More Money, Less Problems?
Essays on Improving College Access & Success

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Abstract

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This dissertation includes three chapters focusing on policies directly related to improving college access and success. The first chapter focuses on lifetime eligibility of federal and state financial aid policies. The Pell Grant plays a critical role in helping students across the US to afford undergraduate education. In spite of its importance to the US system of higher education finance, relatively little is known about the optimal amount of time students should be eligible to receive Pell or other need-based grant aid programs. I exploit changes made during the Obama administration in 2012 that effectively lowered the maximum lifetime eligibility for Pell from 9 to 6 years of full-time equivalent study. I use a student fixed effects model that estimates the impact of reductions in lifetime Pell eligibility on student enrollment and degree completion outcomes for community college and four-year college students at a large, urban public university system. Findings suggest that lower lifetime eligibility reduced the likelihood of student re-enrollment and lowered students' academic performance. The policy change reduced the average amount of grant aid students received. Black and Hispanic students and community college entrants were most likely to be impacted by the policy change and attempted to offset declines in grant aid through increases in outside earnings. These results suggest that there are more effective policy levers to encourage enrollment through degree completion outside of the threat of financial aid loss.

The second chapter centers on the role of guidance counselors in education production. Counselors are a common school resource for students navigating complicated and consequential education choices. However, most students have limited access to school counselors. We study one of the largest US policies to increase access to school counselors - California's Supplemental School Counseling Program. The program increased the average number of high school counselors by .5 and reduced student to counselor ratios by about 40 students. Counselors hired as a result of the program had less experience on average. These changes led to modest increases in high school exit exam pass rates. Our findings hold important

policy implications for schools to address persistent equity gaps in college access and rising concerns over students' mental health.

The third chapter offers further evidence on the efficacy and effectiveness of federal and state financial aid programs. I exploit discontinuities in the Pell Grant formula known as Automatic-Zero Estimated Family Contribution to estimate the effects of Pell Grant aid on community college student enrollment and financial and academic outcomes. I find suggestive evidence that increases in grant aid boost associate degree completion, but these increases do not translate into significantly higher bachelor's degree completion rates or lower student debt burdens. These results fall in line with most of the empirical literature on Pell Grant aid effectiveness finding only modest effects of marginal increases in financial aid on degree completion. Findings further highlight the potential importance of combining efforts to improve financial aid programs with efforts to improve the transfer experience for degree-seeking, transfer-intending community college students.

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Dedicated to Ann Sparks, a teacher at heart.

Preface

The stakes for earning a college degree have arguably never been higher. In the US, education is a key engine for social mobility, and college is increasingly a prerequisite for maintaining economic stability. Despite its promise and potential to lift all boats, higher education faces a series of challenges that have left many Americans disenchanted with the sector. On the whole, access to college continues to be stratified by race and class, degree completion rates remain stubbornly low, and student debt continues to climb to unprecedented levels. These challenges contribute to a bleak narrative around higher education that highlights the need for reform.

This dissertation includes three chapters that use econometric methods and causal inference to evaluate policies with potential to reduce racial and class equity gaps in college access and outcomes. Two of the chapters focus on the role of federal and state financial aid programs in improving access and outcomes for students pursuing postsecondary degrees. I focus, in particular, on outcomes for students entering higher education through community colleges. These broad-access institutions enroll close to 40 percent of undergraduate students and disproportionately serve low-income students and students of color. Amidst ongoing calls for free college and reduced student debt burden, Pell and State Grant programs play a critical role in helping low- and middle-income students afford undergraduate education. Takeaways from these chapters highlight that even marginal increases in Pell and State Grant aid award amounts and lifetime eligibility can help to reduce financial precarity and improve academic outcomes.

Money is important but so, too, is information to support students and families in their educational decision-making. The chapter titled ”Expanding School Counseling” shifts focus from financial aid and credit constraints to the role of information in promoting college access. My co-author, Christine Mulhern, and I use an event study framework to explore the effects of increased access to middle and high school guidance counselors on measures of college readiness and access. We evaluate California’s Supplemental School Counseling Program, one of the nation’s largest investments in school counselors. The policy was designed and implemented with the intent of increasing students’ access to counselors and reducing racial equity gaps in college readiness and access. Our findings highlight the policy's positive impacts on academic outcomes and shed light on the critical role that counselors can play in easing transitions from high school to college or the workforce and improving school climate.

Education can more readily live up to its potential as an equalizing force in society when inequalities in income and access to information are limited. The hope is that these chapters can point toward more effective public policies to support students in their educational pursuits. Policy recommendations range from small, technical fixes to broader, normative assessments. Both are made with the belief that education can be transformative and that we are all better off when given the opportunity to develop and fulfill our potential.

1 To Give or To Take: Exploring Effects of Reductions in Pell Lifetime Eligibility

1.1 Introduction

The Pell Grant is arguably the single most important federal financial aid policy. As the largest source of federal financial aid in the US, about 1 in 3 undergraduate students overall receive Pell and more than 40 percent of community college students receive Pell (Park & Scott-Clayton, 2018). Tuition has increased dramatically across all college sectors since 1990; even community colleges, which are often perceived as low-cost entry points into higher education, have seen dramatic tuition increases over this time. As a means-tested program, the Pell Grant plays a critical role in helping low- and middle-income families afford college. Despite the important role Pell plays in helping students to manage direct and indirect costs associated with higher education, relatively little is known about how certain policy elements, such as lifetime eligibility, impact student outcomes. In 2012, the Obama administration reduced lifetime Pell Grant eligibility from 9 to 6 full-time equivalent (FTE) years. I use this policy change to explore the relationship between lifetime Pell Grant eligibility and academic and labor market outcomes for undergraduate students at a large public university system in the US that includes both community and four-year college students.[1] I focus on how financial aid loss as manifested through reductions in lifetime eligibility impacted student enrollment behavior, degree completion, and labor market outcomes, centering my analysis on the following research questions:

1. Which student groups are most likely to exhaust financial aid prior to degree completion?
2. Did reductions in Pell lifetime eligibility promote or discourage more timely degree completion or alter student decisions beyond enrollment?
3. How does other financial aid exhaustion impact enrollment and academic outcomes?

While college costs continue to outpace inflation, academic outcomes such as associate degree completion, transfer to four-year colleges, and bachelor's degree completion remain low and are especially poor for historically underrepresented student groups and students pursuing degrees in certain academic disciplines such as in STEM fields (Wang, 2015; Bahr et al., 2017). Within my sample, less than 30 percent of community college starters ever complete an associate degree and less than 60 percent of four-year college

[1] I use the term "four-year" and "senior college" students interchangeably. Both refer to students who initially enrolled at a bachelor's degree-granting university.

starters ever complete a bachelor's degree. Even among bachelor's degree completers, the time to degree often extends well beyond lifetime eligibility for financial aid programs – 21 percent of community college students and 8 percent of four-year college entrants who earned a bachelor's degree exhausted lifetime Pell Grant eligibility prior to graduation.

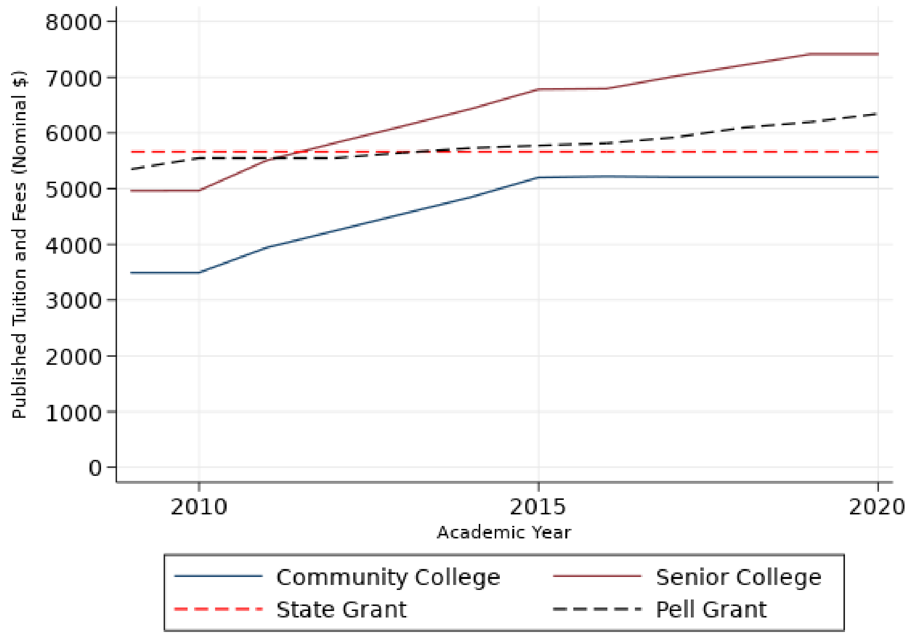
This paper's contribution to the literature is twofold. This is the first study to causally estimate the effects of lifetime Pell eligibility for both community college and four-year college students, which is particularly important given my results showing community college students are more likely than four-year college starters to exhaust financial aid prior to degree completion. Results offer insight into the effects of financial aid exhaustion more generally on student outcomes, including exhaustion of state need-based grant aid. Second, my analysis expounds on the relationship and interaction between employment, school, and financial aid. Findings suggest that community college entrants and students of color were disproportionately impacted by reductions in lifetime Pell eligibility. Causal estimates show that students were less likely to re-enroll as a result of the policy change. Students who re-enrolled after exhausting grant aid offset declines in grant aid with increased earnings in the labor market. They were also more likely to enroll part-time rather than full-time and experienced decreases in the number of credits attempted and earned and cumulative GPA.

1.2 Policy Background

Since its earliest iteration as the Basic Educational Opportunity Grant in the 1965 Higher Education Act, the Pell Grant has become an essential part of higher education finance policy. At the time, the policy represented a marked shift in the way the federal government sought to support higher education access, namely by providing funds directly to students rather than institutions. Since 1972, expenditures on Pell have increased from roughly $250 million to over $30 billion, and these spending increases have helped to grow Pell's reach from serving 176,000 students to more than 9 million students in recent years (Baum, 2015).

To receive a Pell Grant, students must first complete the Free Application for Federal Student Aid (FAFSA), which more than 1 in 2 high school seniors currently do (DeBaun, 2022). There is no precise income cutoff for Pell; rather, eligibility and award size are based on a given student's estimated family contribution, which is itself a function of family size, income, and cost of attendance, among other factors. The maximum Pell Grant award increased to $6,495 for the 2021 academic year, but year over year changes to the maximum Pell grant have failed to keep pace with increases in the cost of college attendance (Delisle,

Figure 1: Tuition & Fees and Maximum Grant Awards Over Time



2021).[2]

As a result of increased college enrollments and the number of students eligible for Pell during and after the Great Recession, the program faced a multibillion-dollar deficit in 2011 that officials sought to remedy through programmatic changes. These changes included eliminating eligibility for students without a high school diploma, eliminating eligibility for students with award sizes below $555 for a given year, and lowering the lifetime eligibility from 9 to 6 full-time equivalent (FTE) years (Mabel, 2020). In Spring 2012, the Department of Education's Office of Federal Student Aid sent notifications to students who had used at least 4.5 years of Pell eligibility, letting them know of the policy change and providing instructions on how to view their Pell use to date; the Federal Student Aid Office also enacted accountability measures to ensure that students did not receive Pell Grants beyond the new lifetime eligibility (US DOE, 2015). This analysis focuses on the reduction in Pell lifetime eligibility from 9 to 6 FTE years, which was enacted in academic year 2012-2013 and offers the potential to better understand how lifetime award eligibility and financial aid loss impact college student enrollment behavior and academic and labor market outcomes.

[2]Figure 1 shows maximum Pell and State Grant award size in nominal dollars from 2009 to 2020 as well as published tuition and fees for the university system of interest for this analysis.

1.3 Literature Review

The cost of higher education holds direct implications for students' enrollment decisions, major selection, and ability to earn credentials (Becker, 2009; Rothstein & Rouse, 2011; Denning, 2017). The literature evaluating the impacts of Pell Grants on improving student outcomes in college is decidedly mixed, with most papers finding limited to modest increases in enrollment and degree completion rates (Matsudaira, 2017; Park & Scott-Clayton, 2018; Denning et al., 2019; Eng & Matsudaira, 2021). Much of the literature evaluating financial aid policies focuses on impacts on students' college enrollment decisions and academic achievement during early college years (Leuven et al., 2010; Barrow et al., 2014; Dawson et al., 2020). While financial and other interventions to improve college access and early college success are certainly warranted, less research focuses on how financial aid can be used to support students who are close to the finish line. This is especially critical given the substantial proportion of college dropouts who have completed more than three-quarters of required credits for graduation as well as the earnings premium associated with completed credentials (Mabel & Britton, 2018). Limited effects of nudge campaigns and other efforts to improve outcomes for students close to completing their degrees further motivate this analysis (Bettinger et al., 2021).

There is ambiguity in how financial aid loss stemming from reductions in Pell lifetime eligibility from 9 to 6 FTE years might impact student enrollment and degree completion outcomes. Prior studies address the threat of aid loss and its effects on student outcomes by looking at performance-based eligibility requirements, such as GPA and credit requirements (Scott-Clayton, 2010; Schudde & Scott-Clayton, 2016; Scott-Clayton & Zafar, 2019). While academic requirements embedded in financial aid eligibility can improve student performance, responses to reduced lifetime eligibility and aid exhaustion may be different. Reduced lifetime eligibility may encourage students to complete their degrees more quickly if students are at once aware of the policy change and sufficiently concerned that financial aid loss will hinder their ability to afford college. On the other hand, exhaustion of Pell lifetime eligibility might discourage students from further enrollment and slow time to degree completion if they view degree completion within a shortened time frame as infeasible, especially in the absence of financial support through Pell and other grant aid receipt.

Financial aid loss may be particularly consequential for students attending the university system in my analysis. About 1 in 2 students receive Pell in their first year of enrollment, and students on average receive close to $7,000 in Pell Grant dollars over the course of their time enrolled in college. Among students who received Pell in their first year of enrollment, the average amount of Pell Grant aid received was more than $12,000. The maximum Pell Grant award for academic year 2012-2013 was $5,550 while average undergraduate tuition at the university system of interest was about $12,000 (see Figure 1). Accordingly, Pell helps to cover a substantial portion but not all of undergraduate tuition. In addition, most college entrants in this university system first enroll at a community college. Community college students may be more likely to exhaust lifetime Pell and other financial aid eligibility compared to four-year college starters given longer average time to degree and other issues associated with upward transfer such as credit loss and lack of program structure and advising supports (Bailey et al., 2015).

Changes in lifetime Pell eligibility immediately effected financial aid receipt for students who had already used 6 or more years' worth of eligibility by Spring 2012. Drawing on inferences from the human capital model, students who are credit constrained or anticipate being credit constrained as a result of losing or having less Pell eligibility should look to other financial aid sources or increased employment levels. Even if reductions in Pell lifetime eligibility do not induce students to drop out, we might still expect students to increase borrowing or increase earnings through employment to cover reductions in grant aid. They might also be more likely to enroll part-time as a means of lowering costs, which is likely to lengthen time to degree. The substitution of earnings from employment for lost financial aid is of particular interest in this context since more than three-quarters of students in the sample population are employed while enrolled in college.

1.4 Data

I use administrative data from a large, urban public university system that includes student demographics, financial aid, enrollment, and course-taking from academic years 2005 through Spring 2022. Student transcripts records are available for community college, four-year college, and graduate students. I limit the sample to undergraduate students since Pell and relevant state grant aid cannot be used for graduate studies. The decision to reduce Pell lifetime eligibility from 9 to 6 FTE years was finalized in Spring 2012 and implemented in Fall 2012.

I use students' term-over-term enrollment intensity as measured by full- and part-time status and credits attempted to construct a measure of cumulative Pell usage. For instance, students who enrolled full-time and attempted at least 12 credits in both Fall and Spring semesters used 1 FTE year of Pell. Students who enrolled part-time in Fall and/or Spring semesters used an amount of Pell equal to the number of credits attempted in an academic year divided by 24 (the minimum number of credits for full-time status). If a student enrolled part-time and attempted 6 credits in the Fall and Spring, for example, then that student used .5 FTE Pell years. Students were eligible to receive Summer Pell Grants during academic years 2009-10 and 2010-11. Summer Pell was then discontinued before being reinstated by Congress in July 2017 (Liu, 2020). While receiving Summer Pell did not preclude students from receiving Pell in the Fall or Spring, it did count toward students' lifetime Pell usage.

In addition to student transcript data, I also have access to Unemployment Insurance (UI) data from an anonymized State Department of Labor. These records include quarterly employment and earnings, employer ID, employment industry codes based on the North American Industry Classification system (NAICS), and flags for whether an employer is a government entity. I observe student's term-over-term earnings while enrolled or if not enrolled in college and can see how employment, in terms of number of jobs, job type, and total quarterly earnings vary over time.

1.5 Research Design

In the ideal experiment, students who had used 5 years worth of Pell by Spring 2012 would have been randomly assigned to a treatment group eligible for 6 FTE years of lifetime Pell eligibility and a control group with 9 FTE years of lifetime Pell eligibility. Differences between treatment and control students in enrollment, financial, and academic outcomes could then be attributed to reduced lifetime Pell eligibility. However, in the absence of random assignment, I use student fixed effects to estimate causal effects of the policy change. My preferred analytic sample includes students in entering undergraduate cohorts from 2005 through 2008. Only students in these three cohorts met the criteria of receiving at least 5 FTE years of Pell by Spring 2012, the semester prior to implementation of the policy change. Student financial aid records were only available from 2005 on, which prohibits me from looking at additional student entry cohorts. To see whether changes to lifetime Pell eligibility impacted enrollment intensity and other financial and academic outcomes, I use the following student fixed effects model,

$$Y_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 P_{it} + \beta_3 T_{it} P_{it} + \phi_i + \chi + \epsilon_{it} \tag{1}$$

where Y is a series of outcome variables for student i in term t including full-time enrollment, part-time enrollment, any enrollment, credits attempted and earned, cumulative GPA, Pell Grant aid and total grant aid, loans, and quarterly earnings. T is an indicator for treatment, which in this instance indicates whether students have 5 or more full-time equivalent (FTE) years of cumulative Pell Grants in a given semester. I run alternative model specifications using a range of 4 to 6 years of FTE Pell to define treatment, though results and takeaways do not differ significantly (see appendix Table A.1). In this context, 5 years of FTE Pell is my preferred treatment threshold for detecting effects of lifetime Pell reduction on student enrollment behavior since students were informed of the policy change and had time to change their enrollment behavior accordingly. Alternative model specifications using 6 FTE Pell years provide more insight into effects of immediate financial aid exhaustion on enrollment behavior whereas a 4 year threshold may leave too much time for students to modify enrollment behavior such that effects are attributable to factors beyond the policy change reducing lifetime Pell eligibility.

P is an indicator for the post period, or any semesters on or after Fall 2012. ϕ represents student fixed effects, χ represents year fixed effects, and β_3 is the interaction between indicators for the treatment and post period. This interaction term represents the causal estimate of reductions in lifetime Pell eligibility on student outcomes. The main assumptions of the individual fixed effects model are that unobservable differences across individuals and that correlate with outcomes of interest are fixed across time. Similar to a difference-in-differences identification strategy, this model also assumes that student enrollment behavior would have evolved similarly had lifetime Pell eligibility not been reduced from 9 to 6 years. I use a placebo group of students who never received Pell to show that these assumptions are likely to hold.

This identification strategy falls short of the ideal randomized experiment previously mentioned. While the longitudinal data used for this analysis is extremely detailed, student fixed effects cannot control for all unobservable characteristics. Moreover, models that estimate effects of reduced lifetime Pell eligibility for placebo students who never received Pell and alternative treatment threshold students who used 4 and 6 FTE years of Pell offer useful robustness checks on causal estimates but, again, cannot completely control for selection bias. These limitations should be taken into account when interpreting causal findings.

Results from individual fixed effects models estimating the impact of reduced lifetime Pell Grant eligibility on student enrollment behavior still provide useful insight into whether changes in lifetime Pell eligibility promoted or hindered student enrollment and other academic outcomes. These results raise additional questions around how student enrollment behavior changes after financial aid exhaustion more generally. I observe student enrollment, term academic outcomes, and financial aid outcomes (e.g. Pell, total grant aid, and loan amounts) before and after a student exhausts Pell or state need-based grant aid. After 2012, students were eligible for a maximum of 6 FTE years of Pell while students across all cohorts were eligible to receive up to 4 years of state need-based grant aid.[3] I use a similar student fixed effects

[3]The state included in this analysis has the highest proportion of state funding for higher education going toward grant aid (Li & Zumeta, 2019). The state's grant aid program, which was established in 1974, offers students up to $5,665 in grant aid annually. The amount that students receive is determined by a number of factors including family income, dependent status, and amount of Pell or other grant aid received. Part-time students were not eligible for this particular state aid program until 2022. Students pursuing an

model to estimate the impact of exhausting each type of financial aid:

$$Y_{it} = \beta_0 + \beta_1 P_{it} + \phi_i + \chi + \epsilon_{it} \tag{2}$$

I focus on the same set of outcomes as before, but this time the model does not include a treatment variable indicating the amount of financial aid received or an interaction term. P is an indicator for whether student i exhausted Pell or state need-based grant aid in semester t. These results are useful for several reasons. First, they show how enrollment behavior is impacted by aid exhaustion and how students account for reductions in grant aid through other aid sources or participation in the labor market. Second, they offer insight into how federal and state aid interact, which is important in this context since students have two fewer years of eligibility for state grant aid as compared to Pell Grant eligibility and must enroll full-time to qualify for state grant aid.

1.6 Results

1.6.1 Descriptive Findings

I present descriptive statistics for entry cohorts 2005-2008 who started at community versus four-year colleges in Table 1. I further disaggregate descriptives for all students, students who enrolled for five or more years, and students who used five or more years of lifetime Pell eligibility. This enables me to discern similarities and differences in subsamples of students for whom the reduction in lifetime Pell eligibility was most likely to impact.

Within the university system of interest, the student body is racially diverse, and 62 percent of the sample started at a community college. About half of community college entrants and 40 percent of four-year college entrants received Pell Grants within their first year of enrollment, and Pell Grants account for 90 percent of students' total grant aid. Students on average use less than 2 years of Pell eligibility, but this is in part a result of enrollment attrition and low degree completion rates, particularly for community college students – 20 percent of community college entrants and 57 percent of four-year college starters ever earned a bachelor's degree. Students who enrolled for five or more years received about 3 years of Pell Grants, and about one-third of these students received more than five years of Pell. More than three-quarters of students in the sample worked while enrolled, earning roughly $4,000 on average per quarter. Students on average borrowed about $3,000 over the course of their enrollment while students who enrolled for at least

associate degree can receive up to 6 semesters of state grant aid and become eligible for an additional 2 semesters upon transferring to a four-year college.

Table 1: Sample Characteristics, 2005-2008 Entry Cohorts

	Community College Entrants			***Senior College Entrants***		
	All Students	Enrolled for 5+ Years	Received 5+ Years of Pell	All Students	Enrolled for 5+ Years	Received 5+ Years of Pell
Race & Gender						
Female	.56	.61	.65	.59	.61	.64
Asian	.14	.19	.20	.20	.24	.32
Black	.33	.29	.30	.21	.19	.20
Hispanic	.33	.31	.37	.23	.24	.32
White	.20	.21	.13	.36	.32	.15
Enrollment						
Total FTE Years Enrolled	3.58	7.41	7.80	4.87	7.17	7.50
Total Credits Attempted at Year 5	88	88	94	95	95	101
Total Credits Earned at Year 5	80	81	87	89	89	94
Cumulative GPA at Year 5	2.78	2.79	2.78	3.02	3.02	2.94
Enrolled for 5+ Years	.31	1	1	.51	1	1
Financial Aid & Earnings						
Received Pell in Year 1	.49	.49	.82	.35	.40	.83
Total Pell FTE Years	1.64	3.37	6.12	1.71	2.62	5.90
Total Pell Grant Aid	$6,675	$12,985	$23,983	$6,237	$9,543	$22,442
Total Grant Aid	$7,164	$14,141	$25,889	$6,664	$10,221	$23,796
Total Loans	$2,403	$5,865	$6,875	$3,903	$5,423	$4,873
Employed While Enrolled	.75	.87	.91	.79	.86	.89
Quarterly Earnings While Enrolled	$3,732	$4,082	$3,539	$4,753	$4,631	$3,976
Received 5+ Years of Pell	.11	.36	1	.14	.27	1
Degree Outcomes						
Earned Associate Degree	.29	.64	.70			
Transferred to Senior College	.51	.90	.95			
Earned Bachelor's Degree	.20	.62	.75	.57	.85	.89
N	134,642	42,173	15,288	81,663	42,876	11,840

Notes: Total Pell Grant Aid, Total Grant Aid, and Total Loans are totals across all terms that an undergraduate student enrolled in. Quarterly earnings are limited to earnings during semesters in which a student enrolled at least part-time.

