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Three Essays on the Economics of Higher Education: How Students and Colleges Respond to Financial Aid Programs

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To the Graduate Council:

I am submitting herewith a dissertation written by Jilleah Gayle Welch entitled "Three Essays on the Economics of Higher Education: How Students and Colleges Respond to Financial Aid Programs." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Economics.

Celeste K. Carruthers, Major Professor

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Three Essays on the Economics of Higher Education: How Students and Colleges Respond to Financial Aid Programs

A Dissertation Presented for the

Doctor of Philosophy

Degree

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Jilleah Gayle Welch

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To my husband, Billy Welch, and my parents, Gayle and Gerald Boroughs

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[1] Opinions and findings reported in this dissertation do not necessarily reflect the views of the Tennessee Higher Education Commission.

Abstract

This dissertation consists of three chapters that examine the impact of financial aid programs on students' enrollment decisions, student outcomes, and colleges' financial decisions. In the first chapter, I use discontinuities in eligibility criteria for a large merit scholarship program to examine the impact of aid on community college students' outcomes both during and after college. Community colleges enroll a large share of first-time freshmen but represent a much smaller share of financial aid research. Furthermore, researchers have focused on the impact of aid on enrollment and outcomes during college, but none have yet considered the impact of aid on earnings after college. The findings suggest that reducing the cost of community college does not impact persistence, academic performance, degree completion, expected earnings, or short-term earnings after college for marginally eligible students. In the second chapter, I examine whether colleges are sensitive to state-sponsored merit aid programs. Previous research has emphasized demand-side effects such as how merit aid impacts enrollment and post-matriculation outcomes. Yet much less is known about how merit aid programs affect the supply side of higher education. Using differences-in-differences identification, I collectively analyze multiple programs and explore numerous college-level outcomes. Results suggest that colleges do not capture state-funded merit scholarships through significant increases in published tuition, and colleges increase expenditures on students in response to merit aid programs. Lastly, in the third essay, we use discontinuities in Pell grant eligibility to examine the effect of the Pell grant on college enrollment and college choice. Consistent with prior work, we find no evidence that marginal Pell eligibility increases college-going. We go on to show that just meeting the Pell cut-off has little bearing on where students choose to enroll, in terms of sector or quality dimensions. Below the threshold, where applicants are needier and the grant is more generous, students sort into colleges with modestly higher published tuition, but no other measure of college quality or college selectivity significantly diverges from the counterfactual. We conclude that students do not use the Pell grant as a tool to shop among college options in ways that systemically improve enrollment outcomes.

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Introduction

Traditionally, financial aid programs in the U.S. have intended to provide access to college to financially constrained capable students. A prime example is the Pell grant program, which originated from the Higher Education Act of 1965 and continues to be the largest source of federal need-based aid for undergraduate students. Although, there has also been substantial growth over the last two decades in state-sponsored merit aid programs, such as the Tennessee HOPE scholarship and the Georgia HOPE scholarship. A large body of literature has examined the impact of financial aid, including both need-based programs such as the Pell grant and state-sponsored merit aid programs, and studies have found that financial aid in general increases access to college. Although previous work has also found that program design, the application process, and renewal requirements can greatly affect the effectiveness of specific programs (Deming and Dynarski (2010) provide a recent review of this literature and findings). However, there still remain many unknowns as to how these programs affect students and colleges. This dissertation consists of three chapters that aim to contribute to this literature by examining the impact of financial aid on students' enrollment decisions, student outcomes, and colleges' financial decisions.

The first chapter examines the impact of the Tennessee HOPE, a broad merit-based scholarship, on community college students' outcomes both during and after college. Community colleges enroll greater than forty percent (National Center for Education Statistics)[1] of first-time freshmen but represent a much smaller share of financial aid research. Furthermore, researchers have focused on the impact of aid on enrollment and outcomes

[1]This percentage is calculated by the author from total enrollment including enrollment at public, private non-profit, and private for-profit Title IV institutions using table 1 on page 7 of the following source: Knapp, L.G., Kelly-Reid, J.E., and Ginder, S.A. (2012). *[Enrollment in Postsecondary Institutions, Fall 2010: Financial Statistics, Fiscal Year 2010; and Graduation Rates, Selected Cohorts, 2002-2007]* (NCES

during college, but none have yet considered the impact of aid on earnings after college. I use a regression discontinuity estimator which essentially compares students who are barely eligible for the HOPE scholarship to students who just fall short of eligibility, and I find there is no local impact of HOPE eligibility on persistence, academic performance, degree completion, transfer rates to four-year universities, or earnings after college for marginally eligible students. These results are important since merit aid packages are premised on the idea that broad-based aid will increase access to college, improve success in college, and ultimately support employment.

In addition to examining student responses, I also examine whether colleges are sensitive to state-sponsored merit aid programs in chapter two. Previous research has emphasized demand-side effects such as how merit aid programs impact enrollment, student choices, and post-matriculation outcomes. Yet much less is known about how state-implemented merit aid programs affect the supply side of higher education such as colleges' financial decisions. Using college financial data from the Delta Cost Project database and differences-in-differences identification, I collectively analyze how colleges respond to merit aid programs in terms of multiple outcomes including tuition and fees, expenditures on students, institutional grants, and Pell grants disbursed in addition to other revenue sources. Results suggest that colleges do not capture state-funded merit scholarships through significant increases in published tuition prices. Instead, private and public colleges seem to respond to merit aid programs with increases in expenditures on students.

In a third essay, which is co-authored with Celeste K. Carruthers, we examine how the Pell grant, the largest source for federal need-based aid, impacts college enrollment and college choice. Using enrollment decisions of four cohorts of Tennessee high school graduates and discontinuities in Pell eligibility, we find no evidence that marginal Pell eligibility increases college-going which is consistent with previous work. We also go on to show that just meeting the Pell eligibility threshold has little bearing on where students choose to enroll, in terms of sector and quality dimensions. Below the threshold, where applicants are needier and the grant is more generous, students sort into colleges with modestly higher published tuition,

2012-280). U.S. Department of Education. Washington, D.C.: National Center for Education Statistics. Retrieved on 10/31/2012 from http://nces.ed.gov/pubs2012/2012280.pdf but other measures of college quality or college selectivity do not significantly diverge from the counterfactual. We conclude that students do not use the Pell grant as a tool to shop among college options in ways that systemically improve enrollment outcomes.

Both federal and state governments expend a substantial amount of limited budgets on financial aid programs such as the HOPE scholarship and the Pell grant. Given program expenditures and the extensiveness of these programs, it is certainly important to understand both the intended and unintended consequences of these programs. In addition, my research aims to assess whether financial aid programs lead students to not only enroll but to enroll in higher quality institutions, succeed academically, and reap some of the benefits from postsecondary education such as increased earnings after college.

Chapter 1

HOPE for Community College Students: The Impact of Merit Aid on Persistence, Graduation, and Earnings

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Abstract

Community colleges play a major role in postsecondary education, yet previous research has emphasized the impact of merit aid on four-year students rather than two-year students. Furthermore, researchers have focused on the impact of merit aid on enrollment and outcomes during college, but to my knowledge, none have yet considered the impact of aid on earnings after college. This paper utilizes discontinuities in eligibility criteria for a large merit scholarship to examine the local impact of aid on student outcomes both during college and after college. The findings suggest that reducing the cost of community college does not impact persistence, academic performance, degree completion, expected earnings, or short-term earnings after college for marginally eligible students.

JEL: I22, I23, H75, J08
Keywords: Education Policy, Higher Education, Financial Aid, Community College

1.1 Introduction

Many imagine the typical college student as one attending a four-year public university, but two in five college enrollees attend two-year institutions rather than four-year institutions (National Center for Education Statistics).[1] This emphasizes how community colleges play a major role in postsecondary education, yet the majority of the previous literature that examines the impact of merit aid on student outcomes emphasizes the effect on four-year students rather than two-year students. Furthermore, researchers have focused on the impact of merit aid on enrollment and outcomes during college, but to my knowledge, none have yet considered the impact of aid on earnings after college. States have limited budgets for scholarships and funding for higher education, and knowing how the cost of community college impacts students is influential for policymakers. Reducing the cost of attending a community college may lead students to make better decisions while enrolled which in turn could result in higher earnings after college. This paper directly examines this point in question by analyzing the impact of eligibility for a large merit-based scholarship program, the Tennessee HOPE Scholarship, on post-matriculation outcomes, earnings while enrolled, expected earnings based on major choice, and short-term earnings after college specifically for community college students.[2]

Previous studies have found that there are gains from both community college attendance and completion relative to no college attendance,[3] but this is the first study to examine whether community college students who receive aid have higher earnings immediately after college compared to those students who do not receive aid. Receiving the HOPE scholarship may impact earnings after college through several channels. For example, receiving a

[1]This fraction is calculated by the author from total enrollment including enrollment at public, private non-profit, and private for-profit Title IV institutions using table 1 on page 7 of the following source: Knapp, L.G., Kelly-Reid, J.E., and Ginder, S.A. (2012). *[Enrollment in Postsecondary Institutions, Fall 2010: Financial Statistics, Fiscal Year 2010; and Graduation Rates, Selected Cohorts, 2002-2007]* (NCES 2012-280). U.S. Department of Education. Washington, D.C.: National Center for Education Statistics. Retrieved on 10/31/2012 from http://nces.ed.gov/pubs2012/2012280.pdf

[2]Tennessee's thirteen state community colleges offer certificate programs and two-year associate's degrees.

[3]Following the canonical study by Kane and Rouse (1995), a considerable amount of research has examined the labor-market returns to postsecondary education. Belfield and Bailey (2011) provide a review of this literature that specifically focuses on community college students, and in a recent study, Jepsen et al. (2014) likewise find gains from community college attendance and earning an associate's degree, diploma, and certificate. Jepsen et al. (2014) find that associate's degrees have quarterly returns of about $1,500 for men and $2,400 for women, and for both women and men, certificates have a quarterly return of about $300.

scholarship may reduce the need for students to work while enrolled, and this along with the opportunity of receiving the scholarship could increase persistence, performance in school, transfer rates to four-year colleges, and degree completion. Also, the HOPE scholarship has renewal requirements such that students must meet a minimum grade point average (GPA) in order to maintain their scholarship throughout their studies which could incentivize students to more diligently study and raise their GPA. The accumulation of these outcomes could positively affect earnings after college.

On the contrary, the HOPE scholarship might yield lower earnings after college if students choose less financially beneficial degrees or professions since they will face less debt from the cost of college. Also, if students do work less during enrollment, then students may not make as many professional connections which could lead to lower short-term earnings after college. This is a particularly relevant mechanism in community colleges, where part-time work may complement vocational study. Higher earnings not only imply private benefits such as increasing the standard of living for individuals, but higher earnings also have public benefits such as increasing government revenues.

Community colleges serve many different kinds of students, including high school graduates who are marginally interested in and/or marginally capable of succeeding in college. Also, community college students compared to four-year university students are more likely to come from economically disadvantaged families and be first generation college students (Horn and Neville, 2006; Berkner and Choy, 2008). Given these typical student characteristics in addition to community colleges being touted as cost-effective routes to obtaining job skills and earning a bachelor's degree (through transferring credits), community college freshmen may be particularly sensitive to the cost of college. Merit aid alleviates the cost of college for a targeted group of students with college-ready ACT scores,[4] and this study examines whether a generous merit aid program makes a difference in terms of those students' postsecondary success. In addition, these results will serve as context for emerging

[4]ACT's college-ready benchmarks in each of its four subject-area tests average to 21 points overall. See the following source: ACT, Inc. Retrieved on 7/31/2014 from http://www.act.org/solutions/college-career-readiness/college-readiness-benchmarks/.

community college aid programs such as the Tennessee Promise which will broaden the focus to all high school graduates going to a community college.[5]

Furthermore, while it is important to examine the effect of merit aid programs on student outcomes during college, it is equally as important to assess whether these programs impact students after college. Individuals and governments often invest in education because of the outcomes after college such as higher earnings, professional growth, increasing the stock of educated workforce, and creating a more productive and competitive economy. Yet to date, the research on the impact of large financial aid programs on student outcomes after college is limited. Therefore, this paper contributes to the literature by exclusively focusing on the impact of merit aid on community college students and including outcomes such as earnings after college in addition to outcomes during college.

The analytic sample consists of entering freshman in academic years 2005-2009 at any of Tennessee's thirteen state community colleges. Students who receive either a minimum weighted final high school GPA of 3.0 or a minimum ACT score of 21 are eligible for the HOPE scholarship. Receiving the HOPE scholarship is likely correlated with unobserved characteristics such as ability that also affect student outcomes and earnings. Therefore, I exploit a regression discontinuity design using ACT scores as the forcing variable to overcome such biases. Given that students very close to the 21 ACT cutoff are comparable both in terms of observables and unobservables, I find the local impact of HOPE eligibility by comparing the outcomes of interest for the students who marginally surpass the 21 ACT threshold to those that marginally fall short of the 21 ACT threshold. I find that students re-take the ACT multiple times in order to push themselves past the required 21 ACT points which implies that students marginally above 21 ACT points are no longer comparable to students marginally below 21 ACT points. Therefore, I utilize students' *first* ACT score rather than maximum ACT score as *first* ACT scores exhibit no signs of heaping at the threshold.

Results indicate that HOPE eligibility does not significantly impact number of semesters students enroll, cumulative hours after one year, last observed GPA, transfer rates, or

[5]The Tennessee Promise will be a last-dollar scholarship available to all Tennessee high school graduates who directly enroll in a community college or college of applied technology in Tennessee beginning with the graduating class of 2015, and the program will entail both a community service and mentoring component.

whether students obtain an associate's or a bachelor's degree. However, surpassing the 21 ACT requirement does have a weakly significant local impact on cumulative hours after two years for students whose ACT score determines HOPE eligibility. HOPE eligibility is also found to have no local impact on earnings while enrolled, expected earnings, or earnings after college.

Overall, results for the marginally HOPE-eligible student near the ACT threshold are informative for policymakers. Generous merit aid packages are premised on the idea that broad-based aid will increase access to college, improve success in college, and ultimately support employment. Moreover, individuals without college credentials require more public assistance (Vernez et al., 1999). While understanding the mechanism through which financial aid affects students' choices and outcomes are beyond the scope of this paper, students losing their HOPE scholarship after enrollment may be a contributing factor to HOPE having an insignificant local impact on students. Additionally, the HOPE scholarship considerably reduces the cost of attending a community college in terms of tuition and fees, but the value of the HOPE scholarship is relatively small compared to students' future life-time earnings which may also be a contributing factor.

To ensure robustness of the overall zero local effect of HOPE, I show that the results are consistent across various specifications and bandwidths in Section 1.6. I also show that the overall zero local effect of HOPE is homogeneous across students' income status in Section 1.7.1. Furthermore, Bruce and Carruthers (2014) find that the Tennessee HOPE scholarship induced a small but significant substitution away from two-year schools and toward four-year schools. In Section 1.7.2, I show that the overall zero effect of the HOPE scholarship for the marginally eligible student is not due to this substitution effect, and that the effect of HOPE eligibility is homogeneous across the likelihood of attending a community college versus a four-year university.

The remainder of this paper is organized as follows. The subsequent section discusses related literature, and Section 1.3 provides an overview of the HOPE scholarship program. Section 1.4 provides a description of the data and delves into the details of the empirical method used. Section 1.5 discusses the impact of HOPE eligibility on post-matriculation outcomes, earnings while enrolled, expected earnings, and earnings after college. Section

1.6 demonstrates that the results are robust across different specifications and bandwidths while Section 1.7 discusses the heterogeneity of the results across income and the potential for selection bias. The paper concludes with a discussion and a conclusion in Section 1.8.

1.2 Related Literature

In terms of economic theory, researchers often turn to a classical life cycle model to predict the impact of aid on student outcomes. According to a classical life cycle model, when the cost of college decreases, people are more likely to invest in their education both in terms of attendance and completion (Becker, 1993). This theory implies that the HOPE scholarship should positively affect student outcomes such as persistence and degree completion. However, the value of the HOPE scholarship is relatively small compared to the discounted present value of a student's lifetime earnings which implies that the HOPE scholarship may have no meaningful impacts if students can borrow against their future earnings. Thus, many researchers have turned to empirical analyses to examine the impact of aid on student choices.

There is a large body of empirical literature that analyzes the impact of both merit-based and need-based scholarships on student outcomes. Several studies have found that merit-based aid programs overall increase access to higher education (Dynarski, 2000; Nyshadham, 2013; Upton, 2013), but the gain in attendance is centered around four-year colleges (Dynarski, 2004; Cornwell et al., 2006b; Bruce and Carruthers, 2014; Cohodes and Goodman, 2014). The research on post-enrollment outcomes is mixed and does not directly focus on the community college sector. Some studies have utilized census data to assess the impact of merit aid (Dynarski, 2008; Sjoquist and Winters, 2014b; Fitzpatrick and Jones, 2012). Dynarski (2008) finds that Arkansas and Georgia's merit-based scholarships increased degree receipts. On the contrary, Sjoquist and Winters (2014b) and Fitzpatrick and Jones (2012) find that merit aid programs have no effect on degree completion. A limitation of using census data is the lack of availability of other student outcomes measuring persistence and academic achievement; therefore, several studies have opted to use administrative data to assess specific scholarship programs (Cornwell et al., 2005, 2006a; Lee, 2014; Sjoquist and Winters, 2014b,a;

Scott-Clayton, 2011; Castleman and Long, 2013).[6] For example, Scott-Clayton (2011) and Castleman and Long (2013) find that aid increases cumulative credits earned and bachelor graduation rates by examining the West Virginia Promise and the Florida Student Access Grant (FSAG), respectively. On the other hand, Castleman and Long (2013) and Sjoquist and Winters (2014b) find that aid had no impact on students earning an associate's degree by studying FSAG eligibility and the Georgia HOPE scholarship, respectively. Other studies have also considered whether merit aid programs affect major choice and have found that students are less likely to pursue majors in science, technology, engineering, and math (STEM) fields due to the introduction of merit aid programs (Cornwell et al., 2006a; Sjoquist and Winters, 2014a). While some of these studies do include community college students and four-year students in their sample, none have yet focused exclusively on community college students or examined post-college earnings. This paper extends the literature on the impact of aid on student outcomes by specifically focusing on students who start at a community college and examining additional outcomes such as GPA, transfer rates to four-year universities, earnings while enrolled, expected earnings based on major, and short-term earnings after college. Students at community colleges are very different from students at four-year universities in terms of academic preparedness, professional goals, and typical financial position, and aid may impact community college students differently than four-year university students.

This paper also complements a small but growing literature on the impact of debt on student decisions and outcomes after college. Rothstein and Rouse (2011) utilize a natural experiment where a highly selective anonymous university replaced loans with grants, and they find that debt causes graduates to choose jobs in substantially higher-salary industries. Their findings also suggest that the effect of debt on employment outcomes is more due to students' preferences as they found that debt had no significant or large impact on students' academic performance or employability (e.g. choice of major, GPA, and graduating with

[6]There is also a small but growing literature that examines how colleges respond to the introduction of state merit aid programs (Long, 2004a; Calcagno and Alfonso, 2007; Topal, 2014). Long (2004a) finds that private four-year colleges in Georgia increase tuition and decrease institutional aid in response to the Georgia HOPE scholarship. On the contrary, Topal (2014) finds no evidence that four-year colleges capture scholarship funds by examining the Tennessee HOPE scholarship, and Calcagno and Alfonso (2007) find that public community colleges actually supplement the Florida Bright Futures Program (FBFP) with an increase in institutional aid.

honors). Zhang (2013) uses the Baccalaureate and Beyond 93/97 survey data and finds that debt does not impact salary one year after receiving a bachelor's degree or the probability of working in the public or non-profit sector. This paper deviates from this literature in that it focuses on the impact of merit aid rather than debt on future salaries, does not rely on survey data for student outcomes, and examines whether receiving aid impacts post-college earnings five years after initial enrollment rather than directly after graduation as in Zhang (2013). Furthermore, the results will be more relevant to community college students compared to Rothstein and Rouse (2011) who focus on one highly selective university and Zhang (2013) who focus on students receiving a bachelor's degree.

Lastly, Bruce and Carruthers (2014) examine how the Tennessee HOPE scholarship affects enrollment on both the intensive and extensive margin, and they find that the HOPE scholarship induces a significant but small substitution away from two-year colleges and toward four-year colleges. Given that recent studies have found that attending a two-year rather than a four-year college negatively impacts earning a bachelor's degree (Long and Kurlaender, 2009; Doyle, 2009; Reynolds, 2012), the HOPE may positively impact students who choose to attend a four-year university rather than a community college because of the HOPE. On the other hand, community colleges increase access to higher education and positively impact students who otherwise would not have attended college (Rouse, 1995; Leigh and Gill, 2003; Gonzalez and Hilmer, 2006). While there is an extensive amount of aforementioned research on enrollment and four-year, post-matriculation outcomes, this is the first paper to my knowledge to solely focus on how merit aid impacts community college student outcomes including earnings after college. As states, including Tennessee, continue to emphasize and create educational policies for the community college sector, it is essential to understand how financial aid, including merit aid, impacts these students.

1.3 Background

The Tennessee Education Lottery Scholarship (TELS) program was enacted in 2004 and utilizes state lottery revenue to provide scholarships and grants to eligible students. The HOPE scholarship is the largest portion of the TELS program both in terms of number of recipients and expenditures, and the HOPE scholarship can be applied toward tuition at eligible four-year and two-year colleges.[7] To be considered for the HOPE, a student must have been a Tennessee resident for at least one year, enroll in an eligible public or private college within 16 months of high school graduation, and apply by completing the Free Application for Federal Student Aid (FAFSA). A 3.0 final weighted high school GPA or a minimum score of 21 on the ACT has been the initial HOPE requirement since 2005.[8] For the entering class of fall 2004, the requirement was a 3.0 final weighted high school GPA or a 19 (instead of a 21) minimum ACT score.

When the program began in the 2004-2005 academic year, the base HOPE scholarship award was $3,000 per year at a four-year institution and $1,500 per year at a community college. The award amounts have increased over time and currently are $6,000 per year at a four-year institution and $3,000 per year at a community college.[9] To maintain the HOPE scholarship, recipients must maintain a minimum cumulative GPA of 2.75 after 24 and 48 attempted hours and a 3.0 minimum cumulative GPA after 72 and 96 attempted hours.[10] The HOPE scholarship is terminated if a recipient earns a baccalaureate degree or five years passes from the date of the student's initial enrollment. A HOPE recipient who meets the renewal requirements can transfer between eligible institutions as long as there is no break

[7] In the 2011-2012 academic year, 11,011 students at community colleges in Tennessee received the HOPE scholarship, and 3,894 of these students received supplements in addition to the HOPE scholarship. The TELS total expenditure for these students was $23.5 million. These values were calculated by the author and are the sum of actual recipients and dollars for the traditional HOPE scholarship, HOPE scholarship with GAM supplement, and HOPE scholarship with Aspire supplement from the following source: Tennessee Lottery Scholarship Program, 2011-2012 TELS Year End Report, Retrieved on 7/17/2013 from http://www.tn.gov/collegepays/mon_college/lottery_scholars.htm.

[8] Students can also qualify for the HOPE scholarship by obtaining a minimum SAT score of 980 rather than a 21 on the ACT. However, less than two percent of the analytic sample have a record of taking the SAT, and less than one percent of the analytic sample qualified for the HOPE award by their SAT score alone. Since the administrative data contains students' maximum rather than first SAT score, I do not convert these SAT scores to ACT scores. Rather, I include these students in the sample and empirically treat them the same as the other students in the analytic sample. Moreover, this demonstrates how a fuzzy rather than sharp regression discontinuity design should be used since other factors than ACT score alone affect HOPE eligibility.

[9] The base annual HOPE scholarship awards increased from $3,000 at four-year schools and $1,500 at two-year schools in 2004 to $3,300 ($1,650) in 2005, $3,800 ($1,900) in 2006, $4,000 ($2,000) in 2007, and $6,000 ($3,000) in 2011. Prior to 2011, the annual awards were divided equally between fall and spring semester. Starting in fall 2011, HOPE recipients of fall 2009 and after could use the award for summer semesters, and the annual award was divided equally between fall, spring, and summer semesters.

[10] Prior to the 2008-2009 academic year, the HOPE renewal requirement was a 2.75 minimum cumulative GPA after 24 attempted hours and a 3.0 minimum cumulative GPA after 48, 72, and 96 attempted hours

in enrollment. For example, a community college HOPE recipient can transfer to a four-year eligible institution and still receive the HOPE scholarship as long as the student meets the HOPE renewal criteria and maintains continuous enrollment in eligible higher education institutions.

1.4 Data and Methods

The goal of this paper is to analyze the causal impact of HOPE eligibility on post-matriculation and post-college outcomes for community college students. This causal effect can be measured by the difference in the outcomes of interest in which a student receives the HOPE scholarship and in which the same student does not receive the HOPE scholarship. Obviously, both potential outcomes are not observed for a single student. Additionally, those students that receive the merit-based scholarship are likely of higher unobserved ability than students who do not receive the scholarship, and this positive selection bias means the observed difference in average outcomes could exaggerate any benefits of the scholarship. Ideally, the optimal approach to solve this selection bias problem is to randomly assign the HOPE scholarship to community college students in order to equate HOPE recipients and non-HOPE recipients in terms of both observables and non-observables. Rarely though, is there an opportunity for such a study, and HOPE eligibility is far from being randomly assigned. Students are eligible for the HOPE scholarship by obtaining either a 3.0 final weighted high school GPA or a minimum ACT score of 20.5.[11] This structure of assignment of the HOPE scholarship to students lends itself to using a regression discontinuity (RD) design. Specifically, a fuzzy RD design is exploited rather than a sharp RD design because the probability of being eligible for the HOPE increases significantly but by much less than one at the 20.5 ACT threshold since GPA along with other requirements (e.g. Tennessee residency, enrolling in an eligible college within 16 months of high school graduation, and completing the FAFSA) also affect HOPE eligibility.

[11] The actual ACT requirement for HOPE eligibility is 20.5 ACT points instead of 21 because composite test scores are computed to two decimal places.

1.4.1 Data

For the RD analysis, the data encompasses entering freshman in academic years 2005-2009 at any of Tennessee's thirteen state community colleges. Administrative data maintained by the Tennessee Higher Education Commission that spans from spring 2002 to spring 2012 are used to construct post-matriculation outcomes for these five cohorts of students such as persistence, last observed GPA, transfer rates, last observed major, and degree receipts. Since students must enroll in college within 16 months of high school graduation to be eligible for the HOPE scholarship, the analytic sample is restricted to students with an estimated age between 17 and 21 at initial enrollment.[12] These data are then merged with ACT records, FAFSA data, and unemployment insurance records. The ACT data provides students' ACT scores for each attempt. Students must complete a FAFSA to apply for the HOPE scholarship, and HOPE eligibility is available in the administrative FAFSA records for Tennessee resident applicants. The unemployment records from the state's Department of Labor and Workforce Development contain quarterly earnings for all workers with wages in Tennessee covered by unemployment insurance from the first quarter of 2002 to the second quarter in 2012.[13]

In addition, these data are also merged with expected annual wage data to assess whether HOPE leads students to sort into more or less lucrative fields. Students' last observed major is mapped to occupations or Standard Occupational Classifications (SOCs) using a crosswalk provided by the National Center for Education Statistics.[14] Wage estimates by occupations are from the Bureau of Labor Statistics' Occupational Employment Statistics (OES) survey.[15] Each major is mapped to multiple occupations or SOCs; therefore, expected annual wage for a major is the average of the annual wages for all SOC codes mapped to that major.

[12] Age as of enrollment is estimated by taking the difference between the year of initial enrollment and the year of birth.

[13] The earnings data does not include self-employment earnings, investment income, or earnings from other states.

[14] The Classification of Instruction Program (CIP) codes to SOC codes crosswalk was obtained from the following source: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics. Retrieved on 10/24/2013 from http://nces.ed.gov/pubs2002/cip2000/index.asp.

[15] The wage estimates exclude self employed persons, owners and partners in unincorporated firms, and household workers. Annual wage estimates were obtained from the following source: Bureau of Labor Statistics, Occupational Employment Statistics. Rerieved on 10/24/2013 from http://www.bls.gov/oes/tables.htm.

Furthermore, I assume students begin forming their expectations regarding wages for their chosen field of study when they enter school, so estimated wages are based on year of entry into college. The majority of the majors are accurately mapped to SOC codes with the exception of the liberal arts and sciences/liberal studies major which is only mapped to one occupation, postsecondary teachers. Since forty-five percent of the analytic sample has this general liberal arts major as their last observed major,[16] expected earnings based on level of highest degree (e.g. some college but no degree, associate's degree, or bachelor's degree) from the Current Population Survey is used for these students.[17] All earnings including earnings while enrolled, expected earnings, and earnings after college were converted to 2005 dollars using the Consumer Price Index.

1.4.2 Test for Manipulation of the Running Variable

Final high school GPA data are not included in the administrative data, so ACT scores are used as the forcing variable in the RD analysis. Even if final high school GPA was available, ACT scores would be a more suitable forcing variable. High school GPA may be more easily manipulated by students and schools at the 3.0 GPA threshold for the HOPE scholarship compared to the 20.5 ACT threshold. Yet there is still concern that ACT scores might shows signs of manipulation around the 20.5 cutoff. Pallais (2009) finds that students increased their last observed ACT scores in response to the introduction of the TELS program in 2004. Therefore, it is likely that students may improve their preparation or re-take the ACT exam in order to increase their chance of becoming HOPE eligible.

To test for manipulation of ACT scores, the discontinuity in the density function of ACT scores is estimated following McCrary (2008). As can been seen in Figure A.1, the density of students is not smooth around the ACT cutoff for the HOPE scholarship when students' maximum or best ACT score is used. The spike in the density function just above 20.5

[16]This major does not appear to be a pipeline major for transferring to four-year colleges as only 3% of students with this major transferred to a four-year school. Moreover, most students (roughly 91%) with the liberal arts major did not complete college. Eight percent of students in this major obtained an associate's degree as their highest obtained degree while less than one percent earned a bachelor's degree.

[17]Estimated wages based on education level for each cohort was obtained from the following source: United States Census Bureau, Current Population Survey. Retrieved on 10/24/2013 from http://www.census.gov/compendia/statab/cats/education/educational_attainment.html

implies that students are re-taking the exam until they surpass the necessary score of 20.5 for the HOPE scholarship. This behavioral response implies that students who marginally surpass the 20.5 cutoff are not comparable to students who marginally fail to surpass the 20.5 cutoff. For example, the students who re-take the ACT exam and push themselves above the 20.5 cutoff may be more highly motivated compared to those below the cutoff, and the RD analysis may overstate any benefits of the HOPE scholarship when best ACT scores are used as the forcing variable. When the ACT score from students' *first* attempt is used, there is no statistically significant discontinuity in ACT scores around the cutoff (see Figure A.1). Therefore, all the empirical analysis will utilize students' ACT score from their *first* attempt of taking the exam.

1.4.3 Empirical Strategy

In fuzzy RD design, the local average treatment effect of HOPE eligibility is found by dividing the discontinuity in the outcome variable of interest at the 20.5 threshold by the discontinuity in the probability of treatment at the 20.5 threshold. Hahn et al. (2001) show that this local average treatment effect is numerically equivalent to a local instrumental variables estimator where passing the 20.5 threshold is used as an instrument for HOPE eligibility. Therefore, one can find the effect of passing the 20.5 ACT threshold on HOPE eligibility by first estimating the following first stage equation of two stage least squares:

$$H_i = \alpha_0 + \alpha_1 Higher_i + f[(ACTgap_i) * Lower_i]\alpha_2 + g[(ACTgap_i) * Higher_i]\alpha_3 + \nu_i \quad (1.1)$$

The dependent variable in Equation 1.1 is a binary indicator for HOPE eligibility for student i. $Higher_i$ is a binary indicator equal to one for students who score greater than or equal to 20.5 points on the ACT while $Lower_i$ is a binary indicator equal to one for students who score less than 20.5 points. $f(\cdot)$ and $g(\cdot)$ are functions of the gap between i's ACT score and the threshold, 20.5 ACT points. Specifying separate functions, $f(\cdot)$ and $g(\cdot)$, on either side of the cutoff, allows for the slope and intercept to differ for the regression line above and below the cutoff.

