# Financial Incentives for Low-Income College Students: Effects on College Choice and Early-Career Decisions

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# Abstract

The first chapter of my dissertation investigates how student loans affect early-career decisions for low-income students. The University of Virginia's (UVa) financial aid policy included an income threshold to determine whether students received approximately $5,500/year as a grant or a loan. I use the income threshold to employ a regression discontinuity (RD) approach. Moreover, a policy change replaced approximately $3,000/year of grant aid with loans for low-income students. I use the policy change to employ a difference-indifferences (DD) approach. I find that student loans did not impact enrollment at UVa or subsequent academic performance, such as GPA and degree completion. However, higher student loan levels increased the share of students who majored in business or economics by 6 percentage points and increased the share of students who accepted a job in a high-paying industry by 9 percentage points. There is suggestive evidence that student loans decreased the likelihood of students immediately attending graduate school after college. My findings show that debt leads students to alter their career path before reaching the labor market.

The second chapter examines whether colleges respond to a financial incentive that could incentivize colleges to enroll more low-income students. I use variation in funding created by an accountability system in one state, which awards public colleges substantially higher funding for the outcomes of students who are below an income threshold. For example, public four-year colleges receive approximately $3,600 in additional state funding when such a student graduates. Using state-wide administrative data on high school graduates, I compare the enrollment of students around the income threshold. Overall, the regression discontinuity estimates show no difference in college attendance or choice of four-year versus two-year college. However, analysis by college selectivity shows that the public flagship responded to equity measure by enrolling more Pell-eligible students. There is suggestive evidence that the response by the flagship diverted ineligible students to less selective public four-year colleges. Using ACT scores to measure baseline academic ability, I find that these effects are largest among students who are likely on the margin of being accepted at the flagship.

The third chapter examines the effects of conditional cash transfers on college enrollment. Together with Professor Ben Castleman, I designed and implemented a large-scale field experiment that encouraged a national sample of low-income, high-achieving students to apply to high-quality schools in their state. Students were randomized to receive: 1) a customized list of high-quality colleges in their state; or 2) the list of schools and an offer of $100 for applying to schools from their list. We find that the incentive offer increased the likelihood that students enrolled at a high-quality institution by 2.6 percentage points (4.5 percent) and reduced the share of students attending a lower-quality college. Impacts of the incentive were more than double in magnitude for low-income students. These results suggest that leveraging conditional cash transfers at high leverage decisions points—such as when students are choosing which colleges to apply to—could be an efficient use of resources to reduce socioeconomic disparities in the quality of higher education institution students attend.

JEL Classification: H75, I21, I22, I24, J24

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# Contents

# Chapter 1

# The Impacts of Student Loan Debt on College and Early-Career Decisions

## 1.1 Introduction

The federal student loan programs play a pivotal role in higher education by allowing students, who would otherwise be unable to borrow to attend college.[1] The 140% increase in outstanding student loan debt nationally over the past decade, however, has led to concerns about the effect of debt after college (Household Debt and Credit, 2018). One concern is whether student loan debt affects an individual's career decisions. This is a policy-relevant question because changes in career decisions can have spillover effects on the amount of tax revenue collected. By changing an individual's career decisions, student loan debt could also influence the stock of human capital if it impacts the decision to attend graduate school. Lastly, many states and selective universities have proposed plans to reduce student loan debt by providing students with additional grant aid with the hope that it will reduce borrowing. To evaluate the costs and benefits of these plans, we need to know how student loan debt and the composition of financial aid impacts individuals after college. Despite the concerns over how debt impacts individuals after college, we do not know enough about the effects of student loan debt. In this paper, I examine the effects of student loans on major choice, initial occupation choice, and the decision to attend graduate school.

Identifying the effects of student loans on career decisions is often difficult because of student-level and college-level confounders. Differences in borrowing by student ability could

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[1] The federal government provides these loans because students cannot put themselves up for collateral, and as a result, private lenders would be reluctant to make such loans. It is difficult to observe whether a student is credit constrained, but much of the literature attributes the positive relationship between income and college enrollment as evidence of credit constraints (Ellwood and Kane, 2000). Even conditional on ability, Belley and Lochner (2007) find that a positive relationship between family income and children's educational attainment and show it has increased over time.

lead to an observed negative relationship between debt and earnings. [2] Furthermore, if debt affects where students enroll in college, then it becomes difficult to disentangle the effect of loans and college quality on earnings. Most research tries to eliminate these threats to identification by relying on variation in student debt created by natural experiments (e.g., financial aid policy changes, large changes to tuition) or eligibility criteria (e.g., academic eligibility for state merit aid, income eligibility for federal loans). Overall, the literature provides mixed evidence on the direction of the effect of debt on earnings, little differences in major choice, and mainly negative effects on graduate school enrollment (Field, 2009; Rothstein & Rouse, 2011; Chapman, 2015; Fos, Liberman, & Yannelis, 2017; Gervais & Ziebarth, 2017).

This paper provides new evidence on the impacts of student loan debt on early-career decisions among high-achieving low-income students using similar sources of variation used in the previous literature. Specifically, I exploit two aspects of the financial aid formula at the University of Virginia (UVa). UVa's financial aid policy includes an income threshold to determine whether a student receives an offer of $5,000/year as a grant or a loan. Using the change in loan offers at the threshold, I employ a regression discontinuity design (RD) approach and compare the outcomes for students whose family income is just below the threshold—high-need students—to students whose family income is just above the threshold—regular-need students.[3] As a second source of variation, I use a policy change, which replaced $3,500/year of grant aid with loans for high-need students and left the financial aid offers to regular-need students unchanged.[4] Using a difference-in-differences (DD) approach, I compare the change in outcomes for high-need students after the policy change to the change in outcomes for their regular-need peers who were unaffected by the policy.[5]

Administrative data from UVa allows me to match a student's first-year financial aid to enrollment, subsequent academic performance, and survey data on post-graduation plans. I find that regular-need students, just above the threshold, borrowed roughly $2,750 more in their first-

[2] If high-achieving students, who are more likely to receive a high-paying job offer because of their ability, received merit-based grants instead of loans, then we would conclude that loans reduce earnings. Descriptive analysis by Luo and Mongey (2017) provides evidence that there is negative selection on ability in how much students borrow.
[3] This strategy estimates the reduced form effect of loan on career decisions. To estimate the effect of student loans, I instrument for student loan debt with the income eligibility threshold. However, since I do not observe total student loan debt upon graduation, my main estimates come from the reduced form equation.
[4] The change in loan aid was capped at $7,000/year for high-need out-of-state students.
[5] I use the DD strategy to examine impacts on academic performance because post-policy change cohorts have not been enrolled long enough time to graduate.

year compared to their peers below the threshold. The policy change resulted in a slightly smaller change in debt, with high-need students borrowing $2,000 more in their first year. I find that the change in the composition of financial aid from grants to loans at the threshold did not impact enrollment at UVa, which I discuss more below.  Further, I find that receiving loans instead of grants had statistically insignificant impacts on academic performance such as GPA, credits accumulated, or degree completion.

From semester-level data on major choice, I find a statistically significant effect of student loans on the share of students majoring in business or economics. Regular-need students at the threshold were 5.7 percentage points more likely to earn a degree in business or economics. The size of the effect increases across semesters as more student declare economics as a second major. While the estimates are not precise, business and economics tended to draw students from STEM fields, as opposed to humanities or other social sciences. Since STEM majors often pursue a graduate a degree, a shift away from STEM fields could reduce the share of students enrolling in graduate school after college and therefore lower total human capital.[6]

Consistent with shifting students away from majors that likely lead to graduate school, I find suggestive evidence that students sought different work experiences in college. Regular-need students were more likely to report ever having an internship and a summer job, and extensively using resources from the career center, all of which are steps in securing a full-time position after college. Regular-need students were also less likely to report having experiences a student might benefit from before going to graduate school, such as a job shadowing or working as a research assistant.

Using survey data on post-graduation plans, I find that the impacts of student loan debt on major choice persist to differences in initial occupation choice.  Planned employment for regular-need students at the income threshold increased by a statistically significant 13.4 percentage. The effects were driven by employment in high-paying industries (finance, consulting, IT & engineering). The share of students who accepted a job in a high-paying industry increased by a statistically significant 9.0 percentage points at the threshold. The increase in planned employment resulted in equally sized decreases in attending graduate school and seeking employment, but these differences are not statistically significant. Controlling for major choice

[6] Baum and Steele (2017) shows that 67% of Bachelor's degree recipients majoring in science or math as undergraduates were enrolled in a graduate program five years after graduation.

reduces the difference in accepting a job in a high-paying industry, which suggests part of the effect of debt on occupation choice is due to effects on major choice.

The main concern for identification is that the difference in loan offers might have affected an applicant's decision to enroll at UVa. I find that a $1,000 increase in net price (cost of attendance less grant aid) reduces the probability of attending UVa by a statistically insignificant 0.8 percentage points (1 percent). I explain the weak enrollment response by providing evidence that UVa's top competitors also change their price discontinuously. Therefore, the relative cost of UVa is constant across the threshold. Consistent with a small response to price, the density of matriculates is smooth around the income threshold, indicating that students did not manipulate their income to receive more grant aid.[7] While I cannot examine intended major, I find balance on the share of students accepted to the School of Engineering, and on academic characteristics such as passing the AP economics and calculus exam, and on SAT scores. The similarity of the characteristics of the students around the threshold combined with the null impacts on enrollment indicates that the effects are due to student loans as opposed to selection into UVa. For robustness, I show that the main results are not affected by the inclusion of covariates and are similar across multiple bandwidths.

This paper contributes to several strands of literature. First, I contribute to the literature on the effects of student loans on career decisions. The previous literature (Field, 2009; Rothstein & Rouse, 2011; Gervais & Ziebarth, 2017) finds mixed evidence on the direction of the effect of debt on earnings, with little evidence of differences in major choice. I add to the evidence base by showing that debt leads students to accept a job in a high-paying occupation. I also provide evidence that the effects of debt start in college when students choose a major. The shift of students away from majoring in STEM fields, which often lead to graduate school, could help explain the negative effects of debt on graduate school enrollment found in the literature. Fos, Liberman, & Yannelis (2017) find that higher levels of undergraduate debt decreased the likelihood of attending graduate school but are unable to examine whether differences in undergraduate major choice drove part of the effect.

I also contribute to the literature on the effects of student loans and financial aid on educational performance. Much of the literature (e.g., Scott-Clayton, 2011; Wiederspan, 2015;

[7] The density of admitted students is also smooth, suggesting that students around the threshold are not differentially applying.

Castleman and Long, 2016; Solis, 2017; Denning, 2018; Marx & Turner, 2017) finds that more financial aid leads to positive effects on academic performance. These papers do not tell us whether the composition of financial aid matters, which is a policy-relevant question as many states announce free college proposals to reduce student loan burdens by providing students with more grant aid. Clotfelter, Hemelt, & Ladd (2017), and Park & Scott-Clayton (2018) find, as I do, that providing aid in the form of loans instead of grants does not impact academic performance. This growing body of evidence, showing that the composition of financial aid does not impact performance, suggests that efforts to increase academic attainment by only reducing student loan burdens could be an ineffective policy lever for students inframarginal to enrolling in college. Instead of reducing the amount students pay to attend college, current free college proposals could be more effective by increasing spending on academic supports for students, as demonstrated by Deming and Walters (2018).

The remainder of the paper is organized as follows. Section 2 discusses the prior research, and Section 3 discusses the policy background. Section 4 discusses the data; Section 5 discusses the empirical strategy, and 6 presents tests for the validity of the research design. Finally, Section 7 presents the results, and Section 8 concludes.

## 1.2 Literature Review

Given the public debate over the rising student loan debt, there is an active and growing literature studying the effects of loan aid on student outcomes in and after college. Much of the recent research has focused on the effects of changes in the level of aid while students are in college because of the availability of data sources. Theoretically, access to student loan aid or grant aid can remove credit constraints and increase college enrollment or academic performance. Several papers support these predictions by showing that an increase in grant aid leads to a reduction in hours worked and increased time spent volunteering, and studying with classmates (DesJardins et al., 2010; Boatman and Long, 2016). Another set of recent papers show that community college students, with access to student loans, completed more credits and had higher GPAs (Dunlop, 2013; Wiederspan, 2016; Marx & Turner, 2017).[8] Stinebrickner and Stinebrickner (2008) show, however, that credit constraints do not play a large role in the decision to drop out of college.

This paper focuses on changes in the composition of aid—loans versus grants—rather than changes in the levels, which has received less attention in the literature. Understanding the effect of changes in the composition of aid has become increasingly policy-relevant due to recent free college proposals, which aim to reduce student loan burdens by providing students with additional grant aid.[9] A recent paper by Park & Scott-Clayton (2017) examines the effects among community college students of receiving a modest Pell Grant (approximately $550) instead of loan aid. They find that receiving grant aid leads to an initial increase in enrollment intensity, but that the effects on academic outcomes fade out. The financial aid policy and the setting studied in this paper is closest to the one in Clotfelter et al. (2017). There, the authors provide some evidence that only after layering a higher composition of grant aid with additional academic supports do students perform better compared to similar students with a higher composition of loan aid.

Student loans could affect post-graduation outcomes through multiple potential channels. While loans reduce credit constraints before and during college, they could constrain students after college. Theoretically, credit constraints have an ambiguous effect on earnings. By preventing an individual from smoothing consumption, credit constraints could shift students to a more lucrative career choice.[10] On the other hand, to make loan payments, an individual might accept a lower paying job earlier in the search process rather than waiting for a better offer. A 2016 report by the Council of Economic Advisors argues that the standard 10-year repayment window for student loans could constrain borrowers because of the relatively short repayment period, which is also at a time when earnings are at their lowest and most volatile.[11] Research by Kessler (n.d.) provides evidence of post-graduation credit constraints among low-income

[8] Using a regression discontinuity approach, Solis (2017) found that students in Chile who gained access to student loans were significantly more likely to enroll and complete college.
[9] Policymakers in 36 states introduced bills for some type of free college (Source: https://www.ecs.org/free-college-and-adult-student-populations/, accessed on 8/28/18). In 2018, Democrats from the House of Representatives introduced the Aim Higher Act to provide two years of community college tuition free.
[10] An earlier version of Rothstein and Rouse (2011) proposes a two-period life-cycle model to illustrate the effects of credit constraints.

[11] Dynarski and Kreisman (2013) calculate that the monthly loan payment for an individual with $25,000 in debt and a starting salary of $23,000 will consume 13 percent of their income.

students who were accepted to Teach for America (TFA). The author finds that the offer of a $600 grant or loan to help students transition between college and the start of their service meaningfully and significantly increased the share of students who accepted a job at TFA.

The literature provides mixed evidence on the direction of the effect of debt on earnings. In a closely related paper to mine, Rothstein & Rouse (2011) find that higher levels of debt, from a change in financial aid policy, decreased the probability that graduates would accept a job in a low-paying industry. The authors argue that the difference between what students pledged to donate to the university and their subsequent donations is evidence of credit constraints.[12] Using an income eligibility threshold for need-based Federal student loans, Gervais & Ziebarth (2017) differ from the previous study by finding debt lowers earnings immediately after graduation. They attribute the effects to borrowers accepting part-time work and working in jobs less related to their degree.[13] This paper will add the literature

Credit constraints could also affect the decision to enroll in graduate school. Using data from a random sample of federal student loan borrowers, Fos Liberman and Yannelis (2017), examine the effect of undergraduate student debt on graduate school enrollment. They leverage variation in debt within a college over time created by large tuition changes after students enrolled. The authors show that undergraduate debt decreases the probability of enrolling in graduate school. Specifically, $4,000 in debt reduces the probability of enrolling in graduate school by 1.5 percentage points relative to a 12% mean. They argue that credit constraints explain their results because the effects are smaller in years following an increase in student debt caps. The authors do not observe information of major choice for examining whether differences in major choice as an undergraduate is a channel through which debt impacts graduate school enrollment.

Debt aversion—the disutility of held debt—could also impact early-career decisions. If merely holding debt reduces utility, individuals will try to repay loans faster by pursuing a career with a higher salary. Field (2009) provides compelling evidence of debt aversion, albeit using a highly selective and relatively small sample. The author leverages a financial aid experiment

[12] Chapman (2016) uses the eligibility criteria for state merit aid policies, which reduce the amount of loan aid eligible students receive to examine effects of debt on post-graduation plans. While the author finds that students who were barely eligible for a merit scholarship had lower earnings compared to barely ineligible students, merit scholarship changed the type of college eligible student attended. It is therefore hard to disentangle the effects of debt from other factors related to college quality.

[13] Weidner (2016) and Ji (2018) have similar findings to Gervais & Ziebarth (2017) using a dynamic model of schooling, borrowing, and job search.

where entering law students were randomly assigned to receive a financial aid package of equal value but which varied in the length of indebtedness. The author finds that receiving an offer of longer indebtedness significantly reduced the likelihood of taking a job in public interest law, which should only occur if there is a psychic cost to holding debt. The generalizability of Field (2009) is unclear. In an experiment conducted among a sample of adults ages 18-55 in Canada, Eckel et al. (2007) show that debt aversion is not an important barrier to investment in postsecondary education.

## 1.3 The University of Virginia's Financial Aid Policy

In 2004, the University of Virginia launched its flagship financial aid program, AccessUVa. Under AccessUVa, UVa provided financial aid to meet 100% of a student's demonstrated financial need— the cost of attendance minus expected family contribution—with a combination of grants, need-based loans, and work-study. In 2013, the cost of attendance was roughly $26,000 and $54,000 for in-state and out-of-state students, respectively. To be eligible, students must submit two financial aid applications, the Free Application for Federal Student Aid (FAFSA) and CSS PROFILE.[14] During my sample period, 53% of UVa matriculates filed both applications.[15] Among AccessUVa-eligible students, 63% had a positive demonstrated financial need and were therefore eligible for need-based aid.

Under AccessUVa, UVa offers students a combination of grants and loans, which is determined by a student's family income and assets. Before 2014, "high-need" students received financial aid offers consisting entirely of grant aid. UVa defined a student as high-need if he met the following criteria:

1. Family income less than 200% of the federal poverty line. [16]
2. Eligible for a Pell Grant

[14] The CSS/Financial Aid Profile is run by the College Board, and is required for over 240 colleges, universities, and scholarships. Unlike the FAFSA, the PROFILE can contain questions specific to a school, requires a minimum student contribution, and uses a different methodology to determine financial need. For more information, visit https://student.collegeboard.org/css-financial-aid-profile. UVa replace an institution-specific form with the CSS PROFILE starting with the 2011 entering class.

[15] 16% of matriculates filed only the FAFSA, and 31% did not file either application. While there is no income eligibility requirement, students from affluent families might not apply since they won't receive need-based aid.

[16] In 2015, 200% of the federal poverty line for a family of four was $48,500.

3. Family assets less than $75,000.

Financial aid offers to the remaining AccessUVa-eligible students, henceforth "regular-need" students, included loans. Annual loan offers to regular-need students were subject to a cap of 25% of the in-state cost of attendance. In practice, the annual federal borrowing limit constrained the loan cap below 25%. For example, first-year students were offered up to $5,500 in loans (the federal limit) even though 25% of in-state was $6,000-$6,500.[17] Based on the criteria listed above, I find that 11% of AccessUVa-eligible students (or 5.8% of new entrants) were high-need and that the income threshold determined need status for most students.[18] To preview my empirical strategy, I use income as my running variable in the RD and exploit the income threshold to examine the effect of debt on early-career decisions.

Figure 1 shows how AccessUVa influences the price students pay to attend UVa and previews a source of variation in student loan debt. The figure plots the estimated net price (cost of attendance minus grant aid) of attending UVa by income.[19] As family income crosses the high-need threshold between $45,000 and $50,000, net price sharply increases by roughly $5,000. The sharp increase occurs because regular-need students receive $5,000 as a loan instead of as a grant.

In 2014, facing an increasing cost of AccessUVa, UVa reduced the relative generosity of grant aid offered to high-need students. Under the new policy, in-state and out-of-state high-need students were offered $3,500 and $7,000 annually in loans, respectively.[20] This policy change did not impact the financial aid offers to regular-need students. With respect to Figure 1, this policy shifted up the net price plot by $3,500 for students below $45,000. As a second source of variation, I use the change in loan offers to high-need students to identify the impacts of debt.

## 1.4 Data and Study Sample

[17] The annual federal loan limit increases with years of education, so the loan cap was not constrained by the federal loan limit once students were sophomores or juniors. Source: https://studentaid.ed.gov/sa/types/loans/subsidized-unsubsidized

[18] 82% of students who meet the high-need income requirement also meet the asset requirement, whereas only 17% of students who meet the asset requirement also meet the income requirement.

[19] I used the net cost calculator to estimate net price. https://npc.collegeboard.org/student/app/virginia

[20] The university has since reduced the amount of loans offered to in-state and out-of-state high-need students to $1,000 annually and $4,500 annually, respectively.

The data I use to test whether debt affects early-career decisions is from multiple administrative sources within the University of Virginia. I combine records on financial aid applications with financial aid offers to identify regular- and high-need students and show how the income threshold and policy change create variation in student loan debt. For students who matriculate to UVa, I obtain term-level credits, GPA, and major choice to examine the effects of student loan debt while students are in college. Using a student's major at graduation, I create indicators for whether a student majored in business or economics, a STEM field, or any other major.[21] Since over 25% of students double major, these outcomes are not mutually exclusive. I examine economics and business together because there is a cap on the number of students accepted to the undergraduate school of business and economics could serve as a substitute.[22] In additional analysis, I disaggregate major choice and look at effects for economics and business separately. To examine post-graduation outcomes, I merge in self-reported data on initial occupation choice and graduate school enrollment for the 64% of graduates who completed the exit survey. From the industry of employment, I create indicators for whether a student accepted a job in a high-paying industry (consulting, finance, IT & engineering) or public-interest industry (education, government, NGO, service/volunteering). In the remainder of the section, I describe my sample in more detail.

### 1.4.1 Regression Discontinuity Sample Construction

My regression discontinuity analysis focuses on the subset of UVa students who were eligible for AccessUVa and had income near the income threshold. From the pool of students admitted to UVa between 2009 and 2015, I exclude the 2014 and 2015 cohorts. My empirical strategy relies on the discontinuous change in loans at the income threshold, and the 2014 policy change drastically reduced the discontinuity. To limit the sample to AccessUVa-eligible students, I drop students who did not submit the FAFSA *and* CSS PROFILE. This restriction is

[21] For the small share of students who are still enrolled after the fourth year, but have not graduated, I use the major from their last semester available.

[22] Examining occupation choice by major provides evidence that economics and business are substitutes. According to survey data from UVa graduates, 81% of business majors take a job in finance or consulting compared to 54% for economics majors. The next highest share of students from a given major working in finance or consulting is engineering at 27%. The size of each class in the undergraduate business school usually ranges from 340 to 350.

necessary to my empirical strategy because I only observe income for students who submit the CSS PROFILE. I then drop students with no financial need because they do not receive need-based financial aid.[23] These restrictions limit the generalizability of my results but allow me to draw causal inference about the effect of student loan debt among low-income students.[24] The resulting analytic sample consists of 9,625 admitted students, and 5,338 UVa matriculates. My analysis will focus on a bandwidth restricted sample of 1,422 matriculates with family income between 125% and 275% of the federal poverty line. When examining differences in post-graduation outcomes, I use the sample of approximately 1,000 students between 100% and 300% of the federal poverty line.[25]

### 1.4.2 Difference-in-Differences Sample Construction

I use similar sample restrictions for analysis using a difference-in-differences empirical strategy. The main difference between the RD and DD sample is that the DD sample includes students from the 2014 and 2015 entering cohorts. These cohorts entered UVa after the change to the financial aid policy and serve as the "post" cohorts in my empirical strategy. The other difference is that I limit the sample to students who have unmet financial need using the 2009 cost of attendance. As the cost of attendance increases, more regular-need students become eligible for need-based financial aid. Restricting the sample using the 2009 cost of attendance helps prevent changes in the composition of regular-need students over time. The resulting analytic sample consists of 7,056 UVa matriculates.

### 1.4.3 Descriptive Statistics

[23] This restriction drops students from higher income families who are not near the income threshold. The average income of students dropped because they did not qualify for need-based aid was over $250,000.

[24] I further exclude foreign students because they are not eligible for federal or state student aid, as well as athletes and Jefferson Scholars because their financial aid offers are not based on need. In the applicant sample, I also drop students who are missing a financial aid offer.

[25] The post-graduation survey sample is similar demographically and financially to the bandwidth restricted sample. The post-graduation sample, however, is higher achieving based on SAT scores, and GPA while at UVa. They are also slightly more likely to major in business or economics. In general, these differences are relatively minor and should not reduce the generalizability of these results to the restricted sample.

Table 1 displays selected summary statistics for the RD and DD samples, as well as for all UVa matriculates. Column 1 includes all UVa matriculates; columns 2 and 3 include the full and bandwidth-restricted regression discontinuity samples, respectively; and column 4 includes the difference-in-differences sample. As reported in panel A of column 1, most matriculates are from Virginia, white, and high achieving. The average SAT score was 1350, which is at the 93rd percentile nationally. Additionally, students passed 5.0 AP exams in high school, which is nearly double the national average of exams taken among AP test takers (Avery et al., 2017).[26] Comparing columns 1 and 2 shows that AccessUVa-eligible students are less likely to be white, pass fewer AP exams, and score lower on the SAT compared to all matriculates. These differences are more pronounced when comparing all matriculates to the bandwidth restricted sample. The matriculate sample consists of 63% white students compared to 37% among the restricted sample. The average student in the restricted sample also passed 1.1 fewer AP exams and scored at 6 percentiles lower on the SAT compared to the matriculate sample.

Panel B of Table 1 contains the main explanatory variables. Using financial aid application records, I determine a student's eligibility for AccessUVa and need-status. Based on who submitted the FAFSA and CSS PROFILE, 53% of UVa matriculates are eligible for AccessUVa. Based on income, family size, assets, and Pell Grant eligibility among AccessUVa-eligible students, 89% are regular-need, and 11% are high-need. By construction, the bandwidth-restricted sample is lower income relative to the matriculate sample. Column 3 shows that 60% of students around the income threshold are regular-need, and the average income is $100,000 lower than the full sample average.

Panel C of Table 1 contains the main outcomes of interest. Approximately 30% of UVa matriculates took out a student loan during their first year, borrowing an average of $3,000. In the bandwidth-restricted sample, just over 50% of students took out a loan during their first year and borrowed $3,385 on average. Given the background of students at UVa, it should be no surprise that the enrollment rate is over 85% after three years. Due to the timing of the study, third- and fourth-year academic outcomes are not available for the cohorts who entered after the financial aid policy change. Nearly 20% of UVa matriculates major in business or economics, and 45% of students major in STEM, and 48% have an "other" major, such as the humanities or

[26] Percentile scores based on the 2013 college-bound seniors. Source: http://media.collegeboard.com/digitalServices/pdf/research/SAT-Percentile-Ranks-Composite-CR-M-2013.pdf

social science.[27] Students from the bandwidth-restricted sample are less likely to major in business or economics and more likely to have an "other" major. Based on responses to the exit survey, 66% of UVa graduates have employment plans, 20% plan to attend graduate school, and the remaining 14% are seeking employment or do not have plans. Students from the bandwidth-restricted samples are less likely to have employment plans and more likely to be seeking a job. Relative to the full sample of UVa graduates, students from the bandwidth-restricted sample are also less likely to have plans to work in a high-paying industry and equally likely to plan to work in a public-interest industry.

## 1.5 Empirical Strategy

To eliminate bias driven by different types of students making different borrowing decisions, I exploit variation in student loan debt created by two aspects of the financial aid policy at the University of Virginia. As described in section 3, students above an income threshold are offered $5,000/year as a loan instead of grant aid. I use a regression discontinuity design (RD) to compare a wide variety of college and post-college outcomes between students just above and just below the threshold. As a second source of variation, I use a 2014 change to the financial aid policy, which replaced a portion of grants with loans for low-income students. Using a difference-in-differences (DD), I compared the outcomes for low-income students before and after the policy change, with unaffected students as a comparison group. By comparing students at the same university, I am also able to eliminate bias driven by differences on college quality.

### 1.5.1 Regression Discontinuity Approach

To identify the effects of student loan debt, I exploit the nonlinear relationship between income and need status, which generates a discontinuous increase in student loan debt. Let $income_c^{RN}$ be the income cutoff for regular-need status for cohort c.[28] For student *i* in the entry cohort *c*, $\widetilde{Income}_{ic} = Income_{ic} - income_c^{RN}$ is the distance his family's income falls from the

[27] Recall, 25% of students double major, so these major categories do not sum to one hundred.
[28] The cutoff also differs according to family size.

threshold. Students with $\widetilde{Income} > 0$ are regular-need and therefore receive loans as part of their financial aid offer instead of all grant aid.

I estimate the first-stage relationship between income and student loan debt using the following regression equation:

1) $StudentDebt_{ic} = \alpha + \gamma_1 RegularNeed_{ic} + \gamma_2 \widetilde{Income}_{ic} + \delta_c + \upsilon_{ic}$

where $StudentDebt_{ic}$ is the student loan debt held by student $i$ in cohort *c*, $RegularNeed_{ic}$ indicates whether a student is above the income threshold, and $\widetilde{Income}_{ic}$ is a student's family income as defined above. I control for time-varying shocks by including cohort fixed effects ($\delta_c$). If the identifying assumptions are met, which I discuss in section 6, then $\gamma_1$estimates the change in student loan debt at the income threshold.

I estimate the impact of being above in the income threshold on outcomes from the following reduced form regression equation:

2) $Y_{ic} = \alpha + \beta_1 RegularNeed_{ic} + \beta_2 \widetilde{Income}_{ic} + \delta_c + \epsilon_{ic}$

I consider three different sets of outcomes. The first are measures of academic performance in college, in order to estimate whether debt affects a student's academic qualifications when making early-career decisions. The second is the measures of major choice, to estimate whether students are altering their career paths before reaching the job market. The third are measures of post-graduation plans, to estimate the effects on both occupation choice and graduate school enrollment. If debt causes students to pursue more lucrative majors or careers, then $\beta_1 > 0$. If debt reduces graduate school enrollment, then $\beta_1 < 0$.

I also generate instrumental variable estimates using the following regression equation:

3) $Y_{ic} = \alpha + \theta_1 StudentDebt_{ic} + \theta_2 \widetilde{Income}_{ic} + \varepsilon_{ic}$

where *Student Debt* is instrumented by *Regular Need* using equation (1). The resulting estimated effect of debt on major choice is given by $\hat{\theta} = \frac{\hat{\beta}_1}{\hat{\gamma}_1}$, the ratio of the reduced from and first-stage coefficients for $RegularNeed_{ic}$, where $\gamma_1$is the change in student loan debt at the income threshold, and $\beta_1$is the change in outcome at the income threshold. Assuming montonicity—regular-need status weakly increases loan aid for all students—IV estimates represent the average effect of an increase in student loan debt for the set of students who received a loan offer by being above the income threshold (Angrist and Imbens, 1995). Since I only observe first-year debt, I am unable to directly estimate the effect of total debt on outcomes. As a result, much of my analysis relies on equation 2), the reduced form equation.

In practice, I estimate local linear regression models with a uniform rectangular kernel and a bandwidth of 75% of the federal poverty line (i.e., between 125% and 275% of the federal poverty line). This bandwidth is similar to the optimal bandwidth computed according to Calonico, Cattaneo, Titiunik (2014). For post-graduation outcomes, I use a bandwidth of 100% because the sample is smaller than the entry cohort sample. I also present results for a variety of bandwidths, the inclusion of individual controls, and other functional form assumptions of the relationship between income and the outcome.

### 1.5.2 Difference-in-Differences Approach

As a second source of variation, I use a 2014 change to the financial aid policy, which affected students below the income threshold. I compare changes over time among those below the income threshold (who received more loan aid after 2013) to those above the income threshold (who were unaffected by the policy change). I estimate the following difference-in-differences specification:

4) $Y_{ic} = \alpha + \beta_1 HighNeed_{ic} + \beta_2 Post_c + \beta_3 HighNeed * Post_{ic} + \epsilon_{ic}$

where $Y_{ic}$ is an outcome of interest for student $i$ in cohort $c$. $HighNeed_{ic}$ indicates whether student *i* from cohort *c* was below the income threshold and controls for constant differences between high- and regular-need students. $Post_c$indicates whether the cohort entered UVa after the financial aid policy change and controls for time-varying differences between the pre- and post-cohorts. When estimating equation 4), I include cohort fixed effects instead of $Post_c$ for more flexibly control for differences across cohorts. The main coefficient of interest is $\beta_3$, which represents the difference in a given outcome after the policy change among high-need students. I test the main specification with and without covariates to ensure the results are not due to changes in the composition of students.

The RD and DD each have their advantages and disadvantages. In the RD approach, under relatively weak assumptions, student loan debt is “as good as” randomly assigned around the cutoff (Lee and Lemiuex, 2010). One drawback of the RD approach is that the estimated effects apply to low-income students at the cutoff. In contrast, the DD approach identifies the effect among all low-income students below the cutoff. It also provides greater power to detect effects than the RD approach. The main drawback of the DD approach is that I do not observe the graduation or post-graduation outcomes for the cohorts entering after the policy change. Due to this data limitation, I rely on the RD approach for my main analysis and use the DD approach for robustness and to discuss the generalizability of the RD estimates.

## 1.6. Validity of Research Designs

### 1.6.1 Regression Discontinuity Identifying Assumptions

Identification in the RD approach relies on students above and below the income threshold being nearly identical. Specifically, the assumptions are: (1) that the effect of income on the outcome is continuous around the threshold, and (2) that students on either side of the threshold are similar on observable and unobservable characteristics. Below I provide evidence in support of assumption (2), which allows me to attribute differences in outcomes at the threshold to the difference in student loan debt.

Following McCrary (2008), I test for the similarity of students across the threshold by first checking for sorting around the threshold. If students systematically lower their income to be below the threshold, then the variation in student debt at threshold is not as good as random. For example, if students, who wanted to pursue lower-paying majors, reported a lower income to qualify for more grant aid, then students across the threshold would differ in both their intended major and how much grant aid they received. Differential sorting is a potential concern because UVa publishes the eligibility criteria for high-need status on their financial aid webpage. Figure 2 shows, however, that the income distribution for students admitted to and enrolling at UVa is smooth through the income threshold.[29] The figure also shows that there are very few students at more than 150% below the threshold.

As a second test of the identifying assumption, I check for the balance of the characteristics across the threshold. If students are similar across the threshold, then baseline characteristics

[29] I cannot examine the income distribution for applicants because I do not observe income for students who were not admitted.

should also be the same across the threshold. I start by estimating whether the decision to enroll at UVa is sensitive to price around in the income threshold. If the decision to enroll at UVa is sensitive to price, then the students who matriculate across the threshold could vary. One concern is that the regular-need matriculates intend to major in more lucrative careers. The smooth density around the income threshold for both admits and matriculates implies the decision to enroll is not very price sensitive, which I formally test this with regression analysis.

Table 2 and Figure 3 show little evidence that regular-need students are less likely to enroll at UVa despite facing a higher net price. Among the bandwidth restricted sample, column 3 shows that students above the threshold are 2.6 percentage points less likely to enroll at UVa compared to students below the threshold. The difference, however, is not statistically significant. In column 7, I instrument for net price (cost of attending UVa – grant aid) with high-need status and find that a $1,000 increase in net price reduces the probability of enrolling by 0.8 percentage points, and I can rule out effects larger than 3.2 percentage points. The remaining columns in Table 2 show that the null enrollment results are robust across different specifications. Figure 8 illustrates the enrollment results from bandwidths ranging from 30% to 500% of the federal poverty line, where the darker line plots point estimates of $\beta_1$ from different bandwidths and lighter lines show the 95 percent confidence intervals. The figure shows great gains in precision as the bandwidth increases from 30% to 200%. Further, in the neighborhood of the optimal bandwidth, the difference in enrollment hovers around negative 2.5 percentage points. For bandwidths greater than 200%, the enrollment difference hovers around zero. The results using the wider bandwidth, however, are likely biased because the linear specification is less likely to be accurate.[30]

To understand a weak response to a change in the price of attending UVa, I consider the following theoretical framework. As described in Manski and Wise (1983), the enrollment process can be viewed as a series of decision stages. In the last stage, individuals decide whether to enroll or not. This decision depends on the costs and benefits of UVa *relative* to the other schools in a student's choice set. Under this framework, if there is not a difference in the

[30] These results are consistent with Linsenmeier, Rosen, and Rouse (2006), which finds that a large change in the financial aid policy at a selective university resulted in a statistically insignificant change in enrollment for low-income students

probability of enrolling at UVa across the income threshold, then the relative costs and benefits should be constant across the threshold.

