Merit-Based Aid, College Affordability, and Student Success

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To my parents and grandmother with love and gratitude

부모님과 외할머니께,
감사합니다. 그리고 사랑합니다.

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CHAPTER I

INTRODUCTION

Since the early 1990s, statewide merit-based aid has been prevalent across the country. As merit-based aid subsidizes full tuition and required fees at public colleges for students with solid academic records, it has gained popularity to the point where the number of students who meet its eligibility exceeds the amount of available funds. As a result, several states with large merit-based aid programs considered raising their eligibility standards or reducing the award amount. At this point, it is timely to examine whether merit-based aid achieves its stated goals.

In this dissertation, I examine whether or not merit-based aid promotes college affordability, student persistence, and degree attainment in three different studies. Despite positive effects of merit-based aid on college enrollment and graduation (e.g., Dynarski, 2004; Scott-Clayton, 2011), there is evidence that suggests that merit-based aid might have some unintended consequences on students due to the very fact that merit-based aid provides a generous amount of grant aid to students with solid academic records. To date, only a few studies (e.g., Long, 2004) have directly tested these unintended consequences of merit-based aid. In order to fill the gap in the literature, I explore whether the availability of merit-based aid leads to these potential negative consequences on students as follows.

Above all, as the adoption of merit-based aid makes the direct costs of college education almost free for its recipients, it makes going to in-state public four-year colleges more attractive than in the past. When a state's merit-based aid program has a relatively lenient academic requirement, it might have attracted many students who are academically underprepared for college education to public four-year colleges (Binder, Ganderton, & Hutchens, 2002). If these students find themselves not ready for college education, they might choose to drop out anyway even with the generous support from their state. If this were the case, merit-based aid might

unintentionally incentivize students to make an unwise college choice and states to spend their resources inefficiently.

Even if these underprepared students decide to remain in college, it is very likely that they lose their merit-based aid after a couple of years. In most states with merit-based aid, merit-based aid recipients must renew their scholarship eligibility by maintaining their cumulative GPA above a certain point. Each year, a large number of students fail to renew their scholarship eligibility due to the GPA requirement. Given the positive relationship between family income and academic performance, these students could be more likely to come from disadvantaged backgrounds and be more responsive to college prices than their counterparts from advantaged backgrounds. Combined with their lack of academic preparation, losing scholarships may make it more difficult for these students to remain in college and earn their degrees. However, to date, little is known about what happens to these students after they lose their scholarships.

Lastly, the fact that students with solid academic records are guaranteed to get a majority of their tuition and fees subsidized (through merit-based aid) may incentivize colleges to raise their tuition or reduce the amount of institutional aid, which mitigates the intended effect of state merit-based aid. Under the long and constant decreases in state support as well as in the number of college-going students, colleges have been actively involved in revenue-generating activities such as seeking more external grants or recruiting students who can pay full tuition (Slaughter & Rhoades, 2004). Given the context, the adoption of new financial aid policy that guarantees subsidizing a majority of tuition and fees for a large number of students could be a great opportunity for colleges to increase their revenue by raising their student charges or reducing the amount of institutional aid provided. If this were the case, creating a new merit-based aid program or increasing the funding for the program might not achieve its stated goal of making college education more affordable for students with solid academic records.

As a whole, this dissertation asks whether merit-based aid policy, which has been popular among the public and known to promote educational attainment of state residents, has some unintended consequences on

students. Although separately explored, all three scenarios described above are built upon the suspicion that the seemingly positive quality of merit-based aid (e.g., generous funding, academic criteria, etc.) can actually have the opposite effects on students. It is not uncommon to observe that a policy has some unexpected results when it is implemented. This dissertation asks if any of the unexpected results occurred and attempts to provide policy implications. In addition, as a whole, this dissertation explores if merit-based aid policy can be an answer to address some of the most pressing issues in American higher education: rising college prices and stagnant college graduation rates.

The rest of this dissertation is organized as follows. In the second chapter of this dissertation, I examine whether public and private four-year colleges whose states adopted a merit-based aid policy significantly change their tuition and fees, the amount of institutional aid per student, and room and board charges more than colleges whose states did not adopt a merit-based aid policy. To answer the question, I analyze data from the Integrated Postsecondary Educational Data System using the difference-in-differences method. Results from this study will answer one of the important questions regarding government financial aid: Does it really make college more affordable, or does it generally serve as a new source of college revenue?

While the second chapter investigates the effect of merit-based aid at the institutional level, the third and fourth chapters explore its effect on student persistence and degree attainment using the example of Tennessee. In 2004, Tennessee implemented its statewide merit-based aid, Tennessee Educational Lottery Scholarships (TELS). TELS shares some common features with other states' merit-based aid programs as its eligibility is solely determined by students' test scores and high school grades. However, it is unique in that it has supplementary components for either highest-achieving students or low-income students. This leads to two different possibilities. On one hand, the income supplements as well as its relatively lower ACT requirement in the first year of implementation are expected to help students from disadvantaged backgrounds attend college, who would have not been eligible for merit-based aid in other states (Heller & Marin, 2004). On the other hand, it is also possible that some students who benefitted from the lower eligibility standards might not fare well in

college and discontinue their study at some point. Given the two contrary possibilities, I evaluate the effect of receiving merit-based aid on student success using administrative data from Tennessee.

In the third chapter, using the regression discontinuity method, I examine whether receiving TELS in the freshman year affects college graduation within six years as well as the timing of graduation. In this chapter, I compare initial recipients to non-recipients regardless of their scholarship renewal status in terms of their graduation probabilities. In the fourth chapter, I examine whether a student's scholarship status affects persistence and graduation in each semester up to six years. In Tennessee, more than one-half of initial scholarship recipients lose their aid in college. If students lose their scholarship, will it affect their persistence, graduation, and time taken to obtain their degree? I answer these questions using event history analysis and regression discontinuity models.

The last chapter provides concluding remarks for all three studies. I first summarize the results from each study. Then, I explain what I have learned about merit-based aid from this dissertation and suggest what needs to be done in terms of policy as well as future research. At the end of this dissertation, I introduce my future research agenda.

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CHAPTER II

Does Merit-Based Aid Improve College Affordability?

