eligible for a Cal Grant B in all aspects, *except* income eligibility.

Table 3.4 shows that there are no large, consistent differences in other types of financial aid receipt across the Cal Grant B income threshold. While there are some statistically significant differences. For example, in Pell Grant receipt during the "No Supplemental Grant" time period from 2010-2014 the magnitude of this difference is quite small at 2.78% relative to the large differences of Cal Grant B receipt at 24.9%. Furthermore, this difference in Pell Grant receipt is not consistent across time periods, suggesting that if Pell Grant receipt was the true driver of any observed changes, then we would expect to see any effects on outcomes to only show up where we see Pell Grant discontinuities. In addition, Table 3.5 confirms that students were receiving nontrivial increases in overall Cal Grant B as new supplemental grants were introduced.

Next, we test for discontinuities at the cutoff in pre-determined observable covariates such as race, socioeconomic status, gender, and high school test scores. Just as baseline covariates should be "balanced" between treatment and control observations in an RCT, baseline covariates should "trend smoothly" as a function of income through the eligibility cutoff.

In particular, we might be concerned if we also observe large discrete differences in the average academic ability of students across the income threshold for Cal Grant B eligibility. If this is the case, then any changes we observe with respect to credits taken might be attributed to the differences in academic ability, rather than to receiving Cal Grant B.

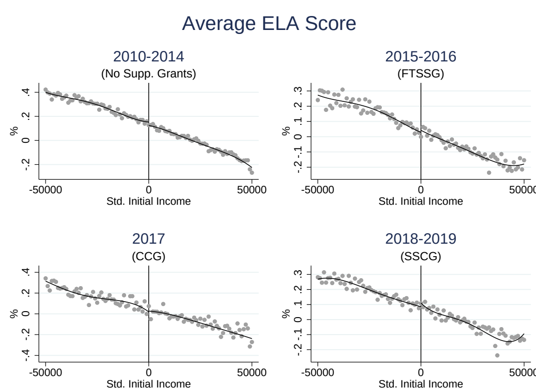


FIGURE 3.4. Average ELA Score

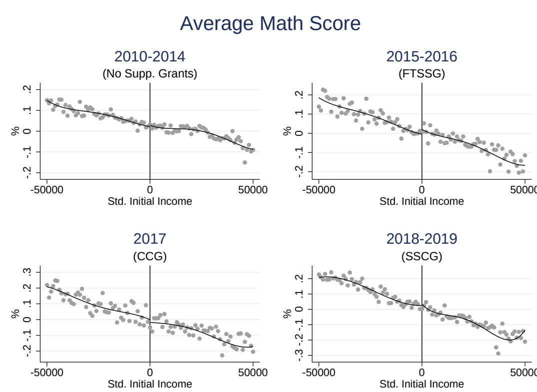


FIGURE 3.5. Average Math Score

Table 3.5. Financial Aid Outcomes - Amount

Period	Overall 2010-2019 (1)	No Supp. Grant 2010-2014 (2)	FTSSG 2015-2016 (3)	CCG 2017 (4)	SSCG 2018-2019 (5)
CGB Amount (Yearly)	416.6***	299***	373.2***	659.1***	567.1***
	(9.493)	(13.79)	(21.85)	(23.77)	(13.43)
CGB Amount (Fall)	221.7***	166.8***	196.2***	342***	292.1***
	(4.948)	(7.337)	(11.09)	(12.17)	(6.904)
CGB Amount (Spring)	203.2***	156.8***	177.1***	309.1***	265.9***
	4.902	7.486	10.99	12.44	6.747
FTSG Amount (Yearly)	60.10***		106***	303.3***	18.75***
	(2.511)		(6.755)	(14.10)	(3.272)
FTSG Amount (Fall)	30.87***		55.10***	155***	8.975***
	(1.334)		(3.570)	(7.666)	(1.687)
FTSG Amount (Spring)	28.73***		51.36***	143.2***	8.964***
	(1.272)		(3.487)	(7.167)	(1.845)
Completion Grant Amount (Yearly)	9.464***			47.25***	19.28***
	1.241			6.599	3.965
Completion Grant Amount (Fall)	5.168***			25.73***	10.56***
	(0.727)			(3.902)	(2.336)
Completion Grant Amount (Spring)	4.210***			20.68***	8.245***
	(0.587)			(3.176)	(1.876)
SSG Amount (Yearly)	140.8***				583.7***
	(5.885)				(19.90)
SSG Amount (Fall)	69.35***				290.1***
	(3.053)				(9.984)
SSG Amount (Spring)	64.08***				264.8***
	(3.037)				(10.84)
Total Cal Grant Amount (Fall)	326.6***	166.8***	250.9***	525.4***	600.5***
	7.726	7.337	14.35	20.23	16.51
Total Cal Grant Amount (Spring)	299.6***	156.8***	227.9***	474.8***	547.8***
	7.519	7.486	14.14	19.97	16.90
Pell Amount (Year)	56.85*	83.70*	37.91	-9.528	31
	(31.75)	(46.46)	(61.98)	(84.55)	(59.29)
Pell Amount (Fall)	34.37**	62.68**	15.06	-6.182	0.207
	(16.21)	(24.47)	(31.81)	(41.27)	(28.93)
Pell Amount (Spring)	42.54***	66.77**	38.10	-1.397	19.54
	(16.47)	(26.09)	(31.41)	(41.24)	(28.46)
Observations	520,295	210,596	107,124	60,086	142,489

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, are enrolled in at least one credit-bearing course in a California community college, and are eligible for a Cal Grant B in all aspects, *except* income eligibility.

We check to see if this is the case, looking to see if there are changes in the average high school test scores in ELA and math, as well as high school GPA.[5] Figure 3.4 shows that the average

[5]We standardize test scores by finding the "z-score" to make them comparable over time. We also take the average of the 8th grade and 11th grade test score as there were no standardized tests taken in 2014 due to institutional changes.

ELA test score trends smoothly across the income threshold, across all time periods of differing supplemental grants. Similarly, Figure 3.5 shows that the average math test score trends smoothly across the income threshold for all different grant periods, suggesting that academic ability, at least measured by standardized test scores, is not discretely different across the income eligibility threshold.

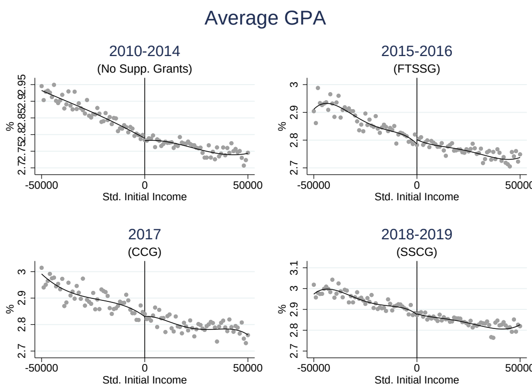


FIGURE 3.6. Average High School GPA

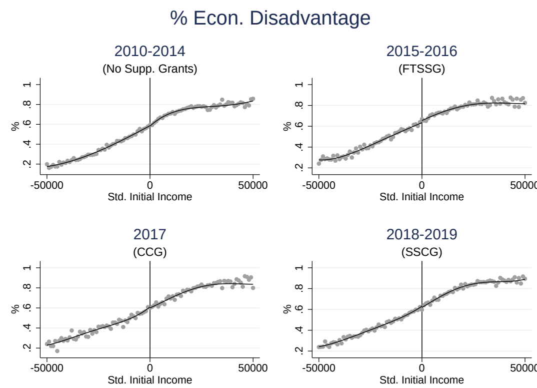


FIGURE 3.7. Economically Disadvantaged

In addition, we check if there are any large changes in the average GPA by income eligibility. Figure 3.6 indicates that there are no large discrete differences in the average GPA among students just above and below the income eligibility threshold for Cal Grant B receipt.

Additional graphs showing the trends in proportion of students by race, gender, and socioeconomic status across initial income suggest that there are no consistent discontinuities across the income threshold for Cal Grant B eligibility.[6] Conducting this check more formally, Table 3.6 shows that there are no consistent statistically significant differences in any of the covariates across all four time periods, and overall (2010-2019).

As a result of these verification checks, we are confident that any changes in the outcomes we observe are a consequence of being Cal Grant B eligible.

[6]These graphs are available in Appendix C.

Table 3.6. Covariates - Validity Check

	Overall	No Supp. Grant	FTSSG	CCG	SSCG
Period	2010-2019	2010-2014	2015-2016	2017	2018-2019
	(1)	(2)	(3)	(4)	(5)
Econ. Disadvantage	-0.00338	-0.00635	0.0180	-0.00318	-0.0112
	(0.00728)	(0.0115)	(0.0128)	(0.0204)	(0.0127)
Average ELA Score	-0.000737	-0.00284	0.0168	-0.00764	0.0127
	(0.00818)	(0.0151)	(0.0187)	(0.0252)	(0.0144)
Average Math Score	0.00458	0.00793	0.0223	-0.0170	7.28e-05
	(0.00882)	(0.0139)	(0.0193)	(0.0257)	(0.0172)
Average GPA	-0.00782	-0.00998	0.00374	0.00217	-0.0153
	(0.00558)	(0.00887)	(0.0117)	(0.0189)	(0.0111)
Hispanic	-0.00875	-0.0131	-0.0155	0.0165	-0.0105
	(0.00714)	(0.0112)	(0.0143)	(0.0209)	(0.0121)
Black	0.00151	-0.000431	-0.00185	0.00694	0.00451
	(0.00318)	(0.00498)	(0.00626)	(0.00821)	(0.00502)
Male	-0.00322	0.00333	0.000972	-0.0392**	-0.00696
	(0.00565)	(0.00873)	(0.0117)	(0.0192)	(0.0115)
White	0.000358	0.00676	-0.00565	-0.0191	0.00781
	(0.00547)	(0.00938)	(0.0111)	(0.0167)	(0.00963)
"Other" Race	0.00116	0.00223	0.00401	-0.00	-0.00134
	(0.00193)	(0.00298)	(0.00452)	(0.00658)	(0.00390)
Asian	0.000387	0.00295	0.0156*	-0.00401	0.000661
	(0.00358)	(0.00680)	(0.00873)	(0.0116)	(0.00764)
Observations	520,295	210,596	107,124	60,086	142,489

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, are enrolled in at least one credit-bearing course in a California community college, and are eligible for a Cal Grant B in all aspects, *except* income eligibility.

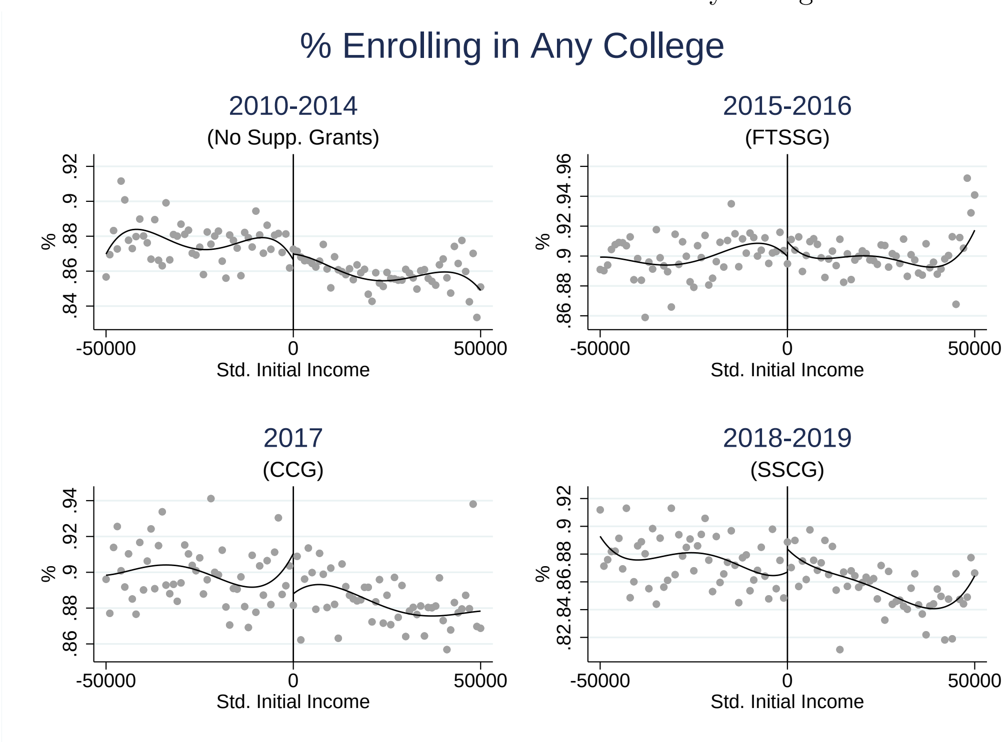
3.6. Results

Our rich and detailed datasets enable us to examine the different impacts of changes in the structure of a large state-wide grant program on numerous student outcomes. For our first research question examining how changes in Cal Grant B financial incentives affect college choice for first-time college students, we measure whether a student enrolls in college and in which college sector (e.g., two-year versus four-year; public versus private).

For our second research question, which examines how changes in Cal Grant B financial incentives affect medium-run outcomes among recent high school graduates, we measure whether students persist to the second year of community college, in addition to the number of credits attempted and earned.

3.6.1. College Choice. We first examine if being Cal Grant B eligible during the introduction of more generous supplemental grants changes students' decision regarding college choice. There are many potential reasons to think why increases in financial aid could change the composition of students attending community college. For one, increased financial aid might induce students who were under financial constraints to enroll in community college, instead of not enrolling in any postsecondary institution. On the other hand, students who would have initially enrolled in four-year institutions may be induced to take advantage of this increased generosity by first enrolling in community college and then transferring to their four-year institution.

FIGURE 3.8. Enrollment Outcome: Any College



For this analysis, we restrict our sample to students who are eligible for Cal Grant B in all aspects, except for income eligibility. We further limit our sample to include CSAC applicants who were able to be matched to the CDE data, which is then linked to the NSC data that has information on the entire set of where students could enroll.[7] Another nuance of the data is that we only possess NSC data for students who are seniors in the 2017-18 cohort, and thus who submit applications for 2018.