Two conditions must be met for the relative benefit of attending UVa to be constant across the threshold. First, the quality of a student's outside option must be equal across the threshold. For example, students above and below the threshold are deciding between UVa and William & Mary. If students are similar across the threshold, then they should have similar outside options. If students differed across the threshold in baseline academic ability, then we would worry their outside options differed. Below I show that students across the threshold are similar on baseline demographics. Second, the benefit of attending UVa must not depend on the composition of aid they receive. I assume this holds, but in the results section, I examine whether academic performance differs across the threshold.

Turning to the cost side, I find evidence that the relative cost of attending UVa is constant across the threshold. To define the relative cost of attending UVa, I first defining their competitors. Over 75% of the students who were accepted to UVa but enrolled at other institutions, attended one of six institutions.[31] I consider these six institutions as UVa's competitors. After researching the details of their financial aid policies, I found that three competitors have a similar change in price at the same income threshold or in the neighborhood of the threshold used by UVa. Therefore, the relative cost of attending UVa is similar above and below the threshold.

While I cannot produce figures plotting the change price by income for UVa's competitors as presented in Figure 1, I show how price changes across income bins. Using data from IPEDS, Figure A1 shows the average price paid for students with income of $30,001-$48,000 and income of $48,001-$75,000. These income bins correspond to below and above UVa's income threshold. The figure shows that students at UVa and its competitors pay a similar price across the income bins. We should therefore expect to a small enrollment response because relative costs and benefits of attending UVa are similar across the threshold.

Finally, I examine whether the difference in aid offers changed the composition of students enrolling at UVa. Table 3 contains results of the baseline balance test with the visual representation in the Appendix. Column 1 and 2 reports estimates from equation 2), where the outcome is the baseline characteristic listed in each row. Column 1 includes the bandwidth

[31] The sample is students within a bandwidth of 200% of the FPL.

restricted sample, and column 2 includes the survey respondents for the bandwidth restricted sample. While I do not observe intended major, I observe acceptance to the School of Engineering. I find balance on acceptance to the School of Engineering, as well as balance on passing the AP economics and calculus exams, which could indicate a balance of interest in business or economics. Tests for balance on academic background show no difference in performance on college entrance exams or acceptance as a scholar. There is an imbalance in family assets, where students above the threshold have on average $58,000 more in assets. The difference in assets, however, is driven by outliers because there is balance in assets if I condition the sample on having assets less than $1 million. As further evidence of outliers, the share of students with assets greater than $75,000 (the requirement for high-need status) is smooth across the threshold. Column 2 shows an imbalance on race, where students above the threshold are more likely to be white, and less likely to be Asian. While not presented in the table, the survey response rate did not differ (statistically or meaningfully) across the threshold. In the outcome analysis, I show the results are robust to the inclusion of baseline covariates.

### 1.6.2 Difference-in-Differences Identifying Assumptions

The main identifying assumption for the DD approach is that the change in outcomes after the policy change for regular-need students is a good counterfactual for the difference in outcomes for high-need students in the absence of the policy change. Although this "parallel trends" assumption is not directly testable, below, I use an event study approach to test for differential pre-trends in the outcomes of interest. As with the RD approach, there is the potential for selection bias if the policy change impacted who enrolled at UVa. Below, I also test for test differences in the composition of students after the policy change.

I conduct an event-study analysis to test for a common pre-trend in the outcomes of interest. Instead of including the interaction of *high need* and *post* indicators in equation 4), I fully interact *high need* with the set of cohort indicators.[32] The interpretation of the coefficients for the years before the policy change is the effect of policy change before the enactment of the policy. The pre-period coefficients should be equal to zero if the outcomes of the high- and

[32] The year prior to the policy change is the omitted year.

regular-need students are trending together before the policy change. Figure A3 presents the event study results. The top panel shows no differential pre-trend in the decision to enroll at UVa and a small statistically insignificant decrease in enrollment after the policy. Estimates from the baseline specification of equation 4) reveal a 2.5 percentage point decrease in the probability of enrolling at UVa. In terms of a change in net price, the DD estimates are similar to the RD estimate reported in the previous section. In further support of the common pre-trend, the bottom panels of Figure A3 show that trend in credits and GPA after one year in college is similar for high- and regular-need students.

I next test for compositional changes in who matriculates to UVa. Column 3 of Table 3 includes estimates of $\beta_3$ from equation 4) where a given demographic is the outcome. This test reveals several changes in the composition of students. High-need students were more likely to be female, less likely to take the AP Economics exam, and score lower on SAT. The differences in gender and interest in economics would likely bias my results for majoring in business or economics towards finding no effect. If SAT scores are positively correlated with GPA, then the difference in SAT scores could upward bias my estimates of the effect of debt on GPA. In the subsequent analysis, I include baseline covariates to control for these observable differences. There could be unobservable differences, however, in the high-need students who matriculated after the policy change that I cannot control for.

## 1.7. Results

### 1.7.1 Variation in Student Loan

I start by showing the variation in student loan debt at the income threshold. While the difference in cumulative debt is the ideal for interpreting differences in outcomes at the end of college, I have access only to first-year loan debt. After presenting results for first-year loan debt, I discuss estimating the difference in cumulative debt.

Figure 4 shows the relationship between family income and first-year financial aid outcomes. The point estimates and standard errors for $\beta_1$ from equation 2) are presented in Table 4. While I do not observe regular- or high-need status, I can determine who satisfies the requirements. I find that 20% of the students who are below the income threshold do not meet the asset requirement, so regular-need status increases from 20% to 100% at the income threshold. The top panels of Figure 4 shows that the change in regular-need status at the threshold provides variation in the composition of financial aid offers. Students above the threshold received $3,200 less grant aid, which was offset by an increase of $2,750 in loan aid. Across specifications, the difference in loan aid ranges from $2,500 to $3,000.

The difference in debt at the threshold is less than the maximum annual debt limit for regular-need students set by the AccessUVa guidelines for several reasons. First, merit aid and outside scholarships can displace loan-aid for regular-need students. Second, regular-need students can borrow less than the amount offered. Third, high-need students can choose to borrow to cover their expected family contribution. Finally, not all students below the income threshold are high-need.

To examine the effect of total debt on later academic and post-graduation outcomes, I must estimate the difference in cumulative debt. The most straightforward method is to assume that the difference in first-year debt is the same for subsequent years. In this case, the expected difference in total loan debt at the threshold would be 4*2,743= $10,972. Assuming a constant difference, however, will underestimate the true difference because UVa can offer a higher level of loans to reach UVa's loan cap for juniors and seniors without reaching the annual federal loan cap. Specifically, UVa can only offer first-year students $5,500 in federal loans, which is *less than* UVa's loan cap of 25% of the in-state cost of attendance. In subsequent years, however, UVa can offer students loans *equal to* 25% of the in-state cost of attendance (between $6,000 and $6,500 during my sample period) without surpassing the federal limit. This method will also underestimate the true difference if upperclassmen face a higher net price and students above the threshold borrow more to cover the higher price.[33] Using a constant difference across years will lead to an overestimate of the true difference if students move across the threshold in subsequent years. In Figure A4, I show there is movement across the threshold during the four years of colleges. At the threshold, students who were above the threshold when applying to UVa, were above the threshold for an average of three years of college, while students who were originally below the threshold were above for an average of one year. The movement across the

[33] A 2017 report by the National Association of College and University Business Officers showed that tuition discounting is highest for freshmen.

threshold suggests that the difference in total debt is less than $11,000. Given the uncertainty about the differences in total debt, I rely on the reduced form estimates from equation 2).

The bottom right panel of Figure 4 displays the variation in student loan debt I use in the DD approach. The figure shows that for the post-policy cohorts, the average loan aid for students below the threshold is $2,000 higher than the pre-policy levels and unchanged for students above the income threshold. Columns 3 and 4 of Table 4 contain the DD estimates of changes in first-year financial aid from equation 4). The increase in loans did not perfectly offset the $3,200 decreases in grant aid. To fill the remaining gap in aid, UVa offered students work study. I do not observe who accepted the work-study, but the addition of it in the offers means that I am estimating the combined effect of switching grants to student loan debt and work-study with the DD approach.

### 1.7.2 Effect of Debt on Academic Performance

The interpretation of any differences in post-graduation outcomes depends on whether debt impacts students in college. If a higher level of debt reduces the chance of graduating or lowers a student's GPA, then differences in early-career decisions could be due to lower academic qualifications as opposed to credit constraints after college. Since students can use grants and loans to reduce hours worked while in college, students near the threshold should have a similar amount of time to spend on class work, and we should expect no difference in academic performance. To test this mechanism, later in the section, I will examine differences in employment levels. However, I do not have robust data on hours worked to make any definitive statements.

In Table 5, I present results for a variety of academic performance measures estimated using equation 2). Column 1 presents the mean of a given outcome for students below the threshold, and column 2 presents the difference in the outcome at the threshold. While not presented, there were no differences in the probability of re-enrolling in college for a second, third, or fourth year. Column 2 shows small positive but statistically insignificant differences in credits accumulated, and no differences in cumulative GPA.[34] There is also no difference in the probability of earning a degree within four years. Columns 3-6 of the table shows these results are robust across different specifications, and Figure 8 shows the results are robust to bandwidth selection around the optimal bandwidth.[35] The null impacts on academic performance are consistent with recent work showing that the composition of aid offers—loans versus grants—has no impact on academic performance (Clotfelter et al., 2017; Park and Scott-Clayton, 2018).

In Table 6, I report difference-in-differences estimates of the effect of the policy change on academic performance. Given the compositional changes of the high-need sample after the policy change, my preferred specification is column 3, which includes baseline demographic controls. Recall, the policy change affected all students below the threshold, not just those near the income threshold. As a result, any differences between the RD and DD estimates could reflect heterogeneity in impacts by income. Consistent with the RD results, Table 6 shows nearly identical credits accumulated or GPA through two years of college. I can rule reductions larger than 1.5 credits accumulated and 0.07 GPA.

### 1.7.3 Effect of Debt on Major Choice

To understand how debt affects post-graduation outcomes, I start by examining major choice. If students anticipate they will be credit constrained after graduation or are averse to holding debt, then students above the threshold might major in more lucrative careers relative to students below the threshold. Figure 5 displays the reduced form effect of student debt on major choice. I find that students above the threshold are 5.7 percentage points (50%) more likely to major in business or economics. Table 5 includes the corresponding results across multiple specifications. The IV estimates in column 7 of Table 5 show that an additional $1,000 in student loan debt increases the probability a student majors in business or economics by 0.5 percentage points. This is likely an underestimate because I am likely overestimating the cumulative difference in total loan debt. Figure 8 shows that the difference in majoring in business or economics is around 6.5 percentage points in the neighborhood of the optimal bandwidth. For bandwidths larger than

[34] To estimate differences in GPA at a given point in college, I condition on being enrolled because only these students have a GPA. Since there is no difference in persistence between high- and regular-need, conditioning on enrollment does not introduce selection bias.
[35] The visual representation of the results are available upon request.

200%, the difference is around 3.5 percentage points. As discussed in Section 6.1.3, the results for wider bandwidths are likely biased because of misspecification. While imprecise, the point estimates suggest that business and economics are pulling from STEM fields as opposed to other social sciences or the humanities. Columns 3-6 of Table 5 and Figure 6 and 8 show the major choice results are robust across different bandwidths and specifications. In wider bandwidths, the difference in majoring in economics or business is slightly larger at 6.5 percentage points.

Table A1 presents the evolution of treatment effects over a student's academic career. This analysis reveals two main findings. First, the difference in majoring in business or economics increases as students get closer to graduation. At the start of the third year, which is the first time a student can declare business as a major, regular-need students at the threshold were 2.8 percentage points more likely to major in business or economics. By the start of the fourth year, the effect is 5.3 percentage points. Second, the increase in the effect across semesters is due to more students majoring in economics. The difference in majoring in business is constant across semesters at approximately 2.2 percentage points, while the effect on majoring in economics increases from 0.7 to 3.6 percentages points between the start of the third and fourth year. Examining the changes in economics majors between the third to fourth year reveals that approximately 30 percent of students were undeclared at the start of the third year, while the remaining 70 percent had a different major. This pattern of results is consistent with some students choosing economics after being rejected at the business school, as well as students choosing economics as a second major as they get closer to applying for jobs. In fact, column 6 shows that the difference drops to from 5.7 to 3.6 percentage points when using a student's final first major.

I also examine differences in college work experience. This analysis is underpowered because the outcome measures are available for a subset of the cohorts in my sample. As such, these results prove nothing more than suggestive evidence. Table A2 presents differences in college work experiences at the threshold. While I do not observe hours worked during the school year, students above the threshold are more likely to have a part-time and hold a summer job. The type of work experience students had during college appears to differ at the threshold. Regular-need students at the threshold were more likely to have an internship and less likely to have experience as a research assistant and job-shadowing. Regular-need students at the threshold were also more likely to report using services at the career center. We might expect this pattern of results if higher levels of debt caused students to forego graduate school in favor of working immediately after college.

The differences in major choice presented above are consistent with a story of credit constraints or debt aversion. While STEM fields lead to well-paying jobs, business and economics majors tend to have higher initial base salaries. Based on results from a survey of post-graduation from the graduating Class of 2016, the average base salary for undergraduate business majors was just above $72,000 with an average signing bonus of over $9,000 and an estimated annual bonus of $24,000. The average base salary among graduates of the College of Arts and Sciences and College of Engineering is roughly $50,000 and $70,000, respectively.[36] Given the quantitative skills required for the undergraduate business school or a major in economics, we might expect business and economics to draw from other quantitative fields as opposed to the humanities, even though the humanities lead to less lucrative jobs than STEM fields. The undergraduate business school requires students to take calculus and statistics as well as accounting and economics in their first two years before applying, and students must take statistics and calculus II before declaring economics as their major. [37]

### 1.7.4 Effect of Debt on Post-Graduation Plans

Table 7 examines the impact of student loan debt on a student's post-graduation plans. Column 2 reports the point estimates, and Figure 7 provides the visual representation of those results. I find that regular-need students at the income threshold were 12.2 percentage points (21%) more likely to have accepted a job. Column 7 contains the IV estimates and shows that a $1,000 in student loan debt increases the probability a student is employed immediately after college by 0.94 percentage points. The increase in planned employment resulted in equally sized decreases in attending graduate school and seeking employment, but these differences are not statistically significant. Columns 3-5 show that the main results are robust across different specifications and bandwidth selections. The RD plots in Figure 7 reveals a fair amount of noise in the outcome, especially in among income bins near 150% below the threshold. This noise

[36] Sources: https://www.commerce.virginia.edu/sites/default/files/CCS-Documents/DestinationsReport2016.pdf https://career.virginia.edu/sites/career.virginia.edu/files/Destinations%20Brochure-2017-EMAILandWEB.PDF
[37] See http://economics.virginia.edu/major and https://www.commerce.virginia.edu/undergrad/prerequisites

originates from a smaller sample of students who completed the exit survey, and as Figure 2 shows, there are very few students with income near 150% below the threshold, which is not captured in the Figure 7. Figure 8 illustrates the difference in planned employment is stable across different bandwidths around the optimal bandwidth. For bandwidths of greater than 200%, which is double the optimal bandwidth, the difference trends towards zero and becomes insignificant. The results using the wider bandwidth, however, are likely biased because the linear specification is less likely to be accurate.

Turning to occupation choice, I find that regular-need students at the threshold were 9.4 percentage points (22%) more likely to accept a job in a high-paying industry (finance, consulting, IT & engineering). There was a small negative but statistically insignificant impact on working in a public-interest industry (education, government, NGO, service/volunteering).[38] This pattern of results suggests that higher levels of debt induced more students to pursue a job in a high-paying industry instead of going to graduate school or spending more time searching for employment. Columns 3 of Table 7 demonstrates that the results are robust to the inclusion of individual baseline characteristics. However, for wider bandwidths (column 4 and 5), the effect on accepting a job in a high-paying industry falls to between 6.1 and 7.1 percentage points and is no longer statistically significant, which is summarized inf Figure 8. The difference in working in a high-paying industry is highest at the optimal bandwidth, and hovers above 6 percentage points the neighborhood around the optimal bandwidth. As I show in the next section, the overall effect is masking considerable heterogeneity by gender.

I next examine the role of major choice in the post-graduation outcomes. As demonstrated earlier in the section, regular-need students at the threshold were more likely to major in business or economics. While not presented in the paper, I find that differences in major choice for the sample of survey respondents are similar to the estimates presented in the paper.[39] The similarity of the results across samples suggests that differences in the composition of survey respondents are not driving the post-graduation effects. To examine whether the effects on occupation choice are due to within- or across-major differences in occupation choice, I re-ran the analysis presented above controlling for major choice. Column 6 of Table 7 shows that controlling for

[38] These results are *not* conditional on accepting any job after college because the above results show that debt impacted the decision to accept a job immediately after college.
[39] Results are available upon request.

major choice reduces the effects across most outcomes. For example, the difference in accepting a job in a high-paying industry falls from 9.0 to 5.6. The decrease in the effect provides evidence that effects on major choice can explain some of the differences in occupation choice, but that within-major differences in occupation choice are an important channel through which debt impacts occupation choices.

### 1.7.5 Subgroup Analysis

Given gender gaps in undergraduate economics and business majors (e.g., Ball, 2012; Goldin, 2015) and differences in responses to college interventions by gender (e.g. Angrist, Lang, & Oreopoulos, 2009; Angrist & Lavy, 2009), I examine effects by gender. As concluded by Goldin (2015), the gender difference in the decision to major in economics is somewhat of a mystery. Since females are less likely to major in economics, we might expect a smaller response in major choice to higher debt. Table 8 contains the RD estimates by gender and shows that males drove the impacts on major and occupation choice.[40] Specifically, the effect on majoring in business or economics was 13 percentage points for males and 1 percentage point for females. Perhaps females are changed their major within STEM, but I cannot examine this margin given my coarse measure of major choice. The effects for males carried through to initial occupation choice by shifting males between high- and low-paying occupations. Specifically, the effect on accepting a job in a high-paying occupation was 16 percentage points, while the effect on accepting a job at a public-interest occupation was negative 12 percentage points. Overall, there was little difference for males in the decision to attend graduate school or to take a job immediately after college.

Table 8 also shows that females responded very differently to student loan debt than males. There was little difference in major choice or the decision to work in a high-paying occupation. The results suggest, however, that females responded to higher levels of student loan debt by trying to graduate sooner and immediately join the workforce. Gender differences could be due to differences in debt aversion, however, first-year borrowing does not differ by gender and

[40] I find no evidence of differential selection into UVa by gender. The enrollment decision for both groups was unaffected by the change in the composition of financial aid at the threshold, and there is good balance on baseline covariates among the sample who matriculated to UVa.

recent research shows that females are less likely to be debt averse than males (Boatman, Evans, & Soliz, 2017). Females at the threshold were 14.7 percentages points (20 percent) more likely to declare a major before the beginning of the third year. They also completed 3 more credits by the end of the second year enrolled. This pattern of results suggests that the increase in debt is driving females to graduate on-time to avoid taking on additional debt. Females at the threshold are 5.5 percentage points more likely to graduate within four years. This difference, however, is not statistically significant. After college, females were 19.5 percentage points more likely to accept any job, 9.2 percentage point less likely to still be seeking employment, and 10.3 percentage points less likely to attend graduate school. Given the small sample, the effect on graduate school enrollment and still seeking employment are not statistically significant.

## 1.8 Conclusion

In this paper, I show that higher student loan debt leads low-income students at a selective college to alter their career path before reaching the labor market. Specifically, higher student loan levels caused more individuals to major in business and economics and subsequently accept a job in a high-paying occupation. I find suggestive evidence that the increase in business and economics majors shifted students out of STEM fields and lead students to forgo graduate school immediately after college. The magnitude of the effects on major and occupation choice is larger than predicted by income effects. I interpret these results as students anticipating being credit constrained after college or are averse to holding debt.

Current loan repayment policies provide the foundation for reducing the unintended consequences of student loan debt on occupation choice but should be improved. If individuals are credit constrained after college, then income-based repayment can lower the monthly payments for borrowers who are unable to afford the payments under the standard plan. By lowering an individual's monthly payments, income-based repayment can increase their consumption and reduce the need to choose a higher paying job. Choosing an income-based plan, however, is complicated and misaligned with when individuals start to make choices about their career path (see Baum and Chingos, 2017 for recommendations on reducing the complexity of choosing a repayment plan). Currently, students choose a repayment plan when they exit college. However, if students alter their career plans before graduation, then waiting till after graduation to learn about repayment options is too late.[41] Improving information about repayment options during the mandatory entrance counseling and throughout college could provide students with a better understanding of their expected loan payments under different plans and career choices.[42]

I also show that debt does not affect academic attainment or performance. In my setting, the counterfactual differed from the treated students only in their composition of financial aid offers, and not the total amount of aid offered. Once enrolled, if loans and grants both allow students to reduce their employment during the school year, then the difference in the composition of loans and grants should not affect performance, which is what I find. These results are consistent with other work showing that the provision of grant aid alone does not increase academic performance (Clotfelter et al., 2017). Increasing spending on student supports could improve the efficacy of financial aid policies as shown in recent research (Scriver et al., 2015; Page et al., 2017; Deming & Walters, 2018).

[41] If debt impacts occupation choice but not major choice, then learning about repayment options after graduation is too late if students search for jobs while still in college.

[42] Research by NERA Economic Consulting (2012) finds that 40% of borrowers surveyed had no memory of going through mandatory student loan counseling. See Sheets and Crawford (2014) for recommendations about improving information at the point of loan origination and during college.

Table 1: Summary Statistics

| | All UVa Matriculates | RD Sample | | DD Sample |
|---|---|---|---|---|
| | | Full | Restricted | |
| | (1) | (2) | (3) | (4) |
| A. Demographic | | | | |
| In-state | 0.71 | 0.69 | 0.75 | 0.68 |
| Female | 0.56 | 0.56 | 0.54 | 0.57 |
| White | 0.62 | 0.50 | 0.37 | 0.49 |
| AP exams passed | 5.09 | 4.40 | 3.90 | 4.46 |
| Max SAT score | 1349 | 1305 | 1274 | 1308 |
| B. Financial | | | | |
| AccessUVa-eligible | 0.54 | 1.00 | 1.00 | 1.00 |
| Regular-need cond. on AccessUVa | 0.89 | 0.82 | 0.60 | 0.81 |
| Income (1,000s) cond. on CSS | 156 | 97 | 50 | 94.1 |
| Pell-eligible cond. on FAFSA | 0.20 | 0.39 | 0.86 | 0.39 |
| C. Outcomes | | | | |
| Borrowed in first year | 0.31 | 0.63 | 0.52 | 0.65 |
| First-year loan aid | 3113 | 5781 | 3385 | 5937 |
| Persist through 3rd year | 0.88 | 0.85 | 0.86 | |
| GPA after 3rd year | 3.32 | 3.23 | 3.16 | |
| Major in Business or Economics | 0.18 | 0.13 | 0.11 | |
| Major in STEM | 0.43 | 0.45 | 0.45 | |
| Major in other field | 0.49 | 0.50 | 0.52 | |
| Plans employment | 0.66 | 0.64 | 0.60 | |
| Plans graduate school | 0.20 | 0.19 | 0.20 | |
| High-paying industry (consulting, finance, IT & engineering) | 0.30 | 0.25 | 0.23 | |
| Public-interest industry (education, government, NGO, service/volunteering) | 0.12 | 0.14 | 0.14 | |
| Observations | 22,898 | 5,338 | 1,422 | 7,056 |

Notes: The table contains mean values for baseline demographic variables and selected outcomes. Column (1) includes all matriculates from the 2009-2015 entering cohorts. Column (2) limits the sample to students eligible for need-based aid under AccessUVa (see Section III for details) from the 2009-2013 cohorts. Column (3) further limits the sample to students within 75% of the high-need income threshold. Column (4) contains the AccessUVa-eligible students with unmet need from the 2009-2015 entering classes. Post-graduation outcomes are available for approximately 60% of graduates from the 2009-2013 cohorts.

Table 2: RD Estimate of the Effect of Net Price on Enrollment

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Regular-Need | -0.000 | -0.002 | -0.026 | -0.007 | -0.029 | -0.008 | |
| | (0.030) | (0.041) | (0.038) | (0.028) | (0.041) | (0.057) | |
| Net Price ($1,000s) | | | | | | | -0.008 |
| | | | | | | | (0.012) |
| Observations | 9,355 | 2,051 | 2,019 | 3,669 | 3,669 | 2,019 | 2,019 |
| R-squared | 0.263 | 0.009 | 0.166 | 0.178 | 0.178 | 0.167 | 0.167 |
| Bandwidth | Full | 75% FPL | 75% FPL | 150% FPL | 150% FPL | 75% FPL | 75% FPL |
| Polynomial Form | Quadratic | Linear | Linear | Linear | Quadratic | Quadratic | Linear |
| Individual Controls | X | | X | X | X | X | X |

Notes: Columns 1-6 shows the difference in the probability of matriculating to UVa at the high-need income threshold across different specifications. The estimate reported is β1 from equation 2) with the bandwidth and functional form assumption listed at the bottom of each column. Column 7 contains the IV estimate of a $1,000 increase in net price on the probability of attending UVa where net price is instrumented by regular-need status. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

Table 3: Covariate Balance Test

| | RD Sample | | | | | | DD Sample | | |
|---|---|---|---|---|---|---|---|---|---|
| | Restricted | | | Survey | | | | | |
| | (1) | | | (2) | | | (3) | | |
| Black | -0.049 | (0.040) | | 0.013 | (0.047) | | 0.011 | (0.024) | |
| Asian | 0.004 | (0.047) | | -0.073 | (0.055) | | -0.028 | (0.029) | |
| Hispanic | 0.008 | (0.033) | | -0.052 | (0.040) | | 0.026 | (0.021) | |
| Multi-Race | -0.026 | (0.025) | | -0.037 | (0.027) | | -0.001 | (0.014) | |
| Unknown | 0.019 | (0.022) | | 0.017 | (0.024) | | -0.010 | (0.012) | |
| White | 0.037 | (0.051) | | 0.129 | (0.059) | ** | 0.002 | (0.030) | |
| Female | 0.013 | (0.053) | | 0.057 | (0.062) | | 0.047 | (0.032) | |
| In-State | 0.044 | (0.046) | | 0.009 | (0.057) | | -0.018 | (0.030) | |
| Above Asset Requirement | 0.007 | (0.040) | | 0.010 | (0.048) | | -0.003 | (0.024) | |
| Assets ($1,000s) | 59.1 | (31.385) | * | 68.322 | (48.317) | | 18.992 | (12.258) | |
| EFC | 632 | (466.102) | | 951 | (662.679) | | -437.002 | (367.895) | |
| Family Size | 0.144 | (0.139) | | 0.228 | (0.161) | | 0.011 | (0.086) | |
| Scholar | 0.012 | (0.020) | | 0.036 | (0.026) | | -0.008 | (0.011) | |
| SAT | 1.353 | (16.596) | | -0.795 | (18.861) | | -20.731 | (10.121) | ** |
| AP exams passed | 0.424 | (0.397) | | 0.722 | (0.471) | | -0.126 | (0.317) | |
| Passed AP Calculus | -0.022 | (0.048) | | 0.030 | (0.058) | | 0.020 | (0.038) | |
| Passed AP Econ | -0.018 | (0.026) | | 0.003 | (0.034) | | -0.034 | (0.020) | * |
| Engineering School | 0.022 | (0.041) | | -0.040 | (0.046) | | 0.026 | (0.024) | |
| Observations | 1422 | | | 1003 | | | 7056 | | |

Notes: Each row represents a separate test for the change in the relevant demographic characteristic at the high-need threshold. Column 1 contains the bandwidth-restricted RD sample of 75%, columns 2 contains the survey respondents from the bandwidth-restricted sample of 100%, and column 3 the DD sample. Column 1 and 2 displays the point estimates for β1 and robust standard errors from equation 2), and column 3 displays the point estimates for β3 and robust standard errors from equation 4). (*** p<0.01, ** p<0.05, * p<0.1).

Table 4: First-Year Financial Aid

| | RD Estimate | | DD Estimate | | Control Mean |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| A. Regular-Need | 0.81*** | 0.80*** | -0.02 | -0.02 | 0.15 |
| | (0.03) | (0.03) | (0.02) | (0.02) | |
| B. Grant Aid Accepted | -5324.35*** | -3168.07*** | -2396.75** | -3175.60*** | 27554 |
| | (1298.00) | (756.53) | (1118.52) | (645.98) | |
| C. Loan Aid Accepted | 2759.09*** | 2742.07*** | 1938.97*** | 2019.02*** | 1335 |
| | (389.25) | (384.23) | (423.43) | (417.23) | |
| Bandwidth | 75% FPL | 75% FPL | | | |
| Polynomial Form | Linear | Linear | | | |
| Individual Controls | | X | | X | |
| Observations | 1412 | 1412 | 7056 | 7056 | 671 |

Notes: The table shows the difference in first-year financial aid outcomes. Column 1 and 2 contains point estimates for β1 from equation 2) using a bandwidth of 75% for the RD sample. Column 3 and 4 contains point estimates for β3 from equation 4) using the DD sample. Column (5) contains the control group mean for a given outcome. Robust standard errors in parenthesis. ( *** p<0.01, ** p<0.05, * p<0.1)

Table 5: RD Estimates of the Effects of Debt on Academic and Major Choice Outcomes

| | Control Mean | RD | | Robustness Checks | | | | | | | | RD-IV | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome | (1) | (2) | | (3) | | (4) | | (5) | | (6) | | (7) | |
| Credits after 1st year | 30 | 0.195 | (0.507) | 0.08 | (0.504) | 0.47 | (0.427) | 0.33 | (0.356) | 0.23 | (0.379) | 0.007 | (0.046) |
| Credits after 2nd year | 60 | 1.179 | (1.150) | 0.84 | (1.157) | 1.89 | (1.009) * | 1.14 | (0.842) | 1.17 | (0.844) | 0.076 | (0.105) |
| Credits after 3rd year | 87 | 1.132 | (1.838) | 1.16 | (1.839) | 2.16 | (1.633) | 1.25 | (1.379) | 1.36 | (1.365) | 0.104 | (0.167) |
| Credits after 4th year | 110 | 1.263 | (3.189) | 1.30 | (3.186) | 2.97 | (2.802) | 2.74 | (2.313) | 2.37 | (2.347) | 0.116 | (0.286) |
| GPA after 1st year | 3.09 | -0.026 | (0.058) | -0.04 | (0.050) | -0.04 | (0.044) | -0.03 | (0.036) | -0.07 | (0.038) * | -0.003 | (0.005) |
| GPA after 2nd year | 3.11 | 0.045 | (0.052) | 0.04 | (0.045) | 0.03 | (0.040) | 0.00 | (0.033) | -0.03 | (0.035) | 0.003 | (0.004) |
| GPA after 3rd year | 3.15 | 0.055 | (0.055) | 0.04 | (0.047) | 0.00 | (0.041) | -0.02 | (0.034) | -0.01 | (0.037) | 0.003 | (0.004) |
| GPA after 4th year | 3.20 | 0.074 | (0.062) | 0.05 | (0.056) | -0.01 | (0.048) | -0.02 | (0.039) | -0.01 | (0.042) | 0.004 | (0.005) |
| Earned BA in < 4 years | 0.13 | 0.013 | (0.041) | - 0.010 | (0.039) | -0.027 | (0.033) | - 0.034 | (0.027) | -0.003 | (0.032) | -0.001 | (0.003) |
| Earned BA within 4 years | 0.85 | 0.014 | (0.041) | 0.011 | (0.042) | 0.031 | (0.037) | 0.018 | (0.030) | 0.010 | (0.032) | 0.001 | (0.004) |
| Declare major early | 0.65 | 0.080 | (0.050) | 0.060 | (0.049) | 0.080 | (0.042) * | 0.011 | (0.035) | 0.025 | (0.039) | 0.005 | (0.005) |
| Double major | 0.24 | 0.013 | (0.048) | 0.019 | (0.048) | 0.039 | (0.042) | 0.008 | (0.035) | 0.003 | (0.037) | 0.002 | (0.004) |
| Major in economics or business | 0.11 | 0.049 | (0.034) | 0.057 | (0.033) * | 0.065 | (0.029) ** | 0.064 | (0.024) *** | 0.065 | (0.025) *** | 0.005 | (0.003) * |
| Major in STEM | 0.45 | -0.049 | (0.055) | - 0.050 | (0.048) | -0.035 | (0.042) | - 0.040 | (0.035) | -0.023 | (0.037) | -0.004 | (0.004) |
| Major in other field | 0.51 | 0.019 | (0.055) | 0.021 | (0.050) | 0.011 | (0.043) | 0.013 | (0.036) | -0.027 | (0.038) | 0.002 | (0.004) |
| Observations | | 1422 | | 1422 | | 1854 | | 2548 | | 5269 | | 1422 | |
| Bandwidth | | +/- 75% FPL | | +/- 75% FPL | | +/- 100% FPL | | +/- 150% FPL | | Full Sample | | +/- 75% FPL | |
| Individual Controls | | | | X | | X | | X | | X | | X | |
| Functional Form | | Linear | | Linear | | Linear | | Quadratic | | Quadratic | | Linear | |

Notes: Each cell in Columns 2- 6 contains estimates of β1 from equation 2), the difference an academic outcome at the income threshold. The relevant outcome is listed as the row header. Each column represents a different specification. Column 7 contains the IV estimate of a $1,000 increase in student loan aid on major choice where student loan aid is instrumented by regular-need status. Column 1 contains the mean among high-need students for a given outcome. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

Table 6: Diff-in-Diff Estimates of the Effects of Debt on Academic Outcomes

| | Control Mean | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | | (3) | |
| Credits after 1st year | 30.3 | -0.372 | (0.280) | -0.18 | (0.341) |
| Credits after 2nd year | 60.6 | 0.226 | (0.750) | 0.27 | (0.732) |
| GPA after 1st year | 3.12 | -0.021 | (0.033) | 0.00 | (0.037) |
| GPA after 2nd year | 3.13 | -0.026 | (0.039) | -0.01 | (0.034) |
| Individual controls | | | | X | |
| Observations | | 7493 | | 7493 | |

Notes: Each cell in Columns 2 and contains β3 from equation 4) across different specifications, where the relevant outcome is listed as the row header. Each column represents a different specification. Column (1) contains the mean among high-need students in the pre-period for a given outcome. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

Table 7: RD Estimates of the Effects of Debt on Career Choices

| | Control Mean | RD | | | Robustness Checks | | | | | | | | | | | | RD-IV | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | | | (3) | | | (4) | | | (5) | | | (6) | | | (7) | | |
| Post-graduation plans: | | | | | | | | | | | | | | | | | | | |
| Employed | 0.59 | 0.122 | (0.062) | ** | 0.134 | (0.061) | ** | 0.102 | (0.051) | ** | 0.141 | (0.068) | ** | 0.11 | (0.060) | * | 0.009 | (0.004) | * |
| Attending/Seeking Graduate School | 0.19 | -0.052 | (0.052) | | -0.06 | (0.052) | | -0.05 | (0.043) | | -0.06 | (0.059) | | -0.04 | (0.051) | | -0.004 | (0.003) | |
| Seeking Job/Undecided | 0.21 | -0.07 | (0.050) | | -0.07 | (0.049) | | -0.05 | (0.042) | | -0.08 | (0.054) | | -0.07 | (0.049) | | -0.005 | (0.003) | |
| Occupation choice: | | | | | | | | | | | | | | | | | | | |
| High-paying industry | 0.22 | 0.094 | (0.053) | * | 0.09 | (0.051) | * | 0.061 | (0.042) | | 0.071 | (0.056) | | 0.058 | (0.047) | | 0.007 | (0.003) | * |
| Public-interest industry | 0.16 | -0.028 | (0.041) | | -0.02 | (0.040) | | -0.04 | (0.035) | | -0.04 | (0.047) | | -0.02 | (0.040) | | -0.002 | (0.003) | |
| Any other industry | 0.22 | 0.056 | (0.054) | | 0.068 | (0.055) | | 0.083 | (0.045) | * | 0.111 | (0.059) | * | 0.071 | (0.055) | | 0.004 | (0.003) | |
| Observations | | 1002 | | | 1002 | | | 1372 | | | 1687 | | | 1002 | | | 1002 | | |
| Bandwidth | | +/- 100% FPL | | | +/- 100% FPL | | | +/- 150% FPL | | | +/- 200% FPL | | | +/- 100% FPL | | | +/- 100% FPL | | |
| Individual Controls | | | | | X | | | X | | | X | | | X | | | X | | |
| Major choice | | | | | | | | | | | | | | X | | | | | |
| Functional Form | | Linear | | | Linear | | | Linear | | | Quadratic | | | Linear | | | Linear | | |