Testing the Bennett Hypothesis in the Era of Merit-Based Aid

Abstract

This study tests the Bennett hypothesis by examining whether or not four-year colleges changed listed tuition and fees, the amount of institutional grants per student, and room and board charges after their states implemented statewide merit-based aid programs. Because many statewide merit-based aid programs cover full tuition and fees for students enrolled in their state colleges, I hypothesize that colleges whose states implemented merit-based aid programs would raise student charges or reduce institutional aid for more revenue. Using the difference-in-differences method, I analyze data from the Integrated Postsecondary Education Data System (IPEDS) from 1987 to 2009. My results show that colleges significantly changed their prices, but did not always increase the net price that students had to pay. Public colleges in many states with merit-based aid reduced published tuition and fees and increased the amount of institutional grants per student. These results suggest that the implementation of merit-based aid programs could make college education more affordable for those who received the aid, and may not harm non-recipients.

Key words: merit-based aid, the Bennett hypothesis, tuition, institutional aid

Introduction

College affordability has become an important concern across the country. According to a national survey in 2009, more than half of American adults surveyed believed that academically qualified students did not have enough opportunities for college education in spite of its importance for their future success

(Immerwahr et al., 2010). Rising college costs and student debt levels also suggest that college education is out of reach even for many students from middle-income families. To mitigate the financial burden of students and families, governments and colleges spend a huge amount of money on financial aid. In 2010, federal and state governments invested approximately 141.3 billion dollars, and colleges spent 29.7 billion dollars on undergraduate financial aid (College Board, 2011).

Among many scholarship programs, statewide merit-based aid is a new type of financial aid that covers tuition and fees for in-state college students exclusively based on their academic merit. Due to its simple rules and broad coverage, statewide merit-based aid has been widely available across the country and draws lots of attention from researchers. To date, researchers show that merit-based aid largely increases freshman enrollments, particularly in four-year public colleges (Cornwell, Mustard, & Sridhar, 2006; Dynarski, 2002). Given these results, it seems clear that merit-based aid is successful at boosting freshman enrollments. However, little is known about its potential, and possibly negative, impact on students and colleges. One of the potential consequences is its impact on college tuition as suggested in the Bennett hypothesis. According to the Bennett hypothesis, increases in federal aid make it easier for colleges to raise their tuition because the aid will help students pay tuition (Bennett, 1987). The hypothesis seems plausible in the case of merit aid given that merit aid programs cover a substantial portion of listed tuition and fees for a majority of students in state public colleges. If state governments paid tuition, why would students and colleges be concerned much about tuition increases?

My study examines how colleges responded to the creation of statewide merit-based aid and the impact of these responses on college affordability. Although many studies show the positive impact of financial aid on student demand, little is known about its impact on the supply side (Leslie & Brinkman, 1987; Heller, 1997). However, understanding institutional responses to financial aid is important because colleges can modify the impact of financial aid by changing tuition, room and board charges, and the amount of institutional grants (Long, 2004). This study investigates a relatively unexplored area, institutional responses to financial aid, and

tests the possibility that a financial aid policy that provides institutions with different incentives may not be as effective as originally intended. The results can inform state policy makers who are interested in whether or not their merit-based aid policy has improved college affordability, especially given the recent tuition hikes and economic recessions (Baum & Ma, 2011; Quizon, 2011).

Background

Since the early 1990s, statewide merit-based aid has been popular across the country. After Arkansas started its Academic Challenge Scholarship in 1991, more than a dozen states implemented statewide merit-based aid programs.[1] Following previous studies (e.g., Dynarski, 2002; Zhang & Ness, 2010), I define statewide merit-based aid programs as those whose eligibility is solely determined by students' academic achievement and those that target a wide population of resident students rather than a few elite students. Table 1 summarizes each of these programs' inception year, eligibility requirements and award amounts when each program was first implemented. In some states, eligibility standards and award amounts have changed over time. Because my study examines changes in student charges right before and after merit-based aid was implemented, I focus on the academic requirements and award amounts applied during the first year of implementation of each merit-based aid program in this study.

According to Table 1, merit-based aid programs have some common features although their academic requirements and award amounts differ. First, most programs set the academic standard around a 3.0 high school GPA. This standard makes it possible for these programs to benefit a large number of resident students considering that approximately "40% of high school seniors in 1999 met this standard" across the country (Dynarski, 2002, p. 64).

[1] According to the NASSGAP (2011), all except seven states provided non-need-based grants in the academic year of 2010 to 2011. However, some of these non-need-based grants consider students' financial needs as well as academic performances.

Table 1. Statewide Merit-Based Scholarship Programs

State (Start Year)	Initial Criteria	Award amounts per year
Alaska (1999/ 2011)	1. UA Scholars Program (UA System only; top 10% of graduating class) 2. Alaska Performance Scholarship (three-tiered) 1) 3.5 GPA & 25 ACT or 1680 SAT 2) 3.0 GPA & 23 ACT or 1560 SAT 3) 2.5 GPA & 21 ACT or 1450 SAT	$2,750 1) $4,755 2) $3,566 3) $2,378
Arkansas (1991)	Arkansas Academic Challenge Scholarship - 2.5 GPA in HS core & 19 ACT	Public & Private: 1st year: $2,500 2nd year: $2,750 3rd year: $3,000 4th year: $3,500
Florida (1997)	Florida Bright Futures Scholarship (two-tiered) 1. Florida Academic Scholar H.S. (3.5 GPA & 1270 SAT or 28 ACT) 2. Florida Medallion Scholar H.S. (3.0 GPA & 970 SAT or 20 ACT)	Public 1. 100% tuition & fees 2. 75% tuition & fees Private The average public tuition & fees
Georgia (1993)	Helping Outstanding Pupils Educationally - 3.0 GPA	Public: full tuition & fees Private: $3,000
Kentucky (1999)	Kentucky Educational Excellence Scholarship - 2.5 GPA	Public: $125-$2,500 Private: equivalent (Award varies based on a high school GPA from 9th to 12th grade. Additionally, students can earn bonus amount based on SAT/ACT scores and AP/IB exams)
Louisiana (1998)	Taylor Opportunity Program for Students (three-tiered) 1. Honors Award - 3.0 GPA & 27 ACT 2. Performance Award - 3.0 GPA & 23 ACT 3. Opportunity Award - 2.5 GPA & ACT score above the state's average	Public: 1. Tuition & fees + $800 2. Tuition & fees + $400 3. Tuition & fees Private: The average public tuition & fees
Maryland (2002-2005)	Maryland HOPE Scholarship - 3.0 GPA in HS Core	Public & Private: $3,000 (subject to availability of fund)
Massachusetts (2005)	The John and Abigail Adams Scholarship - Score "advanced" or "proficient" AND top 25% of graduating class in their district in MCAS math and English	Public only: tuition only (up to six semesters)