TABLE 3.7. Enrollment Results

Period	Overall 2010-2019 (1)	No Supp. Grant 2010-2014 (2)	FTSSG 2015-2016 (3)	CCG 2017 (4)	SSCG 2018 (5)
Any Enrollment	-0.000412	-0.00334	0.00105	-0.0182	0.0189*
	(0.00380)	(0.00595)	(0.00721)	(0.0127)	(0.0105)
4-year Enrollment	-0.00108	-0.00475	0.00775*	-0.000169	-0.000116
	(0.00242)	(0.00450)	(0.00410)	(0.00610)	(0.00444)
CCC Enrollment (NSC)	0.000300	-0.00320	0.00213	-0.0173	0.0203*
	(0.00388)	(0.00604)	(0.00728)	(0.0131)	(0.0107)
CSU Enrollment	-0.000197	-0.00168	0.00317	0.00287	0.00781**
	(0.00144)	(0.00277)	(0.00210)	(0.00356)	(0.00331)
UC Enrollment	-0.00203	-0.00549*	0.00559*	-0.00229	-0.00416
	(0.00181)	(0.00314)	(0.00290)	(0.00405)	(0.00361)
ISP Enrollment	0.000733	0.00258*	-0.000473	-6.87e-05	-0.000781
	(0.000776)	(0.00152)	(0.00117)	(0.00199)	(0.00211)
OOS Enrollment	-0.000271	-0.000103	-0.000230	-0.000983	0.00215
	(0.000645)	(0.00104)	(0.00136)	(0.00194)	(0.00234)
Observations	437,061	204,520	106,645	60,076	65,820

Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, and are eligible for a Cal Grant B in all aspects, *except* income eligibility.

We explore whether these more generous Cal Grant B awards induce students who would have otherwise not enrolled in college to attend any postsecondary college. Figure 3.8 informally shows us that there does not appear to be any large changes in overall college enrollment across the Cal Grant B threshold, and Table 3.7 confirms this more rigorously. However, this result might be hiding movement of students between different college sectors, e.g. students shifting enrollment from the four-year institutions to community colleges. However, again, an informal check of regression discontinuity graphs displaying the average proportion of students enrolling in each type of post-secondary institution[8] shows that there are no discrete differences in enrollment patterns across

[7]The matching process between the NSC data and the CDE data is done by National Student Clearinghouse. Not all students in the CDE data can be found in the NSC data. This can be for various reasons, including, but not limited to FERPA blocks.
[8]These graphs are located in Appendix C.

the Cal Grant B income threshold, and this conclusion is supported by Table 3.7.[9] These results suggest that students are not changing their behavior regarding college choice as a result of being Cal Grant B eligible.

3.6.2. Community College Outcomes. That we do not see large increases in community college participation as a result of Cal Grant B eligibility during the periods of more generous financial aid provides additional support that we should not be concerned about changes in the composition of students driving any observed results, and that any observed changes are a result of changes in Cal Grant B eligibility.

We next investigate if being Cal Grant B-eligible induces students to change their course-taking behavior. Although there was already an incentive to be a 'full-time" student (at 12 credits) to receive the maximum Cal Grant B amount, subsequent changes to the structure of the Cal Grant B award started to offer even more aid tied to students enrolling in 12 credits (or "full-time") in 2015-16 at $600, and then increased again to $1000 in 2017-2018.

However, if students only took 12 on average credits every semester, students would actually be unable to complete their coursework in the suggested amount of time of two years (or four semesters). That is, graduating or transferring within two years actually requires students to take, on average, 15 credits a semester. Consequently, in 2017-2018, Cal Grant B began to offer a total bonus of $2,500 for students to enroll in 15 credits, as well as still offering a bonus for enrolling in at least 12 credits.

Thus, if these more generous supplements tied to enrollment requirements had any effect, we should expect to see particular patterns emerging regarding course enrollment, depending on the time period. For example, we would expect to see a large spike in the proportion of students enrolling in at least 15 credits after 2017, compared to the other periods of 2010-2014 (no supplemental grants) and 2015-2016 (Full-time Student Success Grant, hinging on taking 12+ credits).

[9]We are able to use a different measure of community college-going only, by identifying students who match from the CDE data to the CCCCO data. The results are similar in that we find no statistically signficant changes in the proportion of students going to community college as a result of the changes in grant structure (see, Table C1 in Appendix C).

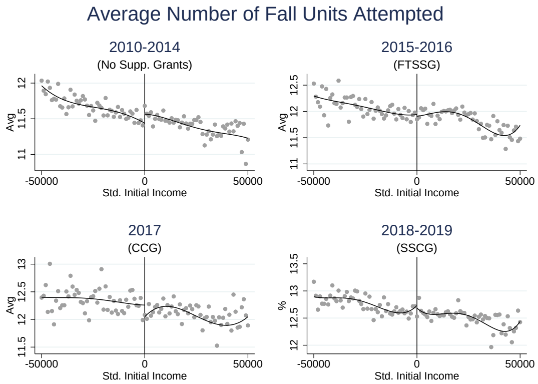


FIGURE 3.9. Fall Units Attempted

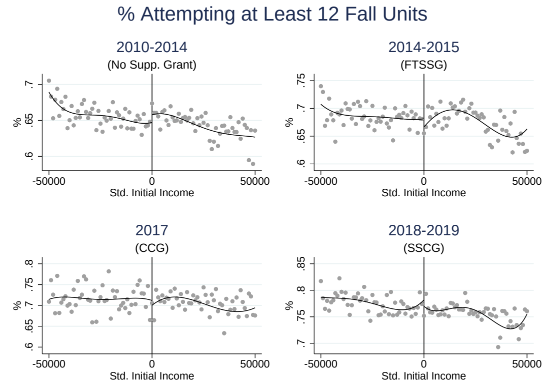


FIGURE 3.10. Attempted at Least 12 Units (Fall)

An informal look at the regression discontinuity graphs indicate, however, that there are no consistent patterns in changes in student behavior regarding course-taking. For example, in Figure 3.9, we do not see an increase in the average number of credits attempted in the fall semester of a student's first year across any time period. There looks to be a small increase for students enrolled in 2010-2014, but column (2) in Table 3.8 suggests that this is not statistically significant, nor is it economically meaningful, at an increase of less than 1 credit.[10] However, it is possible that this continuous measure hides more subtle changes, and so we also check binary outcomes for whether or not students take: 1) at least 12 credits and 2) at least 15 credits.

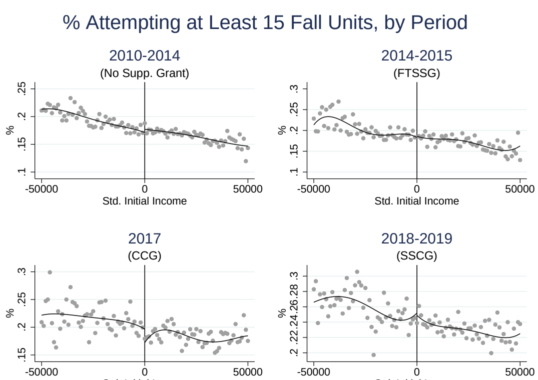


FIGURE 3.11. Attempted at Least 15 Units (Fall)

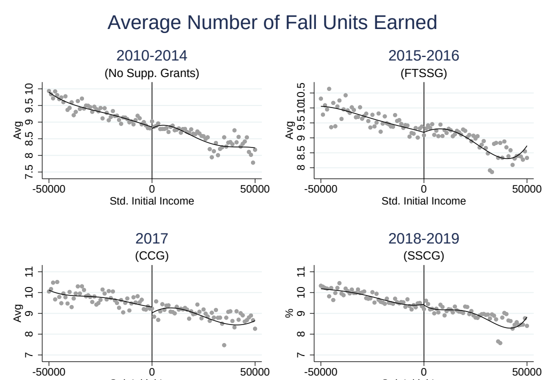


FIGURE 3.12. Units Earned (Fall)

[10]A course is, on average, three credits.

Table 3.8. Community College Outcomes

Period	Overall 2010-2019 (1)	No Supp. Grant 2010-2014 (2)	FTSG 2015-2016 (3)	Comp. 2017 (4)	SSG 2018-2019 (5)
Units Attempted (Fall)	0.00482	0.0713	0.00406	-0.201	-0.0287
	(0.0444)	(0.0661)	(0.0993)	(0.145)	(0.0770)
Attempted at least 12 Units (Fall)	-0.00142	0.00758	-0.00536	-0.0113	-0.0109
	(0.00571)	(0.00820)	(0.0130)	(0.0180)	(0.00987)
Attempted at least 15 Units (Fall)	-0.00172	0.00555	-0.00641	-0.0267	0.00131
	(0.00448)	(0.00643)	(0.00975)	(0.0168)	(0.00845)
Units Earned (Fall)	0.0210	0.107	0.103	-0.169	-0.0639
	(0.0629)	(0.103)	(0.146)	(0.209)	(0.119)
Units Attempted (Spring)	0.0754	0.127	0.180	-0.319	0.0536
	(0.0624)	(0.106)	(0.142)	(0.221)	(0.121)
Attempted at least 12 Units (Spring)	0.0114	0.0149	0.0239*	-0.0183	0.00936
	(0.00567)	(0.00984)	(0.0131)	(0.0197)	(0.0105)
Attempted at least 15 units (Spring)	0.00150	-0.00474	0.00119	0.00110	0.00298
	(0.00424)	(0.00802)	(0.00992)	(0.0164)	(0.00886)
Units Earned (Spring)	0.0985	0.0906	0.106	-0.221	0.137
	(0.0700)	(0.115)	(0.161)	(0.231)	(0.130)
Persist Fall to Fall	0.0113**	-0.00383	0.0221**	-0.0191	0.00773
	(0.00537)	(0.00872)	(0.0110)	(0.0170)	(0.0130)
Observations	520,295	210,596	107,124	60,086	142,489

Standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, are enrolled in at least one credit-bearing course in a California community college, and are eligible for a Cal Grant B in all aspects, *except* income eligibility.

Figure 3.10 and Figure 3.11, however, again suggest that changes in Cal Grant B generosity did not lead to changes in students' course-taking behavior. Although both graphs seem to indicate that students in 2010-2014 who were just eligible for Cal Grant B may have been induced to take more credits, Table 3.8 again indicates that this difference is not statistically significant, nor is the magnitude economically significant. Finally, we check whether being Cal Grant B eligible could affect the number of credits a student earns. It could be that although students are not increasing the number of credits attempted, receiving additional aid might allow them to concentrate more on their courses, and pass them at a higher rate. However, Figure 3.12 does not indicate that there are large differences in the number of credits earned across the Cal Grant B income threshold.

There are a few explanations for why we see null results. One reason could be that even with the increase in generosity, it still would not compensate for the loss of a part-time job. However, quick calculations suggest that if a student was working at a minimum wage of $15 an hour, 20 hours a week, for 10 weeks (the length of a semester), they'd make $3,000, which would be less than the maximum potential under the Student Success Completion Grant introduced in 2018. This again suggests that neglecting to take enough credits is inefficient.

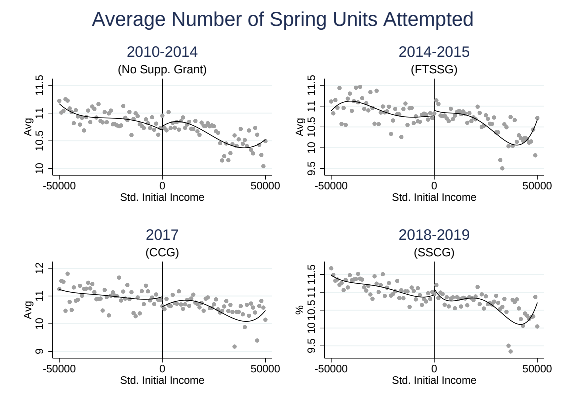


Figure 3.13. Spring Units Attempted

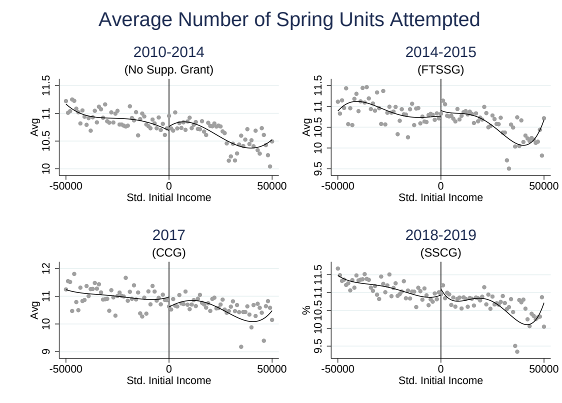


Figure 3.14. Attempted at Least 12 Units (Spring)

Another potential reason for why students are not responding to increased generosity could be that first-time students are unaware of all the changing nuances tied to their financial aid package, and that they'll have learned about it in time to implement behavioral changes by the spring semester. We next test this hypothesis by seeing if students begin to change their behavior during the spring semester of their first year, when they might be more aware of the conditions associated with their financial aid.

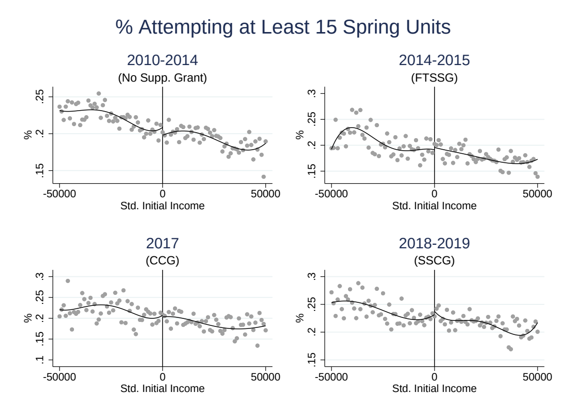


Figure 3.15. Attempted at Least 15 Units (Spring)

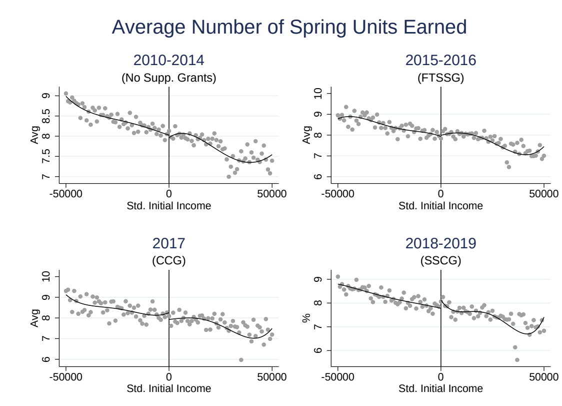


Figure 3.16. Units Earned (Spring)

However, Figure 3.13 suggests that this might not be the case either. Unfortunately, there seems to be no observable differences in the course-taking behavior of students who are just Cal Grant B income eligible. Similarly, there are no changes in the proportion of income-eligible students taking at least 12 credits (Figure 3.14) or students taking at least 15 credits (Figure 3.15). Figure 3.16 indicates that there are no changes in the average number of units earned either in the Spring. In addition, Figure 3.17 and Figure 3.18 indicates that there are no changes in the proportion of students persisting to either the spring semester or next year's fall semester.