Although final high school GPA is not available in the administrative data, students are asked to report their grades for several classes in the high school curriculum (e.g. English, algebra, calculus, biology, U.S. history, etc.) in a survey each time they take the ACT exam. The ACT calculates a GPA based on these survey responses, and this calculated GPA from students' *last* ACT attempt is used as a proxy for final high school GPA. Using a similar methodology as Bruce and Carruthers (2014), the sample is divided between students with a proxy high school GPA that meets the required HOPE high school GPA of 3.0 and students with either a missing proxy GPA or a proxy GPA that is less than 3.0. Figure A.2 graphically demonstrates the discontinuity in HOPE eligibility as indicated in the FAFSA data for these two groups of students. The discontinuity in HOPE eligibility is much larger and significant for students with either a missing GPA or a proxy GPA less than 3.0. Specifically, the discontinuity in HOPE eligibility is estimated using Equation 1.1 for the lower high school GPA group, and the discontinuity is 27.3 percentage points and significant at the one percent significance level. The discontinuity for students with at least a 3.0 proxy high school GPA is insignificant and only 1.6 percentage points. A significant discontinuity in the probability of receiving treatment is necessary for identification of treatment effects in a RD framework (Hahn et al., 2001). Since there is a significant and much larger discontinuity in HOPE eligibility at the 20.5 threshold for students whose high school GPA is missing or is less than the required 3.0 for the HOPE scholarship, the main analysis and baseline results are centered around the lower high school GPA students. Students with at least a 3.0 high school GPA are used as a falsification test of the identification strategy, and the outcomes for these students should not be affected by HOPE eligibility at the 20.5 ACT threshold as there is not a significant discontinuity in HOPE eligibility at the 20.5 cutoff.

The local average treatment effect of HOPE eligibility is determined by estimating the following second stage equation of two stage least squares:

$$Y_i = \pi_0 + \tau_{FRD}\hat{H}_i + f[(ACTgap_i) * Lower_i]\pi_1 + g[(ACTgap_i) * Higher_i]\pi_2 + \eta_i \qquad (1.2)$$

The dependent variables in Equation 1.2 are the outcomes of interest including measurements of persistence through community college, last observed GPA, transfer rate to a four-year college, degree receipts, earnings while enrolled, expected earnings, and earnings after college. $\hat{H}_i$ are the fitted values for H_i obtained from Equation 1.1, and the other variables in Equation 1.2 are defined as in Equation 1.1. The coefficient of interest, τ_{FRD}, represents the local average treatment effect of HOPE eligibility on the student outcomes.

I estimate Equation 1.2 for each post-matriculation outcome of interest for lower GPA students whose ACT score is within five points of the 20.5 cutoff, and $f(\cdot)$ and $g(\cdot)$ are linear functions of the gap between i's ACT score and the 20.5 ACT threshold. Bandwidth selection is an integral component in RD design as there is a tradeoff between bias and precision. Imbens and Kalyanaraman (2012) adapt alternative methods specifically for the RD setting and provide a data dependent algorithm for optimal bandwidth selection that minimizes the mean squared error $(\hat{\tau}_{SRD} - \tau_{SRD})^2$. Using this data dependent algorithm, I calculate the optimal bandwidth separately for each outcome, and a five-point bandwidth is utilized in the baseline analysis as the mean and median of the optimal bandwidths, is generally close to five points.[18] Also, linear functions for the gap between i's ACT score and the 20.5 ACT threshold are used rather than a quadratic or cubic in an effort to prevent students with ACT scores further away from the 20.5 cutoff from carrying too much weight in the specification (Lee and Lemieux, 2010). Yet in Section 1.6, I test whether the results are stable across alternative specifications including a quadratic and a cubic function for both $f(\cdot)$ and $g(\cdot)$ and additional bandwidths including the calculated optimal bandwidth for each outcome. Additionally, robust standard errors, clustered by first ACT score increment, are used in all regressions.

1.4.4 Descriptive Statistics

Table A.1 lists the descriptive statistics for HOPE eligibility, post-matriculation outcomes, and control variables. Column I lists the summary statistics for all students who were entering freshman in 2005-2009 at any of Tennessee's thirteen public community colleges and whose ACT score is within five points of the 20.5 cutoff. Of these students, 50.3 percent are eligible for the HOPE scholarship according to FAFSA records. Several post-matriculation

[18] For further discussion on optimal bandwidth selection in a regression discontinuity framework see Imbens and Kalyanaraman (2012).

outcomes are examined including: total number of semesters enrolled including summer semesters, cumulative hours after one calendar year from initial enrollment, cumulative hours after two calendar years from initial enrollment, last observed GPA, and a binary indicator for transferring to a four-year college.[19] On average, students enroll in college for 6 semesters and accumulate about 19 and 37 hours after one and two years, respectively. The mean last observed GPA is 2.53, and about 38% of all students transfer to a four-year college. Degree receipt is examined by including outcome variables for earning an associate's degree within three years for cohorts 2005-2008 and earning a bachelor's degree within five years for cohorts 2005-2006.[20] Roughly 9% earn an associate's degree within three years and 15% earn a bachelor's degree within five years. Lastly, earnings while enrolled, expected earnings, and earnings after college are considered. The majority of the sample has at least some earnings while enrolled, and the average quarterly earnings while enrolled is $1,352. The average expected annual wage based on major or level of highest degree is about $50,000. For the 2005-2006 cohorts, approximately 73% of students who exit school prior to the fifth year after initial enrollment with or without a degree have post-college earnings, and the average quarterly earnings in the fifth year is $2,645.[21]

Columns II and III of Table A.1 split the sample into the analytic sample, those students with a missing or sub-3.0 proxy high school GPA, and students with a proxy high school GPA greater than 3.0. As expected, HOPE eligibility is lower for the lower GPA students. Also, lower high school GPA students complete slightly fewer number of semesters, are less likely to transfer to a four-year college, and have a lower first and maximum ACT score than the higher GPA students. In addition, expected annual wage based on major or level of highest degree for those with a general liberal arts major is less for lower high school

[19] Cumulative hours after one and two calendar years from initial enrollment includes enrolled students, dropouts, and transfer students. For students who transfer, cumulative hours includes both hours before and after transferring.

[20] The administrative data only has degree attainment through spring 2012 which does not provide ample time for the 2009 cohort to earn an associate degree within three years, so the 2009 cohort is excluded for the analysis of this outcome variable. Also, the analytic sample is restricted to cohorts 2005-2006 for earning a bachelor's degree within five years given that degree attainment is only available through spring 2012.

[21] The sample is restricted for any earnings and average quarterly earnings in the fifth year to students who exit college prior to the fifth year with or without a degree and to cohorts 2005-2006 for which earnings data is available in the fifth year after initial enrollment. Earnings data is available through the second quarter of 2012.

GPA students. Both GPA groups are almost equally likely to have earnings while enrolled and earnings in the fifth year after initial enrollment, but there are small differences in their average quarterly earnings.

Table A.1 also includes descriptive statistics for control variables including gender, race, cohort indicators, and an indicator for lower-income. A student is specified as being lower-income if parental adjusted gross income from the FAFSA data is less than $52,000 which is the median parental adjusted gross income. If parental adjusted gross income is missing, then a student is considered lower-income if their estimated total combined income of their parents before taxes is less than $60,000 according to a categorical survey response from the ACT exam. Students with proxy GPAs that are less than 3.0 have a higher percentage of males, black, and lower-income students.

1.4.5 Test for Discontinuity in Predetermined Characteristics

RD is thought of as a local randomized quasi-experiment where students on the right and left of the cutoff are comparable, but in order for this to be true, predetermined characteristics should exhibit the same distribution above and below the HOPE threshold. To test whether this is the case, I estimate the discontinuity in the control variables at the 20.5 cutoff by replacing the dependent variable with each control variable in Equation 1.2. The results are reported in Table A.2 for students with and without HOPE qualifying proxy high school GPAs. Any significant discontinuity in a covariate would imply that changes in a post-matriculation outcome might be from a different mechanism than the HOPE scholarship. The majority of the estimates are small and insignificant for both the lower and higher GPA groups of students. Surprisingly, the percentage of black students increases at the HOPE threshold for sub-3.0 GPA students. In the Appendix, I provide a graphical analysis for the percentage of black students at the threshold. The percentage of black students does not appear to have a significant discontinuity at the 20.5 threshold, and it appears that the relationship is quadratic rather than linear for the gap between i's ACT score and the 20.5 threshold. I estimate Equation 1.2 using both a quadratic and a cubic rather than a linear specification, and the percentage of black students no longer registers as significant in either case. Therefore, it is unlikely that the discontinuity in the percentage of black students in Table A.2 is due to omitted variables that may threaten internal validity of the results. Additionally, baseline findings are robust to including controls as shown in Section 1.6.

1.5 Results

1.5.1 Regression Discontinuity Empirical Analysis

I first estimate Equation 1.1 to assess the effect of passing the 20.5 ACT threshold on HOPE eligibility, and then I estimate Equation 1.2 to find the local average treatment effect for each post-matriculation outcome. The estimations include students whose ACT score is within five points of the 20.5 cutoff, and $f(\cdot)$ and $g(\cdot)$ in Equation 1.2 are linear functions of the gap between i's ACT score and the 20.5 ACT threshold. Identification relies on the assumption that in absence of treatment, the outcome of interest would be continuous over the 20.5 threshold and HOPE treatment is causing the discontinuity in the outcome variable. The results are shown in Table A.3, and robust standard errors, clustered by first ACT score increment, are in parentheses. Column I lists the mean values for each variable for the typical student in this ACT range, and Columns II and III list the treatment on the treated estimates for low and high GPA students, respectively.

As discussed in Section 1.4.3, the first stage discontinuity is 27.3 percentage points and significant at the one percent significance level for students with a missing or sub-3.0 GPA while the discontinuity is 1.6 percentage points and insignificant for students with a GPA that exceeds 3.0. Since a significant discontinuity in HOPE eligibility at the 20.5 ACT threshold is needed for identification and this discontinuity is only significant for the lower GPA students, the main analysis focuses on students with lower high school GPAs.

Focusing on the outcomes of interest, one might expect that HOPE recipients would not be as financially constrained and thus would not need to work as much while enrolled which may enable students to more easily persist through school by attending full-time and taking more credit hours per semester. Surprisingly, I find that HOPE eligibility has no impact on persistence measures such as the number of semesters enrolled or cumulative hours after one year.[22] However, HOPE receipt does have a small but significant impact on cumulative hours after two calendar years from initial enrollment. Specifically, students who are above the 20.5 cutoff attempt 3.367 more hours over the course of two years than students below the 20.5 cutoff. This finding is significant at the ten percent level and represents a 9.1 percent increase above 37 hours, the mean cumulative hours after two years for typical students in this ACT range.

As mentioned in Section 1.3, students must obtain at least a cumulative GPA of 2.75 after 24 and 48 attempted hours and a 3.0 after 72 and 96 attempted hours to keep their HOPE scholarship. One would expect that students who are HOPE eligible would diligently study and increase their GPA to ensure the renewal of their HOPE scholarship. Nevertheless, the RD findings imply that HOPE does not impact students' last observed GPA at the 20.5 threshold. Furthermore, marginally achieving 20.5 ACT points has no impact on transferring to a four-year college, attaining an associate's degree within three years, or attaining a bachelor's degree within five years.[23]

Lastly, earnings while enrolled, expected earnings, and earnings post-college are examined. It is easy to hypothesize that receiving financial aid such as the HOPE scholarship would reduce the need for students to work during school and thereby decrease earnings during enrollment. Contrary to this hypothesis, students that are marginally HOPE eligible are no more likely or unlikely to receive earnings while enrolled, and HOPE eligibility has no impact on the average quarterly earnings while enrolled. Also, the HOPE scholarship might adversely affect earnings after college because students may choose a less financially beneficial major or degree since they will face less debt from the cost of college. However, HOPE eligibility has no impact on expected earnings based on major choice or level of highest degree for those with a general liberal arts major. Therefore, HOPE does not appear to

[22]While not included in the main results, I also examined additional measures of persistence such as maximum cumulative hours and full-time status (i.e. greater than 12 hours per semester) in the first year, and both of these measures were also insignificant even though full-time status is needed for a student to be awarded the full value of the HOPE scholarship.

[23]HOPE eligibility also did not impact whether students received a less than one year certificate, a 1-2 year certificate, an associate's degree within two years, or a bachelor's degree within four years. For brevity, these findings are not included in the main results.

impact major choice or expected earnings based on major choice.[24] Also, meeting the HOPE ACT requirement has no significant local impact on receiving any post-college earnings in the fifth year after initial enrollment nor on the average quarterly earnings during that fifth year for students who exit school prior to the fifth year either with or without a degree.[25] The findings regarding earnings after college are consistent with the other RD results in this paper in that it would be questionable if HOPE eligibility did not have a local impact on persistence, GPA, transfer rates, or degree receipts but did have an impact on post-college earnings. In addition, these findings suggest that students are not choosing different majors or occupations with different salaries because the HOPE scholarship possibly decreased their debt level.

To synthesize these findings, there is no local impact of surpassing the 20.5 ACT threshold for the HOPE scholarship on the majority of post-matriculation outcomes examined with the exception of a significant local impact on cumulative hours after two years. While this local average treatment effect is significant, the impact is modest as the point estimate is only an increase of about three hours over the course of two years. Therefore, the results indicate that HOPE eligibility does not impact student persistence or earnings for marginally HOPE eligible students, and these findings are robust to several specifications and various bandwidths as discussed and demonstrated in Section 1.6.

Column III of Table A.3 lists the local average treatment effect of surpassing the 20.5 threshold on the outcomes of interest for the high GPA students which are estimated using Equation 1.2. Since high GPA students do not have a significant discontinuity in HOPE eligibility at 20.5 ACT points, these students should not have a discontinuity in the outcomes variables at 20.5 ACT points. This is the case as all of the discontinuities in the post-matriculation outcomes are small and insignificant for the high GPA students.

[24]HOPE eligibility also did not impact whether students chose a STEM major or a major in humanities, social sciences/education, business/economics, or other majors for their first declared major, last declared major, associate major, or bachelor major. For conciseness, these results are excluded from the main results.

[25]There is also no significant impact on average, quarterly, post-college earnings during the third and sixth year after initial enrollment although these findings are excluded in effort to streamline the main results.

1.5.2 Regression Discontinuity Graphical Analysis

Graphical analyses are an integral component of RD and provide an informative illustration of the RD empirical strategy. Figure A.3 consists of a graphical analysis for each post-matriculation outcome with the forcing variable on the horizontal axis. The mean value of the outcome for students with the same ACT score is plotted for all ACT scores within five points of the 20.5 threshold. The size of the circles in the scatter plots increase proportionate to the number of students who received a specific ACT score. The plotted fitted lines on either side of the threshold are the predicted outcome values from estimating the reduced form equation,[26] and the discontinuity or reduced form intent-to-treat effect is listed for each outcome. It is clear from each graph that the number of low GPA students decreases as ACT score increases since the size of the markers decrease in ACT score. Number of semesters, GPA, transferring to a four-year college, and expected annual wage all increase as ACT score rises, but all of these outcomes vary smoothly across the 20.5 threshold. There visually looks like there might be a small discontinuity for cumulative hours after one year, associate's degree within three years, and bachelor's degree within five years, but after considering the scales on the vertical axis, these outcomes also seem to not have a significant discontinuity at the 20.5 threshold. It appears that the percentage of students with any earnings while enrolled or any post-college earnings in the fifth year is relatively constant across ACT scores. Also, there does not appear to be a significant large discontinuity in average quarterly earnings while enrolled or average, post-college, quarterly earnings in the fifth year. The only variable that depicts a possible significant discontinuity is cumulative hours after two years, and this same outcome is the only one that registered a significant discontinuity in Equation 1.2. Yet the local impact is extremely small both visually and in terms of economic magnitude.

[26]The reduced form equation is found by substituting Equation 1.1 into Equation 1.2 which yields the intent-to-treat effect of HOPE eligibility. The intent-to-treat effect is the numerator of the fuzzy regression discontinuity average treatment effect, and the intent-to-treat effect is equivalent in significance yet smaller in magnitude compared to the local average treatment effects presented in the baseline findings.

1.6 Robustness and Falsification Checks

I test the robustness of the baseline results to various specifications and bandwidths. The results of the robustness and falsification checks are in Table A.4. All reported estimates reflect the local average treatment effect of HOPE eligibility on student outcomes, and robust standard errors, clustered by first ACT score, are in parentheses. Column I lists the baseline results for low GPA students which are the same results as reported in Table A.3 and estimated using Equation 1.2. Columns II through IX also estimate the impact of HOPE on low GPA students whose HOPE receipt likely depended on the running variable, but each column differs from the baseline specification in one respect. First, controls for gender, race, lower-income, and cohort indicators are added in Column II. Secondly, optimal bandwidths are calculated separately for each outcome using the data dependent algorithm from Imbens and Kalyanaraman (2012) which minimizes the mean squared error $(\hat{\tau}_{SRD} - \tau_{SRD})^2$.[27] Then, local linear estimates from a triangle kernel are found for the calculated optimal bandwidths in Column III (Nichols, 2011).[28] Columns IV and V use a quadratic and a cubic, respectively, rather than a linear function of the gap between i's ACT score and the 20.5 threshold for $f(\cdot)$ and $g(\cdot)$ in Equation 1.2. In Column VI, the bandwidth is expanded to include students whose first ACT score is within seven points of the 20.5 threshold, and in Column VII, the bandwidth is contracted to students whose first ACT score is within two points of the 20.5 threshold. Columns VIII and IX test for discontinuities in the outcomes at 18.5 and 22.5 ACT points, respectively, which are not associated with the HOPE scholarship. Lastly, Column X contains the baseline results for high GPA students which are the same discontinuities presented in the main analysis in Table A.3.

Overall, the baseline findings indicate that there is no local impact of HOPE eligibility on persistence, academic performance as measured by GPA, degree receipt, earnings during college, expected earnings, or actual earnings after college. Columns II-VII in Table A.4 show that the nearly zero local impact of HOPE eligibility on the student outcomes is

[27]I estimate the optimal bandwidth for the numerator or the outcome variable as if it was a sharp regression discontinuity design. As noted in Imbens and Kalyanaraman (2012), this basic optimal bandwidth is very similar to the optimal bandwidth for a fuzzy regression discontinuity design.

[28]Specifically, the stata -rd- command from Nichols (2011) is utilized with plugged in optimal bandwidths that are calculated according to Imbens and Kalyanaraman (2012).

robust across different specifications and bandwidths as almost all of the estimates remain insignificant across these columns. Moreover, the significant, albeit modest, impact of HOPE on cumulative hours over the course of two years also remains significant across these columns with the exception of when the bandwidth is expanded to seven points in Column VII. Cumulative hours after one year exhibits some inconsistent significant discontinuities, but the coefficients for this outcome remain insignificant for many of the different specifications. Also, number of semesters is significant when a two-point bandwidth is used, but this coefficient is only significant at the ten percent level and remains insignificant for all of the other specifications.[29]

Columns VIII and IX test whether surpassing the threshold of 18.5 and 22.5 ACT points, respectively, impact the outcomes of interest. There may be other scholarship programs or admission criteria that require a minimum ACT score that is not associated with the HOPE scholarship, and any discontinuities at an alternative threshold would imply that a portion of the magnitude of the HOPE impact on the outcomes of interest could be attributed to these other ACT thresholds. Yet cumulative hours after two years is insignificant at both of these alternative thresholds which further confirms the robustness of the baseline findings. The other significant coefficients in Columns VIII and IX, representing 1.7% of the table, may be significant by chance.

Lastly, Column X serves as a falsification test using high GPA students. There could be other scholarships or admission criteria that also require a 20.5 ACT score. As can be seen in Column X of Table A.4 and similarly in Column III of Table A.3, none of the outcomes exhibit a significant discontinuity at 20.5 ACT points for the high GPA students. Therefore, it is unlikely that a different scholarship program that also has a minimum 21 ACT score requirement is driving the significant result for cumulative hours after two years. It should be noted though that this does not mean that HOPE eligibility does not impact higher GPA students. Higher GPA students show no local impact of surpassing the 20.5 cutoff on the outcomes of interest, but HOPE eligibility may still impact higher ability students with higher ACT scores that are not examined in the RD analysis.

[29] I also demonstrate in the Appendix that the overall zero local impact of HOPE eligibility is robust to using a one-point, three-point, four-point, and six-point bandwidth.

1.7 Extensions

1.7.1 Heterogeneity by Income

While the baseline results indicate an overall zero effect of HOPE on student persistence and earnings, the HOPE scholarship may impact students differently depending on their income status. Therefore, I examine whether the baseline findings are heterogeneous across lower-income status in Table A.5. Column I lists the baseline results for low GPA students which are estimated using Equation 1.2 and are the same as those reported in Table A.3. Columns II and III are also estimated for low GPA students using Equation 1.2, but estimations are limited to a subsample of students based on their income status. Cumulative hours after two years remains significant for lower-income students, but the point estimate remains modest for these students. HOPE eligibility increases the likelihood of having any earnings in the fifth year after enrollment for lower-income students while HOPE eligibility decreases the likelihood of having any earnings in the fifth year for higher-income students. Yet the average quarterly earnings in the fifth year remains insignificant for both lower and higher-income students. Also, average quarterly earnings while enrolled decreases for higher-income students, but all of the other student outcomes of interest remain insignificant, similar to the baseline findings, regardless of income status.[30]

1.7.2 Discussion of Potential Selection Bias

I also examine whether negative selection bias could be driving the overall zero effect of HOPE eligibility on the majority of the student outcomes of interest. Bruce and Carruthers (2014) examine how Tennessee's HOPE scholarship affects students' choice of college state and sector, and they find that the HOPE scholarship induces a significant but small substitution away from two-year colleges and towards four-year colleges. This finding suggests that there could be the possibility that students above the 20.5 ACT cutoff in the

[30] I also examine whether the baseline results are heterogeneous across gender and race in addition to income status in the Appendix. There is suggestive evidence of persistence and degree effects on non-white students, but the results are not strong enough to ascribe the discontinuities to a causal effect of HOPE rather than noise. Moreover, the results for non-white students are not robust to small changes in specification or bandwidth.

analytic sample in this paper could be negatively selected in that these students did not substitute away from two-year schools into four-year schools. Is the overall zero effect of HOPE eligibility on the majority of the student outcomes and earnings due to a sorting-induced negative selection bias at the threshold? To examine this possibility, I test whether the discontinuity in the outcomes of interest for community college students vary by the ex-ante likelihood of going to a two-year college rather than a four-year college.

The administrative data for first time freshman at two-year and four-year colleges are used to find the predicted probability of attending a two-year community college. The predicted probability of attending a two-year college rather than a four-year college is found from a linear least squares regression with control variables including gender, race, lower-income status, cohort indicators, parent adjusted gross income (AGI),[31] an indicator for parents' highest level of college attainment[32], indicators for students' ex-ante preference for attending a two versus four-year college[33] and indicators for which Tennessee high school the student attended last according to ACT records.[34]

To examine whether there are heterogeneous effects of HOPE eligibility across the probability of attending a community college, the baseline specification, Equation 1.2, is estimated for low GPA students, and students with lower predicted probabilities of attending a community college are omitted. The results for this estimation are in Table A.6. Column I repeats the baseline results which are the same as those reported in Table A.3, and students in the lower percentiles for the predicted probability of attending a community college are omitted in ascending order starting with omitting the 5^{th} percentile in Column II through the 50^{th} percentile in Column XI. There is some suggestive but weak evidence of the possibility of negative selection bias as average quarterly earnings in the fifth year after enrollment becomes positive and significant when the 20^{th} through the 35^{th} percentiles are omitted in

[31] An auxiliary regression is used to impute parent AGI when parent AGI is missing.

[32] An auxiliary probit regression is used to predict the probability that at least one parent completed college when parent education is missing

[33] Students' ex-ante preference for attending a two versus four-year college are from categorical survey responses from the ACT exam.

[34] Model fit statistics indicate that the linear least squares model with these control variables is the optimal model for predicting the probability of attending a two-year versus four-year college. Less than 1 percent of the analytic sample had a predicted probability less than zero, and 5.8% of the analytic sample had a predicted probability greater than one. Indicators for missing FAFSA data, missing college preferences, and missing a student's high school code were also included as controls.

Columns V through VIII, but the coefficients become insignificant after the 40^{th} percentile and beyond are omitted. All of the other outcome variables that are insignificant in the baseline results remain consistently insignificant across the probability of attending a two-year school even after a substantial number of students in the lower percentiles are excluded from the estimation. Also, cumulative hours after two years becomes insignificant after the 5^{th} percentile is omitted. Table A.6 suggests that HOPE eligibility does not have a local impact on student persistence, academic performance, degree receipt, or earnings, and this finding is homogeneous across the likelihood of attending a community college.[35] Therefore, the overall finding of a zero effect of HOPE on students' post-matriculation outcomes and earnings is not due to a negative selection bias that stems from negative selection out of two-year schools at the HOPE eligibility threshold.

1.8 Conclusion and Implications

There is an abundant amount of research on the impact of aid on student outcomes, but to date no study has focused exclusively on the impact of merit aid on community college students. Community colleges are a large component of higher education in the U.S., and aid may impact community college students differently than four-year university students especially as these two groups of students tend to differ in terms of academic preparedness, professional goals, and financial position. Also, to my knowledge, this is the first study to examine how financial aid impacts earnings after college. Higher earnings not only imply private benefits such as increasing the standard of living for individuals, but higher earnings also have public benefits such as increasing government revenues. Therefore, to extend the literature, I analyze the impact of eligibility for Tennessee's HOPE scholarship on post-matriculation and post-college earnings for community college students. To overcome biases associated with unobservables such as ability being correlated with both receiving the HOPE scholarship and the outcomes of interest, I exploit a regression discontinuity design and focus

[35] The estimates from interacting the predicted probability of attending a community college with $Higher_i$, the binary variable equal to one for students who surpass 20.5 ACT points, in the reduced form of Equation 1.2 also provides some suggestive but weak evidence that discontinuities in the outcomes vary by the likelihood of attending a community college.

on ACT scores in determining HOPE eligibility. The local impact of HOPE eligibility is found by comparing outcomes for the students who marginally surpass the required 21 ACT score for the HOPE scholarship to those that marginally fall short of 21 ACT points.

I find that HOPE eligibility does not significantly impact marginally eligible students who lack a HOPE-qualifying GPA in terms of persistence through community college, performance in school measured by GPA, obtaining an associate's or bachelor's degree, or the likelihood of transferring to a four-year college. However, I do find a signicant but small effect on cumulative hours after two years. HOPE eligibility is also found to have no effect on earnings while enrolled, expected earnings, or actual earnings after college.

How do these results for community college students compare to the results for four-year university students? Findings surrounding four-year university students are mixed. Castleman and Long (2013) consider the impact of the Florida Student Access Grant (FSAG) which is a need-based grant while Scott-Clayton (2011) examines the impact of the West Virginia Promise scholarship, a large merit-based scholarship that is similar to the Tennessee HOPE scholarship. Both Castleman and Long (2013) and Scott-Clayton (2011) find that aid positively affects some student outcomes including cumulative credits and earning a bachelor's degree. These findings may conflict with the results from this paper in part because aid affects community college students differently than four-year university students. Secondly, need-based aid, such as the FSAG, targets low-income students, and aid may induce different behavioral responses among these students. Lastly, the findings may be divergent because the requirements for receiving aid are heterogeneous. For example, the West Virginia Promise requires a 3.0 high school GPA *and* a 21 ACT score while the Tennessee HOPE requires a 3.0 high school GPA *or* a 21 ACT score. This literature as a whole suggests that the impact of aid on post-matriculation outcomes for students may largely depend on the details in the structure of assignment and renewal criteria for such programs which should be considered when designing scholarship and grant programs. Sjoquist and Winters (2014b) and Fitzpatrick and Jones (2012) collectively analyze the impact of large merit-based aid programs for nine and fifteen states, respectively, and they find that merit-aid programs have no effect on degree completion. While they focus almost entirely on degree completion to measure academic persistence and achievement, my results do align with these results as eligibility for the HOPE scholarship had no highly significant effects on student persistence or earnings.

In addition to program design details, what other factors may contribute to the HOPE having an insignificant local impact on community college students in Tennessee? Over the 2005 to 2009 academic years, the average HOPE award at a two-year college was about $1800 per year while the average tuition and fees at Tennessee's community colleges over the same time period was $2,511 (in 2005 dollars). Therefore, the HOPE scholarship notably reduces the cost of attending a community college as the scholarship accounts for about 72% of tuition and fees at a community college and around 14% of the cost of attendance (COA) including tuition and fees, books and supplies, and off campus room and board (not living with family).[36] However, some students may be receiving other scholarships in addition to the HOPE that already cover a significant portion of tuition and fees. For example, roughly two in five of the students who received the HOPE in the analytic sample also received the Pell grant which had an average annual award of $2,704 (in 2005 dollars) over the time period studied.[37] Students who are eligible for both the Pell and the HOPE scholarship receive the full amount of the HOPE scholarship as the Pell and HOPE scholarship together do not exceed students' allowable aid or COA.[38] For example, receiving the HOPE and the average Pell award is only a little over a third of the COA at a community college in Tennessee, and the HOPE's limited impact might be due to its small share of the COA. Also, the average value of the HOPE scholarship equates to about $35 per week or around five hours of work per week earning the federal minimum wage rate (assuming 52 weeks per year). Thus, the HOPE may not necessarily provide a substantial exchange for labor hours over the course of a year which may also explain the program's minimal impacts on community college students.

[36] Averages and percentages are calculated by the author using tuition and fees and total cost of attendance estimates from IPEDS for Tennessee's thirteen state community colleges for the 2005-2006 through 2009-2010 academic year.

[37] Average Pell grant award was calculated by the author using average Pell grants for 2005-2006 through 2009-2010 academic years which are provided in the following source: U.S. Department of Education, Office of Postsecondary Education, 2011-2012 Federal Pell Grant Program End-of-Year Report. Retrieved on 5/13/2014 from http://www2.ed.gov/finaid/prof/resources/data/pell-2011-12/pell-eoy-2011-12.pdf

[38] For details and rules regarding HOPE scholarship award amounts and eligibility, see the following source: Tennessee Student Assistance Corporation, Tennessee Education Lottery Scholarship Program, Rules of Tennessee Student Assistance Corporation Chapter 1640-01-19. Retrieved on 7/31/2014 from http://www.tennessee.gov/sos/rules/1640/1640-01-19.20100531.pdf.

The HOPE scholarship also may not positively impact students after matriculation because students lose their HOPE scholarship by failing to meet renewal requirements. Carruthers and Özek (2014) find that over a third of Tennessee HOPE recipients at two-year and four-year colleges lost their scholarship within three years by failing to meet renewal criteria for cumulative GPA, and that losing the HOPE has a small but negative impact on credits and decreases the likelihood of continuous enrollment. In the analytic sample in this paper, less than two percent of HOPE recipients in the 2005 cohort lost their HOPE scholarship within one year, but around 30% of HOPE recipients lost their scholarship within two years.[39] These percentages likely understate the percentage of students who lost their HOPE scholarship because losing HOPE records are missing for students who drop out of college immediately after losing the HOPE. While understanding the mechanisms through which financial aid affects students' choices and outcomes are beyond the scope of this paper, losing the HOPE scholarship may help explain why the HOPE scholarship does not have lasting local impacts on community college students in Tennessee.

Lastly, it is important to acknowledge that the findings of this paper come with a caveat. Regression discontinuity is known for having strong internal validity, but regression discontinuity lacks external validity in that the estimated effects of HOPE eligibility are local around the discontinuity. HOPE eligibility does not significantly impact marginal students near the 21 ACT threshold with non-qualifying HOPE GPAs, but HOPE eligibility may impact students who have substantially higher ACT scores. Further research is needed to determine if HOPE eligibility significantly impacts students of higher ability in contrast to marginally eligible students, and future research in addition to the existing research will continue to be influential for policymakers in designing and implementing aid programs for both four-year university students and community college students.

[39]Data on students losing the HOPE scholarship are not available after 2008, so losing HOPE statistics are only calculated for the 2005 cohort.

Chapter 2

The Incidence of Financial Aid: How Colleges Respond to Merit Scholarship Programs

Abstract

I examine how colleges respond to the introduction of broad merit aid programs. Previous research has emphasized the impact of merit aid on enrollment, student choices, and post-matriculation outcomes. Yet much less is known about how state-implemented merit aid programs, especially those funded through lottery revenues or other semi-external revenue sources, affect colleges' financial decisions. I use college financial data from the Delta Cost Project database to assess how colleges respond to merit aid programs in terms of tuition and fees, expenditures on students, institutional grants, and Pell grants disbursed in addition to other revenue sources. Results suggest that colleges do not capture state-funded merit scholarships through significant increases in published tuition prices. Instead, public and private colleges react to state-funded merit scholarships with increases in expenditures on students.