Table 1. Statewide Merit-Based Scholarship Programs (continued)

State (Start Year)	Criteria	Award
Michigan (2000-2008)	Michigan Merit Award & Promise Scholarship - Level2 on all four components of MEAP test or, - Level2 on two components of MEAP test and 75th percentile of SAT/ACT	In-state public & private: $2,500 Out-of-state public & private: $1,000 Not renewable (one-time award)
Mississippi (1996)	1. Mississippi Resident Tuition Assistance Grant (MTAG) - 2.5 GPA & 15 ACT 2. Mississippi Eminent Scholars Grant (MESG) - 3.5 GPA & 29 ACT	Public & Private: Freshmen/Sophomore: $500 Junior/Senior: $1,000 Up to $2,500 per year (no more than tuition and fees)
Missouri (1987)	Bright Flight Scholarship - Top 3-5% of all MO students taking either ACT or SAT	Depending on annual funds (Up to $3,000 for public & private)
Nevada (2000)	Millennium Scholarship - 3.0 GPA & pass the state's exit exam	Public only Up to $2,500
New Mexico (1997)	Lottery Success Scholarship - No H.S. criteria - 2.5 GPA at the1st semester in college	Public only: tuition & fees
South Carolina (1998)	LIFE Scholarship - 3.0 GPA & 1100 SAT/24ACT	Public & Private: $2,000
Tennessee (2004)	Tennessee Educational Lottery Scholarship (four-tiered) 1. GAMS - 3.75 GPA & 28 ACT 2. HOPE Base - 3.0 GPA or 19 ACT 3. ASPIRE - 3.0 GPA or 19 ACT & AGI<36k 4. ACCESS - 2.75 GPA & 18 ACT & AGI<36k	Public & Private: 1. $4,000 2. $3,000 3. $4,000 4. $2,000
West Virginia (2002)	PROMISE - 3.0 GPA & 1000 SAT/21 ACT	Public: tuition & fees Private: average tuition & fees

Source: Dynarski (2002); Dynarksi (2005); Hu, Trengove, and Zhang (2012); Orsuwan & Heck (2009); Zhang & Ness (2010); States' web sites

Second, most programs cover more than half of tuition and fees at their state's public four-year colleges, especially when each of these programs was adopted. Table 2 provides the average tuition and fees at public and private four-year institutions when each state started its merit-based aid program. Several states (e.g., Georgia and Florida) subsidized 100% of tuition and fees for students enrolled in public four-year colleges, while other states such as New Mexico and Nevada provided a fixed amount of money that was sufficient to pay tuition and fees at public four-year colleges.

Lastly, most of these programs, except ones in Massachusetts, Nevada, and New Mexico, provide the equivalent amount of money to students enrolled in private colleges in their states. The amount was not sufficient, but it would still have been helpful for these students to pay their tuition. Considering the lenient academic standards and generous award amounts, merit-based aid might be an easier target from which colleges can appropriate revenue compared to other financial aid programs.

Table 2. Merit-Based Aid Award Amounts and the Average Tuition Levels

State	Award Amount	Average Tuition & Fees (in Current Dollars)	
		Public Four-Year	Private Four-Year
AR[2]	$2,500	$1,805	$5,721
FL	Pub: 75% to 100% of tuition and fees Priv: Weighted average t&f of Public 4-year colleges	$1,911	$11,525
GA	Pub: Tuition and fees Priv: $1,500	$1,886	$9,040
KY	$500-$2,500 (Depending H.S GPA & ACT score)	$2,723	$9,614
LA	Pub: Tuition and fees Priv: Weighted average t&f of Public 4-year colleges	$2,390	$14,003
MA	Tuition only (up to 8 semesters)	$7,290	$27,335
MI	$1,250 (for the first 2 years)	$4,615	$11,155
MS	MTAG: Freshmen & Sophomore: $500 Junior & Senior: $1,000 MESG: $2,500	$2,497	$7,226
NM	Tuition only (from the 2nd semester) (public only)	$2,073	$8,943
NV	$2,500	$2,344	$11,465
SC	Pub: $2,000	$3,414	$10,660
TN	$4,000	$4,039	$15,074
WV	Pub: Tuition & Fee Priv: Equivalent Amount	$2,898	$12,441

[2] Due to the data availability, the average tuition and fees at public and private four-year colleges in Arkansas are for the academic year of 1993 to 1994. Considering that the tuition and fees are non-decreasing over time, I assume that tuition and fees in 1991 would have been even lower than the amount shown in the table.

Conceptual Framework

My study is grounded on the Bennett hypothesis and Bowen's revenue theory of costs. The Bennett hypothesis suggests that colleges increase listed tuition and fees to capture additional revenue resulting from an increase in financial aid. This hypothesis rests on the revenue theory of costs which argues that colleges try to increase revenue as long as it does not harm their reputations (Bowen, 1980). According to Bowen, there is a spiral effect among college finances (including tuition and institutional aid), educational quality, and reputation. Colleges with large external subsidies from governments and philanthropies can afford charging less for students and spending more on education. This investment in turn attracts high-performing students and scholars to their institutions, which enhances the colleges' reputations. Colleges with high-achieving students and scholars then attract more external funding from governments and private sectors, and the spiral goes on. The fact that most colleges are non-profit also allows them to spend as much revenue as they have (Martin, 2011).

To summarize, colleges seek for more revenue to invest in their students, scholars, and facilities to enhance their reputation. Because most merit-based aid programs cover a substantial portion of tuition and fees for a majority of their state resident students, I hypothesize that colleges attempt to capture this new source of revenue by increasing their listed tuition and fees, reducing their own spending on institutional grants, or increasing room and board charges.

Literature Review

Overall, researchers find positive effects of merit-based aid on students' academic preparation, college enrollment, and graduation. First of all, merit-based aid appears to motivate high school students to work hard to meet the academic requirements. After the HOPE scholarship started in Georgia, the average SAT scores of high school seniors and college freshmen significantly increased (Cornwell, Mustard, and Sridhar, 2006; Henry and Rubenstein, 2002). In Tennessee, the number of students who scored a 19 or above on ACT, which was the

cut-off score to receive the state's merit-based aid at the time, increased after the implementation of the merit-based aid (Pallais, 2009).