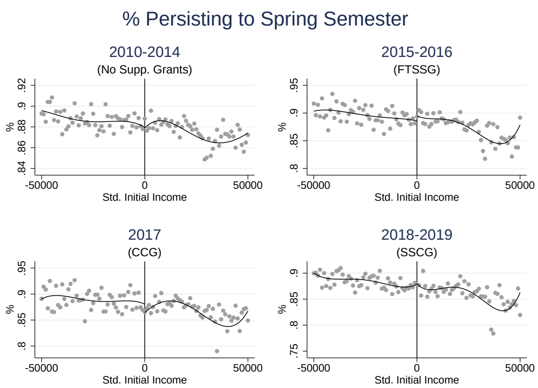


FIGURE 3.17. Persist to 1st Year Spring

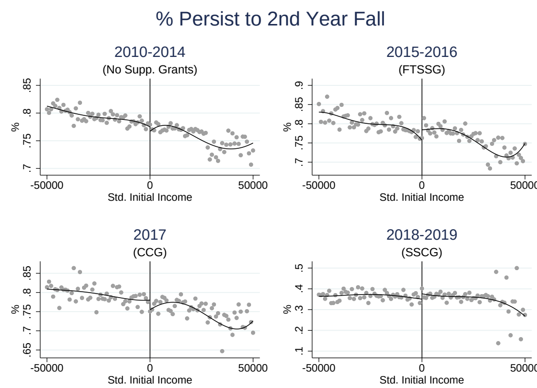


FIGURE 3.18. Persist to 2nd Year Fall

These results regarding students' course-taking behavior in the spring semester, after they have feasibly had time to learn more about their financial aid package, suggests that even with the additional semester, students are still not aware of additional bonuses for full-time enrollment. In fact, a survey of CCC financial aid applicants has suggested that students are not aware of its separate components, and rather view their financial aid as a large lump sum instead of being able to pinpoint the amount and from which source a particular portion might come.

Specific facts regarding this particular context might also indicate why community college students in California are so unaware about where they're getting their financial aid from, and any conditions tied to it. As Table 3.2 indicates, 95% of Cal Grant B income-eligible students also received the CCPG waiver, which essentially covers all of their community college tuition. This means, many students probably never have a need to look at their college tuition bill and actively assign certain financial awards towards certain bill line items. Our results, in conjunction with survey results,[11] suggest that students might be more unaware of the specific details of their financial aid packages than previously assumed. Our findings have important policy implications regarding the dissemination of important information surrounding the details of each financial aid award.

3.7. Conclusion

Combining financial aid with academic requirements meant to expedite student success has become an important policy tool to address financial constraints and low completion rates across colleges. As community colleges in particular become more and more popular as a way to increase access to postsecondary education, understanding the effects of financial aid on this understudied population has become increasingly crucial for policymakers. California policymakers in particular began to implement structural changes to the Cal Grant B, a large need-based state financial aid program targeted towards community college students. Of particular interest is an increase in grant amounts conditional on students taking 15 or more credits that occurred in 2017-2018.

We leverage changes in generosity tied to full-time enrollment requirements to understand how these incentives influence student behavior and academic success. We find that despite increasingly generous financial aid tied to credit requirements intended to help support and encourage students to achieve their college goals, students do not change their course-taking behavior to take advantage of these increases in award amount. Further investigation in conjunction with survey results suggest that students are not learning about their financial aid packages as time progresses.

Several factors might help explain the lack of effects these structural changes in Cal Grant B had on student behavior, in particular in the community college context. As many California community college students have their fees waived entirely through the CCPG fee waiver, it is likely they never receive a line-item bill regarding tuition costs, and how their financial aid might cover those costs. This further minimizes the interaction students might have with their financial aid package. Our results suggest that increased dissemination of the details of specific grants might be critical in facilitating efficient financial aid policies.

[11] Forthcoming. Internal survey results.

APPENDIX A

Remedial Education Reform in California and Community College Student Outcomes

Figure A1. English Course Taking

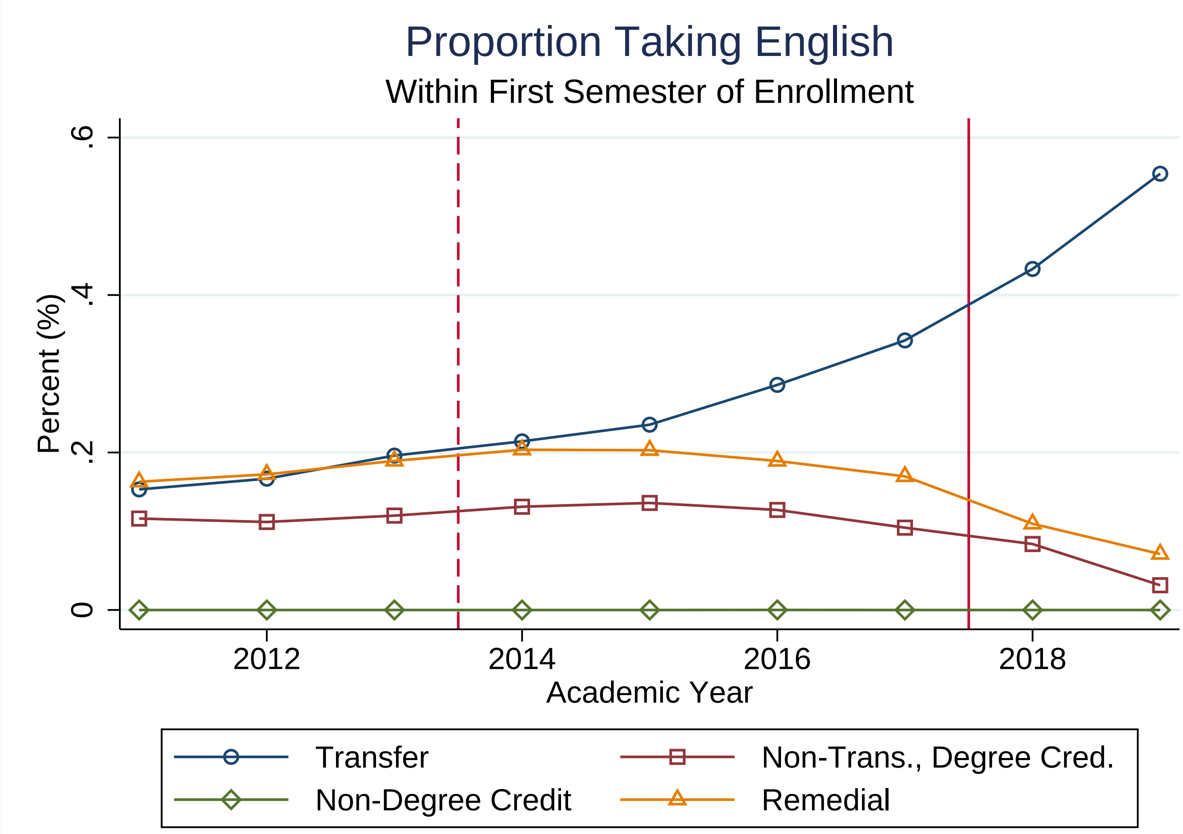


FIGURE A2. Math Course Taking

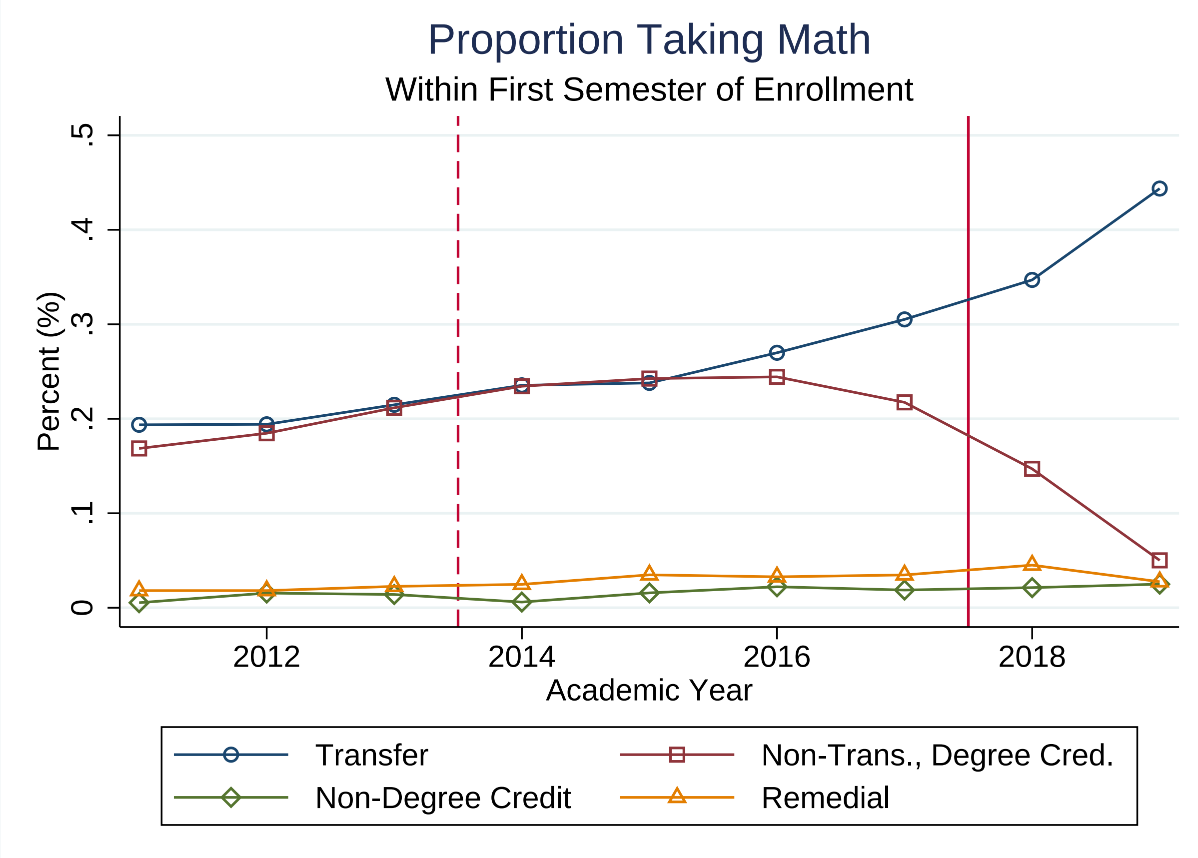


Table A1. Prediction Model Coefficients

	Remedial Eng. Status (1)	Remedial Math Status (2)
Regressors:		
-ELA Z-Score × ELA Perf. Level	-0.176***	-0.088***
	(0.017)	(0.010)
-ELA Z-Score^2	-0.297***	-0.105***
	(0.021)	(0.013)
-Math Perf. Level	-0.110***	-0.434***
	(0.011)	(0.022)
-ELA Z-Score	-0.165**	-0.081**
	(0.065)	(0.037)
-Hispanic	0.171***	0.099***
	(0.032)	(0.036)
-White	-0.176***	-0.100**
	(0.032)	(0.042)
-Male	-0.189***	-0.267***
	(0.018)	(0.020)
-Math Z-Score × Math Perf. Level	-0.018**	-0.086***
	(0.009)	(0.015)
-Math Z-Score	0.037	0.108***
	(0.030)	(0.041)
-Asian	0.180***	-0.345***
	(0.043)	(0.050)
-Disabled	0.204***	0.149***
	(0.042)	(0.040)
-Parent Education Level	0.026***	0.021***
	(0.005)	(0.005)
-Age (in months)	0.010***	0.008***
	(0.001)	(0.001)
-Economic Disadvantage	0.137***	0.077***
	(0.017)	(0.020)
-Limited English Proficiency	0.114***	0.063**
	(0.041)	(0.026)
-ELA Perf. Level	-0.294***	
	(0.016)	
-Black		-0.062
		(0.051)
-Math Z-Score^2		0.004
		(0.007)
-Other Race		-0.057
		(0.045)
-Constant	-0.487**	-1.655***
	(0.222)	(0.187)
Observations	281816	287989
Y Mean	0.176	0.165

List of Covariates Included in Lasso Process:

- Age (in months)
- Age^2
- Age^3
- 6th Grade ELA Standardized Scale Score
- 6th Grade ELA Standardized Scale $Score^2$
- 6th Grade ELA Standardized Raw Score
- 6th Grade ELA Performance Level
- 6th Grade ELA Performance Level × 6th Grade ELA Standardized Scale Score
- 6th Grade Math Standardized Scale Score
- 6th Grade Math Standardized Scale $Score^2$
- 6th Grade Math Standardized Raw Score
- 6th Grade Math Performance Level
- 6th Grade Math Performance Level × 6th Grade Math Standardized Scale Score
- Parent's Education
- Socioeconomic Disadvantaged
- Asian
- Black
- Hispanic
- White
- "Other" Race
- Disability
- Limited English Proficiency
- Gender
- Language
- English Proficiency Level
- Migrant

- Reclassified English Proficiency
- Charter School
- Gifted and Talented
- 6th Grade Science Subject
- 6th Grade Science Raw Score
- 6th Grade History Subject
- 6th Grade History Raw Score
- CST Math Subject

Figure A3. Proportion of Students Enrolled in Remedial English, by Predicted Remedial English Status