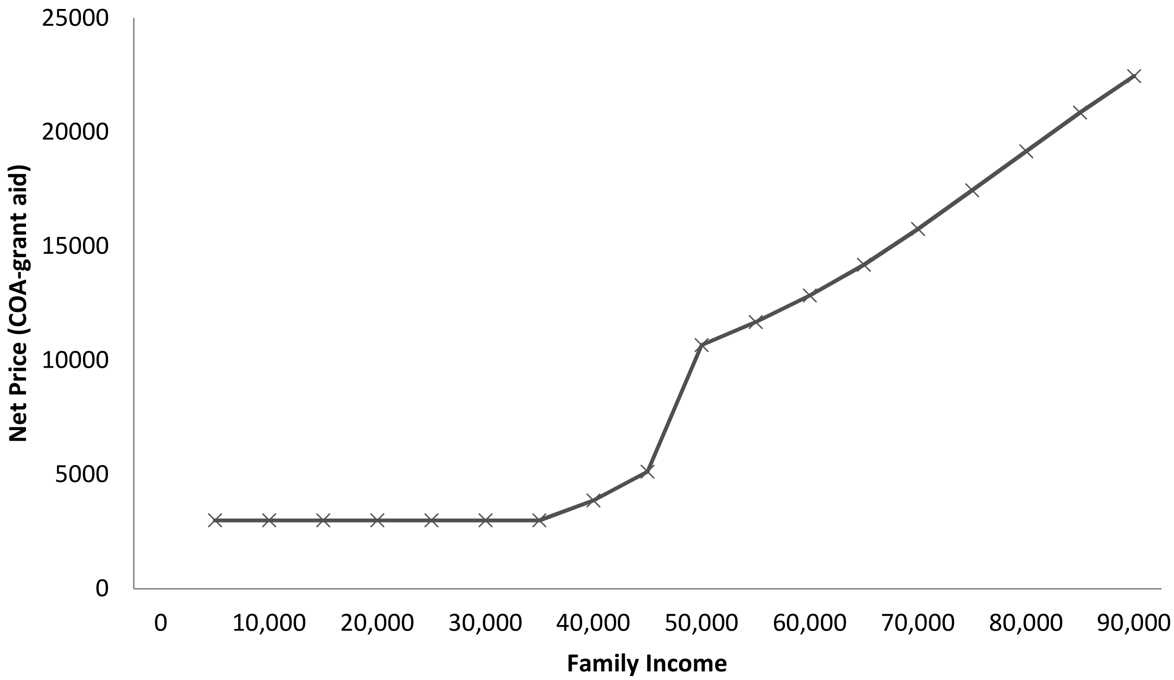
Notes: Each cell in Columns 2- 6 contains estimates of β1 from equation 2), the difference a post-graduation outcome at the income threshold. The relevant outcome is listed as the row header. Each column represents a different specification. Column 7 contains the IV estimate of a $1,000 increase in student loan aid on major choice where student loan aid is instrumented by regular-need status. Column 1 contains the mean among high-need students for a given outcome. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

Table 8: Effects by Gender

| | Male | | Female | |
|---|---|---|---|---|
| | Control Mean | RD Estimate | Control Mean | RD Estimate |
| | (1) | (2) | (3) | (4) |
| **Major choice:** | | | | |
| Econ or Business | 0.14 | 0.114** | 0.07 | 0.013 |
| | | (0.048) | | (0.034) |
| STEM | 0.55 | -0.063 | 0.38 | -0.014 |
| | | (0.062) | | (0.059) |
| Other | 0.39 | 0.008 | 0.62 | 0.024 |
| | | (0.064) | | (0.059) |
| **On-time completion:** | | | | |
| Declare major early | 0.68 | -0.001 | 0.66 | 0.148** |
| | | (0.060) | | (0.058) |
| Credits after year 2 | 61.18 | 0.222 | 60.05 | 3.006** |
| | | (1.607) | | (1.260) |
| Graduate within 4 years | 0.87 | -0.014 | 0.83 | 0.055 |
| | | (0.058) | | (0.048) |
| **Post-graduation plans:** | | | | |
| Employed | 0.58 | 0.049 | 0.070 | 0.197** |
| | | (0.096) | | (0.083) |
| Attending/Seeking Graduate School | 0.19 | -0.016 | 0.049 | -0.108 |
| | | (0.081) | | (0.072) |
| Seeking Job/Undecided | 0.23 | -0.034 | 0.077 | -0.089 |
| | | (0.075) | | (0.065) |
| High-paying industry | 0.26 | 0.157* | 0.091 | 0.032 |
| | | (0.088) | | (0.062) |
| Public-interest industry | 0.08 | -0.119*** | 0.057 | 0.021 |
| | | (0.045) | | |
| Any other industry | 0.24 | 0.012 | 0.040 | 0.144** |
| | | (0.083) | | (0.073) |
| Observations | | 767 | | 925 |

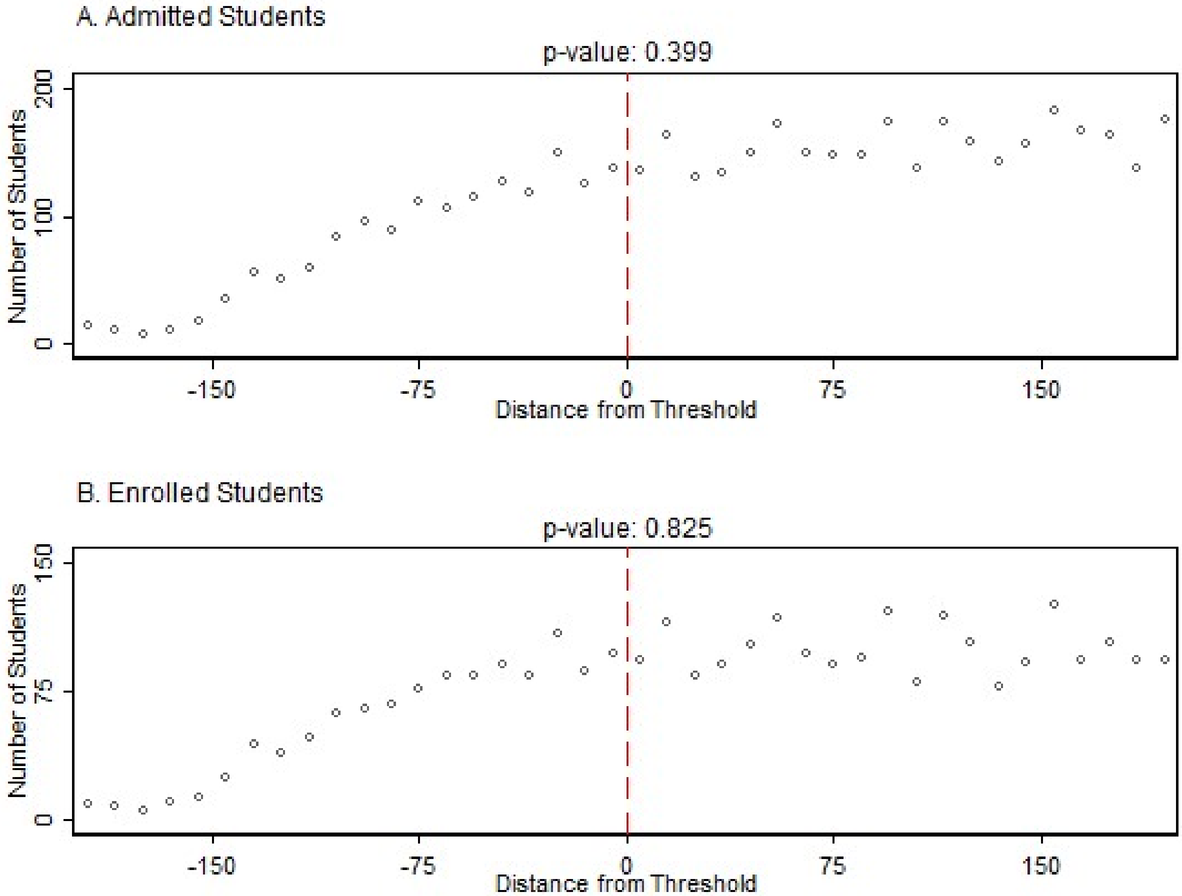
Notes: The table presents results by gender. Column 1 and 3 contains the mean among high-need students for a given outcome. Columns 2 and 4 contains estimates of β1 from equation 2), the difference in an outcome at the income threshold. The relevant outcome is listed as the row header. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

Figure 1: UVa Net Price Profile



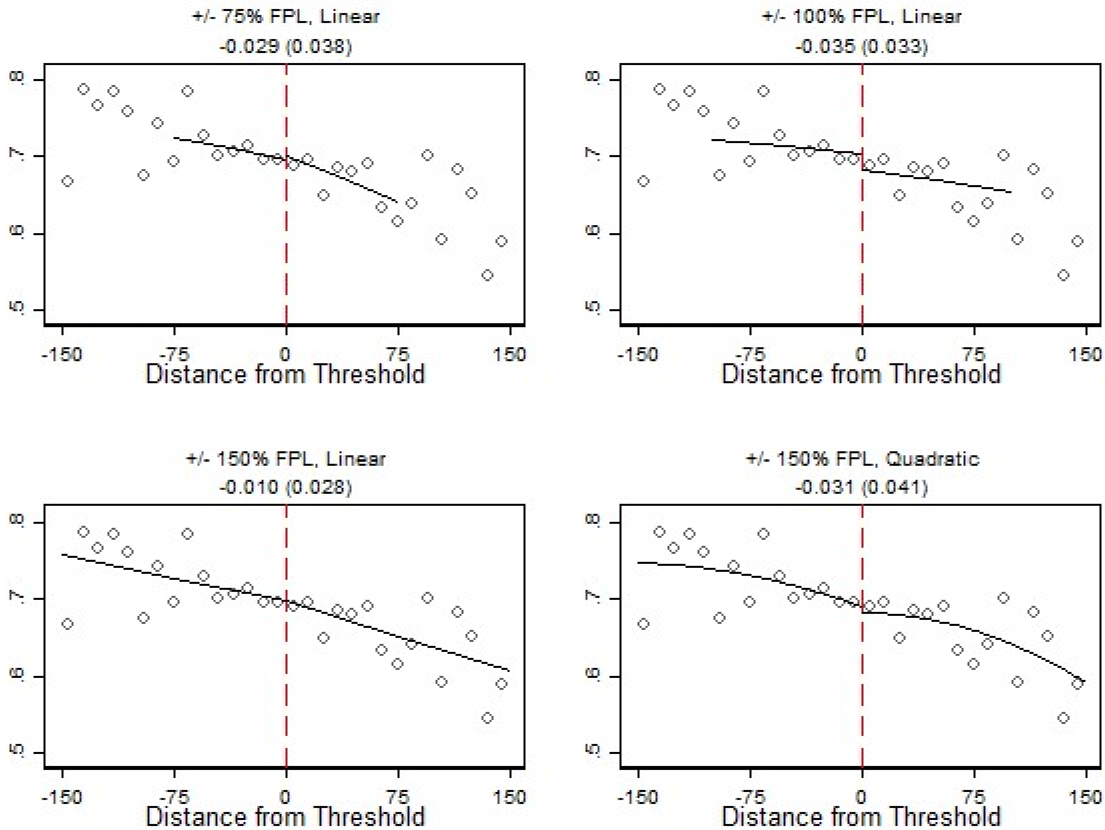

Source: Turner (2014) Notes: Data are collected from institution-specific “net cost calculators”. Data were accessed the week of January 12 and collect data on the net cost for a dependent student with the indicator family income and the following default settings: student born in 1995, U.S. citizen, oldest parent born in 1970, 2 parents + 1 sibling not college age, parents married / student single, in-state residency, 0 student income, and 0 parental assets.

Figure 2: Income Distribution



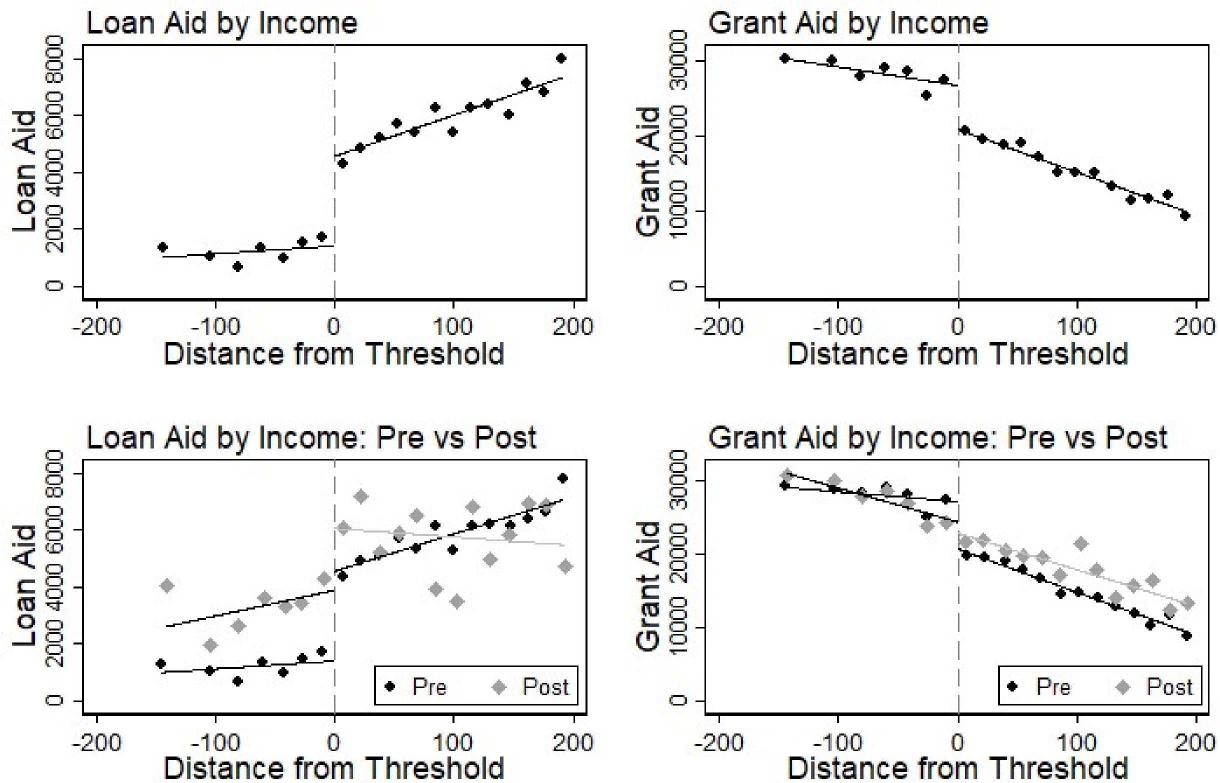

Notes: See text for sample restrictions. The figure shows the density of students within +/- 200% of the relevant federal poverty line given the year and family size. Each marker represents the number of students within a given income bin. The p-value from a test for manipulation using a local polynomial density estimation on the full analytic sample is displayed below the title.

Figure 3: Enrollment



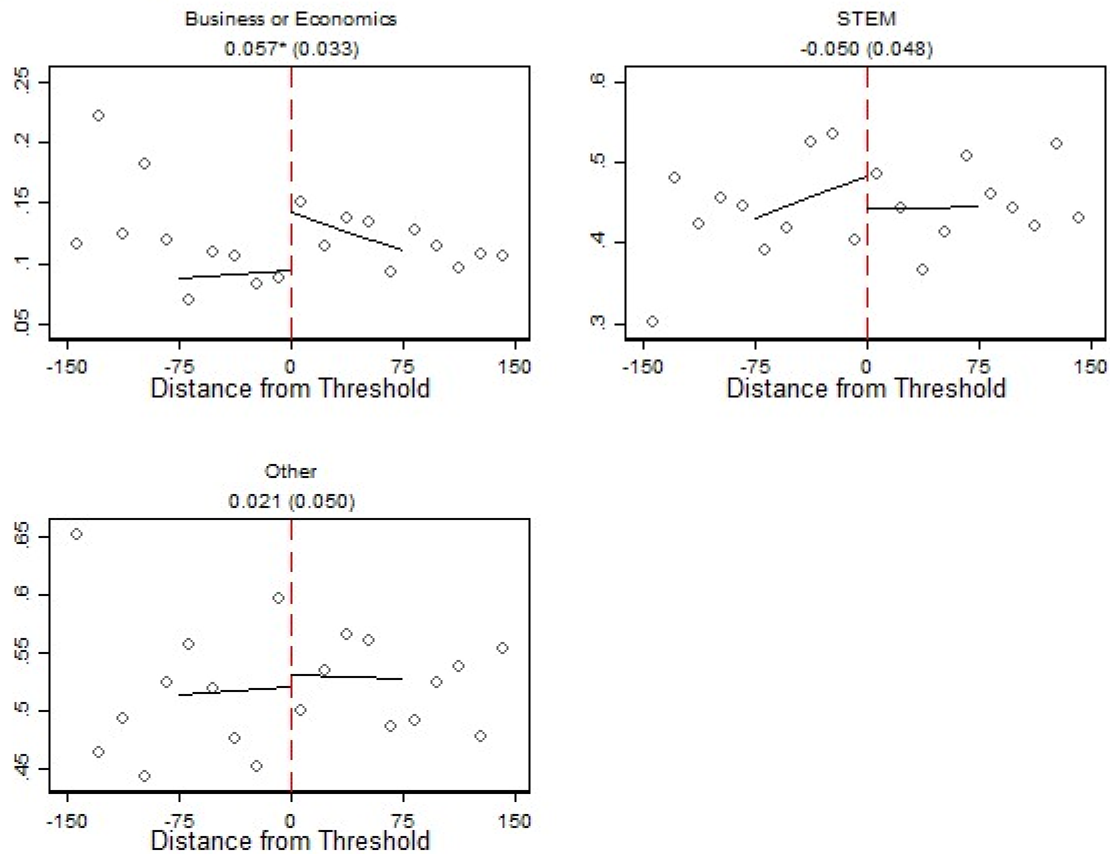

Notes: The figure shows the share of students enrolling at UVa as a function of distance to the high-need income threshold. The solid line represents a linear or quadratic fit of the outcome on standardized income estimated seperately on each side of the threshold. Point estimates for β1 and robust standard errors from equation 2) across multiple bandwidths are reported under the figure heading. See text for sample restrictions.
(*** p<0.01, ** p<0.05, * p<0.1)

Figure 4: First-Year Financial Aid



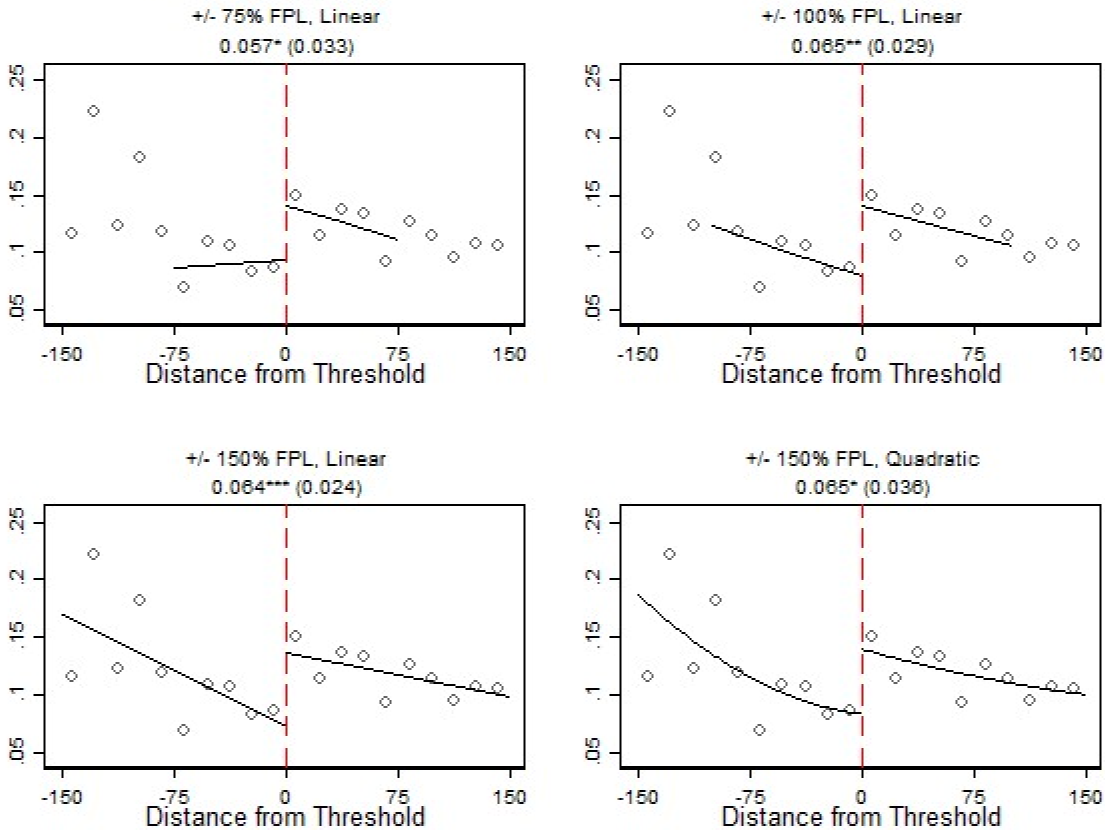

Notes: The figures show first-year student financial aid outcomes as a function of distance to the high-need income threshold. The solid line represents a linear fit of the outcome on standardized income estimated seperately on each

Figure 5: Major Choice



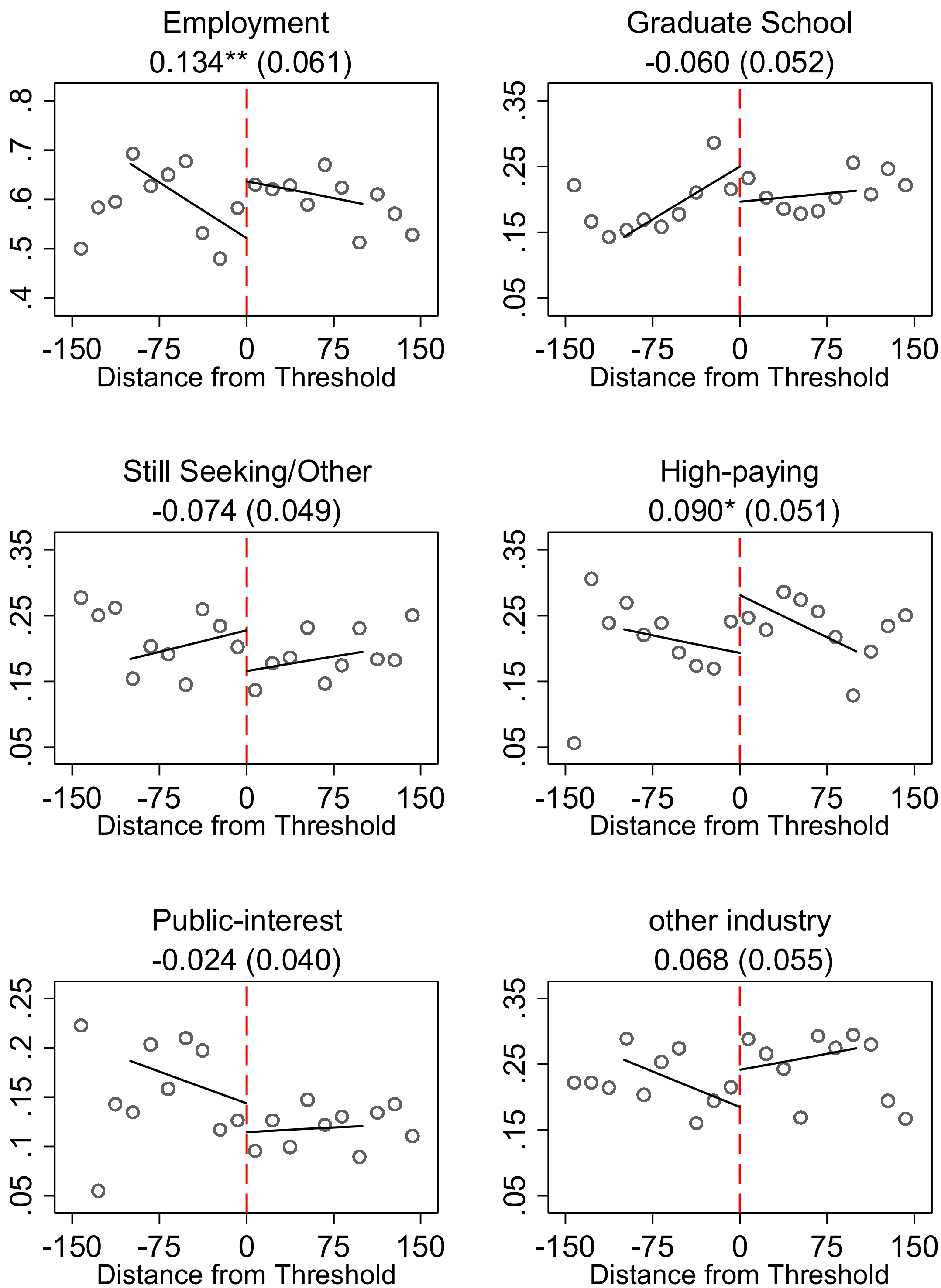

Notes: The figures show the share of students with a given major as a function of distance to the high-need income threshold. The solid line represents a linear fit of the outcome on standardized income estimated seperately on each side of the threshold. Point estimates for β1 and robust standard errors from equation 2) using a bandwidth of 75% are reported under the figure heading. See text for sample restrictions.(*** p<0.01, ** p<0.05, * p<0.1)

Figure 6: Major in Economics or Business Robustness



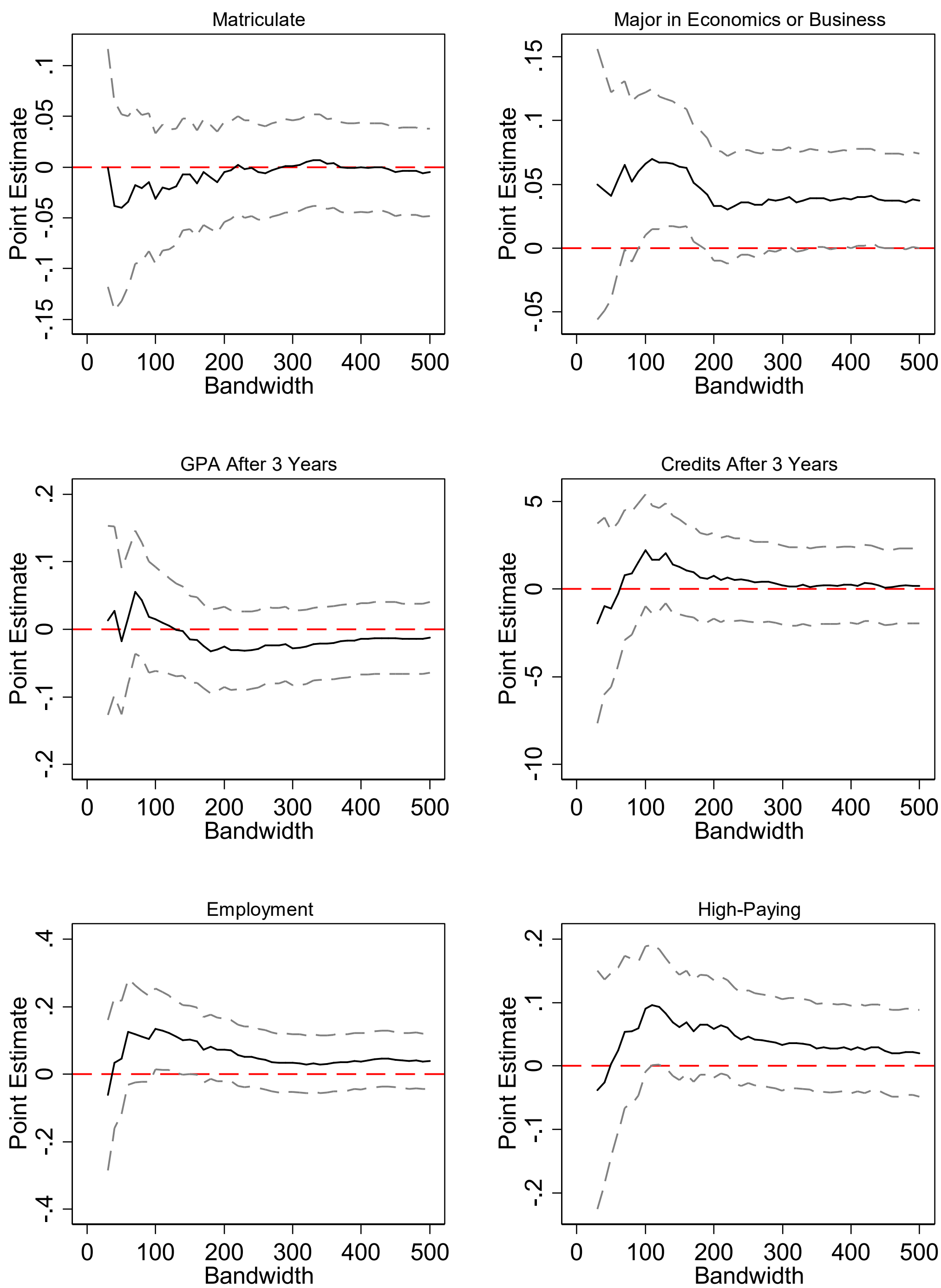

Notes: The figures show the share of students majoring in business or economics as a function of distance to the high-need income threshold. The solid line represents a linear or quadratic fit of the outcome on standardized income estimated seperately on each side of the threshold. Point estimates for β1 and robust standard errors from equation 2) across multiple bandwidths and specifications are reported under the figure heading. See text for sample restrictions. (*** p<0.01, ** p<0.05, * p<0.1)

Figure 7: Post-graduation outcomes




Notes: The figures show the share of students with a given post-graduation plan as a function of distance to the high-need income threshold. The solid line represents a linear fit of the outcome on standardized income estimated seperately on each side of the threshold. The outcome is listed as the figure heading. See text for sample restrictions.

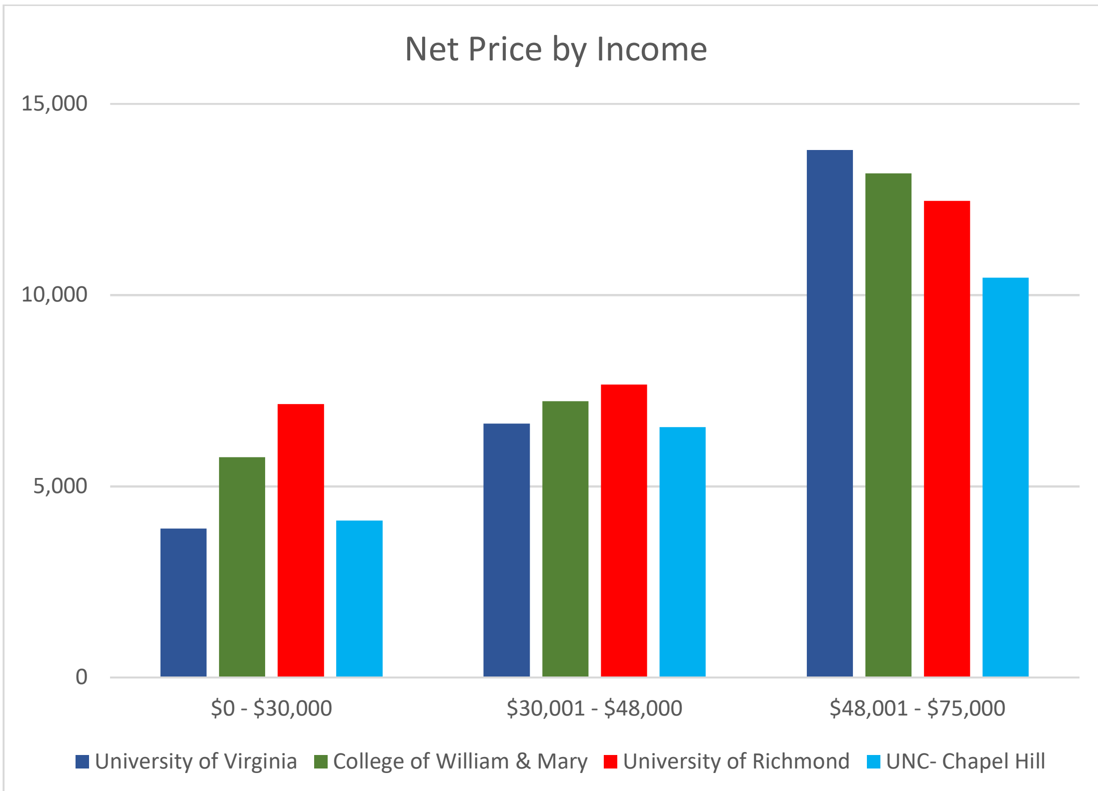
Figure 8: Robustness to Bandwidth Selection




Notes: The figure plots regression discontinuity treatment effects and confidence intervals for college enrollment, major choice, and academic performance by bandwidths ranging from 30% to 500%

# 1.A Appendix

Figure A1: College Net Price Profiles




Source: Student financial aid and net price file from Integrated Postsecondary Education Data System (IPEDS)
Notes: The figure presents the average net price (cost of attendance minus grants and scholarships) by income bin for UVa in three of its competitors that have similar financial aid policies.