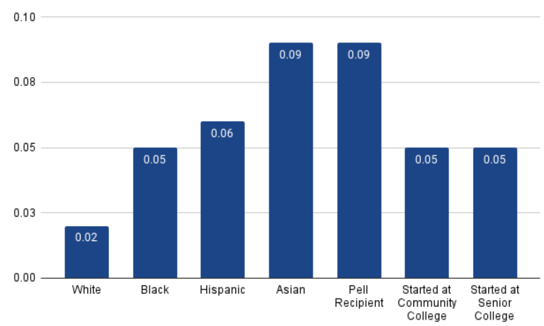
5 years accumulated around $5,000 in loans, amounts that are low compared to average undergraduate debt nationally and reflect relatively low tuition. These summary statistics highlight that students in the sample rely heavily on financial aid and labor market participation to afford college.

Students who enrolled for at least 5 years or received 5 or more years of Pell earned more than 95 credits by their fifth year of enrollment, or about 80 percent of required credits for a baccalaureate degree. It is also worth noting that community college and four-year college entrants who received five or more years of Pell had a cumulative GPA of 2.78 and 2.94, respectively. This suggests that students most likely to be impacted by reductions in lifetime Pell eligibility were close to the finish line at the time of the policy change and, while not in the top quartile of academic performers in terms of GPA, were still above average with respect to academic performance.

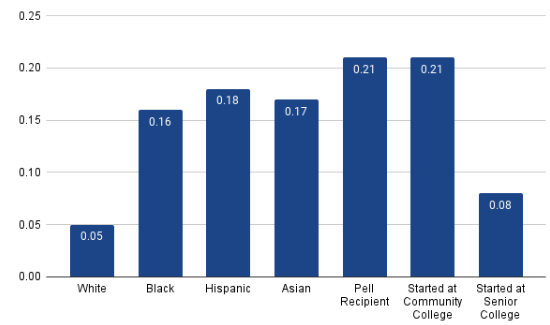
Figures 2a and 2b show the Pell exhaustion rate, or the percentage of students who use 6 years of Pell eligibility, both overall and for students who went on to earn a bachelor's degree. Overall, only 5 percent of community and four-year college entrants exhausted Pell eligibility, with students of color about 3 to 4 times more likely to exhaust Pell compared to White students. These numbers are slightly underwhelming but, again, reflect low enrollment persistence and degree completion among all students who ever enrolled. When I backwards map to look at Pell exhaustion rates prior to degree completion for students who go on to earn a bachelor's degree in Figure 2b, the significance of the policy change heightens: 21 percent of community college students and 8 percent of four-year college students exhausted Pell, and students of color were about 3 times as likely to exhaust Pell compared to White students. Reductions in lifetime Pell eligibility were most likely to impact community college entrants and students of color.

Disparities in the likelihood of exhausting Pell before graduating might not be an issue if these students eventually complete their degrees. Bachelor's degree completion rates among students who exhaust Pell, however, are low and uneven across demographic subgroups. As shown in Figure 2c, only 76 percent of students who started at a community college and exhausted Pell go on to earn a bachelor's degree, and Black and Hispanic students are less likely to earn a degree after exhausting Pell as compared to White and Asian students. This again highlights that students most likely to be impacted by the policy change reducing lifetime Pell eligibility from 9 to 6 FTE years predominantly started at a community college and were also more likely to be non-White. Estimated effects of lifetime Pell eligibility reductions on students' academic outcomes from the student fixed effects analysis hold important implications for racial equity, as the threat of financial aid could reduce or exacerbate extant racial equity gaps in student enrollment behavior and degree completion.

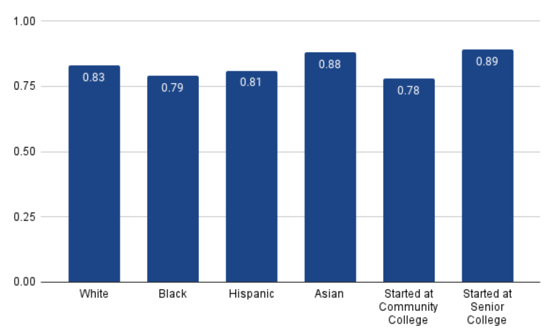
Figure 2: Pell Exhaustion and Bachelor's Degree Completion Rates



(a) Pell Exhaustion Rate Prior to Degree Completion



(b) Pell Exhaustion Rate Prior to BA Degree Completion Among Students Who Earned BA



(c) BA Completion Rate Among Students Who Exhausted Pell Lifetime Eligibility

1.6.2 Main Results

I present estimated effects of reductions in lifetime Pell eligibility from 9 to 6 FTE years on students' financial aid and earnings outcomes using the fixed effects model outlined in the methodology section. All regression results in Table 2 account for student and year fixed effects, and each student is observed for the equivalent of 10 academic years. All models are limited to students who used 5 or more years of Pell eligibility and exclude observations once a student has graduated. I run separate models that exclude Summer and Winter terms and that are limited to terms students enrolled in at least part-time. Including only Fall and Spring terms is useful in this instance because the vast majority of students within this university system do not enroll during Summer and Winter terms and also because few students receive Pell during Summer terms. It is also helpful to run the model after limiting to terms that students enrolled in to better understand the extent to which changes in financial aid receipt and quarterly earnings are the result of reduced Pell eligibility versus dropping or stopping out.

For each model specification, I show coefficient estimates for treatment and post-policy change indicators. The interaction term between treatment and post indicators represents the causal estimate. Across all models, reductions in lifetime Pell Grant eligibility that took effect in Fall 2012 are associated with declines in Pell awards between $460 and $780. These declines account for 85 to 95 percent of the decline in total grant aid received. I observe small declines in loan amounts; however, when I exclude summer and winter terms and limit to terms that students enrolled in, students borrowed $118 more to account for declines in grant aid. Results also suggest that students cope with losses in financial aid receipt through increased labor market participation. Students increased quarterly earnings by roughly $700. This estimate attenuates to $314 after limiting to terms students enrolled in, but remains statistically significant at $p < .001$. These results show that reduced lifetime Pell eligibility decreased Pell and total grant aid amounts and that students responded to these decreases primarily by increasing earnings in the labor market.

Using the same model specifications, I show results on students' enrollment and academic outcomes. Findings reported in Table 3 show overall declines in enrollment between 15 and 20 percentage points, and these declines are predominantly driven by changes in full-time enrollment. Reductions in lifetime Pell eligibility are also associated with modest declines in credits attempted (.59-2.54) and earned (.85-2.33) and in cumulative GPA (.10-.15). Declines in academic performance hold after limiting to students who enrolled in a given semester, and all interaction coefficient terms are statistically significant at the .001 level. To check for heterogeneity, I run models separately for community college entrants, Black, Hispanic, and American Indian students, and students who were 25 or older at initial entry. Treatment effect estimates

Table 2: Estimated Effects of Reductions in Lifetime Pell on Financial Aid and Earnings

	(1)	(2)	(3)
Pell			
Treat	165***	100***	-34***
Post	42***	223***	191***
Interaction	-465***	-781***	-547***
	(8)	(10)	(10)
R^2	.113	.378	.384
Total Grant Aid			
Treat	167***	100***	-49***
Post	56***	256***	232***
Interaction	-500***	-852***	-624***
	(9)	(12)	(12)
R^2	.102	.328	.343
Loans			
Treat	74***	101***	43***
Post	6	68***	54***
Interaction	-14***	-25*	118***
R^2	.200	.317	.462
Quarterly Earnings			
Treat	-12	-18	108***
Post	-38*	14	181***
Interaction	770***	698***	314***
	(19)	(25)	(27)
R^2	.568	.571	.603
Excludes Summer & Winter Terms	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes
N	27,212	27,212	27,207

Notes: All regressions include student and year fixed effects and are limited to students who ever received at least 5 FTE years of Pell. Student observations are limited to 40 terms. N refers to unique student observations. $***p < 0.001, **p < 0.01, *p < 0.05$.

Table 3: Estimated Effects of Reductions in Lifetime Pell on Enrollment and Academic Outcomes

	(1)	(2)	(3)
Full-Time Enrollment			
Treat	.040***	.005	-.027***
Post	.021***	.061***	.034***
Interaction	-.110***	-.192***	-.095***
	(.003)	(.004)	(.004)
R^2	.114	.413	.376
Part-Time Enrollment			
Treat	.056***	.073***	.027***
Post	-.085***	-.002	-.034***
Interaction	-.004***	-.023***	.095***
	(.003)	(.004)	(.004)
R^2	.103	.243	.376
Any Enrollment			
Treat	.096***	.078***	
Post	-.064***	.059***	
Interaction	-.155***	-.216***	
	(.003)	(.003)	
R^2	.094	.329	
Credits Attempted			
Treat	.647***	.009	-.766***
Post	-.094*	.852***	.430***
Interaction	-1.54***	-2.40***	-.591***
	(.041)	(.050)	(.046)
R^2	.115	.400	.345
Credits Earned			
Treat	.597***	-.093	-.808***
Post	-.031	.916***	.644***
Interaction	-1.51***	-2.33***	-.848***
	(.040)	(.049)	(.049)
R^2	.127	.406	.347
Cumulative GPA			
Treat	-.001	.009***	-.011***
Post	.090***	.093***	.089***
Interaction	-.151***	-.149***	-.109***
	(.003)	(.004)	(.005)
R^2	.609	.611	.602
Excludes Summer & Winter Terms	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes
N	27,212	27,212	27,207

Notes: All regressions include student and year fixed effects and are limited to students who ever received at least 5 FTE years of Pell. Student observations are limited to 40 terms. N refers to unique student observations. $***p < 0.001, **p < 0.01, *p < 0.05$.

are similar for these subgroups as compared to estimates presented in Tables 2 and 3 with a few exceptions: estimated effects on quarterly earnings for all three subgroups were slightly higher whereas impacts of reduced lifetime eligibility on grant aid and loans were slightly more muted for students 25 or older at entry (see appendix Table A.2). These results offer evidence that reduced Pell eligibility and associated losses in financial aid discouraged subsequent enrollment and ultimately decreased the number of credits attempted and earned by students as well as their academic performance as captured through cumulative GPA.

1.7 Robustness Checks and Extensions

Detected effects on enrollment and academic performance may be upwardly biased after taking into account the fact that college students have at least some propensity to stop or drop out in any semester regardless of changes in lifetime Pell eligibility. To try to account for this, I run a similar set of models on a placebo group of students who never received any Pell Grants but still enrolled for at least 5 years. The post coefficient in this case represents the likelihood of re-enrollment after the policy was implemented in Fall 2012. Results included in Table 4 highlight that students who were not impacted by the policy change and had yet to earn a bachelor's degree were in fact less likely to enroll after the policy change. Students in the placebo group were about 14 percentage points less likely to enroll in semesters after the policy change, and this estimate attenuates to 6 percentage points after excluding Summer and Winter terms. With regard to academic outcomes, students attempted fewer credits, but effect sizes are very small and lose some precision after excluding Summer and Winter terms. I find no effects on cumulative GPA.

The magnitude of these results is substantially lower than results for students impacted by reductions in lifetime Pell eligibility. For instance, after excluding Summer and Winter terms students who received 5 or more years of Pell were 21 percentage points less likely to enroll compared to 6 percentage points for students who never received any Pell Grants. Among students who re-enrolled after the policy change, placebo students were 3 percentage points more likely to enroll part-time compared to 10 percentage points for treated students. While students on the whole were less likely to enroll in a given semester after the policy change, this discrepancy provides evidence that reduced lifetime Pell eligibility is driving more drastic declines in the likelihood of enrollment and in academic outcomes.

In the methodology section, I noted that preferred model specifications and main regression results presented in Tables 2 and 3 flag a student as treated if they used 5 or more FTE years of Pell by Spring 2012. The US Department of Education notified students who had accrued more than 4.5 years of Pell to

Table 4: Enrollment and Academic Outcomes for Students who Never Received Pell, Post Policy Change

	(1)	(2)	(3)
Full-Time Enrollment			
Post	-.032***	-.035***	-.029***
	(.003)	(.004)	(.005)
R^2	.196	.568	.534
Part-Time Enrollment			
Post	-.103***	-.028***	.029***
	(.003)	(.005)	(.005)
R^2	.125	.321	.534
Any Enrollment			
Post	-.136***	-.063***	
	(.004)	(.004)	
R^2	.153	.475	
Credits Attempted			
Post	-.934***	-.600***	-.212***
	(.038)	(.050)	(.053)
R^2	.195	.587	.512
Credits Earned			
Post	-.862***	-.510***	-.157**
	(.037)	(.049)	(.055)
R^2	.204	.587	.503
Cumulative GPA			
Post	-.002	-.002	.002
	(.002)	(.003)	(.004)
R^2	.604	.740	.725
Excludes Summer & Winter Terms	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes
N	28,007	28,007	27,976

Notes: All regressions include student and year fixed effects and are limited to students who never received Pell but enrolled for at least 5 FTE years. Student observations are limited to 40 terms. N refers to unique student observations. N refers to unique student observations. $***p < 0.001, **p < 0.01, *p < 0.05$.

inform them of the policy change lowering lifetime Pell eligibility, which in turn motivated my decision to use 5 FTE years as a treatment threshold. Still, students may not have been aware of the policy change given limited access to financial aid advising and extant knowledge of lifetime eligibility across financial aid programs. I present additional results from student fixed effects models on the same set of financial, enrollment, and academic outcomes, but adjust the treatment specification to include students who used 4 or more years of Pell and 6 or more years of Pell at the time of the policy change (see Appendix Table A.1).

Results from these models with altered treatment groups do not appreciably differ from the original models that used 5 or more years of Pell receipt as a treatment group. Across all models, students receive less Pell and total grant aid, increase borrowing, and increase outside earnings to offset reductions in financial aid. They are significantly less likely to enroll part- or full-time and experience declines in credits attempted and earned as well as in cumulative GPA. Impacts on enrollment are even more pronounced when using 4 or more years of Pell as the treatment group, suggesting that financial aid loss plays a critical role in discouraging subsequent enrollment.

Findings thus far suggest that financial aid loss as manifested through reductions in lifetime Pell eligibility discouraged subsequent enrollment, lowered academic performance, and increased borrowing and quarterly earnings to offset declines in financial aid. These results beg the question of how financial aid exhaustion more generally impacts students' financial, enrollment, and academic outcomes. I observe student entry cohorts from 2014-2016 to see how exhausting Pell and state grant aid impacts relevant outcomes. This state's need-based grant aid program operates as a last-dollar grant, where award amounts for income-eligible students are determined in part by tuition, family income, and unmet need after Pell Grants. For academic years 2014-2021, only full-time students could receive state need-based grant aid, and lifetime eligibility was 4 FTE years. Students in pursuit of an associate degree are eligible for 3 years of state need-based grant aid and receive an additional year of eligibility after transferring to a four-year college.

I estimate the effects of exhausting state and Pell Grant lifetime eligibility using student fixed effects models and, similar to previously reported results, run separate models that exclude Summer and Winter terms and limit to terms that students enrolled in. For state grant aid exhaustion, the post indicator switches to 1 once a student has used all 4 years of eligibility. For Pell, the post indicator switches to 1 once a student has used 6 years of eligibility, the lifetime limit that was established by the policy change in Fall 2012.

Results estimating the effects of state grant aid exhaustion on financial aid and earnings outcomes are presented in Table 5. The preferred models in this case are those that exclude Summer terms since very few students receive state grant aid over the Summer. State grant aid exhaustion is associated with a $550 decline in state grant aid and a $75 decline in Pell Grant aid. Since Pell lifetime eligibility exceeds that of state grant aid, there is the potential for state grant aid exhaustion to crowd in Pell Grant aid. I do not find this to be the case and, instead, observe declines in Pell Grant aid between $75 and $100. State grant aid exhaustion is also associated with a $25 increase in loans and $67 increase in quarterly earnings.

Estimates for Pell exhaustion are associated with large declines in both state and Pell Grant award amounts. I find no effects of exhausting Pell lifetime eligibility on loans but do find increases upwards of $200 in quarterly earnings. These results corroborate prior findings in my analysis showing how students increase employment and labor market earnings to offset losses in grant aid.

With respect to enrollment and academic outcomes, both state need-based grant aid and Pell exhaustion are associated with significant increases in part-time enrollment and decreases in full-time enrollment (see Table 6). State grant aid exhaustion seems to have limited effect on enrollment overall while Pell exhaustion is associated with a 2 percentage point decline in overall enrollment. The lack of detected effects of state grant aid exhaustion on overall enrollment may reflect remaining Pell eligibility that students can still use, or it may reflect differences in enrollment behavior at the point of state grant aid versus Pell exhaustion. The latter seems more likely given limited evidence of crowding in Pell. Both state and Pell Grant exhaustion are associated with modest and statistically significant declines in credits attempted and credits earned and small or null declines in cumulative GPA. In the following section, I discuss potential mechanisms driving these results and policy implications for designing financial aid programs to better support students through degree completion.

1.8 Conclusion

Reducing lifetime Pell Grant eligibility adversely impacted student enrollment behavior and academic outcomes. Estimates from additional student fixed effects models further suggest that grant aid exhaustion discourages full-time enrollment and hinders academic performance. Instead of motivating students to complete degrees more quickly, as suggested in Mabel (2020), reduced financial aid eligibility lengthens time to degree and lowers academic performance.

There are several reasons why my results may differ from those found in Mabel (2020), which was the first study to explore causal effects of changes in lifetime Pell eligibility. The author used data from Georgia four-year public universities to employ a matched DID design that compared 'High Pell' students who received at least 5 FTE years of Pell to 'Low Pell' students who received less than 5 FTE years of Pell before and after the policy change. The analysis, however, was limited to four-year college students and did not

Table 5: Estimated Effects of Lifetime Pell and State Grant Exhaustion on Financial Aid and Earnings

	State Grant Exhaustion			***Pell Exhaustion***		
	(1)	(2)	(3)	(1)	(2)	(3)
State Grant						
Post	-96***	-550***	-540***	-318***	-694***	-692***
	(11)	(13)	(13)	(17)	(26)	(27)
R^2	.081	.425	.444	.109	.337	.342
Pell						
Post	.155***	-75***	-92***	-129***	-333***	-305***
	(11)	(9)	(7)	(18)	(17)	(15)
R^2	.111	.481	.610	.042	.278	.312
Total Grant Aid						
Post	99***	-660***	-676***	-511***	-1,162***	-1,133***
	(23)	(20)	(18)	(34)	(39)	(37)
R^2	.084	.462	.538	.072	.365	.386
Loans						
Post	34***	25***	24***	7	6	8
	(5)	(7)	(7)	(11)	(16)	(16)
R^2	.273	.514	.536	.250	.453	.475
Quarterly Earnings						
Post	171***	67**	82***	270***	207***	208***
	(17)	(23)	(23)	(42)	(54)	(54)
R^2	.531	.521	.536	.528	.528	.541
Excludes Summer & Winter Terms	No	Yes	Yes	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes	No	No	Yes
N	14,400	14,400	14,400	4,695	4,695	4,695

Notes: All regressions include student and year fixed effects. Student observations are limited to 40 terms. Coefficients for Post indicate changes in financial aid and earnings outcome variables in the semester after exhausting state grant aid and Pell. All regressions estimating the effects of state grant aid exhaustion are run on 2014-2016 entry cohorts, whereas regressions estimating effects of Pell exhaustion are for 2014-2015 entry cohorts. $***p < 0.001, **p < 0.01, *p < 0.05$.

Table 6: Estimated Effects of Lifetime Pell and State Grant Exhaustion on Enrollment and Academic Outcomes

	State Grant Exhaustion			*Pell Exhaustion*		
	(1)	(2)	(3)	(1)	(2)	(3)
Full-Time Enrollment						
Post	.062***	-.050***	-.044***	-.045***	-.141***	-.128***
	(.004)	(.002)	(.002)	(.006)	(.007)	(.007)
R^2	.016	.230	.195	.056	.357	.362
Part-Time Enrollment						
Post	.014***	.047***	.044***	.038***	.122***	.128***
	(.002)	(.002)	(.002)	(.006)	(.007)	(.007)
R^2	.085	.178	.195	.124	.324	.362
Any Enrollment						
Post	.076***	-.003		-.007	-.019***	
	(.003)	(.002)		(.006)	(.003)	
R^2	.044	.195		.040	.155	
Credits Attempted						
Post	.555***	-1.15***	-.960***	-.575***	-1.73***	-1.46***
	(.052)	(.038)	(.031)	(.085)	(.081)	(.073)
R^2	.048	.438	.489	.054	.387	.418
Credits Earned						
Post	.535***	-1.19**	-1.01***	-.428***	-1.50***	-1.25***
	(.051)	(.040)	(.035)	(.082)	(.083)	(.077)
R^2	.055	.425	.455	.058	.356	.371
Cumulative GPA						
Post	-.043***	-.019***	-.013***	-.022***	.002	.007
	(.002)	(.003)	(.003)	(.004)	(.006)	(.006)
R^2	.574	.657	.672	.498	.572	.582
Excludes Summer & Winter Terms	No	Yes	Yes	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes	No	No	Yes
N	14,400	14,400	14,400	4,695	4,695	4,695

Notes: All regressions include student and year fixed effects. Student observations are limited to 40 terms. Coefficients for Post indicate changes in financial aid and earnings outcome variables in the semester after exhausting state grant aid and Pell. All regressions estimating the effects of state grant aid exhaustion are run on 2014-2016 entry cohorts, whereas regressions estimating effects of Pell exhaustion are for 2014-2015 entry cohorts. $***p < 0.001, **p < 0.01, *p < 0.05$.

include estimated effects on outside earnings. These are important limitations to consider, especially after taking into account results presented throughout this paper. In my analysis, 60 percent of students who received at least 5 years of Pell by Fall 2012 started at a community college; these students experienced longer average time to degree, relied more heavily on federal and state grant aid, and had lower Expected Family Contribution (EFC) compared to four-year college starters. They were also more likely to work while enrolled in college. Greater economic precarity and increased labor market participation may make re-enrolling in college that much more difficult after losing financial aid eligibility.

Results from fixed effects models show that students' primary response to lower lifetime Pell eligibility and other financial aid loss is to increase earnings in the labor market. This takeaway aligns with prior literature showing increased earnings to offset financial aid (Broton et al., 2016; Carruthers & Ozek, 2016). It also aligns with intuition from the human capital investment framework: credit constrained students use Pell and state grant aid to alleviate financial constraints to subsidize investment in higher education and substitute earnings from employment once grant aid eligibility is used up. In this particular setting, the opportunity to increase employment hours may involve fewer transaction costs since close to 80 percent of students in a given semester work while enrolled in college.

The fact that students substitute increased employment for lost grant aid holds immediate implications for student performance and degree completion outcomes. Increased employment likely contributes to observed declines in credits attempted, credits earned, and cumulative GPA and the shift away from full- to part-time enrollment. More time spent working in the labor market may come at the expense of time spent studying or completing assignments. The loss of financial aid and increase in hours worked may present heightened challenges in enrolling and excelling in college for these students.

Exhausting financial aid may present students with a more salient opportunity to evaluate the costs and benefits of attending college. For many students, this opportunity resulted in opting not to re-enroll and to instead increase quarterly earnings. Credit constraints are more likely to bind for these students in the absence of grant aid, and it may simply be unrealistic for students to complete coursework alongside an increase in hours worked. Results highlight the extent to which financial aid matters when it comes to supporting students through degree completion. The students directly impacted by reductions in lifetime Pell eligibility were within 1 to 2 semesters of completing their degree. They were also more likely to be students of color and to have started at a community college. Extended financial aid eligibility could help to support these students by reducing the need to work outside of school hours and ensuring that they have sufficient time and resources needed to complete their degrees.

Findings from this analysis hold important implications for lifetime eligibility of both federal and state financial aid programs. Most states, including New York, California, and Florida, have lower lifetime eligibility for their grant programs as compared to Pell (Mabel, 2020). This could lead to students exhausting state aid prior to Pell, which then further limits financial aid options once Pell has also been exhausted. Interestingly, this was not the case for students in my analysis. Only 41 percent of students who exhausted Pell exhausted state grant aid as well (author's calculation). Students were required to enroll full-time to be eligible for state grant aid, which likely prohibited many students from applying. The state opened up program eligibility to part-time students in 2022, a change that should dramatically expand the program's reach. Unused financial aid eligibility and/or inefficient use of state and Pell Grant aid programs underscores the need for financial aid advising. Student interactions with financial aid counselors are limited, especially for community college students (Eichelberger et al., 2017; McKinney & Roberts, 2012). Expenditures on student services, which includes financial aid counseling, declined at the university system in this analysis in the year immediately after lifetime Pell eligibility was reduced. Institutional resources devoted to financial aid advising can help to ensure students know all of their options when it comes to paying for college through degree completion.