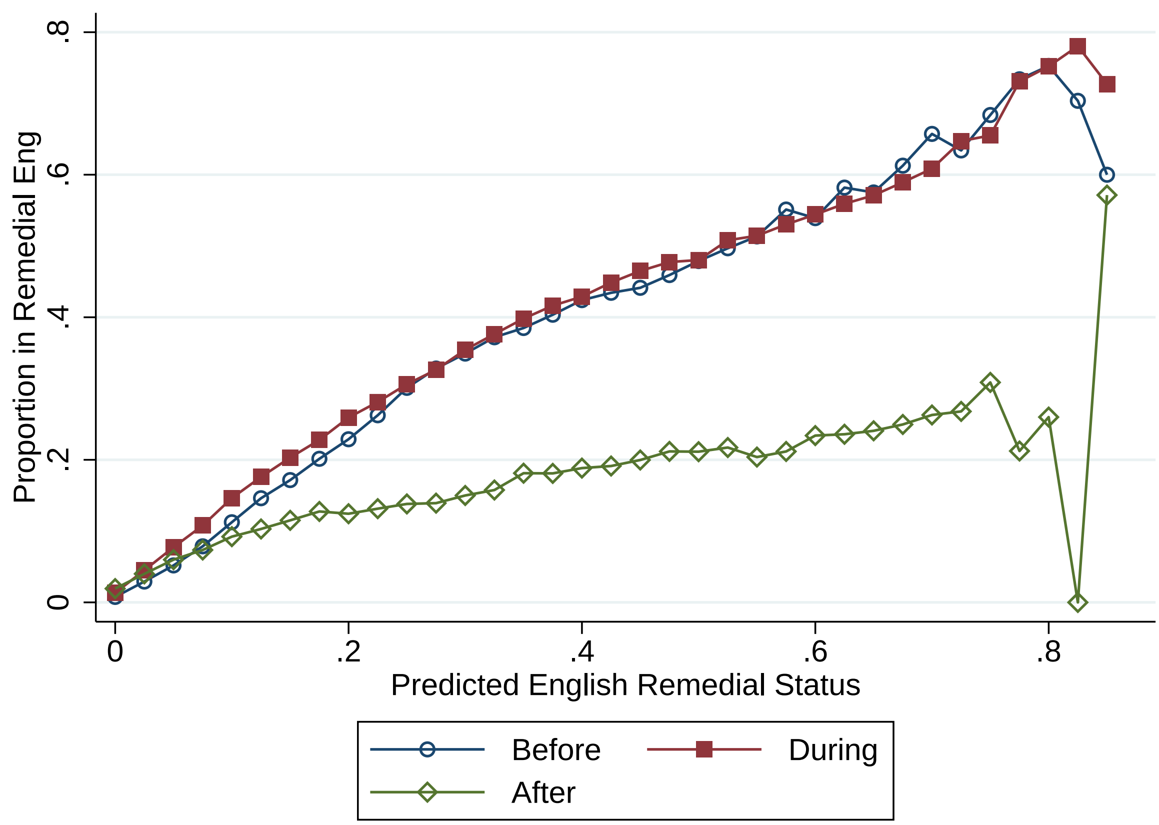


Figure A4. Proportion of Students Enrolled in Transfer English, by Predicted Remedial English Status

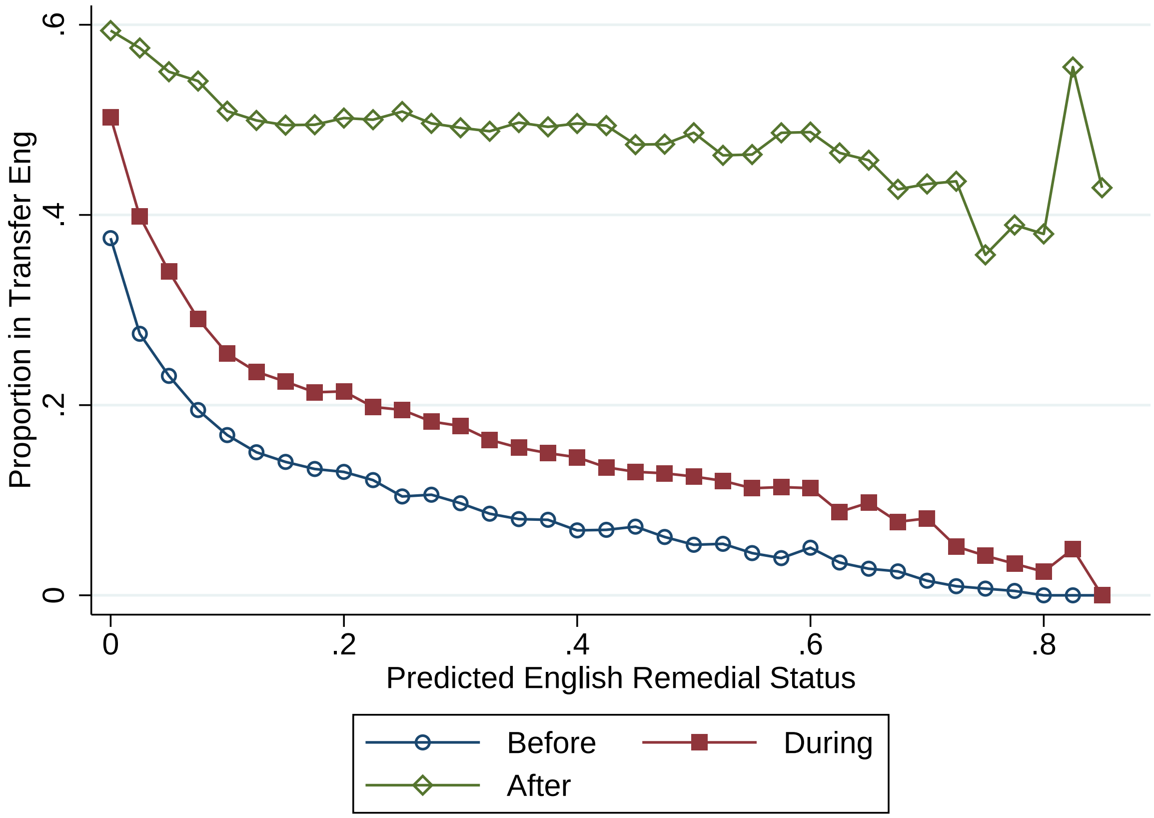


Figure A5. Proportion of Students Enrolled in Remedial Math, by Predicted Remedial Math Status

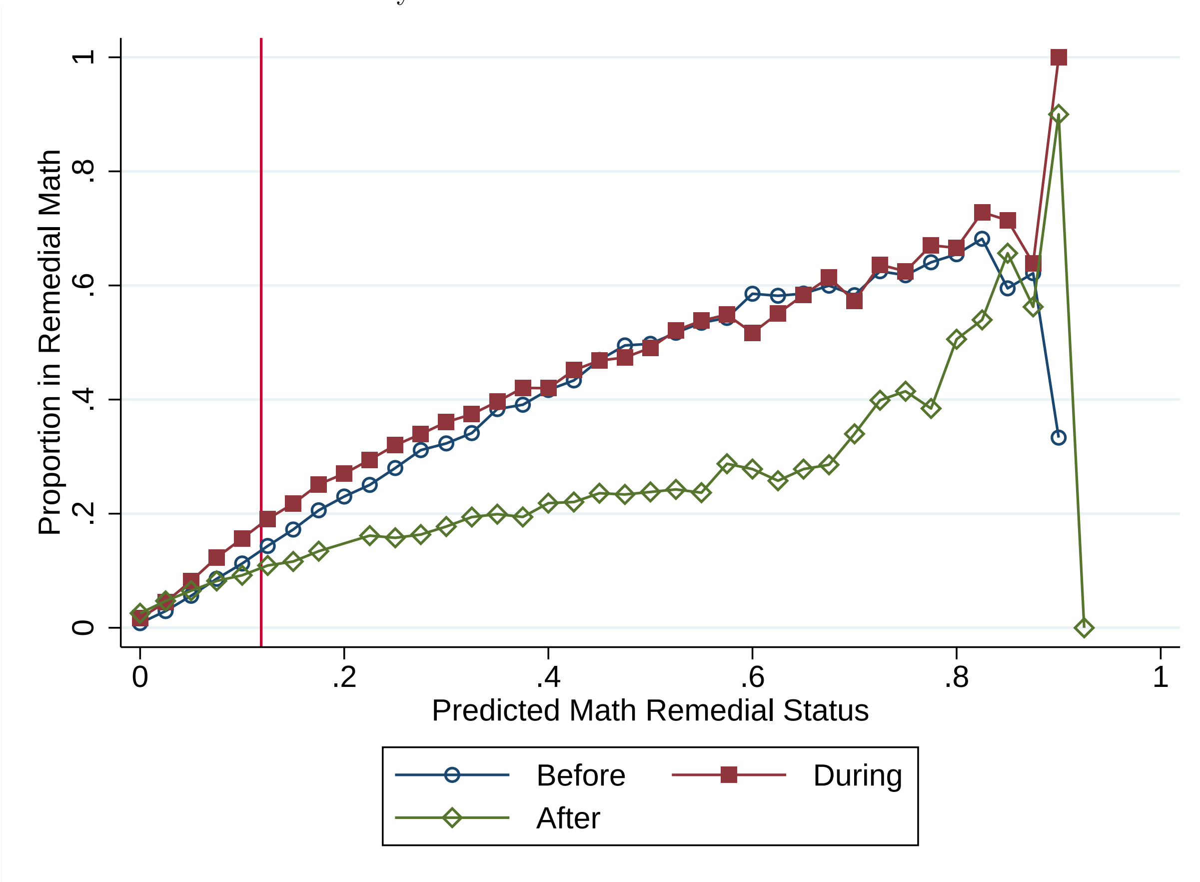


FIGURE A6. Proportion of Students Enrolled in Transfer Math, by Predicted Remedial Math Status

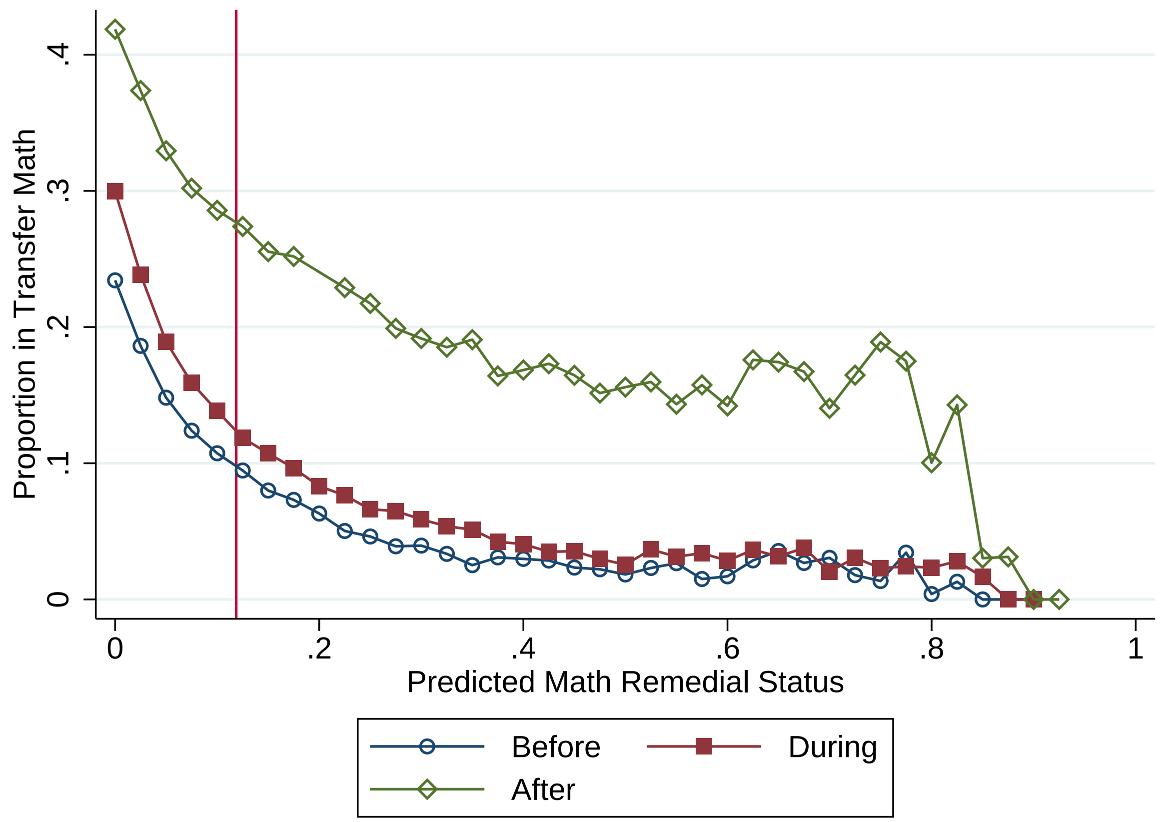


Figure A7. Average ELA Z-Score of Students
by Predicted Remedial English Status

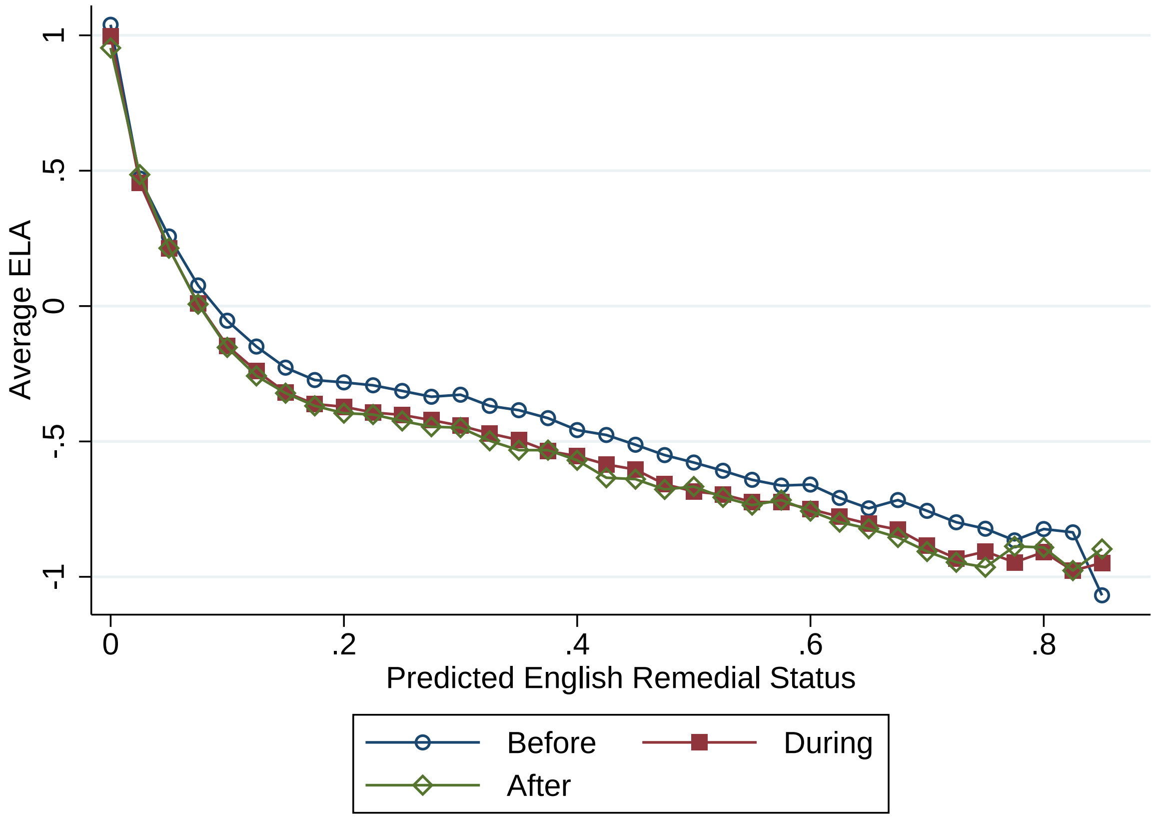


Figure A8. Average Math Z-Score of Students, by Predicted Remedial English Status