Second, the availability of merit-based aid increases college enrollments, especially in four-year colleges (Cornwell, Mustard, and Sridhar, 2006; Dynarski, 2002). After analyzing the data for seven Southern states that adopted merit-based aid before 2000, Dynarski (2002) shows that merit-based aid in all seven states but South Carolina has significant and positive effects on college enrollments. Zhang and Ness (2010) argue that merit-based aid keeps the best and brightest students in their states because the enrollment increases after the implementation of merit-based aid programs are the largest in research universities that are typically more selective than any other types of institutions.

Lastly, merit-based aid also promotes degree attainment. At the state level, Dynarski (2005) finds that the share of adults with college degrees (at least an associate's degree) significantly increased after Arkansas and Georgia introduced their merit-based aid programs. In Georgia and Florida, Zhang (2011) also reports the increased number of bachelor's degree-holders after the implementation of their merit-based aid programs. Both studies show that the positive effects are larger for women than men. At the student level, Henry, Rubenstein, & Bugler (2004) show that HOPE recipients in Georgia are more likely to persist and graduate within four years compared to students who lost or never received the HOPE scholarship. Looking at West Virginia, Scott-Clayton (2011) also provides evidence that the incentive embedded in the state's PROMISE scholarship encourages students to take sufficient credits to graduate within four years.

Despite these positive effects, there are concerns about the unintended consequences of merit-based aid. First, Heller and Marin (2002; 2004) suggest that the fact that merit-based aid is assigned solely based on academic achievement could limit college access for racial-minority or low-income students who, on average, have lower test scores. In fact, Dynarski (2004) shows that the enrollment effect can be heterogeneous across racial groups by finding a clear and negative effect of merit-based aid on African-American and Hispanic students in Georgia. However, Singell Jr., Waddell, and Curs (2004) find that the number and proportion of

low-income students have not decreased in both two-year and four-year colleges since the HOPE scholarship started. In addition, there is some evidence that merit-based aid increases enrollment and degree attainment of both white and non-white students in a few states (Cornwell, Mustard, and Sridhar, 2006; Dynarski, 2002, Dynarski, 2005). Considering these results, overall, merit-based aid appears to have positive effects on students despite its heterogeneous effects across different groups. Another concern regarding merit-based aid is its impact on institutions. Because merit-based aid provides a majority of students enrolled in public colleges with grants of high monetary values, colleges might try to capture rents[3] by raising their tuition, as suggested in the Bennett hypothesis.

To date, only a few studies have examined whether the Bennett hypothesis holds true in the context of merit-based aid. Long (2004) finds that private four-year colleges in Georgia directly increased tuition and fees, while public four-year colleges decreased tuition and increased room and board charges. These price increases were the most pronounced in colleges with many HOPE recipients. In contrast, since the inception of Florida's Bright Futures Scholarship, Calcagno and Alfonso (2007) find that community colleges in Florida increased the amount of institutional grants without changing tuition and fees. The authors explain that these community colleges seemed to cover the unmet needs (the difference between listed tuition and fees and grants from all sources) of their students because many community college students were eligible for the Florida Medallion Scholars, which provided only 75% of tuition and fees.[4]

The Bennett hypothesis was also tested with other federal and state financial aid programs. When the revenue from the Pell Grants or federally subsidized loans increased, there were significant tuition raises in

[3] Rent-seeking activities occur when a government imposes restrictions on economic activities (Krueger, 1974). Agents attempt to influence social or political environments in a way that limits entry of their competitors into the market and guarantees their profits to some extent. Lobbying, occupational licensing, and education subsidies are the example of rent-seeking activities (Pasour, 1987). However prevalent, rent-seeking activities have been regarded as socially problematic because they redistribute wealth to the agents' favor rather than creating new wealth. In the context of my study, colleges might pursue increasing their revenue from their state's merit-based aid program by raising their prices. If so, it not only attenuates the intended effect of state efforts to make college education affordable but also makes it hard for students to pay their tuition bill.

[4] At the time of the study, there were four types of merit-based aid in Florida depending on a student's academic performance. The first two programs for the highest-achieving students covered all tuition and fees, while the other two programs provided only 75% of tuition and fees. Considering the relatively lower academic achievement of community college students, many students in community colleges were expected to receive one of the latter two aid programs.

public four-year colleges (McPherson & Shapiro, 1991), state flagship universities (Rizzo & Ehrenberg, 2004) and both public and private four-year colleges (Singell & Stone, 2007). These results suggest that colleges change their tuition in response to the federal policy changes. On the other hand, Lan and Winters (2011) do not find significant tuition changes in colleges that enrolled many D.C. residents after the District of Columbia College Access program started.[5]

Recent studies emphasize the importance of looking at institutional grants in addition to listed tuition and fees when investigating colleges' responses to government financial aid policies. When focusing on Pell Grants recipients, Turner (2012a) finds that students who received more Pell Grants were awarded fewer amounts of institutional grants. In addition, Turner (2012b) shows that colleges reduced the amount of institutional grants approximately dollar-for-dollar for students who received federal tax credits. Because students had to wait until April to receive their tax benefits, they took out loans to pay tuition before their academic year began. These results show that the intended benefit of federal aid programs, to ease the financial burden of students and families by subsidizing tuition and fees, was offset by decreased college grants. Lastly, Curs and Dar (2010) also show that colleges responded differently to state financial aid depending on their governance structures. Public colleges in states with coordinating governing boards and private colleges, both of which enjoy more institutional autonomy, raised their net price in response to increased state aid. In contrast, public colleges in states with consolidating boards, which are granted less autonomy, reduced listed tuition and increased institutional grants.

Lastly, there are many factors other than the adoption of state merit-based aid policy that can affect demand for higher education, which could indirectly but eventually be related to college prices. According to the economics of higher education literature (e.g., Cohn & Geske, 1990), there are several factors that shift the demand for higher education. For example, increases in the size of young adult population as well as state unemployment rates generally increase the demand for higher education (e.g., Lehr & Newton, 1978; Stafford,

[5] The D.C. Access program covered the difference between in-state and out-of-state tuition for D.C. residents enrolled in out-of-state colleges.

Lundstedt, & Lynn, 1984). The relationship between unemployment rates and demand for higher education can also be explained by the human capital theory as the indirect costs of college education (opportunity costs) decrease as unemployment rates go up.

In addition, according to Long (2004), a state's wealth, culture, and traditional approach regarding higher education can be related to supply and prices of college education in the state. To take into account these factors, she adds the percentage of the population with a bachelor's degree, annual per capita income, and annual unemployment rate in her statistical model.