JEL: I22, I28, H75

Keywords: Education Policy, Higher Education, Financial Aid

2.1 Introduction

Merit aid programs have grown substantially over the last two decades. For example, non-need-based aid has increased from 11% of total undergraduate state grant aid in 1990-1991 to 26% in 2011-2012 (The College Board, 2013). Over a dozen states have implemented broad merit aid programs, led by the Georgia HOPE scholarship, and many researchers have examined the impact of merit aid programs on student outcomes such as college enrollment, choice of college sector, persistence, degree completion, and migration (Dynarski, 2000, 2004; Cornwell et al., 2005, 2006b; Dynarski, 2008; Scott-Clayton, 2011; Nyshadham, 2013; Cohodes and Goodman, 2014; Bruce and Carruthers, 2014; Sjoquist and Winters, 2014b,a; Fitzpatrick and Jones, 2012). However, much less is known about how colleges respond to state-implemented merit aid programs. In order to fully assess the incidence of broad merit aid programs, it is necessary to know whether the supply side of higher education is sensitive to these programs. Do colleges raise tuition after merit aid programs are introduced? If colleges do capture a portion of scholarship funds, are colleges using captured funds for initiatives or activities that still benefit students? Do colleges supplement merit aid programs with increases in expenditures on students? This paper aims to shed light on some of these unintended consequences of merit scholarship programs which in turn could impact the net reduction in cost or benefits for scholarship recipients. More specifically, this paper collectively analyzes how broad merit aid programs affect colleges' tuition and fees, expenditures on students, institutional grants, and Pell grants disbursed in addition to other revenue sources.

The idea that colleges increase tuition in response to increases in financial aid is referred to as the Bennett Hypothesis, named after former Secretary of Education, William Bennett, who suggested this notion in a 1987 *New York Times* opinion article (Bennett, 1987). The majority of the literature that has examined the Bennett Hypothesis or how aid impacts the supply side of higher education revolves around federal programs. Some studies have found support for the Bennett Hypothesis as increases in federal aid result in increases in colleges' tuition prices (Cellini and Goldin, 2014; Frederick et al., 2012). However, conclusions have been mixed regarding the Pell grant, a federal need-based grant for low-income students

(McPherson and Schapiro, 1991; Li, 1999; Rizzo and Ehrenberg, 2004). Although recent studies reveal that private four-year colleges increase tuition in response to Pell generosity (Singell and Stone, 2007), and colleges capture 12 percent of all Pell Grant aid by reducing institutional aid (Turner, 2014). Studies that specifically examine federal aid programs in the form of tax credits, such as the HOPE Tax Credit (HTC) and the Lifetime Learning Tax Credit (LLTC), find limited evidence of effects on tuition, but find that tax-based aid almost completely crowds out institutional aid at public and private colleges (Long, 2004c; Turner, 2012).

Broad merit aid programs include a large portion of undergraduate students as eligibility requires a moderately high standardized test score and/or high school GPA. In addition, merit scholarships often cover a significant portion of tuition at public four-year or two-year colleges, and states usually allow students to use scholarships at either public or private colleges within the state. Given the breadth and transparency of these highly publicized programs, colleges may have larger reactions to them compared to more complex federal programs.

A few studies have previously examined the supply-side effects of broad merit aid programs (Calcagno and Alfonso, 2007; Long, 2004a; Jensen, 2011). Calcagno and Alfonso (2007) focus on community colleges and find that public community colleges in Florida do not alter tuition and actually supplement the Florida Bright Futures Program (FBFP) with increases in institutional aid. Long (2004a) finds that some private colleges in Georgia capture as much as 30 percent of the Georgia HOPE scholarship by increasing tuition and decreasing institutional aid. Lastly, Jensen (2011) separately analyzes large merit aid programs in five states and finds that public colleges raise tuition and fees after merit aid programs are introduced. This paper extends this literature in a few ways. First, I collectively analyze ten broad merit-aid programs, and findings will help to evaluate whether previous results from specific states are generalizable. With the exception of Long (2004a), the relevant literature has primarily focused on tuition prices and institutional aid. This paper also examines tuition and institutional aid, but I also extend the literature by considering numerous other outcomes such as instructional expenditures, student services expenditures, Pell grants disbursed, and other revenue sources.

Additionally, some studies do not directly focus on the introduction of broad merit aid programs but are certainly related to this literature. Curs and Dar (2010a) examine how states' expenditures on need and merit-based aid affect tuition prices and average institutional aid (and thus net price). Using data from 2002-2007 and instrumental variables, Curs and Dar (2010a) find that colleges decrease published tuition prices due to increases in state merit aid.[1] I depart from this work in a few ways. First, I expand the years studied to include 1986-2009 which covers the time period for which states introduced broad merit aid programs.[2] Secondly, I use differences-in-differences identification, and several non-price college outcomes are explored. While some aforementioned studies suggest that colleges may capture some portion of financial aid through either increases in tuition or decreases in institutional aid, it is unknown whether colleges alter expenditures on students which in turn may affect education quality.[3]

Lastly, Jones (2015) examines the impact of lotteries earmarked for education, either K-12 or higher education, on state governments' finances and charitable contributions and finds that education lotteries crowd out education contributions. The majority of the broad merit aid programs are funded through states' lotteries, and one outcome of interest explored here is colleges' revenue from private gifts, grants, and contracts. However, this paper is more so focused on colleges' responses to merit aid rather than donors' responses, but the literature will benefit from comparing results when college-level versus donor-level data as in Jones (2015) is used.

The analytic sample consists of a 1986-2009 panel of public and private not-for-profit four-year colleges in the U.S. This data spans the time period for which multiple states implemented broad merit aid programs. For example, Georgia was one of the first states to introduce a merit aid program in 1993, and now, over one fourth of the states in the U.S. have implemented merit aid programs. While some states have implemented small

[1] In a closely related paper, Curs and Dar (2010b) use a similar data set and methodology to examine supply-side effects of total state-sponsored financial aid (including both merit and need-based aid) and find that increases in state financial aid lead public colleges to decrease published tuition prices.

[2] For example, Georgia implemented the Georgia HOPE scholarship in 1993, and Florida implemented the Florida Bright Futures Scholarship in 1997. Out of the ten broad merit aid programs examined, Tennessee was one of the last states to implement a program in 2004.

[3] Griffith (2011) also examines merit aid but analyzes how private four-year colleges fund or react to their own aid programs rather than how colleges interact and respond to large state aid programs.

and very selective programs which only include, for example, top ranked students from each high school within a state, the focus of this paper is on broad merit aid programs. States and colleges are more likely to respond to these programs since there are a large number of recipients. Also, almost all broad merit aid programs are funded through semi-external revenue sources such as lottery revenues or tobacco settlement funds which also may induce larger responses especially regarding states' financial decisions.

Merit aid programs in general aim to increase academic preparedness among high school students, increase access to college, incentivize high ability students to attend college in-state, and promote academic success and completion. Yet unintended consequences including colleges' responses to these programs may in turn affect the net benefit of merit scholarships for recipients. However, results indicate that public and private colleges do not seem to be capturing HOPE-like scholarships through significant increases in tuition, and public and private colleges rather supplement merit aid programs with some increases in expenditures on students. Findings are informative for policymakers since students seem to be receiving the full value of their merit scholarships as intended by policymakers.

The remainder of this paper is organized as follows. The following section provides an overview of broad merit aid programs. Section 2.3 describes the details of the data and empirical method used, and Section 2.4 discusses the impact of broad merit aid programs on colleges' revenues and expenditures. An extension is provided in Section 2.5 that uses an alternative method for measuring the broadness of merit aid programs. Lastly, the paper concludes with a discussion and conclusion in Section 2.6, and in the Appendix, I examine whether results are sensitive to omitting college-specific time trends and check for differences in pre-treatment trends across merit and non-merit states.

2.2 Background

Table B.1 lists the characteristics of the merit aid programs which are funded through either lottery revenues or tobacco settlement funds. Generally, to apply for a merit scholarship, students must be a resident of the specified state and complete the Free Application for Federal Student Aid (FAFSA) or a separate financial aid application for the state. Eligibility requirements vary depending on the program, but as shown in Column IV of Table B.1, initial eligibility often requires a particular ACT/SAT score (e.g. ACT composite score of 21) and/or a moderate final weighted high school GPA (e.g. 3.0). Most programs enable students to use their merit aid scholarship at a two-year or four-year, public or private not-for-profit, college. The award amounts differ by state but often cover a significant portion of tuition and fees.[4] Typically, students can renew their scholarship by meeting specified minimum college GPA requirements, and after meeting these renewal requirements, students can maintain their scholarship until they receive a baccalaureate degree or for four to seven years, depending on the program, after the student's initial enrollment.

2.3 Data and Methods

Institutional financial data is available from the 1986-1987 to the 2009-2010 academic year which covers the time period for which states implemented broad merit aid programs. Therefore, a panel data model which mimics differences-in-differences identification is used to assess how colleges respond to the introduction of merit aid programs with all non-merit states in the U.S. serving as the control group. All analyses examine the impact of merit aid on public and private colleges separately given that responses between the two different types of schools are likely to differ.

2.3.1 Data

The data encompasses colleges that are consistently undergraduate degree granting, Title IV, four-year public or private not-for-profit colleges within the U.S.[5] Institutional characteristics stem from the National Center for Education Statistics' (NCES) Integrated Postsecondary Education Data System (IPEDS). These data are then merged with institutional financial data from the Delta Cost Project IPEDS database.[6] The Delta Cost Project database

[4]Several states also have supplemental awards to the base award amount for either high ability students with exceptional ACT scores or low-income students.

[5]The sample excludes colleges in Guam, Puerto Rico, and the Virgin Islands.

[6]The Delta Cost Project Database was originally created in 2007 by The Delta Cost Project, an independent, non-profit organization, and NCES began administering the database in 2012.

originates from IPEDS, but in the Delta Cost Project database, adjustments have been made to institutional financial data to account for various accounting and reporting changes over time.[7] The outcomes of interest come from this consistent longitudinal data set of institutional revenue and expenditure data. This results in a total sample of 1,508 four-year colleges of which 477 are public and 1,031 are private not-for-profit.[8]

2.3.2 Empirical Strategy

The impact of broad merit aid programs on colleges' financial decisions is found by estimating the following fixed effect model.

$$log(Y_{ist}) = \pi_0 + \pi_1 Merit_{st} + W_{st} + X_{it} + \delta_t + \delta_i + f(t)\delta_i + \epsilon_{ist} \tag{2.1}$$

The dependent variable in Equation 2.1 is the log of the outcomes of interest such as tuition and fees, expenditures on students, institutional grants, state appropriations, and revenues from private donors for college i in state s in year t. $Merit_{st}$ is a binary indicator equal to one for the treatment states or states that introduced broad merit aid programs after implementation and zero otherwise.[9] W_{st} contains controls for variation in economic conditions across states including annual unemployment rates[10], annual personal income per capita[11], and annual real GDP per capita.[12] To account for college demand, state controls also include the percent of the state population age 18 to 24 who have a high school

[7]One example of a modification made in the Delta Cost Project Database that is pertinent to this paper is adjustments made to instructional expenditures. For example, to create consistent data over time, operations and maintenance and interest amounts attributed to instruction are deducted from instructional expenditures in years in which institutions reported according to the standards set by the Financial Accounting Standards Board (FASB). Please see the Delta Cost Project Database documentation files for more information which can be obtained from the following source: U.S. Department of Education, National Center for Education Statistics, Retrieved on 3/5/2014 from http://nces.ed.gov/ipeds/deltacostproject/

[8]Private for-profit colleges are excluded from the sample since data is more often missing for these institutions, and each states' merit aid program typically only allow students to use their scholarship at public or private not-for-profit colleges.

[9]In Section 2.5, I use a more continuous rather than binary measure of merit aid by using using actual program expenditures per FTE enrollment for $Merit_{st}$.

[10]Unemployment rates were obtained from the U.S. Department of Labor, Bureau of Labor Statistics.

[11]Personal income per capita were acquired from the U.S. Department of Commerce, Bureau of Economic Analysis.

[12]Real GDP per capita is missing for all states in 1986. Linear interpolation is used to fill in this missing data. Real GDP per capita was collected from the U.S. Department of Commerce, Bureau of Economic Analysis.

degree which stem from a combination of the American Community Survey (ACS), decennial Census, and Current Population Survey (CPS).[13,14] X_{it} contain institutional control variables including the percent of undergraduates who are women[15] and the percent undergraduates who are non-white.[16] Institutional controls also include the log of the full-time equivalent (FTE) enrollment at an institution.[17] Lastly, $f(t)\delta_i$ accounts for linear, college-specific time trends, and δ_i and δ_t are institutional fixed effects and time fixed effects, respectively. All of the financial outcomes of interest and controls were converted to 2010 dollars using the Consumer Price Index. Robust standard errors are clustered by institution.

The coefficient of interest, π_1, describes the average treatment effect of merit aid programs on states' and colleges' financial decisions. The main identification threat is unobserved heterogenous trends. More specifically, a potential concern is there might be broad changes in education policy or states' sentiment toward higher education that are coincident with the introduction of merit aid programs. Or in other words, there may be broad education initiatives such that merit aid states implement merit aid programs in tandem with adjusting state educational expenditures such as state appropriations. Furthermore, such an education initiative may be in response to a decrease in higher education funding in merit aid states pre-treatment. I address this policy endogeneity concern in a few ways. First, a policy endogeneity concern is greatest at the point of program implementation rather than years

[13]The 2005-2009 estimates stem from the ACS. The 1990 and 2000 Census estimates are from the IPUMS-USA database, and estimates from the IPUMS-CPS data set are used for years 1992-1999 and 2001-2004. Linear interpolation is used to fill in missing data for years 1986-1989 and 1991. ACS educational attainment data is from the U.S. Department of Commerce, United States Census Bureau, and the American Fact Finder. Census and CPS data were obtain from the Integrated Public Use Microdata Series (IPUMS) (King et al., 2010; Ruggles et al., 2010).

[14]Since previous economic conditions likely influence colleges' current financial decisions, the lagged value of state controls are mapped to the current academic year. For example, the unemployment rates for 2009 are mapped to the academic year 2009-2010.

[15]Undergraduate enrollment by gender is missing for all institutions for the academic year 1990-1991 in addition to less than one percent of missing observations. Linear interpolation is used to fill in this missing data; however, five colleges are omitted from the analytic sample as they are either missing enrollment data by gender for either all years or for several years such that linear interpolation can either not be used or does not produce reasonable extrapolations.

[16]Enrollment by race is missing for academic years 1987-1988, 1989-1990, 1990-1991, 1995-1996, and 1996-1997 for all colleges and for an additional less than one percent of observations. Linear interpolation is used to fill in these gaps in the data, but five institutions are omitted from the analytic sample as linear interpolations do not perform well since several years of enrollment data are missing.

[17]FTE enrollment is missing for less than one percent of the sample, and linear interpolation is used to fill in missing data.

following implementation, and the concern is presumably for outcomes that are determined by state governments who also administer state-funded merit aid programs. Therefore, I focus more so on college level decisions that are made independently at the college level, but I include state level outcomes such as state appropriations and tuition at public colleges (which is sometimes made through oversight or negotiation with state legislatures) in order to compare results to the previous literature that has examined these variables (Long, 2004a; Calcagno and Alfonso, 2007; Jensen, 2011; Curs and Dar, 2010a). Also, the long panel allows me to control for college-specific time trends. In the Appendix, I demonstrate that results are sensitive to including these college-specific time trends and for some outcomes, there does appear to be non-parallel pre-treatment trends for treatment and control states. Therefore, college-specific time trends are included to account for linear trends at the institution level or higher, and time fixed effects control for general time trends.

2.3.3 Descriptive Statistics

Summary statistics are presented separately for merit and non-merit states in Tables B.2 through B.5. Table B.2 and Table B.3 demonstrate that merit and non-merit states are comparable in terms of state and institutional control variables, respectively. For the last year in the panel data set, non-merit states have slightly higher average personal income per capita and real GDP per capita than states with merit aid programs, but the average unemployment rate and percent of the population with a high school degree are similar across the states. Also, average student composition and FTE enrollment are comparable across treatment and control states.

Table B.4 and B.5 present summary statistics for the outcomes of interest per FTE enrollment for public and private not-for-profit colleges, respectively. Several revenue outcomes are examined for public colleges including: in-state tuition, out-of-state tuition, revenue from private donors,[18] state grants and contracts,[19] and state appropriations. Over the time period studied, the mean in-state and out-of-state tuition and fees (or sticker price)

[18]Includes gifts, grants, and contracts that are directly related to instruction, research, public service, or other institutional purposes.

[19]Includes both operating and non-operating grants and contracts.

at public colleges is roughly $4,000 and $10,000, respectively, for both merit and non-merit states. The average revenue from private gifts, grants, and contracts per FTE is about $1,000, and state appropriations per FTE are about $9,000 for all states. It should be noted that state grants and contracts include revenues from states for specific programs, and institutions should report merit scholarship funds such as the HOPE scholarship when reporting state grants and contracts. Including this outcome helps confirm that colleges in merit aid states are consistently reporting revenues for merit aid programs. Several college outcomes related to students are also examined including: instructional expenditures, student services expenditures,[20] the gross amount of Pell grants disbursed by an institution, institutional grants, and the number of full-time faculty per 100 FTE students.[21] For all states, the mean instructional expenditures and student services expenditures per FTE are approximately $8,000 and $1,000 respectively. The mean gross amount of Pell grants per FTE is about $900, and the average institutional grant per FTE is roughly $700. Lastly, the average number of full-time faculty per 100 FTE students is approximately six.

Similar outcomes are also examined for private colleges including: tuition, revenue from private donors, instructional expenditures, student services expenditures, the gross amount of Pell grants disbursed by an institution, institutional grants, and the number of full-time faculty per 100 FTE students. The mean tuition or sticker price at private colleges is higher in non-merit versus merit states. The average revenue from private donors per FTE is roughly $5,000 and significantly higher for private versus public colleges. Likewise, the mean institutional grant per FTE is about $4,000 and significantly higher for private versus public colleges, and the average student services expenditures per FTE is approximately $2,500 and higher for private versus public colleges.

2.4 Results

I estimate Equation 2.1 for all outcomes of interest separately for public and private colleges where the control group consists of colleges in non-merit states and colleges in merit states

[20] Includes expenses for activities such as admissions, registrar activities, student activities, cultural events, intramural athletics, student organizations, student newspapers, etc.

[21] Faculty includes faculty for instruction, research, and public service.

pre-treatment. To prevent data composition from driving results, only institutions with less than or equal to two years of missing data for the dependent variable are included in each estimation.[22] The results for public colleges are shown in Table B.6, and robust standard errors, clustered by institution, are in parentheses. Column I lists the mean value of each outcome for public colleges in merit states the year before program implementation, and values are in millions except for the outcomes tuition and faculty per 100 FTE enrollment. Column II lists the estimates of the effect of merit aid programs on colleges' revenues and expenditures.

State merit aid programs arguably provide funds for a lot of students from middle and upper income families. One might expect for colleges to raise tuition in response to state-funded merit scholarships since receiving financial aid raises these families' ability to pay. However, the implementation of merit aid programs does not significantly affect in-state tuition, and merit aid programs induce only slightly significant and a modest increase in out-of-state tuition. Specifically, public colleges in merit states have a 3.4% increase in out-of-state tuition post-merit relative to what prices would have been in absence of merit aid programs. However, this minimal impact on tuition may not be altogether surprising since tuition at public colleges are often set through oversight or negotiation with state legislatures. State legislatures may prohibit significant tuition hikes at public colleges to ensure that the full value of HOPE-like scholarships are passed on to students as intended by policymakers. However, it should be noted that published tuition prices or sticker prices are often not the price that students pay, and it remains to be seen whether merit aid programs affect the net price that students actually pay.

One may also expect an increase in state-funded scholarships to crowd out other revenue sources such as private contributions to colleges especially since broad merit aid programs are generally very transparent, simple, and highly publicized programs. Yet the introduction of merit aid programs does not significantly affect revenue from private gifts, grants, and contracts. Colleges in merit aid states do, nonetheless, experience significant increases in state appropriations and state grants and contracts. Merit state colleges experience a

[22]Included observations vary depending on the outcome of interest, but on average, roughly 90% of the sample is included in estimations for both public and private not-for-profit colleges.

relative 4.8% increase in state appropriations and a 90.6% increase in state grants and contracts. The enormous increase in state grants and contracts is expected and reassuring since colleges in merit aid states should report merit aid dollars in this category on annual IPEDS surveys. However, finding that merit aid programs actually crowd in rather than crowd out state appropriations is surprising. Many might expect states to instead decrease state appropriations to public colleges in the years after merit aid programs are introduced especially since most of these programs are funded through semi-private sources such as lottery revenues or tobacco settlement funds instead of general revenues. Yet states create merit aid programs in order to improve educational outcomes in their state, and states' commitment to these goals may explain increases in state appropriations.[23]

Expenditures on students are also examined. While merit aid programs appear to not affect instructional expenditures, colleges in merit aid states increase student services expenditures by 3.9% and increase the number of full-time faculty per 100 FTE enrollment by 3.9%. Many merit aid programs aim to incentivize high ability students to attend college in-state, and colleges may be supplementing these merit aid programs with increases in spending on various student-oriented activities and initiatives in order to further attract students to their university especially given the increasing competition among both colleges within and outside of a state. Previous work has found that students do value amenities such as student activities, cultural events, intramural athletics, residence halls, food services, college unions, etc., and colleges respond to market demand for such non-price amenities (Jacob et al., 2013).

Lastly, for public colleges, I examine whether increases in merit aid crowd out institutional grants or affect the gross amount of Pell grants at an institution. Colleges could adjust institutional grants in several ways in response to merit aid programs. Colleges may view state merit scholarships as a substitute for institutional grants and reduce institutional grants. On the other hand, colleges may not change the overall spending level of institutional grants, but redirect institutional grants from typical merit scholarship recipients to more needy students, for example. Unfortunately, this kind of substitution can not be examined

[23]This finding is consistent with that of Long (2004a) who finds that public colleges in Georgia with a greater percentage of HOPE recipients experience an increase in state appropriations.

here since only the aggregate level of institutional grants is available, but results indicate that colleges do not make any significant changes to institutional grants, at least at the aggregate level, in response to merit aid programs. Yet the gross amount of Pell grants disbursed by an institution increases by 5.1% for merit state colleges after program implementation. This increase in the gross amount of Pell grants may arise through a couple different channels. The introduction of merit aid programs may increase the enrollment of Pell eligible students or it may increase the number of current students (who would have enrolled regardless of merit aid programs) who are Pell eligible. While the analysis here can not distinguish between these two mechanisms, several programs require students to apply for merit scholarships by completing the FAFSA which may result in more students being Pell eligible post program implementation.

Turning to private colleges, results for private colleges are presented in Table B.7. Similar to Table B.6, Column I lists the mean value of each outcome for private colleges in merit states the year before program implementation, and Column II lists the estimates of the effect of merit aid programs on private colleges' financial decisions. Given that private colleges set tuition prices independently, private colleges may be more likely than public colleges to increase tuition in response to merit aid programs. Results, however, indicate that private colleges do not significantly alter tuition prices. Furthermore, similar to public colleges, private contributions to private colleges are not affected by the implementation of merit aid programs, and private colleges in merit aid states increase expenditures on students. However, private colleges increase instructional expenditures rather than student services expenditures contrary to findings for public colleges. Private colleges increase instructional expenditures by 3.9%, but this finding is only significant at the ten percent level. Also, similar to public colleges, private colleges experience an increase in the gross amount of Pell grants disbursed to students. Private colleges in merit states experience a 4.5% increase in the gross amount of Pell grants compared to the level that would have transpired in absence of merit aid programs. The underlying reasons for this increase are unknown, but several programs requiring students to file a FAFSA for eligibility likely plays a role.

Overall, public and private colleges do not seem to be capturing HOPE-like scholarship funds through significant increases in tuition prices. Instead, after merit aid programs are introduced, public and private colleges slightly increase expenditures on students and experience increases in the gross amount of Pell grants.

2.5 Extensions

In this section, I extend the baseline analysis by considering other possible ways to measure merit aid programs. The baseline specification, Equation 2.1, uses a binary indicator, $Merit_{st}$, that is equal to one for states that introduced broad merit aid programs after implementation and zero otherwise. This is precisely how previous studies that examine both demand and supply-side effects have specified the introduction of merit aid programs in similar panel data models (Dynarski, 2004; Fitzpatrick and Jones, 2012; Sjoquist and Winters, 2014b,a). Yet, in this extension, I use actual program expenditures per FTE enrollment rather than an indicator for $Merit_{st}$.[24] Using expenditures per FTE allows for the specification to account for not only switching a merit aid program from off to on, but it accounts for the overall broadness and size of the program. Additionally, results can be interpreted in terms of dollar values. Unfortunately, program expenditures are only available going back to the 2002-2003 academic year; therefore, *eventual* size of each merit aid program is used. Specifically, each states' expenditures on their merit aid program in the 2006-2007 year is divided by state undergraduate FTE enrollment in the same year. The FTE enrollment includes students typically eligible for HOPE-like scholarships and includes undergraduate students at four-year and two-year, public and private not-for-profit colleges within a state. For scaling purposes, expenditures per FTE for each state are divided by 100 to align better with the dependent variables which are the log of expenditure and revenue outcomes.

Equation 2.1 is estimated with $Merit_{st}$ now equaling the 2006-2007 expenditures per FTE for states that introduced merit aid programs after implementation and zero otherwise,

[24]Expenditures for state, merit aid programs were collected from The National Association of State Student Grant and Aid Programs (NASSGAP) Program Quick Finder. Expenditure and recipient data for specific scholarship programs including states' merit aid programs is only recently available, and expenditure and recipient details can be obtained going back to the 2002-2003 academic year. The NASSGAP Program Quick Finder can be found at the following website: http://www.nassgap.org/survey/program_finder/program_finder.asp.

and results for public colleges are presented in Table B.8. Column I lists the baseline results which are the same as those found in Column II of Table B.6, and Column II lists estimates of the effect of merit aid on colleges' financial decisions using eventual program size or expenditures per undergraduate FTE (divided by 100). Out-of-state tuition which was only weakly significant in the baseline specification no longer registers as significant when using expenditures per FTE. Merit aid now appears to crowd out some revenue from private donors, but the estimate is only significant at the ten percent level. Merit aid continues to increase expenditures on students including both instructional and student services expenditures. While these estimates increase in terms of significance as they are now significant at the one percent level, point estimates continue to be relatively modest. For every \$100 dollar increase in merit aid per FTE, instructional expenditures and student services expenditures increase by 0.4% and 0.7%, respectively. Also, the gross amount of Pell grants disbursed by merit state colleges continues to significantly increase post-treatment as does state appropriations and state grants and contracts.

Focusing on private colleges, Table B.9 presents results for private colleges using expenditures per FTE to indicate merit aid programs. Results from the baseline specification and using expenditure data are very similar. Merit aid does not significantly affect tuition, revenues from donors, or institutional grants, but private colleges in merit states do slightly increase instructional expenditures. Also, the introduction of merit aid programs increases the gross amount of Pell grants disbursed at private colleges.

Overall, results from both the baseline specification and from using actual expenditure data suggest that neither public nor private colleges in merit states capture merit scholarship funds. Instead colleges increase expenditures on students and experience significant, albeit modest, increases in the gross amount of Pell grants disbursed.

2.6 Conclusion and Implications

Merit aid programs have grown substantially over the last two decades, and an extensive amount of research examines the demand-side effects of these programs. Yet much less is known about how colleges respond to state-implemented merit aid programs. This paper examines whether merit aid programs affect the supply side of higher education and contributes to the literature by analyzing multiple programs and exploring numerous college-level outcomes. Additionally, an extension is provided that utilizes actual program expenditures as a measure of the size of merit aid programs. Using a 1986-2009 panel of colleges, I find that public and private colleges do not seem to be capturing HOPE-like scholarships through significant increases in tuition prices or decreases in institutional aid, at least at the aggregate level. Instead, public and private colleges supplement merit aid programs with increases in expenditures on students such as instructional and student services expenditures. Also, colleges in merit states experience an increase in the gross amount of Pell grants disbursed after program implementation. Overall, colleges do not seem to be extremely responsive to the introduction of merit aid programs, and findings are informative for policymakers. Results suggest that students are receiving the full value of their merit scholarships as intended by policymakers, and if anything, might even receive additional financial aid through Pell eligibility.

How do results compare to those in the existing literature? Long (2004a) examines supply-side effects specifically for the Georgia HOPE scholarship. Using the data set from this paper, I conduct a very similar analysis using the traditional differences-in-differences identification, with Southeast states as the control group for Georgia over the academic years 1989-1990 to 1996-1997.[25] Long (2004a) finds that HOPE leads to modest decreases in tuition at Georgia's public colleges and modest increases in tuition at private colleges, which contradicts the findings discussed above. I am able to replicate the results from Long (2004a), particularly regarding public tuition, instructional expenditures per FTE, and institutional grants, and this exercise suggests that some of the responses from colleges in Georgia are not generalizable to colleges in other states with merit aid programs.

[25] Long (2004a) examines whether public and private four-year colleges respond to the introduction of the Georgia HOPE in terms of state appropriations, tuition, room and board fees, institutional aid, and instructional expenditures per student and uses two different specifications. One is a traditional differences-in-differences identification while the other is also a differences-in-differences identification but incorporates the proportion of the student body that are HOPE recipients. Additionally, the analysis uses data from IPEDS rather than the Delta Cost Project database, and competitor colleges where more than five percent of the student population are from Georgia are omitted since these colleges may respond to the HOPE. Thus, there are differences between the number of observations and the data set as well as slight variations in the control variables used in the replication compared to those in Long (2004a).

In conclusion, previous research that examines how colleges respond to merit aid programs has produced mixed results, and comparisons are difficult given variations in programs studied, data sets used, and empirical strategies. This paper aims to broaden the time period studied in order to collectively analyze how merit aid programs affect colleges in terms of multiple outcomes. Altogether, this study along with previous work suggest that colleges are not extremely sensitive to merit scholarship programs as colleges are not drastically altering tuition prices. In fact, findings here suggest that colleges may even slightly increase expenditures on students. However, an open question still remains. How do merit aid programs affect the actual net price that students pay for college? Although I detect little to no change in published tuition prices and institutional aid at the aggregate level in the wake of merit aid, future analyses using student-level data may reveal that state-funded merit scholarships affect students' financial aid packages and thus net price. Additionally, do colleges in merit aid states that increase expenditures on students realize positive demand-side effects? Connecting both demand-side and supply-side analyses will provide insight into the incidence of broad merit aid programs which can help shape future financial aid programs.

Chapter 3

Not Whether, but Where? Pell Grants and College Choices

This work is in collaboration with Celeste K. Carruthers.

Abstract

Pell grants are the largest financial aid vehicle in the United States, and yet, their role in shaping students' college choices is not clear. We identify the effect of initial Pell grant eligibility on college enrollment and college choice, drawing on the enrollment decisions of four cohorts of Tennessee high school graduates and discontinuities in Pell eligibility as a function of federal formulas. Consistent with prior work, we find no evidence that marginal Pell eligibility increases college-going. We go on to show that just meeting the Pell cut-off has little bearing on where students choose to enroll, in terms of college sector or college quality. Below the threshold, where applicants are needier and the grant is more generous, students sort into colleges with modestly higher published tuition, on the order of 12 - 14 cents per dollar of Pell aid. And yet, no other measure of college quality or college selectively significantly diverges from the counterfactual. We conclude that Tennessee's traditional-aged students do not use the Pell grant as a tool to shop among college options in ways that systematically improve enrollment outcomes.

JEL: I22, I23, H75

Keywords: Education Policy, Higher Education, Financial Aid

3.1 Introduction

Over 9.4 million students received Pell grants, the largest source of federal grant aid in higher education, in the 2011-2012 academic year, amounting to $33.6 billion in expenditures (U.S. Department of Education, 2012). This extensive grant program intends to increase access to college for financially constrained students, but research shows that initial Pell eligibility has little to no positive impact on college enrollment (Hansen, 1983; Kane, 1995; Bettinger, 2004; Rubin, 2011), with notable exceptions provided by older, non-traditional students (Seftor and Turner, 2002).[1] The idea that traditional student demand for college is insensitive to Pell eligibility is not altogether surprising, for three reasons. Foremost, the application process for federal financial aid is complex and can stretch well into a student's senior year of high school, which weakens and delays the communication of aid eligibility to students on the margin of going to college (Dynarski and Scott-Clayton, 2006, 2008; Dynarski and Wiederspan, 2012; Dynarski et al., 2013). Easing this process meaningfully increases college enrollment (Bettinger et al., 2012). Second, applicants observe Pell eligibility at the same time as institutions, who may capture a portion of federal funds by reducing institutional aid (Turner, 2014). And third, marginal Pell eligibility is associated with a fairly small grant, so local treatment effects could understate the impact of Pell grants more broadly. With these ideas in mind, two of the remaining questions about Pell eligibility and college-going center around students' choice of college, conditional on enrollment, and treatment effects for Pell grants larger than the modest scholarship available at the beginning of the phase-in range.