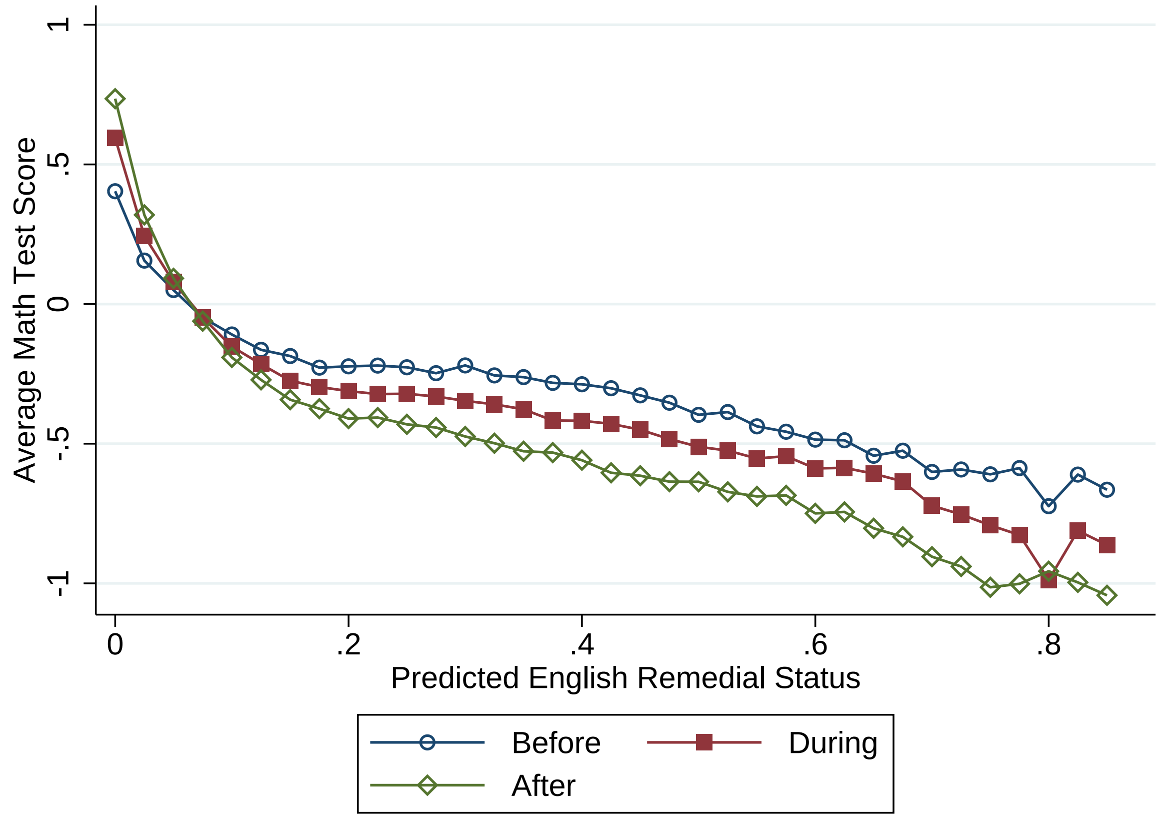


Figure A9. Average ELA Z-Score of Students, by Predicted Remedial Math Status

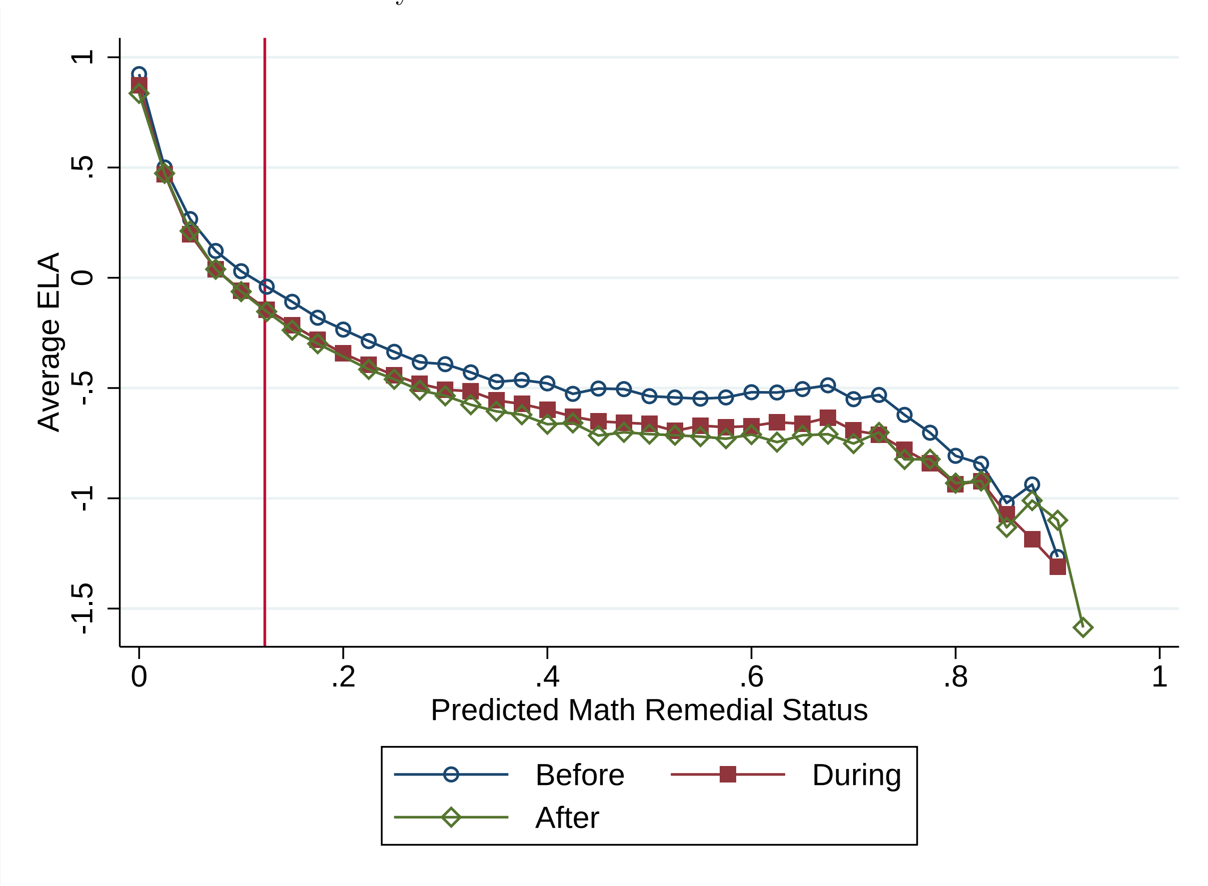


FIGURE A10. Average Math Z-Score of Students, by Predicted Remedial Math Status

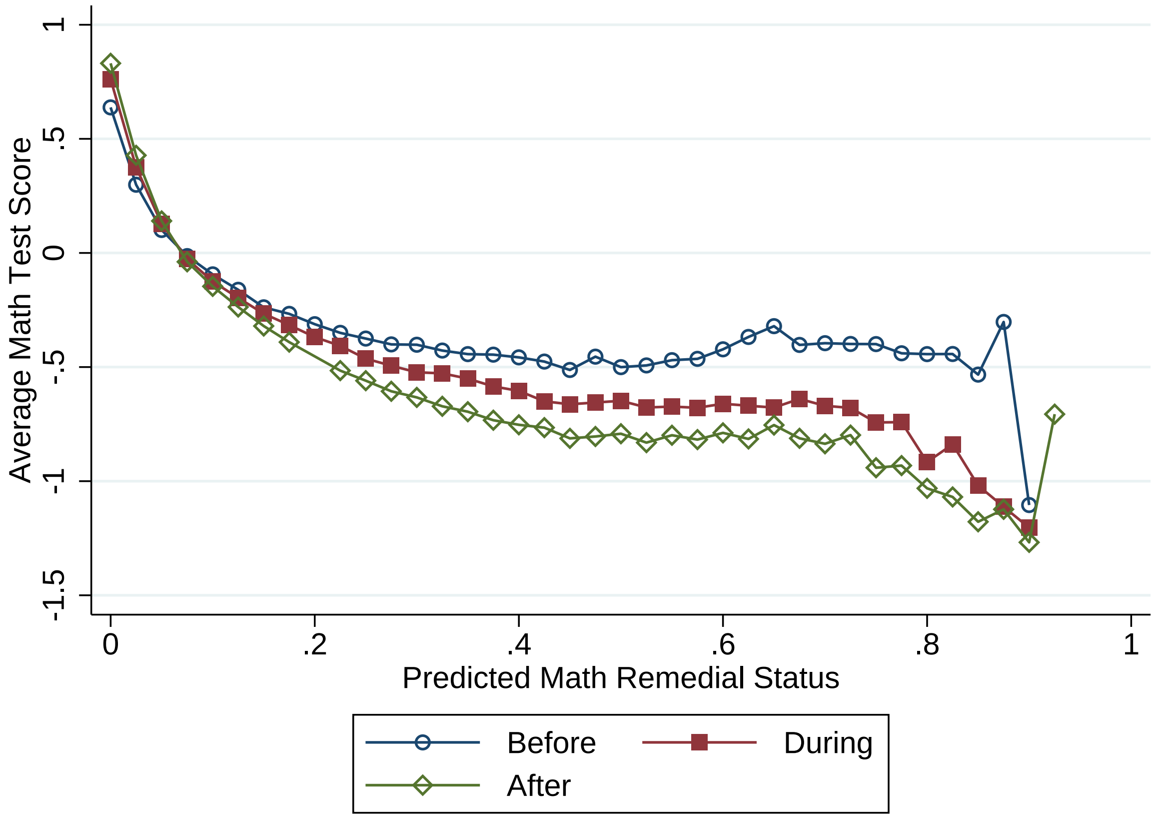


Table A2. Overall Units, By Math Quartile

	Overall	1st Qrt	2nd Qrt	3rd Qrt	4th Qrt.
Total Units Attempted	(1)	(2)	(3)	(4)	(5)
During	0.696***	0.721***	0.715***	0.697***	0.626***
	(0.069)	(0.099)	(0.079)	(0.082)	(0.076)
After	1.130***	1.132***	1.123***	1.143***	1.085***
	(0.091)	(0.121)	(0.095)	(0.108)	(0.124)
Average	10.37	10.83	10.64	10.38	10.16
Total Units Earned					
During	0.355***	0.537***	0.424***	0.274***	0.187***
	(0.044)	(0.071)	(0.056)	(0.053)	(0.051)
After	0.324***	0.758***	0.391***	0.165**	-0.047
	(0.064)	(0.083)	(0.077)	(0.083)	(0.089)
Average	7.62	8.63	8.54	7.56	7.11
Observations	951506	228026	241467	242209	238062
Completion Rate					
Before	0.735	0.797	0.803	0.728	0.700
During	0.510	0.745	0.593	0.393	0.299
After	0.287	0.670	0.356	0.144	0.043
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

Table A3. Transfer Units, By Math Quartile

Transfer Units Attempted	Overall (1)	1st Qrt (2)	2nd Qrt (3)	3rd Qrt (4)	4th Qrt. (5)
During	1.037***	0.845***	0.896***	0.988***	1.235***
	(0.054)	(0.104)	(0.065)	(0.063)	(0.064)
After	2.361***	1.847***	2.151***	2.464***	2.724***
	(0.081)	(0.146)	(0.091)	(0.091)	(0.112)
Average	8.09	9.52	8.54	7.85	7.02
Transfer Units Earned					
During	0.872***	0.717***	0.773***	0.831***	1.007***
	(0.039)	(0.079)	(0.051)	(0.043)	(0.062)
After	1.571***	1.418***	1.452***	1.531***	1.631***
	(0.062)	(0.110)	(0.072)	(0.068)	(0.092)
Average	6.08	7.69	6.55	5.81	5.02
Observations	951506	228026	241467	242209	238062
Completion Rate					
Before	0.752	0.808	0.767	0.740	0.715
During	0.841	0.849	0.863	0.841	0.815
After	0.665	0.7628	0.675	0.621	0.599
Student Controls	X	X	X	X	X
High School FE	X	X	X	X	X
College FE	X	X	X	X	X
Predicted Treatment Intensity	X	X	X	X	X

Notes: For purposes of comparison, the average is calculated using only students enrolled in community college before any policy change (2011-2013). Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months), and standardized test scores. Standard errors are clustered at the community-college level.