The pretense for cutting Pell lifetime eligibility from 9 to 6 FTE years arose from budgetary concerns rather than any evidence suggesting that shorter grant eligibility could improve student outcomes. Findings from Mabel (2020) suggest shorter Pell lifetime eligibility may act as an incentive for students to graduate sooner. In contrast, I find that the reduced lifetime Pell eligibility significantly lowers student enrollment intensity and academic performance. Community college entrants and students of color were most likely to be impacted by the policy change. Students who exhausted lifetime financial aid eligibility offset declines in grant aid through increased labor market participation. These results beg the question of whether there might be non-threatening financial aid incentives for students to graduate sooner as well as how state and federal financial aid policies might better serve community college entrants and historically underrepresented student groups through degree completion. Policymakers might consider extending lifetime Pell eligibility for community college entrants and/or allowing students to roll over unused eligibility in the pursuit of further education. States and institutions might also consider additional grants or scholarships for students who are near to completing their degree as well as maintaining affordable tuition rates. Federal and state financial aid programs will continue to play a critical role in enabling students to enroll and persist in college. Extending lifetime Pell eligibility limits might offer students more certainty in how they will afford college through degree completion while also recognizing that the lack of timely degree completion on the part of many students stems more from structural issues related to college access than student ability or motivation.

2 Expanding School Counseling: Impacts of California Funding Changes (joint with Christine Mulhern)

2.1 Introduction

School counselors can be important resources for students, helping them navigate educational options, develop social and emotional skills, and succeed in and beyond school (Carrell & Hoekstra, 2014; Mulhern, 2022; Reback, 2010). Despite their important role, most students have limited access to counselors. On average, one school counselor serves over 400 students per year and typically only spends a few hours with each student. Recently, there has been a push at federal and state levels to increase funding for and access to school counselors. For instance, many states used some of their Federal Covid-19 relief funding to hire more counselors, and California is pushing to double its number of school counselors (Modan, 2022; Prothero & Riser-Kositsky, 2022). However, little research examines how to effectively design policies for expanding access to school counseling and the impacts of expanded access to school counselors on students.

We study one of the largest policies to increase funding for school counselors. Starting in 2006, California's Supplemental School Counseling Program provided block grants totaling $200 million to schools to support increased access to middle and high school counselors. The legislation required the funds "supplement, and not supplant expenditures" for school counseling programs, and "provide supplemental counseling services delivered by personnel who hold a valid pupil personnel services credential." Over 1,000 additional counselors were hired in the first two years of the program, and student-to-counselor ratios fell by about 100 students on average. In 2009, the spending guidelines were eliminated and the number of counselors on staff declined. We study the funding, equity, and achievement implications of the funding expansion.

We use student, school, and educator data from the California Department of Education to study how schools responded to the policy and funding changes, and the subsequent impacts on student outcomes. Overall, we find that schools used the funding to hire more school counselors, with the average high school hiring .5 additional counselors with the funding. This represented an average reduction in caseloads of about 40 students. Schools in less rural areas serving a larger student body were most likely to hire counselors. We also see a large influx of counselors with fewer years of professional experience.

We use an event study design to examine how changes in student outcomes over time correspond to the timing of the policy. We leverage variation over time and across schools in a two-way fixed effects model, finding that increased funding for counselors improved high school exit exam pass rates in Math and ELA by 2 to 4 percentage points but cannot rule out null effects on high school graduation and SAT test-taking rates. This is consistent with research on high school counselors in other settings and with the general goals of the program (Mulhern, 2022; SSCP, 2006). We use a simulated instruments model and alternatively specified difference-in-differences models to complement these results.

This study builds on a large literature on school spending by studying the impacts of spending school resources on one particular input to education production, in this case school counseling. While much evidence indicates that student outcomes, including educational attainment and achievement, improve with more school spending (e.g., Jackson, Johnson, & Persico, 2016; Lafortune, Rothstein, & Schanzenbach, 2018), much less is known about the benefits of spending money on particular resources or staff. Much of the literature on specific resources focuses on teachers and classroom size. This work indicates that increasing school funding and reducing the number of student-to-teacher ratios can improve educational outcomes (Fredriksson, Ockert & Oosterbeek, 2013; Krueger, 1999).

Thus, increased spending on school counseling could be beneficial, but little research rigorously examines the importance of access to school counselors, and counselor caseloads, for students' educational attainment. The causal papers in this space show that increased access to elementary and middle school counselors improves behavior and test scores for subgroups of students (Reback, 2010; Carrell & Hoekstra, 2014), but these papers focus on small policy changes for counselors with very large caseloads. One of them focuses on hiring counseling interns to lower caseloads and neither study high school counselors and their effects on high school students, for whom we may expect counselors to be especially important. Estimates from papers on high school counselor caseloads are very noisy and subject to important limitations (Hurwitz & Howell, 2014; Mulhern, 2022). While many papers document a negative correlation between counselor caseloads and academic achievement, much of this work is not causal and may simply pick up on the fact that higher resourced schools, with students from higher income backgrounds, tend to have counselors with smaller caseloads (Gagnon & Mattingly, 2016; Lapan, Whitcomb & Aleman, 2012; Woods & Domina, 2014). We build on this work by providing some of the first evidence of how expanding spending on and access to middle and high school counselors impacts educational attainment.

Finally, this work builds on studies such as Jepsen & Rivkin (2009) which study the general equilibrium effects of large scale public policies. Since we study a statewide policy change, rather than an increase in the number of counselors in a few schools, our estimates account for how such a large policy may change the types of counselors to which students have access. Such rapid hiring of counselors could have negative effects if it pulls less qualified counselors into the profession (Jepsen & Rivkin, 2009; Mulhern, 2022). We find that the increase in the number of counselors stemming from program funding reduced average counselor experience by about three years. Nevertheless, our estimates suggest the positive effects on academic outcomes from greater access to counselors exceed the negative effects of access to potentially less qualified counselors. These results hold timely recommendations for policymakers as schools look for ways to reduce equity gaps in college access and address student mental health concerns coming out of the COVID-19 pandemic.

2.2 Policy Background

California State Assembly Bill 1802 established the Supplemental School Counseling Program (SSCP) in 2006 to make available additional state funds for districts to spend on school counselors. These funds were intended to help address low high school graduation and college matriculation rates in the state. An influential 2006 report on California Educational Opportunity called out the state's declining performance in terms of both high school graduation and college access relative to other states (Rogers et al., 2006). In the early 2000s, less than a third of high school graduates in California matriculated to a four-year college and the list of contributing factors included disparities in access to quality teachers, limited access to rigorous curriculum, and lack of adequate (or any) college advising, as highlighted by California's average student-to-counselor ratio in excess of 500 to 1. Declines in the percentage of students enrolling in the California State University and University of California systems were a red flag for policymakers, who turned their attention to school counselors as a potential remedy for improving college access and reducing persistent racial equity gaps (Rowell et al., 2008).

In January 2006, California State Assembly Bill 1802 passed, and SSCP funds were made available to all California districts serving grades 7 through 12 students in the 2006-2007 academic year. Funds were allocated to districts based on reported enrollment and had to be used specifically for school counseling programs. Funds from the SSCP could not supplant extant school district expenditures on counselors or be used on non-counseling inputs (SSCP, 2007). Districts that opted to receive program funds were required to ensure that all students had access to a school counselor and received individualized reviews of academic and career goals (SSCP, 2007). Other notable provisions of the program include the requirement for districts receiving funds to develop a high school transition and college preparation curriculum and ensure that students meet with a school counselor at least once during grades 7, 10 and 12 (SSCP, 2007). The program stipulated that counseling services be prioritized for students at risk of failing out of school or who had already failed the California State High School Exit Exam (CAHSEE).

The policy contained limited restrictions for school district receipt of funds or accountability, and more than 85 percent of districts received SSCP funding prior to program termination. Funding allocations were based on student enrollment, with the total annual SSCP funding of $200 million working out to $79 in additional per student spending for counseling services. While the money had to be used on counseling services, schools took a number of approaches to spending SSCP funds. For instance, some districts used funds to hire new counselors while others used the money to increase counseling services specifically for students at-risk of dropping out. With regard to accountability, CDE asked school districts to provide an annual report including data on relevant student outcomes and metrics on student-to-counselor interactions, but receipt of funds or funding amount was not dependent on year-over-year results presented in these reports (Zubko, 2010).

Both the policy stakeholders who advocated for the law and the law itself indicate that the SSCP targeted lowering student-to-counselor ratios as a means of improving high school graduation rates, college preparation, and matriculation to California colleges and universities and decreasing persistent equity gaps in college access. This study examines whether the legislation achieved its intended goals and how the school counseling workforce changed during this time period.

2.3 Anticipated Effects

Successfully navigating the college application process requires a tremendous amount of information, preparation, and advising to complete, for instance, FAFSA applications, requisite standardized tests, essays and college applications (Dynarski, Page, & Scott-Clayton, 2022). Complexity in the college application process may disproportionately act as a barrier to college access for low-income students and students of color in the US in spite of these student groups' comparable expectations on college-going with their white, higher-income peers (Perna & Titus, 2005). Similarly, information constraints are more likely to bind for students that lack access to social and cultural capital that can support students as they navigate their academic and professional plans post-high school (Dynarski, Page, & Scott-Clayton, 2022; Dynarski et al., 2021; Hoxby & Turner, 2013; Mulhern, 2021).

Since the SSCP offered additional funding for school counselors, we anticipate that the program reduced student-to-counselor ratios and increased students' likelihood of having any interactions with a counselor as well as the quality of interactions. The SSCP required any newly hired counselors to be licensed, so the quality of counselors and counseling sessions may have improved as a result of increased funding.

However, the policy could also have led to the hiring of less experienced counselors since it was unclear whether there was sufficient supply of high quality counselors to meet the rapid growth in demand spurred by the policy. While funds received from SSCP had to be spent on counseling services, school districts had discretion in hiring decisions, which in turn is likely to impact variation in effect sizes on outcomes of interest. For instance, some school districts may have opted to use funds to hire counselors full-time whereas others may have opted for an increase in part-time counselors. We hypothesize hiring additional full- as opposed to part-time counselors, counselors with more in-district experience, or counselors that are more likely to share racial identities of the student body had larger effects on student outcomes.

We further anticipate reduced student-to-counselor ratios to boost the percentage of students completing college preparatory exams such as the SAT since counselors can help to notify students of these exams, help register them, and provide fee waivers. Expanded access to counseling may improve student test scores if expanding counselors' capacity helps counselors better assign students appropriate courses, provide accommodations, or support student needs that can impact learning. Still, we expect increases in the number of counselors on staff to have larger effects on SAT participation rather than actual scores.

Moreover, since the California policy was designed to prioritize support for students at-risk of dropping out or failing the California High School Exit Exam (CAHSEE), we expect to find reductions in achievement gaps, and improvements in high school graduation rates and the percentage of students scoring proficient on this exam. At the time of SSCP implementation, passing high school exit exams was a requirement for high school graduation. If a student failed one or multiple sections of the exam during their sophomore year, they were allowed to retake failed sections of the exam in subsequent years. Per language in SSCP legislation, guidance counselors were encouraged to help at-risk students enroll in or retake exit exams and may also have informed or reminded students of the requirement to pass exit exams in order to graduate from high school.

Upticks in high school exit exam pass rates as a result of reduced student-to-counselor ratios should in turn boost high school graduation rates. Exit exams were a key barrier to high school graduation up until 2015, when California passed legislation to remove the exit exam as a requirement for high school graduation. We expect effects of SSCP on high school graduation rates to be less immediate relative to other outcomes included in our analysis. In other words, it likely takes several years to observe counselor effects on graduation rates.

As a result of SSCP funding, we anticipate several spillover effects on school climate and instruction. For instance, increasing the number of counselors in a school may reduce teachers' time spent on behavior management or other tasks assigned that could be assigned to counselors, which may increase instructional time or quality. Greater counselor availability likely improves the number and quality of interactions with students, which could help counselors to provide more targeted or individualized interventions. Both of these counselor effects should improve mental health resources available for students and school climate more generally. While not fully addressed in this paper, we hope to expand on this more in future research.

2.4 Data

We construct a school-by-year level data set using publicly available data from the California Department of Education (CDE). CDE data include school enrollment by grade, race and gender, staff assignments and demographics, school financial records, and high school outcomes data. From staff assignment and demographic data, we are able to identify the number of counselors at each school and further disaggregate this information by race/ethnicity, gender, and years of professional experience both within district and overall. School finance data include detailed records on school revenues and expenditures broken down by specific function and goals as reported through the Standardized Account Code Structure (SACS). This includes year-over-year school expenditures on, for instance, guidance and counseling services overall as well as expenditures made specifically through the Supplemental School Counseling Program. Outcome variables of interest are constructed through high school graduation rates from Adequate Yearly Progress data, SAT test-taking rates as captured through Standardized Testing and Reporting (STAR), and California High School Exit Exam (CAHSEE) pass rates in Math and English Language Arts (ELA).

2.4.1 Sample

Our analytic sample focuses on students enrolled in California high schools from 2003 to 2012.[4] Student outcomes included in our analysis are at the high school level, hence our exclusion of middle schools from main results. Many schools had no counselors on staff, which prohibits us from calculating baseline student-to-counselor ratios for these schools. We limit our preferred analytic sample to schools with non-zero counselors to address this issue and include summary statistics and main results for excluded schools (see Appendix Tables B.1 and B.2 and Figure B.4). We omit Los Angeles Unified School District from our analysis due to reporting errors in the data.

Table 7 shows high school student demographics over time for the more than 700 high schools included in our sample. The total number of students enrolled is about 1.2 million per year, and average high school

[4]We refer to school years based on the fall term. For instance, the 2006-2007 academic year is referred to as 2006.

enrollment declined from 1,827 in 2003 to 1,680 in 2012. The share of Hispanic students increased by 10 percent over this same time period while the share of White students declined. The share of students receiving free or reduced-price meals (FRPM) also increased by more than 15 perecnt. High school student outcomes in Table 8 highlight stable high school graduation rates near 90 percent and high school exit exam pass rates in math and ELA near 80 percent. The percentage of students taking the SAT prior to high school exit increased from a low of 38 percent in 2003 to 46 percent in 2011.

Table 7: High School Student Demographics - Analytic Sample

Year	Enrollment (millions)	Avg. School Enrollment	Female	Asian	Black	Hispanic	White	FRPM
2003	1.10	1827	0.49	0.09	0.06	0.37	0.43	.
2004	1.15	1848	0.49	0.09	0.06	0.38	0.41	0.33
2005	1.17	1860	0.49	0.09	0.06	0.39	0.40	0.34
2006	1.17	1848	0.49	0.09	0.06	0.40	0.38	0.37
2007	1.18	1836	0.49	0.09	0.06	0.41	0.37	0.39
2008	1.19	1806	0.49	0.09	0.06	0.42	0.36	0.42
2009	1.18	1767	0.49	0.09	0.06	0.44	0.35	0.45
2010	1.18	1742	0.49	0.09	0.06	0.45	0.33	0.47
2011	1.18	1712	0.49	0.09	0.06	0.46	0.32	0.33
2012	1.18	1680	0.49	0.09	0.06	0.47	0.31	0.51

Notes: FRPM refers to the proportion of students enrolled at a high school who are eligible for Free or Reduced Price Meals.

Table 8: High School Student Outcomes - Analytic Sample

	Graduation Rate	SAT	Math	ELA
2003	0.92	0.38	0.76	0.76
2004	0.92	0.39	0.78	0.76
2005	0.92	0.40	0.78	0.78
2006	0.91	0.41	0.78	0.78
2007	0.90	0.41	0.79	0.79
2008	0.89	0.39	0.79	0.80
2009	0.91	0.42	0.80	0.81
2010	0.89	0.44	.	.
2011	0.90	0.46	0.82	0.83
2012	0.91	.	0.82	0.83

Notes: SAT refers to SAT test-taking participation rates across high schools, and Math and ELA refer to the percentage of students who scored proficient in that section of the California High School Exit Exam. Missing cells are a result of issues processing data from CDE for those years.

2.4.2 Counselor Spending and Characteristics

First, we describe how schools responded to the SSCP funding and whether the legislation had its intended effects on access to school counseling. In 2006, 88 percent of sample districts received SSCP funds; this jumped to 95 percent of districts in 2007 and fell to 77 percent in 2008. Table 9 shows the difference in high school student demographics and outcomes for districts that used SSCP funds versus those that did not. Districts that took up SSCP funds were less likely to be located in rural areas, served a lower percentage of students eligible for free or reduced price lunch, and performed slightly better with respect to SAT participation and exit exam pass rates. Districts that used SSCP funds also had significantly larger student-to-counselor ratios despite having more experienced counselors and total counselors on staff, which in part reflects the larger average school size within districts that used SSCP funds.

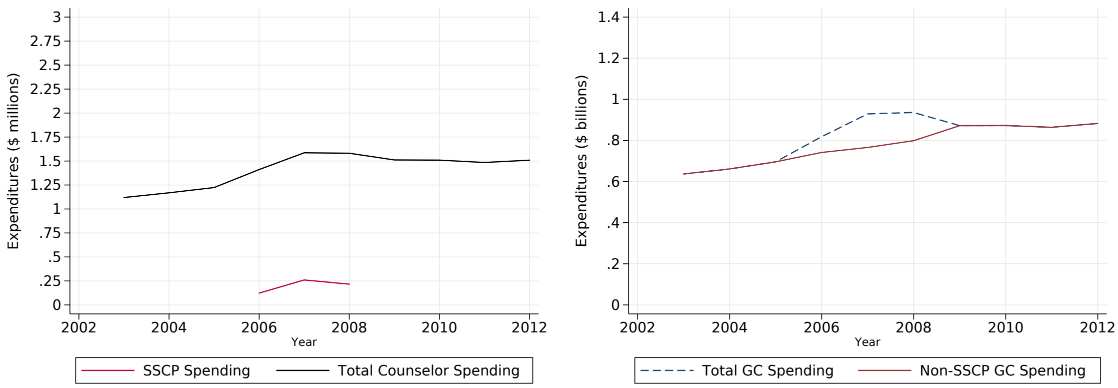
Table 9: Sample High School Characteristics, by SSCP Takeup

	(1) Used SSCP Funds	(2) Never Used SSCP Funds	(3) Difference
Student Characteristics			
FRPM	0.37	0.47	.10***
Female	0.49	0.48	-.01**
Asian	0.08	0.04	-.03**
Hispanic	0.39	0.43	.04
Black	0.05	0.04	-.01
White	0.42	0.39	-.02
Rural	0.25	0.39	.13***
Urban	0.32	0.30	-.02
Outcomes			
Graduation Rate	0.91	0.89	-.01
SAT	0.40	0.36	-.04**
HSEE Math	0.81	0.79	-.02*
HSEE ELA	0.80	0.78	-.02*
Counselor Characteristics			
Counselor Ratio	440	368	-72**
Counselors	4.20	3.40	-.79***
<5 Years Experience	.92	.77	-.15
5-10 Years Experience	.89	.75	-.14
10+ Years Experience	2.38	1.88	-.503***

Notes: We report means in student characteristics, outcomes, and counselor characteristics at high schools included in our analytic sample in columns (1) and (2) for years 2006-2008. Counselors details the average overall number of counselors per high school whereas the experience variables capture the number of counselors at high schools by experience levels. Column (3) reports p-values from ttests comparing means for high schools that used SSCP funds versus those that did not. $***p < 0.01, **p < 0.05, *p < 0.10$.

Average district expenditures on guidance counseling increased by about a quarter of a million dollars

Figure 3: Trends in Counselor Spending



(a) Average District Counseling Expenditures

(b) Total Counseling Expenditures

as result of SSCP. Figure 3a shows that districts spent over $1.5 million on average during SSCP program years before declining in 2010; similarly, Figure 3b shows that total counseling expenditures increased by about $200 million before dropping off in 2010. The vast majority of counselor spending overall and through SSCP goes toward salaries and health benefits (see Appendix Figure B.1). Increases in spending increased the total number of high school counselors by about 1,000 and helped to lower student-to-counselor ratios by nearly 100.[5] Per Figure 4, the number of counselors stagnated after program termination, and counselor ratios increased.

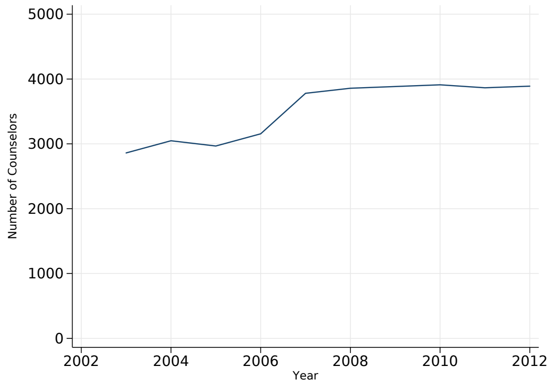
Increased counselor hiring as a result of SSCP impacted counselor demographic and employment characteristics (see Figure 5). Counselors hired during SSCP program years were slightly more likely to be Hispanic and less likely to be White. The proportion of tenured counselors declined from 70 percent of all counselors in 2005 to about 60 in 2007. Declines in the proportion of tenured counselors coincide with increases in the proportion of temp and probationary counselors. Counselors with less than 5 years of experience were most likely to be hired during SSCP program years and average counselor experience declined. We explore the potential repercussions of changes in counselor experience further in the discussion section.

2.5 Research Design

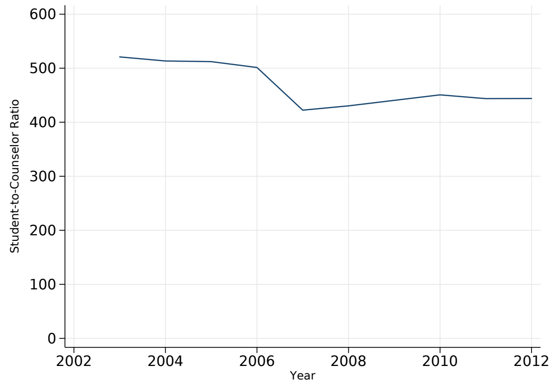
Next, we describe our approach for estimating the causal effects of the program on student outcomes. First, we leverage the sharp timing of the SSCP policy in an event study design. Second, we use alternatively specified difference-in-difference models and simulated instruments to extend on our main results measur-

[5]Reductions in student-to-counselor ratios here do not refer to causal effects of the program, but rather to the decline in average student-to-counselor ratios observed from 2005 to 2008.

Figure 4: Trends in School Counselors

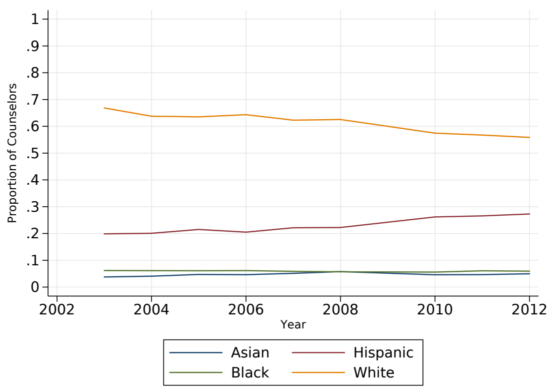


(a) Total Number of High School Counselors

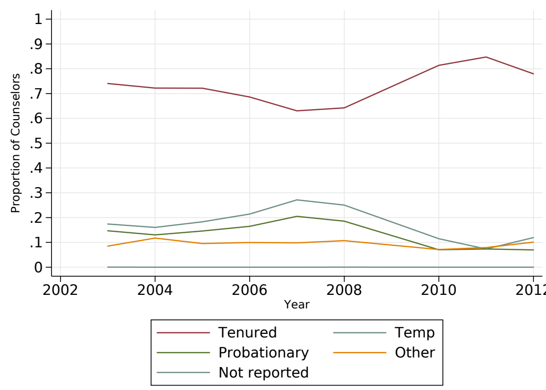


(b) Average Student to Counselor Ratios (HS)

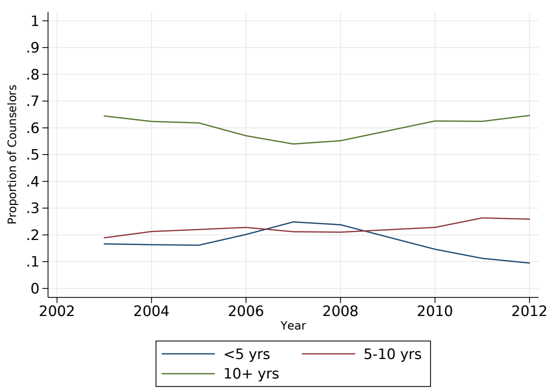
Figure 5: Trends in Counselor Characteristics



(a) Race/Ethnicity



(b) Employment Status



(c) Experience Level

ing causal effects of access to additional counselors on student outcomes.