Using data on four cohorts of Tennessee high school graduates from the classes of 2006-2009, we utilize discontinuities in Pell eligibility along continuous values of "expected family contribution," a relatively opaque construct of federal aid processing, to estimate the impact of Pell grants on the extensive margin of college enrollment, and we go on to examine whether Pell eligibility affects *where* a student chooses to enroll, in terms of college sector and college quality.[2] Are Pell recipients more inclined to enroll out-of-state? Are students receiving the Pell more likely to attend four-year universities rather than two-year colleges? Do Pell

[1] Limited or null effects on the college entry margin do not rule out a response from enrolled students to the threat of *losing* Pell (Scott-Clayton and Schuddee, 2015).

[2] Throughout the study we use "college quality" interchangeably with "college selectivity," although we acknowledge that selectivity measures do not equate with institutional value added.

recipients attend higher quality institutions? To date, researchers have exploited the Pell eligibility threshold to examine on-time college enrollment (Rubin, 2011) and institutional aid (Turner, 2014), but it is not yet known whether Pell grants affect student choices along the spectrum of college quality. The same Pell award can be used at almost any institution, so Pell has the potential to help the neediest students enroll in better colleges. One reason for this gap in the literature is the rare confluence of data on complete cohorts of high school graduates, linked to both college enrollment outcomes and detailed data from financial aid applications. We draw from linked administrative data on Tennessee's public high school graduates that makes all of the necessary connections for a regression discontinuity analysis of college enrollment behavior and Pell eligibility.

Other aid programs have been shown to affect college choice, sometimes with unintended consequences. Bruce and Carruthers (2014) find that the Tennessee HOPE scholarship, a broad-based merit scholarship, induces a small substitution effect towards four-year colleges and away from two-year colleges for marginally eligible students, and Goodman (2008) and Cohodes and Goodman (2014) find that students favor in-state public colleges rather than higher quality private or out-of-state colleges in response to merit-based tuition waivers in Massachusetts. There is a fundamental difference between Pell grants and merit aid, however, that may dampen the effect of Pell on college choice. Eligibility criteria for merit aid programs are widely publicized and tied to standardized test scores or grade point averages. Students are well-informed as to their merit aid eligibility, sometimes long before they start applying to college. Eligibility for the Pell grant, by contrast, is much less transparent and students are notified late in twelfth grade.

In addition to characterizing the effect of Pell aid on college choices, we extend the literature in a second way by assessing whether the local impact of the Pell grant generalizes away from the eligibility threshold, where students are needier and where the grant is more generous. We extrapolate treatment effects below the eligibility cut-off based on the relationship between college enrollment and students' observable characteristics just above the cut-off (Angrist and Rokkanen, 2015).

Consistent with previous work, our findings indicate that Pell eligibility does not significantly impact college enrollment *per se* for traditional-aged students. Moreover, we find little evidence that applicants use Pell eligibility to shop among college options. With three exceptions, passing the Pell eligibility threshold has no impact on college sector or college quality. The exceptions are thus: we find that (1) marginally Pell-eligible males enroll in more selective colleges relative to marginally ineligible males, (2) females substitute into out-of-state four-year public colleges upon gaining Pell eligibility, and (3) Pell-eligible students enroll in institutions where tuition and fees are modestly higher than the counterfactual. But these findings are economically small, statistically imprecise, or sensitive to specification changes. We conclude that Pell grants – the largest need-based aid vehicle in the United States – have no discernible impact on whether traditional-aged students go to college and at best modest effects on where students go to college.

3.2 Conceptual and Policy Framework

The federal Pell Grant program initially began under Title IV of the Higher Education Act of 1965. The specific timeline of Pell grant determination illustrates how students might leverage the grant to shop among college options as well as the difficulties of doing so in reality. Students seeking to be eligible for the Pell as well as other federal grants, subsidized loans, or work-study programs must complete a Free Application for Federal Student Aid (FAFSA). Students begin to file FAFSAs in January for fall enrollment the same year, and colleges often require that high school seniors submit their FAFSA between February and March in order to be given priority for need-based institutional aid.

The FAFSA collects demographic information, federal income tax information, family size, the number of family members in college (excluding parents), and other financial data such as assets and untaxed income. Students indicate at least one college to receive the output of FAFSA processing. The 2006-2009 cohorts in our study could submit the FAFSA electronically or by mail. Up to four colleges could be listed on paper FAFSAs while online applications allowed for six, increasing to ten with 2009 applicants (U.S. Department of Education, 2007).

Once a student submits her FAFSA, the Central Processing System calculates her Expected Family Contribution (EFC). EFC is the amount that the student or her family are expected to provide for college expenses, based on ability to pay. To begin calculating EFC, allowances for taxes and basic living expenses are subtracted from the family's income to yield "available" income. The family's available income along with a percentage of net assets is divided by the number of dependents in college (including the applicant) to generate EFC.[3] Within three days to three weeks after a student submits her FAFSA, the Central Processing System sends the student a Student Aid Report, and all schools listed on the student's FAFSA receive an Institutional Student Information Record. The Student Aid Report lists the student's FAFSA information as well as her EFC and potential Pell grant eligibility.

At this point a student is notified that she *may be* eligible for a Pell grant but she is not made aware of the amount. Pell grants are a simple function of EFC, and Pell-to-EFC schedules are published each year after federal appropriations have been determined (typically in January). In principle, students can easily find out what their Pell grant should be upon learning their EFC. But the Student Aid Report – the primary communication between the federal aid process and students – did not include this information for the cohorts we study. One reason for this omission is that each component of the EFC formula is subject to audit and verification by institutions, who are responsible for compiling aid packages for each admitted applicant.[4]

Financial aid offices at each college where a student is admitted use the relayed Institutional Student Information Record to determine how much need-based aid she can receive based on the reported EFC. Students' need-based and merit-based aid is collectively bound by the relevant cost of attendance. The cost of attendance for a particular college includes tuition and fees, room and board, books and supplies, miscellaneous expenses, and child care or dependent care allowances. There is a maximum and a minimum Pell grant for eligible students and a maximum EFC for Pell eligibility. These criteria vary from year to

[3]There are actually six different EFC formulas according to the student's dependency status and whether the student qualifies for a simplified formula. The dependency-based formulas are very similar and mainly differ by the percentages and allowances used. The simplified formula uses a reduced set of financial indicators and is available to families who are eligible to file a 1040A or 1040EZ tax form instead of the longer 1040 form, or who participate in means-tested federal benefits programs and have an adjusted gross income less than a specific amount ($49,999 for 2012-2013).

[4]Current applicants are presented with an estimate of their Pell grant upon completing the online FAFSA, although EFC components are still frequently subject to verification.

year according to federal appropriations. Each year, Pell grant eligibility is a sharp function of a particular value of EFC. For instance, year 2006 applicants with EFC equal to $3,851 were ineligible for Pell while applicants with EFC between $3,650 and $3,850 were eligible for a $400 grant. Then for EFC values below $3,650, the Pell grant increased one-for-one until the maximum Pell grant of $4,050 was reached for applicants with zero EFC.[5]

Typically, admitted students receive letters in March or April from college financial aid offices specifying the amount of Pell grants and institutional scholarships they are eligible to receive if they enroll. A student's EFC and potential Pell grant is constant across her choice set of colleges, but financial aid packages will differ across institutions according to cost of attendance, college resources, and available state aid. The idea that Pell grants could help students choose between different colleges rests on the non-trivial assumption that students can jointly consider admissions and aid information from multiple schools when deciding where to enroll. The federal aid application timeline offers two ways to do this. When submitting the initial FAFSA, as noted, students can request that their financial details be sent to up to ten schools. After the initial application, students can add or delete schools from this list online, by mail, over the phone, or via the financial aid office of a prospective college. In practice, however, a majority of students list just one school on their FAFSA applications (Turner, 2014).

Could Pell grants lead students to choose more selective institutions? Theoretical expectations are unclear. Pell-eligible students can apply the same grant to any Title IV institution: two-year, four-year, public, private, in-state, or out-of-state.[6] In that sense, Pell grants are conditional transfers that relax households' income constraint for college expenditures and other goods. Ignoring for a moment the institutional response to Pell aid, the grant may affect students' relative preferences for different colleges if demand for college quality is responsive to net price. If the returns to college are greater for those educated at

[5]Minimum Pell awards were $400, $890, and $976 for 2007, 2008, and 2009 applicants, respectively, and maximum Pell awards were $4,310, $4,731, and $5,350.

[6]There are rare cases where Pell awards are reduced for exceeding a school's cost of attendance.

more selective institutions,[7] then basic human capital theory holds that a rational applicant will leverage Pell aid into a more costly, more selective institution.[8]

This simple prediction rests on students having complete information, but in reality they may be uninformed about colleges that make up their choice set, uncertain about their chance of success at a more selective institution, or otherwise unsure about the future benefits of a college education. Moreover, the visage of a rational applicant is more nuanced than one who considers only net price and lifecycle income. Students sort into colleges based on a number of other factors: debt aversion, distance from home (Long, 2004b), college amenities (Jacob et al., 2013), and so forth. And the price elasticity of demand for college quality is not readily understood - since the value of the Pell grant is *de jure* equivalent across institutions, Pell eligibility changes the relative price of institutions and could actually lead students to favor less costly, less selective institutions. Consider the Alchian and Allen (1967) theorem, usually applied to a consumer's decision to buy either a high quality or low quality version of the same good. When a fixed cost, typically a transportation cost, is added to both goods, the relative price of a high quality good decreases, leading consumers to purchase the higher quality good in spite of the overall price increase. By contrast, uniform Pell grants increase the relative price of more costly colleges, perhaps leading some students to favor less selective, less costly institutions. The "free ride" that a $5,000 Pell grant affords at a community college, for instance, may have more salience than a $5,000 discount at a four-year university.

Empirical evidence related to our question is less ambiguous than theory. Recent work has shown that gains in families' ability to pay for college – actual or perceived – lead students to select higher quality colleges. Lovenheim and Reynolds (2013) find that rising

[7]See Kane and Rouse (1995) and Reynolds (2012) for evidence that labor market returns to four-year institutions dominate those for two-year community colleges, Goodman et al. (2015) for evidence that just meeting the academic threshold for four-year college admission increases the likelihood of bachelor's degree completion, and Hoekstra (2009) for evidence that just gaining admission to an anonymous and selective flagship university significantly increases earnings as a young adult. The question of returns to college quality is not settled, however: Dale and Krueger (2011) find that ability bias explains all of the wage returns to college selectivity for white students and students with more educated parents.

[8]See Avery and Hoxby (2004) for an overview of the human capital model specifically in the context of college choice. Rational agents calculate present discounted values for each potential college in their choice set and include the cost of college, grants, scholarships, loans, future earnings, consumption, etc. Then, a rational student chooses the school that maximizes the difference between the present discounted value of benefits and the present discounted value of costs.

home equity increases the probability a student attends a high quality institution, and this was especially true for students from low-income families. Hoxby and Turner (2014) report on a randomized controlled trial that provided ACT and SAT test-takers with application fee waivers and information on net price at several different colleges, under the hypothesis that high-achieving, low-income applicants are not fully aware of the aid available to them at more selective institutions. The treatment effectively increased families' perceived ability to pay and led students to enroll in substantially better colleges at a cost of $6 - 15 per student. But for administrative reasons outlined above, the effect of a Pell grant on families' budget constraint may be much less apparent than new housing wealth or an informative mailer.

Expectations from theoretical and empirical frameworks are further clouded when institutional responses are taken in consideration. Colleges publicize the rate of Pell-eligible students they enroll, a signal of economic diversity that may have intrinsic value to institutions and alumni. Nevertheless, colleges allocate institutional grants with an approximation of students' ability to pay as well as full knowledge of applicants' Pell eligibility. The grant may crowd out other sources of aid (Turner, 2014), yielding little difference in net price across a student's choice set.

The opaque and sharp nature of Pell eligibility as a function of EFC motivates a quasi-experimental analysis of the effect of Pell on college choices. But the timing and delivery of official Pell grant amounts, alongside uncertainty surrounding students' price sensitivity and demand for college quality, leads to ambiguous *a priori* expectations at and below the Pell grant threshold. In the reduced form, then, the empirical questions surrounding Pell grants and college choices necessitate a large set of students making college decisions. With this in mind, we examine college enrollment choices for the universe of 2006-2009 Tennessee high school graduates filing FAFSA applications.

3.3 Data and Methods

Data on 2006-2009 Tennessee high school graduates and their FAFSA records are provided by the Tennessee Higher Education Commission, who match students to college enrollment outcomes using the National Student Clearinghouse. We merge information on college selectivity (the ACT scores of incoming classes, instructional expenditures per student, Carnegie class, and published tuition and fees) from the federal Integrated Postsecondary Education Data System (IPEDS). National Student Clearinghouse data on these four cohorts cover observed postsecondary enrollment up to and including the 2009-2010 academic year.[9] For each 2006-2009 graduate, we identify the first instance of a successful FAFSA (excluding dual enrollment) and link that application to college enrollment outcomes the following academic year.[10,11]

From the starting-point sample of 2006 - 2009 Tennessee high school graduates, we omit 22 percent who do not file a FAFSA before 2011. This sample selection is regrettable but necessary to evaluate the Pell grant's impact on matriculation decisions. Pell eligibility is determined via FAFSA processing, so students who neglect to file an application are automatically ineligible. Moreover, the forcing variable used in regression discontinuity analyses – each student's expected family contribution – is unavailable for students without an application on record. Although results will not necessarily generalize to students who do not file for federal aid, findings allow for causal inference regarding financial aid eligibility and college choices among low-income students who are considering college.

Table C.1 lists descriptive statistics for the FAFSA-filing sample as a whole, by subsequent college enrollment (Columns 1-2), as well as for the subset of students whose EFC falls within $3,000 of Pell eligibility (Columns 3-4). Note that 80.5 percent of the bandwidth-restricted sample enrolled in college during the window of time between graduation and 2009-2010. This is considerably higher than the rate of college enrollment for Tennessee high school graduates

[9] The National Student Clearinghouse is a non-profit entity whose enrollment data cover, as of this writing, in excess of 98 percent of United States college students. Coverage tends to be narrower, but still over 90 percent, among Tennessee college students due to a preponderance of small religiously-affiliated colleges that do not participate in the Clearinghouse. We have no reason to expect that non-participation rates will affect results and/or vary discontinuously over the Pell eligibility threshold.

[10] This necessarily leads to imbalance across cohorts, in that we allow the 2006 cohort to wait up to four academic years after graduation to file a FAFSA and enroll in college, whereas we observe just one year after high school for 2009 graduates. As we show in the appendix, results are robust to conditioning on student covariates and cohort fixed effects. Additionally, in an unreported analysis, we find that results are robust to the omission of students whose first FAFSA application is more than one year after high school.

[11] The market for higher education is somewhat concentrated among Tennessee high school graduates, but not so much as to motivate a discrete choice model rather than regression discontinuity identification across a spectrum of college quality. The top five destinations for students in this sample draw 30 percent of college-going seniors, and the Herfindahl-Hirschman Index for all college destinations is 366.

a whole (63.5 percent among these four cohorts), which is a consequence of inherently higher rates of college-going among successful FAFSA filers. Table C.2 lists summary statistics for college outcomes of interest, conditional on enrolling in college at all. We examine a mutually exclusive and exhaustive set of six college sector options, demarcated by control (public or private), state (in Tennessee or not), and level (two-year or four-year). We additionally examine nine measures of college quality and college selectivity, listed and summarized in the bottom portion of Table C.2.

The specification for baseline regression discontinuity analysis takes the following form:

$$Y_{ic} = \alpha + \beta_1 PELL_{ic} + \beta_2 PELL_{ic} * (EFC_{ic} - \bar{E}_c) + \beta_3 (EFC_{ic} - \bar{E}_c) + \epsilon_{ic}, \quad (3.1)$$

where Y_{ic} is a matriculation or institutional outcome for student i in cohort c, $PELL_{ic}$ is an indicator equal to one for students whose EFC is at or below the Pell eligibility threshold, and $\bar{E}_c$ is the relevant Pell eligbility threshold for cohort c. We limit the analysis to students whose nominal EFC falls within \$3,000 of the relevant Pell eligibility threshold, and we round EFC values to the nearest hundred. Robust standard errors are clustered by \$100 EFC bin. The appendix discusses results under wider EFC aggregations, alternative bandwidths (narrower, optimal), the inclusion of controls, and the use of a quadratic rather than a linear function for the gap between i's EFC and the Pell eligibility threshold for i's cohort $(EFC_{ic} - \bar{E}_c)$.

Ours is a sharp regression discontinuity analysis, since Pell eligibility rates rise from zero to very nearly one hundred percent at the $EFC_{ic} = \bar{E}_c$ threshold.[12] Figure C.1 depicts Pell eligibility relative to the EFC cut-off alongside grantees' potential scholarship.[13] The local average treatment effect estimate of Pell eligibility on Y_{ic} is given by β_1 in Equation 3.1. The standard regression discontinuity identification assumption applies: in the absence of Pell grants, the propensity to enroll in different colleges would have varied smoothly around the EFC threshold.

[12]Estimates of Equation 3.1 for reported Pell eligibility point to a first-stage discontinuity of between 99 and 100 percent.

[13]We observe official Pell eligibility in the FAFSA data on hand, but not the amount of the grant students were ultimately awarded. Panel I of Figure C.1 represents students' actual, reported eligibility, whereas Panel II depicts the grant that students would have been eligible for according to their EFC. Pell grants, worth no more than \$5,350 for these cohorts, are rarely scaled back for exceeding the cost of attendance constraint, which generally measures in the tens of thousands of dollars.

We indirectly test the identification assumption in three ways. First, we follow McCrary (2008) and test for discontinuities in the density of EFC values around the threshold, finding that students do not appear to cluster on one side of the threshold.[14] This implies that applicants are not strategically responding to the publicly available (albeit, complex) EFC formula to gain Pell eligibility. Second, we estimate discontinuities in pre-college student characteristics like gender, race, ethnicity, and parental education. Results are listed in the top portion of Table C.3. Other than a significant 1.6 percentage point decline in the likelihood of one's father having a college education, we see no indication that students below the Pell threshold were very different from students just above the threshold. Another way to test the fundamental identification assumption is to estimate college enrollment outcomes as a function of students' observable characteristics (listed in Table C.1) and then examine whether *predicted* outcomes shift at the threshold. The bottom portion of Table C.3 lists findings from that exercise, where we show that expected enrollment outcomes vary smoothly over the Pell eligibility threshold.

As noted in Section 3.2, marginal Pell eligibility yields a modest grant, which may limit the extent to which the students who drive local treatment effects realistically use the Pell to shop for different colleges. A variety of methods are available to estimate treatment effects away from an eligibility threshold based on extrapolations of $Y_{ic} = f(EFC_{ic} - \bar{E}_c)$ functional forms or based on changes in the $f(\cdot)$ slope at the threshold. These hinge on assumptions about counterfactual $f(\cdot)$ in the absence of treatment. Rather than make assumptions about the relationship between counterfactual college choices and the running variable, we pursue a method proposed by Angrist and Rokkanen (2015) that first *explains* that relationship using exogenous characteristics of subjects, and then predicts counterfactual outcomes as functions of those characteristics. Our identifying assumption is thus: in the absence of Pell, outcomes would be mean-independent of $EFC_{ic} - \bar{E}_c$ when conditioned on a set of X_{ic} student variables.

This conditional independence assumption is tested using the following specification, with the sample limited to Pell-ineligible students with EFC values up to $3,000 above the

[14]The log difference in bin height is estimated to be 0.060 at the eligibility threshold with a standard error of 0.045.

cut-off:[15]

$$Y_{ic} = \alpha + \pi(EFC_{ic} - \bar{E}_c) + X_{ic}\gamma + \varepsilon \tag{3.2}$$

A parsimonious set of X_{ic} observables describing student i are used to model college enrollment choices. If the running variable $EFC_{ic} - \bar{E}_c$ is an insignificant determinant of enrollment outcomes, conditional on X_{ic}, we can more confidently argue that $\hat{Y}_{ic}$ predictions *below* the eligibility threshold based on parameter estimates from students above the threshold are adequate counterfactual outcomes. Our task, then, is to explain enough of the variation in Pell-ineligible students' college choices to render the conditional correlation with distance from the cut-off null. To do so we include in X_{ic} measures of demographics (gender, Caucasian race), families' ability to pay (real adjusted gross income and the ratio of EFC to adjusted gross income), parental education (binary indicators for mothers' college education and fathers' college education), and student ability and commitment to college (first ACT composite score, the number of ACT attempts, indicators for missing ACT data). We find that π estimates from Equation 3.2 for the set of college sector and college quality outcomes are generally small and insignificant (see Table C.10 in the appendix). Predictions from Equation 3.2 are extrapolated as counterfactual outcomes below the Pell eligibility threshold using linear re-weighting estimators (Kline, 2011).

3.4 Results

3.4.1 The Impact of Pell Eligibility on College Enrollment and the Choice of College Sector

Figure C.2 summarizes findings for the effect of Pell eligibility on any college enrollment. Point estimates and standard errors for β_1 in Equation 3.1 are listed above each figure. Echoing earlier work that utilizes a similarly sharp identification strategy, but in a survey

[15]We limit the conditional independence test to ineligible students above the Pell threshold since the Pell grant treatment grows as EFC falls. Note that this is a weaker test than that articulated by Angrist and Rokkanen (2015), who apply this method to a setting where outcomes are assumed to be mean-independent of the running variable on both sides of the threshold. We use a $3,000 bandwidth for the sake of consistency with the regression discontinuity sample, although inframarginal results are largely unchanged under wider bandwidths.

setting with less statistical power (Rubin, 2011), we find no discernible increase in college-going below the Pell eligibility threshold. On the contrary, we observe a marginally significant *decline* in college-going among white FAFSA-filers, although as we show in the Appendix, this counter-intuitive result is sensitive to modifications of the Equation 3.1 specification.

One reason that Pell eligibility might be ineffective at pushing marginal college students to enroll is that eligibility is communicated to students fairly late in their senior year of high school, after students have committed to enrolling (or not enrolling) in college. This would also tend to work against Pell as a tool for students choosing between different colleges, but in principle applicants have leeway to consider aid packages from multiple schools, and to date, the idea that Pell eligibility helps students shop among their choice set is untested. With this in mind, we limit the sample to college-going students and estimate Equation 3.1 for a mutually exclusive and exhaustive set of six college sectors. Results – for the entire sample of college enrollees and subsamples divided by gender and race – are listed in Table C.4.

Column (1) lists point estimates and robust standard errors for β_1 from Equation 3.1, applied to the college sector choices of all college enrollees in the four-cohort sample of Tennessee high school graduates. Column (1) results are also depicted in Figure C.3. We find no significant discontinuities in students' enrollment behavior broadly, although there are three marginally significant results for subsamples that are worthy of note. First, males are 0.4 percentage points more likely to enroll in a private two-year college below the Pell eligibility threshold. These are typically for-profit sub-baccalaureate colleges. Though the point estimate is small and weakly significant, it represents 80 percent of the share of students enrolling in this sector. Second, females are somewhat less likely to enroll in an in-state four-year college and somewhat more likely to enroll in an out-of-state four-year college just below the Pell threshold, relative to students who are marginally ineligible for Pell. As we show in the Appendix, these findings are sensitive to wider bandwidths but otherwise robust across alternative specifications.

Among the thirty estimates presented in Table C.4, we might expect three to fall within the 10 percent level of statistical significance. Even if we consider them to be genuine consequences of Pell eligibility, they do not amount to strong evidence that Pell eligibility helps students shop across college options or enroll in better colleges than they would have otherwise.

We note an important observation from Figure C.3 before moving on to results for student decisions in terms of college quality. The slope of some outcomes with respect to the running variable changes noticeably at the threshold in ways that suggest that the elasticity of student responses with respect to the grant *value*, rather than grant eligibility, favors more selective colleges. In particular, enrollment in Tennessee two-year community colleges appears to become *less* likely for needier students who qualify for larger Pell grants, and enrollment in out-of-state four-year public universities becomes *more* likely. Since the grant rises dollar-for-dollar in this range of EFC, one way to quantify changes in slope is to interpret β_2 in Equation 3.1 as a regression kink estimate of student responses to aid.[16] We do not pursue a regression kink design here because a number of predetermined covariates listed in Table C.3 also exhibit a small but significant change in slope proximate to the eligibility threshold, as do regression-adjusted enrollment outcomes predicated on those covariates.[17] Rather, in Section 3.4.3 we present results based on supra-threshold extrapolations that condition on student observables.

3.4.2 Pell and the Quality of College

Next, we examine the potential effect of Pell eligibility on the quality of college chosen, as proxied by nine institutional characteristics drawn from IPEDS: the 25th and 75th percentiles of enrollee ACT scores, per-student instructional expenditures,[18] student-faculty ratios, graduation rates, Carnegie class, tuition and fees for in-state residents, tuition and fees for out-of-state residents, and lastly, tuition and fees conditional on students' actual residency (i.e., students attending Tennessee institutions are assigned in-state tuition and students attending out-of-state institutions are assigned out-of-state tuition). Results are

[16] Regression kink methods are described in detail by Card et al. (2012).

[17] Nevertheless, conclusions from regression kink analyses are in accord with those discussed in Section 3.4.3.

[18] Instructional expenditures per student are also available in the Delta Cost Project Database. The Delta Cost Project database consists of IPEDS financial data for colleges, but the data have been adjusted to account for changes in reporting and accounting standards over time. Results and robustness checks when using instructional expenditures per FTE from Delta Cost are consistent with those when using instructional expenditures directly from IPEDS.

listed in Table C.5. We see little evidence in Column (1) to suggest that the broad population of Tennessee college-goers systematically sorts across college quality domains in response to Pell eligibility. All of the point estimates in Column (1) of Table C.5 are consistent with the idea that students move to higher-quality institutions because of Pell, but none of these estimates are statistically significant at conventional levels.

Columns (2) through (5) of Table C.5 lists results by gender and race. The standout result from this stratification is that male college choices may be somewhat sensitive to Pell eligibility. Marginally Pell-eligible males attend institutions with a slightly higher interquartile range of student body ACT scores, by 0.22 composite points at the 25th percentile and 0.31 points at the 75th percentile. These discontinuities reflect 1.2 percent and 1.3 percent, respectively, of bandwidth-restricted samples means for these selectivity measures (8.1 and 10.0 of their respective standard deviations). We also detect weakly significant discontinuities in student-faculty ratios for males, but not for other subsamples. Of the fifteen college outcomes we examine in this study, results for the interquartile ACT range of males' college choices are thus far the strongest evidence for quality upgrading at the Pell eligibility threshold. The magnitude of ACT improvements at males' chosen colleges may yield significant returns that outweigh the direct cost of Pell support.[19] Note, however, that discontinuities in interquartile ACT are not only local to the male subgroup, but more specifically to males at the Pell eligibility margin (see Appendix Table C.11).

3.4.3 Treatment Effects Away from the Eligibility Threshold

With the notable exception of gender-specific findings for certain college measures, regression discontinuity results imply that Pell eligibility and Pell awards have little bearing on students' demand for college overall, nor on their relative demand across the spectrum of college quality

[19]Hoekstra (2009) finds that marginally successful applicants to a selective flagship university earn 18 - 28 percent more, annually, as 28 - 33 year-olds than marginally ineligible applicants. The anonymous flagship drew freshmen scoring 65 points higher on the SAT than the next most selective in-state university, a difference equivalent to 6.4 percent of the mean among the relevant cohort per The College Board (1996, Table 4-2). Here, we find that white Pell-eligible males sort into colleges with higher ACT scores by 1.2 - 1.3 percent of the mean. If, in agreement with Hoekstra (2009), this results in 3.4 - 5.3 percent gains in annual earnings for affected students, the value of additional earnings could amount to $37,000 - $57,000 over a 30-year career, under a 3 percent discount rate and baseline weekly earnings of $1,101 (the average weekly earnings for full-time and salary workers with a bachelor's degree over age 25, per the Current Population Survey).

and selectivity. This may be because the marginal Pell grant is somewhat modest, $400 for the first two cohorts in our sample, and $890 and $976 for subsequent cohorts.[20] It may be the case that null local treatment effects do not generalize, and that needier students who qualify for larger Pell grants well below the threshold are more affected.

Therefore, we follow a method proposed by Angrist and Rokkanen (2015) and examine treatment effects below the eligibility threshold. As outlined in Section 3.3, we first focus on Pell-*ineligible* students whose EFC falls within $3,000 of the Pell cut-off and estimate college outcomes as a function of exogenous student characteristics listed in Section 3.3. We then map parameter estimates to Pell-*eligible* students with EFC values up to $3,000 below the threshold. Treatment-on-the-treated effects are taken to be the average difference between observed and predicted outcomes, with standard errors calculated following Kline (2011).

Key results are depicted in Figure C.4 for any college enrollment, Figure C.5 for college sector outcomes, and Figure C.6 for college quality outcomes (including the only four with statistically significant treatment effects). Circles represent observed student outcomes and "X" markers represent counterfactual estimates, summarized by $100 EFC bin. Treatment effects and standard errors are listed under each figure heading. We find no significant treatment effect on college-going below the Pell cut-off (Figure C.4), and no meaningful effect on students' chosen college sector (Figure C.5). A possible exception is found in the sub-figure for private four-year enrollment, where we see a weakly significant 1.1-point positive treatment effect below the threshold.

Regarding the quality of colleges chosen (Figure C.6), we do not see a significant gap between observed and counterfactual outcomes for any measure of selectivity or quality *except* for published tuition and fees and colleges' graduation rate. Sub-threshold treatment effects indicate that Pell-eligible students attend institutions with higher tuition and fees than they would have otherwise, by $261 on in-state schedules and $299 on out-of-state schedules. Within this range of Pell-eligible students, grants were $2,120 on average. This means that each dollar of Pell aid was potentially offset by 12.3 - 14.1 cents of higher tuition,

[20] A modal finding in the early financial aid literature is that each $1,000 yields a 4-percentage-point increase in college-going (see Deming and Dynarski (2010) for a review), so despite the small initial value of the grant, our null results run counter to expectations of 2 - 4 percentage-point gains in college-going as a result of marginal eligibility.

without considering reductions in out-of-pocket costs from institutional grants. But when we look at published tuition and fees at each students' institution and condition on their residency, it is harder to make the case that Pell grants give students more buying power. Residency-based tuition is higher than the counterfactual, but by a weakly significant $214. Pell-eligible students also attend institutions with a higher graduation rate, but estimated treatment effects are small and weakly significant (0.54 percentage points is 2.8 percent of a standard deviation among college-going students in this bandwidth).

3.5 Discussion

The effect of Pell grants on traditional students' college-going has been found to be small or null (Hansen, 1983; Kane, 1995; Bettinger, 2004; Rubin, 2011), but to date, it is unclear whether Pell eligibility affects *where* students enroll. We extend the need-based financial aid literature in this direction, utilizing unique administrative data that links high school graduates to detailed financial aid data as well as college enrollment outcomes. Based on discontinuous changes in Pell eligibility along the schedule of expected family contribution, we find little evidence to suggest that students use the Pell as a tool to expand or shop among their choice set of colleges. Scattered results in favor of quality upgrading among marginally Pell-eligible students include females' substitution out of state and males' choice of more selective colleges at the threshold, but these results are economically small, sensitive to specification, or statistically imprecise.

Perhaps our most intriguing finding regards the tuition and fees of Pell-eligible students' selected institutions. By examining treatment effects below the threshold, we find support for the idea that students sort into more costly colleges as a consequence of successively more generous Pell aid. It is worth noting, however, that we cannot discern whether this arises from student-led demand for more expensive colleges, or from institution-led demand for Pell-eligible students. Moreover, higher tuition does not necessarily equate with higher quality, and several other dimensions of college selectivity and quality change very little (e.g., graduation rates) or not at all (instructional expenditures, Carnegie research status) at and below the eligibility threshold.

One likely explanation for our null results lies with the way in which Pell eligibility and Pell amounts are announced to students. The same opaque and formulaic eligibility criterion that motivates sharp regression discontinuity analysis may hinder applicants' awareness of the program. Moreover, results from financial aid applications are delivered late in the senior year of high school, when many college plans are already in place. More salient changes in families' perceived ability to pay have been shown to lead to quality upgrading (Lovenheim and Reynolds, 2013; Hoxby and Turner, 2014), although the theoretical implications of a uniform change in net price throughout the college quality spectrum remain unclear. And of course, the Pell-induced price differential is blunted to the extent that Pell grants crowd out institutional aid. Though we cannot deconstruct the relative contributions of these four candidate mechanisms, we can conclude that their collective effect is to rule out Pell grant eligibility as a systematic factor in college choice.