APPENDIX B

Minimum Wage and Higher Education

Table B1. City Fixed Effects Instead of High School Fixed Effects

	Any College	2 Year (CCC Only)	4 Year (CSU, UC, OOS, ISP)
	(1)	(2)	(3)
Minimum Wage	0.689***	-0.022	0.846***
	(0.243)	(0.320)	(0.211)
Observations	3608106	3608106	3608106

	CSU	UC	ISP	OOS
	(4)	(5)	(6)	(7)
Minimum Wage	0.358*	0.366***	0.079*	0.037
	(0.185)	(0.139)	(0.046)	(0.117)
Observations	3608106	3608106	3608106	3608106
City FE	X	X	X	X
Student Controls	X	X	X	X
City-Time Trend	X	X	X	X

Notes: Minimum wage measured in dollars. Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. Standard errors are clustered at the city level.

Table B2. HS-Time Trends Instead of City-Time Trends

	Any College (1)	2 Year (CCC Only) (2)	4 Year (CSU, UC, OOS, ISP) (3)	
Minimum Wage	0.461**	-0.021	0.601***	
	(0.222)	(0.250)	(0.197)	
Observations	3607986	3607986	3607986	
	CSU (4)	UC (5)	ISP (6)	OOS (7)
Minimum Wage	0.422*	0.139	0.022	0.019
	(0.236)	(0.103)	(0.047)	(0.116)
Observations	3607986	3607986	3607986	3607986
High School FE	X	X	X	X
Student Controls	X	X	X	X
HS-Time Trend	X	X	X	X

Notes: Minimum wage measured in dollars. Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. Standard errors are clustered at the city level.

Table B3. No Time Trends; City Fixed Effects Instead of HS Fixed EFfects

	Any College (1)	2 Year (CCC Only) (2)	4 Year (CSU, UC, OOS, ISP) (3)	
Minimum Wage	1.732***	0.420	1.262***	
	(0.489)	(0.474)	(0.242)	
Observations	3608106	3608106	3608106	
	CSU (4)	UC (5)	ISP (6)	OOS (7)
Minimum Wage	0.341*	0.474**	0.128*	0.343*
	(0.182)	(0.214)	(0.076)	(0.201)
Observations	3608106	3608106	3608106	3608106
City FE	X	X	X	X
Student Controls	X	X	X	X

Notes: Minimum wage measured in dollars. Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. Standard errors are clustered at the city level.

Table B4. Borusyak et al (2021) Estimator

	Any College	2 Year (CCC Only)	4 Year (CSU, UC, OOS, ISP)
	(1)	(2)	(3)
Minimum Wage	10.043***	3.928***	5.708***
	(0.658)	(0.725)	(0.366)
Observations	3594079	3594079	3594079

	CSU	UC	ISP	OOS
	(4)	(5)	(6)	(7)
Minimum Wage	1.161***	2.893***	0.007	1.529***
	(0.399)	(0.492)	(0.143)	(0.358)
Observations	3594079	3594079	3594079	3594079
City FE	X	X	X	X

Notes: Minimum wage measured in dollars. Student controls include indicators for gender, disability, race, and socioeconomic disadvantaged, with linear controls for age (in months) and standardized test scores. Standard errors are clustered at the city level.