Lastly, it is possible that colleges change the amount of institutional aid if their states change the amount of state need- and merit-based aid. One of the widespread concerns about state merit-based aid is that states might have moved their resources from need-based aid to merit-based aid. Although previous studies show that state merit-based aid does not crowd out state need-based aid (Doyle, 2010; Longanecker, 2002), colleges still might consider the amount of state need-based grants when they decide how much institutional aid they spend on their students.

To summarize, colleges and universities appear to respond to external financial aid policies. Their response is the greatest in colleges where a large number of students benefit from the policy, or in colleges with more market power and institutional autonomy over tuition setting. Colleges also change the amount of institutional grants as well as listed tuition and fees in response to external aid changes. Based on my review of the literature, my study investigates three research questions. First, after merit-based aid policies were implemented, did four-year colleges in merit-based-aid states increase tuition and fees more than colleges in states without merit-based aid? Second, after the policies were implemented, did four-year colleges in merit-based-aid states reduce the dollar-value of institutional grants awarded per student[6][7] more than colleges in states

[6] Throughout my study, "the dollar-value of institutional grants per student" means the average amount of money that a student receives from his/her institution in the form of grants.

[7] When looking at the amount of institutional aid, it can be important to differentiate institutional merit-based aid from institutional need-based aid. Colleges might have switched some money from their own merit-based aid to need-based aid in order to complement financial unmet needs of their students who are not eligible for state merit-based aid. Unfortunately, IPEDS do not provide separate data for institutional merit- and need-based aid.

without merit-based aid? Third, after the policies were implemented, did four-year colleges in merit-based-aid states increase room and board charges more than colleges in states without merit-based aid?

By looking at all the three price measures, this research provides a more complete picture of whether and/or how four-year colleges responded to their state's merit-based aid policy. It is important to examine all three prices because colleges sometimes use an indirect way of raising their prices instead of directly increasing their tuition, as Long (2004) demonstrates. In addition, this research examines all thirteen states that have adopted merit-based aid. This allows me to explore whether colleges' responses are different depending on each state's merit-based aid design, which has rarely been considered in previous studies.

Data & Sample

I use data from the Integrated Postsecondary Educational Systems (IPEDS) collected by the National Center for Education Statistics from 1987 to 2009. IPEDS is the most appropriate existing data set to study postsecondary education institutions because it provides college characteristics, enrollments, and finance information on every postsecondary institution that applied for or participated in any federal financial aid program authorized by Title IV.

I limit my sample to public four-year colleges and nonprofit private four-year colleges across the U.S. I first excluded for-profit colleges because they may have different pricing policies given their explicit goal of making profits. Moreover, a majority of students enrolled in for-profit colleges were non-traditional adult students who were not eligible for state merit aid in most states.[8] I also excluded two-year colleges because introducing merit-based aid can have different effects on these institutions compared to four-year colleges, as illustrated in Calcagno and Alfonso (2007).[9] Lastly, I dropped specialized (e.g., seminary or art school) and

[8] Most merit aid programs were limited to students who immediately went to college after high school graduation.

[9] There are some colleges who were originally classified as two-year institutions, but changed into four-year institutions in later years. Most of these colleges are branch campuses or community colleges that offer bachelor's degrees in some areas. I treated these colleges as two-year colleges and excluded them from my study.

tribal colleges because many of these colleges are very small, pursue a specific educational goal, and have different revenue structure compared to four-year colleges.[10]

After excluding these colleges, my sample has 449 public four-year colleges and 840 private four-year colleges. Table 3 provides the descriptive statistics of my sample in the academic year of 1990-1991. At this time, there was no statewide merit-based aid program available.[11] The top panel in Table 3 shows the descriptive statistics for colleges in all 50 states, while the bottom panel provides the descriptive statistics only for colleges in Southern states.[12]

Across the country, colleges in merit-based-aid states charged slightly lower tuition and room and board charges, provided less amounts of institutional grants, and received less amounts of external resources than colleges in non-merit-based-aid states. This pattern is consistent when I focus on Southern states, except that public colleges in merit-based-aid states charged slightly higher tuition and fees than public colleges in non-merit-based-aid states. This pre-policy trend poses a question if the argument, which many states adopted statewide merit-based aid programs because their colleges charged higher tuition in the first place, were true.

[10] In order to exclude these special purpose colleges, I dropped colleges that are classified as associate's institutions, specialized institutions, or tribal institutions according to the Carnegie Classification 2000/2005.

[11] Missouri's Bright Flight Scholarship was available during the year. However, I do not count the program as a state merit-based aid program because it was limited to the top 3-5% students who took the ACT or SAT.

[12] I compare the descriptive statistics of the treatment group to other Southern states because most states in the treatment group are located in the South.

Table 3. Descriptive Statistics of Samples in 1990-1991 (in current dollars)

All 50 States

	Public (N=449)		Private (N=840)	
	Merit	Non-Merit	Merit	Non-Merit
Tuition & Fees	1,713.3 (460.4)	1,816.1 (683.4)	7,681.4 (3,555.8)	8,632.3 (2,989.0)
Institutional Grant Aid	2,606,379.8 (5,184,482.8)	3,409,412.1 (6,062,356.3)	4,152,109.5 (9,987,164.4)	4,364,755.4 (7,866,662.7)
Room & Board Charges	2,813.0 (654.6)	3,129.4 (809.5)	3,648.9 (1,203.9)	3,719.3 (915.5)
State Appropriation	51,293,205.0 (71,667,659.5)	64,142,718.7 (97,174,295.8)	1,737,566.7 (3,155,878.4)	1,639,749.7 (3,793,545.5)
Private gifts, contracts, endowments, & investments	7,116,868.8 (17,706,073.0)	8,717,136.8 (20,883,783.7)	8,032,662.3 (32,652,977.1)	7,253,309.6 (24,931,330.2)

Southern States Only

	Public (N=187)		Private (N=258)	
	Merit	Non-Merit	Merit	Non-Merit
Tuition & Fees	1,595.8 (334.8)	1,566.3 (697.2)	6,312.4 (2,783.6)	6,990.9 (2,498.8)
Institutional Grant Aid	2,093,384.6 (2,662,123.6)	2,787,509.8 (5,659,199.2)	2,805,554.1 (6,799,777.7)	2,890,136.5 (4,725,112.3)
Room & Board Charges	2,668.9 (615.0)	2,997.7 (766.0)	3,246.5 (983.3)	3,411.6 (877.0)
State Appropriation	47,769,720.8 (67,380,897.3)	53,827,388.6 (105,777,121.4)	4,500,639.2 (5,800,593.0)	1,639,556.6 (3,021,694.5)
Private gifts, contracts, endowments, & investments	5,651,463.6 (14,615,697.6)	8,361,070.6 (22,783,298.9)	4,365,614.1 (10,043,727.8)	7,797,420.9 (24,378,874.9)

Methods

To answer my research questions, I use the difference-in-differences method. The difference-in-differences method compares the before-and-after-policy change in an outcome variable for the treatment group to that of the control group. For example, in my study, I compare change in tuition observed in the treatment group (colleges whose states implemented merit-based aid policies) to that of the control group (colleges in other states that have not adopted merit-based aid policies) four years before and after the introduction of merit-based aid. I choose this eight-year window because it takes at least four years for a newly adopted merit-based aid program to be available for all students from freshmen to senior.