2.5.1 Event Study

The event study design compares changes before and after the policy implementation in 2006 while controlling for statewide trends in student outcomes. We include school fixed effects to account for differences across high schools in things like other services offered and community factors, and time fixed effects to account for other changes over time. We begin by estimating an event study model as follows:

$$Y_{st} = \tau_t + \gamma_s + X_{st}\delta + \beta SSCP_{st} + \epsilon_{st} \tag{3}$$

Here, an outcome, Y_{st} , is defined for students in school s and year t. We control for fixed effects by school, γ_s, and year, τ_t. We run the model with and without a vector of student and school-level covariates, such as racial composition, percent of students receiving free or reduced-price meals, and instructional spending (X_{st}). $SSCP_{st}$ indicates exposure to the SSCP based on year of enrollment and switches to one after implementation. β corresponds to the difference in outcomes across cohorts exposed to the SSCP policy versus not. We cluster standard errors at the school level.

Our event study design will yield valid causal estimates so long as there were no anticipatory effects prior to the SSCP and outcomes would have evolved in a similar fashion after the SCCP was implemented if not for the program. The former is unlikely in this instance given limited red tape and criteria in applying for and receiving SSCP funds. We implement an event study design that is robust to the concerns raised in the recent literature on two way fixed effects (e.g., Clark & Tapia-Schythe, 2021) and offer visual evidence of a lack of pre-trends across most outcomes of interest.

2.5.2 Alternative Models

To complement event study results and better identify causal effects and treatment heterogeneity, we use a simulated instruments model as well as other approaches to difference-in-differences. The simulated instruments approach enables us to measure the effects of different levels of treatment intensity and is useful for understanding how much different levels of funding, or access to counselors, are associated with student outcomes. Funding for the SSCP was determined by school enrollments with schools allocated approximately $80 per student. The potential for this allocation to be sufficient for paying a FTE counselor thus varied by school size. The simulated instruments are also advantageous for causal inference because they are not subject to bias in who accepts the funds. We construct them by estimating, for each school, the amount of funding for which they were eligible to receive through the SSCP based on their 2006 enrollment. Then, we use these simulated instruments in a two-stage least-squares regression. The first stage equation estimates the relationship between the simulated instruments and actual changes in counselors and student-to-counselor ratios (equation 4). This amounts to instrumenting the number of counselors in a school or the change in student-to-counselor ratios using funding allocations.

$$Counselors_{st'} = \theta_t + \gamma_s + X'_{st'}\delta + \beta_1 SSCP_{st} + \epsilon_{st} \quad (4)$$

$$Y_{ist'} = \theta_t + \gamma_s + X'_{st'}\delta + \beta_2 Coun\hat{s}elors_{st} + \epsilon_{st} \quad (5)$$

In the second stage (equation 5), we estimate the relationship between student outcomes $Y'_{ist'}$ and access to counselors using our estimates $Counselors'_{st}$ from the first stage. Since the simulated instruments approach leverages variation across schools and time, both first and second stage equations include year fixed effects θ_t to flexibly control for other time-varying changes, such as state economic conditions. Equations 4 and 5 also include school fixed effects γ_s and control for the number of school counselors in 2005, the year prior to policy implementation. We cluster standard errors at the school-level.

Last, we use two alternative versions of a difference-in-differences model. The first is a simple a difference-in-differences model comparing outcomes before and after policy implementation for high schools in districts that used SSCP funds compared to those that did not use SSCP funds. Within this framework, the interaction term between treatment, or receipt of SSCP funds, and the post-period, or any year after 2005, represents program effects on outcomes of interest. This is represented by β in equation 6. Internal validity of these results hinges on parallel trends across outcomes for treatment and control schools. This model may be under powered given high take up rates of SSCP funds across districts, which limits the number of available control high schools. Still, if parallel trends hold, this model serves as a useful robustness check on event study results.

$$Y_{ist'} = \theta_t + \gamma_s + \alpha_1 Post_t + \alpha_2 Treatment_s + \beta Post_t x Treatment_s + \epsilon_{st} \quad (6)$$

We also estimate program effects using a continuous difference-in-differences model, where simulated SSCP funding is the continuous treatment variable. This model is described in equation 7, and it includes school and year fixed effects. The treatment effect estimate, β, indicates the extent to which (eligibility for) additional SSCP funds are associated with different student outcomes. Thus, this model offers further insight into variation in treatment effects over time and by treatment intensity.

$$Y_{ist'} = \theta_t + \gamma_s + \alpha_1 Post_t + \alpha_2 SSCPFunds_s + \beta Post_t x SSCPFunds_s + \epsilon_{st} \tag{7}$$

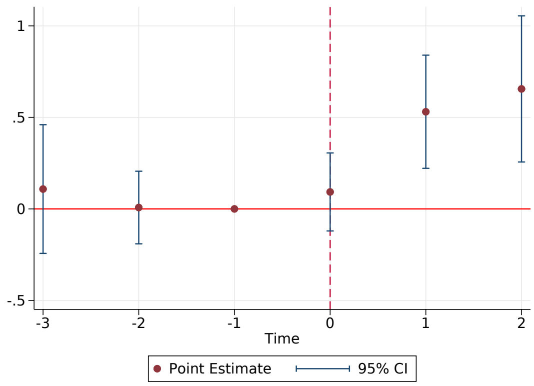
2.6 Results

2.6.1 Event Study

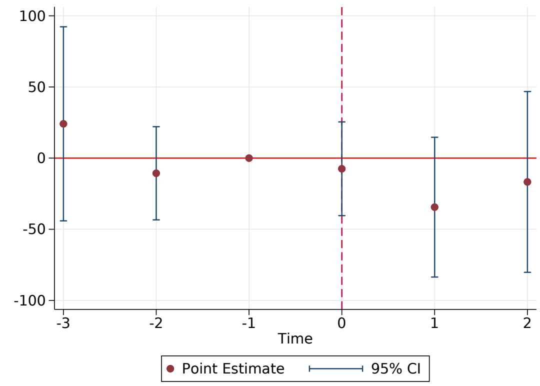
First, we present results from our event study design. Figures 6a and 6b show SSCP impacts on the number of counselors at a given high school and the change in student-to-counselor ratios relative to 2005. The average number of counselors increases by 0.53 and 0.63 counselors in 2007 and 2008, respectively. Student-to-counselor ratios decreased by 35 students in 2007 (statistically significant at $p < .10$) and by 17 students in 2008, though the latter is imprecisely estimated. Panels (c) to (f) of Figure 6 show policy effects on selected student outcomes. We cannot rule out null effects on high school graduation rates, which also exhibit positive pre-trends. Students who entered 9th grade when the policy began would have been in the graduating cohort four years after the policy began, so the lack of detected effects on this outcome is not too surprising and indicates that it may take longer for counselors to have an impact on graduation rates.

In contrast, we observe positive effects on SAT test-taking and high school exit exam pass rates in Math and ELA. SSCP increased the percentage of students taking the SAT by 2 percentage points in 2007 (significant at $p < .10$) and 2008, though the latter year is imprecisely estimated. SSCP is also associated with a 2 to 4 percentage point increase in Math and ELA pass rates. We include event study results inclusive of middle and high schools as well as inclusive of student demographic and instructional spending controls specified in equation 3 in the appendix. When middle schools are included in the analysis, first stage effects on the number of counselors are comparable to the main results of a .5 to .6 increase, but effects on counselor ratios are much larger given lower average student enrollments at middle schools (see Appendix Figure B.2). After including demographic controls, first stage effects on counselors are similarly between .5 and .6, though effects on counselor ratios are larger (-50) and statistically significant at $p < .05$. Results using demographic controls in Appendix Figure B.3 further show null effects on high school graduation rates, SAT test-taking, and Math while effects on ELA remain positive and significant.

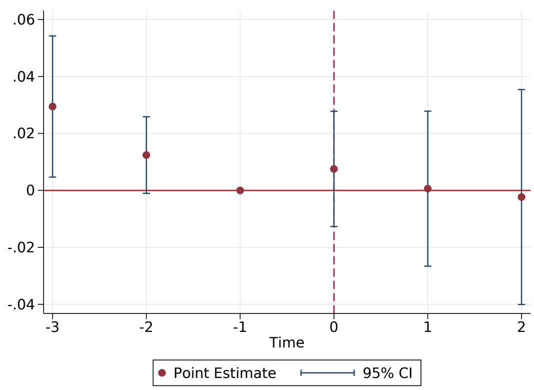
Figure 6: Event Study Results



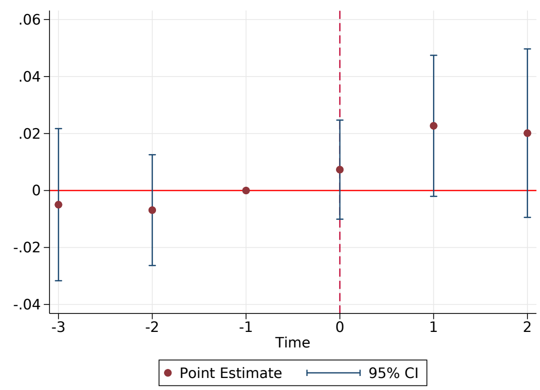
(a) Counselors



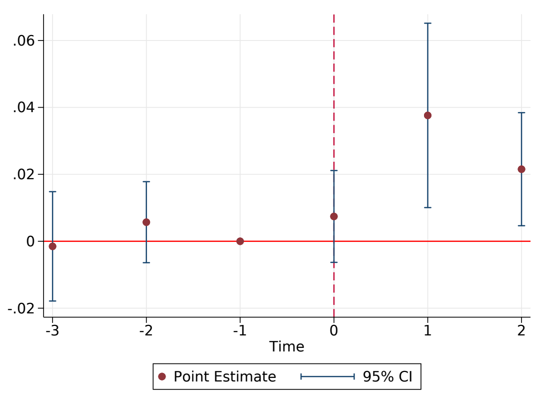
(b) Counselor Ratio



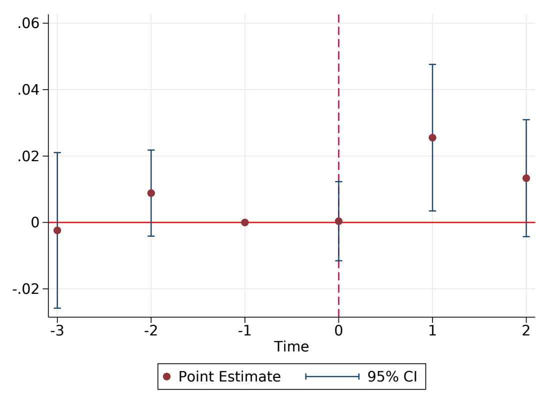
(c) HS Grad Rate



(d) SAT



(e) ELA



(f) Math

2.6.2 Additional Results

Next, we fit our instrumental variables models. In Table 10, we present four first stage effects of estimated school level SSCP funding on the number of counselors at a school as well as the change in student-to-counselor ratios. For these regressions, SSCP funding is scaled per 100,000 dollars. These estimates indicate that every $100,000 increase in available SSCP funding is associated with an additional .5 counselors at the school and reduces the average counselor's caseload by 40 to 50 students, effect sizes that are comparable to those estimated in previously reported event study models. To reiterate, we focus on estimated rather than actual spending for two reasons. First, estimated spending is less subject to bias in terms of which types of schools used the funding. Second, actual spending amounts are only available at the district level, so it is harder to assess how district level changes in spending are associated with school-level changes in staffing and outcomes. Point estimates for both the number of counselors and the change in student-to-counselor ratio are stable across models with and without school demographic controls such as the racial composition of the student body, instructional expenditures, and the number of counselors at a school in 2005, the year prior to program implementation.

Table 10: First Stage Results: Simulated SSCP Funding

	(1) Counselors	(2) Counselors	(3) Counselor Ratio	(4) Counselor Ratio
Estimated SSCP Funding	0.505***	0.487***	-40.016***	-47.328***
	(0.069)	(0.073)	(12.367)	(14.362)
Observations	3,730	3,059	3,730	3,059
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	No	Yes

Notes: Estimated SSCP Funding is scaled per $100,000. Coefficients represent estimates from OLS regressions that include school and year fixed and demographic controls where specified. All regressions include controls for the number of counselors at a given high school in 2005, the year prior to program implementation. Regressions are limited to years prior to 2009 and exclude schools with simulated SSCP spending in excess of $1,000,000. Counselor Ratio as an outcome variable in columns (3) and (4) refers to the change in student-to-counselor ratios from the prior year. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

We then instrument the number of counselors at a school with simulated SSCP funding, and estimate effects of additional counselors on student outcomes (see Table 11). We find that an additional counselor at the school increases the percentage of students who pass Math and ELA exit exams by 2 to 3 percentage points, results that again align with those previously reported from the main event study model. We cannot rule out null effects on SAT test taking and observe a negative effect on high school graduation rates. Event study and IV results are relatively consistent across effects on the number of counselors, student-to-counselor ratios, and exit exam scores. We explore this issue of observed negative effects on high school graduation rates in the following section of the paper.

Table 11: IV Results: Simulated SSCP Funding

	(1) Graduation Rate	(2) SAT	(3) ELA	(4) Math
Counselors	-0.051***	0.012	0.032***	0.018**
	(0.013)	(0.008)	(0.009)	(0.008)
Observations	3,675	3,608	3,704	3,704
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Coefficients represent estimates from two-stage least squares regressions that include school and year fixed. The number of counselors is instrumented with estimated SSCP funding scaled per $100,000. All regressions include controls for the number of counselors at a given high school in 2005, the year prior to program implementation. Regressions are limited to years prior to 2009 and exclude schools with simulated SSCP spending in excess of $1,000,000. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

We present results from a simple difference-in-difference in Table 12, where we regress outcomes on an indicator for whether high schools ever took up SSCP funds, a post indicator for years after 2005, and an interaction term between the two. We show parallel trends for key outcomes in Appendix Figure B.5. Coefficients are much less precisely estimated as compared to main results from the event study. Despite the lack of precision, the direction and magnitude of coefficients mostly align with previously reported results: a .4 increase in the number of counselors, a decrease in counselor ratios of about 30 (statistically insignificant), and null effects on high school graduation rates. These results hint that SSCP may have improved SAT test-taking and exit exam Math and ELA pass rates, though the latter two are again imprecisely estimated.

Results from the continuous DiD are shown in Table 13. Program effects on the number of counselors is .37 while the decline in student-to-counselor ratios is slightly lower than prior estimates at 15. Detected effects on graduation rates and SAT test taking are -.016 and .012, respectively, both of which are significant at $p < .01$. Effects on exit exam Math and ELA pass rates are near zero and statistically insignificant.

Table 12: DiD with Schools that Didn't Take Up as Control

	(1) Counselors	(2) Counselor Ratio	(3) Graduation Rate	(4) SAT	(5) Math	(6) ELA
Treat	0.717*	32.508	0.024	0.060*	-0.022	-0.022
	(0.433)	(21.893)	(0.034)	(0.035)	(0.033)	(0.027)
Post	0.185	-64.450**	-0.012	-0.020	0.010	0.001
	(0.195)	(27.069)	(0.025)	(0.020)	(0.011)	(0.010)
Interact	0.430**	-28.212	-0.009	0.037*	0.018	0.013
	(0.201)	(28.384)	(0.025)	(0.021)	(0.011)	(0.011)
Observations	3,784	3,730	3,711	3,635	3,757	3,757

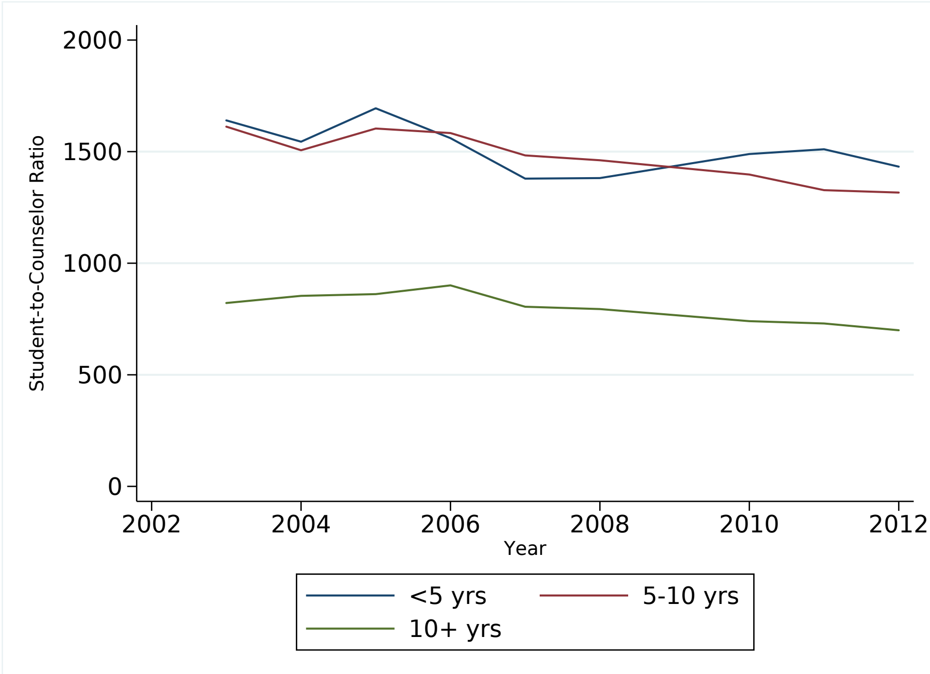
Notes: Coefficients represent estimates from OLS regressions where $Treat$ is a binary indicator for high schools in districts that ever took up SSCP funding, $Post$ is a binary indicator for years on or after 2006, and $Interact$ is an interaction between the two. Regressions are limited to years prior to 2009. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 13: Continuous DiD

	(1) Counselors	(2) Counselor Ratio	(3) Graduation Rate	(4) SAT	(5) Math	(6)) ELA
Estimated SSCP Funding	0.376***	-14.743**	-0.016***	0.012***	0.001	0.004
	(0.055)	(7.182)	(0.003)	(0.005)	(0.003)	(0.004)
Observations	5,856	5,856	6,320	5,521	5,802	5,802

Notes: Estimated SSCP Funding is scaled per $100,000. Regressions are limited to years prior to 2009. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

Figure 7: Student to Counselor Ratios, by Counselor Experience



2.7 Discussion

Taken together, results affirm that increased spending on counselors as a result of SSCP increased the number of counselors on staff by just under half a counselor, which in turn reduced student-to-counselor ratios. Impacts of the program on SAT test-taking and exit exam pass rates are modest but positive whereas estimated effects on high school graduation rates are more mixed. Estimated effects across models may mask variation in treatment effects, particularly with respect to counselor experience. We previously noted that counselors hired during the policy had less professional experience on average, and student-to-counselor ratios by counselor experience level fell most dramatically for least experienced counselors (see Figure 7).

We take several approaches to explore treatment heterogeneity by counselor experience. We include an interaction term between our treatment indicator and the number of counselors with 10 plus years of experience on staff in our event study and DiD models. Interaction term coefficients in the event study model are positive but statistically insignificant. We use the same interaction term in our simple DiD model; Appendix Table B.5 shows that schools that hired more experienced counselors improved Math and ELA pass rates by 1 percentage point more than schools that hired less experienced counselors, though we cannot rule out null effects on graduation rates and SAT test-taking.

Using a similar model from equations 4 and 5, we first instrument the change in counselor ratios at a school with the number of least experienced counselors and then the number of most experienced counselors. This helps to better understand the extent to which program effects varied based on whether counselor ratios shrank from more versus less experienced counselor hires. Results in Appendix Table B.4 suggest that reduced counselor ratios from more experienced counselor hires had a more positive effect on Math pass rates, whereas less experienced counselor hires reduced the proportion of students completing the SAT. Hiring more as opposed to less experienced counselors seems to have had a more positive effect on certain outcomes, but these effects are small and near zero for others.

Per policy guidelines, SSCP funds were required to supplement rather than supplant counselor spending. Treatment effect estimates may be biased toward zero if SSCP funds crowded out counselor spending. We regress total counselor spending on SSCP spending, including district fixed effects and limiting to program years, and find that districts spent $1.04 dollars on guidance counseling services for every $1 of SSCP money spent (see Appendix Table B.6). This suggests schools followed policy guidelines and that SSCP funds did not crowd out overall counselor spending.

Program legislation emphasized targeting students at risk of dropping out of high school, so it is encouraging that the largest and most consistent results are improved exit exam pass rates in Math and ELA. During program years, passing these exams was a requirement for high school graduation. Students were allowed multiple attempts to pass these exams, which may have enabled counselors to have a more immediate impact relative to other outcomes of interest. Despite these modest improvements in exit exam pass rates, we find no evidence of improvements in high school graduation rates. This may be a product of plateauing graduating rates, whereby high schools in the analytic sample already had graduation rates in excess of 90 percent at the time of program implementation. Observed negative effects of the program on high school graduation rates from the IV and continuous DiD models may reflect increases in graduation rates among schools with lower treatment intensity rather than decreases in graduation rates among schools with higher treatment intensity.

Several other factors may help to explain modest effects of the program on student outcomes. While most districts used SSCP funds prior to program termination, take up rates were much lower for rural areas with lower student enrollment and counselor ratios. The amount of money allotted based on enrollment may not have been enough to hire a full time counselor, and these districts may have faced additional hurdles in recruiting and hiring credentialed counselors. This underscores the importance of pairing increased funding for school counselors with policies that support and ensure adequate counselor labor supply.

Average observed treatment effects of a .5 increase in the number of school counselors, while significant, fell well short of reducing counselor ratios to the American School Counselor Association recommendation of 250 to 1. In addition, the program may simply not have been implemented for a long enough period of time. Program funding only existed for 3 years before being cut in 2009 as a result of the financial crisis. We might expect to observe larger effects on student outcomes with more sustained and consistent program funding.

With that said, our results run up against several limitations. We focus exclusively on high school outcomes even though the program provided funding for counselors serving students in all of grades 7 through 12. We might expect to observe more downstream effects from lower middle school counselor ratios, and also hope to use school climate data to develop outcome variables at the middle school level. The role of counselors is varied, and our outcomes of interest more narrowly focus on high school completion and college prep outcomes. Increases in the number of counselors on staff and decreases in student-to-counselor ratios may improve social-emotional outcomes for students and school climate more generally. There may also be spillover effects on teachers, who may take on certain counselor responsibilities in their absence. We hope to explore these questions in greater depth using recently acquired student-level data, data on mental and physical health measures, and school and teacher survey data on school climate as well as more explicitly examine equity implications of program take up and implementation.

2.8 Conclusion

This paper presents some of the first evidence on the impacts of a large scale program to expand school counseling. The additional funds that California provided to middle and high schools as part of the Supplemental School Counseling Program led to increases in the number of counselors in these schools and a reduction in the number of students assigned to each counselor. Schools in less rural areas were more likely to use SSCP funds and hire counselors. The expansion of school counseling also appears to have pulled many newer and less experienced counselors into schools, suggesting that the supply of counselors is important to consider before implementing large programs like this. The program modestly improved exit exam pass rates whereas we are unable to rule out null effects on other outcomes of interest. Counselors play an important role in supporting students' social-emotional health and academic success. As policymakers and school officials continue to grapple with mental health crises and equity gaps in college access post-Covid 19, counselors may be an underappreciated resource for students and families.

3 Dollars to Degrees: How Pell Grant Aid Impacts Community College Student Outcomes

3.1 Introduction

The system of higher education finance in the US can be characterized as a high-tuition, high-aid model. While this has not always been the case, the majority of states has reduced per student appropriations for public higher education over the past 40 years and increasingly placed the burden of tuition on students and families (Laderman & Kunkle, 2022). Tuition has outpaced inflation in all college sectors and, notably, risen by nearly 75 percent at community colleges since 1990 (Hanson, 2021). To offset rising tuition costs, students rely on a mix of federal, state, and institutional financial aid programs. Roughly 40 percent of community college students receive Pell Grants, the biggest source of federal financial aid in the US (Park & Scott-Clayton, 2018). Community colleges are perceived to be a relatively low-cost entry point into higher education higher. Yet, students who enroll in two-year institutions rely heavily on financial aid sources such as Pell Grants, and inefficiencies in transfer and persistently low graduation rates often inhibit community colleges from being a cost effective pathway to earning a bachelor's degree (Belfield et al., 2017). Limited institutional and state financial aid at community colleges further contribute to the importance of Pell Grant design and implementation (Taylor, 2021).