Conclusion

This dissertation consists of three chapters that examine the impact of financial aid programs on students' enrollment decisions, student outcomes, and colleges' financial decisions. The first chapter examines the impact of the Tennessee HOPE, a broad merit-based scholarship, on community college students' outcomes both during and after college. Community colleges are a large component of higher education in the U.S., but to date no study has focused exclusively on the impact of merit aid on community college students. Also, to my knowledge, this is the first study to examine how financial aid impacts earnings after college. Focusing on ACT scores in determining HOPE eligibility, I use a regression discontinuity estimator which essentially compares students who are barely eligible for the HOPE scholarship to students who just fall short of eligibility. Findings suggest that there is no local impact of HOPE eligibility on persistence, academic performance, degree completion, transfer rates to four-year universities, or earnings after college for marginally eligible students. While understanding the mechanisms through which financial aid affects students' choices and outcomes are beyond the scope of this paper, possible contributing factors may include program design details, students losing their HOPE scholarship after enrollment, and the relatively small value of the HOPE scholarship compared to students' future life-time earnings.

In the second chapter, I examine whether colleges are sensitive to the introduction of state-sponsored merit aid programs. Previous research has emphasized demand-side effects such as how merit aid impacts enrollment and post-matriculation outcomes. Yet much less is known about how merit aid programs affect the supply side of higher education. Additionally, this paper extends the literature by analyzing multiple programs and exploring numerous college-level outcomes. Using differences-in-differences identification, I find that public and private colleges do not seem to be capturing HOPE-like scholarships through significant increases in tuition prices or decreases in institution aid, at least at the aggregate level. Instead, public and private colleges seem to supplement merit aid programs with increases in expenditures on students. Results suggest that colleges are not extremely sensitive to merit scholarship programs and that students are receiving the full value of their merit scholarships as intended by policymakers. Although, the literature and policymakers will benefit from additional work in this area. Specifically, future analyses that use student-level data to assess how merit aid affects students' financial aid packages and thus net price will continue to be informative. Also, connecting both demand-side and supply-side analyses will provide further insight into the incidence of broad merit aid programs which can help shape future financial aid programs.

In the third chapter, we use discontinuities in Pell grant eligibility to examine the effect of Pell grant eligibility on college enrollment and college choice. Similar to previous work, we find no evidence that Pell grant eligibility increases college-going. We go on to find that students do not seem to use the Pell grant as a tool to expand or shop among their choice set of colleges in terms of sector or quality dimensions. Below the threshold, where applicants are needier and the grant is more generous, students sort into colleges with modestly higher published tuition. However, we cannot discern whether this arises from student-led demand for more expensive colleges, or from institution-led demand for Pell-eligible students. Furthermore, higher tuition does not necessarily equate with higher quality, and other measures of college quality or college selectivity do not significantly diverge from the counterfactual below the eligibility threshold. Some possible explanations for our nearly null result may include applicants not receiving financial aid packages until well into their senior year. Additionally, other factors beyond financial aid have been found to influence college choice, and Pell grants crowding out institutional aid may also play a role. While we cannot deconstruct such contributing factors, we can conclude that their collective effect is to rule out Pell grant eligibility as a systematic factor in college choice.

While the goals of specific financial aid programs vary, expansive programs such as the HOPE scholarship and the Pell grant are generally established in order to increase access to college, promote academic success and completion, and ultimately support employment. The papers in this dissertation examine such intended outcomes as well as some potential unintended outcomes, and together, this research aims to be informative for those designing and implementing financial aid programs.

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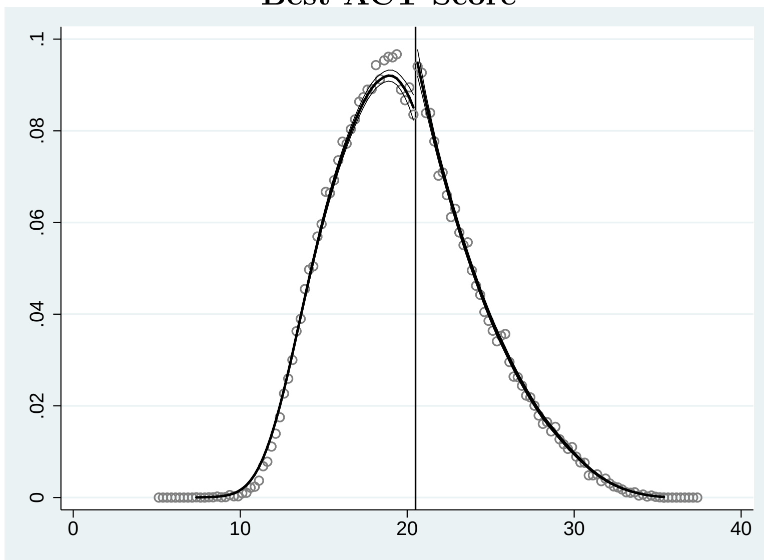
Appendices

Appendix A

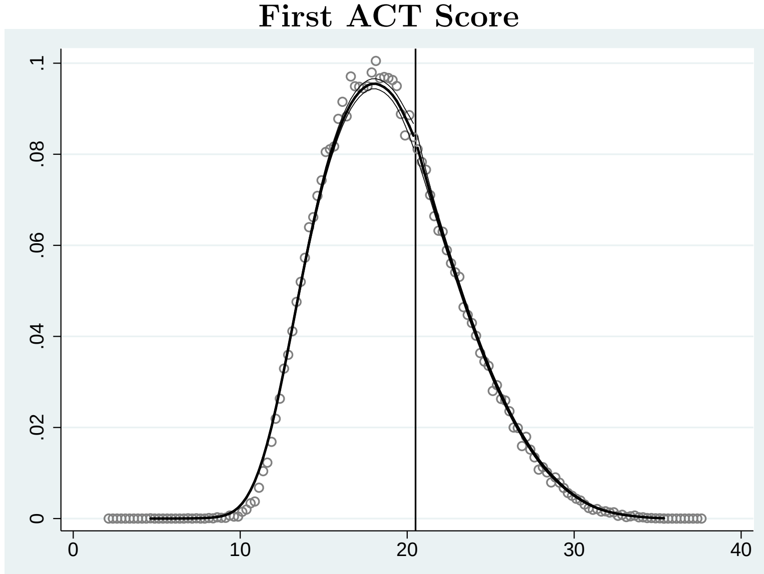
HOPE for Community College Students: The Impact of Merit Aid on Persistence, Graduation, and Earnings

Best ACT Score



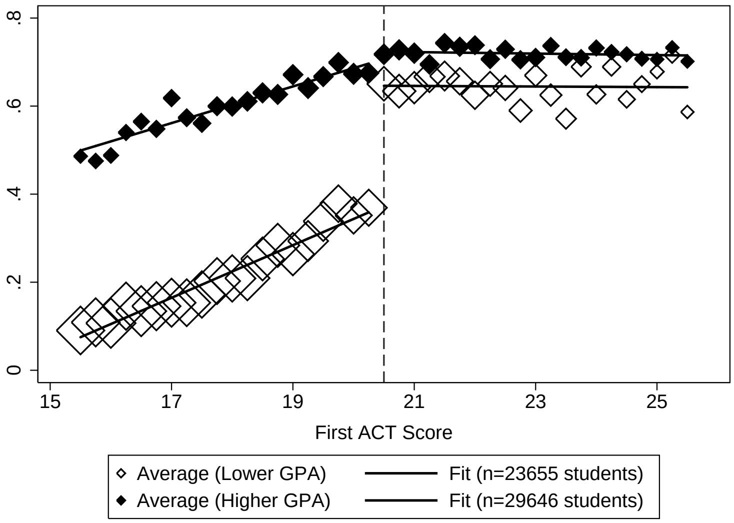
Discontinuity estimate: 0.1450
Standard error: 0.0259
t-statistic: 5.60

First ACT Score



Discontinuity estimate: 0.0025
Standard error: 0.0261
t-statistic: 0.10

Figure A.1: McCrary (2008) density test for ACT scores, entering freshman in academic years 2005-2009 at Tennessee public community colleges (n = 72,640)



Discontinuity (standard error)

Figure A.2: HOPE eligibility by first ACT score for low GPA students 0.273 (0.012) and high GPA students 0.016 (0.010)

Table A.1: Summary Statistics: entering freshman in academic years 2005-2009 at Tennessee public community colleges within five points of the 20.5 ACT threshold

	(I) All Students	(II) Low GPA Students	(III) High GPA Students
HOPE Eligible	0.503	0.303	0.663
Number of Semesters	6.116	5.217	6.833
	(3.573)	(3.559)	(3.419)
Cumulative Hours after 1 Year	18.865	20.777	17.340
	(9.806)	(8.600)	(10.422)
Cumulative Hours after 2 Years	37.097	35.188	38.620
	(17.013)	(17.565)	(16.400)
Last Observed GPA	2.529	2.217	2.753
	(0.987)	(0.970)	(0.937)
Transferred to Four-Year College	0.381	0.218	0.512
Associate's Degree within 3 Years	0.094	0.066	0.117
Bachelor's Degree within 5 Years	0.147	0.072	0.215
Any Earnings While Enrolled	0.884	0.883	0.886
Average Quarterly Earnings While Enrolled	1,351.873	1,525.773	1,213.116
	(1,323.719)	(1,415.132)	(1,228.469)
Expected Annual Wage	49,504.838	45,128.210	52,975.728
	(19,612.702)	(18,582.191)	(19,714.168)
Any Earnings in 5^{th} Year After Enrollment	0.728	0.716	0.742
Average Quarterly Earnings in 5^{th} Year	2,645.878	2,516.007	2,815.929
	(2,792.820)	(2,660.090)	(2,949.319)
Male	0.424	0.499	0.363
Black	0.092	0.131	0.060
Hispanic	0.017	0.020	0.015
Other nonwhite	0.054	0.052	0.055
Lower-income	0.421	0.450	0.398
2006 Cohort	0.199	0.205	0.194
2007 Cohort	0.198	0.195	0.201
2008 Cohort	0.220	0.207	0.230
2009 Cohort	0.224	0.214	0.233
First ACT Score	19.585	18.671	20.314
	(2.651)	(2.373)	(2.635)
Maximum ACT Score	20.417	19.185	21.400
	(2.977)	(2.500)	(2.962)
Observations	53,301	23,655	29,646

Notes: Column I lists the mean for all entering freshman in academic years 2005-2009 at Tennesee public community colleges scoring within five points of the 20.5 ACT threshold. Column II and III split the sample into students with a missing or proxy high school GPA that is less than 3.0 and students with a proxy high school GPA greater than 3.0. Standard deviations are in parentheses for continuous variables.

Table A.2: Estimates of discontinuities in control variables

	(I) Low GPA Students	(II) High GPA Students
Male	0.056	0.581
	(0.042)	(0.774)
Black	0.121***	1.588
	(0.042)	(1.158)
Hispanic	0.002	-0.129
	(0.010)	(0.153)
Other nonwhite	-0.004	0.038
	(0.023)	(0.273)
Lower-income	-0.025	0.617
	(0.050)	(0.942)
2006 Cohort	0.023	-0.448
	(0.038)	(0.603)
2007 Cohort	0.058	0.318
	(0.041)	(0.539)
2008 Cohort	-0.033	0.772
	(0.042)	(0.614)
2009 Cohort	-0.036	-0.058
	(0.022)	(0.409)
Observations	23,655	29,646

Notes: The table lists the the estimated discontinuity at 20.5 ACT points for control variables. Discontinuities are estimated using Equation 1.2. Estimations are limited to students whose first ACT score is within five points of the 20.5 threshold. Column I lists results for students with missing high school GPA proxies or high school GPA proxies below 3.0. Column II lists results for students with high school GPA proxies above 3.0. Robust standard errors, clustered by first ACT score, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: The impact of HOPE eligibility on post-matriculation outcomes

	(I) Mean	(II) Low GPA Students	(III) High GPA Students
		First Stage: HOPE Eligibility	
HOPE Eligibility	0.503	0.273***	0.016
		(0.012)	(0.010)
R-squared		0.189	0.021
		Two Stage Least Squares: Effect of HOPE on Outcomes	
Number of Semesters	6.116	0.362	2.919
		(0.285)	(3.000)
Cumulative Hours after 1 Year	18.865	1.197	-11.886
		(1.211)	(14.388)
Cumulative Hours after 2 Years	37.097	3.367*	-11.486
		(1.749)	(31.237)
Last Observed GPA	2.529	0.007	-1.565
		(0.079)	(1.974)
Transferred to Four-Year College	0.381	0.012	0.570
		(0.049)	(0.552)
Associate's Degree within 3 Years[1]	0.094	0.015	-0.852
		(0.026)	(0.989)
Bachelor's Degree within 5 Years[2]	0.147	-0.044	1.357
		(0.044)	(1.887)
Any Earnings While Enrolled	0.884	-0.021	0.341
		(0.030)	(0.514)
Average Quarterly Earnings While Enrolled	1,351.873	-128.399	-377.972
		(134.476)	(1,322.390)
Expected Annual Wage	49,504.838	542.046	25,007.112
		(2,487.106)	(18,836.656)
Any Earnings in 5^{th} Year After Enrollment[3]	0.728	-0.010	0.507
		(0.062)	(1.430)
Average Quarterly Earnings in 5^{th} Year[3]	2,645.878	112.896	893.101
		(363.400)	(8,772.576)
Observations	53,301	23,655	29,646

Notes: The table lists the estimates of the effect of passing the 20.5 ACT threshold on HOPE eligibility (first stage, Equation 1.1) and the local average treatment effect of HOPE on student outcomes (two stage least squares, Equation 1.2). [1] Estimations for an associate's degree within 3 years are limited to cohorts 2005-2008. [2] Estimations for a bachelor's degree within five years are limited to cohorts 2005-2006. [3] Any earnings and average quarterly earnings in 5^{th} year after initial enrollment are limited to students who exit school prior to the 5^{th} year with or without a degree and cohorts 2005-2006. Robust standard errors, clustered by first ACT score, are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

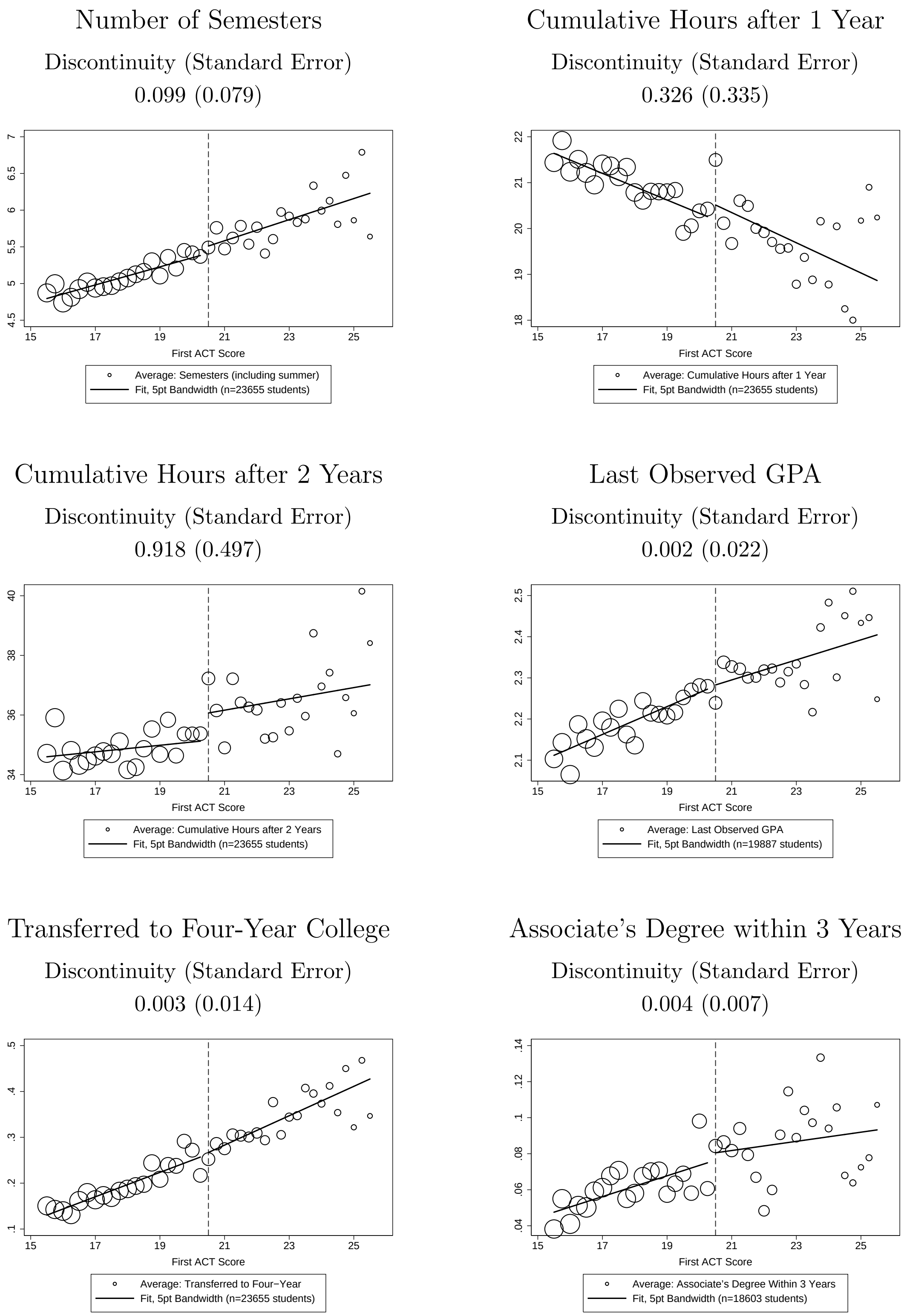
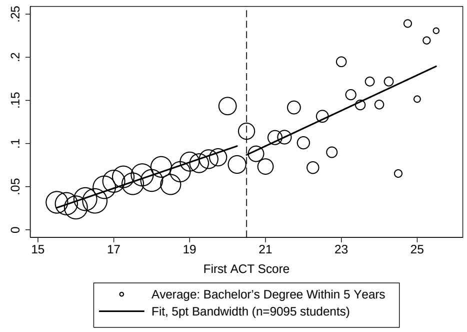


Figure A.3: Intent-to-treat impact of HOPE on student outcomes for low GPA students, reduced form discontinuities

Bachelor's Degree within 5 Years

Discontinuity (Standard Error)

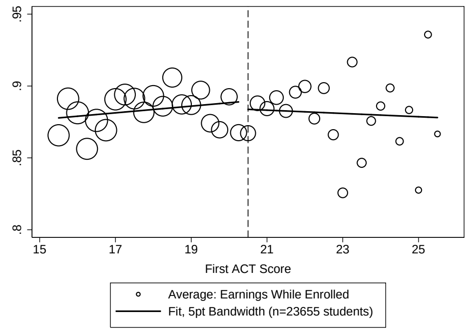
-0.014 (0.014)



Any Earnings While Enrolled

Discontinuity (Standard Error)

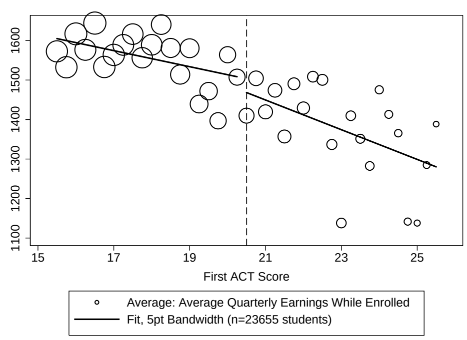
-0.006 (0.008)



Average Quarterly Earnings While Enrolled

Discontinuity (Standard Error)

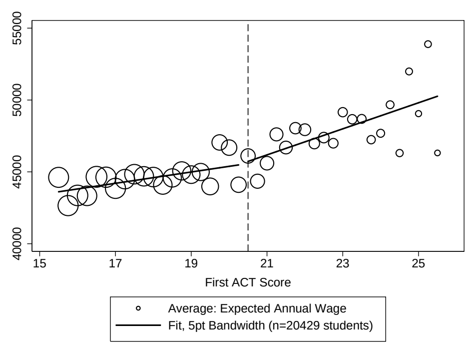
-35.003 (38.008)



Expected Annual Wage

Discontinuity (Standard Error)

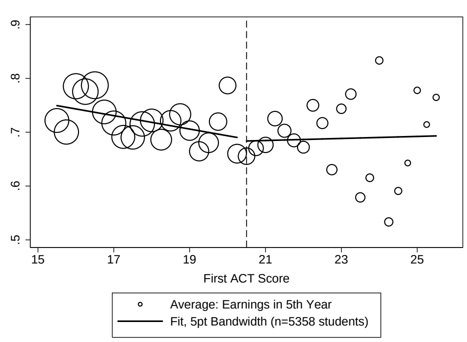
146.089 (682.722)



Any Earnings in 5^{th} Year After Enrollment

Discontinuity (Standard Error)

-0.003 (0.022)



Average Quarterly Earnings in 5^{th} Year

Discontinuity (Standard Error)

40.645 (133.343)

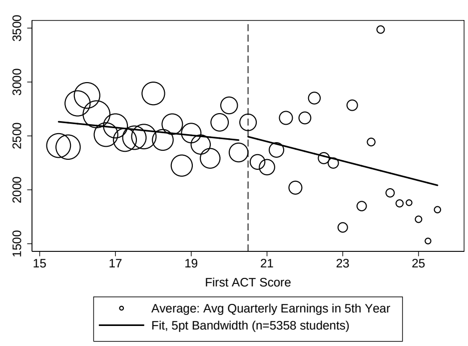


Figure A.3 (continued): Intent-to-treat impact of HOPE on student outcomes for low GPA students, reduced form discontinuities

Table A.4: Robustness checks for outcomes

	(I) Baseline Low GPA	(II) With Controls	(III) Optimal Bandwidth	(IV) Quadratic	(V) Cubic	(VI) Seven-Point Bandwidth	(VII) Two-Point Bandwidth	(VIII) Discontinuity At 18.5 Points	(IX) Discontinuity At 22.5 Points	(X) Baseline High GPA
Number of Semesters	0.362	0.296	0.319	0.192	0.750	0.211	0.605*	0.553	0.137	2.919
	(0.285)	(0.276)	(0.397)	(0.405)	(0.487)	(0.232)	(0.368)	(0.927)	(0.917)	(3.000)
Cumulative Hours after 1 Year	1.197	1.234	2.777**	3.722**	3.032	0.015	2.945*	-6.286	2.078	-11.886
	(1.211)	(1.204)	(1.241)	(1.759)	(2.220)	(0.973)	(1.782)	(4.643)	(2.484)	(14.388)
Cumulative Hours after 2 Years	3.367*	3.505**	3.643*	3.848*	5.442***	1.609	5.112***	-1.721	2.992	-11.486
	(1.749)	(1.729)	(2.037)	(2.091)	(2.029)	(1.361)	(1.915)	(6.819)	(4.527)	(31.237)
Last Observed GPA	0.007	0.059	0.015	0.054	-0.071	-0.052	-0.000	-0.211	0.368	-1.565
	(0.079)	(0.077)	(0.117)	(0.138)	(0.175)	(0.064)	(0.113)	(0.377)	(0.251)	(1.974)
Transferred to Four-Year College	0.012	0.001	0.014	-0.066	0.046	0.029	-0.021	0.280	-0.104	0.570
	(0.049)	(0.049)	(0.041)	(0.108)	(0.141)	(0.035)	(0.109)	(0.210)	(0.166)	(0.552)
Associate's Degree within 3 Years	0.015	0.020	0.027	0.052	0.061	0.011	0.063	-0.112	-0.281*	-0.852
	(0.026)	(0.025)	(0.035)	(0.050)	(0.068)	(0.020)	(0.051)	(0.091)	(0.168)	(0.989)
Bachelor's Degree within 5 Years	-0.044	-0.040	-0.038	-0.030	-0.037	-0.007	-0.059	-0.022	-0.197	1.357
	(0.044)	(0.044)	(0.045)	(0.085)	(0.118)	(0.033)	(0.094)	(0.129)	(0.209)	(1.887)
Any Earnings While Enrolled	-0.021	-0.024	-0.003	0.041	0.056	-0.024	0.040	-0.059	0.015	0.341
	(0.030)	(0.030)	(0.036)	(0.047)	(0.050)	(0.026)	(0.040)	(0.133)	(0.126)	(0.514)
Average Quarterly Earnings While Enrolled	-128.399	-131.366	-96.315	60.790	-148.010	-178.390	-109.582	-1,337.810*	-138.941	-377.972
	(134.476)	(132.558)	(148.105)	(238.857)	(257.146)	(116.052)	(212.214)	(706.934)	(516.019)	(1,322.390)
Expected Annual Wage	542.046	-92.274	845.254	-193.687	-1,062.106	954.642	-992.645	-5,921.074	-3,927.795	25,007.112
	(2,487.106)	(2,453.841)	(1,631.276)	(4,738.596)	(6,304.992)	(1,739.876)	(4,752.273)	(6,254.743)	(6,025.328)	(18,836.656)
Any Earnings 5^{th} Year After Enrollment	-0.010	-0.014	-0.013	-0.057	-0.239	0.012	-0.084	0.050	-0.144	0.507
	(0.062)	(0.061)	(0.073)	(0.110)	(0.188)	(0.050)	(0.111)	(0.852)	(0.446)	(1.430)
Average Quarterly Earnings in 5^{th} Year	112.896	129.443	-28.232	-9.923	-471.818	-102.372	-322.262	-7,155.986	1,269.279	893.101
	(363.400)	(352.077)	(410.127)	(569.128)	(770.905)	(314.143)	(587.394)	(11,860.761)	(2,707.180)	(8,772.576)
Observations	23,655	23,655	35,782	23,655	23,655	31,654	9,365	30,191	15,550	29,646

Notes: The table lists the estimates of the local average treatment effect of HOPE on student outcomes. Column I contains the baseline two stage least squares findings for low GPA students. Columns II-IX also estimate the impact of HOPE on low GPA students, but each column differs from the baseline specification in one respect. Column II adds controls for gender, race, lower-income status, and cohort indicators. Column III uses a triangle kernel (Nichols, 2011) with a plugged in optimal bandwidth that was calculated according to Imbens and Kalyanaraman (2012). Column IV and V use a second order polynomial and a third order polynomial, respectively, for the function of the gap between i's ACT score and the 20.5 threshold. Column VI expands the bandwidth to seven ACT points, and column VII contracts the bandwidth to two ACT points. Columns VIII and IX test for discontinuities in the outcomes at 18.5 and 22.5 ACT points, respectively, which are not associated with HOPE eligibility. Column X contains falsification estimates for the students with high school GPA proxies above 3.0, who should not have significant discontinuities at the 20.5 threshold. Robust standard errors, clustered by first ACT score, are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Heterogeneity by lower-income status

	(I) Baseline Low GPA	(II) Lower-income	(III) Higher-income
Number of Semesters	0.362	0.371	0.294
	(0.285)	(0.314)	(0.421)
Cumulative Hours after 1 Year	1.197	1.072	1.525
	(1.211)	(1.510)	(1.389)
Cumulative Hours after 2 Years	3.367*	3.230*	3.603
	(1.749)	(1.944)	(2.492)
Last Observed GPA	0.007	-0.024	0.040
	(0.079)	(0.116)	(0.143)
Transferred to Four-Year College	0.012	0.084	-0.076
	(0.049)	(0.066)	(0.069)
Associate's Degree within 3 Years[1]	0.015	-0.004	0.040
	(0.026)	(0.036)	(0.041)
Bachelor's Degree within 5 Years[2]	-0.044	-0.038	-0.047
	(0.044)	(0.042)	(0.074)
Any Earnings While Enrolled	-0.021	0.012	-0.065
	(0.030)	(0.044)	(0.041)
Average Quarterly Earnings While Enrolled	-128.399	138.583	-427.015*
	(134.476)	(126.753)	(243.374)
Expected Annual Wage	542.046	2,860.897	-2,500.799
	(2,487.106)	(2,807.866)	(3,870.399)
Any Earnings 5^{th} Year After Enrollment[3]	-0.010	0.184**	-0.233**
	(0.062)	(0.086)	(0.103)
Average Quarterly Earnings in 5^{th} Year[3]	112.896	532.627	-352.872
	(363.400)	(351.230)	(549.352)
Observations	23,655	10,639	13,016

Notes: The table lists the estimates of the local average treatment effect of HOPE on student outcomes by lower-income status. [1] Estimations for an associate's degree within 3 years are limited to cohorts 2005-2008. [2] Estimations for a bachelor's degree within five years are limited to cohorts 2005-2006. [3] Any earnings and average quarterly earnings in 5^{th} year after initial enrollment are limited to students who exit school prior to the 5^{th} year with or without a degree and cohorts 2005-2006. Robust standard errors, clustered by first ACT score, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Omit lower percentiles for the predicted probability of attending a two-year college

	(I) Baseline Low GPA	(II) 5^{th} Percentile Omitted	(III) 10^{th} Percentile Omitted	(IV) 15^{th} Percentile Omitted	(V) 20^{th} Percentile Omitted	(VI) 25^{th} Percentile Omitted	(VII) 30^{th} Percentile Omitted	(VIII) 35^{th} Percentile Omitted	(IX) 40^{th} Percentile Omitted	(X) 45^{th} Percentile Omitted	(XI) 50^{th} Percentile Omitted
Number of Semesters	0.362	0.348	0.369	0.238	0.341	0.229	-0.038	0.170	0.002	0.118	-0.222
	(0.285)	(0.298)	(0.324)	(0.333)	(0.312)	(0.332)	(0.339)	(0.307)	(0.324)	(0.404)	(0.417)
Cumulative Hours after 1 Year	1.197	0.640	0.624	0.652	0.846	0.192	-0.280	-0.180	-0.418	0.214	-0.859
	(1.211)	(1.275)	(1.401)	(1.360)	(1.337)	(1.240)	(1.392)	(1.310)	(1.462)	(1.699)	(1.619)
Cumulative Hours after 2 Years	3.367*	2.921	3.035	2.669	2.958	2.228	0.481	1.337	0.189	0.827	-0.700
	(1.749)	(1.793)	(1.846)	(1.882)	(1.925)	(1.983)	(2.527)	(2.312)	(2.611)	(3.263)	(3.056)
Last Observed GPA	0.007	0.036	0.100	0.099	0.128	0.141	0.077	0.093	0.044	0.051	0.146
	(0.079)	(0.083)	(0.095)	(0.086)	(0.091)	(0.087)	(0.078)	(0.091)	(0.099)	(0.107)	(0.137)
Transferred to Four-Year College	0.012	0.016	0.031	0.014	0.025	0.016	-0.010	-0.009	-0.007	0.017	-0.021
	(0.049)	(0.048)	(0.048)	(0.051)	(0.053)	(0.057)	(0.057)	(0.055)	(0.053)	(0.053)	(0.048)
Associate's Degree within 3 Years[1]	0.015	0.014	0.017	0.013	0.003	-0.002	-0.013	-0.010	-0.014	-0.021	-0.037
	(0.026)	(0.028)	(0.030)	(0.032)	(0.034)	(0.037)	(0.037)	(0.039)	(0.038)	(0.043)	(0.043)
Bachelor's Degree within 5 Years[2]	-0.044	-0.036	-0.027	-0.022	-0.001	0.012	-0.009	0.009	0.003	0.017	-0.008
	(0.044)	(0.040)	(0.039)	(0.033)	(0.033)	(0.036)	(0.041)	(0.040)	(0.036)	(0.039)	(0.040)
Any Earnings While Enrolled	-0.021	-0.020	-0.028	-0.031	-0.029	-0.024	-0.022	-0.036	-0.036	-0.049	-0.053
	(0.030)	(0.033)	(0.035)	(0.035)	(0.036)	(0.037)	(0.037)	(0.035)	(0.036)	(0.042)	(0.049)
Average Quarterly Earnings While Enrolled	-128.399	-163.997	-192.449	-197.297	-108.183	-53.716	-24.513	-27.408	-31.241	-116.243	-117.766
	(134.476)	(132.842)	(125.392)	(135.609)	(144.909)	(140.264)	(162.843)	(146.913)	(158.206)	(191.638)	(182.812)
Expected Annual Wage	542.046	781.830	1,814.425	1,182.744	1,418.409	1,529.155	1,374.499	2,408.629	1,754.725	995.247	-52.942
	(2,487.106)	(2,599.171)	(2,511.649)	(2,639.682)	(2,716.456)	(3,033.112)	(3,141.193)	(2,952.959)	(3,275.522)	(3,375.133)	(3,626.607)
Any Earnings 5^{th} Year After Enrollment[3]	-0.010	0.005	0.038	0.009	0.039	0.039	0.006	-0.023	-0.041	-0.093	-0.137
	(0.062)	(0.060)	(0.068)	(0.067)	(0.064)	(0.067)	(0.071)	(0.067)	(0.074)	(0.092)	(0.104)
Average Quarterly Earnings in 5^{th} Year[3]	112.896	283.676	406.429	406.023	684.586*	767.430**	704.727**	654.139*	468.872	328.820	200.891
	(363.400)	(386.413)	(401.055)	(391.422)	(360.422)	(373.390)	(346.033)	(372.441)	(404.560)	(475.019)	(506.729)
Observations	23,655	22,472	21,289	20,106	18,924	17,741	16,558	15,375	14,193	13,010	11,827

Notes: The table lists the estimates of the local average impact of HOPE on student outcomes. Column I contains the two stage least squares baseline findings. Columns II-XI omit students with the lowest predicted probability of attending a two-year school in ascending order. [1] Estimations for an associate's degree within 3 years are limited to cohorts 2005-2008. [2] Estimations for a bachelor's degree within five years are limited to cohorts 2005-2006. [3] Any earnings and average quarterly earnings in 5^{th} year after initial enrollment are limited to students who exit school prior to the 5^{th} year with or without a degree and cohorts 2005-2006. Robust standard errors, clustered by first ACT score, are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Below I present a graphical analysis for the discontinuity in the percentage of black students at the 20.5 threshold. I also show that the baseline results are robust to varying bandwidths and discuss the heterogeneity of the results by gender and race.