When using the difference-in-differences method, it is important to choose appropriate control groups that are similar to the treatment group except the policy of interest (Angrist & Pischke, 2009). In this study, I employ two control groups: 1) colleges located in states that belong to the same regional compact with the treatment group and 2) colleges in all 50 U.S. states. The first control group is colleges located in states that belong to the same regional compact with the treatment group and that have not adopted merit-based aid programs during the time period studied (four years before and after a treatment state implemented merit-based aid). For example, I compare colleges in Georgia to colleges in the rest of Southern Regional Education Board states that never adopted merit-based aid from 1989 to 1996. Because Arkansas adopted its merit-based aid in 1991, colleges in Arkansas are excluded from the control group for Georgia.

In addition to states that belong to the same regional compact, I also use colleges in all U.S. states that never adopted merit-based aid during the time period studied as a control group. Previous studies use Southern states as a control group because most merit-based aid states are located in the South, and these states are comparable in terms of higher education demand and economic condition (Dynarski, 2002; Long, 2004; Zhang & Ness, 2010). However, my study looks at thirteen states that adopted merit-based aid programs in different years, and I exclude these states from the control group once they adopted merit-based aid. This decision results in only a few states (e.g., five states) left in the control group for the treatment group that adopted merit-based

aid in the mid-2000s. For this reason, I employ the second control group (colleges in all U.S. states) and see if the estimates significantly differ. Using this second control group will complement the sample size issue although it may not be as comparable to the treatment group as the first control group is.

Equation (1) is the statistical model that I use to answer my research questions. I run the model separately for public and private four-year colleges because these two types of colleges substantially differ in terms of tuition levels and the major source of revenue. I also run the model separately for each of the thirteen merit-based-aid states because each state has a different higher education context and merit-based aid program, which could lead to different effects across states. In equation (1), y_{ist} is the dependent variable of institution *i* located in state *s* in year *t*. Note that y_{ist} refers to listed tuition and fees for the first research question, the amount of institutional grants awarded per FTE student for the second research question, and room and board charges for the third research question. I take a natural logarithm of these dependent variables so that I can interpret a coefficient as a percent change as a result of a one-unit change in an independent variable.

$$y_{ist} = \alpha + \gamma(merit) + \lambda(post) + \delta(merit \cdot post) + \phi_s(state_s) + \theta_t(year_t) + X'_{ist}\beta + \varepsilon_{ist} \quad (1)$$

In the model above, *merit* is a dummy variable for each of the treatment states analyzed, and *post* is a dummy variable that indicates whether or not merit-based aid has been adopted in the treatment state. The interaction term between these two variables ($merit \cdot post$) is the key independent variable of this study. If the Bennett hypothesis holds, the coefficient on the interaction term (δ) will be statistically significant and positive for the first and third research questions, suggesting increased tuition and fees and increased room and board charges, respectively. For the second research question, the negative and statistically significant coefficient on the interaction term (δ) means that colleges reduced the amount of institutional grants per student in response to the creation of merit-based scholarships. I also add year (θ_t) and state fixed effects (ϕ_s) to capture potential year-specific and state-specific effects on college prices.

X_{ist} is a vector of state-level and college-level covariates that are known to affect tuition and financial aid. At the college level, I add a dummy variable that indicates a doctoral-granting institution. I also include state appropriation revenue (only for public college), revenue from private sources (such as investment return, endowment income, private gifts, grants, and contracts), the number of full-time and equivalent (FTE) students, and the number of full-time faculty members. I add these variables because selectivity and size of institutions are closely related to the amount of revenue from external sources (Curs & Dar, 2010; Long, 2004; Lowry, 2001; Rizzo & Ehrenberg, 2004; Singell & Stone, 2007). In addition, there are several state-level time-varying covariates in the model: the size of young adult population (20 to 24 years old), state unemployment rates, the percentage of bachelor's degree holders among the population, per capita income, and the total amount of state need-based grants awarded. These state-level covariates are related to a state's higher education demand, which in turn affects college enrollment and tuition levels (Cheslock & Hughes, 2011; Lowry, 2001).

When using a panel data set, serial correlation is a serious problem that significantly reduces the standard error of estimates and hence falsely rejects the null hypothesis (Bertrand, Duflo, & Mullainathan, 2004; Wooldridge, 2005). To address this issue, I use cluster-robust standard errors which minimize the impact of heteroskedasticity of errors (Drukker, 2003).

Results

Tables 4 through 7 show the difference-in-differences estimates. Tables 4 and 5 show the results for public four-year colleges using states that belong to the same regional compact and all 50 states as a control group, respectively. Tables 6 and 7 present the results for private four-year colleges using states that belong to the same regional compact and all U.S. states, respectively. Table 8 summarizes all these results. In order to save space, I present only the coefficient on the interaction term between merit-based aid and post-policy dummy variables (δ). Full results are available upon request.

Table 4 provides coefficients and standard errors for public colleges in each of the thirteen treatment states compared to public colleges in states that belong to the same regional compact. In the first column, I put the name of each treatment state and sample size used in the model for each state. The next three columns show price changes in tuition and fees, the amount of institutional grants per student, and room and board charges after each treatment state implemented its merit-based aid program. For example, 1,298 public four-year colleges in total are used to analyze price changes in response to Arkansas' Academic Challenge Scholarship. After the scholarship was implemented in 1991, public colleges in Arkansas significantly increased in-state tuition and room and board charges by 2.1% and 10.3%, respectively. However, they did not significantly change the amount of institutional grants compared to public colleges in other Southern states.