This paper contributes to the literature by using student transcript data to estimate effects of additional Pell Grant aid on intermediate and longer-term student outcomes for community college students. I also elaborate on the relationship between Pell and one state's grant aid program. Both of these contributions offer timely evidence around the policy efficacy of increasing the amount of Pell Grant aid available to students and designing need-based state and federal grant aid programs such that community college tuition is covered.

Using administrative data from Washington state's community college system, I exploit discontinuities in the Pell Grant formula known as Automatic Zero Estimated Family Contribution (AZ EFC) to answer the following research questions:

1. How does Pell Grant AZ EFC eligibility impact Washington state community college students' academic momentum, transfer, degree completion, and financial aid outcomes?
2. How does Pell interact with Washington state's need-based grant aid program?

Results suggest AZ eligibility boosts the total amount of grant aid dependent community college students receive, which is itself a product of an increase in both Pell and State Grants. I find no effects of AZ eligibility on contemporaneous academic outcomes (e.g. first year credits attempted and earned, persistence and GPA) and some evidence of increases in associate degree completion but not transfer or bachelor's degree completion rates. I also find evidence of an implied enrollment effect, or the effect that AZ eligibility has on initial enrollment decisions, but only weak or negligible effects on student persistence and bachelor's degree completion. These results hold important policy implications for the free college movement and other state and federal efforts to improve college affordability.

3.2 Background & Literature

Credit constraints play a critical role in preventing students from investing optimally in their education, and reductions in credit constraints through financial aid or other policies can help alleviate such constraints. Moreover, several hundred 'promise' programs across the US offer some form of place-based grant aid for free community college; these programs vary along a number of dimensions, which highlights the importance of better understanding program elements that are particularly effective as well as the interaction between such programs and federal financial aid (Perna & Leigh, 2018; Miller-Adams, 2021).

Most of the literature on Pell and other grant aid for community college students finds limited to moderate effects on enrollment and intermediate and longer-term academic outcomes. For instance, Park & Scott-Clayton (2018) estimate effects of minimum Pell Grant awards for community college students in an anonymized state; they find no detectable effects on degree completion or transfer but do find positive effects on enrollment and reductions in student labor supply. Carruthers & Welch (2019) find no effects of Pell eligibility on college enrollment or choice for high school graduates in Tennessee. Marx & Turner (2015) show that Pell Grant aid crowds out borrowing but has negligible effects on degree attainment for students in the City University of New York (CUNY) system. Denning (2017) finds positive effects of community college tuition discounts on enrollment and transfer from two- to four-year colleges, but the tuition discounts in Denning's analysis are driven by geographic factors rather than state or federal grant aid. In short, the literature on Pell and community college students finds only limited effects, and these effects seem more likely to impact students' initial enrollment decisions rather than persistence and degree completion.

Denning et al. (2019) used regression discontinuity to explore the causal effects of Pell Grant AZ EFC eligibility on college student outcomes in Texas. They exploit cutoffs in adjusted gross income (AGI) that qualify students for maximum Pell Grant awards. Using AZ EFC cutoffs, the authors find that eligibility for additional grant aid boosted first time in college (FTIC) community college student enrollment by 7 to

10 percent and improved four-year college student degree attainment within 6 years of entry by 8 percent. Importantly, Denning et al. provide a social welfare analysis that suggests grant aid more than pays for itself within 10 years as a result of improved labor market outcomes and tax payments.

Denning et al. omit community college students from their main analysis over concerns with student density at the cutoff. Evidence of heaping at the cutoff leads them to the conclusion that regression discontinuity estimates will not be unbiased; interestingly, heaping in this instance suggests evidence of an AZ eligibility effect on enrollment, which was also found in Matsudaira (2017). Eng & Matsudaira, on the other hand, find positive effects of AZ eligibility on public two-year college students' associate degree completion rate, albeit in the magnitude of 1-2 percentage points, but show no evidence of sorting or heaping at the cutoff for these students that would in turn bias point estimates.

Taken together, these papers offer somewhat conflicting evidence on the causal effects of additional grant aid on key academic and labor market outcomes and leave open the question of how other state financial aid programs interact with Pell and the extent to which marginal increases in grant aid impact community college student outcomes. Using student transcript data, I explore these questions in Washington state and, more specifically, within the community college context. Washington's State Grant program is particularly generous in comparison to other states such that public two- and four-year college tuition and fees for low-income students are either covered or heavily subsidized by the combination of Pell and State Grants.

This study offers additional insight into how Pell and State Grant aid interact to affect community college students' short- and longer-term outcomes within a state context where state and federal financial aid cover or nearly cover the cost of attending public two-year colleges for low-income students. Denning et al. (2019) and Eng & Matsudaira (2021) both suggest that enrollment and degree completion effect sizes might be larger for community college students compared to more inframarginal college-goers. This study's emphasis on community college students in Washington attempts to get at this question and better understand the effects of marginal increases in grant aid on low-income, first time in college (FTIC) students.

3.2.1 Pell

The Pell Grant formula is a decreasing function of a given student's Estimated Family Contribution (EFC). The dollar amount of Pell a student receives is the lesser of the difference between the maximum Pell award and EFC or the cost of attendance and EFC. Cost of attendance is calculated by summing tuition and fees, academic expenses such as books and supplies, and room and board; EFC is a dollar amount outlining how much a student's family can pay based on reported family adjusted gross income, family size, and cost of attendance, among other factors (Dortch, 2023). The calculation method for EFC is defined in the Higher Education Act.

Much has been written on the complexities of the Pell Grant formula and the Free Application for Federal Student Aid (FAFSA) more generally and the potential for FAFSA simplification to improve aid receipt and college attendance. Experimental evidence from Bettinger et al. (2012) found that FAFSA application support and aid estimates improved FAFSA completion rates and college attendance among low-income students. Baum & Scott-Clayton (2013) highlight the complexities involved in the Pell Grant application process as well as the lack of predictability students face in knowing exactly how much Pell Grant aid they are likely to receive for an academic year. Scott-Clayton (2017) similarly advocates for easing the transaction costs involved in applying for federal financial aid and increasing the transparency of the Pell Grant such that students can more readily discern how much aid they will receive during their first year of college and onward. The FAFSA Simplification Act attempts to address some of these issues by reducing the number of formula factors in calculating EFC, reducing the number of questions on the FAFSA, and increasing the share of responses that can be brought in from federal tax returns; the law was slated to take effect in 2023 but has been delayed to 2024 as a result of the Covid-19 pandemic (Collins & Dortch, 2022).

Students with sufficiently low adjusted gross incomes qualify for Automatic-Zero Estimated Family Contribution (AZ EFC). In 2010, the AZ eligibility threshold was raised to a family adjusted gross income (AGI) of $30,000 for dependent students and created a discontinuity in the amount of Pell Grant aid received for students who were determined to be AZ eligible. The AZ eligibility threshold changed periodically throughout the years included in this analysis: the threshold increased to $31,000 in 2012, decreased to $23,000 in AY 2013, and increased to $24,000 from 2014 through 2016 (Denning et al., 2019).

3.2.2 Conceptual Framework

The ways in which we might expect AZ eligibility to impact student outcomes can be bucketed into simplification, transparency, and realized grant aid effects. Simplification effects relate to reduced transaction costs in the FAFSA completion process, as many AZ eligible students are not required to submit certain information, such as their parents' tax payments. Transparency effects in this instance reflect the fact that students who qualify for AZ EFC are more likely to receive the maximum Pell Grant award and can more readily discern their projected award amounts. Realized grant aid effects refer to the impact of receiving more Pell Grant aid as a result of qualifying for AZ EFC. In 35 states across the country, students who are

AZ eligible face a simplified financial aid application process whereby they only need to submit family income and demographic information; the other 15 states, including Washington state, are 'no-skip' states with financial aid programs of their own that require data from all parts of the application and thus require all FAFSA applicants, AZ eligible or not, to complete the application in its non-abbreviated form.[6] Transparency effects assume students are aware of the maximum Pell award in a given year and also assumes dependent students have a relatively precise idea of their family income. Both of these assumptions seem unlikely to hold based on prior literature documenting information constraints (Scott-Clayton, 2012; Avery & Kane, 2004), and this limits the extent to which we might expect to see simplification or transparency effects from AZ eligibility on community college students in Washington. This particular state context, then, helps to highlight realized grant aid effects on student outcomes.

Washington state has a generous grant program relative to other states that students can apply for through a state or federal financial aid application.[7] The structure and generosity of state aid in this context give reason to believe that AZ eligibility for Pell could crowd in state grant aid. If this is in fact the case, we might expect the Pell Grant effect on student enrollment and academic outcomes to be larger in Washington compared to states where it crowds out other grant aid sources.

There are also reasons to expect Pell Grant effects on community college student outcomes to differ from those on four-year college students. Community colleges are already perceived as a relatively low-cost entry point into higher education. Throughout the years included in this study, the maximum Pell Grant award is commensurate with average public community college tuition in Washington. The associated increase in the likelihood of receiving a maximum Pell Grant award and average Pell Grant award more generally might increase enrollment and degree completion outcomes if marginal increases in Pell Grant aid, and potentially State Grant aid, further help to affirm the affordability of community college. On the other hand, these effects could be moderated if community college tuition and fees are already likely to be covered for most low-income students regardless of AZ eligibility status.

3.3 Data

I use administrative data collected by the State Board for Community and Technical Colleges (SBCTC) that includes student demographics, financial aid, enrollment, and course-taking. The financial aid data include

[6]Student that are not AZ eligible or reside in no-skip states must also submit information on their income and assets as well as their parents' tax payments (Matsudaira, 2017). This information is generally perceived to be a greater barrier to FAFSA completion, as it can be less readily available and more difficult to obtain in a timely manner.

[7]The state grant program referred to throughout this paper is Washington's College Grant program, formerly known as the State Need-Based Grant.

detailed financial aid award information, including award amounts and type, such as Work-Study and Pell, as well as loan amounts and types; it also includes award designations by institution, state and federal grants, and loan programs. I link state data to records from the National Student Clearinghouse (NSC), which include information on student transfer, enrollment, and degree outcomes at four-year colleges and universities.

I limit my sample to First Time in College (FTIC) students who initially enrolled in a community college in Washington state between Fall 2010 and Spring 2017 for first year and longer-term academic outcomes (e.g. completed a bachelor's degree within 6 years of entry). This enables me to explore relevant effects on longer term outcomes that include vertical transfer rates and associate and bachelor's degree completion rates. I limit the sample to degree-seeking, transfer-intending students to provide a more accurate account of treatment effect estimates on transfer and degree completion rates since these students reported aspirations to complete associate and bachelor's degrees upon matriculation at a community college as opposed to pursuing certificates or other short-term degree programs. I present fuzzy regression discontinuity results inclusive of students pursuing non-associate and bachelor's degree tracks in the appendix, though results do not differ appreciably or alter the main findings. The sample size across FTIC, transfer-intending student cohorts is roughly 220,000, including about 50,000 students with financial aid records. I focus specifically on dependent students and, following Eng & Matsudaira (2021) and Denning et al. (2019), exclude observations with reported family incomes that are multiples of $1,000 and prone to reporting bias.[8]

Student transcript data provides in depth information on students' course-taking behavior. This presents significant opportunity to better understand treatment effects of interest on course and major selection among community college students. While students at four-year colleges and universities are generally enrolled in a particular program area or major, community college students often do not officially declare a major and are instead bucketed into general or liberal arts studies. The availability of course data helps to generate proxies for student major, which can in turn be used to better understand the role of financial aid in promoting community college student program momentum as well as to study heterogeneity in treatment effects on other contemporaneous and longer-term academic outcomes. The data also allow for a more robust analysis of treatment effects of interest on course-taking that has shown to be predictive of longer-term academic outcomes for community college students (Belfield, et al., 2019).

I report on demographics for FTIC, transfer-intending community college students in Washington across seven entry cohorts in Table 14. Reported demographics vary overall compared to students who are on

[8] Doing so drops 6,010 observations, or about 10 percent of the sample with observed financial aid records. Results do not differ appreciably with or without these observations.

record as having received any financial aid as well as among students who received financial aid by AZ eligibility status. The extent to which sample sizes vary across specifications in Table 14 is indicative of relatively low FAFSA completion and/or financial aid receipt among community college students relative to their four-year counterparts (McKinney & Novak, 2015). Less than 40 percent of FTIC, transfer-intending students received any financial aid in their first year of study, which warrants further inquiry into why such a high proportion of community college students are ostensibly leaving grant aid on the table (Kofoed, 2017).

Among students who received any financial aid, 77 percent received Pell Grants, 41 percent received State Grants, and 23 percent took out federal or private student loans (see Table 15). Average first-year grant aid received by students in this sample is about $4,500; AZ eligible students on average received about $5,900 in total grant aid in their first year of enrollment. Moreover, average Pell and State Grant amounts, which are the two largest sources of financial aid for students included in this sample, are roughly commensurate with average community college tuition in Washington (see Figure 8). Conditional on financial aid eligibility, community college tuition in Washington is essentially covered by Pell and State Grants, with the latter increasing in the proportion of tuition it covers in more recent years.

3.4 Research Design

I leverage discontinuities in the Pell Grant formula known as AZ EFC to employ a fuzzy RD identification strategy. I re-center FTIC community college students' reported family income for their first year of enrollment based on their entry cohort and the corresponding cutoff for that year. For instance, a student in the 2010 entry cohort with a reported Adjusted Gross Income (AGI) of $30,000 and a student in the 2012 entry cohort with a reported AGI of $31,000 both have re-centered incomes of $0. I use entry cohort fixed effects to control for this variation, but also report on model results that are run separately for each cluster of years with the same AZ eligibility threshold. This year-over-year variation in the AZ eligibility threshold allows for further insight into treatment effect heterogeneity, especially in the later cohort years where the eligibility threshold decreases substantially.

Within this framework, I use OLS regressions to estimate first stage effects of AZ eligibility on the amount of Pell received, where equation 8 below represents the relationship between the distance in family income to AZ EFC thresholds and contemporaneous and longer-term financial aid outcomes for a student while enrolled in community college.

Table 14: Community College Student Demographics

	National	*WA*	*WA Dependent Sample*	
		All	Applied for Financial Aid	AZ Eligible
Gender				
Female	.57	.52	.53	.53
Male	.42	.48	.47	.47
Race				
Asian	.05	.13	.07	.09
Black	.14	.06	.07	.09
Hispanic	.22	.03	.05	.05
Other	.10	.20	.27	.28
Age Category				
< 20	.58	.60	.89	.84
21-24	.22	.16	.11	.16
> 24	.20	.24	.01	.01
Received Need-Based Aid	.38	.40	.86	.99
Enrolled in Remedial Coursework		.40	.52	.57
Cohort				
2010		.16	.15	.17
2011		.16	.15	.18
2012		.14	.15	.18
2013		.14	.16	.14
2014		.14	.16	.14
2015		.13	.12	.10
2016		.13	.12	.10
N	6,107,337	211,120	43,264	16,321

Notes: All refers to all transfer-intending, non-dual enrollment students across cohorts, including both FAFSA filers and non-filers. Some race categories are excluded, so percentages may not add up to 1. National statistics are based on Ma & Baum (2016), which uses NPSAS for the entering Fall 2014 cohort. For the national statistics, Received Need-Based Aid refers to students who received Pell whereas it refers to students who received any need-based financial aid in WA.

Table 15: Contemporaneous Measures of Financial Aid Receipt

	All	***Dependent***	
	All	Applied for Financial Aid	AZ Eligible
Pell Incidence	.31	.77	.98
Max Pell Incidence	.08	.19	.37
Y1 Pell Grants	$1,116	$2,801	$4,153
State Grant Incidence	.16	.41	.53
Y1 State Grants	$342	$915	$1,230
Any Grant Aid Incidence	.37	.91	.99
Y1 Total Grants	$1,724	$4,454	$5,884
Loan Incidence	.12	.23	.13
Y1 Loans	$597	$935	$448
N	217,983	48,639	18,031

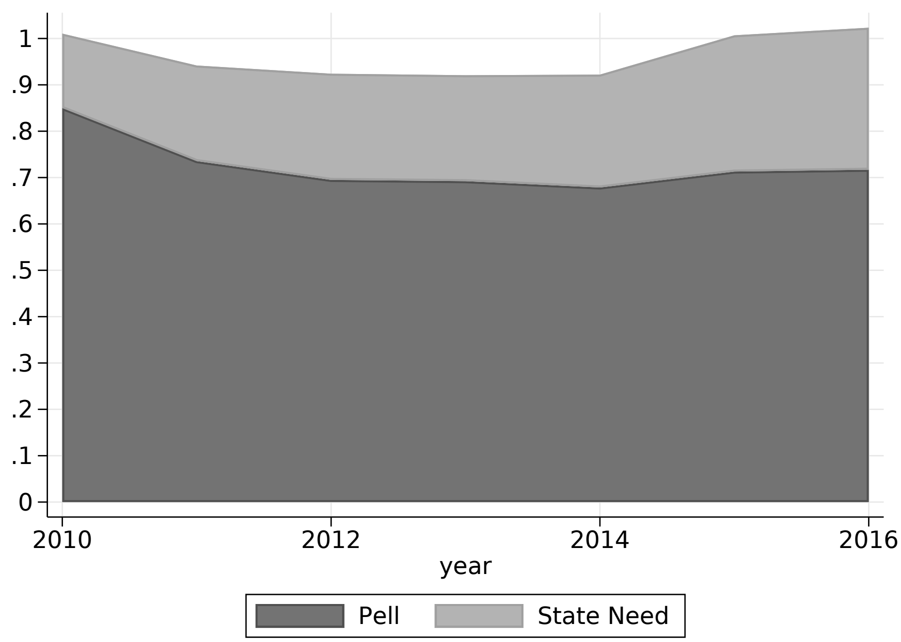
Notes: All refers to all transfer-intending, non-dual enrollment students across cohorts, including both FAFSA filers and non-filers. Aid award amounts indicate the average amount of aid received across year one of community college enrollment.

Table 16: Longer-Term Measures of Financial Aid Receipt: 2010-2014 Cohorts

	All	***Dependent***	
	All	Applied for Financial Aid	AZ Eligible
Total Pell Grant	$2,438	$5,473	$7,938
Total State Grant	$884	$1979	$2,685
Total Grant Aid	$3,890	$8,697	$11,597
Total Loans	$1,493	$2,069	$1,264
N	217,983	26,130	11,017

Notes: All refers to all transfer-intending, non-dual enrollment students across cohorts, including both FAFSA filers and non-filers. Aid award amounts indicate the average amount of aid received across all years of community college enrollment.

Figure 8: Percent of Average Community College Tuition in WA Covered by Pell and State Grant Aid



Average community college tuition for a given year is based on all public two-year colleges in Washington. The percent of tuition covered by Pell and State-Need Grants is based on the average amount of Pell and State need received by students in the sample of interest for this study. Adding together the percent for Pell and State Need indicates the percent of tuition covered by both sources of financial aid.

$$PellAmt_{ic} = \alpha + \beta_1(AZ_i) + \beta_2(Dist_i) + \beta_3(Dist_i * Below_i) + \chi_i\delta + \phi_c + \epsilon_{ic} \quad (8)$$

$PellAmt_{ic}$ is the amount of Pell Grant aid student i in cohort c receives. For the purposes of this identification strategy, it is critical to understand that AZ eligibility increases the likelihood of receiving the maximum Pell Grant award. Given where the AZ eligibility threshold is in the income distribution, essentially all students who are close to the threshold and complete a FAFSA receive some amount of Pell; it is the increased likelihood of receiving the maximum Pell award associated with AZ eligibility that drives the discontinuity in amount of Pell Grant aid received. AZ is an indicator for whether a student's re-centered income is above the AZ eligibility threshold, and β_1 is the coefficient of interest that estimates the jump in Pell for students at the threshold. $Dist_i$ is the distance from the AZ eligibility threshold for year c, $Dist_i * Below_i$ is an interaction term allowing income to vary above and below the threshold, χ is a vector of observable student characteristics, and ϕ is a set of cohort fixed effects. If regression discontinuity identification assumptions hold, these demographic covariates are not necessary for estimating unbiased effect estimates but should help with precision. Following Denning et al. (2019), I cluster standard errors at the cohort by institution level.

I run similar first-stage models with other contemporaneous financial outcomes of interest as well. This includes using the same setup as equation 1 to estimate the effect of AZ eligibility on first-year total grant aid, Pell Grant aid and total grant aid summed across all years of enrollment, and the likelihood of receiving a maximum Pell Grant award. It is worth emphasizing that the first-stage relationship between AZ eligibility and financial aid highlights the extent to which students receive marginally increased amounts of grant aid conditional on completing a FAFSA. Based on prior literature (Denning et al., 2019), there is reason to believe that AZ eligibility, outside of increases in actual grant aid, may also boost college enrollment, which is something I investigate in more detail.

The reduced form relationship between AZ eligibility and outcomes is depicted in equation 9,

$$Y_{ic} = \alpha + \beta_1(AZ_i) + \beta_2(Dist_i) + \beta_3(Dist_i * Below_i) + \chi_i\delta + \phi_c + \epsilon_{ic} \quad (9)$$

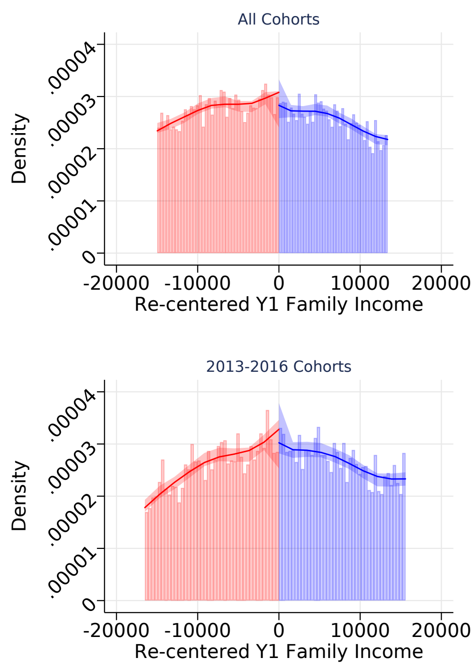
where Y_{ic} represents outcomes for student i in cohort c and the remaining variables are the same as specified above. Shorter-term academic outcomes include first year credits attempted and earned, persistence through one academic year, and performance as captured through GPA. Longer-term academic outcomes include associate degree completion within 3 years and ever, transfer to four-year colleges, and bachelor's degree completion within 6 years of initial entry. The fuzzy regression discontinuity estimates represent the ratio of reduced form and first stage results. For ease of interpretation, I report on treatment effects of AZ eligibility on grant aid scaled per $1,000 of additional total grant aid.

The central threat to identification in this RD context stems from implied enrollment effects at the AZ eligibility cutoff. Internal validity of RD results hinges on a lack of sorting at the AZ eligibility threshold, or the idea that students just above and below the AGI threshold are similar along observable and unobservable characteristics. This logic extends to student enrollment: internal validity may be threatened if students just below the AZ eligibility threshold are more likely to enroll in college than students just above the cutoff. Students might be more likely to enroll in college if AZ eligibility and its associated increase in grant aid is sufficiently salient for the marginal college-goer. We might also expect to see different implied enrollment effects over time depending on where the AZ eligibility is in the income distribution and other macroeconomic conditions (e.g. unemployment rate, minimum wage increases, etc.). I label these effects as implied rather than observed or real enrollment effects given that, to identify real enrollment effects more accurately, I would need data on all high school students and their college enrollment. Since my data is limited to students who actually enrolled in a Washington state community college, the implied enrollment effects that I capture are comparisons of student density just above and below the AZ eligibility threshold.

To capture these implied enrollment effects, I use density plots in Figure 9 to show local polynomial tests of student density above and below the cutoff by re-centered and reported first year family income (McCrary, 2008). P-values from manipulation testing are .105, .104, and .242 for cohorts 2010 and 2011, the 2012 cohort, and cohorts 2013 through 2016, respectively. As a reminder, AZ eligibility was $30,000 in 2010 and 2011, $31,000 in 2012, $23,000 in AY 2013, $24,000 for the remaining cohorts.