Figure A2: Balance of Baseline Characteristics

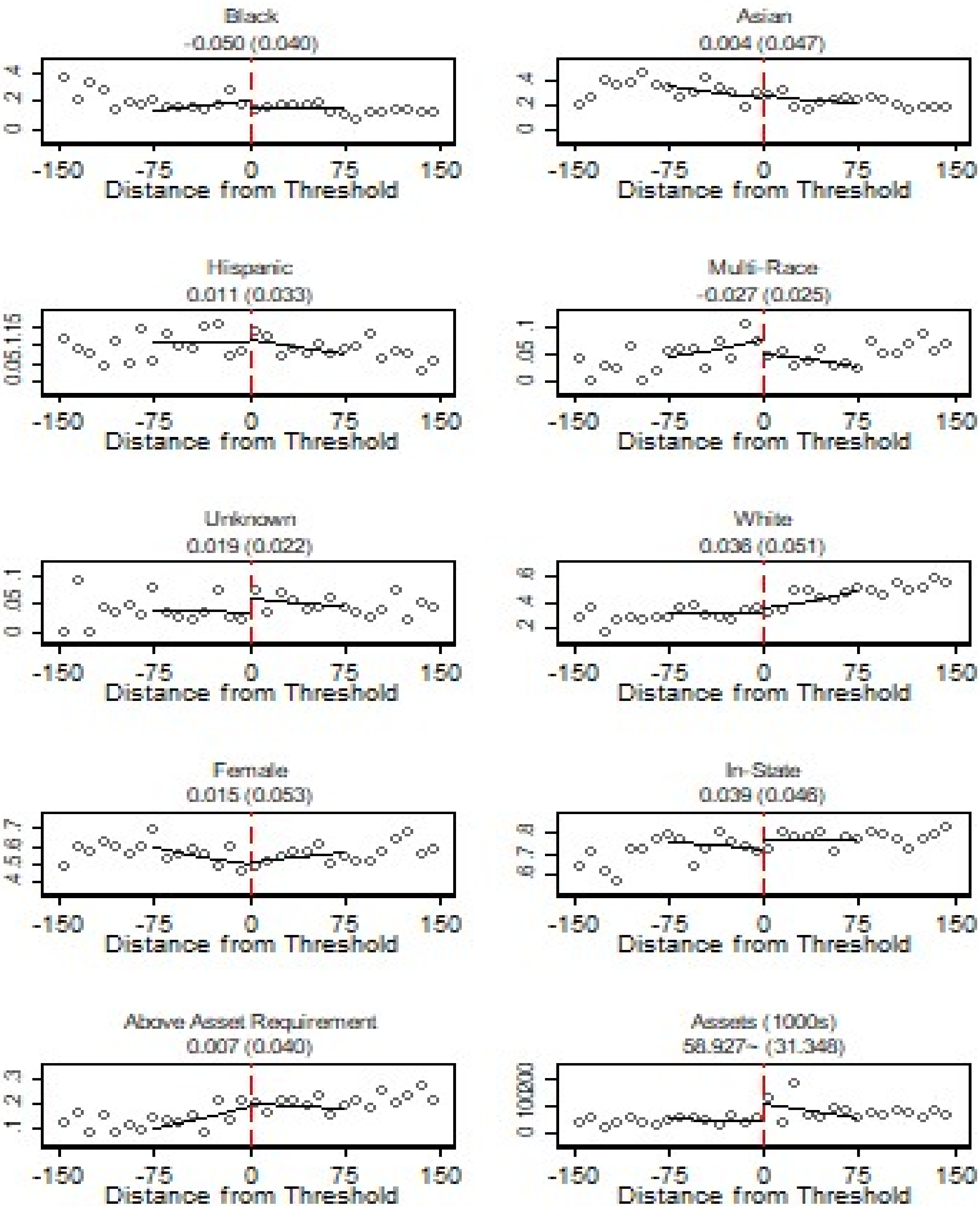
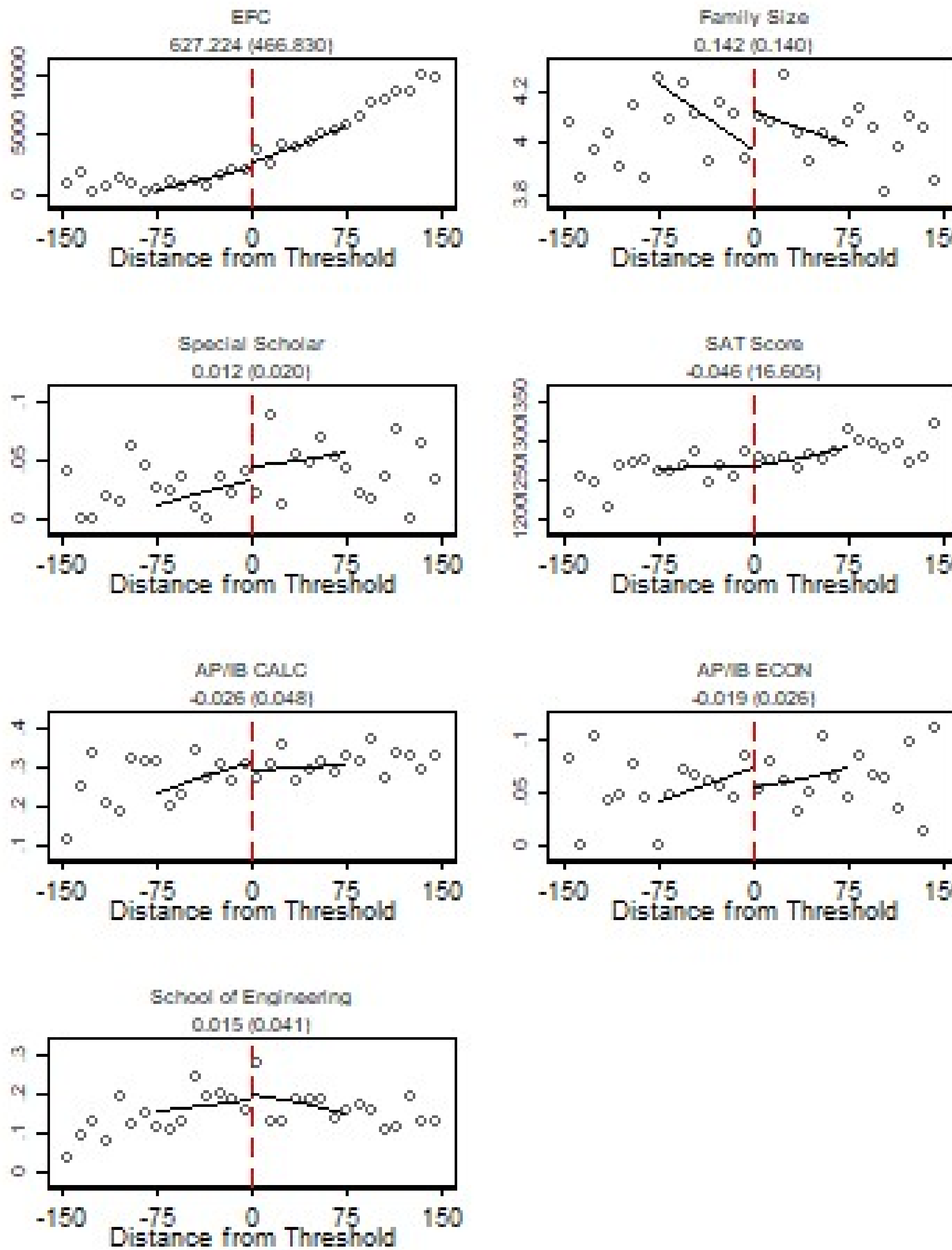
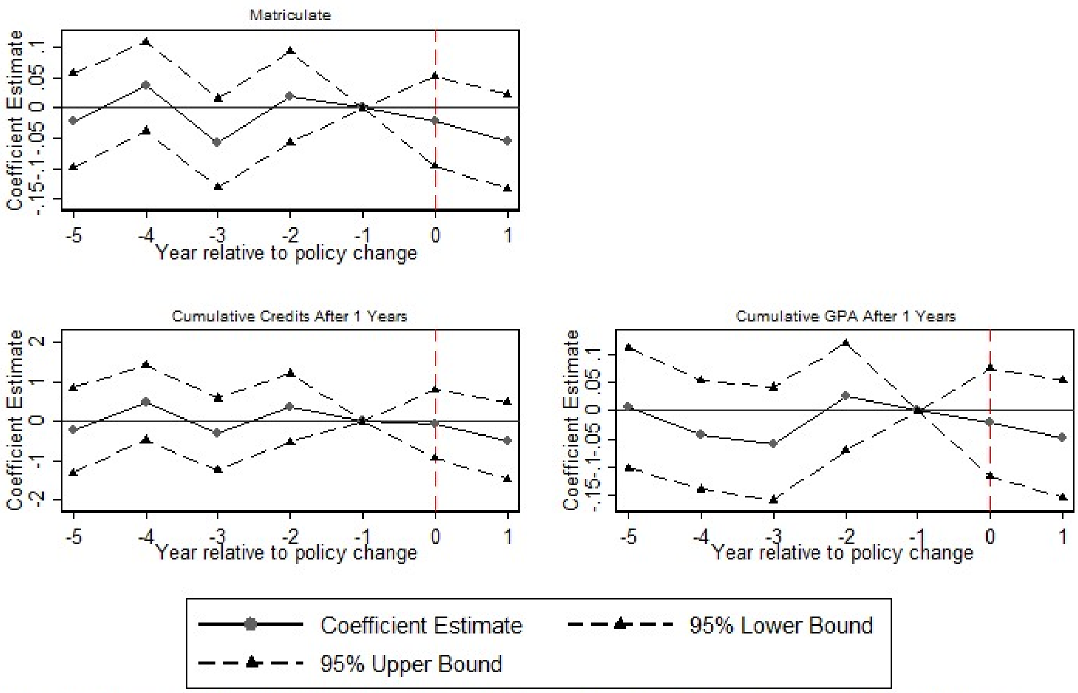


Figure A2, continued



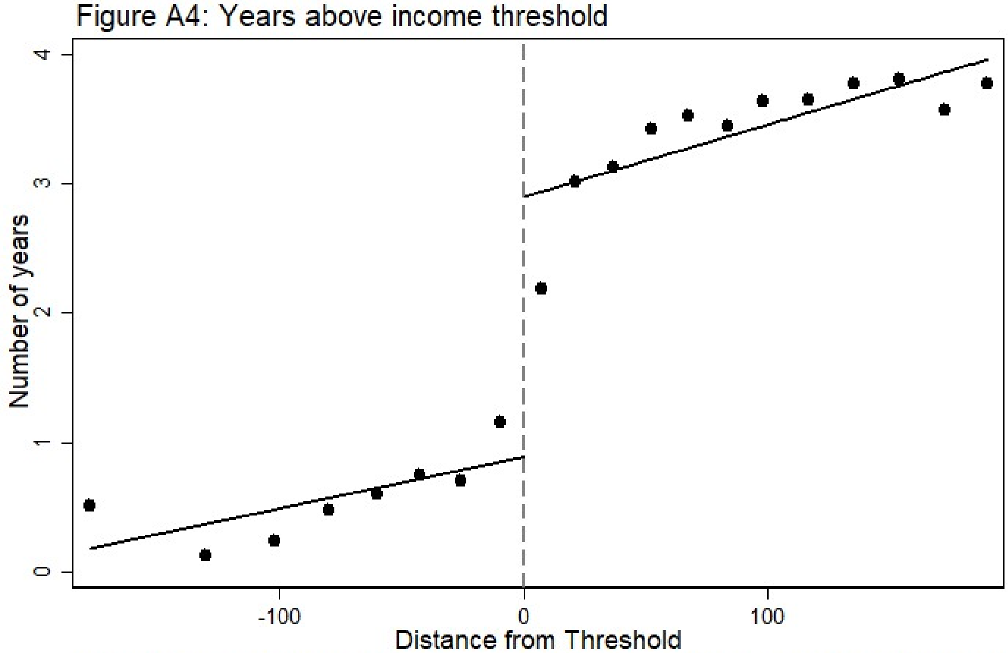

Notes: Each figure shows the average of a given characteristics as a function of distance to the high-need income threshold. The solid line represents a linear fit of the outcome on standardized income estimated seperately on each side of the threshold. Point estimates for β1 and robust standard errors from equation 3) using a bandwidth of 75% are reported under the figure heading. (*** p<0.01, ** p<0.05, * p<0.1)

Figure A3: Event Study Results




Notes: The figures plots coefficient estimates and 95% confidence interalvs of from equation 4) where high-need is interacted with a dummy for each cohort and the year prior to the policy change is omitted.

Figure A4: Years above income threshold




Notes: The figures show the number of years a student was above the income threshold four years after applying to college. The sample includes the 2009-2011 entering cohorts.

Table A1: Difference in major choice across semesters

| | Control Mean Final | 5th Semester | | 6th Semester | | 7th Semester | | Final Major | | | Final First Major | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | | (3) | | (4) | | (5) | | | (6) | |
| Economics or business | 0.11 | 0.028 | (0.031) | 0.04 | (0.034) | 0.053 | (0.035) | 0.057 | (0.033) | * | 0.036 | (0.030) |
| Business | 0.04 | 0.024 | (0.024) | 0.029 | (0.024) | 0.021 | (0.025) | 0.027 | (0.023) | | 0.027 | (0.023) |
| Economics | 0.07 | 0.008 | (0.023) | 0.022 | (0.028) | 0.043 | (0.029) | 0.039 | (0.027) | | 0.009 | (0.022) |
| Observations | | 1247 | | 1216 | | 1164 | | 1316 | | | 1316 | |

Notes: Each cell in Columns 2- 5 contains estimates of β1 from equation 2), the difference in major choice at the income threshold for a given semesters. The relevant outcome is listed as the row header, and the semester is listed as the column header. Column 1 contains the mean among high-need students for a given outcome. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

Table A2: Difference in work experience during college

| | Control Mean | | | |
|---|---|---|---|---|
| | (1) | (2) | | |
| Part-time Employment | 0.63 | 0.073 | (0.085) | |
| Summer-time Employment | 0.54 | 0.186 | (0.087) | ** |
| Internship | 0.62 | 0.125 | (0.067) | * |
| Job Shadowing | 0.14 | -0.087 | (0.061) | |
| Research | 0.36 | -0.063 | (0.081) | |
| Career Center Services | 0.22 | 0.108 | (0.077) | |
| Observations | | 519 | | |

Notes: Each cell in Column 2 contains estimates of β1 from equation 2), the difference in work experience at the income threshold. The relevant outcome is listed as the row header. Column 1 contains the mean among high-need students for a given outcome. Robust standard errors in parenthesis ( *** p<0.01, ** p<0.05, * p<0.1)

# Chapter 2

# Do Colleges Respond to Incentives to Enroll ‘At-Risk’ Students? Evidence from a State Higher Education Accountability Policy

## 2.1 Introduction

States provide substantial financial support to public colleges, and recently the method many states use to allocate appropriations has changed. In FY2016, appropriations totaled $76 billion, or 53% of total educational revenue for public colleges (SHEEO, 2016). Since 2007, 34 states adopted outcome-based funding (OBF). OBF replaced models for allocating appropriations based solely on enrollment levels, and instead use metrics like graduation rate to measure educational outcomes. OBF creates incentives for colleges to increase college completion as opposed to focusing on the number of students enrolled. However, OBF could lead to unintended consequences, such as colleges enrolling students who are more likely to graduate or lowering academic standards to retain students.

Colleges might respond to OBF by restricting admissions to students who are less likely to graduate because the cost to universities of enrolling these students is higher under OBF. For example, under OBF in Tennessee, colleges do not receive any state appropriations for students who drop out before completing 24 credits. Low-income students might be especially negatively affected by OBF because they are on average less likely to complete college (Bailey and Dynarksi 2011). Recent qualitative research on the unintended impacts of OBF shows that college stakeholders were aware of changes or discussed the potential of changes to admissions practices (Lah et al. 2014). A study by Kelchen and Stedrak (2016) shows after the adoption of OBF that colleges received less Pell Grant revenue suggesting that colleges enrolled more high-income students in response to the OBF.

To prevent colleges from decreasing admissions to underrepresented student groups, 16 states included “equity measures”—an incentive to enroll specific student groups—in their OBF formula.[43] For example, the equity measure in Tennessee provides four-year colleges $3,600 (40%) more in appropriations for graduating low-income students. In 2011, spending on equity measures accounted for 13% of total appropriations.[44] By providing more appropriations for underrepresented students, equity measures also serve as a way for states to redistribute appropriations to public colleges serving a high share of underrepresented students. While this type of incentive is widespread and substantial, we do not know whether college admissions practices responded. If colleges do not respond to equity measures, then OBF could lead to a decrease in enrollment for low-income students.

In this paper, I examine whether the equity measure adopted by Tennessee in 2010 affected the enrollment decisions of low-income students. Tennessee's equity measure applies to low-income students, as determined by Pell Grant eligibility. As a result of the eligibility threshold, colleges receive a substantially different amount of appropriations for very similar students. The broad hypothesis of this paper is that public colleges will respond to equity measure by enrolling more Pell-eligible students. Underlying this hypothesis is a model where colleges produce graduates by admitting students and are subject to a budget constraint. Since students around the Pell Grant eligibility threshold are similar, enrolling students near the threshold should produce the same number of graduates, but enrolling Pell-eligible students earns the university more in appropriations. If college admissions practices respond to the Pell premium, then the enrollment decisions for students across the eligibility threshold should differ.

Using a regression discontinuity design, I exploit the threshold for eligibility of low-income status for students applying to college after 2010 and compare the enrollment decisions for students just above and below the threshold. To examine the effect of the equity measure and enrollment, I use student-level data for the 2008-2014 Tennessee public high school graduating cohorts provided by the Measure Tennessee Longitudinal Data System (MeasureTN). MeasureTN tracks students from K-12 into college by matching students to the National Student Clearinghouse. My main explanatory variable comes from the FAFSA. For students who submitted the FAFSA, I observe their expected family contribution (EFC), which determines eligibility for the Pell Grant.

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[43] The term "equity measures" is used in Cielinksi and Pham (2017). The number of states using equity measures is based on Snyder and Fox (2016) which created a typology for describing different models of OBF.
[44] Author's calculation.

Overall, the regression discontinuity estimates show no difference in whether students enroll or in their college sector choice. One explanation for the null effects is that many colleges cannot respond to the premium because they already admit most students. Analysis by college selectivity supports this hypothesis. I find that for college-goers around the eligibility threshold, the public flagship responded to equity measure by enrolling more Pell-eligible students. There is suggestive evidence that the response by the flagship diverted ineligible students to less selective public four-year colleges. The policy implication of these results depends on whether the relative increase in Pell-eligible enrollment at the flagship crowded out Pell-ineligible students, or crowded in Pell-eligible students. Since the size of the entering class at the flagship was relatively fixed over my sample period, the increase in Pell-eligible enrollment crowded out students who were ineligible for a Pell Grant.

Finally, I explore heterogeneity in the effects of the Pell premium at the flagship. While I do not observe where students applied, I use the distance to the flagship as a proxy for applications and show that the effects are larger for students near the flagship. The difference in flagship enrollment is also the largest among relatively middle-achieving students. This pattern of results is what we would expect since the likelihood of graduating is uncertain for middle-achieving students, and the equity measure compensates colleges for taking additional risk by providing additional appropriations.

My results show that most public colleges in Tennessee did not respond to the equity measure by differentially enrolling eligible students. Since college enrollment around the threshold was largely unaffected by the equity measure, OBF could lead to an overall reduction in the enrollment of low-income students. I descriptively show that there was no change in the enrollment rate for Pell-eligible student after the adoption of OBF in 2010, but I cannot causally estimate the combined effect of OBF and equity measure on the enrollment of low-income students. While the equity measure did not have the intended effect on enrollment, using publicly available appropriations data, I show that the equity measured redistributes appropriations to colleges that serve a higher share of low-income students. Taken together, my findings show that states considering adopting OBF should consider both whether colleges can respond to the new incentives, and how these incentives affect the distribution of appropriations between colleges. Future research in this area should examine whether colleges respond to equity measures by targeting resources towards students who qualify for the equity measures.

## 2.2 Literature Review

The recent wave of policy adoption represents a resurgence of OBF. Many states adopted OBF in the 1990s but abandoned their policies in the early 2000s due to state budget cuts. The literature treats these two waves of adoption separately because the details of the funding models differed drastically across waves. The recent wave differs from the initial wave because it ties a larger share of appropriations to outcome indicators which are incorporated into base funding as opposed to bonus funding (See Dougherty et al., 2014 for a comprehensive discussion of the differences between the waves).

Since many states recently adopted OBF, much of the research on the effects on academic outcomes focuses on the initial wave of policy adoption. These studies try to isolate the effect of OBF by comparing the change in academic outcomes in states that adopted OBF to the change in academic outcomes in states without OBF. The challenge for identification is finding a group of control states which have a similar trend in academic outcomes as the states with OBF. In general, these studies find no differences in graduation rates or the number of degrees (Shin and Milton 2004; Shin 2010; Sanford and Hunter 2011; Radford and Rabovsky 2014; Tandberg and Hillman 2014).

One hypothesis for the null findings is that many colleges not have the capacity to respond through organizational learning. Dougherty et al. (2016) through interviews, finds little evidence that colleges increased their collection of student outcome data or tried interventions to increase outcomes. The authors argue that without increasing the capacity to determine effective practices, colleges will not adequately respond to OBF. Hillman, Tandberg, and Fryar (2015) found that community colleges in Washington produced more short-term certificates in response to OBF. The authors argue that colleges focused on short-term certificates because certificates are easier to affect and that colleges lack the internal capacity and resources to increases retention. The authors view their findings as an unintended consequence of OBF.

A growing strand of the literature focuses on the unintended consequences of OBF. By providing an incentive to increase student outcomes, colleges could respond by lowering their academic rigor to pass more students or restrict admissions to students less likely to graduate. Research in this area is largely descriptive and finds that college administrators are aware of these perverse incentives in OBF, but find little evidence of changes in behavior (Lahr et al.

(2014). A recent case study of Indiana's OBF policy by Umricht, Fernandez, and Ortagus (2015) found that public colleges in Indiana became more selective relative to colleges that were unaffected by OBF. Keltchen and Stedark (2016) found that while colleges did not reallocate appropriations once subject to OBF, they did award more merit aid and enrolled students receiving less Pell Grant aid. The authors argue that the difference in aid receipt is evidence of colleges reducing admissions to students who are less likely to graduate.

## 2.3 Background: Outcome-Based Funding and Pell Premium

### 2.3.1 Models of Outcome-Based Funding

States have long used indicators such as enrollment levels to allocate appropriations, and recently, many states have switched to using indicators of student academic outcomes. States using student academic outcomes to allocate appropriations are considered to operate outcome-based funding (OBF) model. The adoption of OBF is geographically widespread and usually applies to both two- and four-year colleges. In 2014-15, student outcomes determined \$3.2 billion of appropriations for public colleges, which is expected to increase as states phase-in outcome-based funding.[45]

While many states have adopted an OBF model, no standard model exists, and some models are closer to enrollment funding than others. Most OBF models include indicators of student progression towards a degree and degree completion but differ in the exact indicators used. The performance indicators used to determine how different OBF is from allocating funding based on enrollment levels. For example, some states measure student progression towards a degree using the number of students reaching a given credit benchmark, which is a step a student needs to take to earn a degree. While other states use the number of students returning for their second year of college, which does not necessarily mean students are progressing towards earning a degree and is like using enrollment levels.

States also differ in the share of appropriations determined by OBF, which could impact how responsive colleges are to the new funding model. Half of all states using OBF award less than 5

[45] Source: http://strategylabs.luminafoundation.org/wp-content/uploads/2013/03/OBF-Table_Notes.pdf

percent of appropriations based on performance indicators, and a quarter of states using OBF allocate between 5 and 10 percent of appropriations using performance indicators. At the top of the distribution, North Dakota, Nevada, Ohio, and Tennessee award more than 80 percent of state appropriations using performance indicators.[46] Colleges in states which allocate a higher share of appropriations using OBF might have a bigger response to OBF since it represents a larger share of their revenue.

Many states compensate colleges for enrolling students who are less likely to graduate by including "equity measures" in their OBF model. Equity measures differ across states but aim to provide colleges more appropriations for the outcomes of underrepresented student groups such as low-income, adult, or academically underprepared. Snyder and Fox (2016) created a typology for describing outcome-based funding models and found that roughly two-thirds of states prioritize underrepresented students in their formulas. Equity measures either provide a premium for the outcomes of underrepresented student above funding provided for the non-underrepresented student or include separate indicators for the outcomes of underrepresented students.

### 2.3.2 Tennessee Outcome-Based Funding Model

Tennessee adopted OBF in January 2010 as part of the Complete College Tennessee Act (CCTA). The policy adoption process started with meetings in the fall of 2009 and ended with the creation of the new funding model in the spring of 2010. Since 2011, Tennessee has allocated roughly 80 percent of annual appropriations using their OBF model, which is significantly higher than most other states. Tennessee's model also includes indicators strongly aligned with student academic outcomes instead of enrollment. Table 1 lists the performance indicators used for four-year colleges. Tennessee measures student progression by the number of undergraduates reaching certain credit benchmarks, and measures degree completion by the number of degrees at the undergraduate level and post-graduate level.[47] The funding model also includes performance indicators for the efficiency of degree completion (e.g., graduation rate) and other university functions. The large share of funding allocated using a model of OBF that represents a

[46] Source: http://hcmstrategists.com/drivingoutcomes/wp-content/themes/hcm/pdf/2016-Report.pdf
[47] Starting in 2016 the credit benchmarks were replaced with 30, 60, 90 credits.

stark contrast to enrollment funding, combined with a relatively mature policy, makes Tennessee an ideal setting to study OBF.

Figure 1 shows the allocation of appropriations by performance indicator. In 2011-12, colleges earned roughly 75 percent of appropriations from their performance on progression and completion indicators. Colleges earned the remaining 25 percent of appropriations through performance on degree efficiency and other university functions.[48] The figure also shows variation across college sectors in performance on progression and completion indicators. Public two-year colleges earned 40 percent of their appropriations from performance on progression indicators, compared to less than 20 percent among four-year public colleges.

The actual calculation of appropriations is a complicated process based on a college's performance relative to the performance of all other colleges. Each performance indicator is measured using a three-year average, meaning outcomes from 2007-08 through 2009-10 school years determine appropriations for the 2011-12 fiscal year. The formula then uses the performance indicators to provide each college with a requested level of appropriations. The actual allocation of appropriations follows a pro-rata share where the share depends on the total available appropriations. For example, if the sum of the formulaic requests is $2 billion and the state has $1 billion for higher education appropriations, then each college receives 50% of their formulaic request. Under this allocation process, a college must increase their performance relative to other colleges to receive a higher share of appropriations. In the following section, I show the changes in the allocation of appropriations over time.

### 2.3.3 Pell Premium

The Tennessee OBF model, like the OBF model used in many states, includes an equity measure to provide an incentive for colleges to enroll more underrepresented students. I refer to the equity measure in Tennessee as a "Pell premium" since colleges receive additional funding for low-income students, where Pell Grant eligibility determines low-income status.[49] The premium acts as an additional 0.4 students completing the relevant progression or completion

[48] Calculations done by the author based formula inputs for the 2011-12 appropriations recommendation.
[49] Colleges also receive additional funding for adult students.

performance indicator.[50] For example, a low-income college graduate counts as 1.4 graduates in the model.

While my empirical strategy relies on comparing students who should have similar graduation rates, the Pell premium perfectly offsets the difference in graduation rates between Pell Grant recipients and non-Pell Grant student. Using graduation rate data for public colleges in Tennessee as reported in IPEDS, I find that the average ratio of the graduation rate for non-Pell Grant students to Pell Grant students is 1.41. To the extent that the cost of educating a student does not vary by income, the size of the Pell premium equalizes the expected net revenue of enrolling a Pell Grant recipients and non-Pell Grant student.

Tennessee provides a good opportunity to study how colleges respond to equity measures because the size of the Pell premium is substantial. Table 2 shows the dollar value of the Pell premium for graduating an additional low-income student. In 2014-15, the average premium for graduating an additional Pell Grant recipient was $3,700 for four-year colleges and $1,700 for two-year colleges.[51] The difference in size of the premium between sectors is primarily due to the difference in the cost of educating students. Due to an increase in total appropriations available during the sample period, the real value of the premium in the four-year sector increased by $213 or 6 percent. Since the value of the premium is nontrivial, colleges are likely aware of the Pell premium and therefore might adjust their admissions practices.

Tennessee also provides a good opportunity to study equity measures because many colleges rely on the Pell premium as a significant source of revenue. Based on the formula inputs from 2011-12, 13 percent of appropriations were from the Pell premium, which is slightly higher than appropriations for degree production efficiency.[52] On average, two-year colleges rely more on the premium as a source of funding compared to four-year colleges. The premium represented 16 percent of appropriations for two-year colleges and 11 percent for four-year colleges. I use the variation in reliance on the premium for funding as a measure of the salience of the incentive and examine whether colleges more reliant on the premium are more likely to increase the enrollment of Pell Grant recipients differentially.

[50]Starting in 2016 low-income, and adult students counted as 1.8 students and a low-income adult student counted as 2.0 students.

[51] Author's calculation based on the additional appropriations college receives for increasing the number of students completing a degree by 0.4 students.

[52] Author's calculation based on the number of additional students reaching progression and completion benchmarks from the Pell premium.

In addition to providing an incentive to enroll more low-income students, the Pell premium provides a way to redistribute appropriations to colleges serving more low-income students. To show the redistributive effects of the Pell premium, I set the premium to 0 in Tennessee's OBF formula, compute appropriations, and then compare the new allocation of appropriations to the allocation of appropriations with the premium. This calculation captures the mechanical effect of changing the formula but ignores any behavioral response of removing the premium by colleges.

Figure 2 shows the negative relationship between the predicted change in appropriations after eliminating the Pell premium, and the share of Pell-eligible students enrolled at a college. The elimination of the Pell premium would increase appropriations to the University of Tennessee - Knoxville, which has the lowest share of Pell-eligible students enrolled, by $12 million or 7.7 percent. In general, the gains to the University of Tennessee -Knoxville would come at the expense of two-year colleges, which would experience an average decrease in appropriations of 4.0 percent. The redistributive function of the Pell premium is nontrivial, and state policymakers should consider it when designing a formula and for evaluating how universities respond to the premium.

## 2.4 Changes in Appropriations after Adoption of Complete College Tennessee Act

Despite the large change in how states allocate appropriations, little research exists on how the adoption of OBF changed the allocation of funding. In this section, I first show that the appropriations for many schools changed significantly after the adoption of OBF in Tennessee. However, the changes in appropriations are not necessarily due to colleges responding to OBF by changing their performance. I then show that the differences in appropriations after the adoption of OBF are due largely to the mechanical response of switching to a new formula.

The distribution of appropriations changed significantly in the years following the adoption of OBF in Tennessee. Figure 3 shows changes at the school level in the share of appropriations a college received since the adoption of OBF compared to their share of appropriations in the base year (2010). I use a college's share of appropriations as opposed to the level of appropriations because total higher education appropriations change over time. The top panel includes four-year colleges, and the bottom panel includes two-year colleges. While not pictured, the adoption of OBF did not change the allocation of funding across two- and four-year colleges.

The top and bottom panels of Figure 3 show there were clear winners and losers in both two-year and four-year college sectors after switching to OBF. The differences in appropriations were smaller at first because the state phased in the new formula over three years to dampen any immediate changes in funding.[53] By 2014, Austin Peay State University and the University of Tennessee- Knoxville received an 18 percent and 10 percent increase in their share of appropriations relative to their share of appropriations in the year before OBF, while the University of Memphis received a 14 percent decrease in their share of appropriations. After the adoption of OBF, there was a divergence in appropriations among the colleges in the two-year sector, where schools either received an increase in their share of appropriations by 10-15 percent or a decrease of 5-10 percent. There were two outliers among the two-year colleges, Pellissippi received a 20 percent increase in their share of appropriations, while Southwest Tennessee received a decrease of 32 percent in their share of appropriations.

The changes in appropriations shown in Figure 3 does not necessarily mean that colleges responded to the OBF by increasing their performance. Differences in appropriations could be mechanical if, for example, the OBF model favored colleges with a high graduation rate. To calculate the predicted mechanical effect of switching to OBF, I use the formula inputs from the pre-policy years to predict the difference in funding under the old formula and OBF in the base year. If the colleges that received higher appropriations in 2014 under OBF would have also received more appropriations in the pre-policy years under OBF, then the differences in appropriations in 2014 are partly due to the mechanical effect of using a new formula, as opposed to how colleges responded to the formula.

Figure 4 shows that the changes in appropriations after the adoption of OBF were largely due to the predicted mechanical effect of using a new formula. The y-series represents the predicted mechanical response, which is the percent change in a college's share of appropriations in the base year (2010) if OBF was used instead of the old formula. If there were no mechanical differences, then colleges would be along the x-axis. The x-series is the observed change in a

[53] A college's appropriations request was either slightly scaled up or down by a pre-determined about for the first two years of OBF. The scaling factors were determined when the formula was adopted based on projections of which schools would benefit and lose under the new formula.

college's share of appropriations between the base year and 2014, which represents the combination of the mechanical and behavioral effects. The figure shows that most colleges are located along the 45-degree line, indicating the changes in the allocation of appropriations after OBF was adopted were due to mechanical changes. In the results section, I use the estimated mechanical effects to examine if the colleges that were likely to lose funding under OBF were more likely to respond to the Pell premium.

## 2.5 Conceptual Framework

I propose a model of college degree production to illustrate how college admission policies could respond to outcome-based funding and the potential effects of the Pell premium. I start by modeling college admissions behavior in the absence of OBF. I then show how OBF and the Pell premium changes the college's problem to derive predictions that I empirically test.

I assume that universities maximize the number of graduates they produce and care about the mix of students they serve. For simplicity, suppose there are three types of students, low-, middle-, and high-income. Low- and middle-income students have the same probability of graduating $\pi_L$, and high-income students graduate with a probability $\pi_H$, where $\pi_L < \pi_H$, and the probability of graduating does not depend on college inputs. Colleges then choose the number of each student type to admit to produce graduates given an elastic supply of each type.[54] Their objective function is given by $G = G(g_L(n_L), g_M(n_M), g_H(n_H))$ where $g_i(\cdot)$ represents then the number of graduates with income *i,* and is a functions of admissions of income group $n_i$. The number of graduates $g_i = n_i\pi_i$ is equal to the number of admitted students $n_i$ multiplied by the probability of graduation $\pi_i$, where all admitted students matriculate.[55] I assume the objective function *G()* is Cobb-Douglas, $G = (n_L\pi_L)^\alpha(n_M\pi_L)^\beta(n_H\pi_H)^{1-\alpha-\beta}$.

Colleges face the following budget constraint:

$$\underbrace{E + \sum_{i=L,M,H}(ws_in_i + (1-w)s_in_i\pi_i)}_{\text{Revenue}} \geq \underbrace{c(n_H + n_M + n_L)}_{\text{Costs}}$$

[54] I am also assuming that all students are in-state. The percent of out-of-state (OOS) students is under 10% at seven of the nine four-year public schools in Tennessee.
[55] Assuming a matriculation rate of less than one does not qualitatively change the results.

They face a constant marginal cost *c* of enrolling students, which does not vary with the student's type. I assume that this cost is greater than the expected state appropriations, requiring some spending out of non-capitated resources, which I represent as the endowment *E* and take as given. [56]

Under enrollment funding and OBF, universities receive state appropriations $ws_i$ for every student who enrolls $n_i$ and $(1-w)s_i$ for every student who graduates $n_i\pi_i$. [57] *w,* and (1-*w)* represent the weight given to admissions and graduation, respectively, in the state funding formula, and $s_i$ represents the state appropriations rate for students of type *i.* The terms in the summation represent the expected state appropriations for admitting students of type $i$.[58] I exclude tuition revenue because Tennessee universities do not offer generous grant aid, so tuition should not vary by income.

Rearranging terms in the budget constraint yields the following:

$$E \geq \underbrace{[c - ws_L - (1-w)s_L\pi_L]}_{p_L(c,w,s_L,\pi_L)}n_L + \underbrace{[c - ws_M - (1-w)s_M\pi_L]}_{p_M(c,w,s_M,\pi_L)}n_M + \underbrace{[c - ws_H - (1-w)s_H\pi_H]}_{p_H(c,w,s_H,\pi_H)}n_H$$

where $p_L(c,w,s_L,\pi_L)$, $p_M(c,w,s_M,\pi_L)$, and $p_H(c,w,s_H,\pi_H)$, represent the net cost of admitting a student. The net cost of admissions is decreasing in $w, s_i,$ and $\pi_i$ because expected state appropriations are increasing in each of the parameters. Below, I detail how performance funding affects the net cost of admissions by type.

Universities choose the number of students to admit from each income group to maximize *G(.)* subject to their budget constraint.

$$(3.2) \quad \max_{n_L,n_M,n_H} (n_L\pi_L)^{\alpha}(n_M\pi_L)^{\beta}(n_H\pi_H)^{1-\alpha-\beta}$$

$$s.t\ E \geq p_L(c,w,s_L,\pi_L)n_L + p_M(c,w,s_M,\pi_L)n_M + p_H(c,w,s_H,\pi_H)n_H$$

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[56]Assuming the university's endowment is exogenous ignores any dynamic effects that the current number or type of graduates has on a university's future endowment.

[57] Under both funding models, universities receive appropriations each year a student complete. I simplify the analysis by assuming a payment at the time of enrollment, and at the time of graduation.

[58] I assume a discount rate of one.

Taking first-order conditions and solving for the number of admissions yields $n_L^* = \frac{E\alpha}{p_L(c,w,s_L,\pi_L)}$, $n_M^* = \frac{E\beta}{p_M(c,w,s_M,\pi_L)}$, $n_H^* = \frac{E(1-\alpha-\beta)}{p_H(c,w,s_H,\pi_H)}$, where the number of admissions is proportional to the endowment divided by the net cost of enrolling a given student type.

In the table below, I detail how the structure of state appropriations differs across funding models and then discuss how this affects the net cost of admissions. Under enrollment funding, state appropriations for enrolling and graduating a student are equal while OBF reduces appropriations for enrolling students and increases appropriations for graduating students. In Tennessee, completing 24 credits is worth \$1,015, while completing a degree is worth roughly \$9,220. Under enrollment funding and OBF, appropriations rates for all types are equal, while under OBF with the Pell premium appropriations for low-income students are higher than for middle-income and high-income students.

**Differences in Funding Models**

| | Enrollment Funding | OBF | OBF with Pell Premium |
|---|---|---|---|
| Weight on Enrollment | *w = (1-w)* | *w < (1-w)* | *w < (1-w)* |
| Rate of Appropriations by Income | $s_L = s_M = s_H$ | $s_L = s_M = s_H$ | $s_L > s_M = s_H$ |

OBF affects university decisions by changing the net cost of admissions. Under enrollment funding $p_L(\cdot) = p_M(\cdot) > p_H(\cdot)$, the net cost of admitting a low- or middle-income student is greater than admitting a high-income because high-income students graduate at a higher rate. OBF increase the relative net cost of admitting low-and middle-income students because they are less likely to graduate. The Pell premium then decreases the net cost of admitting a low-type and leaves the absolute cost of admitting middle-and high-income students unchanged. Taken together, the two changes together yield the following predictions:

1. Admissions to middle-income students should decrease, relative to low- and high-income admissions after the adoption of OBF with a Pell premium.
2. The effect of adopting OBF with a Pell premium on admissions to low-income relative to high-income students is ambiguous. The sign of the effect depends on whether

the Pell premium effect offsets the effect of adopting OBF which is driven by differences in the probability of graduating between student types. [59]

## 2.6 Data and Summary Statistics

To examine whether colleges responded to the Pell premium, I use student-level data for 2008-2014 Tennessee high school graduates provided by the Measure Tennessee Longitudinal Data System (MeasureTN). MeasureTN tracks students from K-12 into college by matching students to the National Student Clearinghouse. The data include background information such as student age, race, gender, and ACT scores if applicable. For ACT takers, I also observe which colleges they sent their scores to when the test was administered. My main explanatory variables come from the FAFSA. For students who submitted the FAFSA, I observe the date it was submitted and their expected family contribution (EFC). From EFC, I identify which students are eligible for a Pell Grant and how close students are to the eligibility threshold. I use the date of a student's first FAFSA application to link to college enrollment data for the following academic year. My main outcomes of interest are whether a student enrolls anywhere and whether they enroll in a given sector (in-state public 4-year, in-state public 2-year, out-of-state public, private).