APPENDIX C

Evaluating Incentives for Full-time Enrollment at California Community Colleges

Figure C1. Covariate: Asian

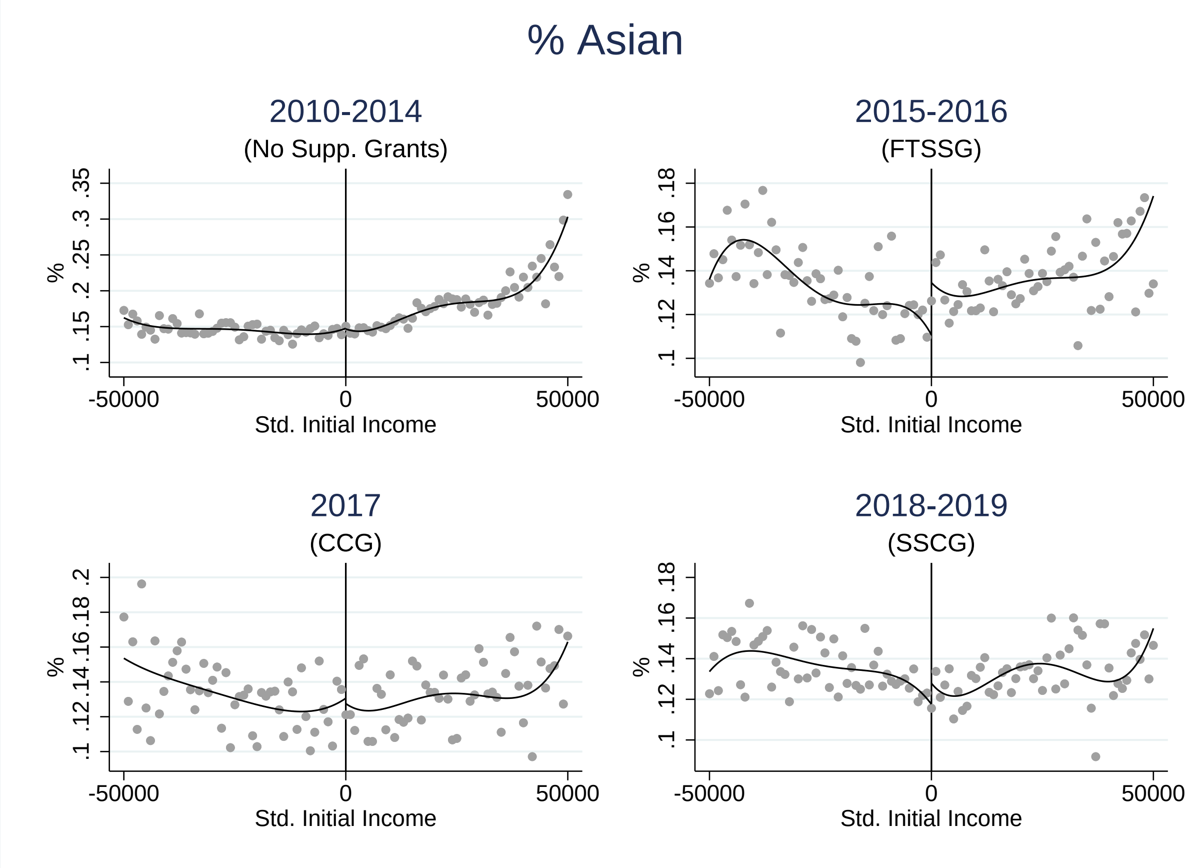


Figure C2. Covariate: Black

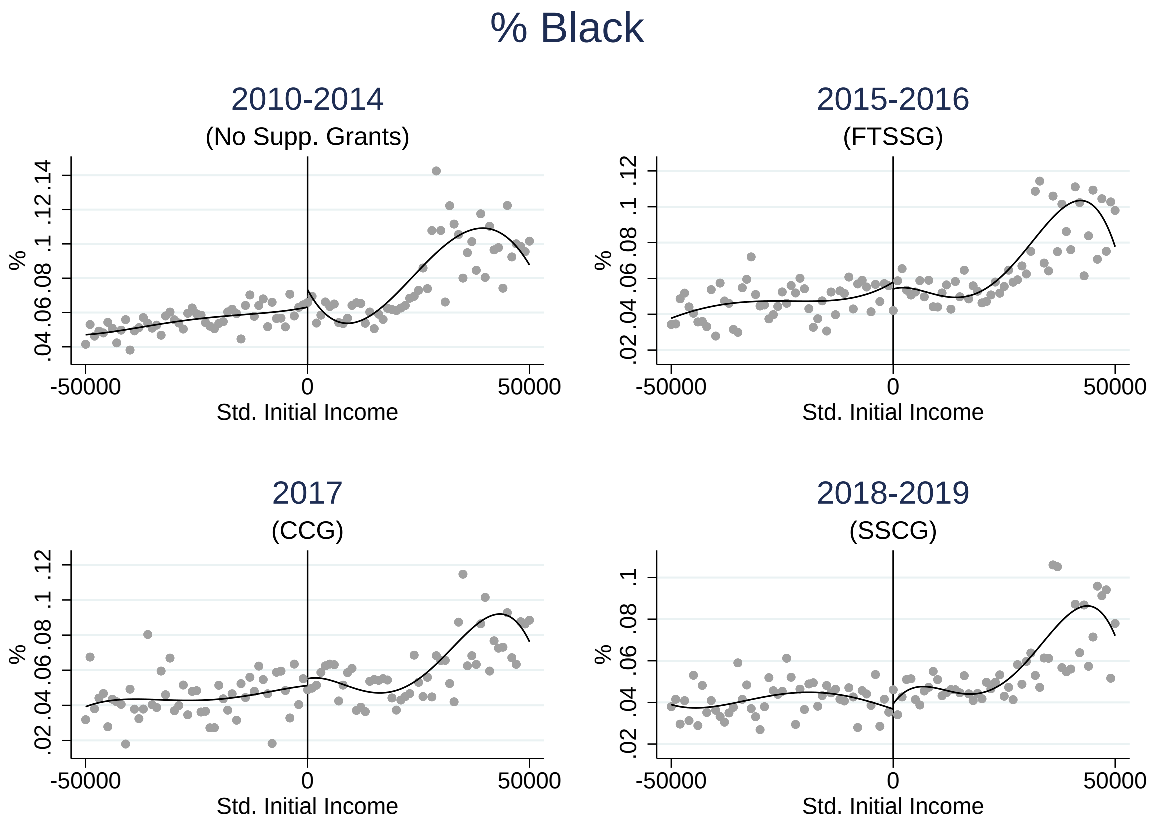


Figure C3. Covariate: Latinx

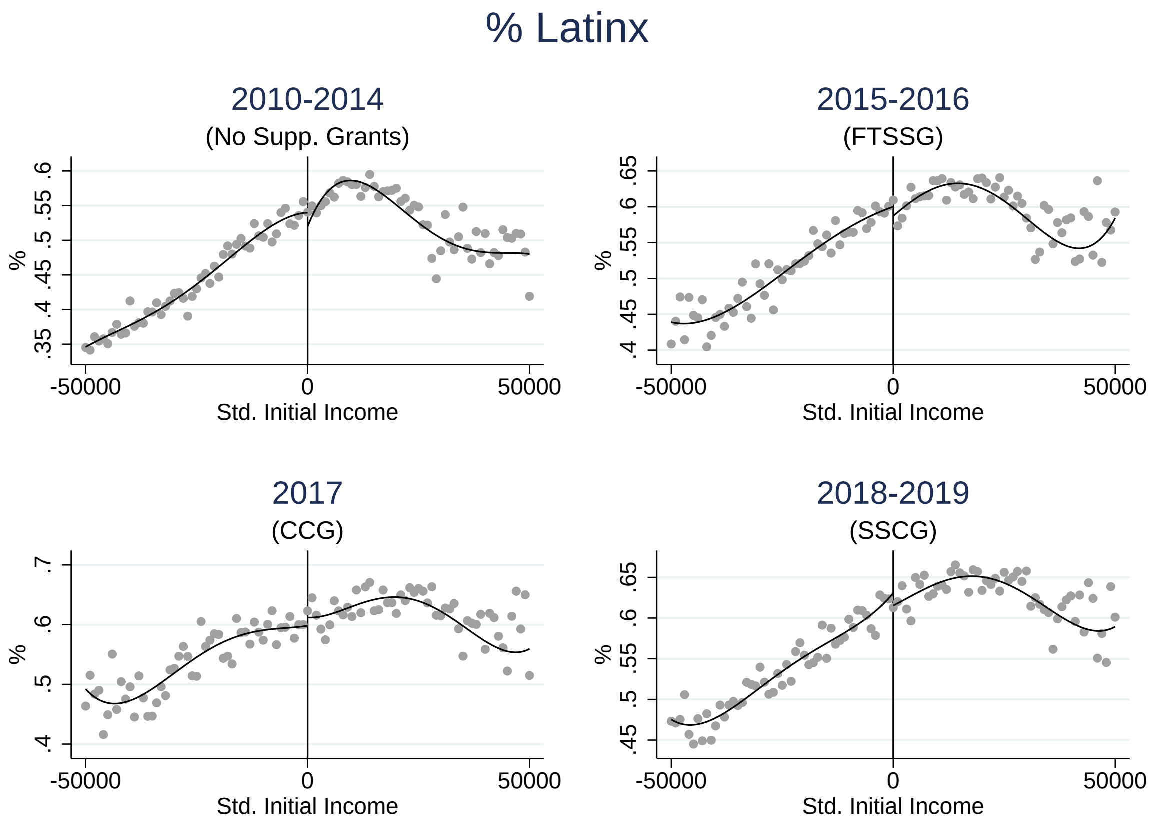


FIGURE C4. Covariate: White

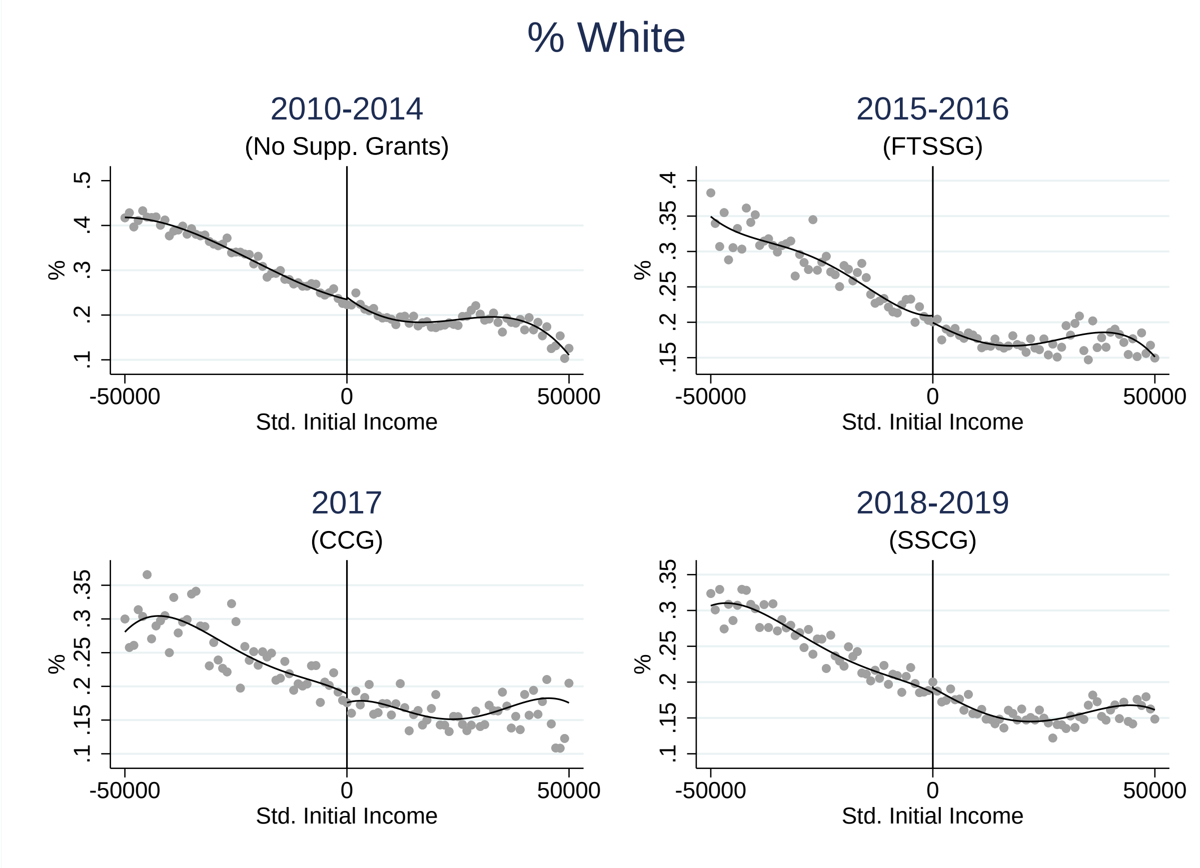


Figure C5. Covariate: Other Race

FIGURE C6. Covariate: Male

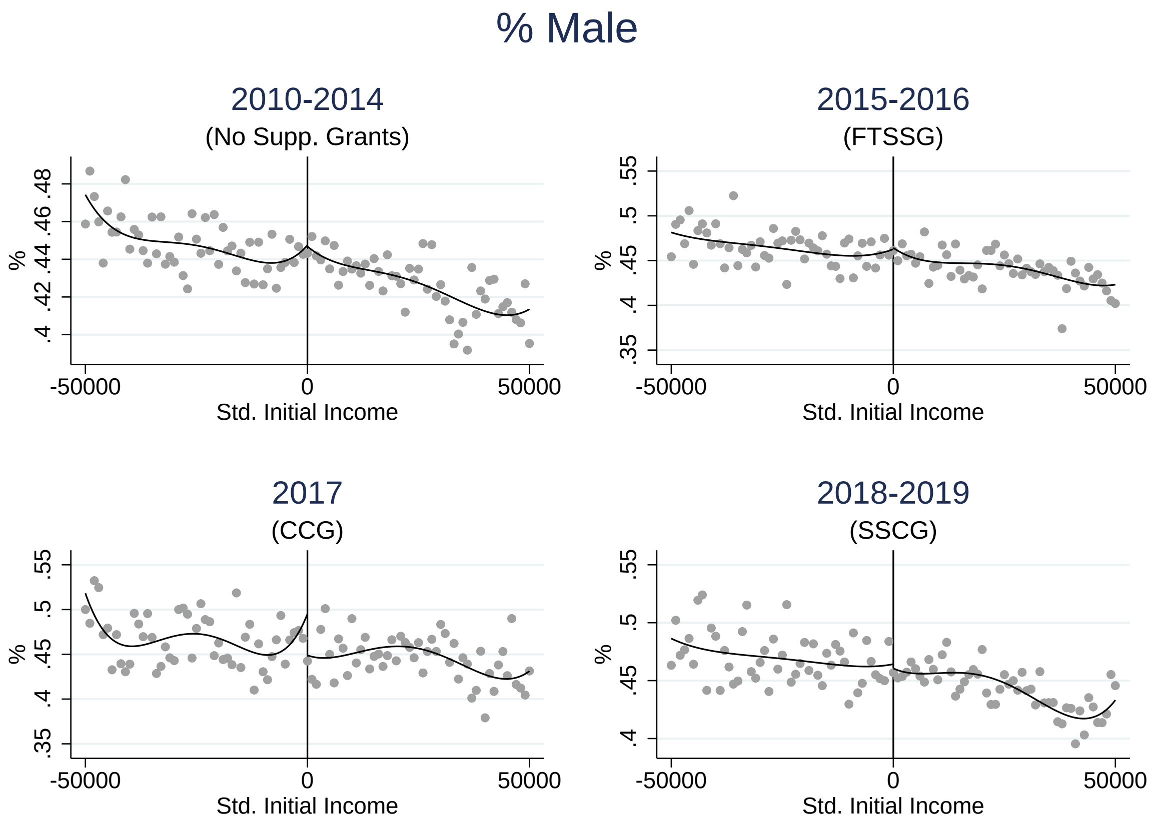


Figure C7. Covariate: Economically Disadvantaged

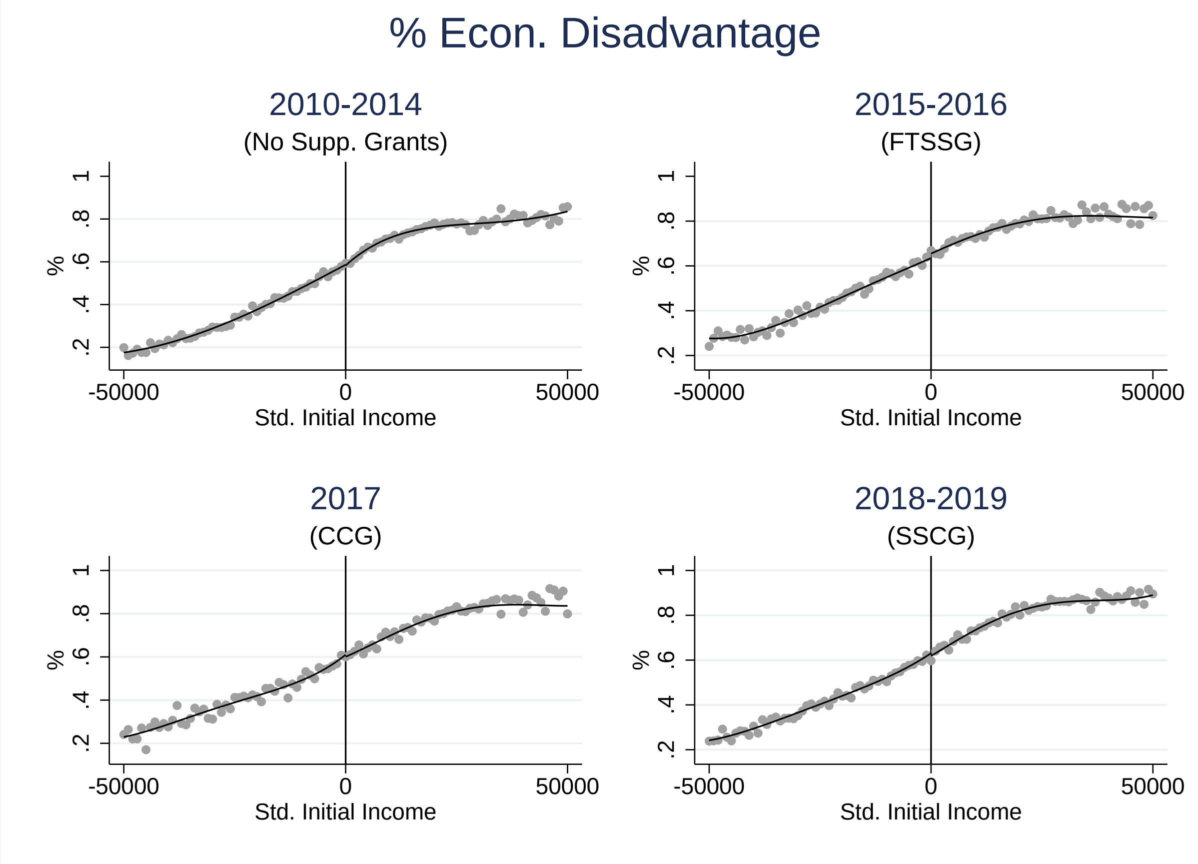


Figure C8. Enrollment Outcome: CCC

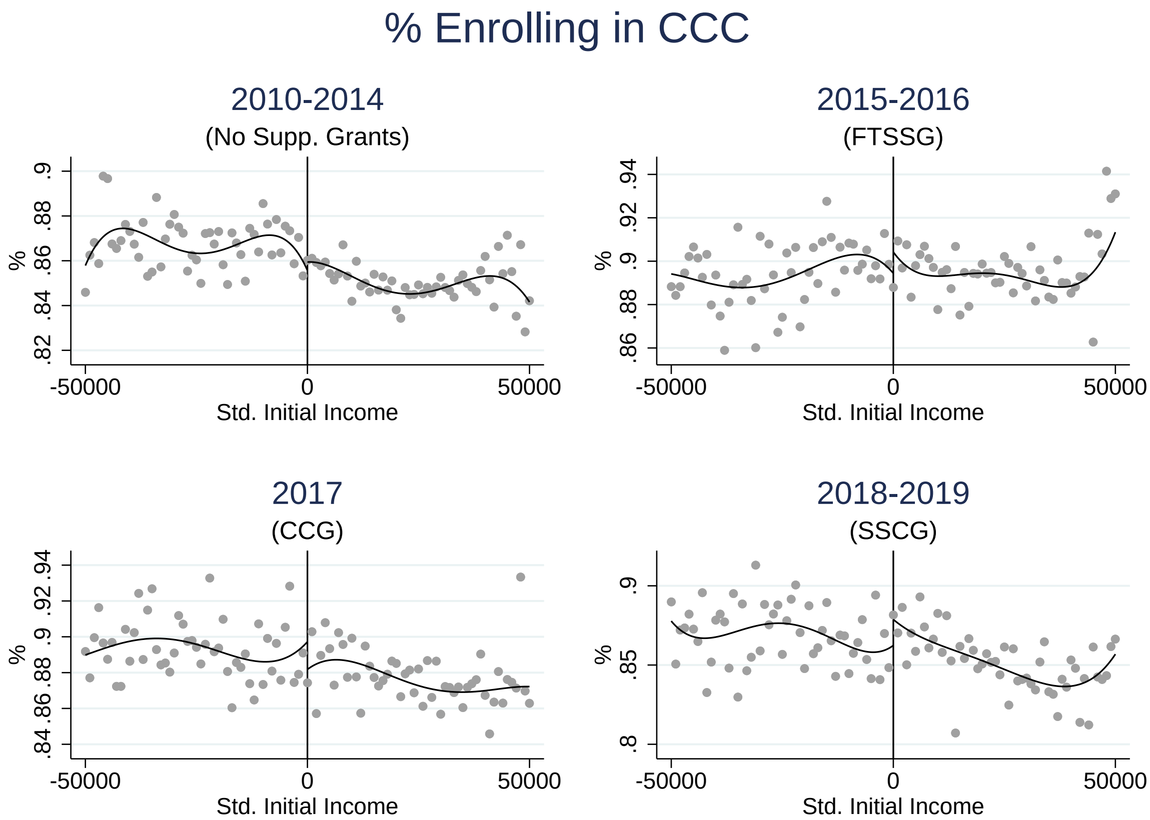


Figure C9. Enrollment Outcome: CSU

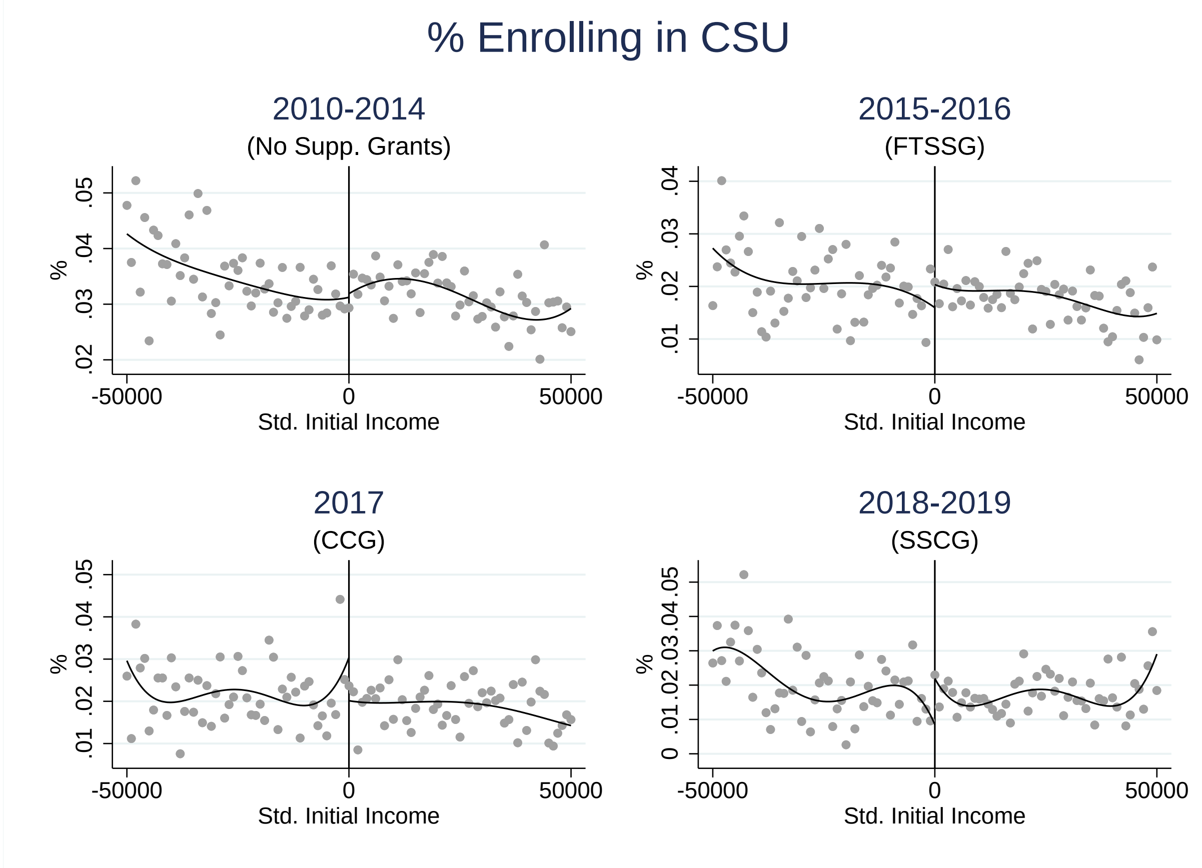


Figure C10. Enrollment Outcome: UC

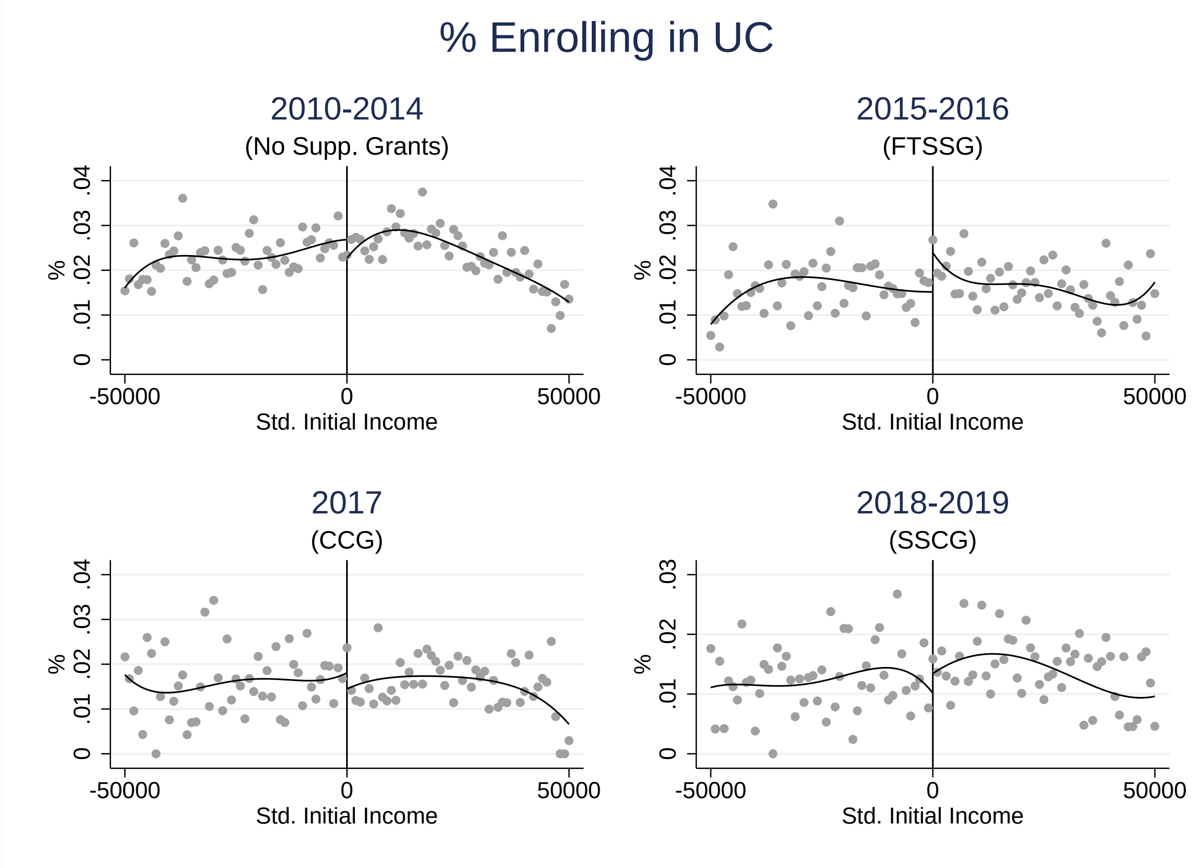


Figure C11. Enrollment Outcome: In-State Private

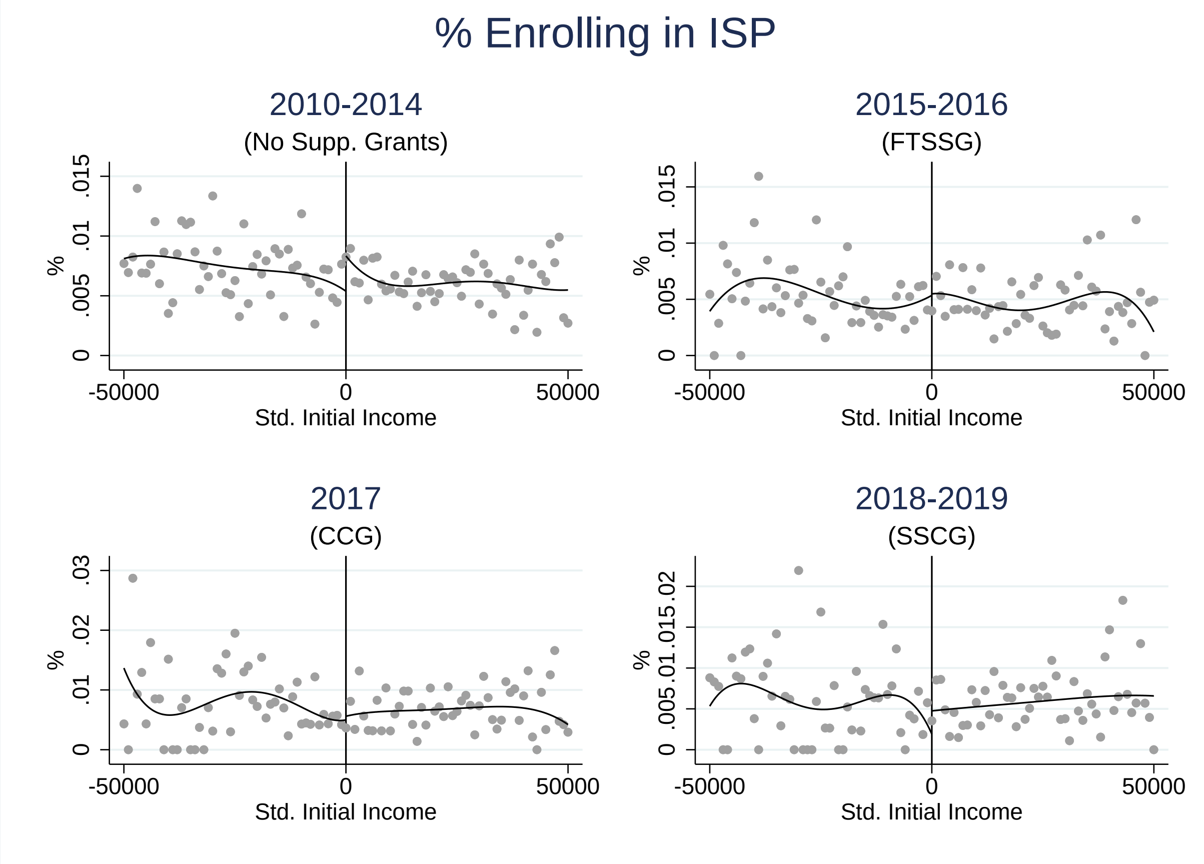


Figure C12. Enrollment Outcome: Out-of-State

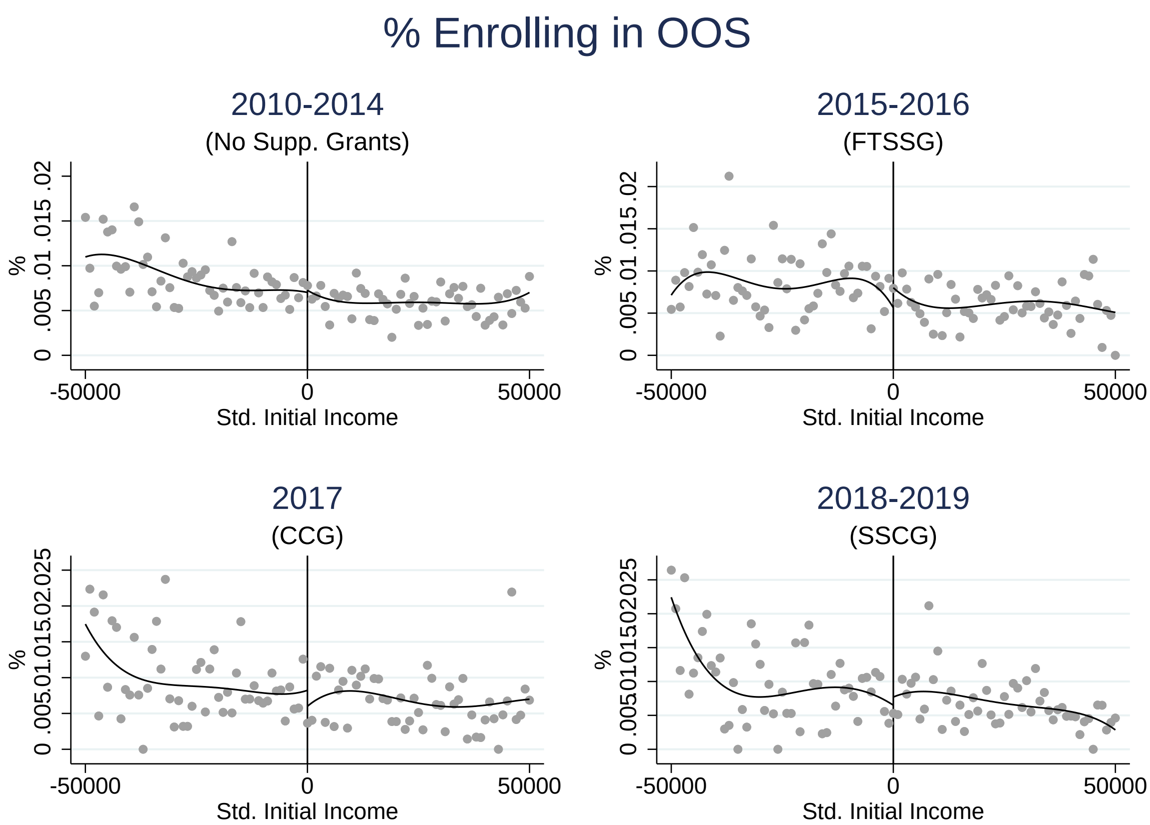
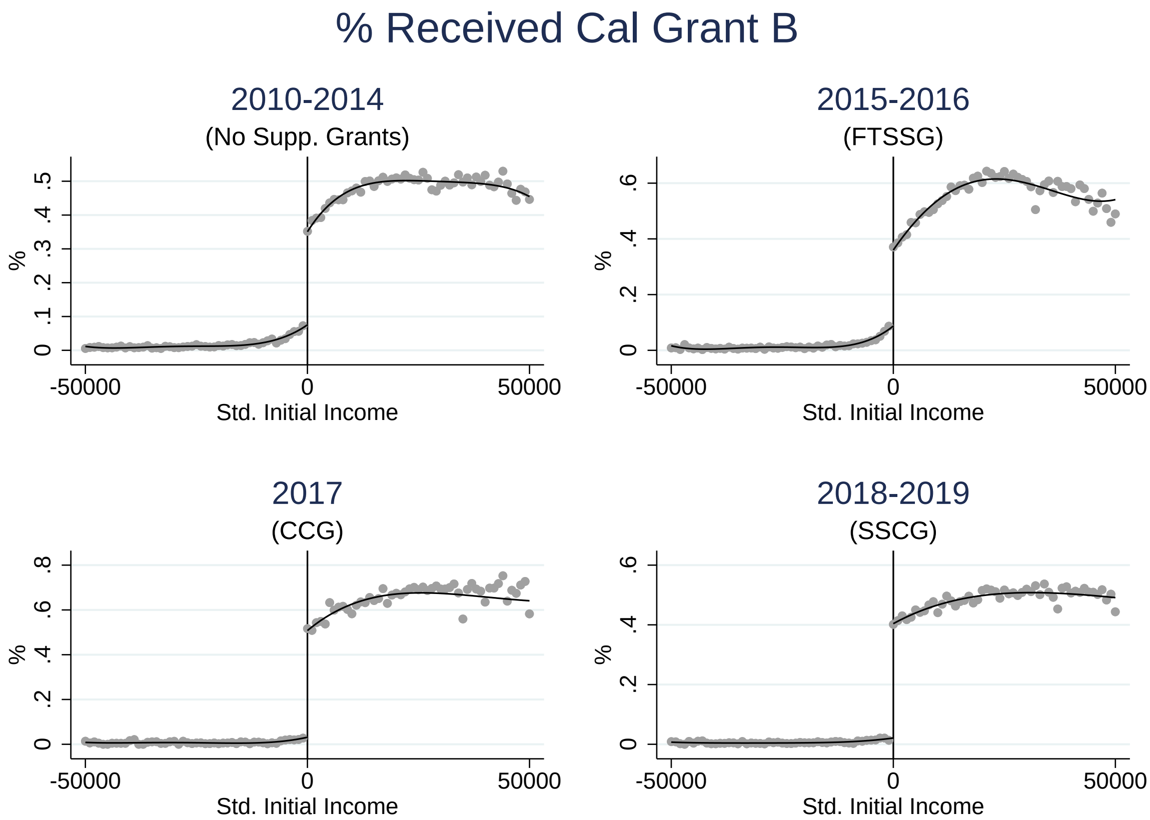


Table C1. Enrollment Results

Period	Overall 2010-2019 (1)	No Supp. Grant 2010-2014 (2)	FTSSG 2015-2016 (3)	CCG 2017 (4)	SSCG 2018 (5)
CCC Enrollment (NSC)	0.000300	-0.00320	0.00213	-0.0173	0.0203*
	(0.00388)	(0.00604)	(0.00728)	(0.0131)	(0.0107)
CCC Enrollment (match)	-0.000251	-0.000119	-0.000680	0.00959	-0.00843
	0.00332	0.00517	0.00615	0.00937	0.00775
Observations	483,580	232,363	115,945	64,651	70,621
Period	Overall 2010-2019	No Supp. Grant 2010-2014	FTSSG 2015-2016	CCG 2017	SSCG 2018-2019
CCC Enrollment (match)	0.00106	-0.000119	-0.000680	0.00959	-0.00868*
	(0.00255)	(0.00517)	(0.00615)	(0.00937)	(0.00467)
Observations	564,449	232,363	115,945	64,651	151,490

Standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: This sample includes first-time CSAC applicants who are applying for financial aid in the first year after graduating high school, who matched to the CDE data, and are eligible for a Cal Grant B in all aspects, *except* income eligibility. For this measure, we present results using only 2018, to be more directly comparable for our results using the NSC measure. We also includes results for 2018-2019 for completeness.

FIGURE C13. Receiving Cal Grant B



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