To explore the potential for manipulation further, I run a balance check on key demographic characteristics above and below income cutoffs using OLS regressions similar to those specified in equation 1, but using indicator variables for student demographic characteristics (e.g. race, gender, age, academic disadvantage, SES quintile, and resident status) as outcome variables. I present the results in Table 17. There are no statistically significant differences with the exception of age and resident status: students just below the cutoff are about 2 percentage points more likely to belong to the 21-24 year old age category as well as 2 percentage points more likely to be a non-resident of the district where the community college is situated. These differences disappear after limiting the sample by optimal bandwidth selections. More intuitively, manipulation seems unlikely in this context, as it assumes students are precisely aware of their family income and that AZ eligibility was salient enough to influence their enrollment decisions.

Figure 9: Density Plots Across Cohorts: Student Density by Re-Centered Family Income.



Moving clockwise, the figures depict density plots for dependent students across all initial entry cohorts, 2010-2012 entry cohorts, and 2013-2016 initial entry cohorts. P- values from manipulation testing using local polynomial density estimation are .1047, 1043, and .2420, respectively.

Table 17: Balance Check for Manipulation at the Cutoff

	No Bandwidth Restriction		***Bandwidth Restriction=$8,600***	
	Coefficient	P-Value	Coefficient	P-Value
Asian	.009	.029	.000	.914
	(.004)		(.008)	
Black	.001	.670	-.004	.638
	(.004)		(.008)	
Hispanic	.003	.323	.008	.232
	(.003)		(.007)	
White	-.004	.623	-.008	.597
	(.008)		(.015)	
Other	.008	.063	.013	.090
	(.004)		(.014)	
Female	.011	.163	-.013	.383
	(.008)		(.014)	
< 20	-.026	.000	-.006	.531
	(.005)		(.011)	
21-24	.025	.000	.005	.619
	(.005)		(.011)	
> 24	.001	.065	.001	.219
	(.000)		(.001)	
Enrolled in Remedial Coursework	.001	.822	-.005	.725
	(.008)		(.014)	
Non-District Resident	.021	.000	.004	.675
	(.005)		(.009)	
SES Quintile	-.019	.466	.019	.675
	(.026)		(.045)	

Notes: Coefficients represent point estimates for an indicator variable for AZ eligibility. I use the same OLS regression model specified in equation 1, but use indicator variables for student demographic characteristics as the outcome variable. Standard errors are in parentheses.

I use OLS to quantify AZ eligibility effects on FTIC community college student enrollment. Table 18 reports on coefficient estimates from a local linear regression of the number of students within a given bin size on an indicator for AZ eligibility, and these results are further broken down by bandwidth restriction. Bin sizes capture the range in family income included in each bin, with larger bin sizes (e.g. $200) effectively including more students per bin than smaller bin sizes (e.g. $50). Selected bandwidth restrictions are $3,000, $6,000, and $9,000; only students with family incomes falling within a selected bandwidth size on either side of the AZ eligibility threshold are included in the sample for those particular regressions. All regressions control for distance from the eligibility threshold and include an interaction term between AZ eligibility and the running income variable. Implied enrollment effects, as measured by implied percent enrollment changes across the AZ eligibility cutoff, vary from about 3 to 8 percent depending on the bin and bandwidth restrictions, which is comparable to enrollment effects uncovered in both Denning et al. (2019) and Eng & Matsudaira (2021). Again, these estimates are for implied rather than observed enrollment effects and essentially capture the difference in bin sizes just above and below the AZ eligibility cutoff among students who enrolled in college. While these sound like large effect sizes, they are small in terms of the implied number of students induced to enroll in community college as a result of AZ eligibility. These implied enrollment effects offer suggestive evidence that AZ eligibility does promote community college enrollment, but the effects are relatively small in real terms. Further in the paper, I use a bounds analysis to help understand the extent to which these implied enrollment effects might bias treatment effect estimates derived from a forthcoming fuzzy regression discontinuity analysis.

3.5 Results

Table 19 highlights regression discontinuity results of the effect of AZ eligibility on financial aid outcomes for year one and totals for all years of enrollment with baseline means and sample sizes. I post results limited by approximate median optimal bandwidth selection across outcomes of interest ($8,600) (Imbens & Kalyanaraman, 2012). In addition to demographic controls and cohort fixed effects, all regressions in Table 19 include an interaction term between AZ eligibility and AGI, which allows the slope to vary with reported income above and below the cutoff. Using optimal bandwidth restrictions, AZ eligibility is associated with a $295 increase in the amount of total grant aid students receive in their first year of enrollment in community college, including a $123 dollar increase in State Grant aid. This suggests Pell crowds in State Grant aid. Visual presentation of the effects of AZ eligibility on total grant aid and loans is shown in Figure 10, and I present results using alternative bandwidth restrictions in the appendix. These results offer some

Table 18: Estimated Effects of AZ EFC Eligibility on Student Enrollment, By Bandwidth and Bin Specifications

	Bandwidth=$3,000			*Bandwidth=$6,000*			*Bandwidth=$9,000*			
	$50	$100	$200	$50	$100	$200	$50	$100	$200	$500
FTIC Students										
AZ Eligible	2***	3***	8***	3***	6***	12***	1***	2***	5***	10***
	(.35)	(.49)	(.65)	(.26)	(.36)	(.51)	(.21)	(.29)	(.41)	(.68)
Mean	37	72	144	36	71	142	36	71	140	350
Implied Change	5.4%	4.2%	5.5%	8.3%	8.4%	8.4%	2.8%	2.8%	3.6%	2.8%

Notes: Sample includes all entry cohorts (2010-2016). AZ Eligible estimates report on coefficient estimates for a regression of the number of students within a given bin size on an indicator for AZ eligibility distance from the eligibility threshold, and an interaction term between AZ eligibility and the running income variable. This table attempts to replicate a similar model for community college students included in the appendix of Denning et al. (2019). For ease of interpretation, I round coefficients to the nearest whole number.

suggestive evidence that AZ eligibility does in fact boost the amount of grant aid dependent community college students in Washington receive over the course of enrollment, but increases are modest in size.

In Table 20 and Figure 11, I present results of AZ eligibility on shorter-term academic outcomes including the number of first-year credits attempted and earned, persistence (i.e. whether a student enrolled in multiple terms in an academic year), and students' first-year GPA. I find no statistically significant effects on any of these outcomes and present findings across bandwidth specifications in the appendix. I also run similar models but using longer-term transfer and degree completion as outcome variables of interest. As presented in Table 21 and Figure 12, results suggest that AZ eligibility boosts associate degree completion by 2 to 3 percentage points, but these coefficients are imprecisely estimated. I cannot rule out null effects on transfer to a four-year college and bachelor's degree completion.

AZ eligibility does not guarantee that students will receive more grant aid than students who do not qualify for AZ eligibility. AZ eligible students are more likely to receive the maximum Pell award and receive a higher Pell award on average, but these differences decline over time as the income threshold for AZ eligibility decreases. This first stage effect lends itself to fuzzy RD analysis, which I use to further test for treatment effects of total grant aid received on longer-term outcomes. I use AZ eligibility as an instrument for the total amount of grant aid students received while enrolled at community college and report treatment effect estimates in Table 22 for my preferred model that uses local polynomial regression and optimal bandwidth calculations as developed by Calonico et al. (2017). Treatment effect estimates represent the

Table 19: Effect of AZ EFC Eligibility on Financial Aid Outcomes, By Preferred Bandwidth Specifications

	(1) Pell Grants	(2) State Grant Aid	(3) Total Grants	(4) Total Loans
Y1 Financial Outcomes				
AZ Eligible	150*	123*	295*	22
	(71)	(58)	(126)	(62)
Mean	3,881	1,063	5,430	472
All Years				
AZ Eligible	336	301*	716	19
	(225)	(151)	386	(137)
Mean	7,364	2,592	11,026	1,223
N	6,998	6,998	6,998	6,998

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, and entry cohort. Standard errors are clustered at the college by cohort level. $8,600 is an approximation for the optimal bandwidth across reported outcome. Data for 'All Years' reports on statistics for 2010-2016 cohorts of students for the duration of their enrollment whereas Y1 reports on statistics for the first year of a student's enrollment for 2010-2014 cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 10: Financial Outcomes by Re-Centered AGI $1k Bins

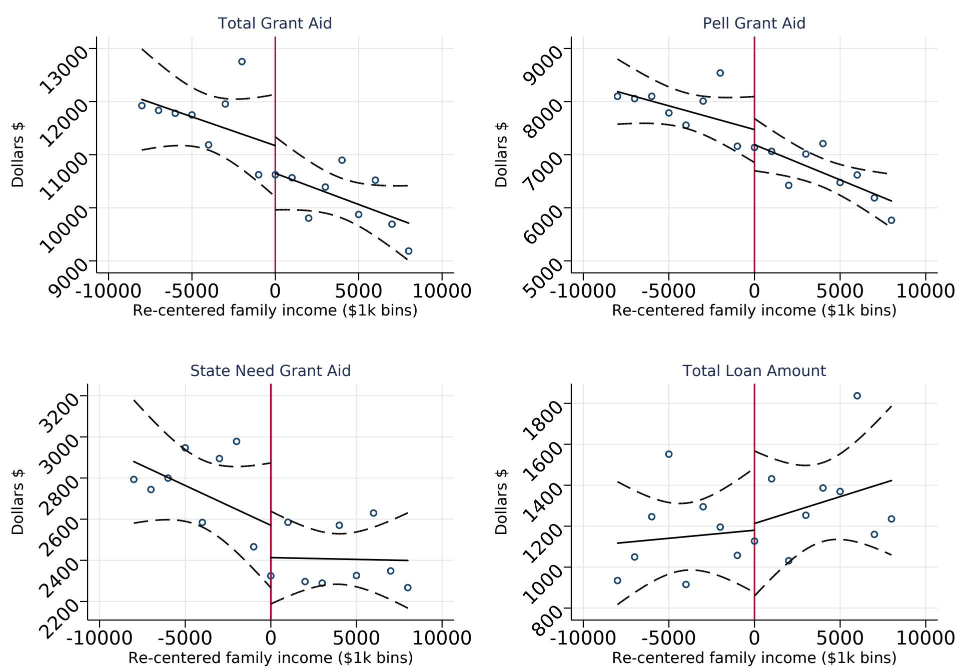


Figure 11: Short Term Academic Outcomes by Re-Centered AGI $1k Bins

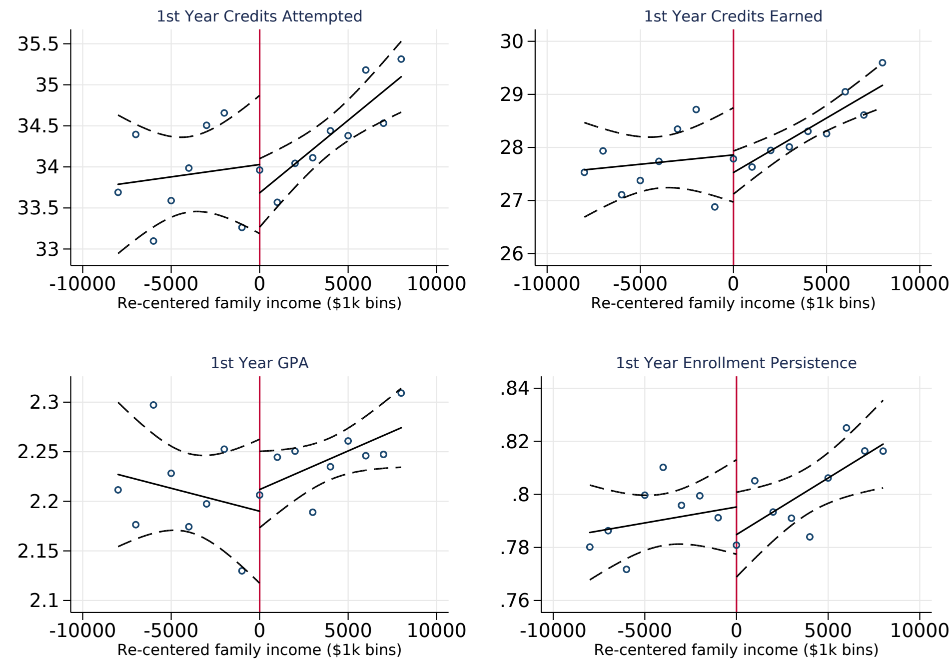


Figure 12: Short Term Academic Outcomes by Re-Centered AGI $1k Bins

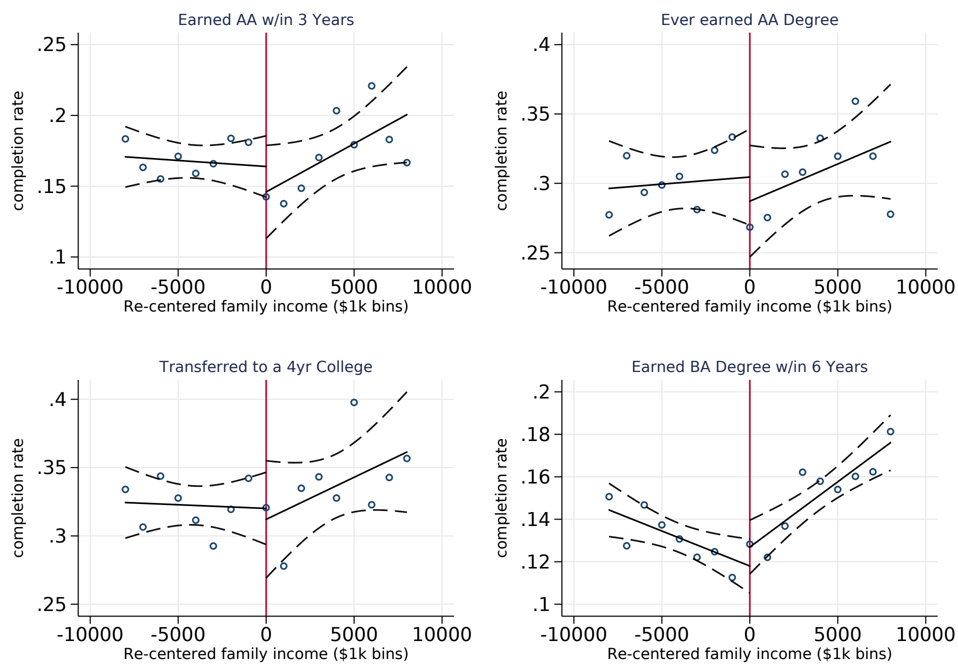


Table 20: Effect of AZ EFC on Contemporaneous Academic Outcomes, By Preferred Bandwidth Specifications

	(1) Credits Attempted	(2) Credits Earned	(3) Y1 Persistence	(4) GPA
Y1 Academic Outcomes				
AZ Eligible	.439	.646	.018	-.013
	(.848)	(.913)	(.018)	(.020)
Mean	34.39	28.25	.793	2.85
N	6,998	6,998	6,998	6,998

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level. Credits attempted refers to total attempted credits in Y1 whereas credits earned refers to credits earned in Y1. Estimates were derived using optimal bandwidth selection of 8,600, or the median optimal bandwidth across outcome variables.Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 21: Effect of AZ EFC on Longer Term Academic Outcomes, By Preferred Bandwidth Specifications

	(1) Earned AA in 3	(2) Ever Earned AA	(3) Transferred to a 4-Year College	(4) Earned BA in 6
Academic Outcomes				
AZ Eligible	.025	.029	.001	-.011
	(.019)	(.022)	(.020)	(.015)
Mean	.171	.307	.331	.141
N	6,998	6,998	6,998	6,998

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level. Preferred bandwidth specification of +/- 8,600 on either side of the cutoff. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

impact of baseline AZ EFC eligibility on associate degree completion in 3 years and overall, transfer to a four-year college, and bachelor's degree completion within 6 years of entry.

Table 22: Fuzzy RD Estimates on Effects of Grant Aid on Longer Term Academic Outcomes

	(1) Earned AA in 3	(2) Ever Earned AA	(3) Transferred to a 4-Year College	(4) Earned BA in 6
AZ Eligible	.043**	.052**	.021	-.009
	(.019)	(.022)	(.021)	(.016)
N	8,538	8,900	9,401	7,358

Notes: AZ eligible represents IV coefficient estimates, where AZ EFC eligibility is instrumented on total grant aid received by a student. Coefficient estimates were derived using local polynomial regression and optimal bandwidth selections (Calonico et al., 2017), and control for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

My preferred model uses optimal bandwidth restrictions, as outlined in Table 22, and suggests that AZ eligibility increases associate degree completion within 3 years of entry and overall by 4 to 5 percentage points (significant at $p < .05$). Treatment effect estimates of baseline AZ eligibility on bachelor's degree completion and transfer outcomes are near zero. While acknowledging the lack of precision, coefficients on bachelor's degree completion are close to zero but negative across models, suggesting that the potential positive effects on associate degree completion do not extend into further degree completion. I present estimates derived using alternative models and bandwidth specifications in the appendix. Results are not robust to model specifications, an issue I explore in more detail.

3.6 Robustness Checks and Extensions

Based on the prior literature, the lack of detected effects across outcomes beyond associate degree completion does not come as too much of a surprise. Both Eng & Matsudaira (2021) and Denning et al. (2019) found small increases in degree completion rates for community college students who were AZ eligible. The lack of positive detected effects on degree completion and transfer might be driven by selection stemming from the positive effects of AZ eligibility on enrollment. If AZ eligibility is inducing students with less academic preparation compared to less marginal college-going students, results could be biased downwards. Based on the implied enrollment effect results presented previously, this seems unlikely. First, the implied enrollment effects are very small in real student terms. Second, students induced to enroll in community college as a result of AZ eligibility are similar along all other available demographic covariates that I control for across models. The fact that my data set is limited to transfer-intending students makes the potential for downward biased treatment effect estimates even less likely. Estimating impacts without demographic controls helps to shed light on how much covariate imbalance matters in this instance; results suggest that treatment effect estimates of interest vary minimally across models with and without demographic controls (see Appendix Table C.7).

Nevertheless, to see how implied enrollment effects may bias treatment effect estimates of grant aid on longer-term academic outcomes, I conduct a bounds analysis similar to the one employed by Scott-Clayton & Zafar (2019). I previously detected an implied enrollment effect of, at most, 8 percent stemming from AZ eligibility. I flag students whose family income was just above the AZ eligibility cutoff (within $3,500 dollars) and contributed to the discontinuity in density. Among those students, I create additional flags for whether they were in the top or bottom decile of first year GPA. In this instance, I use first year GPA as a proxy for student performance to better understand how treatment effects of interest may vary depending on whether students who were induced to enroll in college as a result of AZ eligibility were high- or low-performing. Results in Table 23 show estimates after trimming students from the sample who were just above the cutoff and were in the top 10 percent of students in terms of first year GPA. In assuming that students who were induced to enroll were all high performing, effects on associate degree completion lose their statistical significance, and it appears that there could be a decline of 3 percentage points on the likelihood of bachelor's degree completion within 6 years of entry. In contrast, after trimming the sample for the bottom decile of students in terms of first year GPA, effect sizes on associate degree completion nearly double; the effect on transfer to a 4-year college increases to 5 percentage points while the effect on bachelor's degree completion remains near zero and statistically insignificant. This bounding exercise offers a wider range of potential treatment effect estimates based on whether students who contributed to the implied enrollment effect were high- or low-performing and suggests that effects of additional grant aid from AZ eligibility on associate degree completion could be as high as 10 percentage points if students induced to enroll had a greater likelihood of lower academic performance at college.

One potential reason for small to null treatment effect estimates of interest relates to heterogeneity in first-stage and fuzzy RD results over time. The main RD results presented in Table 22 control for entry cohort fixed effects, which mask any potential variation in treatment effects across entry cohort years. Since the AZ eligibility threshold varies several times throughout the years included in this analysis, we might expect to see larger first-stage and fuzzy RD treatment effects in earlier years when the eligibility threshold

Table 23: Bounding Exercise

	Baseline		***Trimmed Top Decile of Students***		***Trimmed Bottom Decile of Students***	
	Estimate	N	Estimate	N	Estimate	N
Outcome						
AA in 3	.043**	8,538	-.020	8,155	.075***	6,833
	(.019)		(.019)		(.021)	
Ever Earned AA	.052**	8,900	-.025	8,209	.106***	6,330
	(.022)		(.023)		(.027)	
BA in 6	-.009	7,358	-.033*	6,143	.008	7,795
	(.016)		(.017)		(.017)	
Transfer	.021	9,401	-.000	9,810	.051**	8,346
	(.021)		(.020)		(.023)	

Notes: The first two columns reproduce the baseline estimates reproduced from Table 7. Top and Bottom Deciles of students are base on year 1 GPA. Estimates are derived using local polynomial regression, use optimal bandwidth selection, and control for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level. N refers to the effective sample size; hence, the trimmed samples have less observations overall but may have a larger effective sample size for the local polynomial regressions.

is higher. This intuition stems from the idea that, as the AZ eligibility threshold decreases, students should be more likely to already qualify for maximum Pell Grants.

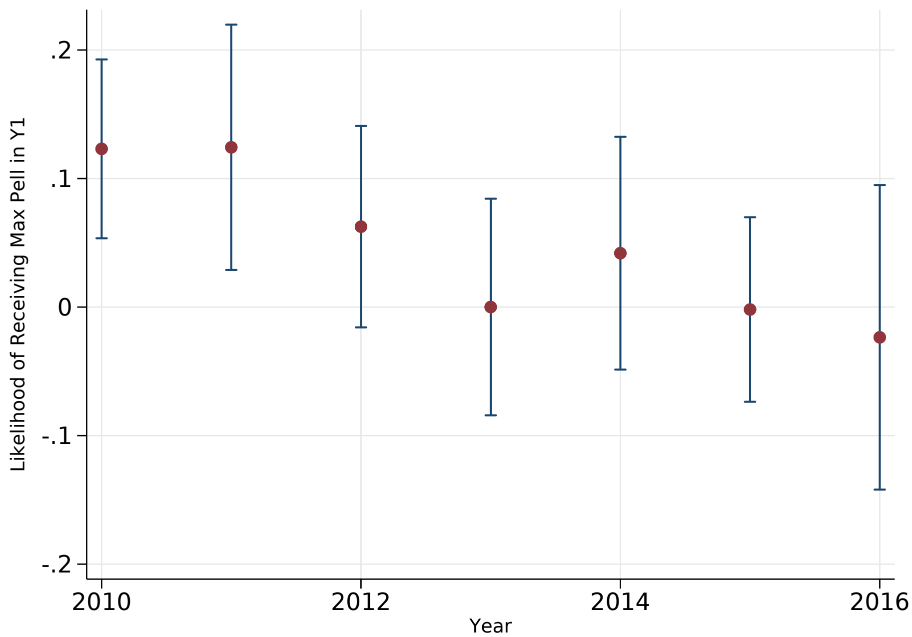
First-stage estimates of AZ eligibility on the likelihood of receiving maximum Pell Grants corroborate this hypothesis: students in later cohorts with lower AZ eligibility thresholds are already more likely to receive maximum Pell Grants. The decline in first-stage effect sizes in later entry cohorts is further evident through IV treatment effects estimated for each entry cohort (see Figure 13). I present scaled IV treatment effect estimates by year in Figure 14. While results are somewhat imprecise, they do suggest that effects vary over time, with stronger effects in earlier entry cohort years. Greater treatment intensity in these earlier entry cohorts, driven by higher likelihoods of maximum Pell Grant receipt and higher average amounts of total grant aid received, result in more positive effects on associate degree completion relative to later entry cohort years.

AZ eligibility also seems to crowd in more State Grant aid in these early years, which likely further contributes to the more positive effects on associate degree completion. Evidence presented throughout the paper suggests that Pell crowds in State Grant aid. Despite limited detected effects on longer-term academic outcomes, this is an important finding and one that shows state and federal programs working together to maximize student grant aid in this particular setting.

To test for other potential effects of AZ eligibility, I run the same regression discontinuity models but on a set of intermediate academic outcomes that have been shown to be predictive of community college student outcomes (Belfield et al., 2019; Fink et al., 2021). I find no significant effects on first-year completion of college-level Math or English, nor do I find any effects on completion of certain transfer-level coursework. I run my preferred model specification using optimal bandwidth selection on student subgroups by proxy for major as well, including students pursuing STEM academic tracks, but find limited evidence of any treatment effect heterogeneity in this regard.