Overall, colleges in many states changed their prices[13] in response to the implementation of merit-based aid; however, their responses differed across states and college types. Due to the heterogeneity in the estimates, I will discuss on a few common patterns across states, and then discuss two factors that partially explain the heterogeneous responses. When discussing my results, I will focus on the results that are consistent across the two control groups. In general, most estimates especially for public colleges are consistent regardless of control groups used. However, estimates tend to be more sensitive when the sample size used in the analysis is relatively small (e.g., estimates for room and board charges or private colleges). The abbreviation of states with consistent results is italicized in Table 8.

[13] Throughout my study, "college prices" or "prices" mean the three outcomes of my study: listed tuition and fees, the amount of institutional grants per student, and room and board charges.

Table 4. Difference-in-Differences Results for Public Colleges (Using regional compact states)

	In-State Tuition (1)	Institutional Grant (2)	Room & Board Charge (3)
Arkansas	0.021*	-0.001	0.103***
(N=171)	(0.012)	(0.045)	(0.010)
Florida	-0.067***	0.214**	-0.012
(N=122)	(0.015)	(0.071)	(0.009)
Georgia	-0.125***	0.414***	0.056***
(N=178)	(0.019)	(0.061)	(0.010)
Kentucky	0.052*	-0.069	0.018
(N=121)	(0.025)	(0.045)	(0.019)
Louisiana	-0.048	0.058	-0.035*
(N=126)	(0.047)	(0.080)	(0.016)
Massachusetts	0.126***	0.272*	0.054**
(N=19)	(0.010)	(0.130)	(0.017)
Michigan	-0.017	0.359	-0.027*
(N=101)	(0.013)	(0.347)	(0.013)
Mississippi	-0.147***	0.176**	-0.004
(N=129)	(0.021)	(0.067)	(0.010)
Nevada	0.097**	-0.482*	-0.053*
(N=77)	(0.032)	(0.229)	(0.027)
New Mexico	-0.035	0.010	0.057**
(N=82)	(0.051)	(0.104)	(0.025)
South Carolina	0.052**	0.066	-0.085***
(N=124)	(0.020)	(0.040)	(0.015)
Tennessee	-0.017	0.144	-0.113***
(N=91)	(0.022)	(0.099)	(0.019)
West Virginia	0.008	-0.065	0.054***
(N=94)	(0.027)	(0.099)	(0.007)

Note: Every model includes state and year fixed effects and covariates. Cluster-robust standard errors are used.
Note: p-value: *: <0.05, **: <0.01, ***: <0.001

Table 5. Difference-in-Differences Results for Public Colleges (Using all U.S. states)

	In-State Tuition (1)	Institutional Grant (2)	Room & Board Charge (3)
Arkansas	0.032**	-0.076	0.086***
(N=422)	(0.013)	(0.059)	(0.009)
Florida	-0.006	0.148**	0.016*
(N=372)	(0.031)	(0.061)	(0.009)
Georgia	-0.080***	0.304***	0.041***
(N=431)	(0.019)	(0.059)	(0.010)
Kentucky	0.042**	-0.009	0.008
(N=354)	(0.019)	(0.071)	(0.011)
Louisiana	-0.042	0.179	-0.021*
(N=359)	(0.026)	(0.137)	(0.013)
Massachusetts	0.099***	0.189**	0.044***
(N=317)	(0.030)	(0.087)	(0.010)
Michigan	-0.029*	-0.002	-0.027***
(N=339)	(0.017)	(0.175)	(0.009)
Mississippi	-0.144***	0.191**	-0.001
(N=379)	(0.015)	(0.083)	(0.005)
Nevada	0.053	-0.588*	-0.061***
(N=326)	(0.029)	(0.242)	(0.012)
New Mexico	-0.044**	0.114*	0.012
(N=368)	(0.017)	(0.062)	(0.008)
South Carolina	0.047***	0.037	-0.087***
(N=357)	(0.014)	(0.066)	(0.009)
Tennessee	0.003	0.010	-0.085***
(N=316)	(0.018)	(0.114)	(0.012)
West Virginia	0.018	-0.643*	0.040***
(N=327)	(0.033)	(0.374)	(0.008)

Note: Every model includes state and year fixed effects and covariates. Cluster-robust standard errors are used.
Note: p-value: *: <0.05, **: <0.01, ***: <0.001

Table 6. Difference-in-Differences Results for Private Colleges (Using regional compact states)

	Out-of-State Tuition (1)	Institutional Grant (2)	Room & Board Charge (3)
Arkansas	0.091***	-0.095**	0.080***
(N=237)	(0.020)	(0.037)	(0.016)
Florida	-0.005	0.330***	-0.003
(N=184)	(0.011)	(0.029)	(0.006)
Georgia	0.018**	0.111***	-0.058***
(N=248)	(0.007)	(0.023)	(0.011)
Kentucky	0.005	0.017	-0.017*
(N=175)	(0.011)	(0.021)	(0.009)
Louisiana	-0.005	-0.128**	-0.005
(N=166)	(0.025)	(0.048)	(0.018)
Massachusetts	0.023	0.045	-0.015
(N=90)	(0.016)	(0.050)	(0.011)
Michigan	-0.023***	0.153***	-0.001
(N=220)	(0.006)	(0.037)	(0.007)
Mississippi	-0.007	0.081**	0.023**
(N=182)	(0.013)	(0.026)	(0.008)
Nevada	0.238***	0.729***	-0.028
(N=93)	(0.011)	(0.050)	(0.019)
New Mexico	0.111***	0.322***	0.130***
(N=96)	(0.020)	(0.060)	(0.031)
South Carolina	0.004	-0.168***	0.035***
(N=175)	(0.009)	(0.023)	(0.007)
Tennessee	-0.025*	-0.051	-0.013*
(N=141)	(0.012)	(0.029)	(0.006)
West Virginia	0.048	0.236*	-0.015
(N=122)	(0.027)	(0.111)	(0.012)

Note: Every model includes state and year fixed effects and covariates. Cluster-robust standard errors are used.
Note: p-value: *: <0.05, **: <0.01, ***: <0.001

Table 7. Difference-in-Differences Results for Private Colleges (Using all U.S. 50 states)