A.1 Test for Discontinuity in Control Variables

Discontinuity estimates for control variables are discussed in Section 1.4.5 and presented in Table A.2. The majority of the estimates are small and insignificant for the analytic sample with one exception. The percentage of black students increases at the HOPE threshold. The graphical analysis for this control variable is presented in Figure A.4. Similar to Figure A.3, the mean value of each outcome for students with the same ACT score is plotted for all ACT scores within five points of the 20.5 threshold. The size of the circles in the scatter plots increase proportionate to the number of students who received a specific ACT score. Discontinuities are estimated by replacing the dependent variable with the control variable in the reduced form equation of Equation 1.2. The plotted fitted lines on either side of the threshold are the predicted values from the estimation, and the discontinuity or reduced form intent-to-treat effect is listed. The percentage of black students decreases as ACT score increases, but there does not visually look like there is a discontinuity in the percentage of black students at the threshold. Moreover, it appears that the relationship describing the gap between i's ACT score and the 20.5 threshold is more likely to be a quadratic rather than a linear function. I estimate the discontinuity in the percentage of black students using both a quadratic and a cubic rather than using a linear specification in Equation 1.2, and the percentage of black students no longer registers as significant in either case. Therefore, it is unlikely that the significant discontinuity in the percentage of black students found in Table A.2 is due to omitted variables that may threaten internal validity of the results. In addition, I demonstrate that the baseline findings are robust to including all controls in Section 1.6 in the main text.

A.2 Robustness Checks for Outcomes at Varying Bandwidths

I demonstrate the robustness of the baseline results by using various bandwidths in Table A.7. Column I contains the baseline two stage least square findings for low GPA students similar to those reported in Column I in Table A.4. Columns II through VIII also estimate the local impact of HOPE on low GPA students, but a different bandwidth is used in each column. In the main text, I include the optimal bandwidth, a seven-point bandwidth, and a two-point bandwidth in the robustness checks in Table A.4. Here, I also report these estimates, but I demonstrate that the baseline findings are also robust to using a one-point, three-point, four-point, and six-point bandwidth. Cumulative hours after two years remains significant when using a three-point and four-point bandwidth, but it is not significant when using a one-point or six-point bandwidth. Cumulative hours after one year continues to exhibit some inconsistent significant discontinuities, but the coefficient for this outcome remains insignificant for many of the different bandwidths. All of the other outcome variables that are insignificant in the baseline results remain consistently insignificant when using the various bandwidths which demonstrates the robustness of the overall zero local impact of HOPE eligibility on the student outcomes.[1]

A.3 Heterogeneity by Gender and Race

In Table A.8, I examine whether the baseline findings are heterogeneous across gender, race, and parents' income. Similar to Table A.5 in the main text, Column I lists the baseline results for low GPA students and Columns II and III list the results for lower-income and higher-income students, respectively. Column IV-VII present estimates for a subsample of low GPA students based on gender and race. As demonstrated in Column IV, cumulative hours after two years remains significant for males, similar to lower-income students, but the point estimate remains modest for this subgroup. All of the other student outcomes of

[1] Average quarterly earnings while enrolled is significant at the ten percent level when a one-point bandwidth is used, but this significant coefficient represents about one percent of the table and may be significant by chance.

interest remain insignificant, similar to the baseline findings, for males, females, and white students. However, there is suggestive evidence of persistence and degree effects on non-white students as can be seen in Column VII in Table A.8. It appears that HOPE eligibility might have a larger and positive impact on non-white students in terms of transferring to a four-year university, earning an associate's degree within three years, and earning a bachelor's degree within five years.

Figure A.5 consists of a graphical analysis for each outcome variable that exhibits a significant discontinuity for non-white students in Table A.8. Similar to Figure A.3, the plotted fitted lines on both sides of the threshold are the predicted outcome values from estimating the reduced form equation except the intent-to-treat estimations are limited to the subsample of non-white students. Transferring to a four-year college and earning an associate's degree within three years increases in ACT score while earning a bachelor's degree within five years and average quarterly earnings in the fifth year after initial enrollment seem relatively constant across ACT scores for non-white students. All four of the graphs exhibit some noise in the data above the 20.5 ACT threshold which is likely due to a small sample size. In addition, the suggestive evidence of persistence and degree effects on non-white students is weakened as there visually does not appear to be a large significant discontinuity in any of the outcome variables in the graphical analyses, especially after considering the scales on the vertical axes.

In addition, I test the robustness of the baseline results for non-white students to various specifications and bandwidths in Table A.9. Column I lists the baseline results for low GPA, non-white students which are the same as reported in Column VII of Table A.8. Similar to the baseline robustness checks in Table A.4, Columns II through X all differ from the baseline specification in one respect (e.g. adding controls, changing the size of the bandwidth, or using a quadratic or cubic rather than a linear function of the gap between i's ACT score and the 20.5 threshold for $f(\cdot)$ and $g(\cdot)$ in Equation 1.2). The coefficients for transferring to a four-year college, earning an associate's degree within three years, earning a bachelor's degree within five years, and average quarterly earnings in the fifth year all remain significant when controls are added in Column II and when a seven-point rather than five-point bandwidth is used in Column VI. However, all of these outcomes are insignificant when a two-point bandwidth is used in Column VII and when a quadratic and cubic function is used for the gap between i's ACT score and the 20.5 threshold in Column IV and V, respectively. Therefore, while the results in Table A.8 suggest that HOPE eligibility has positive persistence and degree effects on non-white students, these results are not strong enough to ascribe the discontinuities to a causal effect of HOPE rather than noise and are not robust to different specifications and bandwidths.

Black

Discontinuity (Standard Error)
0.033 (0.012)

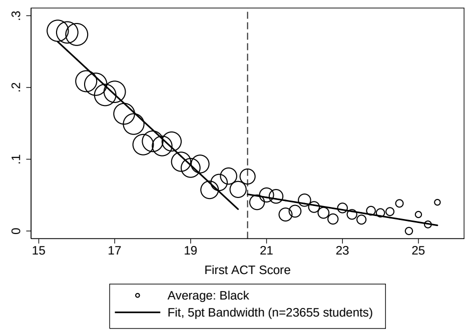


Figure A.4: Intent-to-treat impact of HOPE on controls for low GPA students, reduced form discontinuities

Table A.7: Robustness checks for outcomes at varying bandwidths

	(I) Baseline Low GPA	(II) Optimal Bandwidth	(III) One-Point Bandwidth	(IV) Two-Point Bandwidth	(V) Three-Point Bandwidth	(VI) Four-Point Bandwidth	(VII) Six-Point Bandwidth	(VIII) Seven-Point Bandwidth
Number of Semesters	0.362	0.319	0.215	0.605*	0.288	0.334	0.344	0.211
	(0.285)	(0.397)	(0.429)	(0.368)	(0.309)	(0.287)	(0.249)	(0.232)
Cumulative Hours after 1 Year	1.197	2.777**	0.621	2.945*	2.776*	1.913	0.485	0.015
	(1.211)	(1.241)	(1.864)	(1.782)	(1.469)	(1.301)	(1.077)	(0.973)
Cumulative Hours after 2 Years	3.367*	3.643*	3.015	5.112***	3.827**	3.207*	2.252	1.609
	(1.749)	(2.037)	(1.975)	(1.915)	(1.866)	(1.761)	(1.506)	(1.361)
Last Observed GPA	0.007	0.015	-0.048	-0.000	0.093	0.035	-0.032	-0.052
	(0.079)	(0.117)	(0.120)	(0.113)	(0.114)	(0.087)	(0.069)	(0.064)
Transferred to Four-Year College	0.012	0.014	0.088	-0.021	-0.046	-0.006	0.035	0.029
	(0.049)	(0.041)	(0.123)	(0.109)	(0.077)	(0.058)	(0.042)	(0.035)
Associate's Degree within 3 Years[1]	0.015	0.027	0.037	0.063	0.030	0.020	-0.000	0.011
	(0.026)	(0.035)	(0.069)	(0.051)	(0.039)	(0.031)	(0.023)	(0.020)
Bachelor's Degree within 5 Years[2]	-0.044	-0.038	-0.025	-0.059	-0.049	-0.028	-0.036	-0.007
	(0.044)	(0.045)	(0.115)	(0.094)	(0.066)	(0.052)	(0.037)	(0.033)
Any Earnings While Enrolled	-0.021	-0.003	-0.007	0.040	0.031	-0.003	-0.009	-0.024
	(0.030)	(0.036)	(0.052)	(0.040)	(0.039)	(0.033)	(0.026)	(0.026)
Average Quarterly Earnings While Enrolled	-128.399	-96.315	-361.745*	-109.582	55.940	-79.951	-125.213	-178.390
	(134.476)	(148.105)	(197.733)	(212.214)	(187.467)	(150.514)	(119.945)	(116.052)
Expected Annual Wage	542.046	845.254	-1,426.827	-992.645	145.379	1,844.997	1,158.492	954.642
	(2,487.106)	(1,631.276)	(6,854.286)	(4,752.273)	(3,501.369)	(2,857.228)	(2,077.843)	(1,739.876)
Any Earnings 5^{th} Year After Enrollment[3]	-0.010	-0.013	-0.140	-0.084	-0.071	0.016	0.003	0.012
	(0.062)	(0.073)	(0.180)	(0.111)	(0.088)	(0.078)	(0.054)	(0.050)
Average Quarterly Earnings in 5^{th} Year[3]	112.896	-28.232	-398.090	-322.262	60.670	21.423	48.968	-102.372
	(363.400)	(410.127)	(827.252)	(587.394)	(428.130)	(390.830)	(346.577)	(314.143)
Observations	23,655	35,782	4,866	9,365	14,087	18,997	28,000	31,654

Notes: The table lists the estimates of the local average treatment effect of HOPE on student outcomes. Column I contains the baseline two stage least squares findings for low GPA students. Columns II-VIII also estimate the local average treatment effect of HOPE on low GPA students, but a different bandwidth is used in each column. Column II uses a triangle kernel (Nichols, 2011) with a plugged in optimal bandwidth that was calculated according to Imbens and Kalyanaraman (2012) for each outcome of interest. Column III through VI use a bandwidth of one point to seven points respectively. [1] Estimations for an associate's degree within 3 years are limited to cohorts 2005-2008. [2] Estimations for a bachelor's degree within five years are limited to cohorts 2005-2006. [3] Any earnings and average quarterly earnings in 5^{th} year after initial enrollment are limited to students who exit school prior to the 5^{th} year with or without a degree and cohorts 2005-2006. Robust standard errors, clustered by first ACT score, are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Heterogeneity by gender, race, and lower-income status

	(I) Baseline Low GPA	(II) Lower-income	(III) Higher-income	(IV) Male	(V) Female	(VI) White	(VII) Non-white
Number of Semesters	0.362	0.371	0.294	0.483	0.222	0.286	0.919
	(0.285)	(0.314)	(0.421)	(0.363)	(0.593)	(0.272)	(0.835)
Cumulative Hours after 1 Year	1.197	1.072	1.525	1.229	1.064	1.424	1.200
	(1.211)	(1.510)	(1.389)	(1.080)	(2.300)	(1.474)	(1.495)
Cumulative Hours after 2 Years	3.367*	3.230*	3.603	3.727**	2.719	3.435	5.085
	(1.749)	(1.944)	(2.492)	(1.617)	(3.302)	(2.178)	(3.895)
Last Observed GPA	0.007	-0.024	0.040	0.158	-0.157	0.041	0.076
	(0.079)	(0.116)	(0.143)	(0.102)	(0.168)	(0.085)	(0.219)
Transferred to Four-Year College	0.012	0.084	-0.076	-0.014	0.050	-0.034	0.206**
	(0.049)	(0.066)	(0.069)	(0.056)	(0.076)	(0.050)	(0.087)
Associate's Degree within 3 Years[1]	0.015	-0.004	0.040	0.011	0.024	0.008	0.104**
	(0.026)	(0.036)	(0.041)	(0.032)	(0.049)	(0.029)	(0.052)
Bachelor's Degree within 5 Years[2]	-0.044	-0.038	-0.047	-0.031	-0.062	-0.078	0.204**
	(0.044)	(0.042)	(0.074)	(0.037)	(0.086)	(0.051)	(0.095)
Any Earnings While Enrolled	-0.021	0.012	-0.065	0.007	-0.059	-0.020	-0.024
	(0.030)	(0.044)	(0.041)	(0.037)	(0.054)	(0.029)	(0.082)
Average Quarterly Earnings While Enrolled	-128.399	138.583	-427.015*	-113.653	-149.060	-98.232	-260.431
	(134.476)	(126.753)	(243.374)	(183.797)	(252.475)	(161.877)	(211.974)
Expected Annual Wage	542.046	2,860.897	-2,500.799	832.206	128.372	-211.255	3,437.752
	(2,487.106)	(2,807.866)	(3,870.399)	(2,696.458)	(3,315.112)	(2,449.387)	(3,866.150)
Any Earnings 5^{th} Year After Enrollment[3]	-0.010	0.184**	-0.233**	-0.115	0.144	0.017	-0.207
	(0.062)	(0.086)	(0.103)	(0.089)	(0.116)	(0.057)	(0.166)
Average Quarterly Earnings in 5^{th} Year[3]	112.896	532.627	-352.872	-50.402	354.217	444.502	-1,804.277**
	(363.400)	(351.230)	(549.352)	(471.138)	(619.583)	(340.017)	(833.411)
Observations	23,655	10,639	13,016	11,813	11,842	18,850	4,805

Notes: The table lists the estimates of the local average treatment effect of HOPE on student outcomes by gender, race, and lower-income status. [1] Estimations for an associate's degree within 3 years are limited to cohorts 2005-2008. [2] Estimations for a bachelor's degree within five years are limited to cohorts 2005-2006. [3] Any earnings and average quarterly earnings in 5^{th} year after initial enrollment are limited to students who exit school prior to the 5^{th} year with or without a degree and cohorts 2005-2006. Robust standard errors, clustered by first ACT score, are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

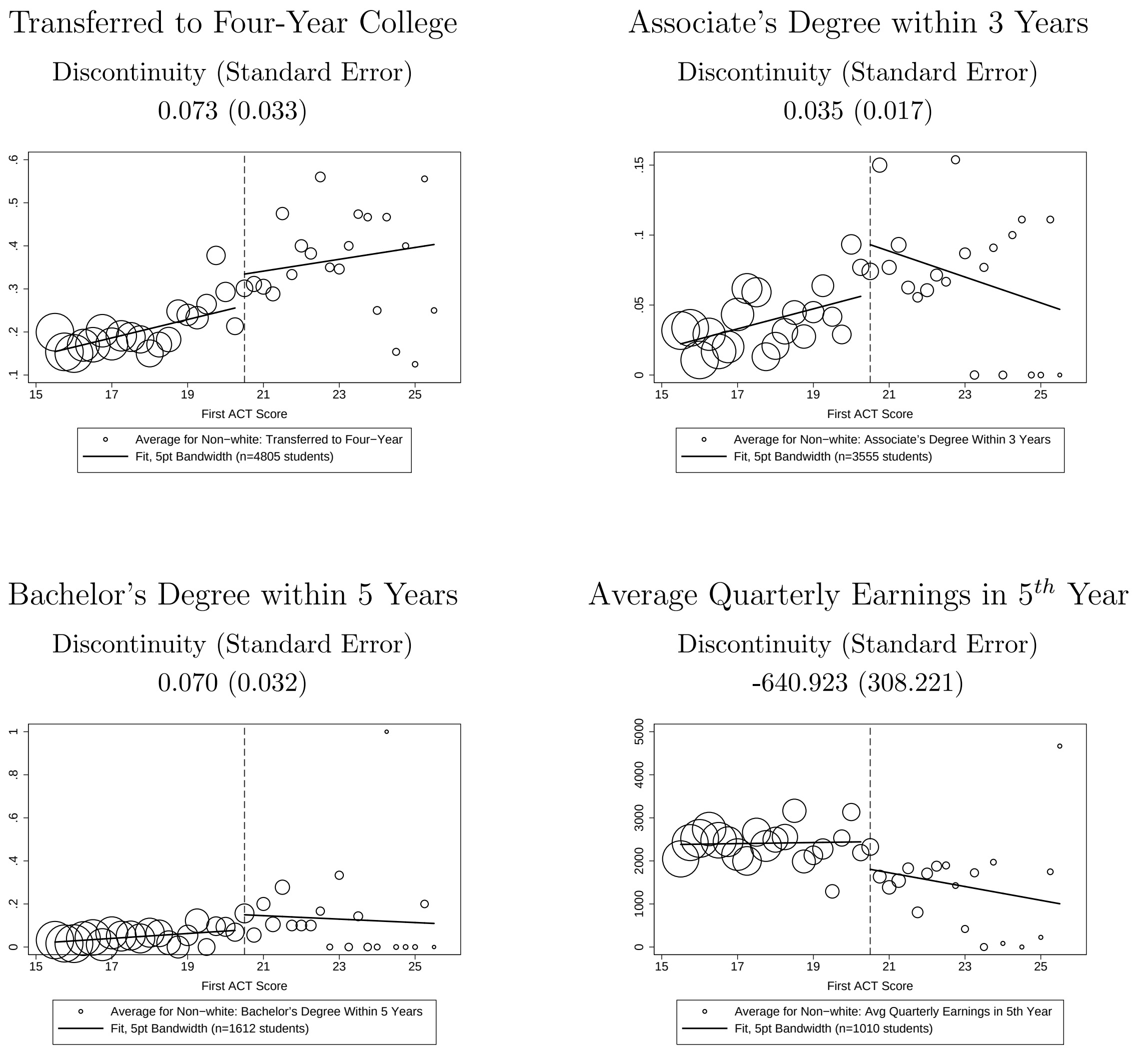


Figure A.5: Intent-to-treat impact of HOPE on student outcomes for non-white low GPA students, reduced form discontinuities

Table A.9: Robustness checks for outcomes for non-white students

	(I) Baseline Non-white Low GPA	(II) With Controls	(III) Optimal Bandwidth	(IV) Quadratic	(V) Cubic	(VI) Seven-Point Bandwidth	(VII) Two-Point Bandwidth	(VIII) Discontinuity At 18.5 Points	(IX) Discontinuity At 22.5 Points	(X) Baseline High GPA
Number of Semesters	0.919	0.869	0.776	0.273	0.863	0.759	-0.007	2.249	2.575	10.430
	(0.835)	(0.793)	(0.834)	(1.364)	(1.530)	(0.686)	(1.468)	(3.031)	(1.996)	(14.017)
Cumulative Hours after 1 Year	1.200	1.176	1.580	1.105	3.455	1.239	1.928	-0.545	2.193	60.407
	(1.495)	(1.492)	(3.172)	(3.255)	(4.878)	(1.308)	(3.579)	(8.439)	(6.605)	(65.445)
Cumulative Hours after 2 Years	5.085	5.035	2.170	-2.565	-1.804	3.997	-2.571	0.167	10.627	86.080
	(3.895)	(3.735)	(4.802)	(7.746)	(10.643)	(3.168)	(8.140)	(17.130)	(11.322)	(96.094)
Last Observed GPA	0.076	0.056	0.050	-0.116	-0.599	0.180	-0.333	-0.420	-0.043	6.002
	(0.219)	(0.212)	(0.274)	(0.351)	(0.460)	(0.173)	(0.336)	(0.746)	(0.770)	(10.067)
Transferred to Four-Year College	0.206**	0.207**	0.147*	-0.131	-0.145	0.146**	-0.127	0.200	0.089	1.084
	(0.087)	(0.088)	(0.087)	(0.167)	(0.303)	(0.061)	(0.194)	(0.378)	(0.301)	(1.656)
Associate's Degree within 3 Years	0.104**	0.100**	0.102	0.097	0.055	0.103***	0.082	0.099	0.035	-10.642
	(0.052)	(0.050)	(0.084)	(0.112)	(0.137)	(0.039)	(0.109)	(0.194)	(0.173)	(82.367)
Bachelor's Degree within 5 Years	0.204**	0.201**	0.214	0.287	0.160	0.170**	0.143	-0.335	0.288	-1.989
	(0.095)	(0.094)	(0.133)	(0.187)	(0.175)	(0.075)	(0.155)	(0.394)	(1.014)	(4.073)
Any Earnings While Enrolled	-0.024	-0.024	-0.040	-0.068	-0.000	-0.047	-0.021	-0.072	0.156	0.446
	(0.082)	(0.085)	(0.087)	(0.153)	(0.179)	(0.068)	(0.140)	(0.363)	(0.185)	(1.203)
Average Quarterly Earnings While Enrolled	-260.431	-287.050	-224.050	-45.974	422.995	-310.751*	275.082	-357.224	472.916	-2,271.017
	(211.974)	(240.887)	(286.740)	(285.679)	(539.926)	(174.798)	(359.089)	(1,297.157)	(815.330)	(3,515.245)
Expected Annual Wage	3,437.752	3,624.359	2,591.573	-4,989.138	-15,765.358	2,765.414	-10,913.492	-6,491.291	16,266.794	-176,867.248
	(3,866.150)	(3,738.076)	(3,497.917)	(6,548.839)	(14,049.066)	(2,891.484)	(7,251.086)	(14,423.408)	(10,114.508)	(617,103.407)
Any Earnings 5^{th} Year After Enrollment	-0.207	-0.204	-0.184	0.245	0.040	-0.271**	0.168	3.371	-4.657	-1.462
	(0.166)	(0.158)	(0.216)	(0.341)	(0.432)	(0.134)	(0.346)	(12.832)	(32.695)	(2.541)
Average Quarterly Earnings in 5^{th} Year	-1,804.277**	-1,784.521**	-1,664.228	-816.755	-2,081.342	-1,943.794***	-1,218.847	18,610.190	-19,239.314	-13,760.000
	(833.411)	(852.769)	(1,113.193)	(1,329.129)	(1,570.157)	(695.665)	(1,560.230)	(67,104.730)	(129,943.797)	(15,069.065)
Observations	4,805	4,805	11,061	4,805	4,805	8,485	1,327	8,351	2,323	3,845

Notes: The table lists the estimates of the local average treatment effect of HOPE on student outcomes. Column I contains the baseline two stage least squares findings for non-white students. Columns II-X differ from the baseline specification in one respect. Column II adds controls for gender, race, lower-income status, and cohort indicators. Column III uses a triangle kernel (Nichols, 2011) with a plugged in optimal bandwidth that was calculated according to Imbens and Kalyanaraman (2012). Column IV and V use a second order polynomial and a third order polynomial, respectively, for the function of the gap between i's ACT score and the 20.5 threshold. Column VI expands the bandwidth to 7 ACT points, and column VII contracts the bandwidth to 2 ACT points. Columns VIII and IX test for discontinuities in the outcomes at 18.5 and 22.5 ACT points, respectively, which are not associated with HOPE eligibility. Column X contains falsification estimates for the students with high school GPA proxies above 3.0, who should not have significant discontinuities at the 20.5 threshold. Robust standard errors, clustered by first ACT score, are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

The Incidence of Financial Aid: How Colleges Respond to Merit Scholarship Programs

Table B.1: Merit aid program characteristics

(I) State	(II) Year Program Introduced	(III) Program Name	(IV) 2014-2015 Initial Eligibility Requirements	(V) Revenue Source	(VI) Private Colleges
Florida	1997	Florida Bright Futures Scholarship Program	3.0-3.5 GPA and 26-29 ACT	Lottery	Yes
Georgia	1993	Georgia HOPE Scholarship	3.0 GPA	Lottery	Yes
Kentucky	1999	Kentucky Educational Excellence Scholarship	Combination of GPA (starting at 2.5) and ACT (starting at 15)[1]	Lottery	Yes
Louisiana	1998	Louisiana TOPS Scholarship	2.5-3.0 GPA and 20-27 ACT	Tobacco Settlement Funds	Yes
Michigan	2000	Michigan Merit Award and Promise Scholarship	Level 2 or above on Michigan Merit Exam (MME)[2]	Tobacco Settlement Funds	Yes
Nevada	2000	Nevada Millennium Scholarship	3.25 GPA and pass all sections of proficiency examination	Tobacco Settlement Funds	Yes
New Mexico	1997	New Mexico Lottery Success Scholarship	2.5 GPA first semester at public college or university	Lottery	No
South Carolina	1998	South Carolina LIFE (and HOPE) Scholarship	Two of the following: 3.0 GPA, 24 ACT, or top 30% of class	Lottery	Yes
Tennessee	2004	Tennessee HOPE Scholarship	3.0 GPA or 21 ACT	Lottery	Yes
West Virginia	2002	West Virginia PROMISE Scholarship	3.0 GPA and 22 ACT	Lottery	Yes

Notes: The table lists merit aid program characteristics for states with programs that are funded through either lottery revenues or tobacco settlement funds. Program information was collected from state agency websites. Column VI indicates whether students can use their merit aid scholarship at in-state private institutions. [1]Kentucky's scholarship amounts depend on students' GPA each year in high school, and bonus awards vary by ACT score. [2]The Promise scholarship, which replaced the Merit Award, was implemented in the 2007-2008 academic year; however, the Promise scholarship was discontinued after three years. To receive the Merit Award, students had to meet or exceed standards on all four sections of the Michigan Educational Assessment Program (MEAP) which was replaced with the MME.

Table B.2: Summary statistics for state control variables in 2009

	(I) Merit States	(II) Non-merit States
Unemployment rate	9.87	8.11
	(2.23)	(1.73)
Personal income per capita	34,745.73	40,950.72
	(2,152.62)	(7,188.60)
Real GDP per capita	38,640.35	49,599.28
	(5,035.11)	(20,427.86)
Percent of population 18-24 with high school degree	81.34	84.92
	(2.77)	(2.88)
Observations	10	41

Notes: Column I and II list the mean of state control variables in 2009 for merit and non-merit states, respectively. All financial variables are in 2010 dollars. Standard deviations are in parentheses.

Table B.3: Summary statistics for institutional control variables in the 2009-2010 academic year

	(I) Merit States	(II) Non-merit States
Percent of undergraduates women	56.16	55.51
	(14.00)	(16.68)
Percent of undergraduates non-white	38.80	36.43
	(27.22)	(24.49)
Log of FTE	7.89	7.83
	(1.36)	(1.38)
Observations	270	1,238

Notes: Column I and II list the mean of institutional control variables in the 2009-2010 academic year for colleges in merit and non-merit states, respectively. Standard deviations are in parentheses.

Table B.4: Summary statistics for outcomes of interest per FTE for public colleges in academic years 1986-1987 to 2009-2010

	(I) Merit States	(II) Non-merit States
In-state tuition	3,832.08	4,258.91
	(1,765.64)	(2,005.61)
Out-of-state tuition	10,357.01	10,792.64
	(4,416.13)	(4,593.85)
Revenue from private gifts, grants, and contracts per FTE	1,111.83	1,137.02
	(3,054.44)	(3,497.81)
Instructional expenditures per FTE	8,278.29	8,783.06
	(11,098.37)	(10,606.75)
Student services expenditures per FTE	1,116.78	1,361.35
	(510.46)	(1,601.40)
Gross Pell per FTE	919.92	827.22
	(813.72)	(495.07)
Institutional grants per FTE	723.76	730.04
	(756.07)	(802.51)
Full-time faculty per 100 FTE students	5.97	6.12
	(5.51)	(5.70)
State grants and contracts per FTE	1,017.04	729.39
	(4,391.97)	(2,313.18)
State appropriations per FTE	9,373.99	9,904.03
	(10,240.96)	(11,778.21)
Observations	2,400	9,048

Notes: Column I and II list the mean of the outcomes of interest per FTE for public colleges in academic years 1986-1987 to 2009-2010 for colleges in merit and non-merit states, respectively. All financial variables are in 2010 dollars. Standard deviations are in parentheses.

Table B.5: Summary statistics for outcomes of interest per FTE for private not-for-profit colleges in academic years 1986-1987 to 2009-2010

	(I) Merit States	(II) Non-merit States
Tuition	13,724.61	17,175.28
	(6,749.64)	(7,817.29)
Revenue from private gifts, grants, and contracts per FTE	4,733.75	5,177.01
	(22,933.83)	(8,848.93)
Instructional expenditures per FTE	6,807.49	8,545.55
	(20,182.16)	(8,841.60)
Student services expenditures per FTE	2,481.35	2,600.27
	(6,836.94)	(1,958.60)
Gross Pell per FTE	1,063.61	801.41
	(944.72)	(946.05)
Institutional grants per FTE	3,833.13	4,680.87
	(3,633.68)	(3,633.79)
Full-time faculty per 100 FTE students	5.63	6.11
	(3.53)	(5.67)
Observations	4,080	20,664

Notes: Column I and II list the mean of the outcomes of interest per FTE for private not-for-profit colleges in academic years 1986-1987 to 2009-2010 for colleges in merit and non-merit states, respectively. All financial variables are in 2010 dollars. Standard deviations are in parentheses.

Table B.6: The impact of merit aid programs on outcomes for public colleges

	(I) Mean for Merit States	(II) Effect of Merit Aid
In-state tuition	3551.72	-0.006
		(0.012)
Out-of-state tuition	9433.81	0.034*
		(0.019)
Revenue from private gifts, grants, and contracts	17.58	-0.147
		(0.092)
Instructional expenditures	82.52	0.020
		(0.013)
Student services expenditures	10.80	0.039**
		(0.020)
Gross Pell	6.88	0.051*
		(0.026)
Institutional grants	8.30	0.066
		(0.089)
Full-time faculty per 100 FTE students	6.04	0.039***
		(0.014)
State grants and contracts	6.31	0.906***
		(0.155)
State appropriations	97.46	0.048***
		(0.012)
Observations		11,129

Notes: The table lists the estimates of the effect of merit aid programs on outcomes for public colleges which are estimated using Equation 2.1. Column I lists the mean of outcomes for merit states one year before program implementation, and all dollar values are in millions except for tuition and faculty outcomes. Observations for each outcome vary as only institutions with no more than two years of missing a dependent variable are included in each estimation, and as an example, observations for in-state tuition are listed. Robust standard errors, clustered by institution, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: The impact of merit aid programs on outcomes for private not-for-profit colleges

	(I) Mean for Merit States	(II) Effect of Merit Aid
Tuition	13,487.75	0.014
		(0.013)
Revenue from private gifts, grants, and contracts	7.77	0.033
		(0.041)
Instructional expenditures	16.90	0.039*
		(0.021)
Student services expenditures	3.80	0.039
		(0.034)
Gross Pell	1.38	0.045*
		(0.026)
Institutional grants	7.85	0.064
		(0.041)
Full-time faculty per 100 FTE students	5.23	0.023
		(0.027)
Observations		22,279

Notes: The table lists the estimates of the effect of merit aid programs on outcomes for private not-for-profit colleges which are estimated using Equation 2.1. Column I lists the mean of outcomes for merit states one year before program implementation, and all dollar values are in millions except for tuition and faculty outcomes. Observations for each outcome vary as only institutions with no more than two years of a missing dependent variable are included in each estimation, and as an example, observations for tuition are listed. Robust standard errors, clustered by institution, are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: The impact of merit aid programs on outcomes for public colleges using state expenditures per FTE

	(I) Baseline	(II) Expenditures per FTE
In-state tuition	-0.006	-3.2E-04
	(0.012)	(0.002)
Out-of-state tuition	0.034*	0.002
	(0.019)	(0.003)
Revenue from private gifts, grants, and contracts	-0.147	-0.022*
	(0.092)	(0.012)
Instructional expenditures	0.020	0.004**
	(0.013)	(0.002)
Student services expenditures	0.039**	0.007***
	(0.020)	(0.002)
Gross Pell	0.051*	0.010***
	(0.026)	(0.004)
Institutional grants	0.066	0.014
	(0.089)	(0.013)
Full-time faculty per 100 FTE students	0.039***	0.005***
	(0.014)	(0.002)
State grants and contracts	0.906***	0.153***
	(0.155)	(0.020)
State appropriations	0.048***	0.008***
	(0.012)	(0.001)
Observations	11,129	11,129

Notes: The table lists the estimates of the effect of merit aid programs on outcomes for public colleges which are estimated using Equation 2.1. Column I lists the baseline estimates. Column II lists the estimates using state expenditures on merit aid programs in 2006-2007 as a measure of merit aid rather than a binary indicator which is used in baseline estimations. Observations for each outcome vary as only institutions with no more than two years of a missing dependent variable are included in each estimation, and as an example, observations for in-state tuition are listed. Robust standard errors, clustered by institution, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: The impact of merit aid programs on outcomes for private not-for-profit colleges using state expenditures per FTE

	(I) Baseline	(II) Expenditures per FTE
Tuition	0.014	0.002
	(0.013)	(0.002)
Revenue from private gifts, grants, and contracts	0.033	0.004
	(0.041)	(0.005)
Instructional Expenditures	0.039*	0.005**
	(0.021)	(0.003)
Student Services Expenditures	0.039	0.007
	(0.034)	(0.004)
Gross Pell	0.045*	0.006*
	(0.026)	(0.004)
Institutional Grants	0.064	0.006
	(0.041)	(0.005)
Full-time Faculty per 100 FTE students	0.023	0.001
	(0.027)	(0.004)
Observations	22,279	22,279

Notes: The table lists the estimates of the effect of merit aid programs on outcomes for private not-for-profit colleges which are estimated using Equation 2.1. Column I lists the baseline estimates. Column II lists the estimates using state expenditures on merit aid programs in 2006-2007 as a measure of merit aid rather than a binary indicator which is used in baseline estimations. Observations for each outcome vary as only institutions with no more than two years of a missing dependent variable are included in each estimation, and as an example, observations for tuition are listed. Robust standard errors, clustered by institution, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.1 Sensitivity of Results and Differences in Pre-treatment Trends

In this Appendix, I examine whether results are sensitive to omitting college-specific time trends, and I examine whether there are differences in pre-treatment trends across treatment and control states. Table B.10 presents the estimates for public colleges. Column I contains the baseline results from estimating Equation 2.1 and are the same estimates as those presented in Column II in Table B.6. Column II also contains results from estimating Equation 2.1 except college-specific time trends, $f(t)\delta_i$, are omitted. Column III contains results from the following estimation:

$$\Delta log(Y_{ist}) = \beta_1 EverMerit_s + \Delta W_{st} + \Delta X_{it} + \delta_t + \epsilon_{ist} \tag{B.1}$$

In Equation B.1, the outcomes of interest, W_{st}, and X_{it} are defined as in Equation 2.1 except the first differences of these variables are used in estimations. Also, $EverMerit_s$ is a binary indicator equal to one for all states that implemented merit aid programs. Lastly, post-merit observations for all merit states are omitted from regressions. The coefficient of interest, β_1 describes any pre-existing trends in the outcomes of interest and is presented in Column III.