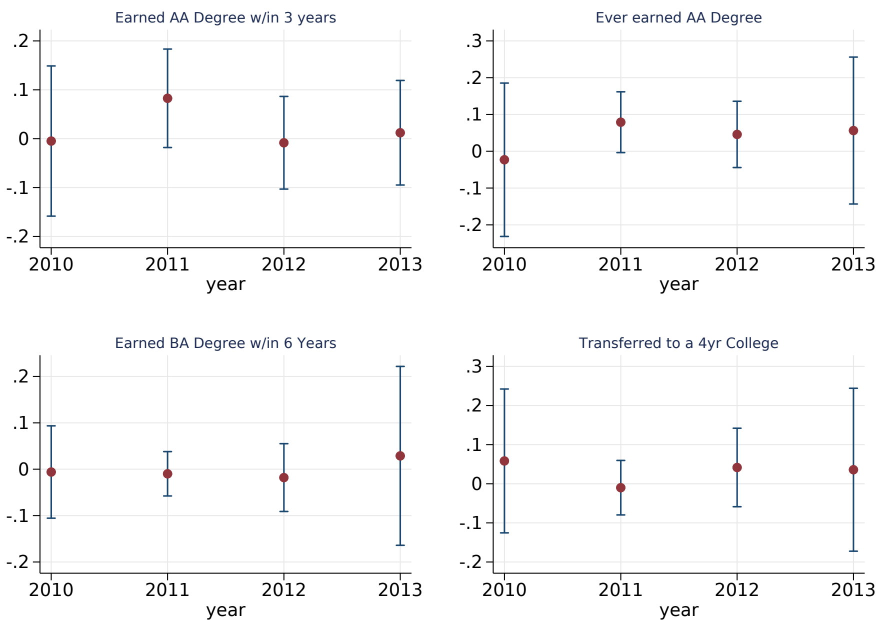
I re-run my first stage and fuzzy regression discontinuity models, limiting the sample first to historically underrepresented students of color and then to students of all transfer-intent codes, including those who did not enroll in community college with the intent of transferring and completing a bachelor's degree. In the first instance, I find no significant treatment effect estimates, and I lose precision as a result of more limited sample sizes. In the latter case, I run this model to see if AZ eligibility might impact students who may not be transfer-intending but rather pursued a vocational or terminal degree. These results are posted in the appendix (see Appendix Tables C.11, C.12 & C.13), and I again find no significant effects on contemporaneous or longer-term academic outcomes for non-transfer-intending community college students.

Figure 13: Treatment Effect Estimates on Likelihood of Receiving Maximum Pell Grant, by Entry Cohort.



Coefficient estimates for each entry cohort year are reported from regressions run on optimal bandwidth selection of $8600 and include the same controls as specified in equation 8, except the dependent variable of interest here is the likelihood of receiving the maximum Pell Grant.

Figure 14: IV Treatment Effect Estimates Over Time



Moving clockwise and starting from the top left, this figure depicts scaled IV treatment effect estimates of AZ eligibility on associate degree completion within 3 years, associate degree completion ever, bachelor's degree completion within 6 years, and transfer to a four-year college. Coefficient estimates are derived from my preferred IV model with optimal bandwidth restrictions, where AZ EFC eligibility is instrumented on total grant aid received by a student and scaled by $1000. IV regressions also control for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level.

Null to small treatment effects more generally could be the product of the relatively high purchasing power of Pell and State Grants for community college students relative to students attending 4-year public and private colleges. Estimated effects are for a very specific student group, namely transfer-intending community college students who qualify for financial aid. On average, community college students included in the main sample for this analysis received between $4,450 and $5,750 in total grant aid during their first year of enrollment, which all but covers tuition and fees at public two-year colleges in Washington. Washington's status as a no-skip state likely moderates potential simplification effects, and other AZ eligibility effects on simplifying the aid process and increasing transparency around how much aid students can expect to receive may not be as salient to students in a state context where community college is already perceived as heavily subsidized for low-income students.

The fact that lifetime grant aid increases stemming from AZ eligibility are not having a larger impact on short- and longer-term academic outcomes outside of some evidence of small, positive effects associate degree completion may be attributed to other phenomena. For instance, it could be that FAFSA completion in the first place is a greater barrier to college access and success and that students who complete the FAFSA and receive Pell are already more committed to completing their degrees. Another potential explanation for the lack of effects stems from structural barriers inhibiting community college student success. Barriers to degree completion and vertical transfer, such as lack of advising, challenges with remedial education, and incoherent transfer pathways, all limit community college students' ability to complete coursework and achieve their intended academic goals, be it vocational degree completion or transfer to a four-year college (Bailey et al., 2016). It could be that marginal increases in total grant aid are simply not enough to help students navigate and overcome the many structural barriers associated with the community college student experience.

Interpretation of these results warrants some additional caution. First, fuzzy RD models estimate local average treatment effects for students near the AZ EFC eligibility cutoff rather than for all Pell eligible students. Moreover, results should not be interpreted as proving or disproving the efficacy of the Pell Grant program more generally, nor do they speak to the effectiveness of having a higher or lower AZ EFC income threshold. Results are pooled for students who enrolled part-time or full-time, but only students enrolled full-time in a given semester are eligible for the maximum Pell Grant award. Future research might explore how term-over-term variation in full-time enrollment status impacts Pell receipt and the extent to which students are aware of the connection between enrollment status and Pell award amounts. Last, I am only able to estimate implied rather than realized enrollment effects; estimating the latter will help policymakers to more accurately assess the importance of tuition and grant aid transparency with respect to higher education enrollment.

3.7 Conclusion

Community colleges are perceived as a cost-effective pathway to earning a bachelor's degree, but community college entrants rely heavily on Pell and other grant aid and too few students who intend to earn a bachelor's degree actually do so. I exploit discontinuities in the Pell Grant formula to estimate the effect of AZ EFC eligibility on community college student enrollment, contemporaneous financial outcomes, and intermediate and longer-term academic outcomes. Using a fuzzy regression discontinuity identification strategy, I present evidence of an AZ eligibility effect for community college enrollees on associate degree completion, but I cannot rule out null effects on other academic outcomes. I also find that AZ eligibility has a small, implied enrollment effect between 3 to 8 percent among community college enrollees and use a bounding exercise to present treatment effect estimates that take into account potential bias from this source.

Though modest in size and imprecisely estimated, treatment effect estimates of 4 to 5 percentage points on associate degree completion hint that increased financial aid matters and can act as a critical resource in helping students access and succeed in college. Within the context of Washington state, Pell crowds in State Grant aid but does not seem to dramatically reduce student borrowing. This crowding in relationship between federal and state financial aid is indicative of a more robust investment in helping students to afford college. Changes to the AZ eligibility threshold over time reduce its impact on student likelihood of receiving the maximum Pell Grant award, which may moderate potential grant aid effects; extant perceptions of low-cost community college in Washington and its status as a no-skip state may further moderate potential simplification and transparency effects from AZ eligibility. It could also be that increases in total grant aid associated with AZ eligibility are not enough to offset other structural barriers within the community college system that hinder student transfer and degree completion. Results do not preclude other effects, such as on the quality of two- or four-year colleges student enroll at or on student perceptions of college cost. Per Eng & Matsudaira (2021), Pell effects are likely to vary across state and institutional contexts. Detected effects in this paper contribute to the body of literature pointing toward modest effects of Pell Grant aid on transfer and degree completion outcomes in Washington, and they also highlight the continued need to pair increases in financial aid and/or reductions in tuition through increased public investment in community colleges with more structural reforms to improve the community college student transfer experience.

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5 Appendix A

Table A.1: Estimated Effects of Lifetime Pell on Student Outcomes Using Alternative Treatment Indicators

	Treatment=4 Years of Pell			*Treatment=6 Years of Pell*		
	(1)	(2)	(3)	(1)	(2)	(3)
Pell	-458***	-872***	-620***	-486***	-800***	-603***
	(6)	(9)	(10)	(14)	(16)	(15)
Total Grant Aid	-486***	-930***	-673***	-534***	-896***	-709***
	(7)	(10)	(12)	(15)	(18)	(18)
Loans	-26***	-61***	116***	7	25	175***
	(4)	(7)	(11)	(11)	(17)	(20)
Quarterly earnings	893***	816***	362***	645***	580***	218***
	(15)	(21)	(24)	(31)	(42)	(43)
Full-Time Enrollment	-.115***	-.242***	-.129***	-.093***	-.158***	-.078***
	(.002)	(.003)	(.004)	(.005)	(.007)	(.008)
Part-Time Enrollment	-.054***	-.029***	.129***	-.047***	-.035***	.078***
	(.002)	(.003)	(.004)	(.005)	(.007)	(.008)
Any Enrollment	-.170***	-.272***		-.141***	-.193***	
	(.002)	(.003)		(.005)	(.005)	
Credits Attempted	-1.66***	-3.16***	-1.00***	-1.29***	-1.92***	-.295***
	(.030)	(.041)	(.070)	(.081)	(.075)	
Credits Earned	-1.59***	-3.01***	-1.29***	-1.32***	-1.94***	-.574***
	(.029)	(.040)	(.044)	(.067)	(.080)	(.079)
Cumulative GPA	-.183***	-.180***	-.144***	-.122***	-.122***	-.086***
	(.002)	(.004)	(.005)	(.004)	(.006)	(.007)
Excludes Summer & Winter Terms	No	Yes	Yes	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes	No	No	Yes
N	14,400	14,400	14,400	4,695	4,695	4,695

Notes: Coefficients represent the interaction term from equation 1. All regressions include student and year fixed effects. Student observations are limited to 40 terms. $***p < 0.001, **p < 0.01, *p < 0.05$.

Table A.2: Estimated Effects of Lifetime Pell on Student Outcomes For Student Subgroups

	Community College Entrants			***Black, Hispanic, AI***			> 25 ***At Entry***		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Pell	-457***	-798***	-569***	-470***	-762***	-524***	-385***	-691***	-516***
	(10)	(13)	(13)	(12)	(14)	(14)	(20)	(27)	(29)
Total Grant Aid	-494***	-876***	-651***	-506***	-834***	-608***	-409***	-741***	-553***
	(11)	(15)	(16)	(13)	(17)	(18)	(22)	(31)	(35)
Loans	-26***	-45***	116***	-11	-17	126***	-154***	-275***	30
	(6)	(13)	(16)	(8)	(13)	(17)	(26)	(41)	(52)
Quarterly earnings	804***	759***	366***	842***	805***	407***	1,104***	1,026***	410***
	(25)	(34)	(35)	(27)	(37)	(39)	(67)	(92)	(103)
Full-Time Enrollment	-.102***	-.195***	-.107***	-.115***	-.191***	-.096***	-.064***	-.124***	-.077***
	(.004)	(.005)	(.006)	(.004)	(.006)	(.007)	(.007)	(.011)	(.014)
Part-Time Enrollment	-.057***	-.027***	.107***	-.045***	-.024***	.096***	-.089***	-.112***	.077***
	(.003)	(.005)	(.006)	(.004)	(.005)	(.007)	(.009)	(.013)	(.014)
Any Enrollment	-.160***	-.223***		-.161***	-.215***		-.154***	-2.37***	
	(.004)	(.004)		(.005)	(.005)		(.010)	(.011)	
Credits Attempted	-1.43***	-2.36***	-.512***	-1.61***	-2.38***	-.591***	-1.03***	-1.79***	-.047
	(.051)	(.063)	(.060)	(.058)	(.070)	(.065)	(.098)	(.130)	(.132)
Credits Earned	-1.37***	-2.26***	-.699***	-1.58***	-2.31***	-.849***	-.989***	-1.69***	-.068
	(.049)	(.062)	(.062)	(.056)	(.069)	(.069)	(.095)	(.128)	(.135)
Cumulative GPA	-.127***	-.128***	-.082***	-.151***	-.150***	-.112***	-.062***	-.063***	-.028*
	(.004)	(.005)	(.006)	(.004)	(.006)	(.007)	(.007)	(.010)	(.013)
Excludes Summer & Winter Terms	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Limited to Terms Students Enrolled in	No	No	Yes	No	No	Yes	No	No	Yes
N	14,400	14,400	14,400	4,695	4,695	4,695			

Notes: Coefficients represent the interaction term from equation 1. All regressions include student and year fixed effects. Student observations are limited to 40 terms. N refers to unique student observations. $***p < 0.001, **p < 0.01, *p < 0.05$.

6 Appendix B

Table B.1: High School Student Demographics

Year	Enrollment (millions)	Avg. School Enrollment	Female	Asian	Black	Hispanic	White	FRPM
2003	1.54	1313	0.47	0.07	0.08	0.37	0.42	.
2004	1.61	1313	0.48	0.07	0.08	0.38	0.41	0.35
2005	1.65	1309	0.48	0.07	0.08	0.39	0.39	0.35
2006	1.68	1292	0.48	0.07	0.08	0.40	0.38	0.39
2007	1.70	1280	0.48	0.07	0.07	0.41	0.37	0.43
2008	1.71	1254	0.48	0.07	0.08	0.42	0.35	0.45
2009	1.71	1244	0.48	0.07	0.07	0.44	0.34	0.49
2010	1.72	1226	0.48	0.07	0.07	0.46	0.33	0.49
2011	1.72	1212	0.48	0.07	0.07	0.47	0.32	0.39
2012	1.73	1189	0.47	0.07	0.07	0.48	0.31	0.55

Table B.2: High School Student Outcomes

	Graduation Rate	SAT	Math	ELA
2003	0.90	0.29	0.68	0.66
2004	0.90	0.30	0.69	0.66
2005	0.88	0.31	0.69	0.68
2006	0.88	0.31	0.69	0.68
2007	0.88	0.38	0.71	0.69
2008	0.86	0.36	0.71	0.70
2009	0.88	0.38	0.72	0.72
2010	0.85	0.40	.	.
2011	0.86	0.43	0.73	0.72
2012	0.89	.	0.73	0.73

Table B.3: High School Characteristics, by SSCP Takeup

	(1) Used SSCP Funds	(2) Never Used SSCP Funds	(3) Difference
Student Characteristics			
FRPM	0.41	0.44	.03
Female	0.48	0.48	.01**
Asian	0.06	0.03	-.03***
Hispanic	0.40	0.34	-.06***
Black	0.06	0.05	-.01
White	0.41	0.49	.08***
Rural	0.36	0.55	.19***
Urban	0.26	0.23	-.04
Outcomes			
HS graduation rate	0.89	0.86	-.03***
SAT	0.35	0.29	-.05***
HSEE Math	0.70	0.71	-.01
HSEE ELA	0.70	0.73	-.02
Counselor Characteristics			
Counselor Ratio	453	378	-76***
Counselors	2.34	1.20	-1.14***
<5 Years Experience	.55	.27	-.28***
5010 Years Experience	.51	.24	-.27***
10+ Years Expereince	1.28	.70	-.58***

Table B.4: IV Results: Counselor Experience

	Least Experienced Counselor				*Most Experienced Counselor*			
	(1) Graduation Rate	(2) SAT	(3) ELA	(4) Math	(1) Graduation Rate	(2) SAT	(3) ELA	(4) Math
Change Ratio	0.001	0.007**	-0.002	0.001	-0.002	-0.001	-0.002	-0.003**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
Observations	3,675	3,608	3,704	3,704	3,675	3,608	3,704	3,704
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients represent estimates from two-stage least squares regressions that include school and year fixed. The change in student-to-counselor ratios is instrumented with the number of least and most experienced counselors employed at a high school. Change Ratio is scaled per 100 students. All regressions include controls for the number of counselors at a given high school in 2005, the year prior to program implementation. Regressions are limited to years prior to 2009 and exclude schools with simulated SSCP spending in excess of $1,000,000. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

Table B.5: DiD with Schools that Didn't Take Up as Control, with Counselor Experience Interaction

	(1) Graduation Rate	(2) SAT	(3) Math	(4) ELA
Treat X Experience	-0.001	0.004	0.011***	0.010***
	(0.002)	(0.004)	(0.003)	(0.003)
Observations	3,711	3,635	3,757	3,757

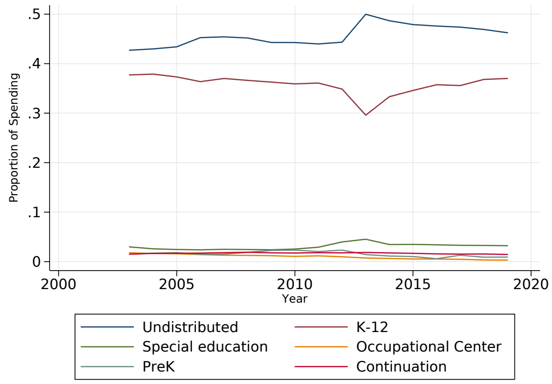
Notes: Coefficients represent estimates from the same OLS regressions in table 12 but include an interaction between indicators for SSCP treatment and the number of most experienced counselors. Regressions are limited to years prior to 2009. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

Table B.6: Effects of SSCP Funding on Guidance Counselor Spending

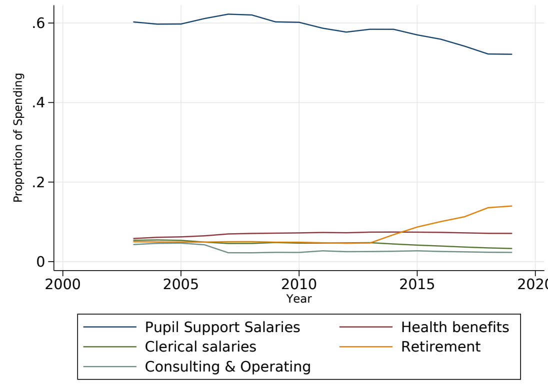
	(1) GC Spending	(2) GC Spending	(3) GC Spending
SSCP Spending	0.759***	0.760***	1.042***
	(0.023)	(0.023)	(0.020)
Observations	1,887	1,861	1,703
Limited to Program Years	Yes	Yes	Yes
Limited to Ever Treated	No	Yes	No
Limited to Treated	No	No	Yes

Notes: Coefficients represent estimates from OLS regressions of observed SSCP spending on total counselor spending. Regressions are limited to program years (2006-2008) and include district fixed effects. Limitations on Ever Treated refer to whether a district received SSCP funds in any of the 3 program years whereas limitations on treated refer to whether a district received SSCP funds in a given year. Robust standard errors in parentheses $***p < 0.01, **p < 0.05, *p < 0.10$.

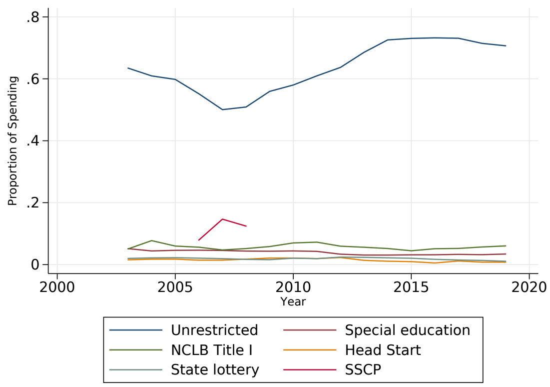
Figure B.1: Trends in Guidance Counselor Spending



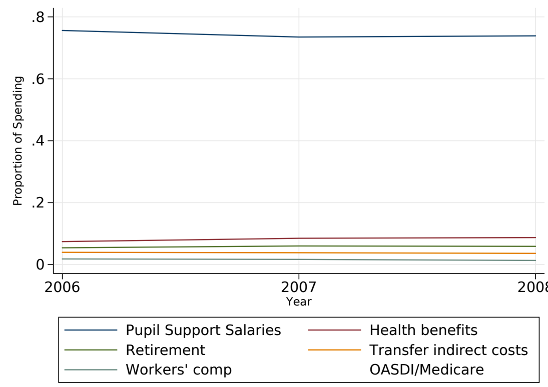
(a) GC Spending by Goal



(b) GC Spending by Object

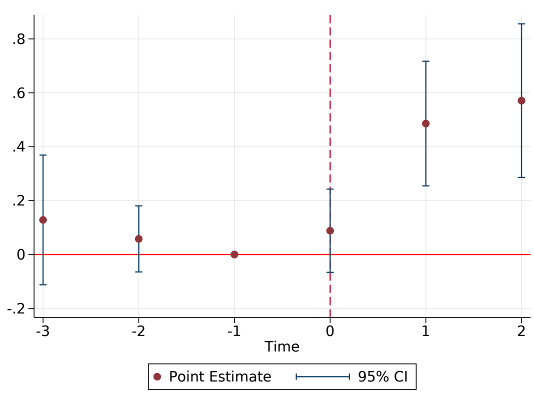


(c) SSCP Spending by Goal

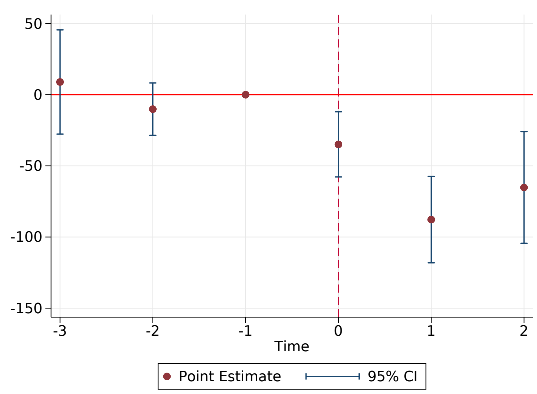


(d) SSCP Spending by Goal

Figure B.2: Event Study Results Inclusive of Middle and High Schools

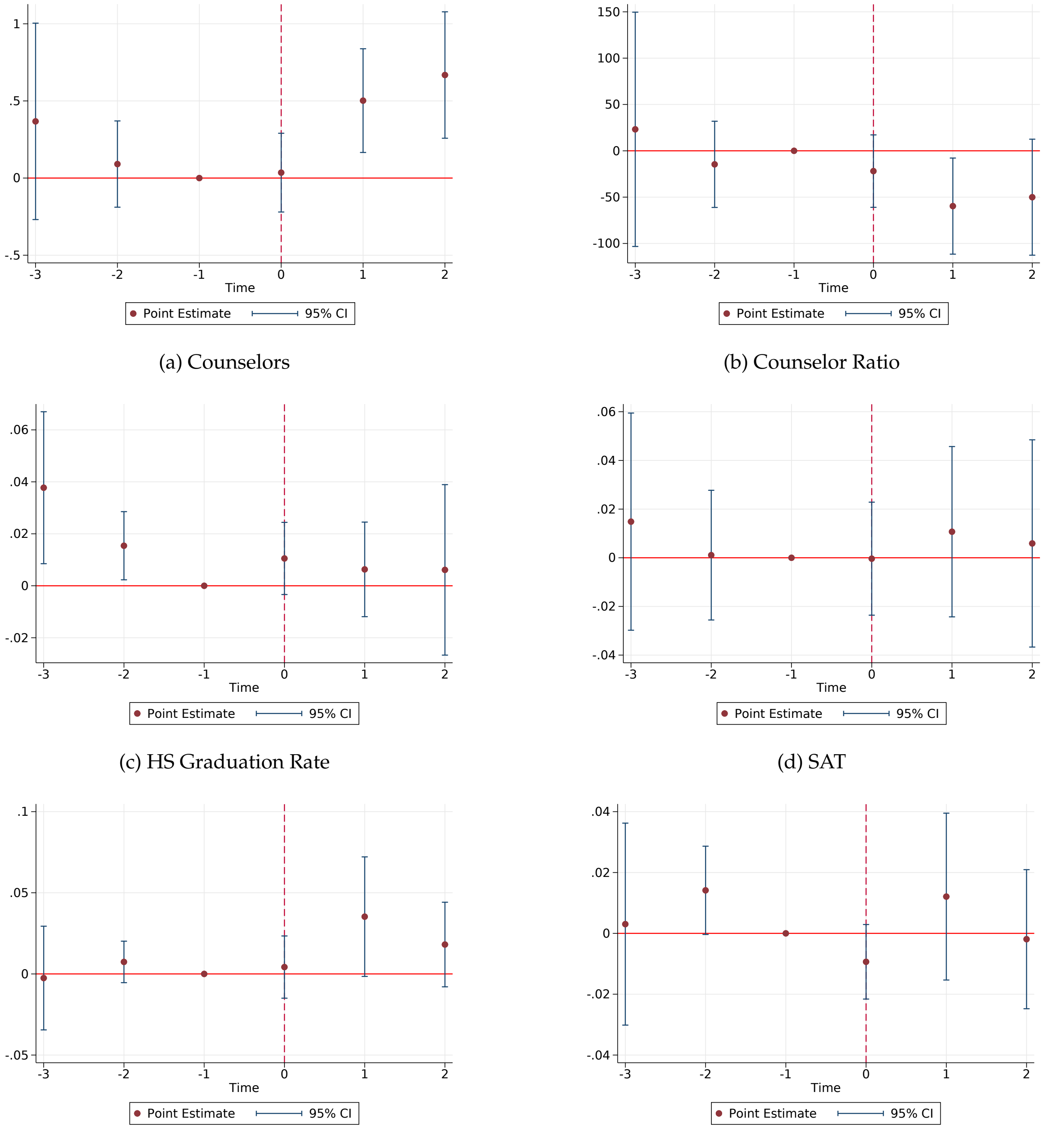


(a) Counselors



(b) Counselor Ratio

Figure B.3: Event Study Results with Demographic Controls



(a) Counselors

(b) Counselor Ratio

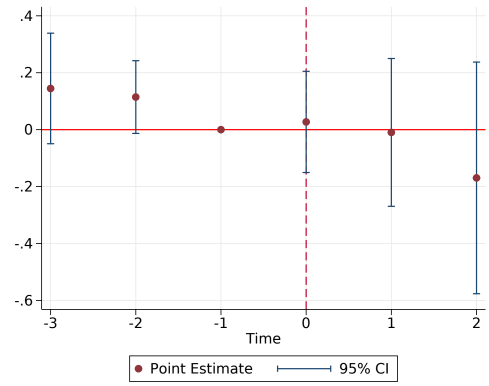
(c) HS Graduation Rate

(d) SAT

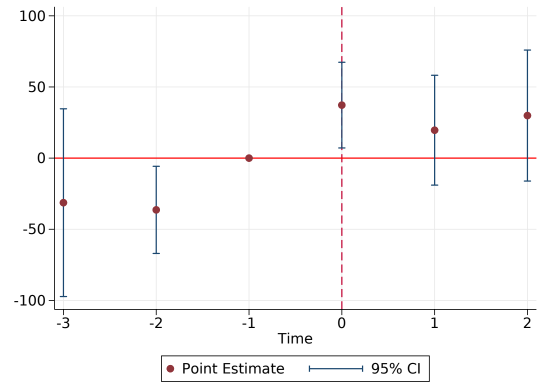
(e) ELA

(f) Math

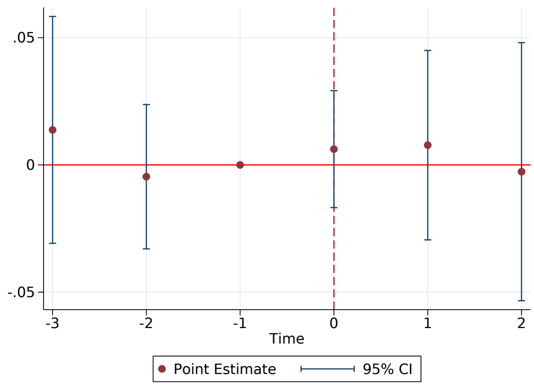
Figure B.4: Event Study Results for No Counselor Schools