From the pool of 2008-2014 Tennessee public high school graduates, I start by restricting the sample to students who submitted the FAFSA. This restriction excludes roughly 20% of the sample but is necessary because my empirical strategy relies on EFC, which is only available for FAFSA filers. I then restrict the sample to students who filed the FAFSA for the first time in 2011 or later because that is when colleges were subject to OBF. Finally, I restrict the sample to students who enrolled in college before filling out the FAFSA. To conduct a test of the validity of my research design, I also apply the same sample restrictions detailed above to students who filed the FAFSA between 2008 and 2010. The resulting sample consists of 191,363 students for

[59] The model also predicts that total admissions will decrease because the net cost of admitting all students increases. This implies that overall appropriations decrease, but in practice the change to OBF was revenue neutral to the state, so admissions might not have declined. The current framework still captures relative changes in admissions to low-, middle-, and high-income students.

the full sample, and 33,482 individuals when restricting the sample to students with an EFC +/- $3,000 from the Pell Grant eligibility threshold.

Table 3 contains summary statistics for the full sample of FAFSA filers and the subsample of students with an EFC +/- $3,000 from the Pell Grant eligibility threshold. The top panel of the table includes baseline student characteristics, and the bottom panel includes the outcomes of interest. My sample is majority white and Pell-eligible, with 70 percent of students identifying as white, and 60 percent eligible for Pell Grant. The restricted sample consists of a higher share of white students. Most students do not have a parent with a college degree: 40 percent of students reported having a mother with a college degree, and 33 percent reported having a mother with a college degree. Since high school students are administered the ACT in school, 91 percent of students have an ACT score. On average, students scored 20 on the ACT, which is one point lower than that national average and lower than the college-ready benchmarks for the ACT subject-area tests.[60] Among ACT-takers, only 69 percent sent their score to at least one college when the exam was administered, but many students sent their score to at least one Tennessee public four-year college. The bottom panel of Table 3 shows the sample average for my outcomes of interest. Over 70 percent of the sample enrolled in college with a year of filing the FAFSA. Conditional on enrolling, 77 percent attend either a Tennessee public college two-year or four-year college.[61]

## 2.7 Empirical Strategy

The main challenge to estimating the effect of equity measures on enrollment is finding a valid counterfactual. Comparing the enrollment of low-income students before and after the policy in Tennessee is a simple calculation but does not isolate the true effect of the equity measure. Changes in the economy or changes in other factors over this time could affect the college enrollment for low-income students in the absence of the policy change. Even in the absence of changes to factors affecting college enrollment, a simple pre/post comparison would

[60] Meeting a college-ready benchmark indicates a 50% chance of obtaining a B or higher in the corresponding college course. The benchmark scores are 18, 22, 22, and 23 for ACT English, ACT Math, ACT Reading, and ACT Science, respectively. https://www.act.org/content/dam/act/unsecured/documents/Natl-Scores-2014-National2014.pdf.

[61] 45 percent of students attending a public four-year college attend their nearest option, and 80 percent of students attending a public two-year college attend their nearest option.

identify the combined effect of OBF and equity measures. One possibility to isolate the effect of the equity measure is to use as the change in the enrollment of low-income students in a state that adopted OBF but not an equity measure as a counterfactual. In addition to the normal assumptions for a valid difference-in-differences design, this approach requires that the models of OBF be similar across states. If the OBF models are too different, then the change in the enrollment in the control state does not plausibly control for the effect of OBF on low-income student enrollment.

To eliminate bias from omitted factors and to isolate the effect of equity measures, I estimate the effect of the Pell premium on college enrollment in Tennessee using a regression discontinuity research design. Specifically, I use variation in a student's eligibility for the Pell premium created by the Pell Grant eligibility threshold and compared the college enrollment decisions between students who are barely eligible and ineligible for the Pell Grant. If students near the eligibility threshold are similar except for the appropriations a university receives for the academic outcomes, then any difference in their enrollment decisions is due to the Pell premium.

My main specification is a reduced form equation which takes the following form:

(1) $Y_{ic} = \alpha + \beta_1 Ineligible_{ic} + \beta_2 \widetilde{EFC}_{ic} + \beta_3 \widetilde{EFC}_{ic} * Ineligible_{ic} + \epsilon_{ic}$

where $Y_{ic}$ is an enrollment outcome for student $i$ in cohort *c*, $Ineligible_{ic}$ is an indicator equal to one for students who are ineligible for the Pell Grant, $\widetilde{EFC}_{ic}$ is a student's expected family contribution standardized around the eligibility threshold for cohort c such that students with $\widetilde{EFC}_{ic} > 0$ are ineligible for the Pell Grant. The interaction of eligibility and standardized EFC allows the relationship of EFC and the outcome to differ by whether a student is above or below the threshold. My main specification includes students whose EFC falls within \$3,000 of the cohort specific eligibility threshold. For robustness, I also present results using wider and narrower bandwidths as well as different specifications. The coefficient of interest is $\beta_1$, which represents the difference in enrollment around the eligibility threshold. If $\beta_1<0$ then relatively fewer ineligible students are enrolling in college.

The validity of this regression discontinuity rests on the similarity of students above and below the eligibility threshold. Another identifying assumption is that the relationship between the running variable (EFC) and the outcome is smooth through the eligibility threshold. Below I provide three tests for the validity of the research design.

To test for the similarity of students across the threshold, I start by testing for smoothness in the density of students around the threshold. Following McCrary (2008), if students manipulate their EFC to be eligible for the Pell Grant, then Pell Grant eligibility is not as good as random. For example, if students who were more motivated to attend college lowered their EFC to qualify for more grant aid, then students across the threshold would differ in both their interest to attend college and how much appropriations universities receive for their outcomes. Figure 5 shows a discontinuous increase in the density of students just above the eligibility threshold in both the pre- and post-policy change periods. This result runs counter to what we would expect if students could manipulate their eligibility status because more students are barely ineligible for the Pell Grant. The increase in density above the Pell Grant threshold for students in my sample is consistent with findings by Carruthers and Welch (2017) and Turner (2017). While the increase in the density of students implies that students are not manipulating their EFC to receive more grant aid, it does raise concerns about the similarity of students across the threshold.

I next test for the similarity of students by checking for balance in baseline observable student characteristics across the threshold. If students are similar across the threshold, then their observable baseline characteristics should be similar. Table 4 reports estimates of $\beta_1$ from equation 1), where the outcome is the baseline characteristic listed in each row.[62] The main concern is that differences on baseline academic ability or application behavior could lead to differences in enrollment. Using ACT score sends when the exam was administered as a measure of intention to apply, I find no evidence that students were interested in attending different colleges. I also find no difference in baseline academic achievement as measured by their ACT score. The table also shows a balance on demographics such as race, gender, and parent's level of education. The similarity of students around the threshold provides evidence that any difference in student outcomes is due to the Pell premium.

Lastly, I test if the relationship between the running variable and outcome is smooth through the eligibility threshold. The difference in Pell Grant aid at the threshold potentially violates the assumption because grant aid could affect the enrollment decision in the absence of the Pell premium. Using data from the pre-policy years, I test for differences in enrollment decision at the eligibility threshold. Table 5 shows no differences in the enrollment decisions for the pre-policy change cohorts. My findings replicate Carruthers and Welch (2017), which looks at

[62] The visual representation of these tests are available upon request

similar cohorts in Tennessee and finds no difference in enrollment across the Pell eligibility threshold. Students above the threshold are slightly more likely to enroll anywhere, but the difference is not statistically significant. These null findings provide evidence that the relationship between the running variable (standardized EFC) and the outcome is smooth through the discontinuity which is a central assumption to the validity of my empirical strategy.

## 2.8 Results

### 2.8.1 College enrollment and sector choice

Table 6 and Figure 6 summarizes the main results for whether colleges responded to the Pell premium by differentially enrolling Pell Grant recipients. Each figure shows the difference in a given enrollment outcome at the Pell Grant eligibility threshold. The point estimates and standard errors for $\beta_1$ from equation 1) reported in Table 6 are displayed beneath the figure heading. Overall, I find that the Pell premium did not affect who enrolled in college. Students above the eligibility threshold were 1.0 percentage points more to enroll anywhere, but this difference is not statistically significant.

Next, I look at college sector choice to see whether the null enrollment result masks changes in sector attended. Since only the funding for public colleges was affected by the policy change, we could see relatively fewer Pell-ineligible students in public colleges and more in other college sectors. The remaining panels of Figure 6 show differences in college sector choice among the sample of students who enrolled in college. Like the overall enrollment results, I find statistically insignificant differences in sector choice. Given the precision of my estimates, I can rule out enrollment differences of larger than 2.8 percentage points at in-state public 4-year colleges. These results suggest that colleges are not responding to the Pell premium by differentially enrolling Pell-eligible students over Pell-ineligible students.

In Table 7 and Figure 7, I show that the above enrollment results are robust to different specifications and the choice of bandwidth. Each column of Table 7 represents a different functional form assumption, and each row represents a different outcome. The specifications differ by functional form assumption of the running variable, and inclusion of individual-level controls. The magnitude and significance of the estimates are largely robust to the inclusion of second, third, and fourth order terms of standardized EFC. In Figure 7, I test the sensitivity of results to the bandwidth selection. Each panel displays a separate outcome where the x-axis represents the bandwidth used in estimation, and the y-axis represents the point estimate of $\beta_1$. The solid black line shows the difference in the outcome at the Pell Grant eligibility threshold as the bandwidth changes, and the dashed lines show the 95% confidence interval. I limit the maximum bandwidth to the full support of Pell-eligible students. The point estimates are noisier at narrow bandwidths but are similar to the results presented in Table 6 using a $3,000 bandwidth.

### 2.8.2 Mechanisms

There are several explanations for the null enrollment findings presented. First, colleges may lack the capacity to respond to the Pell premium. Second, the benefit of responding to the premium may be too small or not salient to colleges. Finally, it might take time for colleges to respond. Below, I propose tests for each explanation and report the results in Table 8. Columns 1 and 2 report the difference in enrollment at Tennessee public four-year and two-year colleges from Table 6 as a comparison for the tests.

Given the margins colleges can adjust to influence where students enroll, selective public colleges have the greatest capacity to respond to the Pell premium. Colleges could recruit more low-income additional applicants, admit more low-income students from their current applicant pool, or award more need-based grant aid to increase the matriculation rate of an admitted low-income student. Since many students in Tennessee attend their local college, recruiting additional applicants from outside a college's established recruitment territory may be difficult for many colleges. Admitting more Pell-eligible students from their applicant pool is difficult for two-year colleges and less selective four-year colleges since they admit all or most applicants. Two-year colleges and less selective four-year colleges are further limited in their ability to respond to the Pell premium because they do not award generous institutional aid to target Pell Grant recipients. Taken together, I expect the strongest responses to the Pell premium from selective colleges.

Columns 3-5 of Table 8 show enrollment differences by college selectivity, to test for differences in capacity to respond to the Pell premium. Consistent with selective colleges having more capacity to respond to the premium, Column 5 shows that the probability of attending the state flagship is 1.2 percentage points lower for ineligible students at the threshold. While not statistically significant, Column 3 shows an increase in enrollment for Pell-eligible students at nonselective public four-year colleges as a result of the Pell premium. In the following section, I explore the effects at the flagship.

Columns 6-9 of Table 8 test for differences in whether colleges respond to the Pell premium by the salience of the incentive to the college. Colleges that rely more on the premium for funding could be more likely to respond. I categorize a college's reliance on the Pell premium by using the proportion of appropriations they receive based on the Pell premium (see section III). Columns 6-7 show no difference in enrollment at reliant Tennessee public colleges four-year college, but a 1.1 statistically significant increase in Pell-ineligible enrollment at reliant public two-year colleges. The premium might be more salient to colleges that are expected to lose appropriations under OBF. Using my estimate of a college's expected difference in appropriations under OBF from section IV, Columns 8-9 show no difference in enrollment at Tennessee public colleges based on their expected gain or loss of funding from OBF. Overall, there is not a clear pattern showing the response differed by the salience of the premium.

Columns 10-11 of Table 8 show enrollment differences at public colleges over time, to test if colleges needed to learn about the policy. If it takes colleges time to learn about the new funding formula and adjust their admissions policies, then we should see larger differences in enrollment among students from later cohorts. The value of the premium also increased slightly over time, which may elicit a larger response from colleges. Columns 10-11 show no difference in the response by public colleges over time.

### 2.8.3 Flagship enrollment

Building on the results from Table 8 showing a difference in response to the premium by college selectivity, I next explore the robustness and heterogeneity of the flagship enrollment results. While the flagship only enrolls 9% of college-bound Tennessee high school graduates, it ranks the highest in the state on many measures of college quality. The flagship spends more on instructional support per student, has a higher graduation rate, and higher average earnings compared to other public colleges in Tennessee. The response to the premium by the flagship could, therefore, affect the quality of college Pell-ineligible students receive, which could lead to lower future earnings.

I begin by providing graphical evidence of the flagship's response to the premium. The top panel of Figure 8 shows that among students attending college, the share enrolling at the flagship decreases by 1.2 percent from 8 percent to 6.8 percent at the Pell Grant eligibility threshold. The top right panel shows a 0.8 percentage point increase in enrollment in other public four-year colleges. However, the difference is not statistically significant. Figure 9 shows the robustness of these results to different bandwidths. As the bandwidth increases, the estimated decrease in enrollment for Pell-ineligible students at the flagship becomes slightly larger. The estimates of the difference in enrollment at other public four-year follow a similar pattern. For the widest bandwidths, the estimates are over 1.5 percentage points and statistically significant. While not conclusive, Figure 9 provides suggestive evidence that the response to the Pell premium by the flagship shifted some Pell-ineligible students to lower quality public four-year colleges.

I next examine the enrollment differences for student groups likely more affected by the flagship's response. The analysis using the full sample in Figure 8 and 9 includes many students who were likely unaffected by the flagship's response to the premium. For starters, the full sample of college-goers includes many students who did not apply to the flagship. While I do not observe where students applied, I proxy for applications using distance to the flagship.[63] If applications follow the same pattern, then the students from the counties surrounding a given college are also more likely to apply there. The bottom right panels of Figure 8 show the enrollment differences by the distance to the flagship. Among the sample college-goers from counties less than 50 miles from the flagship, the Pell premium substantially decreased the probability of enrolling at the flagship for ineligible students. Students ineligible for the Pell premium were 3.8 percentage points (roughly 25 percent) less likely to attend the flagship. In contrast, the sample of students from counties farther than 50 miles to the flagship appear to be much less affected by the Pell premium.

The Pell premium might also differentially impact student based on their academic ability. Since the premium compensates colleges for taking additional risk, the premium should have a smaller effect on the highest and lowest achieving students because their outcomes are better

[63] 45 percent of students attending a public four-year college attend their nearest option, and 80 percent of students attending a public two-year college attend their nearest option.

known. The highest achieving students will graduate with a high probability, and the lowest achieving students will graduate with a low probability. Therefore, the premium is likely to have a larger effect among the academically marginal students. The bottom left panels of Figure 8 show the enrollment differences by academic ability measured using ACT scores. I split students into groups based on the ACT distribution of students enrolling in the flagship in the pre-policy change years. The figures show little difference in enrollment among students scoring above the $75^{th}$ percentile or below the $25^{th}$ percentile. However, among students scoring in the $25^{th}$-$75^{th}$ percentile, students ineligible for the premium are 2.9 percentage points (roughly 20 percent) less likely to attend the flagship.

## 2.9 Conclusion

I find little evidence that public colleges in Tennessee responded to the financial incentive to enroll more Pell Grant eligible students. One explanation for the null effects is that many colleges cannot respond to the premium because they already admit most students. Analysis by college selectivity supports this hypothesis. I find that for college-goers around the eligibility threshold, the public flagship responded to equity measure by enrolling more Pell-eligible students. While not conclusive, I provide suggestive evidence that the flagship's response diverted Pell-ineligible students to less-selective public four-year colleges. Even though the equity measure did not have the intended effect on enrollment, I find that the equity measure redistributes appropriations to institutions serving more low-income students.

Table 1: Tennessee Outcome-Based Funding Performance Indicators

| | |
|---|---|
| Progression | Number of students accumulating 24, 48 and 72 credit hours |
| Completion | Number of degrees (bachelor's, associate's, master's, and doctoral) |
| Efficiency | degrees per 100 FTE, six-year graduation rate |
| Other university functions | research and service expenditures, student transfer |

Note(s): These credit benchmarks are the minimum requirements for full-time status after the first, second, and third year of school. Students must complete 120 credits to graduate

Table 2: Value of Pell Premium for Completing a Degree

| | Avg | Min | 25th %tile | 50th %tile | 75th %tile | Max |
|---|---|---|---|---|---|---|
| *A. Public Four-year* | | | | | | |
| 2011-12 | 3280 | 2879 | 3257 | 3345 | 3377 | 3424 |
| 2012-13 | 3337 | 2902 | 3297 | 3435 | 3450 | 3466 |
| 2013-14 | 3612 | 3125 | 3554 | 3739 | 3745 | 3752 |
| 2014-15 | 3690 | 3170 | 3619 | 3832 | 3835 | 3846 |
| *B. Public Two-year* | | | | | | |
| 2011-12 | 1535 | 1105 | 1278 | 1519 | 1727 | 1934 |
| 2012-13 | 1552 | 1117 | 1292 | 1536 | 1746 | 1955 |
| 2013-14 | 1675 | 1206 | 1394 | 1658 | 1884 | 2110 |
| 2014-15 | 1695 | 1220 | 1410 | 1677 | 1906 | 2134 |

Sources: Notes: The table shows summary statistics for the nominal value of the Pell premium for completing a degree over time and by sector based on the Author's calculation. The calculation depends on factors that vary between years, between sectors, and between colleges.

Table 3: Summary Statistics

| | Full Sample | Restricted Sample |
|---|---|---|
| | (1) | (2) |
| *A. Baseline Demographics* | | |
| Pell Eligible | 0.60 | 0.58 |
| White | 0.69 | 0.78 |
| Black | 0.26 | 0.17 |
| Female | 0.55 | 0.53 |
| Mother had a college degree | 0.40 | 0.44 |
| Father had a college degree | 0.33 | 0.35 |
| ACT Taker | 0.91 | 0.94 |
| Max ACT score | 19.99 | 20.57 |
| Sent ACT score anywhere | 0.69 | 0.71 |
| Sent ACT score to TN public 4-yr | 0.55 | 0.57 |
| Sent ACT score to TN public 2-yr | 0.18 | 0.19 |
| Sent ACT score to other college type | 0.57 | 0.59 |
| *B. Outcomes* | | |
| Enroll anywhere | 0.72 | 0.77 |
| Enroll TN public 4-yr | 0.43 | 0.44 |
| Enroll TN public 2-yr | 0.34 | 0.36 |
| Enroll OOS | 0.07 | 0.06 |
| Enroll private | 0.15 | 0.14 |
| Observations | 191363 | 33482 |

Notes: The table includes average characteristics for Tennessee public high school graduates who filed the FAFSA in 2011-2014 . The restricted sample includes students whose expected family contribution is +/- $3,000 of the Pell Grant eligibility threshold. ACT score sending behavior is available only for ACT takers. The college sector choice outcomes are conditional on enrolling anywhere.

Table 4: Covariate Balance Test

| | Coeff. (1) | Robust SE (2) |
|---|---|---|
| Female | -0.011 | (0.011) |
| Black | -0.012 | (0.008) |
| White | 0.012 | (0.009) |
| Dependent | -0.001 | (0.002) |
| Mom has college degree | -0.012 | (0.011) |
| Dad has college degree | 0.005 | (0.011) |
| ACT Taker | 0.002 | (0.005) |
| ACT tests taken | -0.008 | (0.025) |
| Max ACT score | 0.005 | (0.103) |
| Sent ACT score anywhere | -0.004 | (0.010) |
| Sent ACT score to TN public 4-year | 0.003 | (0.011) |
| Sent ACT score to TN public 2-year | -0.009 | (0.009) |
| Sent ACT score to other college sector | -0.014 | (0.011) |
| Sent ACT score to flagship | 0.013 | (0.010) |
| Lives within 50mi of flagship | 0.01 | (0.009) |
| Observations | 33482 | |

Notes: Each row represents a separate test for the change in the relevant demographic characteristic at the Pell Grant eligibility threshold. Point estimates for β1 and robust standard errors from equation 1) using a bandwidth of $3,000 are reported in the columns (1) and (2), respectively. (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

Table 5: Pre-policy college enrollment and sector choice outcomes

| | Enroll Anywhere | TN Public 4-year | TN Public 2-year | Out-of-State Public | Private |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Ineligible | 0.010 | -0.010 | 0.021 | -0.006 | -0.005 |
| | (0.012) | (0.012) | (0.017) | (0.006) | (0.007) |
| Observations | 26,547 | 20,031 | 20,031 | 20,031 | 20,031 |
| R-squared | 0.003 | 0.001 | 0.001 | 0.000 | 0.000 |
| Cond. on Enrollment | N | Y | Y | Y | Y |

Notes: The table shows the difference in enrollment and sector choice at the Pell Grant eligibility threshold for the pre-policy change cohorts. The estimates reported are of B1 from equation 1) using a linear functional form and a bandwidth of $3,000. Columns (2)-(5) only include students who enroll in college. The outcomes are listed as the column header. Column (1) contains results for whether a student enrolls in college, Columns (2)-(5) are mutually exclusive college sector choice outcomes. Robust standard errors in parenthesis. (*** p<0.01, ** p<0.05, * p<0.1)

Table 6: College enrollment and sector choice outcomes

| | Enroll Anywhere | TN Public 4-year | TN Public 2-year | Out-of-State Public | Private |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Ineligible | 0.011 | -0.004 | 0.015 | -0.005 | -0.005 |
| | (0.010) | (0.012) | (0.009) | (0.007) | (0.008) |
| Observations | 33,482 | 25,659 | 25,659 | 25,659 | 25,659 |
| R-squared | 0.003 | 0.001 | 0.001 | 0.000 | 0.000 |
| Cond. on Enrollment | N | Y | Y | Y | Y |

Notes: The table shows the difference in enrollment and sector choice at the Pell Grant eligibility threshold for the post-policy change cohorts. The estimates reported are of B1 from equation (1 using a linear functional form and a bandwidth of $3,000. Columns (2)-5) only include students who enroll in college. The outcomes are listed as the column header. Column (1) contains results for whether a student enrolls in college, Columns (2)-(5) are mutually exclusive college sector choice outcomes. Robust standard errors in parenthesis. (*** p<0.01, ** p<0.05, * p<0.1)

Table 7: College enrollment and sector choice specification checks

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| A. Enroll Anywhere | 0.011 | 0.012 | 0.010 | 0.020* |
| | (0.010) | (0.010) | (0.010) | (0.010) |
| B. TN Public 4-year | -0.004 | -0.002 | 0.011 | 0.017 |
| | (0.012) | (0.011) | (0.012) | (0.015) |
| C. TN Public 2-year | 0.015 | 0.011 | 0.013 | 0.003 |
| | (0.009) | (0.009) | (0.011) | (0.010) |
| D. Out-of-State Public | -0.005 | -0.004 | -0.005 | -0.008 |
| | (0.007) | (0.007) | (0.008) | (0.007) |
| E. Private | -0.005 | -0.005 | -0.019* | -0.011 |
| | (0.008) | (0.008) | (0.010) | (0.013) |
| Order Polynomial | 1st | 1st | 2nd | 3rd |
| Controls | No | Yes | Yes | Yes |

Notes: Each cell represents an estimate from a separate regression, where the rows list the outcome and each column is a different specification of equation 1). The specifications differ by the degree of polynomial of standardized EFC included, and whether individual-level controls are included. (*** p<0.01, ** p<0.05, * p<0.1)

Table 8: Mechanisms

| | Baseline | | Selectivity | | | High-Reliance | | Mechanical | | Late Cohorts | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 4-yr | 2-yr | Low | Mid | Flag | 4-yr | 2-yr | 4-yr | 2-yr | 4-yr | 2-yr |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Ineligible | -0.004 | 0.015 | 0.012 | -0.004 | -0.012** | 0.006 | 0.011** | 0.006 | 0.003 | -0.003 | 0.018 |
| | (0.012) | (0.009) | (0.010) | (0.006) | (0.005) | (0.011) | (0.005) | (0.009) | (0.006) | (0.016) | (0.015) |
| Observations | 25,659 | 25,659 | 25,659 | 25,659 | 25,659 | 25,659 | 25,659 | 25,659 | 25,659 | 12,826 | 12,826 |
| R-squared | 0.001 | 0.001 | 0.000 | 0.001 | 0.000 | 0.001 | 0.002 | 0.000 | 0.001 | 0.002 | 0.002 |

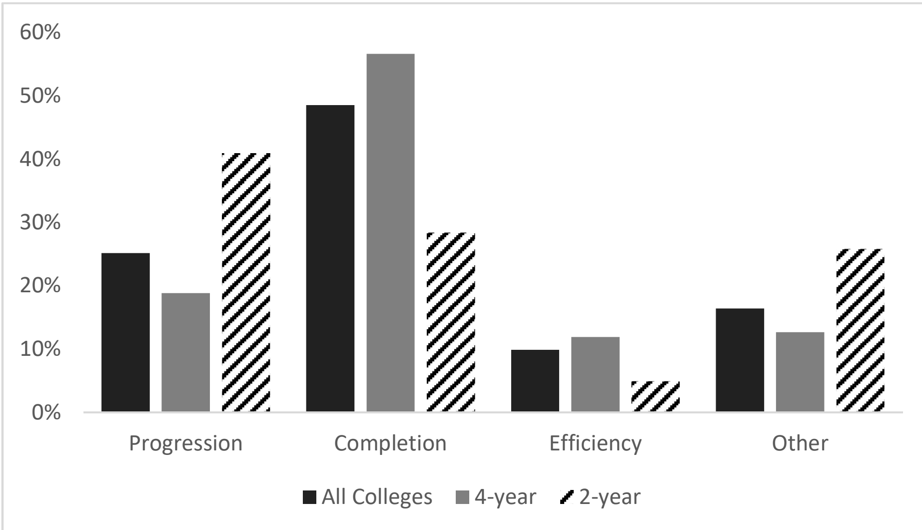
Notes: The table reports differences in enrollment at the Pell Grant eligibility threshold for the post-policy change cohorts. Columns 1 and 2 repeats the baseline specification from Table 6. Columns 3-5 enrollment differences at TN public 4-year colleges by college selectivity. Columns 6 and 7 report differences in enrollment at colleges that rely heavily on funding from the Pell Premium. Columns 8 and 9 report differences in enrollment at colleges that mechanically stand to lose appropriations under OBF. Columns 10 and 11 include students from the 2013 and 2014 high school graduating cohorts. Robust standard errors in parenthesis. (*** p<0.01, ** p<0.05, * p<0.1)

Table 9: Flagship enrollment

| *Outcome:* | Enroll Flagship | Enroll Other TN 4-yr | Enroll Flagship | | | | |
|---|---|---|---|---|---|---|---|
| | Full Sample | | ACT Percentile Score | | | Distance to Flagship | |
| | | | >75th %tile | 25th-75th %tile | < 25th %tile | <50 Miles | >50 Miles |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Ineligible | -0.012** | 0.008 | 0.002 | -0.027* | -0.010 | -0.006 | -0.037** |
| | (0.005) | (0.010) | (0.004) | (0.015) | (0.015) | (0.007) | (0.018) |
| Observations | 25,657 | 25,659 | 15,401 | 6,579 | 2,281 | 21,047 | 4,610 |
| R-squared | 0.000 | 0.001 | 0.000 | 0.001 | 0.010 | 0.000 | 0.001 |

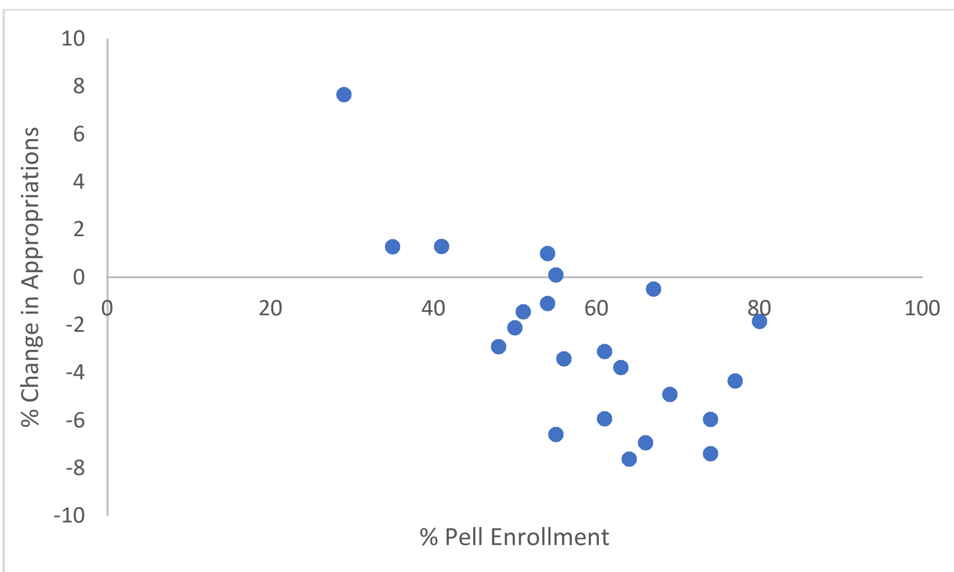
Notes: The table shows the difference in enrollment at the state flagship at the Pell Grant eligibility threshold for the post-policy change cohorts. The estimates reported are of B1 from equation (1 using a linear functional form and a bandwidth of $3,000. The column header contains indicates the subgroup of analysis. Robust standard errors in parenthesis. (*** p<0.01, ** p<0.05, * p<0.1)

Figure 1: Share of Appropriations Awarded by Performance Indicator: 2011-12




Source: 2011-12 Outcomes Formula Data. Notes: The figure summarizes the outcomes of Tennessee public colleges and universities by performance indicator. The share of appropriations earned for performance on indicator $j$ is equal to the ratio of performance on indicator $j$ and total performance, $\frac{Indicator_j}{\sum_j Indicator_j}$. See Table 1 for a description of the performance indicators.

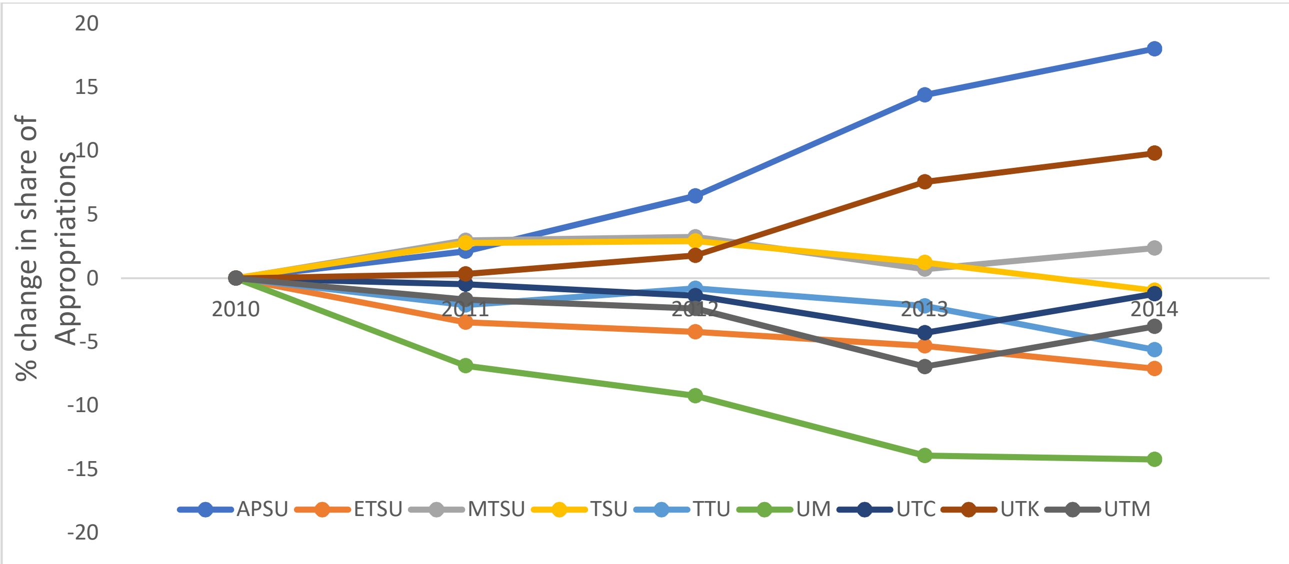
Figure 2: Predicted Change in Appropriations from Removing the Pell Premium



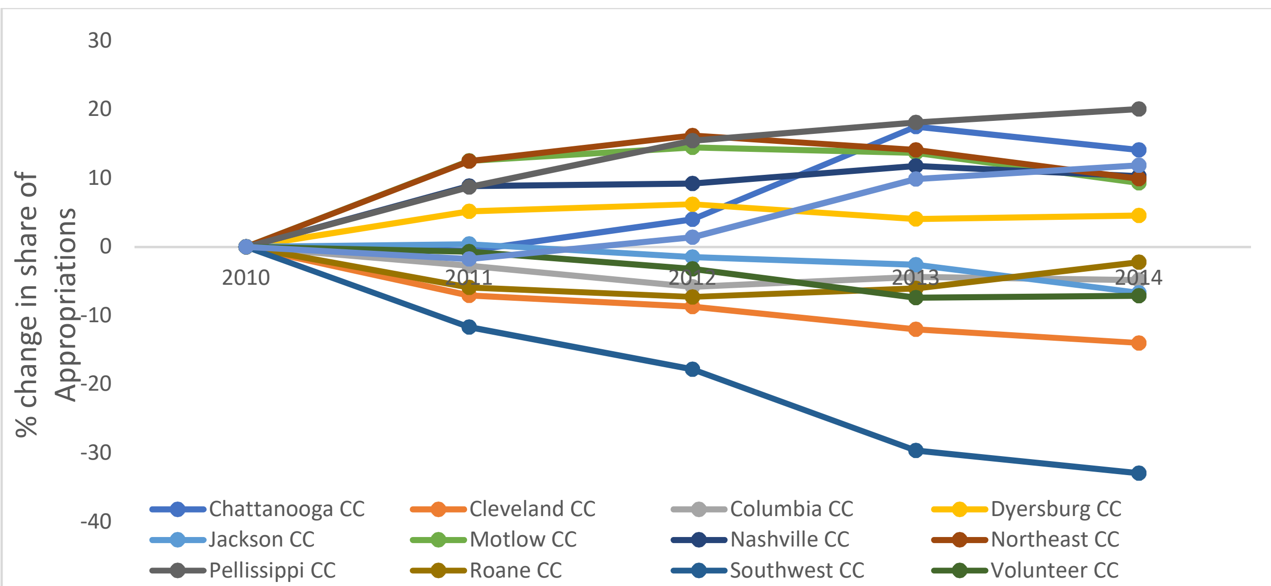

Source: 2011-12 Outcomes Formula Data. Notes: The scatter plot shows the relationship between the predicted percent change in appropriations after removing the Pell premium and the share of students enrolled receiving a Pell Grant. To predict the allocation of appropriations after removing the Pell premium, I use the 2011-12 formula data and set the Pell premium to 0.