Overall, the results indicate that estimates are sensitive to including college-specific time trends, and there does appear to be non-parallel pre-treatment trends for merit and non-merit states. Without accounting for school-specific trends, merit aid programs appear to negatively affect in-state tuition and positively affect instructional expenditures and institutional grants, but these impacts do not register as significant when I condition on college-specific time trends. Also, all of these outcomes exhibit different pre-treatment trends for colleges in merit versus non-merit states. For example, institutional grants increases by 20.6% for merit states when college-specific time trends are omitted, but institutional grants for colleges in merit states were on a 2.2% per year higher trend pre-treatment. If the pre-treatment trend continued for these variables, this pre-treatment difference could accumulate and largely explain the significant results found in Column II. Moreover, Column

I demonstrates that merit aid programs no longer significantly impact institutional grants, in-state tuition, or instructional expenditures when college-specific time trends are included. Overall, Table B.10 demonstrates the sensitivity of results to accounting for linear trends at the institution level or higher.

The estimates for private colleges are presented in Table B.11. Overall outcomes do not seem to have differences in pre-treatment trends as much for private versus public colleges. However, the gross amount of Pell grants for colleges in merit states are on a 1% per year lower trend pre-treatment. This lower pre-treatment trend could be putting a downward bias on the estimate that excludes school-specific trends especially as it registers as significant when they are included in Column I. Altogether, both Table B.10 and B.11 demonstrate the importance and need to account for linear trends at the institution level or higher in addition to general time trends.

Table B.10: Robustness check: Pre-treatment trends for public colleges

	(I) Baseline	(II) Omit College Time Trends	(III) Pre-treatment Trend
In-state tuition	-0.006	-0.070***	-0.007**
	(0.012)	(0.017)	(0.003)
Out-of-state tuition	0.034*	0.044**	-0.007**
	(0.019)	(0.018)	(0.003)
Revenue from private gifts, grants, and contracts	-0.147	-0.162	-0.010
	(0.092)	(0.100)	(0.012)
Instructional expenditures	0.020	0.025**	0.004**
	(0.013)	(0.013)	(0.002)
Student services expenditures	0.039**	-0.020	-0.008**
	(0.020)	(0.026)	(0.004)
Gross Pell	0.051*	0.016	0.006
	(0.026)	(0.033)	(0.004)
Institutional grants	0.066	0.206***	0.022**
	(0.089)	(0.076)	(0.010)
Full-time faculty per 100 FTE students	0.039***	0.015	0.005
	(0.014)	(0.019)	(0.003)
State grants and contracts	0.906***	0.478***	-0.022
	(0.155)	(0.120)	(0.013)
State appropriations	0.048***	0.036**	0.001
	(0.012)	(0.016)	(0.002)
Observations	11,129	11,129	9,232

Notes: Column I lists the baseline estimates of the effect of merit aid programs on outcomes for public colleges which are estimated using Equation 2.1. Column II is also estimated using Equation 2.1 except estimations exclude college-specific time trends. Column III lists estimates for pre-treatment trends in outcomes using Equation B.1. Observations for each outcome vary as only institutions with no more than two years of a missing dependent variable are included in each estimation, and as an example, observations for in-state tuition are listed. Robust standard errors, clustered by institution, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: Robustness check: Pre-treatment trends for private not-for-profit colleges

	(I) Baseline	(II) Omit College Time Trends	(III) Pre-treatment Trend
Tuition	0.014	0.038*	0.003
	(0.013)	(0.021)	(0.002)
Revenue from private gifts, grants, and contracts	0.033	-0.080	-0.010
	(0.041)	(0.050)	(0.006)
Instructional Expenditures	0.039*	-0.014	-0.004
	(0.021)	(0.020)	(0.003)
Student Services Expenditures	0.039	-0.026	-0.001
	(0.034)	(0.033)	(0.005)
Gross Pell	0.045*	0.000	-0.010*
	(0.026)	(0.026)	(0.005)
Institutional Grants	0.064	0.076*	0.001
	(0.041)	(0.046)	(0.007)
Full-time Faculty per 100 FTE students	0.023	0.004	0.001
	(0.027)	(0.021)	(0.006)
Observations	22,279	22,279	18,893

Notes: Column I lists the baseline estimates of the effect of merit aid programs on outcomes for private not-for-profit colleges which are estimated using Equation 2.1. Column II is also estimated using Equation 2.1 except estimations exclude college-specific time trends. Column III lists estimates for pre-treatment trends in outcomes using Equation B.1. Observations for each outcome vary as only institutions with no more than two years of a missing dependent variable are included in each estimation, and as an example, observations for tuition are listed. Robust standard errors, clustered by institution, are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

Not Whether, but Where? Pell Grants and College Choices

Table C.1: Summary statistics by college-going status and bandwidth

	(1)	(2)	(3)	(4)
	Full sample		Bandwidth-restricted sample	
College-going?	No (20.3%)	Yes (79.7%)	No (19.5%)	Yes (80.5%)
Pell eligible	0.721	0.437	0.644	0.583
	(0.449)	(0.496)	(0.479)	(0.493)
Potential Pell award (thousands)	3.079	1.660	1.414	1.225
	(2.175)	(2.100)	(1.280)	(1.248)
Mother has a college education	0.246	0.438	0.272	0.388
	(0.431)	(0.496)	(0.445)	(0.487)
Father has a college education	0.143	0.357	0.151	0.265
	(0.350)	(0.479)	(0.358)	(0.441)
Eligible for TN HOPE	0.066	0.413	0.101	0.409
	(0.249)	(0.492)	(0.302)	(0.492)
Eligible for TN ACCESS	0.005	0.01	0.004	0.01
	(0.070)	(0.102)	(0.061)	(0.097)
Eligible for TN GAM	0.003	0.045	0.002	0.025
	(0.058)	(0.207)	(0.047)	(0.155)
White, non-Hispanic	0.642	0.748	0.733	0.739
	(0.479)	(0.434)	(0.442)	(0.439)
Female	0.529	0.561	0.474	0.564
	(0.499)	(0.496)	(0.499)	(0.496)
Best ACT Composite Score	18.9	21.2	19.0	20.8
	(3.1)	(4.3)	(3.1)	(4.0)
First ACT Composite Score	18.1	20.1	18.3	19.7
	(2.9)	(4.2)	(2.9)	(4.0)
Self-reported high school GPA	3.074	3.244	3.074	3.224
	(0.451)	(0.516)	(0.458)	(0.520)
Number of ACT attempts	1.8	2.1	1.8	2.1
	(0.7)	(1.0)	(0.7)	(1.0)
N (students)	28,470	112,046	6,852	28,340

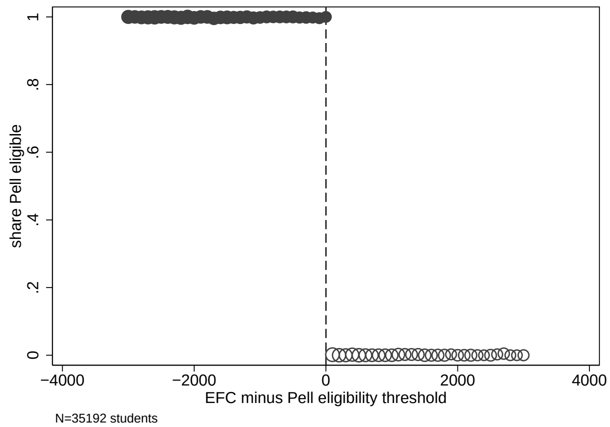
Notes: Authors' calculations. Source data describe four cohorts of Tennessee public high school graduates from the classes of 2006-2009. The bandwidth-restricted subsample includes students whose expected family contribution falls within $3,000 (nominal) of the Pell threshold.

Table C.2: Summary statistics for enrollees, by bandwidth

	(1) Full sample	(2) Bandwidth-restricted
College sector outcomes		
Enrolled in a public TN two-year college	0.334	0.374
	(0.472)	(0.484)
Enrolled in a public TN four-year college	0.461	0.442
	(0.498)	(0.497)
Enrolled in a public out-of-state two-year college	0.014	0.014
	(0.117)	(0.119)
Enrolled in a public out-of-state four-year college	0.047	0.038
	(0.212)	(0.191)
Enrolled in a private four-year college	0.137	0.127
	(0.344)	(0.333)
Enrolled in a private two-year college	0.007	0.005
	(0.082)	(0.068)
College quality outcomes		
ACT Composite 25th Percentile Score (or SAT equivalent)	19.1	18.8
	(2.9)	(2.7)
ACT Composite 75th Percentile Score (or SAT equivalent)	24.3	24.0
	(3.2)	(3.1)
Instructional expenditures per FTE student (thousands)	7.054	6.638
	(6.155)	(5.812)
Student-to-faculty ratio	16.4	16.7
	(5.6)	(5.2)
Graduation rate, total cohort	35.0	33.0
	(20.1)	(19.3)
Published in-state tuition and fees (thousands)	6.501	6.146
	(6.080)	(5.774)
Published out-of-state tuition and fees (thousands)	14.631	14.129
	(4.873)	(4.681)
Tuition and fees conditional on student residency (thousands)	6.978	6.521
	(6.396)	(6.022)
"High research" or "Very high research" Carnegie class	0.341	0.306
	(0.474)	(0.461)
N (students)	112,046	28,340

Notes: Authors' calculations. Source data describe four cohorts of Tennessee public high school graduates from the classes of 2006-2009. The bandwidth-restricted subsample includes students whose expected family contribution falls within $3,000 (nominal) of the Pell threshold.

I. Pell eligibility



II. Full-time Pell award

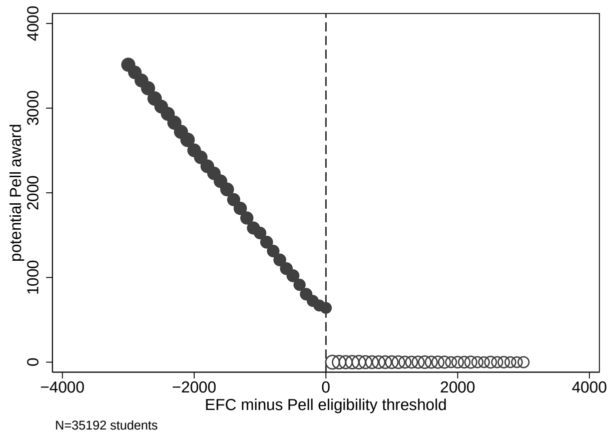


Figure C.1: Pell eligibility (Panel I) and potential grant amounts (Panel II), by distance from EFC thresholds. Scatter points (dots) represent mean eligibility or grant by distance from Pell eligibility thresholds, weighted by the number of students in each $100 EFC bin.

Table C.3: Falsification tests for discontinuities in observables and predicted outcomes

	(1) Coefficient	(2) Robust St. Err.
Student characteristics		
Mother has a college education	0.001	(0.012)
Father has a college education	-0.016**	(0.007)
Eligible for TN HOPE	-0.015	(0.014)
Eligible for TN ACCESS	-4.5E-04	(0.002)
Eligible for TN GAM	0.002	(0.003)
White, non-Hispanic	0.007	(0.008)
Female	0.004	(0.013)
Best ACT composite score	-0.046	(0.086)
First ACT composite score	-0.024	(0.095)
Self-reported high school GPA	0.006	(0.009)
Number of ACT attempts	-0.006	(0.018)
Predicted college outcomes		
Predicted: any college enrollment	-0.006	(0.007)
Predicted: two-year in-state public enrollment	0.002	(0.003)
Predicted: four-year in-state enrollment	-0.007	(0.006)
Predicted: two-year out-of-state public enrollment	8.0E-05	(2.6E-04)
Predicted: four-year out-of-state public enrollment	2.5E-04	(0.001)
Predicted: private four-year enrollment	-0.001	(0.002)
Predicted: private two-year enrollment	1.0E-05	(1.6E-04)

Notes: N = 35,192 students. The table reports estimates of β_1 from Equation 3.1, applied to student observable variables (top panel) or predictions of college enrollment outcomes ($\hat{Y}_i$, bottom panel) based on regressions of Y_i against these same variables. Robust standard errors, clustered by $100 EFC bin, are in parentheses next to each coefficient.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

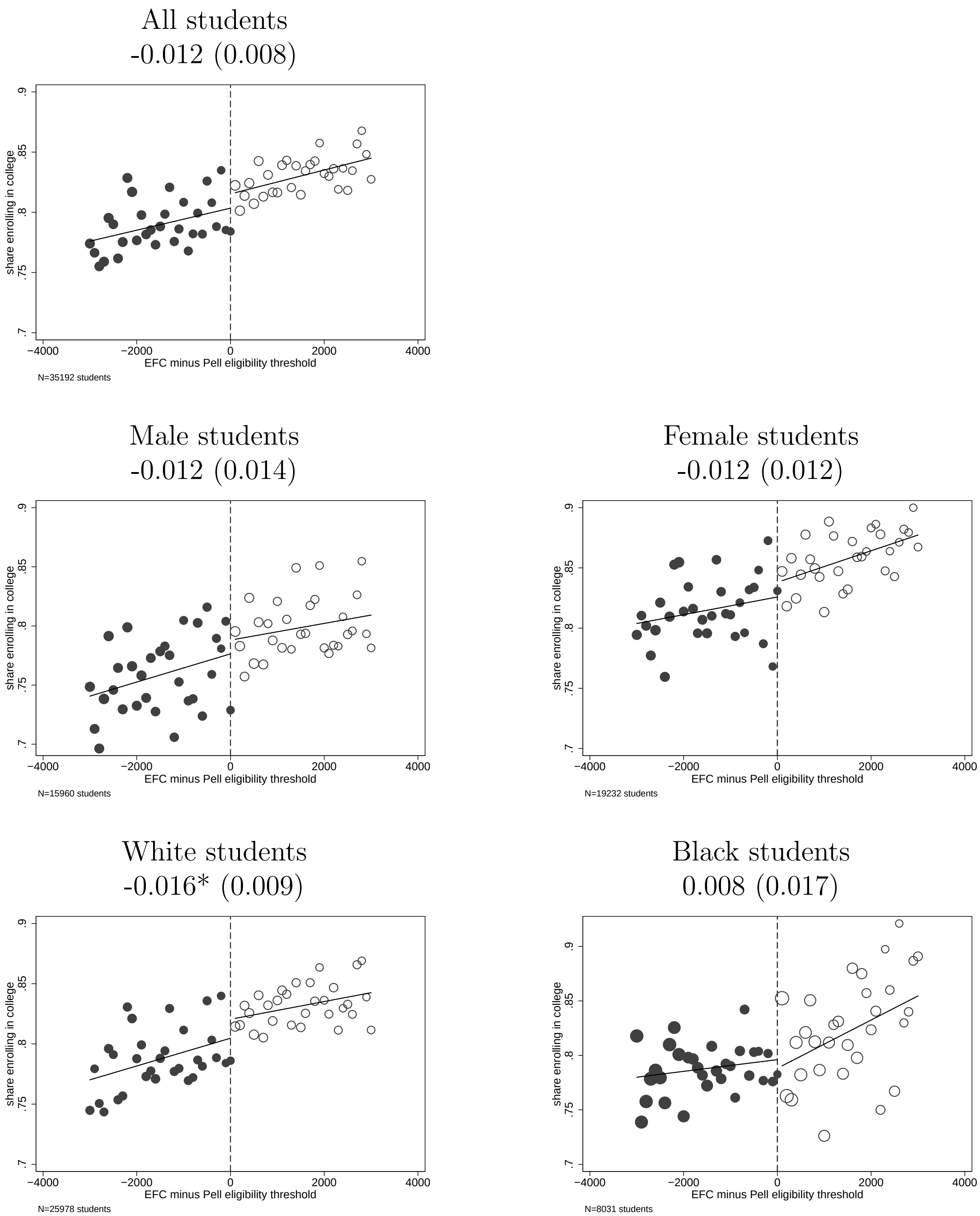


Figure C.2: Regression discontinuity results for any college enrollment. Scatter points (dots) represent mean college enrollment by distance from Pell eligibility thresholds, weighted by the number of students in each $100 EFC bin. Lines fit predictions from Equation 3.1. Point estimates for β_1 are reported below figure headings, with robust standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Regression discontinuity estimates for choice of college sector

	(1) All Students	(2) Male Students	(3) Female Students	(4) White Students	(5) Black Students
Enrolled in a public TN two-year college	0.001	-0.025	0.022	0.001	0.021
	(0.015)	(0.020)	(0.017)	(0.017)	(0.022)
Enrolled in a public TN four-year college	-0.008	0.017	-0.029*	-0.005	-0.022
	(0.016)	(0.020)	(0.017)	(0.015)	(0.030)
Enrolled in a public out-of-state two-year college	0.002	-0.001	0.004	0.004	-0.007
	(0.002)	(0.004)	(0.003)	(0.003)	(0.005)
Enrolled in a public out-of-state four-year college	0.003	-0.007	0.011*	0.005	-0.003
	(0.005)	(0.009)	(0.006)	(0.006)	(0.012)
Enrolled in a private four-year college	0.001	0.013	-0.008	-0.007	0.009
	(0.007)	(0.010)	(0.010)	(0.008)	(0.017)
Enrolled in a private two-year college	0.002	0.004*	1.5E-04	0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.001)	(0.005)
N (students)	28,340	12,359	15,981	20,955	6,388

Notes: The table reports estimates of β_1 from Equation 3.1, applied to mutually exclusive and exhaustive college sector outcomes. Robust standard errors, clustered by $100 EFC bin, are in parentheses next to each coefficient.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

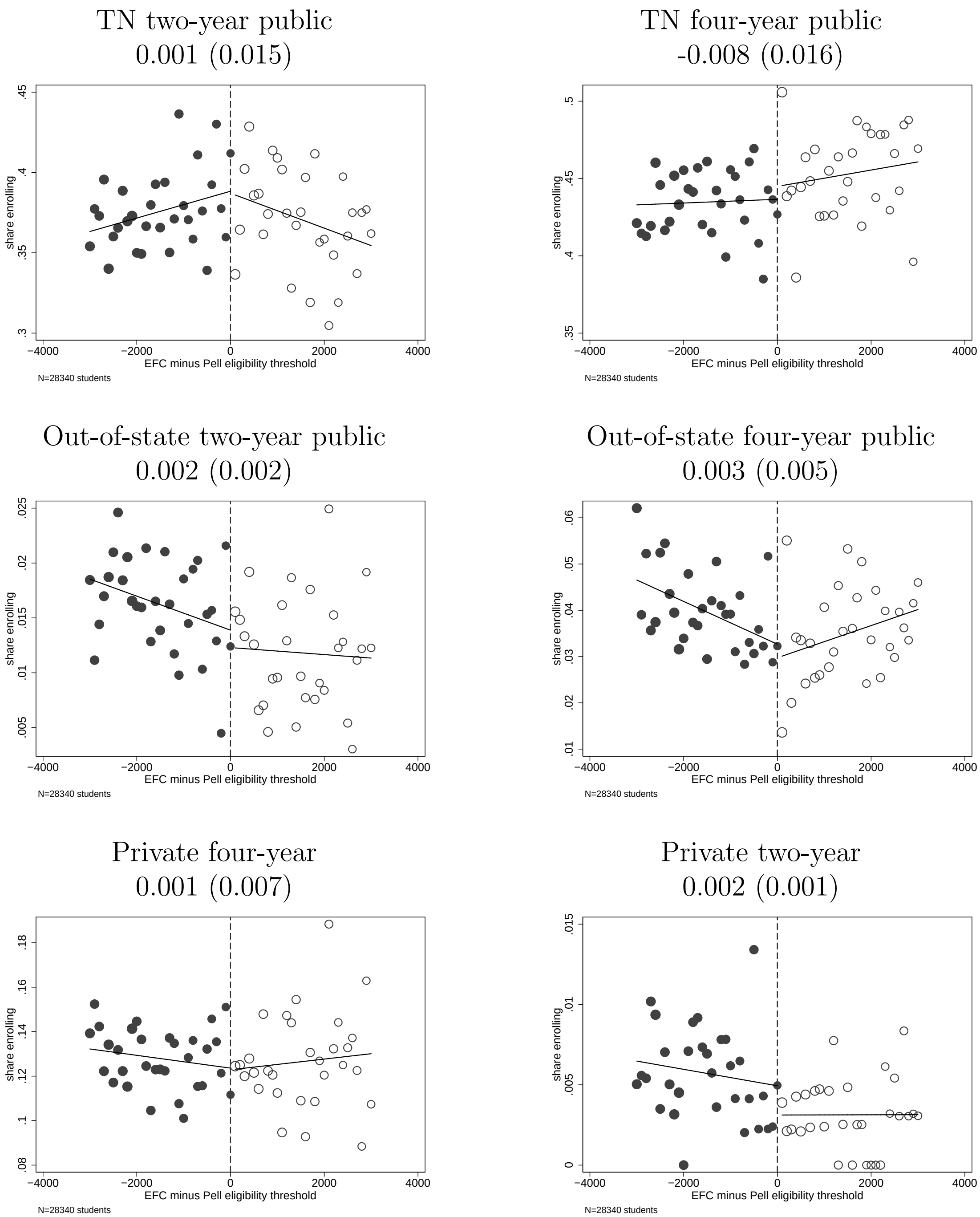


Figure C.3: Regression discontinuity results for choice of college sector. Scatter points (dots) represent college sector outcomes by distance from Pell eligibility thresholds, weighted by the number of students in each $100 EFC bin. Lines fit predictions from Equation 3.1. Point estimates for β_1 are reported below figure headings, with robust standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Regression discontinuity estimates for quality of college chosen

	(1) All Students	(2) Male Students	(3) Female Students	(4) White Students	(5) Black Students
ACT 25th percentile (or SAT equivalent)	0.114	0.217**	0.031	0.083	0.028
	(0.068)	(0.103)	(0.085)	(0.079)	(0.206)
ACT 75th percentile (or SAT equivalent)	0.115	0.314***	-0.043	0.091	0.031
	(0.073)	(0.113)	(0.105)	(0.086)	(0.211)
Instructional expenditures per FTE student	21.4	118.7	-54.5	69.1	-556.7
	(123.9)	(182.5)	(190.3)	(137.8)	(466.3)
Student-to-faculty ratio	-0.224	-0.355*	-0.123	-0.208	-0.109
	(0.135)	(0.195)	(0.153)	(0.143)	(0.261)
Graduation rate	0.814	1.109	0.575	0.614	-0.102
	(0.530)	(0.829)	(0.617)	(0.621)	(1.194)
In-state tuition and fees	162.1	246.5	100.2	78.7	-7.2
	(135.7)	(209.3)	(185.1)	(126.3)	(358.9)
Out-of-state tuition and fees	149.5	260.7	61.5	148.0	-202.4
	(134.6)	(184.5)	(156.6)	(129.3)	(332.5)
Tuition and fees, dependent on students' residency	217.3	200.0	234.0	160.6	-54.3
	(140.1)	(218.0)	(207.8)	(139.8)	(366.2)
"High research" or "Very high research" Carnegie class	0.007	0.009	0.005	0.010	-0.044
	(0.014)	(0.019)	(0.018)	(0.014)	(0.027)
N (students)	24,653	10,675	13,978	18,314	5,429

Notes: The table reports estimates of β_1 from Equation 3.1, applied to quality measures of students' college choice. Robust standard errors, clustered by $100 EFC bin, are in parentheses next to each coefficient.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

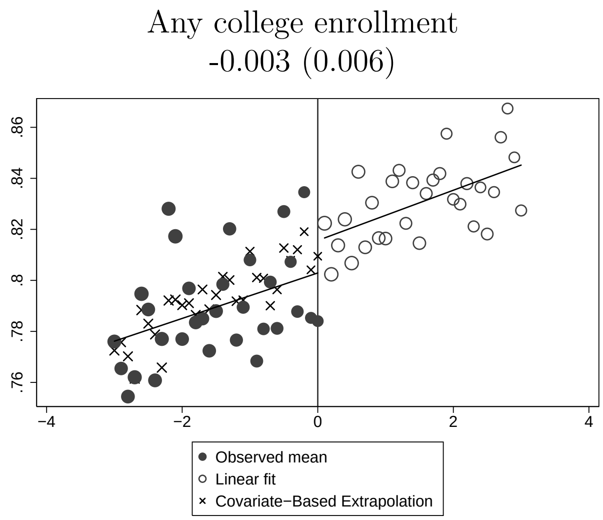


Figure C.4: N = 35,073 high school graduates. Treatment effect estimates for any college enrollment below the eligibility threshold. Scatter points (dots) represent mean college enrollment by distance from Pell eligibility thresholds, weighted by the number of students in each \$100 EFC bin. X-markers represent extrapolated college enrollment as predicted by a regression of outcomes against student observables above the eligibility threshold. Lines fit predictions from Equation 3.1. Point estimates for treatment effects below the threshold are reported below the figure heading, with standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

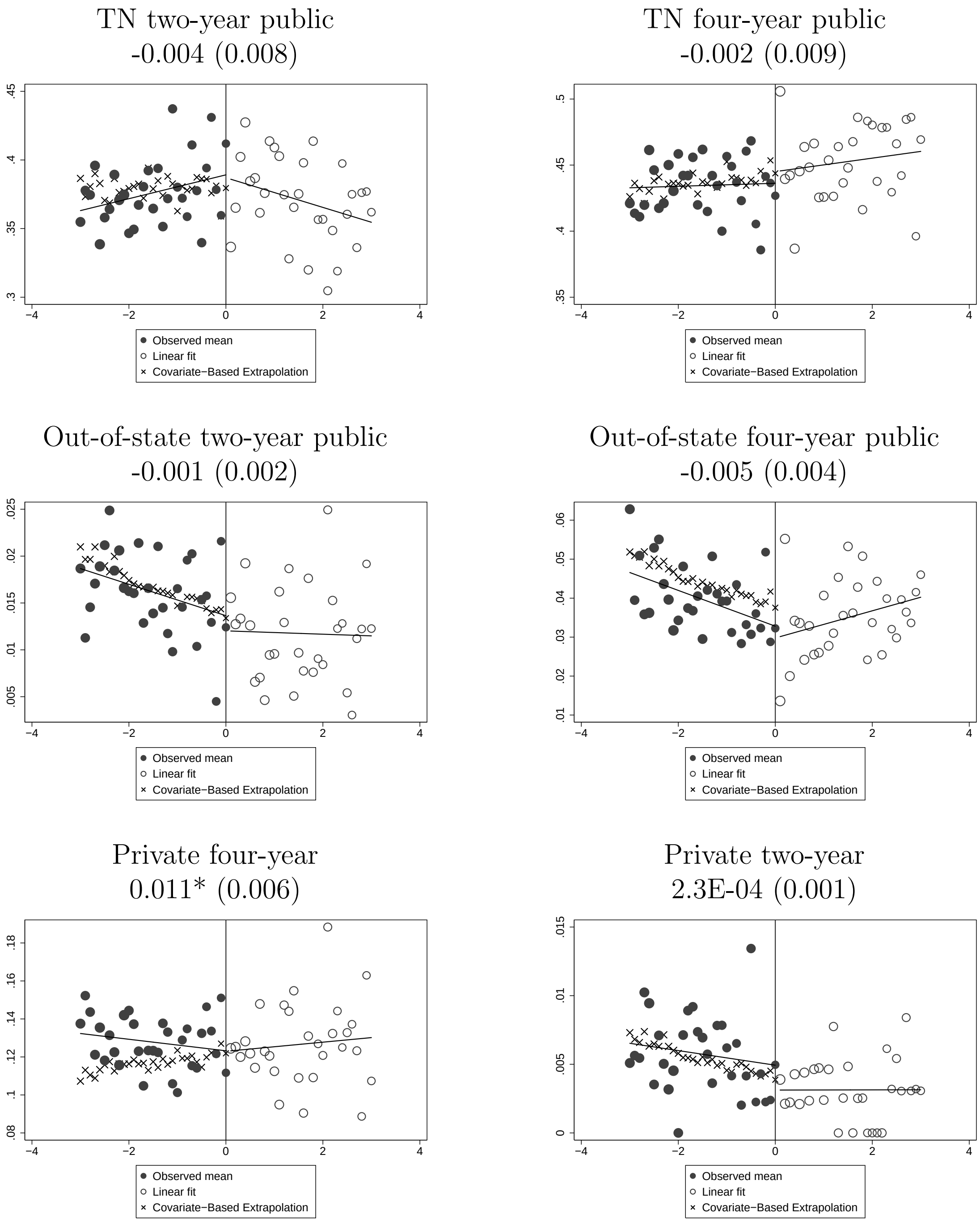


Figure C.5: N = 28,250 college-going students. Treatment effect estimates for college sector below the eligibility threshold. Scatter points (dots) represent college sector outcomes by distance from Pell eligibility thresholds, weighted by the number of students in each $100 EFC bin. X-markers represent extrapolated college enrollment outcomes as predicted by a regression of outcomes against student observables above the eligibility threshold. Lines fit predictions from Equation 3.1. Point estimates for treatment effects below the threshold are reported below figure headings, with standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

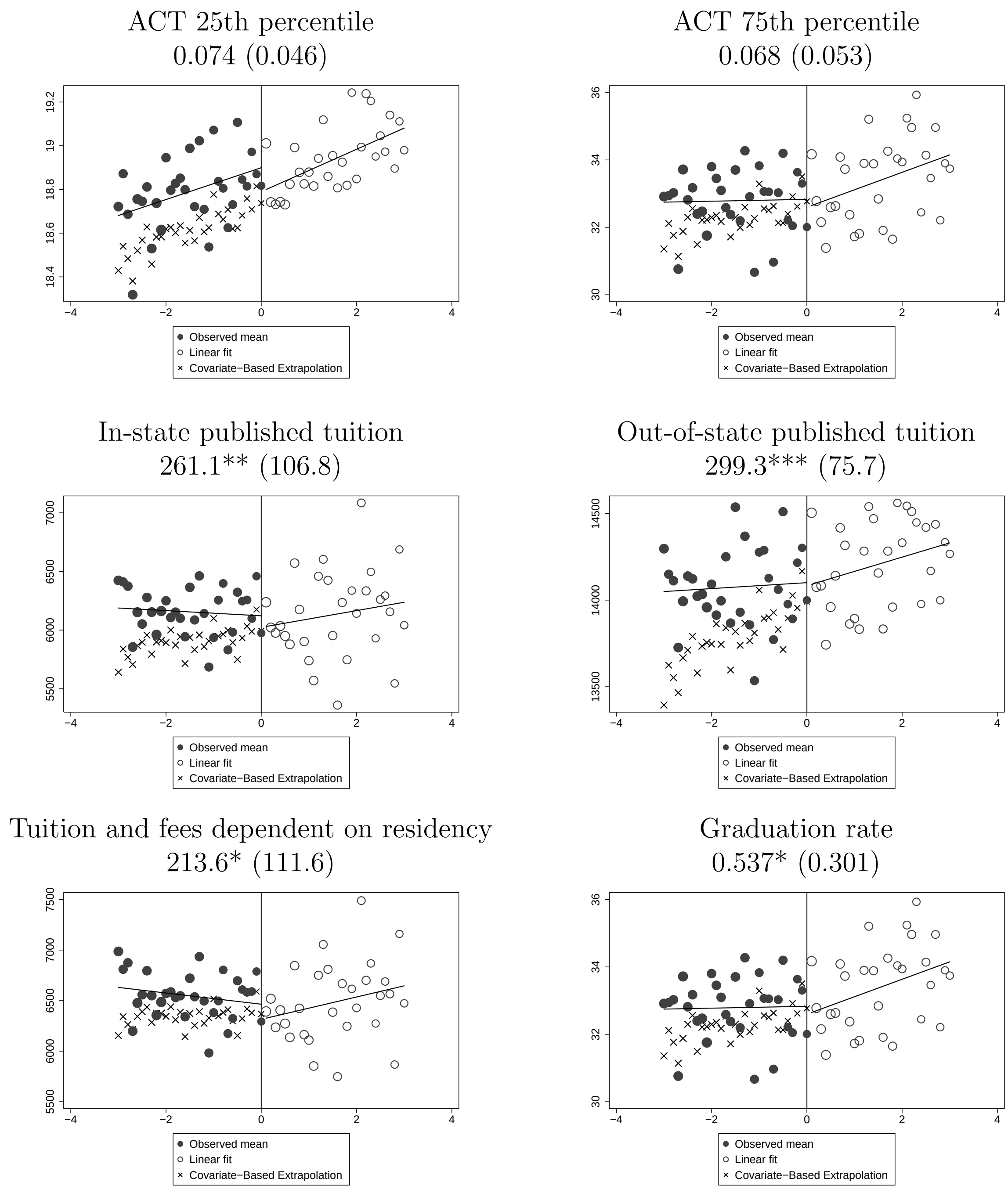


Figure C.6: N = 28,250 college-going students. Treatment effect estimates for college quality below the eligibility threshold. Scatter points (dots) represent college quality means by distance from Pell eligibility thresholds, weighted by the number of students in each $100 EFC bin. X-markers represent extrapolated quality outcomes as predicted by a regression of quality measures against student observables above the eligibility threshold. Lines fit predictions from Equation 3.1. Point estimates for treatment effects below the threshold are reported below figure headings, with standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Below I present robustness and specification checks for our main regression discontinuity analyses of Pell eligibility and college outcomes. Additionally, results for treatment effects away from the eligibility threshold are presented for subsamples of students as well as results for our implicit test of the bounded conditional independence assumption.