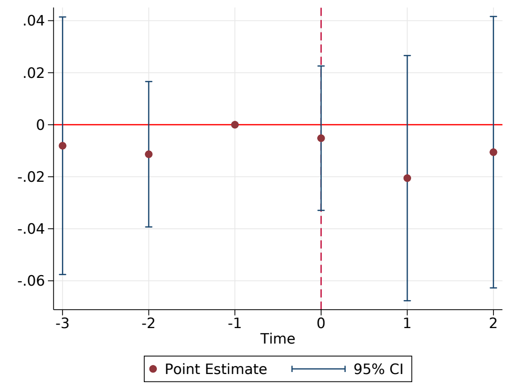
(a) Counselors



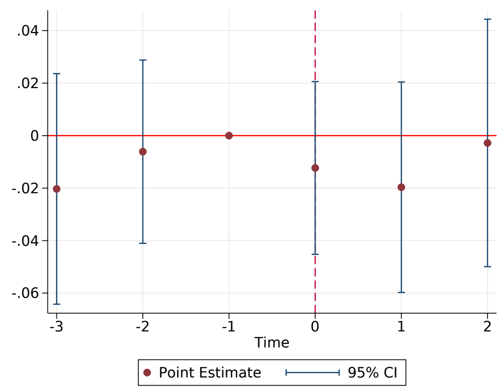
(b) District Counselor Ratio



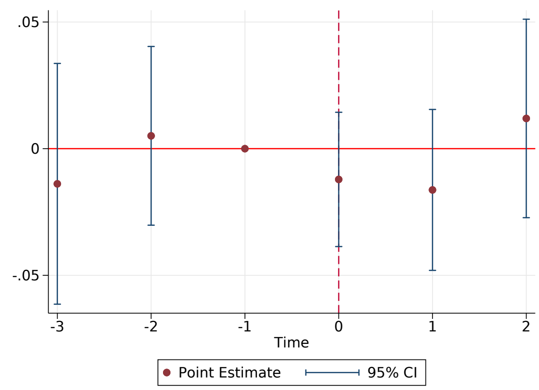
(c) HS Graduation Rate



(d) SAT

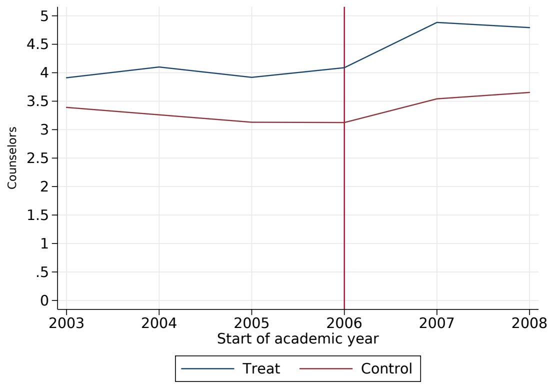


(e) ELA

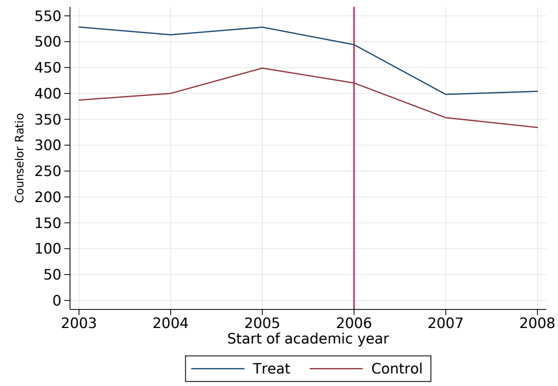


(f) Math

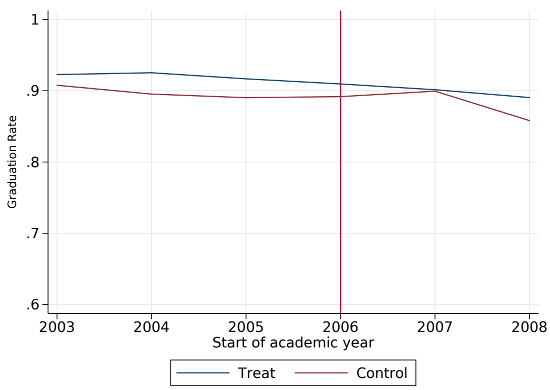
Figure B.5: Parallel Trends Test for Difference-in-Difference



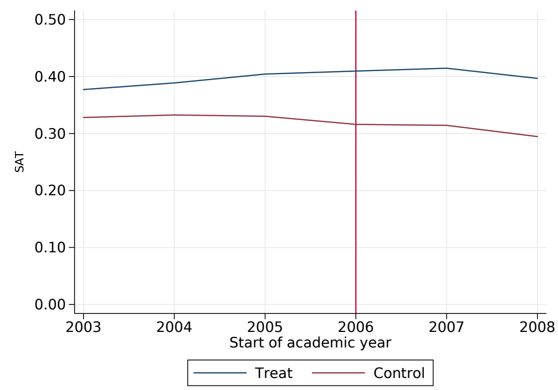
(a) Counselors



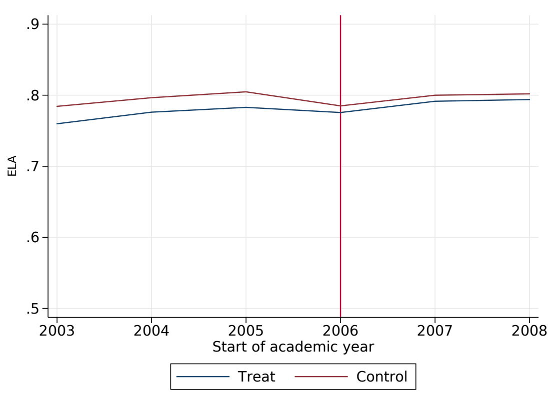
(b) Counselor Ratio



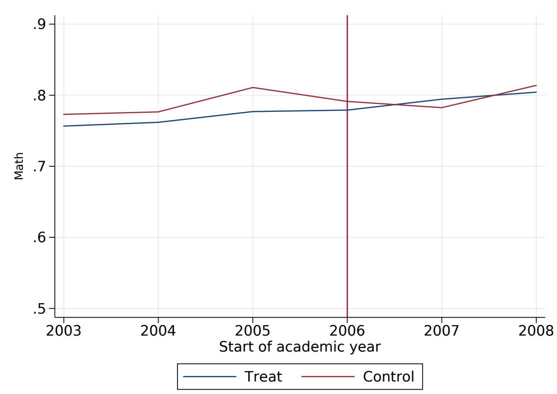
(c) HS Grad Rate



(d) SAT



(e) ELA



(f) Math

7 Appendix C

Table C.1: Effect of AZ EFC Eligibility on Pell and State Grant Aid, By Alternative Bandwidth Specifications

	Pell Grants			*State Grant Aid*		
Bandwidth Restriction	$4,300	$25,800	None	$4,300	$25,800	None
Y1 Financial Outcomes						
AZ Eligible	74	17	760***	41	-40	98***
	(90)	(45)	(43)	(75)	(32)	(25)
Mean	4,054	3,660	2,801	1,273	1,175	915
Observations	5,664	28,151	41,013	5,664	28,151	41,013
All Years						
AZ Eligible	434	427**	1,855***	232	138	425***
	(353)	(145)	(139)	(189)	(98)	(81)
Mean	7,377	6,822	5,473	2,549	2,434	1,979
Observations	3,450	17,588	24,907	3,450	17,558	24,907

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, and entry cohort. Standard errors are clustered at the college by cohort level. Data for 'All Years' reports on statistics for 2010-2014 cohorts of students for the duration of their enrollment whereas Y1 reports on statistics for the first year of a student's enrollment for 2010-2016 cohorts.

Table C.2: Effect of AZ EFC Eligibility on Total Grants and Loans, By Alternative Bandwidth Specifications

	Total Grants			*Total Loans*		
Bandwidth Restriction	$4,300	$25,800	None	$4,300	$25,800	None
Y1 Financial Outcomes						
AZ Eligible	153	-10	792***	-77	34	81**
	(161)	(72)	(61)	(71)	(31)	(28)
Mean	5,924	5,429	4,454	416	500	935
Observations	5,664	28,151	41,013	5,664	28,151	41,013
All Years						
AZ Eligible	759	577*	2,108***	-194	98	39
	(546)	(249)	(228)	(200)	(90)	(86)
Mean	11,008	10,365	8,697	1,176	1,376	2,069
Observations	3,450	17,588	24,907	3,450	17,558	24,907

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, and entry cohort. Standard errors are clustered at the college by cohort level. Data for 'All Years' reports on statistics for 2010-2014 cohorts of students for the duration of their enrollment whereas Y1 reports on statistics for the first year of a student's enrollment for 2010-2016 cohorts.

Table C.3: Effect of AZ EFC Eligibility on Credits Attempted and Earned, By Alternative Bandwidth Specifications

	Credits Attempted			*Credits Earned*		
Bandwidth Restriction	$4,300	$25,800	None	$4,300	$25,800	None
Y1 Academic Outcomes						
AZ Eligible	.478	.013	.396	.357	-.031	.024
	(.929)	(.404)	(.324)	(.909)	(.427)	(.349)
Mean	33.96	33.98	35.00	27.83	27.92	29.16
Observations	5,664	28,151	41,013	5,664	28,151	41,013

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level. Credits attempted refers to total attempted credits in Y1 whereas credits earned refers to credits earned in Y1.

Table C.4: Effect of AZ EFC Eligibility on Persistence and GPA, By Alternative Bandwidth Specifications

	Persistence			*GPA*		
Bandwidth Restriction	$4,300	$25,800	None	$4,300	$25,800	None
Y1 Academic Outcomes						
AZ Eligible	.004	.001	.003	-.044	-.021	.043*
	(.019)	(.009)	(.007)	(.057)	(.027)	(.020)
Mean	.795	.790	.807	2.21	2.23	2.29
Observations	5,664	28,151	41,013	5,664	28,151	41,013

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level. Persistence refers to term-over-term enrollment in Y1, and GPA is an indicator for a 3.0 or higher in Y1. Bandwidth restrictions indicate the range on either side of the cutoff.

Table C.5: Effect of AZ EFC Eligibility on Associate Degree Outcomes, By Alternative Bandwidth Specifications

	Earned AA in 3			*Ever Earned AA*		
Bandwidth Restriction	$4,300	$25,800	None	$4,300	$25,800	None
Degree Outcomes						
AZ Eligible	.044	-.002	-.004	.043	-.009	-.006
	(.027)	(.011)	(.010)	(.032)	(.012)	(.010)
Mean	.161	.170	.192	.301	.301	.319
Observations	3,450	17,558	24,907	3,450	17,558	24,907

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level.

Table C.6: Effect of AZ EFC Eligibility on Longer Term Outcomes, By Alternative Bandwidth Specifications

	Transferred to 4-Year College			***Earned BA in 6***		
Bandwidth Restriction	$4,300	$25,800	None	$4,300	$25,800	None
Academic Outcomes						
AZ Eligible	.031	-.011	-.020	-.007	-.021*	-.017*
	(.028)	(.012)	(.010)	(.023)	(.009)	(.008)
Mean	.316	.332	.358	.128	.143	.162
Observations	3,450	17,558	24,907	3,450	17,558	24,907

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level.

Table C.7: Fuzzy RD Estimates on Effects of Grant Aid on Longer Term Academic Outcomes without Demographic Controls

	(1) Earned AA in 3	(2) Ever Earned AA	(3) Transferred to a 4-Year College	(4) Earned BA in 6
AZ Eligible	.039*	.052**	.017	-.007
	(.022)	(.023)	(.021)	(.017)
Observations	7,335	7,335	7,335	7,335

Notes: Reported fuzzy RD estimates are for local polynomial regressions that use optimal bandwidth selection (Calonico et al., 2017) and omit demographic controls.

Table C.8: Effects of AZ EFC Eligibility on Non-Transfer Intending Student Enrollment, By Bandwidth and Bin Specifications

	Bandwidth=$3,000			***Bandwidth=$6,000***			***Bandwidth=$9,000***			
	$50	$100	$200	$50	$100	$200	$50	$100	$200	$500
Student Enrollment										
AZ Eligible	1***	3***	7***	2***	3***	7***	1***	2***	4***	8***
	(.33)	(.49)	(.69)	(.24)	(.33)	(.48)	(.19)	(.28)	(.43)	(.80)
Mean	22	42	83	22	42	83	21	41	81	202
Implied Change	4.5%	7.1%	8.4%	9.1%	7.1%	8.4%	4.7%	4.8%	4.9%	3.9%

Notes: Sample includes all entry cohorts (2010-2016). AZ Eligible estimates report on coefficient estimates for a regression of the number of students within a given bin size on an indicator for AZ eligibility distance from the eligibility threshold, and an interaction term between AZ eligibility and the running income variable. This table attempts to replicate a similar model for community college students included in the appendix of Denning et al. (2019). For ease of interpretation, I round coefficients to the nearest whole number.

Table C.9: Effect of AZ EFC Eligibility on Pell and State Grant Aid Among Non-Transfer Intending Students, By Alternative Bandwidth Specifications

	Pell Grants				***State Grant Aid***			
Bandwidth Restriction	$4,300	$8,600	$25,800	None	$4,300	$8,600	$25,800	None
Y1 Financial Outcomes								
AZ Eligible	114	-51	13	808***	112	29	19	128***
	(143)	(102)	(61)	(48)	(96)	(71)	(43)	(34)
Mean	3,883	3,798	3,560	2,783	1,235	1,190	1,109	882
Observations	3,343	6,464	16,104	22,837	3,342	6,464	16,104	22,837
All Years								
AZ Eligible	-533	-133	220	1,803***	6	-2	83	403***
	(444)	(328)	(214)	(139)	(263)	(200)	(127)	(85)
Mean	3,825	3,772	3,546	2,857	1,033	1,017	966	786
Observations	2,205	4,279	10,801	14,986	2,205	4,279	10,801	14,986

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, and entry cohort. Standard errors are clustered at the college by cohort level. Data for 'All Years' reports on statistics for 2010-2014 cohorts of students for the duration of their enrollment whereas Y1 reports on statistics for the first year of a student's enrollment for 2010-2016 cohorts.

Table C.10: Effect of AZ EFC Eligibility on Total Grants and Loans Among Non-Transfer Intending Students, By Alternative Bandwidth Specifications

	Total Grants				*Total Loans*			
Bandwidth Restriction	$4,300	$8,600	$25,800	None	$4,300	$8,600	$25,800	None
Y1 Financial Outcomes								
AZ Eligible	317	-15	43	863***	-136	-59	-6	27
	(241)	(170)	(102)	(77)	(85)	(71)	(48)	(44)
Mean	5,718	5,614	5,298	4,366	528	548	670	1,172
Observations	3,343	6,464	16,104	22,837	3,342	6,464	16,104	22,837
All Years								
AZ Eligible	-458	-366	315	2,082***	65	153	90	-40
	(756)	(572)	(377)	(236)	(336)	(269)	(166)	(129)
Mean	5,367	5,305	5,030	4,222	593	622	752	1,228
Observations	2,205	4,279	10,801	14,986	2,205	4,279	10,801	14,986

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, and entry cohort. Standard errors are clustered at the college by cohort level. Data for 'All Years' reports on statistics for 2010-2014 cohorts of students for the duration of their enrollment whereas Y1 reports on statistics for the first year of a student's enrollment for 2010-2016 cohorts.

Table C.11: Effect of AZ EFC Eligibility on Credits Attempted and Earned Among Non-Transfer Intending Students, By Alternative Bandwidth Specifications

	Credits Attempted				*Credits Earned*			
Bandwidth Restriction	$4,300	$8,600	$25,800	None	$4,300	$8,600	$25,800	None
Y1 Academic Outcomes								
AZ Eligible	.549	-.975	-.657	-.346	-.220	-1.62	-.835	-.744
	(1.30)	(.877)	(.575)	(.431)	(1.31)	(.94)	(.619)	(.459)
Mean	33.69	33.21	33.14	33.81	28.13	27.61	27.65	28.51
Observations	3,342	6,464	16,104	22,837	3,342	6,464	16,104	22,837

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level.

Table C.12: Effect of AZ EFC Eligibility on Persistence and GPA Among Non-Transfer Intending Students, By Alternative Bandwidth Specifications

	Persistence				*GPA*			
Bandwidth Restriction	$4,300	$8,600	$25,800	None	$4,300	$8,600	$25,800	None
Y1 Academic Outcomes								
AZ Eligible	-.019	-.034	-.019	-.020*	-.070	-.119*	-.023	-.060**
	(.027)	(.020)	(.013)	(.010)	(.075)	(.059)	(.035)	(.026)
Mean	.777	.768	.767	.779	2.26	2.26	2.28	2.33
Observations	3,342	6,464	16,104	22,837	3,342	6,464	16,104	22,837

Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level.

Table C.13: Effect of AZ EFC Eligibility on Degree Outcomes Among Non-Transfer Intending Students, By Alternative Bandwidth Specifications

	Earned AA in 3				*Ever Earned AA*			
Bandwidth Restriction	$4,300	$8,600	$25,800	None	$4,300	$8,600	$25,800	None
Academic Outcomes								
AZ Eligible	.007	-.038	.009	-.003	-.028	-.043	.001	-.002
	(.029)	(.021)	(.013)	(.008)	(.036)	(.025)	(.017)	(.009)
Mean	.137	.134	.141	.156	.265	.262	.262	.277
Observations	2,205	4,279	10,801	14,986	2,205	4,279	10,801	14,986

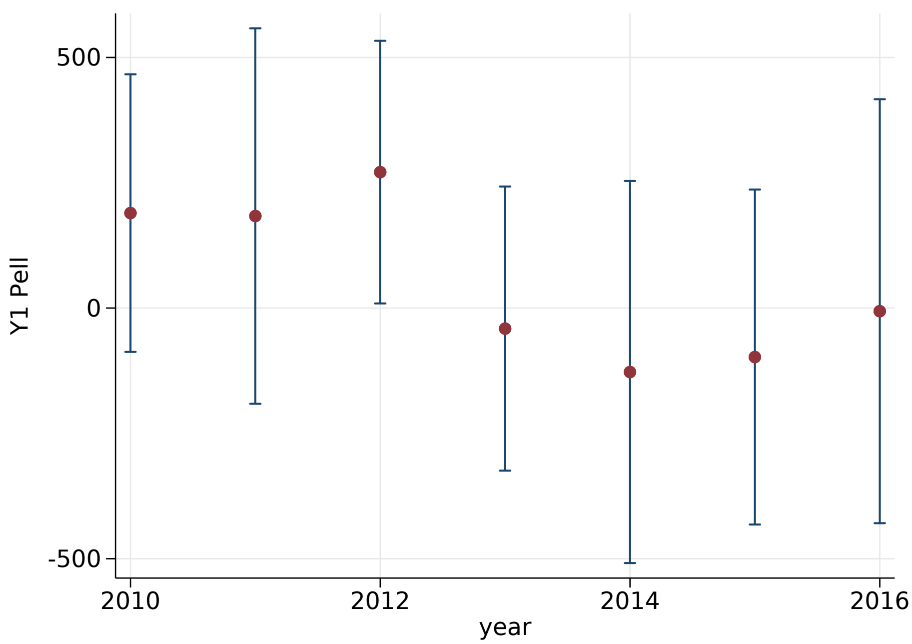
Notes: OLS regressions include indicator for AZ EFC, centered family income, an interaction term between AZ EFC and distance to the cutoff, and controls for race, age, gender, entry cohort and home institution. Standard errors are clustered at the college by cohort level.

Table C.14: Fuzzy RD Estimates on Effects of Grant Aid on Longer Term Academic Outcomes Among Non-Transfer Intending Students

	(1) Earned AA in 3	(2) Ever Earned AA
AZ Eligible	-.049	-.027
	(.028)	(.035)
Observations	14,986	14,986

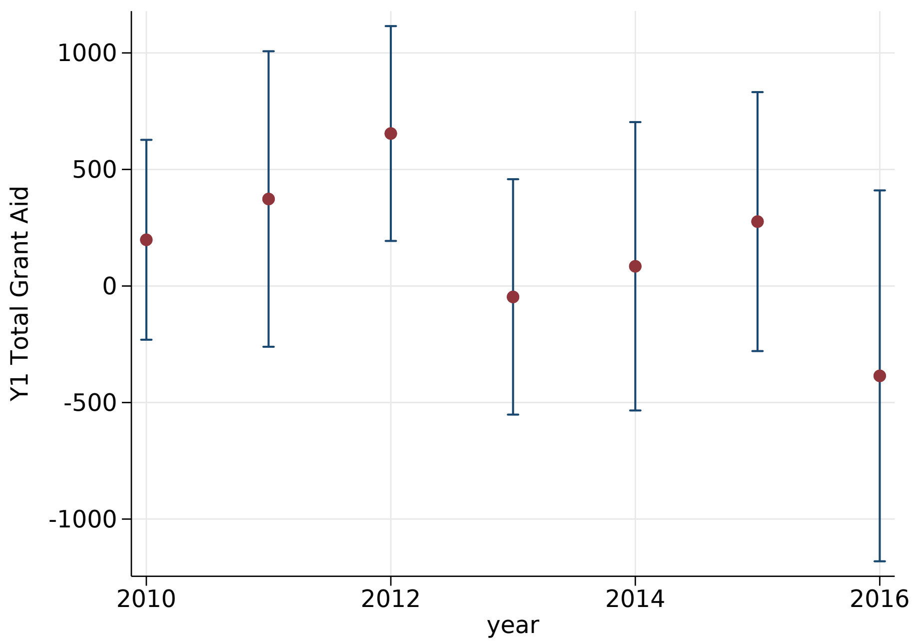
Notes: Reported fuzzy RD estimates are for local polynomial regressions and use optimal bandwidth selections (Calonico et al., 2017).

Figure C.1: Treatment Effect Estimates on Y1 Pell Grant Aid, by Entry Cohort.



Coefficient estimates for each entry cohort year are reported from regressions run on optimal bandwidth selection where the outcome variable of interest is first year Pell Grant aid.

Figure C.2: Treatment Effect Estimates on Y1 Total Grant Aid, by Entry Cohort.



Coefficient estimates for each entry cohort year are reported from regressions run on optimal bandwidth selection where the outcome variable of interest is first year Total Grant aid.