Figure 3: Changes in Share of Appropriations by College 2010-2014

A. University System

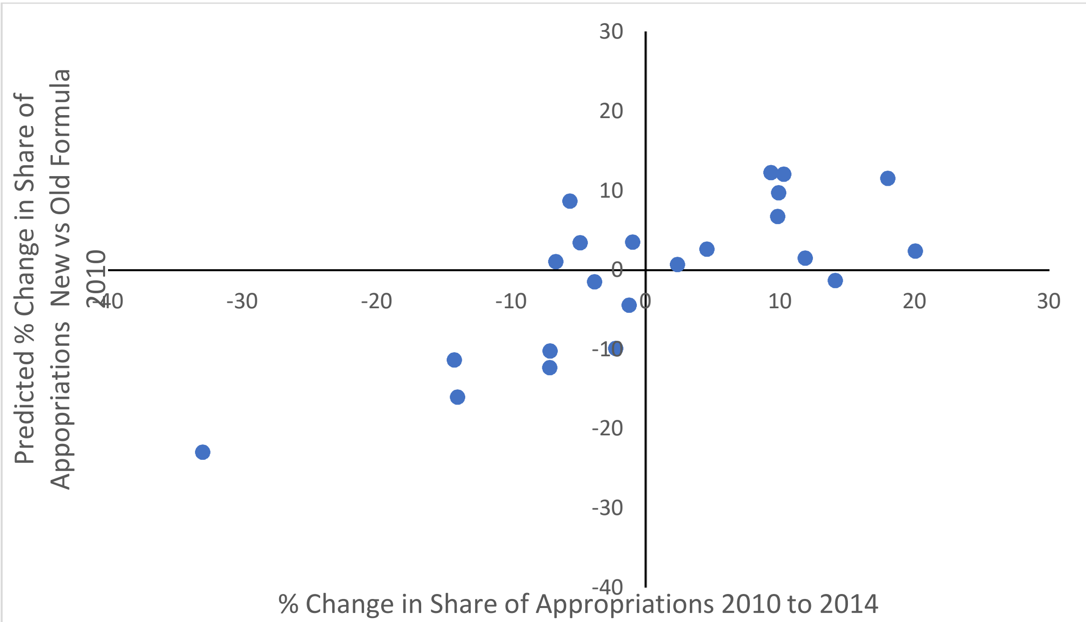



B. Community College



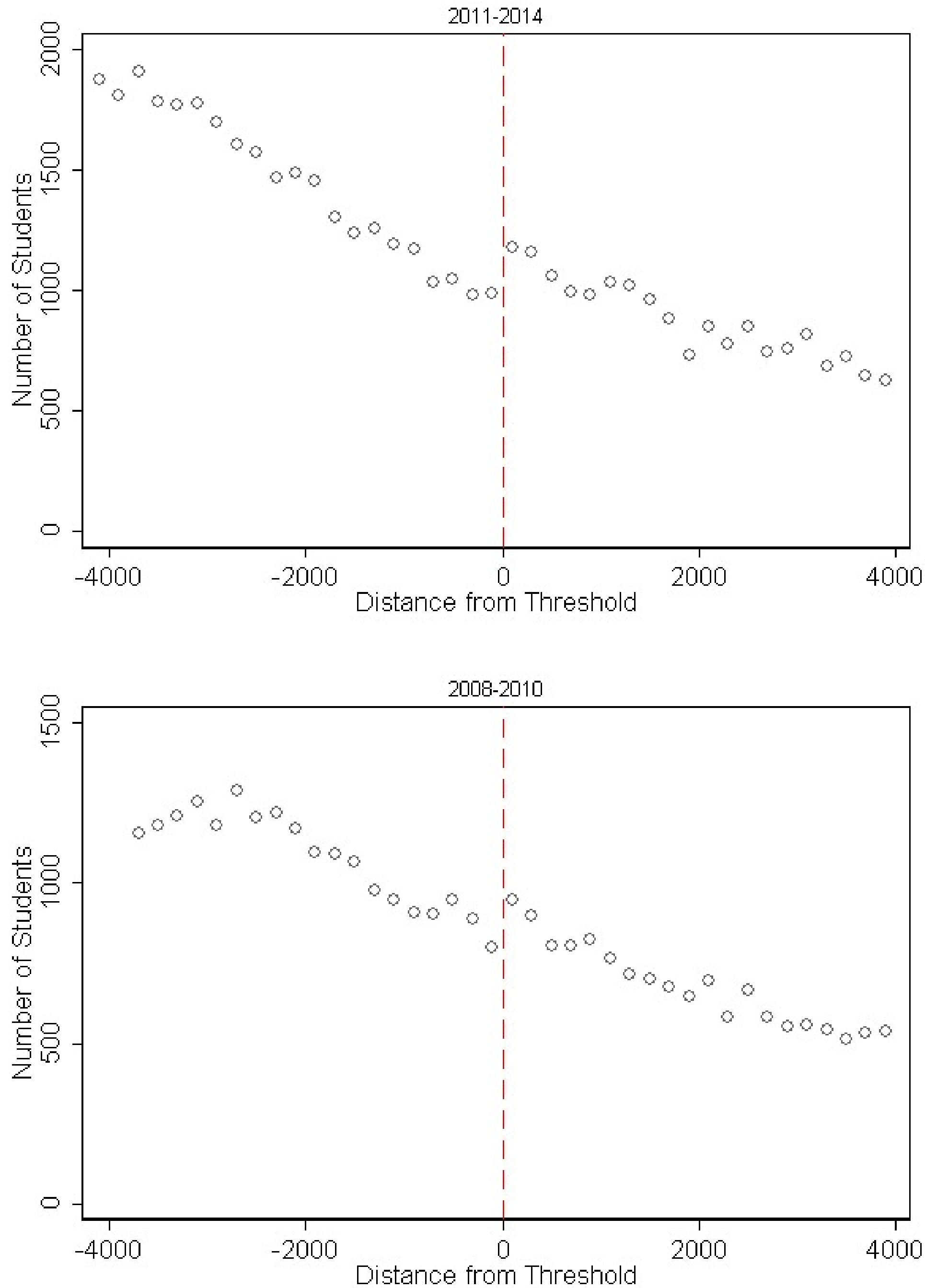

Source: 2014-15 Tennessee Higher Education Fact Book Table 3.16. Notes: The figure shows the annual percentage change in the share of appropriations a college received after OBF was adopted relative to their share of funding in the base year (2010).

Figure 4: Predicted Change in Appropriations from OBF vs. Observed Change in Appropriations



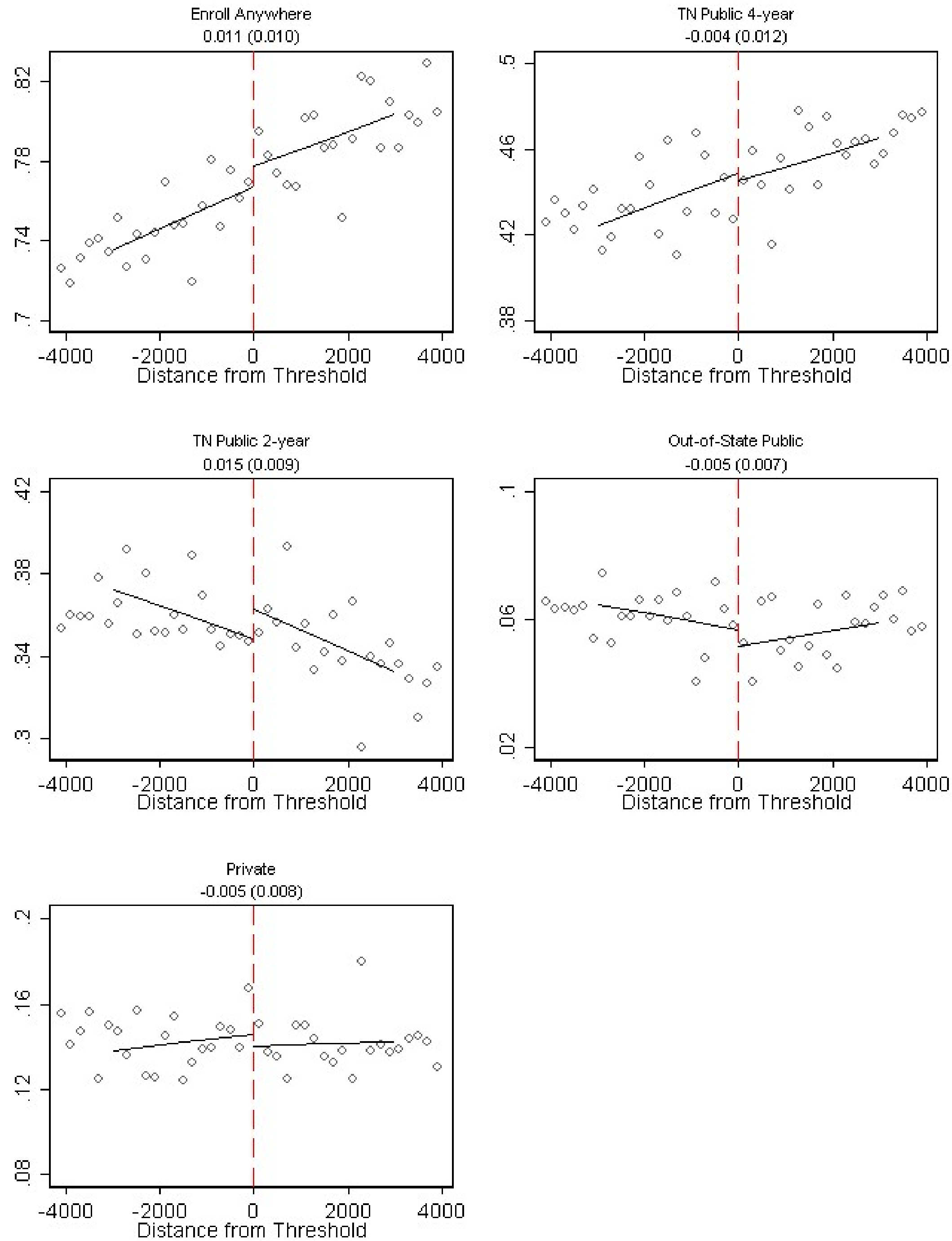

Source: 2014-15 Tennessee Higher Education Fact Book Table 3.16, and 2015-2020 Outcomes-based Funding Formula. Notes: The scatter plot shows the relationship between the predicted mechanical change in appropriations from switching to OBF and the actual change in appropriations since adopting OBF. See text for calculation of predicted mechanical change.

**Figure 5: EFC Distribution**



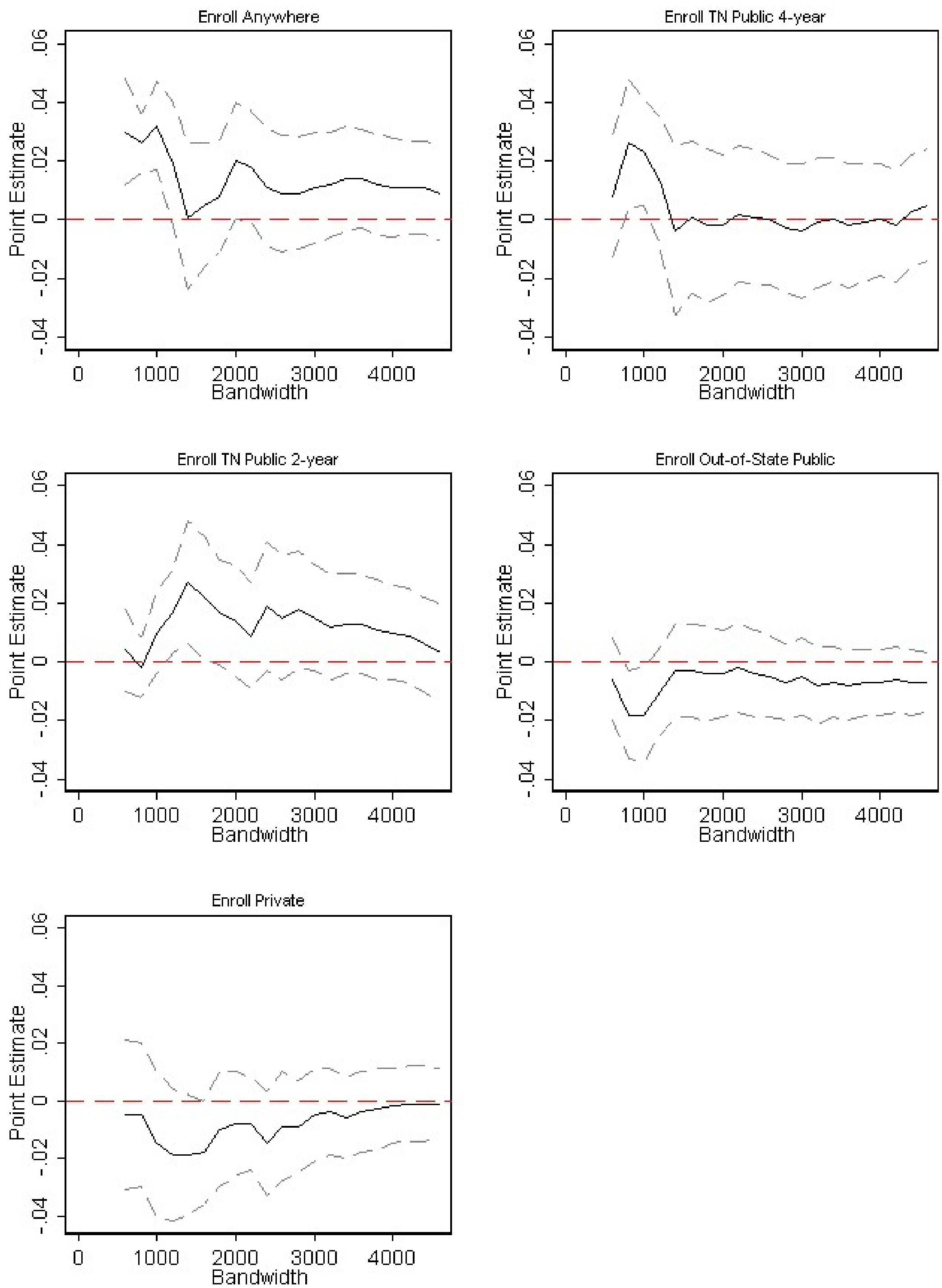

Notes: The figure shows the density of students with an EFC within $4,000 of the Pell Grant eligibility threshold. Each circle represents the number of students in a given $100 EFC bin. The top panel includes the post-policy cohorts and the bottom panel includes the pre-policy cohorts

**Figure 6: College Enrollment and Sector Choice**



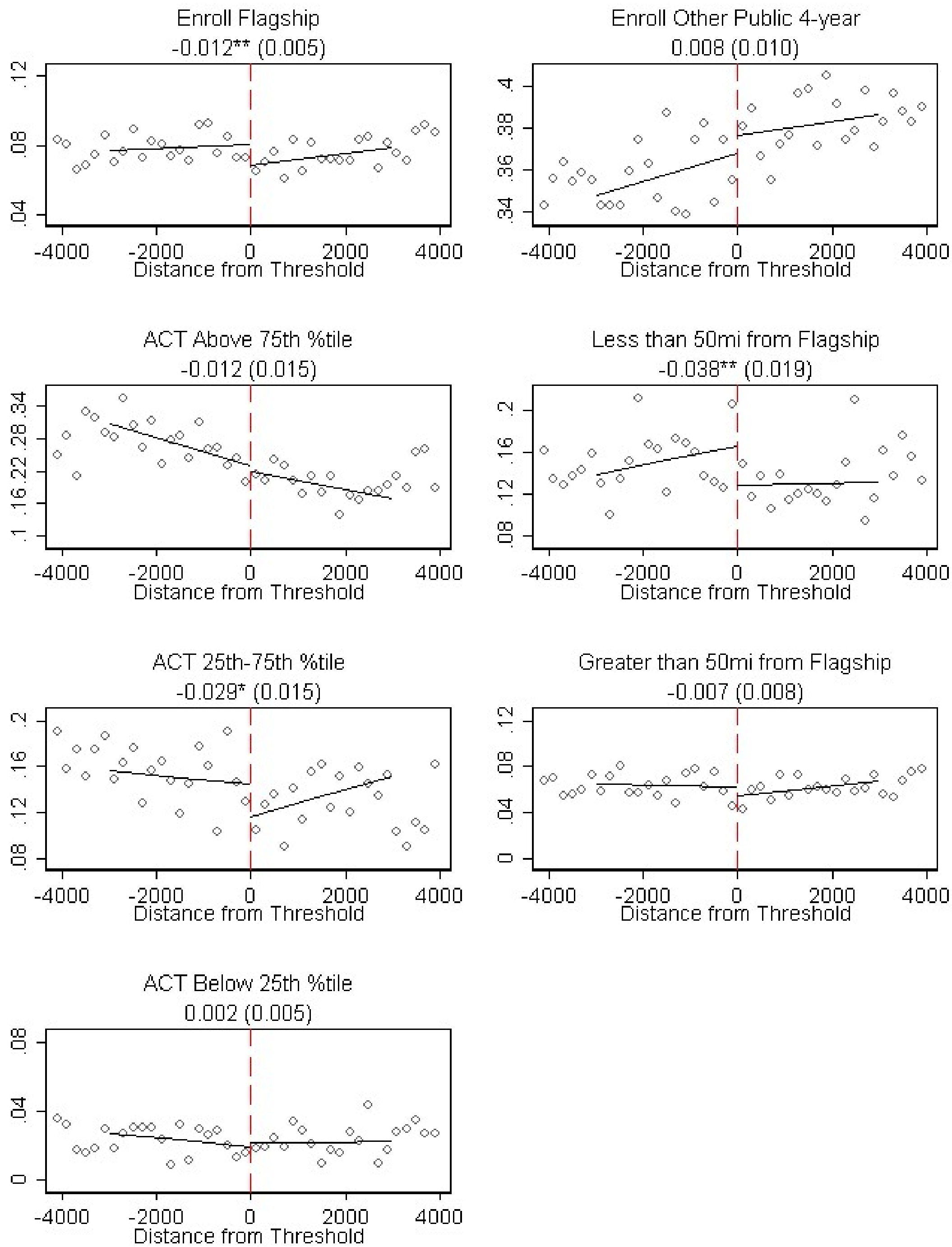

Notes: The figure shows the probability of enrolling as function of distance to the Pell Grant eligibility threshold. The figure heading lists the enrollment outcome. The solid line represents a linear fit of the outcome on EFC estimated seperately on each side of the threshold. Point estimates for $\beta_1$ from equation 1) are reported under the figure heading. (*** p<0.01, ** p<0.05, * p<0.1)

Figure 7: Enrollment and Sector Choice Results by Bandwidth Selection



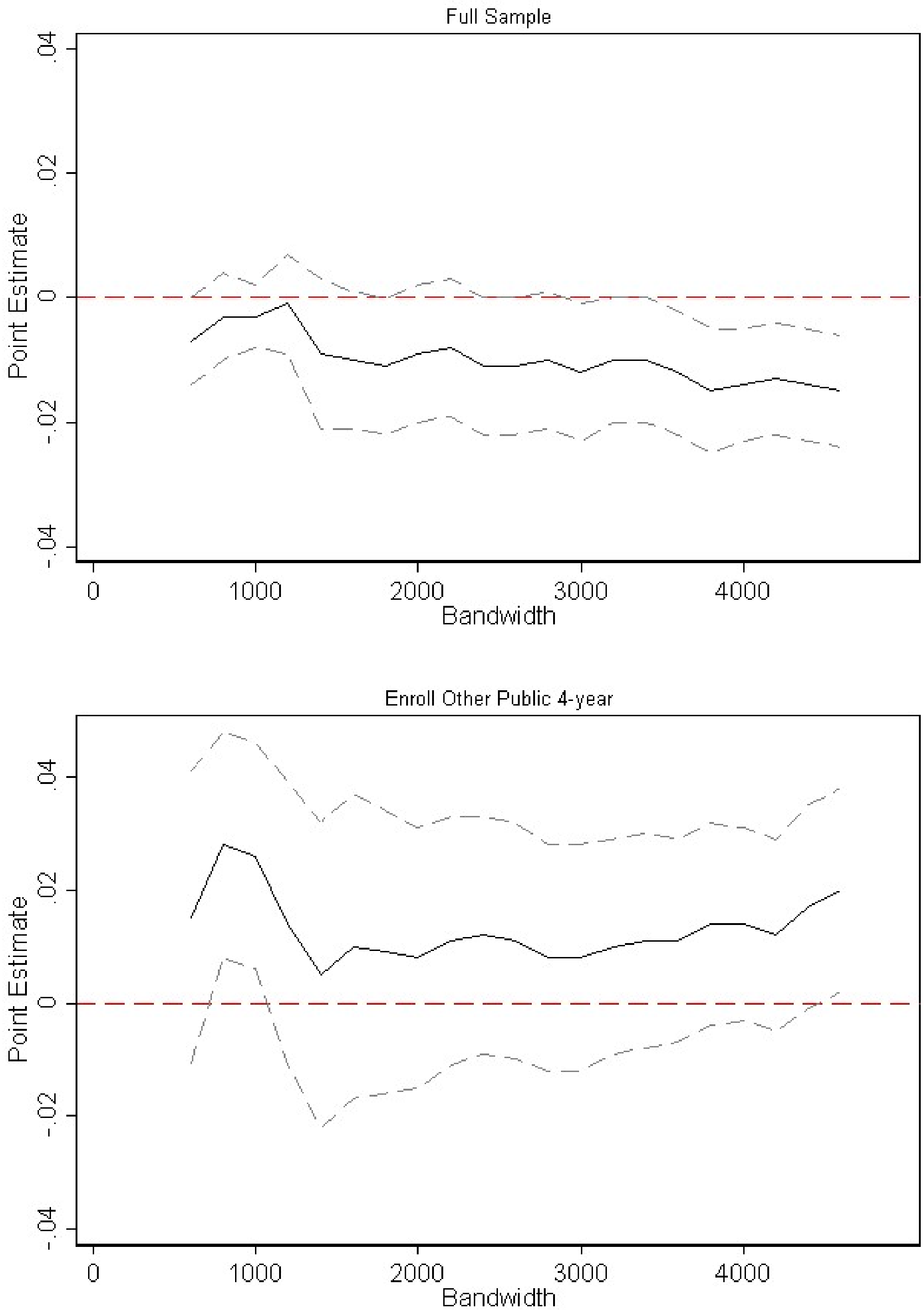

Notes: The figure plots regression discontinuity treatment effects and confidence intervals for college enrollment and college sector by bandwidths ranging from $600 to $4600

**Figure 8: Flagship Enrollment**




Notes: The figure shows the probability of enrolling at the state Flagship as function of distance to the Pell Grant eligibility threshold. The figure heading lists the subgroup. The solid line represents a linear fit of the outcome on EFC estimated seperately on each side of the threshold. Point estimates for $\beta_1$ from equation 1) are reported under the figure heading. (*** p<0.01, ** p<0.05, * p<0.1)

**Figure 9: Tennessee Public Four-year Choice Reults by Bandwidth Selection**




Notes: The figure plots regression discontinuity treatment effects and confidence intervals for enrollmoent at Tennessee public four-year colleges by bandwidths ranging from $600 to $4600

# Chapter 3

# Cash for College Apps: The Effects of Conditional Cash Transfers on Selective-College Enrollment

### with Benjamin Castleman

## 3.1 Introduction

Over the last decade, numerous studies have investigated whether providing students with financial incentives can reduce gaps in educational achievement and attainment. Researchers have investigated the use of incentives with elementary-age students, e.g. paying students to read additional books, all the way through college students, offering performance incentives for students to complete a certain number of credits or maintain a minimum GPA. By paying students for inputs, such as reading books or attending school, or for outputs, such as college credit completion or GPA, the goal of these programs is to motivate students to complete actions or engage in behaviors that position them for longer-term educational success. While evidence suggests input incentives can positively influence student behaviors (Dearden et al., 2008; Dee, 2011; Fryer, 2011), most studies in developed countries find mixed impacts of output incentives on student academic performance (e.g., Angrist et al., 2009; Leuven et al., 2010; Fryer, 2011; Bettinger,2012; Levitt et al., 2016).[64] Fryer (2011) posits that incentives for inputs are more effective because students can exert better control over them—e.g. reading books—than outputs,

[64] Several studies use a hybrid of input and output incentives. For example, Barrow et al., (2014) provided students an incentive to enroll at least half-time (an input) and maintain a C or better (an output),and find positive effects on the number of credits community college students earned. Jackson (2010), which studies the effect of providing students and teachers an incentive for passing AP exams finds a positive effect on AP exam taking. We view this incentive as a hybrid of an input and output incentive because it incentive students to take AP course and exams (an input) as well as passing the test (an output). The passage rates of AP exams are relatively high (nine of the ten most taken exams have a passage rate above 50 percent), so inputs might be particularly important in this setting.

e.g. test scores. Because outputs depend on numerous factors within the education production function, students may not understand how to directly influence them, and therefore are less responsive to output-based incentives.

Despite evidence that input incentives can positively affect student behaviors and achievement, most input-based interventions to date have focused on fairly routine educational practices among elementary- and middle-school age students, such as reading books or attending school. By contrast, there has been little research investigating whether input incentives could positively influence discrete but consequential decisions, such as what middle or high school students choose to attend or whether students choose to take college entrance exams. Specifically, we know of no study that has investigated whether input incentives positively influence which colleges students choose to apply to, despite how important this decision is. Attending a higher-quality college increases both the chances of graduating and future earnings (e.g., Zimmerman, 2015; Hoekstra 2012; Goodman et al., 2015). Yet as many as half of low-income students neither apply to nor attend the quality of institution at which they appear admissible based on their academic credentials, a phenomenon commonly referred to as "undermatching" (Hoxby & Avery, 2013; Bowen, Chingos, and McPherson, 2009; Smith, Pender, and Howell, 2013). Yet low-income students who enroll at higher-quality institutions persist and graduate at similar rates as their high-income peers (Hoxby & Avery, 2013). The undermatching phenomenon has attracted substantial policy focus and investment, in part because the cost to students of expanding their college application set is very low, and where students apply appears to be fairly malleable and influenced by small policy changes (Pope & Pope, 2014; Pallais, 2015; Smith, Hurwitz & Howell, 2015; Hurwitz et al., 2017).

To date, most interventions focused on reducing undermatching have addressed informational barriers that may prevent students from identifying colleges and universities that are a good match for their academic record. For instance, Hoxby and Turner's (2013) Expanding College Opportunity (ECO) intervention sent high-achieving, low-income students customized information packets about well-matched colleges along with fee waivers to apply to these institutions, and found that students who received this information attended substantially higher-quality colleges and universities.

The Hoxby and Turner (2013) intervention, along with others that address undermatch, have largely focused on increasing the share of high-achieving, low-income students that apply to high-quality institutions. Despite the positive impacts that interventions like ECO can have on the quality of students' enrollment, using data from a recent cohort of high-achieving, low- and moderate-income students, we find that even when high-achieving, low-income students apply to top institutions, a sizeable share do not matriculate to selective colleges and universities. In a sample of 5,800 high-achieving, low- and moderate-income students from the high school class of 2016, 84 percent indicated in a survey that they had applied to and were interested in attending a specific selective college or university.[65] Yet only 60 percent matriculated to one of these institutions. One reason is that students did not gain acceptance to a selective college: 7 percent of the sample reported applying to at least one selective college and did not gain acceptance to any. Another reason may be that students did not have a high-quality option near where they live: 17 percent of students were accepted to a selective college and did not attend one. Among these students, 40 percent did not gain acceptance to a selective college within their state. Many college students appear to reveal a preference to attend college relatively close to home. Across the college-going student distribution, the median student attends a college or university that is 13 miles from where she lives (Hillman, 2016).

Balancing the desire to stay relatively close to home and attend a well-matched institution can be difficult for students. In evaluating which colleges to apply to, students--particularly students who would be the first in their family to go to college--may anchor to the colleges they know (a nearby community college) or have heard of (Harvard; popular college sports teams), and not be aware of quality options that are nearby but have a lower profile (Castleman, 2015).

Conditional cash transfers focused on encouraging students to apply to high-quality, geographically-proximate institutions are a potentially high-leverage input incentive approach to substantially improve educational outcomes for low-income youth. The opportunity to earn cash payments (e.g., $100) for applying to specific institutions that satisfy minimum quality benchmarks and which are relatively close to where students live may make these options more salient than information alone about students' postsecondary options. More broadly, given the positive impacts on postsecondary completion and earnings associated with attending high-quality

[65] These data come from a survey completed by students who participated in the CollegePoint initiative. The survey completion rate was 70 percent, but positively selected as the selective college enrollment rate was 6 percentage points higher among respondents. We describe the CollegePoint initiative in section 2.

institutions, leveraging input incentives at this juncture in students' educational trajectories could be an efficient use of scarce resources.

We investigate this hypothesis through a large-scale randomized trial with a national sample of students, in the context of the CollegePoint advising initiative. CollegePoint provided sustained, one-on-one remote college advising through the college and financial aid advising process to over 6,300 students from the high school class of 2018 (we describe CollegePoint in more detail in the next section of the paper). Students were randomized to receive: 1) a customized list of high-quality colleges in their state; or 2) the list of schools and an offer of $100 per application submitted to up to four schools from their list. We also provided both groups with a link to a broader set of over 200 high-quality colleges across the country. This experimental design allows us to investigate the effect of the financial incentive above and beyond any impact on students' application and attendance at selective colleges and universities that stemmed from receiving both remote advising and semi-customized lists of geographically-proximate, high-quality institutions.

We use data from multiple administrative sources as well as self-reported data from surveys to examine the effects of the financial incentive. The College Board provided a rich set of baseline characteristics. Access to data collected by the advising organizations allows us to measure students' response to outreach about the incentive. We measure college enrollment using the National Student Clearinghouse and rely on a survey administered to students the summer following their senior year to measure college applications and acceptances.. , and commitment, from surveys administered to students in the spring and summer of their senior year. The response rate for the spring and summer surveys was over 70percent. In December 2018, we will have college enrollment data from the National Student Clearinghouse,

Overall, we find that offering an incentive to apply to high-quality colleges increased the number of students enrolling at a higher quality college. Treated students were 2.6 percentage points (4.5 percent) more likely to enroll at a high-quality college (as measured by college graduation rate) and 1.8 percentage points (16 percent) less likely to attend a low-quality college (as measured as by whether a school's graduation rate is below the median among four-year colleges). The incentive offer was particularly beneficial for students from backgrounds or contexts in which high-quality information about postsecondary options may otherwise have been lacking. Treatment impacts are nearly double for low-income and first-generation college students. Using self-reported data on application and acceptance outcomes, we show that students in the treatment group were 5.6 percentage points (8.9 percent) more likely to apply to one of the target schools on their list, with more modest effects on the total number of applications a student submitted to high-quality colleges. By changing the mix of high-quality schools that students applied to, the incentive increased the share of treated students accepted to a high-quality school by 3.7 percentage points (4.5 percent). The increase in acceptances persisted to college enrollment.

Our paper builds on several related studies. Hoxby and Turner (2013) combined application fee waivers with semi-customized information about well-matched college options for high-achieving, low-income students. The intervention substantially improved the quality of the institution at which students enrolled. Carrell and Sacerdote (2017) combined fee waivers with peer mentoring and found that the intervention increased the rate at which students enrolled and persisted in college, with effects concentrated among female students.[66] Our paper is the first of which we are aware to isolate the impact of a modest conditional cash transfer on whether students apply to and matriculate at higher-quality colleges and universities.

The remainder of the paper is organized as follows. Section 2 provides background on CollegePoint and the intervention. Section 3 discusses the data and empirical strategy. Section 4 presents results on the implementation of the intervention. Section 5 presents the main results, and Section 6 explores the complementarity of advising and the incentive. Finally, Section 7 concludes.

## 3.2 Background

### 3.2.1 CollegePoint Advising

Bloomberg Philanthropies and America Achieves started CollegePoint in 2014, with the goal of substantially increasing the share of high-achieving, low- and moderate-income students who enroll at high-quality colleges and universities across the country. This initiative was motivated by prior research that high-achieving, low-income students who enroll at top colleges and universities

[66] In ongoing work by Dynarksi, Libassi, Michelmore, & Own, the authors implemented a randomized control trial to attract more low-income, high-achieving students to apply to the University of Michigan by providing them with information about the school and an early commitment of financial aid.

persist at the same rate as their more affluent peers, and by the conviction that providing these students with a high-caliber postsecondary education will lead to positive, lifelong impacts for the students and their communities (Bloomberg Philanthropies, 2016). After a small pilot year with the high school class of 2015, CollegePoint has served thousands of high-achieving, low and moderate-income high school seniors across the country each year, for a total of over 26,000 students. CollegePoint is currently serving over 9,000 students from the high school class of 2019.

Beginning with the high school class of 2015, CollegePoint has worked with various partner organizations, including the College Board, Jack Kent Cooke Foundation, the ACT, and others to promote the opportunity to work with a CollegePoint advisor to high-achieving, lower-income students. To be eligible for the CollegePoint, students must meet the following criteria: (1) family income below $85,000; (2) GPA of 3.5 or higher; and (3) score above the 90$^{th}$ percentile on a college entrance exam.

CollegePoint advising is provided by a consortium of non-profit college advising organizations: College Advising Corps, College Possible, Matriculate, and Scholar Match.[67] CollegePoint recruits students in waves, so students start engaging with an advisor anywhere from the spring of their junior year through the middle of the fall of their senior year in high school. CollegePoint advisors leverage multiple outreach channels to reach and engage with students, including phone calls, mailing, email campaigns, and social media and text messaging outreach, and provide individualized advising and support with college applications, financial aid, and college choice. Because of CollegePoint's focus on increasing the share of high-achieving, low-income students that matriculate to top colleges and universities, advisors place particular emphasis encouraging students to apply to well-matched institutions. CollegePoint uses a list of colleges—CollegePoint schools—with graduation rates above 70 percent to define high-quality or well-matched institutions.[68]

From the high school class of 2018, 77% of students who signed up for CollegePoint and who were offered advising engaged at some point with their advisor. Among engaged students, advisors and students engaged 8 times on average. Advisor-student engagement was most frequent during the fall of senior year when students typically decide which colleges to apply to and complete their

[67] A fifth organization, Strive for College, participated in College Point with the high school classes of 2015 and 2016.
[68] The Appendix includes the list of high-quality colleges, which largely coincides with the list of schools with a Barron's ranking of 1 or 2.

college applications. In a separate paper (still in development) we report on the impacts of CollegePoint advising on the quality of students' postsecondary enrollment. In this paper, we focus specifically on a financial incentive experiment we ran within the CollegePoint program.

### 3.2.2 CollegePoint Financial Incentive

With the generous support of the Thompson Family Foundation, starting with the high school class of 2018 and continuing with the high school class of 2019, CollegePoint added a financial incentive component to the core advising model. The goal of the financial incentive has been to provide additional encouragement for students to consider applying to geographically-proximate, high-quality colleges and universities, in addition to other schools on their college list. Proximity to home may be particularly important for our sample because distance is a strong predictor of college enrollment for low-income students (e.g., Long, 2004; Skinner, 2018). In September 2017 (fall of senior year for the HS class of 2018), we worked with CollegePoint to create a semi-customized list of four colleges and universities that would be eligible for the financial incentive. The lists included four CollegePoint schools from their state. In designing the lists of target schools, we prioritized colleges where a high share of applicants from previous cohorts of CollegePoint students enrolled. For states with fewer than four CollegePoint schools, we included honors colleges from public institutions that met the CollegePoint criteria or CollegePoint schools from neighboring states.[69]

We then worked with CollegePoint to randomly assign students between the treatment and control group. Students received a list of target colleges list or the list along with the offer of up to $400 if they applied to at least two schools on their semi-customized list and two other selective colleges or universities from the broader CollegePoint Schools list.[70] Students in the financial incentive treatment, therefore, received the offer of the incentive, the semi-customized college list, and the opportunity to engage with a CollegePoint advisor. Students in the financial incentive control group received the list and advising but no financial incentive to apply to schools on their

[69] We used graduation rate data for honors colleges from "Inside Honors: Ratings and Review of Sixty Public University Honors Programs" by John Willingham.
[70] In order to receive their incentive, students had to provide their advisor a screenshot of their successful application to eligible colleges or universities by the end of January 2018.

list. In the Appendix, we provide a sample of the college list, with and without the offer of incentive.

We launched the intervention in early October, a time when students were creating their college list and preparing applications. CollegePoint distributed the college lists and incentive offer to students through text message. To minimize sending students information that conflicted with their advising, advisors followed up this digital outreach with the personalized phone, email, and text outreach to students on their caseload. We discussed with advisors the importance of following up with treatment and control groups at similar rates to hold constant the amount of information students received. In January after many application deadlines passed, advisors reached out to the treated students about submitting proof of their applications for verification to qualify for the incentive.

## 3.3 Data and Empirical Strategy

We use data from multiple administrative sources as well as self-reported data to test whether the CollegePoint financial incentives impacted where students enroll in college. The College Board provided a rich set of baseline characteristics, including demographic information, AP and SAT scores, and high school attended.[71] Access to data collected by the advising organizations allows us to measure students' engagement with advising, whether students responded to outreach about the financial incentives, and whether students verified their college applications to receive the incentive payment. We measure students' perception of the incentive using a survey administered in early spring before most students received admissions decisions.[72] We measure college enrollment using administrative data from the National Student Clearinghouse, which captures 92 percent of enrollment nationwide (Dynarski, Hemelt, and Hyman 2015). Finally, we measure college applications and acceptances using a survey administered the summer following senior year to all students, of which over 70 percent responded.

In Tables 1, we provide descriptive statistics and test for balance on covariates for the full sample for the financial incentive experiment and the sub-sample of survey respondents.

[71] We observe a more limited set of baseline characteristics for the 18 percent of students from the ACT universe. Using publicly available data from the National Center of Education Statistics, we merged in high-school level characteristics.

[72] 73 percent of students in the treatment group responded to the survey.

Consistent with the income and academic eligibility requirements for CollegePoint, column 1 shows that our sample is relatively moderate-income and academically high-achieving. The average family income in our sample is approximately $55,300, and approximately 50 percent of students are first-generation.[73] Among students from the College Board who took the SAT before joining CollegePoint, the average score was 1380, which is at the 96th percentile nationally.[74] Additionally, students took 3.5 AP exams before senior year, which is 0.8 more than the national average among AP test takers at the end of senior year (Avery et al., 2017). Comparing column 1 and 3 reveals that the students who responded to the summer survey looked similar to the full sample. Column 2 demonstrates that random assignment successfully balanced the baseline characteristics of treated and control students in the full sample.[75] The characteristics of the survey respondents were also well balanced, as demonstrated in column 4.