C.1 Regression Discontinuity Specification Checks

Table C.6 lists β_1 estimates for any college enrollment across five different specifications of Equation 3.1, enumerated A - F. Model A is our baseline specification, where the sample is limited to students whose EFC falls within \$3,000 of the Pell eligibility cut-off, the EFC running variable is rounded to the nearest \$100, and covariates other than EFC functional forms are excluded. Results in the top row of Table C.6 can also be found in Figure C.2. Model B adds a rich set of students' pretreatment observables, summarized in Table C.1, as well as cohort fixed effects. Model C returns to the specification without controls but narrows the bandwidth to \$1,500. Model D estimates Equation 3.1 under the optimal bandwidth for each subsample, following Imbens and Kalyanaraman (2012). Out of concern that \$100 EFC bins are too narrow and introduce more noise than is necessary, in Model E we aggregate the running variable to \$500 EFC bins and re-estimate Equation 3.1. Lastly, Model F uses a quadratic rather than a linear function for the gap between i's EFC and the Pell eligibility threshold for i's cohort $(EFC_{ic} - \bar{E}_c)$. As we show in Table C.6, these modifications to the baseline specification do not affect our conclusions regarding Pell eligibility and college enrollment *per se*. Discontinuities in college-going are generally insignificant, with some counterintuitively negative point estimates that do not replicate across models A - F.

We do not estimate Equation 3.1 under bandwidths that include the full support of Pell eligible EFC values, for reasons apparent in Figure C.7. The top panel of the figure illustrates β_1 point estimates and confidence intervals for bandwidths ranging from \$500 - \$20,000. Treatment effect estimates for college enrollment are significantly positive for bandwidths of about \$5,000 and greater. But the bottom panel of Figure C.7 illustrates why that is the case. By construction, EFC is bounded by zero, and bandwidths that include students with zero EFC – who constitute a large share of Pell-eligible students and who tend to enroll in college at a significantly lower rate than their peers – overstate the linear discontinuity estimate at the eligibility threshold. These models let zero-EFC and $100-EFC students have too much influence over discontinuity estimates at the cut-off.

Tables C.7 - C.9 depict regression discontinuity estimates for college sector and college quality outcomes, overall and by gender. For college sector outcomes we add one specification check to models A - F described above: namely, we include students who do not enroll in college in the estimating equation.[1] In Table C.7 we show that null college sorting results for the sample overall generally hold up to variations of Equation 3.1. Exceptions are found in Column (5) regarding published tuition and fee discontinuities within the optimal bandwidth, but we note that these bandwidths (reported in brackets in Column (5)) are quite large and include zero-EFC students. Larger EFC bins render more precise point estimates in Column (6) for receiving institutions' ACT interquartile range, student-to-faculty ratio, graduation rate, and residency-based tuition. Turning to Tables C.8 and C.9 and comparing Columns (1) and (6), we see that these alternative EFC aggregations underscore the same subsample results that our baseline model detects.

Table C.8 lists college choice estimates for males, and we find that the positive treatment effects on college selectivity (in terms of the ACT interquartile range of males' selected colleges) are robust to each alternative, albeit with much less precision under the narrow bandwidth. Table C.9 repeats the same exercises for the female subsample, and we note that females' weakly significant substitution between in-state and out-of-state four-year institutions is robust across different models.

C.2 Treatment Effects Away from the Eligibility Threshold by Gender and Race

Now turning to treatment effects away from the eligibility threshold, Table C.10 holds results for our implicit test of the bounded conditional independence assumption that underlies the analysis of these effects. The table lists point estimates and standard errors for the π

[1] College quality outcomes are of course undefined for these students, so Column (1) and (2) results for the nine IPEDS college quality measures are identical.

parameter in Equation 3.2, i.e., the relationship between college outcomes listed in the left-hand column of Table C.10 and the running variable $EFC_{ic} - \bar{E}_c$ for Pell-ineligible students. With the small but statistically significant exception of out-of-state four-year enrollment, we find that the running variable is an insignificant component of college choice, conditional on students' X_{ic}.

Table C.11 lists treatment effects away from the eligibility threshold for subsamples of students. Column (1) lists results for all students which are the same as those presented in Figures C.4, C.5, and C.6, and estimates by gender and race are presented in Columns (2) through (5). Similar to results for all students, Pell eligibility has no significant treatment effect on college going below the threshold for these subsamples of students, and likewise, we find little meaningful impacts on students' chosen college sector below the Pell cut-off. The exception is for enrollment in a private four-year college, and this positive treatment effect below the threshold is concentrated among male and white students. Regression discontinuity results from Tables C.5 and C.8 indicate that marginal Pell eligibility leads males to sort into more selective colleges, in terms of the ACT scores of preceding classes. Table C.11 finds no such treatment effect below the threshold, suggesting that males' sensitivity to Pell grants is very local with regards to colleges' ACT intake. In terms of the price of college, however, students below the Pell cut-off – particularly males and whites – attend colleges with higher tuition and fees,[2] in contrast to null discontinuities at the threshold for all four subgroups (Table C.5).

Perhaps the most intriguing results in Table C.11 are those when the estimating sample is restricted to white students (Column (4)). White students below the threshold are more apt to enroll in private four-year institutions (by 1.3 percentage points, or 10 percent of the mean), and they attend institutions with higher ACT scores (by 0.5 - 0.7 percent of the mean), higher graduation rates (by 2.5 percent of the mean), and higher tuition and fees on the order of 15 - 16 cents per dollar of Pell aid. Given high returns to college quality documented elsewhere, it is conceivable that whites' behavioral changes due to Pell eligibility will yield modest returns over a career. The fact that these findings do not extend

[2]Unreported regression kink estimates similarly find a change in the slope of out-of-state tuition with respect to white students' EFC of 17 cents per dollar of Pell.

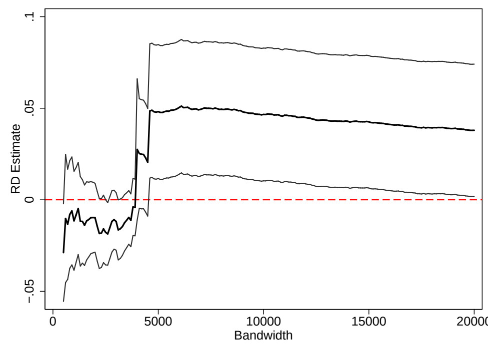
to under-represented minorities is concerning, as is the broader argument that much less costly interventions have proven to be more effective in changing student behavior.

Table C.6: Regression discontinuity specification checks for any college enrollment, by gender and race

	(1) All Students	(2) Male Students	(3) Female Students	(4) White Students	(5) Black Students
A. Baseline estimates	-0.012	-0.012	-0.012	-0.016*	0.008
	(0.008)	(0.014)	(0.012)	(0.009)	(0.017)
B. With covariates	-0.006	0.001	-0.012*	-0.005	0.002
	(0.004)	(0.007)	(0.006)	(0.004)	(0.011)
C. Narrow bandwidth (1,500)	-0.014	-0.007	-0.021	-0.017	-0.007
	(0.011)	(0.019)	(0.019)	(0.012)	(0.023)
D. Optimal bandwidth	-0.011	-0.008	-0.018	-0.013	-0.003
	(0.010)	(0.016)	(0.016)	(0.011)	(0.020)
	[$1,742]	[$2,122]	[$1,905]	[$1,903]	[$2,331]
E. $500 EFC bins	-0.011	-0.015	-0.008	-0.017*	0.016
	(0.008)	(0.014)	(0.012)	(0.009)	(0.018)
F. Quadratic	-0.012	-0.012	-0.013	-0.016*	0.008
	(0.008)	(0.014)	(0.012)	(0.009)	(0.017)
N (thousands)	17.7 - 35.1	8.0 - 16.0	9.7 - 19.2	13.5 - 26.0	3.7 - 8.0

Notes: The table reports estimates of β_1 from Equation 3.1, applied to the binary incidence of any college enrollment. Row A displays results from our baseline specification, also found in Figure C.2. Row B illustrates results when covariates (cohort fixed effects and X_i variables listed in Table C.1) are included in Equation 3.1. Row C lists results when the bandwidth around the Pell eligibility threshold is narrowed from $3,000 to $1,500. Row D lists results under the optimal bandwidth for each outcome and subsample (Imbens and Kalyanaraman, 2012). Row E lists results when we aggregate the EFC running variable to $500 bins rather than $100 bins. Row F lists results when a quadratic rather than a linear function is used for $EFC_{ic} - \bar{E}_c$, the gap between i's EFC and that cohort's Pell eligibility threshold. Robust standard errors, clustered by $100 EFC bin, are in parentheses under each coefficient. Optimal bandwidths are in brackets below standard errors in Row D.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

I. RD estimate for any college enrollment, by bandwidth



II. Any college enrollment, $5,500 bandwidth
0.048*** (0.019)

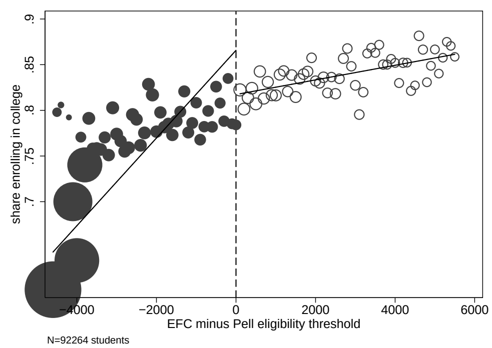


Figure C.7: Sensitivity of regression discontinuity results to bandwidth. Panel I: RD treatment effect estimates and confidence intervals for any college enrollment, by bandwidth. Panel II: Scatter points (dots) represent mean college enrollment by distance from Pell eligibility thresholds, weighted by the number of students in each \$100 EFC bin. Lines fit predictions from Equation 3.1. Point estimates for treatment effects below the threshold are reported below figure headings, with standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Regression discontinuity specification checks for college sector and college quality, all students

	(1) Baseline	(2) All HS graduates	(3) With covariates	(4) Narrow BW	(5) Optimal BW	(6) $500 EFC bins	(7) Quadratic
Enrolled in a public TN two-year college	0.001	-0.004	0.002	0.007	-0.002	-0.005	0.001
	(0.015)	(0.012)	(0.013)	(0.022)	(0.018)	(0.015)	(0.015)
					[$2,418]		
Enrolled in a public TN four-year college	-0.008	-0.012	-0.009	-0.024	-0.011	-0.007	-0.009
	(0.016)	(0.014)	(0.014)	(0.023)	(0.018)	(0.016)	(0.015)
					[$2,491]		
Enrolled in a public out-of-state two-year college	0.002	0.001	0.002	-0.001	0.001	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
					[$6,269]		
Enrolled in a public out-of-state four-year college	0.003	0.002	0.003	0.009	0.004	0.004	0.003
	(0.005)	(0.004)	(0.005)	(0.009)	(0.005)	(0.006)	(0.005)
					[$3,764]		
Enrolled in a private four-year college	0.001	-0.001	0.001	0.009	0.003	0.003	0.001
	(0.007)	(0.006)	(0.007)	(0.008)	(0.005)	(0.007)	(0.007)
					[$8,683]		
Enrolled in a private two-year college	0.002	0.001	0.002	0.001	0.002	0.002*	0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
					[$2,823]		
ACT 25th percentile (or SAT equivalent)	0.114	0.114	0.095*	0.076	0.08	0.120*	0.113
	(0.068)	(0.068)	(0.054)	(0.094)	(0.080)	(0.063)	(0.068)
					[$2,117]		
ACT 75th percentile (or SAT equivalent)	0.115	0.115	0.094	0.102	0.155*	0.127*	0.114
	(0.073)	(0.073)	(0.059)	(0.105)	(0.087)	(0.067)	(0.072)
					[$2,350]		
Instructional expenditures per FTE student	21.4	21.4	-13.8	-64.2	19.6	-15.5	21.2
	(123.9)	(123.9)	(122.7)	(152.7)	(120.9)	(118.0)	(123.0)
					[$3,244]		
Student-to-faculty ratio	-0.224	-0.224	-0.155	-0.151	-0.133	-0.255*	-0.222
	(0.135)	(0.135)	(0.118)	(0.204)	(0.193)	(0.138)	(0.133)
					[$1,691]		
Graduation rate	0.814	0.814	0.645	0.667	0.796	0.983*	0.808
	(0.530)	(0.530)	(0.432)	(0.709)	(0.633)	(0.507)	(0.523)
					[$2,181]		
In-state tuition and fees	162.1	162.1	143.9	197.2	319.5***	204.4	166.4
	(135.7)	(135.7)	(123.7)	(146.8)	(90.0)	(138.1)	(135.2)
					[$32,483]		
Out-of-state tuition and fees	149.5	149.5	148.4	117.3	201.2	173.7	150.8
	(134.6)	(134.6)	(99.0)	(190.8)	(149.9)	(130.6)	(132.0)
					[$2,496]		
Tuition and fees, dependent on students' residency	217.3	217.3	203.5	284.5**	372.6***	273.0*	221.1
	(140.1)	(140.1)	(131.6)	(136.9)	(116.3)	(142.9)	(138.3)
					[$8,217]		
Carnegie: Very High or High Research	0.007	0.007	0.005	-0.016	0.002	0.009	0.006
	(0.014)	(0.014)	(0.013)	(0.019)	(0.015)	(0.014)	(0.014)
					[$2,569]		
N (maximum)	28,340	35,192	28,340	14,324	22,826	28,340	28,340

Notes: The table reports estimates of β_1 from Equation 3.1, applied to the binary incidence of college sector or measures of college quality. Column (1) displays results from our baseline specification, also found in Table C.4 and in Table C.5. Column (2) displays results when the estimating sample covers all high school graduates, including those who did not go to college. Column (3) illustrates results when covariates (cohort fixed effects and X_i variables listed in Table C.1) are included in Equation 3.1. Column (4) lists results when the bandwidth around the Pell eligibility threshold is narrowed from $3,000 to $1,500. Column (5) lists results under the optimal bandwidth for each outcome and subsample (Imbens and Kalyanaraman, 2012). Column (6) lists results when we aggregate the EFC running variable to $500 bins rather than $100 bins. Column (7) lists results when a quadratic rather than a linear function is used for $EFC_{ic} - \bar{E}_c$, the gap between i's EFC and that cohort's Pell eligibility threshold. Robust standard errors, clustered by $100 EFC bin, are in parentheses under each coefficient. Optimal bandwidths are in brackets below standard errors in Column (5). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Regression discontinuity specification checks for college sector and college quality, males

	(1) Baseline	(2) All HS graduates	(3) With covariates	(4) Narrow BW	(5) Optimal BW	(6) $500 EFC bins	(7) Quadratic
Enrolled in a public TN two-year college	-0.025	-0.024	-0.020	-0.038	-0.02	-0.032	-0.025
	(0.020)	(0.017)	(0.019)	(0.030)	(0.023)	(0.020)	(0.020)
					[$2,675]		
Enrolled in a public TN four-year college	0.017	0.008	0.009	0.017	0.019	0.021	0.017
	(0.020)	(0.017)	(0.019)	(0.030)	(0.020)	(0.021)	(0.020)
					[$3,166]		
Enrolled in a public out-of-state two-year college	-0.001	-0.001	-1.6E-04	-0.003	0.003	3.7E-04	-0.001
	(0.004)	(0.003)	(0.004)	(0.006)	(0.003)	(0.004)	(0.004)
					[$18,106]		
Enrolled in a public out-of-state four-year college	-0.007	-0.006	-0.005	-0.002	-0.008	-0.005	-0.007
	(0.009)	(0.007)	(0.009)	(0.016)	(0.009)	(0.010)	(0.009)
					[$2,972]		
Enrolled in a private four-year college	0.013	0.008	0.013	0.025*	0.013*	0.012	0.013
	(0.010)	(0.008)	(0.010)	(0.012)	(0.007)	(0.010)	(0.010)
					[$7,143]		
Enrolled in a private two-year college	0.004*	0.003*	0.004*	0.002	0.001	0.004*	0.004*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
					[$4,463]		
ACT 25th percentile (or SAT equivalent)	0.217**	0.217**	0.185*	0.174	0.194*	0.212**	0.219**
	(0.103)	(0.103)	(0.094)	(0.138)	(0.112)	(0.096)	(0.102)
					[$2,472]		
ACT 75th percentile (or SAT equivalent)	0.314***	0.314***	0.284**	0.242	0.293**	0.305***	0.317***
	(0.113)	(0.113)	(0.107)	(0.151)	(0.114)	(0.106)	(0.114)
					[$2,773]		
Instructional expenditures per FTE student	118.7	118.7	70.5	264.6	44.0	57.6	126.5
	(182.5)	(182.5)	(179.7)	(208.0)	(172.9)	(180.1)	(183.3)
					[$3,971]		
Student-to-faculty ratio	-0.355*	-0.355*	-0.291*	-0.436	-0.263	-0.390*	-0.357*
	(0.195)	(0.195)	(0.174)	(0.302)	(0.263)	(0.198)	(0.191)
					[$1,950]		
Graduation rate	1.109	1.109	0.847	1.037	1.291	1.278	1.122
	(0.829)	(0.829)	(0.764)	(1.178)	(1.009)	(0.797)	(0.809)
					[$2,377]		
In-state tuition and fees	246.5	246.5	216.3	441.7	341.5**	234.0	255.1
	(209.3)	(209.3)	(201.0)	(285.6)	(144.7)	(207.9)	(213.1)
					[$9,905]		
Out-of-state tuition and fees	260.7	260.7	196.7	333.7	250.0	266.5	263.5
	(184.5)	(184.5)	(162.4)	(274.4)	(192.5)	(179.3)	(182.4)
					[$2,875]		
Tuition and fees, dependent on students' residency	200.0	200.0	198.1	346.0	388.1**	209.5	208.3
	(218.0)	(218.0)	(209.7)	(287.5)	(177.7)	(221.1)	(218.9)
					[$4,808]		
Carnegie: Very High or High Research	0.009	0.009	0.004	-0.003	0.009	0.011	0.009
	(0.019)	(0.019)	(0.019)	(0.025)	(0.019)	(0.019)	(0.018)
					[$3,065]		
N (maximum)	12,359	15,960	12,359	6,249	10,787	12,359	12,359

Notes: The table reports estimates of β_1 from Equation 3.1, applied to the binary incidence of college sector or measures of college quality. The estimating sample is limited to males. Column (1) displays results from our baseline specification, also found in Table C.4 and in Table C.5. Column (2) displays results when the estimating sample covers all high school graduates, including those who did not go to college. Column (3) illustrates results when covariates (cohort fixed effects and X_i variables listed in Table C.1) are included in Equation 3.1. Column (4) lists results when the bandwidth around the Pell eligibility threshold is narrowed from $3,000 to $1,500. Column (5) lists results under the optimal bandwidth for each outcome and subsample (Imbens and Kalyanaraman, 2012). Column (6) lists results when we aggregate the EFC running variable to $500 bins rather than $100 bins. Column (7) lists results when a quadratic rather than a linear function is used for $EFC_{ic} - \bar{E}_c$, the gap between i's EFC and that cohort's Pell eligibility threshold. Robust standard errors, clustered by $100 EFC bin, are in parentheses under each coefficient. Optimal bandwidths are in brackets below standard errors in Column (5). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.9: Regression discontinuity specification checks for college sector and college quality, females

	(1) Baseline	(2) All HS graduates	(3) With covariates	(4) Narrow BW	(5) Optimal BW	(6) $500 EFC bins	(7) Quadratic
Enrolled in a public TN two-year college	0.022	0.013	0.020	0.043*	0.016	0.017	0.022
	(0.017)	(0.014)	(0.014)	(0.024)	(0.018)	(0.017)	(0.017)
					[$2,744]		
Enrolled in a public TN four-year college	-0.029*	-0.029*	-0.025*	-0.058**	-0.034*	-0.029*	-0.029*
	(0.017)	(0.016)	(0.014)	(0.023)	(0.018)	(0.017)	(0.017)
					[$2,599]		
Enrolled in a public out-of-state two-year college	0.004	0.003	0.004	0.001	-0.001	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)
					[$5,366]		
Enrolled in a public out-of-state four-year college	0.011*	0.009*	0.009*	0.017**	0.004	0.012**	0.011*
	(0.006)	(0.005)	(0.006)	(0.007)	(0.004)	(0.006)	(0.006)
					[$9,307]		
Enrolled in a private four-year college	-0.008	-0.008	-0.008	-0.004	-0.009	-0.004	-0.008
	(0.010)	(0.008)	(0.010)	(0.015)	(0.006)	(0.010)	(0.010)
					[$15,413]		
Enrolled in a private two-year college	1.5E-04	5.0E-05	0.001	4.9E-04	4.0E-04	4.8E-04	1.6E-04
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
					[$2,801]		
ACT 25th percentile (or SAT equivalent)	0.031	0.031	0.024	-0.001	0.051	0.048	0.027
	(0.085)	(0.085)	(0.070)	(0.112)	(0.093)	(0.080)	(0.086)
					[$2,262]		
ACT 75th percentile (or SAT equivalent)	-0.043	-0.043	-0.055	-0.009	0.028	-0.013	-0.047
	(0.105)	(0.105)	(0.084)	(0.151)	(0.117)	(0.099)	(0.107)
					[$2,494]		
Instructional expenditures per FTE student	-54.5	-54.5	-91.8	-322.7	-86.2	-71.7	-62.5
	(190.3)	(190.3)	(185.7)	(221.7)	(178.1)	(191.0)	(192.4)
					[$3,392]		
Student-to-faculty ratio	-0.123	-0.123	-0.034	0.071	0.005	-0.15	-0.117
	(0.153)	(0.153)	(0.135)	(0.198)	(0.187)	(0.154)	(0.156)
					[$1,853]		
Graduation rate	0.575	0.575	0.462	0.391	0.927	0.744	0.549
	(0.617)	(0.617)	(0.537)	(0.778)	(0.640)	(0.607)	(0.631)
					[$2,500]		
In-state tuition and fees	100.2	100.2	90.0	6.0	199.5	185.4	100.6
	(185.1)	(185.1)	(184.4)	(227.9)	(146.1)	(193.4)	(187.5)
					[$11,950]		
Out-of-state tuition and fees	61.5	61.5	108.8	-52.1	198.1	100.4	61.3
	(156.6)	(156.6)	(122.9)	(194.9)	(159.9)	(155.4)	(156.1)
					[$2,731]		
Tuition and fees, dependent on students' residency	234.0	234.0	210.0	239.6	387.2**	326.4	233.7
	(207.8)	(207.8)	(212.9)	(264.9)	(178.8)	(214.0)	(211.2)
					[$7,182]		
Carnegie: Very High or High Research	0.005	0.005	0.003	-0.026	0.005	0.006	0.004
	(0.018)	(0.018)	(0.017)	(0.023)	(0.019)	(0.018)	(0.018)
					[$2,763]		
N (maximum)	15,981	19,232	15,981	8,075	14,975	15,981	15,981

Notes: The table reports estimates of β_1 from Equation 3.1, applied to the binary incidence of college sector or measures of college quality. The estimating sample is limited to females. Column (1) displays results from our baseline specification, also found in Table C.4 and in Table C.5. Column (2) displays results when the estimating sample covers all high school graduates, including those who did not go to college. Column (3) illustrates results when covariates (cohort fixed effects and X_i variables listed in Table C.1) are included in Equation 3.1. Column (4) lists results when the bandwidth around the Pell eligibility threshold is narrowed from $3,000 to $1,500. Column (5) lists results under the optimal bandwidth for each outcome and subsample (Imbens and Kalyanaraman, 2012). Column (6) lists results when we aggregate the EFC running variable to $500 bins rather than $100 bins. Column (7) lists results when a quadratic rather than a linear function is used for $EFC_{ic} - \bar{E}_c$, the gap between i's EFC and that cohort's Pell eligibility threshold. Robust standard errors, clustered by $100 EFC bin, are in parentheses under each coefficient. Optimal bandwidths are in brackets below standard errors in Column (5). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.10: Conditional independence assumption specification checks

	(1) Coefficient on $EFC_{ic} - \bar{E}_c$	(2) (St. Err.)
Enrolled in any college	-3.2E-04	(0.003)
Enrolled in a public TN two-year college	-0.006	(0.005)
Enrolled in a public TN four-year college	-0.001	(0.005)
Enrolled in a public out-of-state two-year college	0.001	(0.001)
Enrolled in a public out-of-state four-year college	0.006***	(0.002)
Enrolled in a private four-year college	-1.4E-04	(0.004)
Enrolled in a private two-year college	0.001	(0.001)
ACT 25th percentile (or SAT equivalent)	0.039	(0.027)
ACT 75th percentile (or SAT equivalent)	0.049	(0.032)
Instructional expenditures per FTE student	-86.5	(60.8)
Student-faculty ratio	-0.048	(0.056)
Graduation rate	0.181	(0.184)
In-state tuition and fees	2.8	(60.2)
Out-of-state tuition and fees	-41.0	(45.3)
Tuition and fees, dependent on students' residency	67.7	(62.5)
Carnegie: Very High or High Research	0.002	(0.005)

Notes: The table reports estimates of π from Equation 3.2, applied to the binary incidence of college enrollment, binary college sector outcomes, or college quality measures. Outcomes Y_{ic} for students i, cohort c are regressed against observable characteristics (X_{ic}: real family adjusted gross income (AGI), the EFC-AGI ratio, mothers' and fathers' college education, race, gender, first ACT composite score, the number of ACT attempts made, a binary indicator for missing ACT data) and $EFC_{ic} - \bar{E}_c$, the gap between i's EFC and that cohort's Pell eligibility threshold. Standard errors are listed in parentheses beside each coefficient.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.11: Treatment effect estimates for outcomes below the eligibility threshold

	(1) All Students	(2) Male Students	(3) Female Students	(4) White Students	(5) Black Students
Any college enrollment	-0.003	-0.004	0.007	-0.007	0.001
	(0.006)	(0.009)	(0.009)	(0.006)	(0.018)
Enrolled in a public TN two-year college	-0.004	-0.016	-0.004	-0.010	0.003
	(0.008)	(0.012)	(0.012)	(0.009)	(0.021)
Enrolled in a public TN four-year college	-0.002	0.004	0.006	-0.003	0.017
	(0.009)	(0.013)	(0.013)	(0.010)	(0.024)
Enrolled in a public out-of-state two-year college	-0.001	0.005	-0.005	0.002	-0.008
	(0.002)	(0.003)	(0.003)	(0.002)	(0.008)
Enrolled in a public out-of-state four-year college	-0.005	-0.013**	0.003	-0.004	-0.002
	(0.004)	(0.006)	(0.005)	(0.004)	(0.013)
Enrolled in a private four-year college	0.011*	0.019**	0.003	0.013**	0.001
	(0.006)	(0.009)	(0.009)	(0.007)	(0.016)
Enrolled in a private two-year college	2.3E-04	0.002	-0.002	0.002**	-0.011*
	(0.001)	(0.002)	(0.002)	(0.001)	(0.007)
ACT 25th percentile (or SAT equivalent)	0.074	0.107	0.032	0.131***	-0.112
	(0.046)	(0.071)	(0.067)	(0.047)	(0.143)
ACT 75th percentile (or SAT equivalent)	0.068	0.112	0.024	0.121**	-0.057
	(0.053)	(0.083)	(0.078)	(0.056)	(0.162)
Instructional expenditures per FTE student	228.9	330.9*	-116.9	307.0**	-347.3
	(147.3)	(177.5)	(156.5)	(146.3)	(330.3)
Student-to-faculty ratio	-0.081	-0.117	0.062	-0.135	0.211
	(0.088)	(0.124)	(0.138)	(0.095)	(0.292)
Graduation rate	0.537*	0.678	0.488	0.820**	-0.553
	(0.301)	(0.461)	(0.463)	(0.328)	(0.834)
In-state tuition and fees	261.1**	388.6***	96.6	341.1***	-194.5
	(106.8)	(149.7)	(145.4)	(112.7)	(287.0)
Out-of-state tuition and fees	299.3***	334.2***	318.7***	347.4***	242.7
	(75.7)	(112.2)	(114.3)	(78.9)	(232.6)
Tuition and fees, dependent on students' residency	213.6*	289.6*	91.4	311.7***	-207.7
	(111.6)	(160.2)	(151.3)	(118.2)	(299.5)
Carnegie: Very High or High Research	-0.004	-0.002	-0.004	-0.007	0.024
	(0.008)	(0.012)	(0.013)	(0.008)	(0.024)

Notes: $N = 35{,}073$ high school graduates for any college enrollment and 28,250 college-going students for other outcomes. The table reports treatment effect estimates for outcomes below the eligibility threshold and are calculated as described in Section 3.4.3. Column (1) lists results for all students which are the same as those presented in Figures C.4, C.5, and C.6. Columns (2) through (5) list results by subgroups of students. The estimating sample for any college enrollment includes all high school graduates while the estimating sample is restricted to college-going students for outcomes regarding college sector and college quality. Standard errors are calculated following Kline (2011) and are in parentheses under each coefficient.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Vita

Jilleah Gayle Welch was born in Oak Ridge, Tennessee, to Gayle and Gerald Boroughs. She graduated *magna cum laude* from North Carolina State University in 2004, where she received Bachelor of Science degrees in industrial engineering and mathematics. After completing her undergraduate studies, she attended the University of Tennessee, where she received a Master of Business Administration in 2006. Before returning to graduate school, Jilleah worked at The Boeing Company in Seattle, Washington from 2007 to 2010. Thereafter, she accepted a graduate assistantship at the University of Tennessee, where she received a Master of Arts degree in economics in 2011 and a Doctor of Philosophy degree in economics in 2015. Her research interests include public economics, economics of education, and environmental economics.