We study the impact of incentives by exploiting random assignment of the incentive offer. We assigned 6,347 CollegePoint students to one of two groups: (1) a control group that received a list of four academically well-matched schools from their state; and (2) a treatment group that received the list of schools and the offer to earn up to $400 by applying to schools from their list.[76] By randomizing the incentive among students who received remote advising, and providing the control group with information on well-matched colleges, we can isolate the impact of the incentive from the impact of advising and additional information about well-matched colleges. To accommodate students starting CollegePoint at different times in the fall of their senior year, we randomized students in three batches corresponding to when students signed up for CollegePoint. While all students signed up for CollegePoint, engagement with advising varied considerably. We implemented the intervention when some students were highly engaged with CollegePoint while others had yet to speak with their advisor. Since we hypothesized that the incentive would have a differential effect for engaged students, we also stratified our sample by engagement status.[77]

Using this random assignment, we compute intent-to-treat (ITT) effects by comparing the average outcomes for students assigned to different treatment groups. Our main specification takes the following form:

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[73] Based on the College Board's income tagging algorithm.
[74] https://collegereadiness.collegeboard.org/pdf/understanding-sat-scores.pdf
[75] We tested covariate balance by estimating equation (1) with a demographic characteristic as the outcome.
[76] Students also received access to a broader set of over 200 colleges across the country with a graduation rate above 70 percent. See Section 2 for more details on construction of the lists and design of the outreach.
[77] At the point of randomization, slightly under half of students ever interacted with their advisor.

$$(1)\ Y_{is} = \alpha + \beta_1 Incentive\ offer_{is} + \delta_s + \epsilon_{is}$$

for student $i$ in strata *s,* where $Incentive\ offer_{is}$ is an indicator for whether a student was randomized to the treatment group, and $\delta_s$ is a strata fixed effect to account for how we conducted the randomization. The coefficient of interest $\beta_1$, represents the causal effect of assignment to the financial incentive. We also include baseline covariates to increase the precision of our impact estimates.

To understand the effects of actually receiving the offer of the incentive, we also present separate treatment on the treated (TOT) estimates. Roughly 75 percent of students who responded to a survey recalled receiving the incentive offer, so we define receiving the "treatment" as recalling the offer of the incentive. The TOT impact is the ITT effect divided by the difference in the share treated. We estimate the TOT using two-stage least squares, were we instrument for receiving the offer with the assignment to the treatment group. The resulting first-stage and reduced form equations are given by:

$$(2)\ Incentive_{is} = \alpha + \theta_1 Incentive\ offer_{is} + \delta_s + \varepsilon_{is}$$

$$(3)\ Y_{is} = \alpha + \pi_1 Incentive_{is} + \delta_s + \mu_{is}$$

were $Incentive_{is}$ is an indicator equal to 1 if *i* recalled receiving the offer. Since the offer of the incentive was randomly assigned, it should be a valid instrument for recalling the offer.

# 3.4. Intervention Implementation

Students were offered the incentive while working on their college applications in October and had until February to verify their applications to receive an award. To contextualize our results, we provide descriptive statistics about the communication of the incentive offer to students and the amount awarded. From responses to a survey, we also report how students perceived the incentive offer and its influence on their college search.

## 3.4.1 Notifying Incentive-Eligible Students

The potential effect of the incentive depends on successfully communicating the offer to students. To provide credibility to the incentive offer, we leveraged the existing student-advisor relationship by using advisors to communicate the offer. To supplement outreach done by advisors, we also used a CollegePoint account to text students. Table 2 describes the result of these outreach efforts. Based on advisor-reported records, 56 percent of treated students discussed the incentive with their advisor. Among students who discussed the incentive, they did so on average during two separate advising sessions.

Discussing the incentive does not necessarily fully capture the awareness or knowledge of the offer. To measure awareness, we asked students whether they recalled receiving their list of colleges. Among survey respondents, roughly 75 percent of the treated students recalled receiving their college list. If the survey responses generalize to non-respondents then any enrollment differences between treatment and control students were driven by the 75 percent of treated students who *received* the incentive offer.[78] From the survey, only 65 percent of control students recalled receiving their college list. The difference in recalling the college list between treatment and control students could reflect that the incentive increased the salience of the list for treated students.

### 3.4.2 Award Receipt

Many students who were eligible for the incentive award did not take advantage of the offer. Table 2 shows that only 42 percent of treated students earned an award. The average award among recipients was $332 or $140 (=332*.42) per treated student. Comparing the demographic characteristics of recipients to non-recipients reveals many differences. We summarize the differences across the groups using a single index of expected CollegePoint enrollment based on the endogenous stratification procedure detailed in Abadie, Chingos, and West (2018). Categorizing students by quintile of expected CollegePoint enrollment, Table A2 shows that compared to non-recipients, award recipients were more likely (28 percent vs 15 percent) to be in the top quintile and less likely (12 percent vs 25 percent) to be in the bottom quintile. These differences are both statistically and practically significant.

The low participation rate is surprising given the share of students who completed the actions the offer incentivized. Using survey data on applications, we found that 53 percent of students

[78] Just over 70 percent of the treatment group responded to the survey. Awareness of the incentive offer is likely lower among non-respondents because they were nearly 34 percentage points less likely to discuss the incentive and 37 percentage points less likely to submit for verification compared the survey sample.

received an award but that 87 percent of students were eligible for an award.[79] The difference between eligible and realized awards could be due to a lack of awareness of the incentive or because of the reporting burden to receive an award. For instance, in addition to verifying applications to CollegePoint schools, students had to submit a W-9.

### 3.4.3 Perception of the Incentive

Providing an incentive to apply to specific colleges could impact enrollment if students perceive the schools on their list to be a good fit. To assess how students perceived the incentive, we conducted a survey in early spring before most students received their admissions decisions. Over 70 percent of students responded to the survey, but respondents were more likely to receive an award (66 percent) compared to non-respondents (27 percent). We therefore think the perceptions of the incentive for the survey sample will be more positive relative to the full sample.

Table 3 shows that the incentive had a fairly modest influence on students. Depending on the survey question, 11 or 16 percent of students said the incentive influenced where they applied, and 11 percent of students said they communicated with their advisor more frequently because of the incentive. We then asked which schools we induced them to apply to, which we use in analysis below. Given that the incentive was implemented as part of advising initiative as opposed to a stand-alone intervention, the modest self-reported response is not surprising.

Taking the survey results as given, inducing students to apply to a school does not mean the incentive changed the schools they considered attending. In Table 4, we assess a student's level of interest in attending the schools they were induced into applying to. For the 16 percent of students who were induced into applying to a school by the incentive, we compare their responses to several questions about induced and non-induced schools.

We find that students were less interested in attending their induced schools relative to the non-induced schools in their application set. For example, on average, a student visited 23 percent of their induced schools to compared to 43 percent of their non-induced schools. The difference in search intensity could be due to students having less time to examine their induced schools since

[79] Based on authors' calculation. Our award calculation is lower than actual awards by $50 on average because we do not observe all applications a student submitted or whether they applied early-action or early-decision. Students who applied and were accepted early-action or early-decision were eligible for the maximum award.

we offered students the incentive in October. However, if accepted, only 27 percent of students said they were very likely to attend one of their induced schools, whereas, 93 of students said they were very likely to attend one of their non-induced schools. The lack of state interest in the induced schools could limit the impact of the incentive on enrollment.

## 3.5 Results

Since the ultimate aim of the incentive was to increase enrollment at high-quality institutions, we begin by examining effects on enrollment and show that the incentive increased enrollment at CollegePoint schools. Next, using survey data, we show that treated students were more likely to apply and be accepted to a CollegePoint school. Finally, we show that the incentive was particularly beneficial for students from backgrounds or contexts in which high-quality information about postsecondary options may otherwise have been lacking.

### 3.5.1 Effects on College Enrollment

Table 5 presents the effects of the incentive on college enrollment. Column 1 shows that even in absence of the incentive, over half of CollegePoint students are attending a CollegePoint school. Column 2 shows a 2.6 percentage point (4.6 percent) increase in enrollment at CollegePoint schools. We find a similar magnitude effect on enrollment at target and non-target CollegePoint schools. Column 3 shows that these results are not sensitive to the inclusion of individual controls, which we should expect given the balance of baseline characteristics.

The bottom panel of Table 5 presents impacts of the incentive offer on the characteristics of the colleges students attended. Our measures of college characteristics come from IPEDS and include graduation rate, instructional expenditure, SAT scores of the incoming freshman class, and average net price. We find that treated students attended colleges with a 1.1 percentage point (1.5 percent) higher graduation rate. Figure 1 shows that this effect was driven by a 2.1 percentage point increase in enrollment at schools with a graduation rate between the 75$^{th}$ and 90$^{th}$ percentile and a 2.0 percentage point decrease in enrollment at schools with a graduation rate below the 50$^{th}$ percentile among four-year colleges. The decrease in enrollment at lower quality schools represents a reduction of 17.5 percent. There was a small positive but statistically insignificant effects on instructional expenditures and SAT scores of the incoming freshman class and little difference in average net price. We can rule out differences in the average net price of greater than $500.

The estimates presented so far are all intent-to-treat (ITT) effects, which compare enrollment outcomes for students randomized to receive the incentive offer and the control group. As we demonstrated in the previous section, a non-trivial share (23 percent) of students who responded to our survey did not remember receiving the offer. The unaware of the incentive could be due in part because of the relatively low profile of the offer. Since we implemented the incentive in the context of an advising program, it was used as a tool to increase enrollment as opposed to a standalone policy lever. To provide a sense of the impacts of the incentive if it were conducted by an organization solely devoted to implementing the incentive, we present treatment-on-the-treated (TOT) effects in the last column of Table 5. We find that receiving the offer increased enrollment at CollegePoint schools by 4.1 percentage points (6.9 percent).

To examine whether these results occurred by chance, we also conducted randomization-based inferences tests for significance following Athey and Imbens (2017). In randomization-based inference, uncertainty stems from the random assignment of students to the treatment rather than a sampling of students from a population. Given the randomness in assignment, there is a chance that the observed treatment effects are due to which students were assigned to the treatment group rather than the treatment itself. We compute the likelihood of observing our results by chance using the following procedure from Athey and Imbens (2017):

1. Randomly re-assign students to the treatment group following our randomization procedure.
2. Estimate the treatment effect under the re-assignment and store the estimates
3. Repeat the Steps 1 and 2 2,000 times
4. Compute the share of iterations where the estimated treatment effect is higher in absolute value than our observed treatment effect. The resulting share is the exact p-value calculated from the distribution of effects.

We present the results of the randomization-based inference tests in Table 6 and Figure 2. Figure 2 shows the distribution of estimated effects with a dashed line showing the observed effect. In the 2,000 iterations of the randomization procedure, we estimated a treatment effect on

CollegePoint enrollment larger than the observed effect only 75 times or in 3.8 percent of the iterations. It is thus unlikely that our observed effects are due to random chance.

### 3.5.2 Effects on College Applications and Acceptances

To investigate how the incentive offer affected key inputs—college applications and acceptances--that could contribute to the enrollment impacts we observe, we leverage students' self-reported application behavior and acceptance outcomes from a survey completed by 70% of the experimental sample. In Table 1, we demonstrate that the treatment and control students who responded to the survey respondents were similar along baseline measures, which should reduce concerns that our survey results are due to differences in the characteristics of the respondents.[80] As we discussed in the previous section, survey respondents were more likely to discuss the incentive with their advisor than non-respondents. If survey respondents were more aware of the incentive, then our results could be an upper bound for the full sample.[81]

The top panel of Table 7 and Figure 3 shows that the incentive offer had a large effect on which CollegePoint schools a student applied to and smaller effects on the number of applications students submitted. The incentive was structured to encourage students to apply to two of the four target schools from the list we sent them, and a total of four CollegePoint schools. We find that treated students were 5.3 percentage points (19 percent) more likely to apply to at least two of the target schools from their list. Column 3 shows that treated students were 3.1 percentage points more likely to apply to four or more CollegePoint schools.[82] The incentive could influence enrollment if it induced students to apply to colleges with a higher acceptance rate, or institutions where students are more likely to attend if accepted.

By changing the mix of CollegePoint schools that students applied to, the incentive increased the share of treated students accepted to a CollegePoint school. The bottom panel of Table 7 shows that treated students were 3.7 percentage points (4.5 percent) more likely to be accepted to any CollegePoint school. The increase in the share of students accepted was concentrated among

[80] The response rate for treated students was 1.3 percentage points (or 1.8 percent) higher.
[81] In table A3 we show that the enrollment effects for the survey sample was 3.4 percentage points, which is 0.8 percentage points higher than the full sample effects reported in Table 3.
[82] This estimate includes baseline covariates in the model. The uncontrolled estimate in column 2 is of a similar magnitude but not statistically significant.

colleges from their list. Treated students were 5.8 percentage points (12 percent) more likely to be accepted to at least one target school and 2.1 percentage points more likely to be accepted to a CollegePoint school that was not on their list of target schools; this latter difference which is not statistically significant.[83]

### 3.5.3 Subgroup Impacts

Figures 4-6 document the heterogeneity in treatment effects for applications, acceptances, and enrollment by income, parental education, and high school advising resources, where we measure advising resources using the student-to-counselor ratio. [84] We also examine effects by the supply of selective colleges within a student's state. The incentive could potentially have a larger effect on applications for students with fewer in-state options by increasing the salience of their limited in-state options.[85]

The incentive generated larger impacts for groups we expect to have less information about their selective college options in the absence of the treatment. Figure 4 shows a 6 percentage point effect on enrollment at a CollegePoint school among low-income students and a near 0 effect among non-low-income students. We find a similar pattern for first-generation college students, who were 5 percentage points more likely to enroll at a CollegePoint school. Figures 5 and 6 show that treatment effects on applications and acceptances were also larger for low-income and first-generation students. Specifically, Figure 6A shows that low-income and first-generation students were 6 percentage points more likely to be accepted to a CollegePoint school while non-low-income and non-first-generation students were only 2 percentage points more likely to be accepted to a CollegePoint school. Similar to the results by income and parental education, we also see bigger effects on applications and acceptances among students from high schools with low-advising resources. However, the enrollment differences by advising resources were minor. Figure

[83] Comparing the magnitude of the enrollment effects with that of acceptances, we see substantial falloff between acceptance and enrollment at a target college. In future analysis, we will descriptively explore this falloff.

[84] Low advising resources referrers to schools with a student-to-teacher ratio above the median. We calculate the student-to-counselor ratio using high school enrollment data from the Common Core of Data. We measure the number of counselors using the Civil Rights Data Collection supplemented with our ongoing data collection efforts on the location of college advising organizations.

[85] The efect will depend on whether the schools that we induced students to apply to were a good fit for students in terms of quality, price, or distance. See Dynarksi et al. (2018) for a related discussion about the importance of considering a student's choice set when designing an intervention to influence college enrollment.

4 shows that students from a high-school with low advising resources were 4 percentage points more likely to enroll at a CollegePoint school while students from schools with high advising resources was 2 percentage points more likely to do so.

The incentive also differentially affected students by the supply of in-state CollegePoint schools. We find little effect on enrollment for students from low-supply states, but the incentive increased the number of students from middle- and high-supply states to enroll at a CollegePoint school by 4 percentage points. These effects are not individually statistically significant. The lack of an effect among students from low-supply states is surprising given the positive effect on applications and acceptances to CollegePoint schools. Specifically, Figure 5C shows that a 4 percentage point effect on applying to any CollegePoint school and Figure 6A shows a 5 percentage point effect on being accepted to a CollegePoint school.[86] This pattern of results highlights the challenge of increasing the quality of college enrollment among students in states with few geographically-proximate options.

## 3.6 Complementarity between Advising and Incentives

One potential explanation for the effects on applications and college choice is that the incentive increased engagement with advising. If students who received the incentive offer were in turn more likely to seek help and work with their advisor, then part of the overall effect could be due to a higher dosage of college advising. We explore this potential mechanism by comparing engagement and self-reported measures of experience with advising for treatment and control students.

Figure 7 presents differences in the engagement rates by treatment status and month.[87] The control group plot shows that 53 percent of control students engaged at least once in September, and steadily declines to 26 percent by January after most selective college applications are due. During this same period, which coincides with the launch of the incentive, the monthly engagement rate for treated students was 2-4 percentage points higher. In January, after most applications were submitted, there was a 16-percentage point (61.5 percent) increase in the share

[86] In future analysis, we will descriptively explore the falloff between acceptance and enrollment outcomes to examine whether the schools that we induced students to apply to were not a good fit for students in terms of quality, price, or distance.

[87] Results for the survey sample are similar in magnitude and are available upon request.

of treated students engaging. This spike in engagement coincided with when students were verifying their applications to receive their incentive award. Importantly, the figure shows that a higher share of treated students continued to engage with their advisor in March and April when students receive their acceptance letters and deciding where to attend. Ongoing research by Barr and Castleman (2018) show students have less help interpreting financial aid award letters than they do in applying to college and suggests that advising during this time can have a large impact on helping students balance cost and quality considerations.

To examine whether the increase in engagement resulted in a meaningful difference in students' CollegePoint experience, we look at the difference in topics discussed and how students evaluated their advisor. The top panel of Table 8 shows that, based on advisor-reported data, treated students were more likely to discuss key topics related to the application and decision process. Specifically, treated students were more likely to discuss their college list, filling out and submitting college and financial aid applications, and their college enrollment decision. Treated students were also 3.5 percentage points (10 percent) more likely to discuss where to commit with their advisor. The results are larger in magnitude but similar in percentage terms for the survey sample. The bottom panel of Table 8 shows that treated students in the survey sample had a better relationship with their advisor and found advising to be more helpful than control students. For example, treated students were more likely to say they felt close to their advisor, and that their advisor was helpful in the college application and decision process.

Based on the increase in engagement with CollegePoint, advising could be an important mechanism through which the incentive impacted enrollment. It could be that the incentive helped guide students to apply to CollegePoint schools and advising helped students make a more informed decision about where to apply and enroll.

## 3.7 Conclusion

Consistent with prior research evaluating input-based incentives at earlier stages in students' education, we find that students are quite responsive to a conditional cash transfer encouraging them to apply to well-matched colleges and universities. Providing students with a list of target colleges and offering them the opportunity to earn up to $400 by applying to at least four selective institutions led to a 2.6 percentage point increases in the share of students who enrolled at a selective college. Treated students were 1.8 percentage points (16 percent) less likely to commit to a college with a graduation rate below the median among four-year colleges.

We moreover find consistent evidence that making salient geographically-proximate, selective institutions was particularly beneficial for students from backgrounds or contexts in which high-quality information about postsecondary options may otherwise have been lacking. Treatment impacts are nearly double for low-income and first-generation college students. The effects on enrollment were also larger in states with a higher number of in-state selective schools, suggesting that context is a determinant of the incentive's effectiveness.

Using self-reported applications and acceptances, we show that the incentive influenced where students applied and were accepted. Students in the treatment group were 5.6 percentage points (8.9 percent) more likely to apply to one of the target schools on their list and 5.7 percentage points (11 percent) more likely to be accepted to a target school relative to the control group. There were more modest effects on the number of applications students submitted to the broader set of selective colleges. However, treated students were 3.7 percentage points (4.5 percent) more likely to be accepted to a selective college. Since the incentive had only a modest impact on the number of applications submitted to selective colleges, much of our effects on enrollment are likely due to changing which selective schools students applied to.

The incentive may have operated not only by making salient specific colleges to which students should consider applying but also by catalyzing student engagement with professional college advising. Students in the financial incentive treatment maintained consistently higher rates of engagement with their CollegePoint advisor than did students in the control group. As a result of higher engagement rates, treated students were more likely to discuss important topics with their advisor, such as which colleges to apply to or which institution to attend.

This relatively low-touch intervention was able to induce students to attend high-quality colleges at a modest cost. The total expenditure of the incentive was approximately $460,000, or about $140 per treated student. The cost per student is comparable to other financial interventions, like the Advanced Placement Incentive Program, which provided students $100 per AP exam passed.[88] Our intervention is less costly than a similar initiative launched in 2018 in Rhode Island, which provides low-income students up to $2,000 for completing various milestones in the college

[88] Source: http://txrules.elaws.us/rule/title19_chapter74_sec.74.29

matriculation process.[89] Relative to the Expanding College Opportunity informational intervention, however, which cost \$6 per student and led to substantially higher rates of matriculation to top colleges and universities, the incentives we study are considerably more costly. As programs like CollegePoint accumulate multiple cohorts of data, one approach to lower the cost is to target the incentive among students who are most likely to benefit.

While the college commitment results are encouraging, the return on investment depends on the benefits of attending a selective college. The amount spent on the incentive to induce a single student to attend a high-quality college is \$5,384 (=\$140/.026). One factor driving the low cost is that many students did not apply for their award. Even if we eliminated the award application process, the cost per student induced to attend a high-quality college is approximately \$9,230 (=\$240/.026). Even at the higher cost, the incentive will be more than offset through increased tax revenue based on the financial returns to attending a selective college (Hoekstra, 2009). These results suggest that leveraging conditional cash transfers at high leverage decisions points—such as when students are choosing which colleges to apply to—could be an efficient use of resources to reduce socioeconomic disparities in the quality of higher education institution students attend.

[89] Rhode2College provides low-income students up to \$2,000 (\$500 while in high school and \$1,500 once they enroll in college) for completing milestones such as creating a college list, submitting the FAFSA, and submitting college applications. Source: http://rhode2college.org/

Table 1: Descriptive Statistics

| | Full Sample | | | Survey Sample | | |
|---|---|---|---|---|---|---|
| | Mean | Treat Diff | | Mean | Treat Diff | |
| | (1) | (2) | | (3) | (4) | |
| Female | 0.52 | -0.006 | | 0.54 | -0.008 | |
| | | (0.013) | | | (0.015) | |
| Asian | 0.28 | -0.001 | | 0.29 | 0.008 | |
| | | (0.011) | | | (0.014) | |
| Black | 0.07 | -0.014 | * | 0.07 | -0.019 | * |
| | | (0.006) | | | (0.008) | |
| Hispanic | 0.19 | 0.009 | | 0.20 | 0.010 | |
| | | (0.010) | | | (0.012) | |
| White | 0.40 | 0.001 | | 0.36 | -0.004 | |
| | | (0.012) | | | (0.014) | |
| Other race | 0.07 | 0.006 | | 0.07 | 0.005 | |
| | | (0.006) | | | (0.007) | |
| Predicted income | 55296.7 | -323.9 | | 54256.4 | -792.8 | |
| | [22277.0] | ( 613.4) | | [22277.0] | ( 727.6) | |
| Missing income | 0.19 | 0.016 | ~ | 0.18 | 0.012 | |
| | | (0.009) | | | (0.010) | |
| At least one parent earned a Bachelor's degree | 0.33 | -0.013 | | 0.33 | -0.018 | |
| | | (0.012) | | | (0.014) | |
| Neither parent earned a Bachelor's degree | 0.48 | -0.001 | | 0.49 | 0.015 | |
| | | (0.012) | | | (0.014) | |
| Missing parents' education | 0.19 | 0.014 | | 0.18 | 0.003 | |
| | | (0.009) | | | (0.010) | |
| Took SAT before take-up | 0.85 | 0.009 | | 0.85 | 0.001 | |
| | | (0.010) | | | (0.011) | |
| Took AP Exam before take-up | 0.90 | 0.009 | | 0.90 | 0.004 | |
| | | (0.008) | | | (0.010) | |
| Baseline SAT score | 1378.4 | 0.1 | | 1381.1 | 0.7 | |
| | [ 82.4] | ( 2.5) | | [ 82.4] | ( 2.9) | |
| Baseline Number of AP Exams | 3.5 | 0.1 | | 3.6 | 0.1 | |
| | [ 2.4] | ( 0.1) | | [ 2.4] | ( 0.1) | |
| Number of in-state CollegePoint colleges | 13.9 | -0.2 | | 14.0 | -0.3 | |
| | [ 12.1] | ( 0.3) | | [ 12.1] | ( 0.4) | |
| High school student to counselor ratio | 348.0 | -2.7 | | 349.7 | 1.2 | |
| | [ 130.6] | ( 3.5) | | [ 130.6] | ( 4.2) | |
| Observations | 6347 | | | 4437 | | |

Notes: This table reports baseline descriptive statistics and treatment-control differences on baseline characteristics. Column 1 presents the mean for the full sample, and columns 2 presents the treatment-control difference from a regression that includes strata fixed effects. Columns 3 and 4 presents the mean and treatment-control difference among the sample of students who completed the spring or summer survey. Standard errors of the treatment-control difference are reported in parenthesis. (~ p < .10, * p < .05, ** p <.01)

Table 2: Response to intervention outreach

| | Treatment (1) | Control (2) |
|---|---|---|
| Recalled receiving college list on survey | 0.77 | 0.65 |
| Survey respondents | 2237 | 2200 |
| | | |
| Discussed incentive with advisor | 0.56 | |
| Conversations about incentive cond. on ever discussing | 2.2 | |
| Submitted for verification | 0.42 | |
| Award cond. on submitted | 332 | |
| Observations | 3169 | |

Notes: The table presents sample averages for measures of treatment fidelity and responses to the incentive offer. Column 1 includes treated students and column 2 includes control students.

Table 3: Self-Reported Influence of Incentive

| | |
|---|---|
| Influence of incentive on applications | |
| Unaware of incentive | 0.33 |
| No influence | 0.56 |
| Influenced | 0.11 |
| | |
| Did you apply to additional schools you might not have applied to otherwise? | 0.16 |
| | |
| Incentive changed: | |
| How frequently I communicated with my advisor | 0.11 |
| Which colleges I discussed with my advisor | 0.08 |
| What type of information I asked my advisor for | 0.09 |
| When I submitted my applications | 0.07 |
| | |
| Observations | 2312 |

Notes: The table reports how students perceived their response to the incentive offer. The survey was conducted in the spring before most students received admissions decisions.

Table 4: College Search and Preferences for Students who Reported being Influenced by the Incentive

| | Induced Application | Non-Induced Application |
|---|---|---|
| For this college, did you: | | |
| Visit the campus? | 0.23 | 0.43 |
| Contact a faculty member or administrator? | 0.22 | 0.31 |
| Have an admissions interview? | 0.10 | 0.19 |
| Talk about the college with someone who attends now? | 0.40 | 0.55 |
| None of these things | 0.41 | 0.24 |
| | | |
| How likely are you to attend this school? | | |
| Very likely | 0.21 | 0.42 |
| Likely | 0.43 | 0.42 |
| Unlikely | 0.36 | 0.15 |
| | | |
| Visited at least one school | 0.30 | 0.80 |
| Were "very likely" to attend one school if accepted | 0.27 | 0.93 |

Notes: The sample includes the 16 percent of students who reported applying to at least one new school because of the incentive. Students were asked about their college search process and likelihood of attending each school in their application set. Column 1 includes responses for the induced schools and column 2 includes the responses for non-induced schools.

Table 5: Effects on College Enrollment

| | Full Sample | | | | | | | Survey Sample | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Control Mean | ITT Effect | | | | | | Control Mean | TOT Effect | | |
| | (1) | (2) | | | (3) | | | (4) | (5) | | |
| Enrolled Anywhere | 0.87 | 0.002 | (0.009) | | 0.002 | (0.009) | | 0.87 | 0.004 | (0.013) | |
| *A. Enrolled at:* | | | | | | | | | | | |
| CollegePoint school | 0.57 | 0.026 | (0.012) | * | 0.029 | (0.012) | * | 0.59 | 0.041 | (0.019) | * |
| Target school | 0.25 | 0.014 | (0.011) | | 0.013 | (0.011) | | 0.25 | 0.024 | (0.017) | |
| CollegePoint non-Target school | 0.31 | 0.012 | (0.012) | | 0.016 | (0.011) | | 0.34 | 0.017 | (0.018) | |
| Observations | | 6347 | | | 6347 | | | | 4437 | | |
| *B. College Characteristics:* | | | | | | | | | | | |
| SAT mid-point score (in percentile) | 89.3 | 0.363 | (0.303) | | 0.46 | (0.295) | | 89.73 | 0.53 | (0.453) | |
| Instructional expenditures per FTE ($1,000s) | 23.1 | 0.645 | (0.575) | | 0.883 | (0.560) | | 24.30 | 1.62 | (0.910) | ~ |
| Avg Net price: Family Income 30-48k | 12127 | 116 | (163.844) | | 103 | (162.932) | | 11999 | 11.46 | (252.784) | |
| Avg Net price: Family Income 48-75k | 15946 | 55 | (169.927) | | 52 | (168.425) | | 15832 | -32.73 | (261.211) | |
| Graduation rate | 0.74 | 0.011 | (0.005) | * | 0.013 | (0.005) | ** | 0.75 | 0.02 | (0.007) | * |
| Observations | | 5503 | | | 5503 | | | | 3886 | | |
| Strata dummies | | X | | | X | | | | X | | |
| Covariates | | | | | X | | | | X | | |

Notes: This table reports effects of the financial incentive on college enrollment. The college quality sample is restricted to the sample of students who committed to a college. Columns 1-3 include the full sample and columns 4 and 5 include the survey sample. Columns 1 and 4 reports the control group average for each outcome for the full and survey sample, respectively. Columns 2 and 3 report results from equation (1), a regression of the outcome on an indicator for assignment to the incentive. The baseline specification includes strata fixed effects, and the covariate adjusted sample adds student baseline covariates. Column 5 includes TOT impact estimates, where we define receiving the "treatment" as recalling the offer of the incentive. We estimate the TOT using two-stage least squares, were we instrument for receiving the offer with assignment to the treatment group. The resulting first-stage and reduced form equations are given by equations (2) and (3). Standard errors are reported in parenthesis. (~ $p < .10$, * $p < .05$, ** $p < .01$)

Table 6: Randomization-Based Inference on College Enrollment

| College Attended | Estimated Treatment Effect (1) | Sampling-based p-value (2) | Simulated effects greater than estimated effect (3) | Randomization-based p-value (4) |
|---|---|---|---|---|
| CollegePoint School | 0.026 | 0.032 | 75 | 0.038 |
| Grad. Rate >90th %tile | 0.003 | 0.831 | 1683 | 0.841 |
| Grad. Rate 75th-90th %tile | 0.021 | 0.037 | 68 | 0.034 |
| Grad. Rate 50th-75th %tile | -0.004 | 0.632 | 1315 | 0.658 |
| Grad. Rate <50th %tile | -0.018 | 0.022 | 45 | 0.023 |
| Not Enrolled | -0.002 | 0.799 | 1647 | 0.823 |

Notes: This table reports effects of the financial incentive on college enrollment using randomization-based inference. Columns 1 and 2 report the estimated treatment effects and standard errors reported in Table 5 and Figure 1. Column 3 reports the number of simulated treatment effects out of 2,000 that were larger in absolute value than the observed estimated treatment effect. Figure 3 presents the results from these simulations. Column 4 reports the exact p-value, which is the percentage of simulated effects greater than the estimated effect. We conducted the randomization-based inference in STATA using ritest (HeB, 2017).

Table 7: Effects on Applications and Acceptances

| | Control Mean | Treatment Effect | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | | | (3) | | |
| *A. Applications:* | | | | | | | |
| Any CollegePoint | 0.88 | 0.016 | (0.009) | ~ | 0.021 | (0.009) | * |
| Any Target | 0.63 | 0.056 | (0.014) | ** | 0.054 | (0.014) | ** |
| 4+ CollegePoint | 0.56 | 0.023 | (0.015) | | 0.031 | (0.014) | * |
| 2+ Target | 0.28 | 0.053 | (0.014) | ** | 0.054 | (0.014) | ** |
| *B. Acceptances:* | | | | | | | |
| Any CollegePoint | 0.82 | 0.037 | (0.011) | ** | 0.04 | (0.011) | ** |
| Any Target | 0.5 | 0.057 | (0.015) | ** | 0.053 | (0.015) | ** |
| Any non-Target CollegePoint | 0.61 | 0.021 | (0.015) | | 0.031 | (0.014) | * |
| Observations | | 4409 | | | 4409 | | |
| Strata dummies | | X | | | X | | |
| Covariates | | | | | X | | |

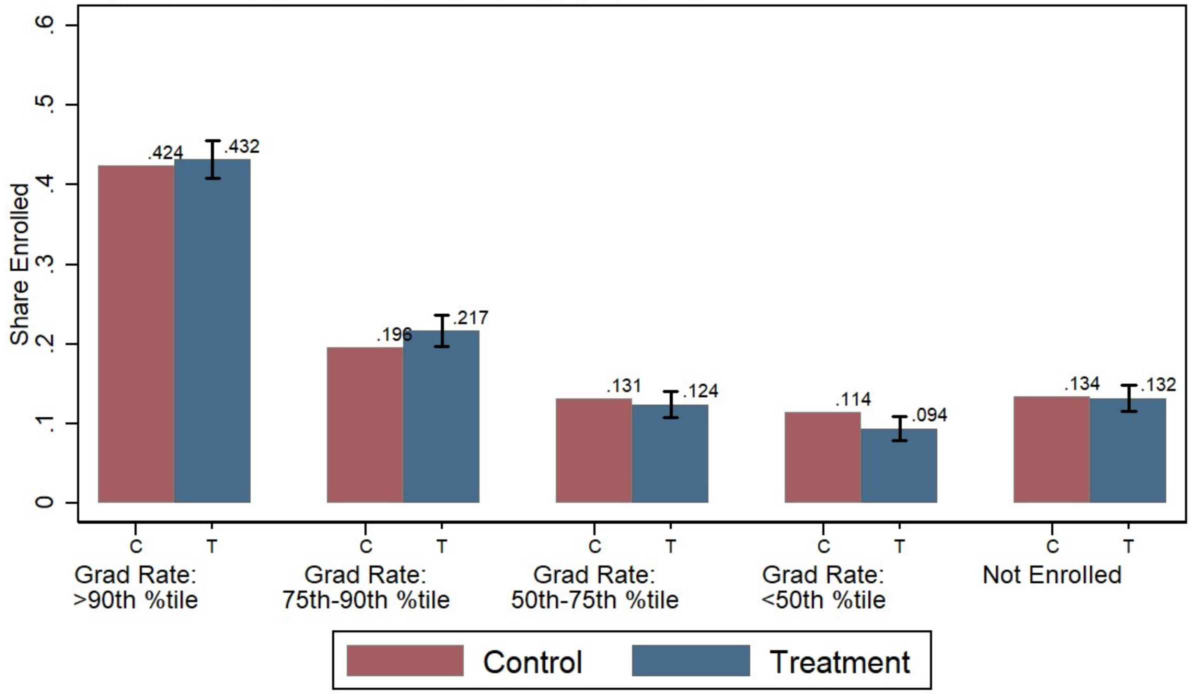
Notes: This table reports effects of the financial incentive on college applications and acceptances. The sample is restricted to students who completed the summer survey. Column 1 reports the control group average for each outcome. Columns 2 and 3 report results from a regression of the outcome on an indicator for assignment to the incentive. The baseline specification includes strata fixed effects, and the covariate adjusted sample adds student baseline covariates. Standard errors are reported in parenthesis. (~ p < .10, * p < .05, ** p <.01)

Table 8: Effects of CollegePoint Experience

| | Full Sample | | | | Survey Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | Difference | | | Control Mean | Difference | | |
| | (1) | (2) | | | (3) | (4) | | |
| *Discussed with advisor:* | | | | | | | | |
| College list | 0.42 | 0.018 | (0.010) | ~ | 0.49 | 0.02 | (0.012) | ~ |
| College application process | 0.56 | 0.034 | (0.011) | ** | 0.63 | 0.046 | (0.012) | ** |
| Financial aid applications | 0.57 | 0.037 | (0.011) | ** | 0.63 | 0.067 | (0.012) | ** |
| College enrollment decision | 0.36 | 0.036 | (0.011) | ** | 0.42 | 0.054 | (0.014) | ** |
| Transition to college | 0.23 | 0.013 | (0.010) | | 0.28 | 0.026 | (0.012) | * |
| *Assessment of CollegePoint advisor:* | | | | | | | | |
| My advisor really listens to me | | | | | 0.56 | 0.047 | (0.014) | ** |
| I feel close to my advisor | | | | | 0.2 | 0.022 | (0.012) | ~ |
| My advisor helped me deal with stress and anxiety about the college process | | | | | 0.4 | 0.008 | (0.014) | |
| My advisor helped me choose where to apply | | | | | 0.37 | 0.054 | (0.014) | ** |
| My advisor helped me decide what college to attend | | | | | 0.31 | 0.031 | (0.014) | * |
| Observations | | 6347 | | | | 4437 | | |

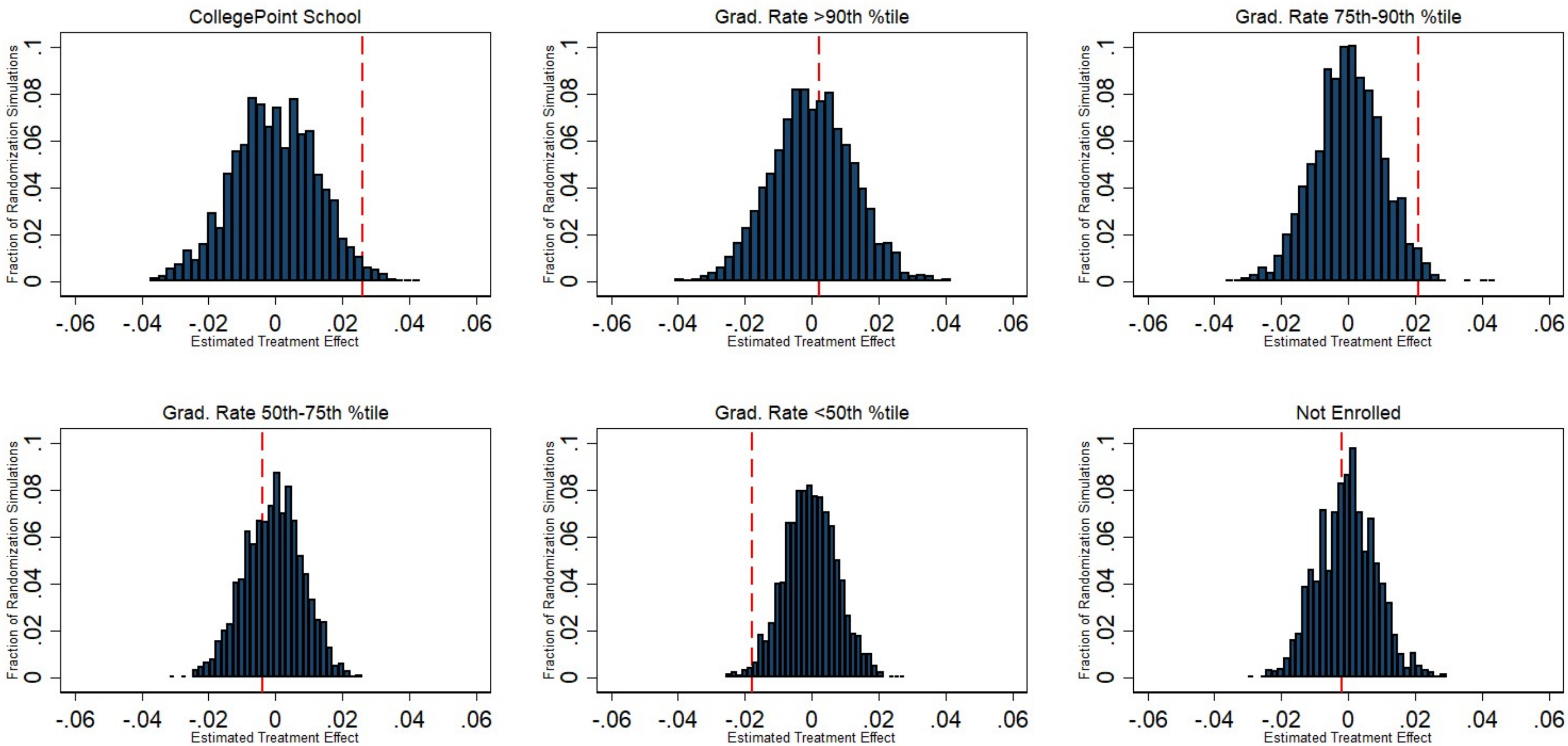
Notes: This table reports effects of the CollegePoint engagement and experience with advising. Columns 1 and 3 reports the control group average for each outcome in the full sample and survey sample. Columns 2 and 4 report results from a regression of the outcome on an indicator for assignment to the incentive for the full sample and survey sample. Standard errors are reported in parenthesis. (~ p < .10, * p < .05, ** p <.01)

Figure 1: Effect on Graduation Rate of College Attended



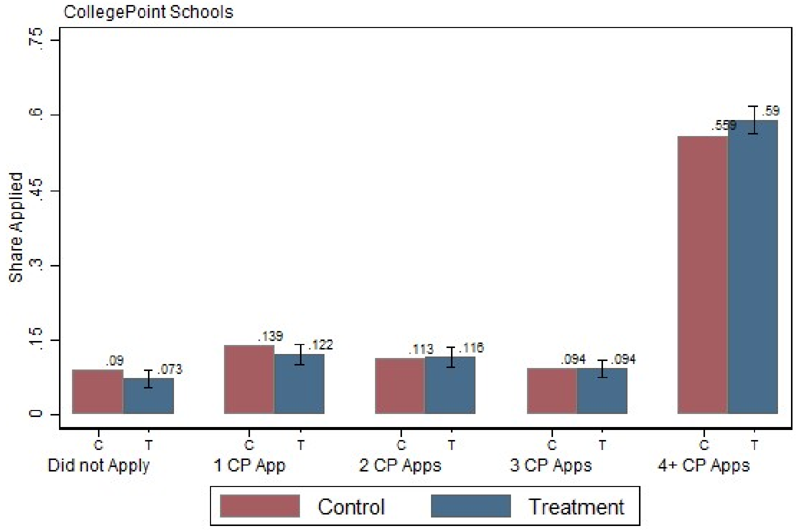

Notes: The figure presents enrollment rates by treatment status. The control bar plots the share of control students enrolled, and the treatment bar plots the sum of the control mean and the estimated treatment effect. The error bars indicate 95 percent confidence intervals. Percentile rankings based six-year graduation rates at four-year colleges from IPEDS

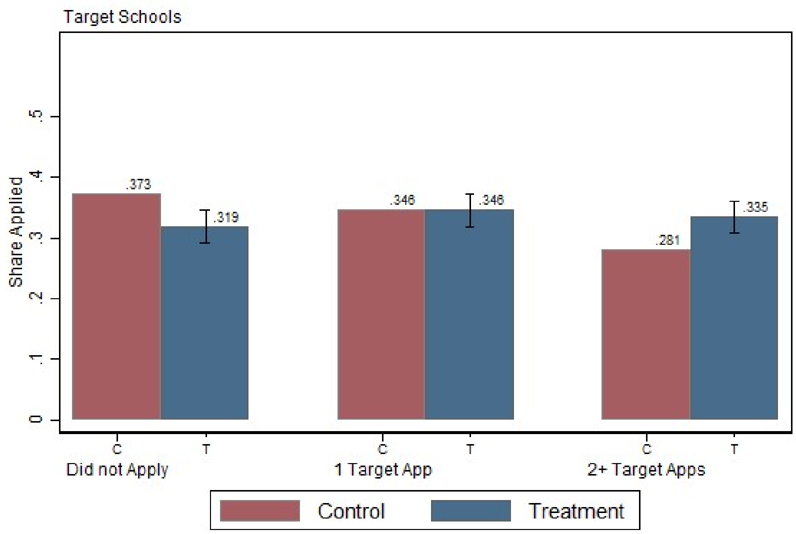
Figure 2: Randomization-Based Inference of College Enrollment




Notes: Each simulated treatment effect comes from first randomly assigning students to treatment using the same randomization algorithm used for true assignment, then running a regression of the outcome on an indicator for assignment to the incentive, including strata fixed effects. Exact p-valueis calculated as the number of simulated effects greater in absolute value than the estimated effect. The dashed line represents the observed treatment effect

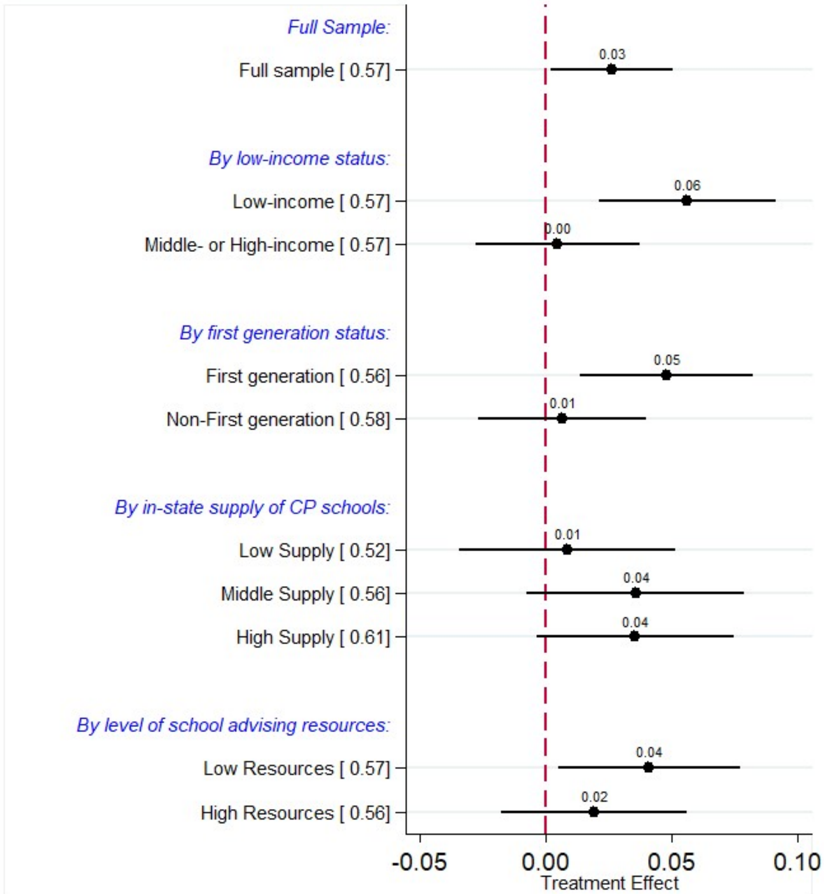
Figure 3: Effects on College Applications



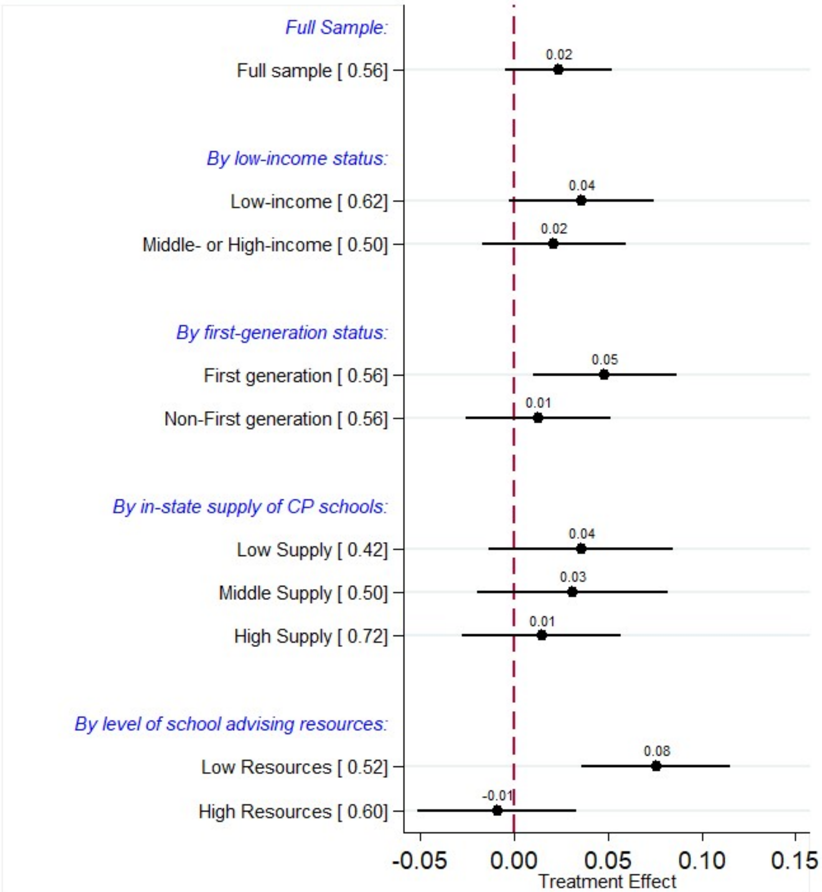

Notes: The figures present applications to CollegePoint and target schools by treatment status. The control bar plots the applications for control students, and the treatment bar plots the sum of the control mean and the estimated treatment effect. The error bars indicate 95 percent confidence intervals.

Figure 4: Effects on Enrollment at CollegePoint School by Sub-Group



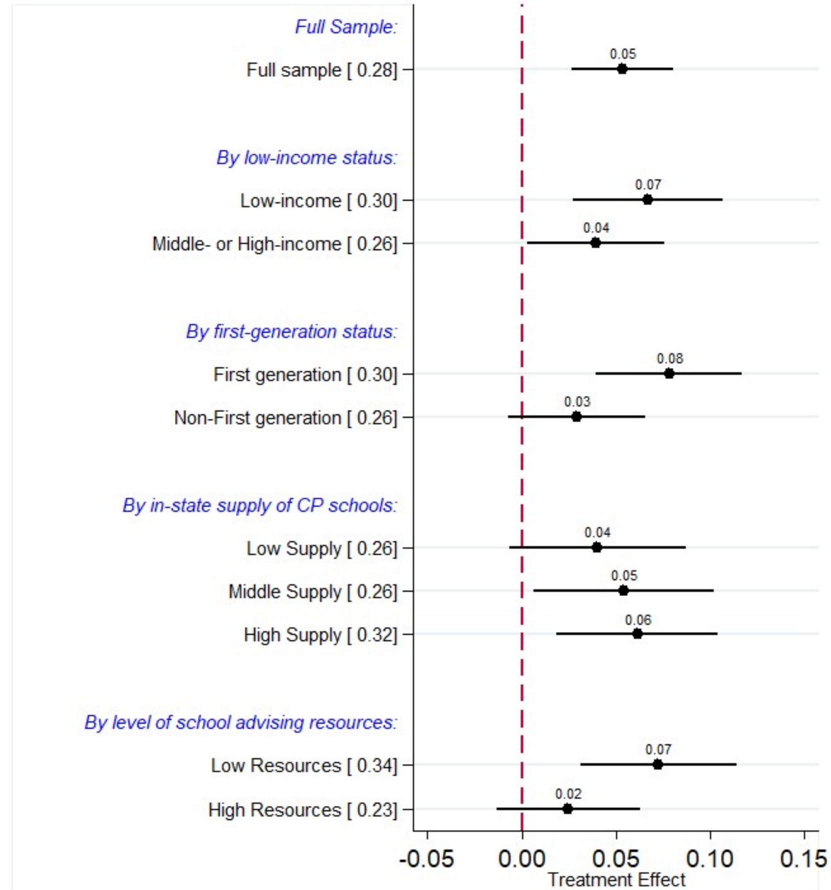

Notes: The figure documents how effects vary by income, parental education, the in-state supply of CollegePoint schools, and high shcool advising resources. The outcome is enrollment at any CollegePoint school. The figure plots the treatment effects for each subgroup separately and displays the control group mean in brackets.

Figure 5A: Effects on 4+ Applications to CollegePoint Schools by Sub-Group



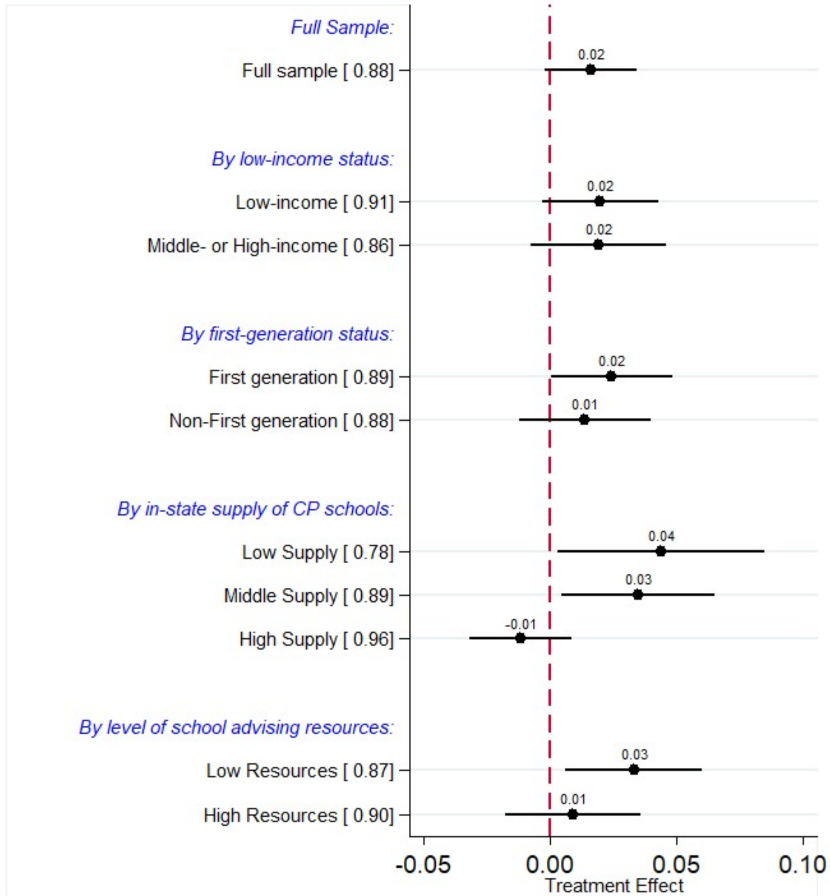

Notes: The figure documents how effects vary by income, parental education, the in-state supply of CollegePoint schools, and high shcool advising resources. The outcome is applying to 4+ CollegePoint schools. The figure plots the treatment effects for each subgroup separately and displays the control group mean in brackets.

Figure 5B: Effects on 2+ Applications to Target Schools by Sub-Group



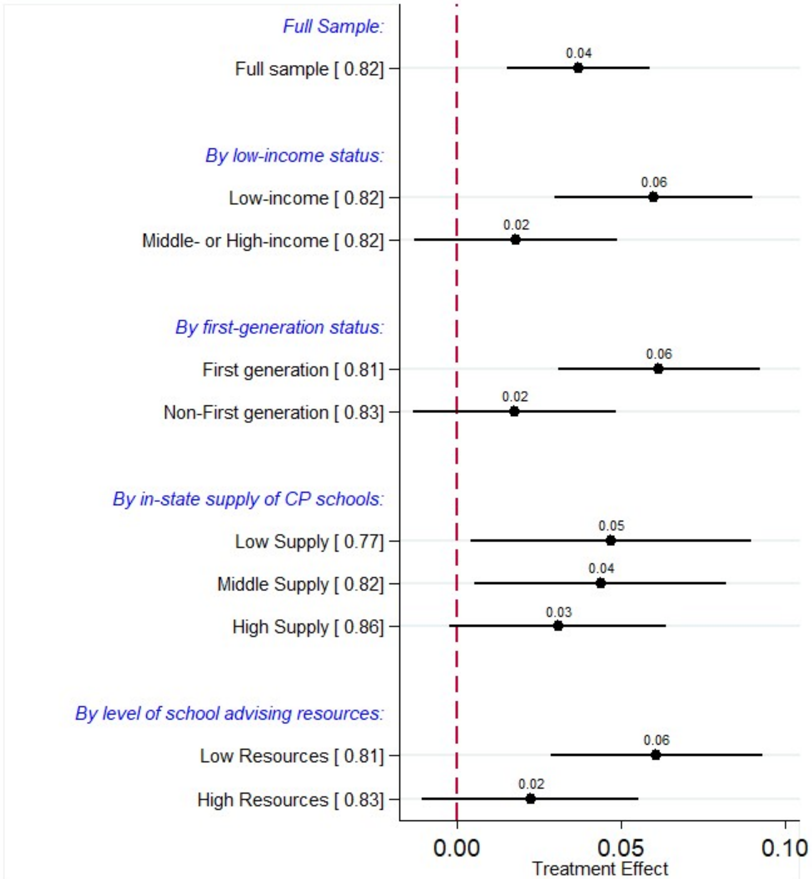

Notes: The figure documents how effects vary by income, parental education, the in-state supply of CollegePoint schools, and high shcool advising resources. The outcome is applying to 2+ schools from their list. The figure plots the treatment effects for each subgroup separately and displays the control group mean in brackets.

Figure 5C: Effects on Applying to Any CollegePoint School by Sub-Group



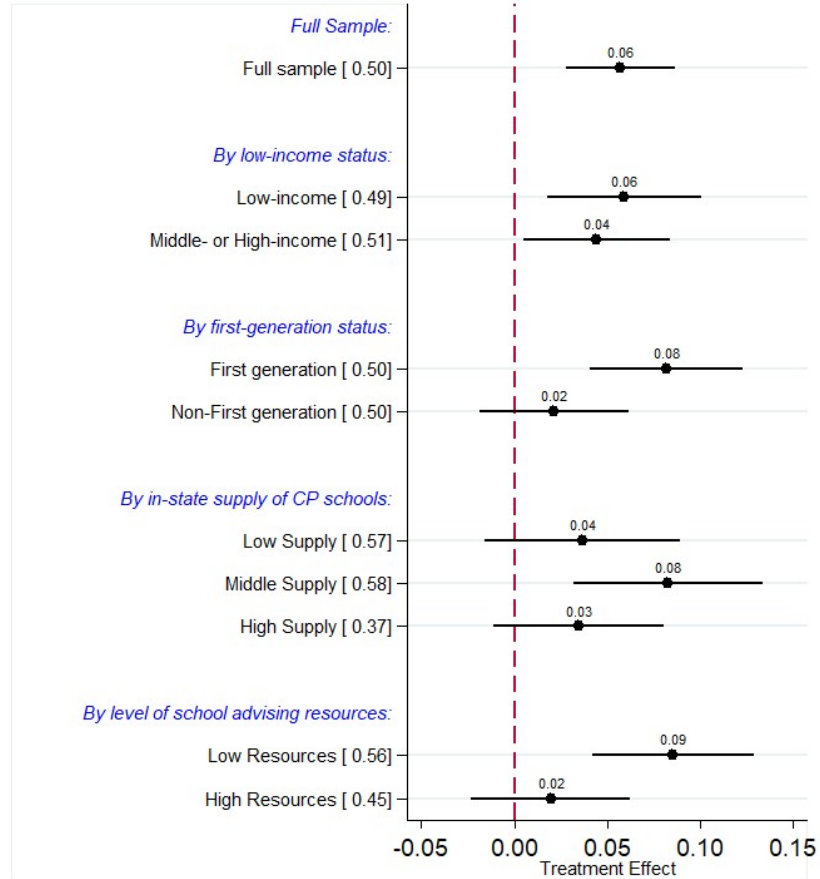

Notes: The figure documents how effects vary by income, parental education, the in-state supply of CollegePoint schools, and high shcool advising resources. The outcome is applying to any CollegePoint school. The figure plots the treatment effects for each subgroup separately and displays the control group mean in brackets.

Figure 6A: Effects on Acceptance to any CollegePoint School by Sub-Group



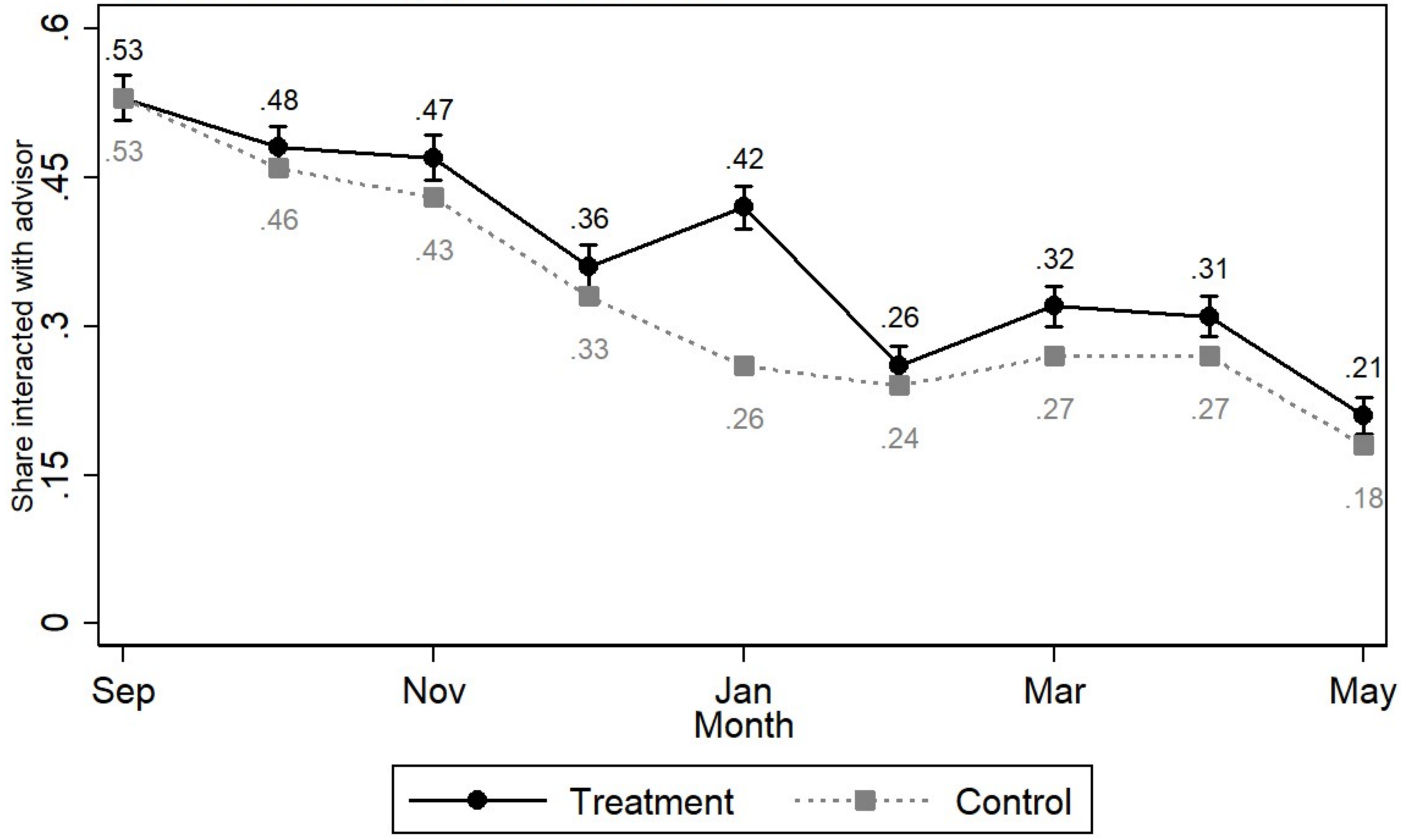

Notes: The figure documents how effects vary by income, parental education, the in-state supply of CollegePoint schools, and high shcool advising resources. The outcome is acceptance to any CollegePoint school. The figure plots the treatment effects for each subgroup separately and displays the control group mean in brackets.

Figure 6B: Effects on Acceptance to any Target School by Sub-Group




Notes: The figure documents how effects vary by income, parental education, the in-state supply of CollegePoint schools, and high shcool advising resources The outcome is acceptance to any school from their list. The figure plots the treatment effects for each subgroup separately and displays the control group mean in brackets.

Figure 7: Effects on Engagement with Advising




Notes: The figure presents engagement rates by treatment status. The grey line plots the share of control students engaged in advisnig, and the black line plots the sum of the control mean and the estimated treatment effect. The error bars indicate 95 percent confidence intervals.

# 3.A Appendix

**Table A1: CollegePoint School List**

Albion College
Allegheny College
American University
Amherst College
Assumption College
Augustana College
Austin College
Babson College
Baldwin Wallace University
Bard College
Barnard College
Bates College
Baylor University
Beloit College
Bentley University
Bethel University
Boston College
Boston University
Bowdoin College
Bradley University
Brandeis University
Brigham Young University-Provo
Brown University
Bryant University
Bryn Mawr College
Bucknell University
Butler University
California Institute of Technology
California Polytechnic State University-San Luis Obispo
Calvin College
Carleton College
Carnegie Mellon University
Case Western Reserve University
Cedarville University
Centre College
Chapman University
Claremont McKenna College
Clark University
Clarkson University
Clemson University
Colby College
Colgate University
College of Saint Benedict
College of the Holy Cross
College of William and Mary
Colorado College
Colorado School of Mines
Columbia University in the City of New York
Concordia College at Moorhead
Connecticut College
Cooper Union for the Advancement of Science and Art
Cornell College
Cornell University
Creighton University
Dartmouth College
Davidson College
Denison University
DePaul University
DePauw University
DeSales University
Dickinson College
Drake University
Duke University
Duquesne University
Earlham College
Elizabethtown College
Elmhurst College
Elon University
Emerson College
Emory University
Endicott College
Fairfield University
Florida State University
Fordham University
Franciscan University of Steubenville
Franklin and Marshall College
Furman University
George Washington University
Georgetown University
Georgia Institute of Technology-Main Campus
Gettysburg College
Gonzaga University
Gordon College
Goshen College
Grinnell College
Grove City College
Gustavus Adolphus College
Hamilton College
Harvard University
Harvey Mudd College
Haverford College
Hendrix College
Hobart William Smith Colleges
Hope College
Houghton College
Illinois State University
Illinois Wesleyan University
Indiana University-Bloomington
Ithaca College
James Madison University
John Carroll University
Johns Hopkins University
Juniata College
Kalamazoo College
Kenyon College
Knox College
Lafayette College
Lake Forest College
Lawrence University
Le Moyne College
Lebanon Valley College
Lehigh University
Lewis & Clark College
Loyola Marymount University
Loyola University Chicago
Loyola University Maryland
Luther College

**Table A1: CollegePoint School List (continued)**

Macalester College
Manhattan College
Marist College
Marquette University
Martin Luther College
Maryland Institute College of Art
Massachusetts College of Art and Design
Massachusetts Institute of Technology
McDaniel College
Messiah College
Miami University-Oxford
Michigan State University
Middlebury College
Moravian College
Mount Holyoke College
Muhlenberg College
Nazareth College
New York University
North Carolina State University at Raleigh
Northeastern University
Northwestern University
Oberlin College
Occidental College
Ohio State University-Main Campus
Pennsylvania State University-Main Campus
Pepperdine University
Pitzer College
Point Loma Nazarene University
Pomona College
Princeton University
Providence College
Purdue University-Main Campus
Quinnipiac University
Ramapo College of New Jersey
Reed College
Rensselaer Polytechnic Institute
Rhode Island School of Design
Rhodes College
Rice University
Ripon College
Robert Morris University Illinois
Rollins College
Rose-Hulman Institute of Technology
Rowan University
Rutgers University-New Brunswick
Saint Anselm College
Saint John Fisher College
Saint Johns University
Saint Joseph's University
Saint Louis University
Saint Mary's College
Saint Michael's College
Saint Norbert College
Saint Vincent College
Salve Regina University
Samford University
Santa Clara University
Sarah Lawrence College
Scripps College
Seattle Pacific University
Seattle University
Sewanee-The University of the South
Siena College
Skidmore College
Smith College
Southern Methodist University
Southwestern University
Spelman College
St Lawrence University
St Mary's College of Maryland
St Olaf College
Stanford University
State University of New York at New Paltz
Stevens Institute of Technology
Stonehill College
SUNY at Binghamton
SUNY College at Geneseo
SUNY Oneonta
Susquehanna University
Swarthmore College
Syracuse University
Taylor University
Texas A & M University-College Station
Texas Christian University
The College of New Jersey
The College of Wooster
The Juilliard School
The University of Texas at Austin
Transylvania University
Trinity College
Trinity University
Truman State University
Tufts University
Tulane University of Louisiana
Union College
United States Merchant Marine Academy
University at Buffalo
University of California-Berkeley
University of California-Davis
University of California-Irvine
University of California-Los Angeles
University of California-Merced
University of California-Riverside
University of California-San Diego
University of California-Santa Barbara
University of California-Santa Cruz
University of Chicago
University of Connecticut
University of Dallas
University of Dayton
University of Delaware
University of Denver
University of Florida
University of Georgia
University of Illinois at Urbana-Champaign

**Table A1: CollegePoint School List (continued)**

University of Iowa
University of Mary Washington
University of Maryland-College Park
University of Massachusetts-Amherst
University of Miami
University of Michigan-Ann Arbor
University of Minnesota-Twin Cities
University of Missouri-Columbia
University of New Hampshire-Main Campus
University of North Carolina at Chapel Hill
University of North Carolina Wilmington
University of Notre Dame
University of Pennsylvania
University of Pittsburgh-Pittsburgh Campus
University of Portland
University of Puget Sound
University of Redlands
University of Richmond
University of Rochester
University of San Diego
University of San Francisco
University of Scranton
University of South Carolina-Columbia
University of Southern California
University of St Thomas
University of the Sciences
University of Vermont
University of Virginia-Main Campus
University of Washington-Seattle Campus
University of Wisconsin-Madison
Ursinus College
Valparaiso University
Vanderbilt University
Vassar College
Villanova University
Virginia Military Institute
Virginia Polytechnic Institute and State University
Wabash College
Wake Forest University
Washington & Jefferson College
Washington and Lee University
Washington College
Washington University in St Louis
Wellesley College
Wesleyan University
Western Washington University
Westminster College
Westmont College
Wheaton College
Wheaton College
Whitman College
Whitworth University
Willamette University
Williams College
Wofford College
Worcester Polytechnic Institute
Xavier University
Yale University
Yeshiva University

Notes: The table includes the list of four-year colleges with a six-year average graduation rate of 70 percent or higher. Average graduation rate is the average of the graduation rates between 2009-10 and 2014-15.

Table A2: Difference in Expected CollegePoint Enrollment between Award Recipient and Non-Recipients

| | Share of non-Recipients in Quintile (1) | Recipient Difference (2) | | |
|---|---|---|---|---|
| Bottom Quintile | 0.25 | -0.13 | (0.014) | ** |
| Second Quintile | 0.21 | -0.03 | (0.015) | * |
| Third Quintile | 0.21 | -0.01 | (0.015) | |
| Fourth Quintile | 0.18 | 0.04 | (0.015) | * |
| Top Quintile | 0.15 | 0.13 | (0.015) | ** |

Notes: Expected CollegePoint enrollment is computed using endogenous stratification detailed in Abadie, Chingos, and West (2018) and implemented in STATA using ESTRAT (Ferwerda, 2014). Column 1 shows the share of non-recipients in a given quintile. Column 2 shows the recipient-non-recipient difference from a regression of a given quintile on an indicator for award receipt. Standard errors are reported in parenthesis. (~ p < .10, * p < .05, ** p <.01)

Table A3: College Enrollment and Commitment Effects for Survey Sample

| | Control Mean (1) | Treatment Effect (2) | | |
|---|---|---|---|---|
| A. Enrollment | | | | |
| Anywhere | 0.88 | 0.004 | (0.010) | |
| CollegePoint school | 0.60 | 0.034 | (0.015) | * |
| Target school | 0.26 | 0.019 | (0.013) | |
| B. Commitment | | | | |
| Anywhere | 0.96 | 0.006 | (0.006) | |
| CollegePoint school | 0.66 | 0.029 | (0.014) | * |
| Target school | 0.28 | 0.022 | (0.014) | ~ |
| Observations | | 4587 | | |
| Strata dummies | | X | | |
| Covariates | | X | | |

Notes: The sample is restricted to survey respondents. The top panel of the table reports the effects on college enrollment using data from the National Student Clearinghouse. The bottom panel reports the same outcomes using self-reported data on college commitment. Column 1 reports the control group average for each outcome. Columns 2 report results from a regression of the outcome on an indicator for assignment to treatment. The specification includes strata fixed effects, and student baseline covariates. Standard errors are reported in parenthesis.(~ p < .10, * p < .05, ** p <.01)

**Appendix Figure 1: Outreach Materials**

<u>Treatment</u>

COLLEGEPOINT

Earn $400 from CollegePoint just by applying to great colleges

| GREAT SCHOOLS FOR YOU | | |
|---|---|---|
| COLLEGE | LOCATION | AVERAGE FINANCIAL AID* |
| University of Michigan, Ann Arbor | MI | $21,934 |
| Michigan State University | MI | $13,632 |
| Albion College | MI | $39,610 |
| Kalamazoo College | MI | $41,040 |

*This is the amount of grant and scholarship aid students like you could expect to be awarded.

EXPLORE MORE GREAT SCHOOLS AT
COLLEGEPOINT.INFO/SCHOOLS

<u>Control</u>

COLLEGEPOINT

Think about applying to these great colleges

| GREAT SCHOOLS FOR YOU | | |
|---|---|---|
| COLLEGE | LOCATION | AVERAGE FINANCIAL AID* |
| University of Michigan, Ann Arbor | MI | $21,934 |
| Michigan State University | MI | $13,632 |
| Albion College | MI | $39,610 |
| Kalamazoo College | MI | $41,040 |

*This is the amount of grant and scholarship aid students like you could expect to be awarded.

EXPLORE MORE GREAT SCHOOLS AT
COLLEGEPOINT.INFO/SCHOOLS

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