	Out-of-State Tuition (1)	Institutional Grant (2)	Room & Board Charge (3)
Arkansas	0.095***	-0.087***	0.064***
(N=796)	(0.010)	(0.025)	(0.008)
Florida	0.026***	0.279***	0.009**
(N=740)	(0.007)	(0.018)	(0.004)
Georgia	0.023***	0.064**	-0.064***
(N=808)	(0.006)	(0.026)	(0.007)
Kentucky	0.020***	0.009	-0.012***
(N=711)	(0.004)	(0.011)	(0.005)
Louisiana	0.018**	-0.133***	0.000
(N=702)	(0.008)	(0.020)	(0.006)
Massachusetts	-0.026***	-0.051*	-0.019***
(N=650)	(0.009)	(0.029)	(0.007)
Michigan	-0.027***	0.097**	-0.013**
(N=696)	(0.005)	(0.020)	(0.005)
Mississippi	0.008	0.041*	0.023***
(N=738)	(0.006)	(0.024)	(0.006)
Nevada	0.215***	0.859***	-0.018*
(N=678)	(0.008)	(0.021)	(0.008)
New Mexico	0.082***	0.388***	0.096***
(N=718)	(0.007)	(0.019)	(0.006)
South Carolina	0.019***	-0.169***	0.035***
(N=711)	(0.005)	(0.014)	(0.004)
Tennessee	0.002	-0.046**	0.002
(N=631)	(0.006)	(0.018)	(0.005)
West Virginia	0.052***	0.242***	-0.011
(N=658)	(0.011)	(0.031)	(0.018)

Note: Every model includes state and year fixed effects and covariates. Cluster-robust standard errors are used.
Note: p-value: *: <0.05, **: <0.01, ***: <0.001

Table 8. Results Summary Table by Outcomes

Outcome	Sign	Public		Private	
		Neighbors	All	Neighbors	All
Tuition	+	5 (***AR, KY, MA,*** NV, ***SC***)	5 (***AR, KY, MA***, NM, ***SC***)	4 (***AR, GA, NV, NM***)	9 (***AR***, FL, ***GA,*** KY, LA, ***NV, NM***, SC, WV)
	-	3 (FL, ***GA, MS***)	3 (***GA,*** MI, ***MS***)	2 (***MI,*** TN)	2 (MA, ***MI***)
Institutional Aid	+	4 (***FL, GA, MA, MS***)	4 (***FL, GA, MA, MS***)	7 (***FL, GA, MI, MS, NV, NM, WV)***	7 (***FL, GA, MI, MS, NV, NM, WV***)
	-	1 (***NV***)	3 (***NV,*** NM, WV)	3 (***AR, LA, SC***)	5 (***AR, LA,*** MA, ***SC,*** TN)
Room & Board	+	5 (***AR, GA, MA,*** NM, ***WV***)	5 (***AR,*** FL, ***GA, MA, WV***)	4 (***AR, MS,*** **NM**, ***SC***)	5 (***AR,*** FL, ***MS, NM, SC***)
	-	5 (***LA, MI, NV, SC, TN***)	6 (***LA, MI, NV,*** NM, ***SC, TN***)	3 (***GA, KY,*** TN)	6 (***GA, KY,*** MA, MI, NV, NM)

First of all, there are several states whose four-year colleges increased either tuition and fees or room and board charges, if not both. For instance, both public and private four-year colleges in Arkansas significantly raised both types of student charges after their state adopted merit-based aid. Public colleges in Kentucky, Massachusetts, and South Carolina as well as private colleges in Georgia, Nevada, and New Mexico also raised their tuition and fees more than their comparison groups. These results show that in many states colleges raised their student charges in response to the adoption of merit-based aid.

However, the increased student charge does not necessarily mean that the net price students paid out of pocket has gone up. When I look at all three outcomes simultaneously, colleges in some states increased the amount of institutional aid per student when they raised student charges. For example, public colleges in Georgia and Massachusetts as well as private colleges in New Mexico and Nevada increased both the amount of institutional aid and tuition. If colleges provided more money for institutional aid per student, the negative impact of tuition increases would be mitigated to some extent. Moreover, there are many states whose colleges

have not significantly changed all three outcomes (e.g., Tennessee) or whose colleges only increased institutional aid without raising student charges (e.g., Florida). In these states, attending four-year colleges has not become more expensive than their comparison groups.

Table 9. Results Summary Table (Common Patterns)

Pattern	Description	Public	Private
Increased student charges	Colleges increased tuition and/or room and board charges without raising institutional aid	3 (AR, KY, WV)	3 (AR, SC, LA)
Increased student charges, Increased Aid	Colleges increased institutional aid as well as tuition and/or room & board charges	1 (MA)	3 (MS, NV, NM)
Increased aid	Colleges increased institutional aid without changing student charges	-	2 (FL, WV)
Decreased student charges (Increased aid)	Colleges decreased tuition and/or room and board charges (and increased aid in some cases)	5 (FL, MS, LA, MI, TN)	2 (KY, MI)
Inconclusive results	1) Results are sensitive depending on control groups, or 2) One type of student charges (e.g., tuition) increased, while the other (e.g., room and board charges) decreased.	4 (GA, NV, SC, NM)	3 (GA, MA, TN)

Considering all three outcomes within each state, Table 9 summarizes the direction of the net price change in each state. Except the last (fifth) category, I present the common patterns observed in the order of less affordable to more affordable. For example, colleges in the first category raised student charges without increasing student aid. This is the worst scenario observed. If students did not receive merit-based aid, they should pay higher prices than before, compared to their peer students in other states. Public colleges in Arkansas, Kentucky, and West Virginia as well as private colleges in Arkansas, South Carolina, and Louisiana belong to this category. In the second category, colleges also raised student aid in addition to increasing student

charges. Although colleges charged their students more than before, the increased aid would most likely help students pay for their education.

In a state that belongs to the third or fourth category, college education appears to become more affordable. In the third category, colleges increased the amount of student aid without changing student charges. Private colleges in Florida and West Virginia fall into this third category. After the adoption of merit-based aid, students in these colleges have more money to pay their tuition that has not been significantly increased compared to colleges in other states. In the fourth category, colleges in these states decreased student charges relative to colleges in other states.[14] In other words, after the adoption of merit-based aid, going to colleges in these states becomes less expensive than going to colleges in other states. Public colleges in five states and private colleges in two states show these results. Moreover, some of these colleges (e.g., public colleges in Florida and Mississippi or private colleges in Michigan) increased the amount of institutional aid per student. As a result, college education in these states could become more affordable than before.

Lastly, there are some states where there is not sufficient evidence to determine the overall direction of the net price changes. In the fifth category, estimates in some states (e.g., public colleges in New Mexico) are sensitive depending on the control group choice. Or, colleges in other states show contrasting results between tuition and fees and room and board charges. For example, public colleges in Georgia significantly reduced tuition and fees and increased room and board charges. Long (2004) finds the same results and explains that these contrasting results suggest limited capability of public colleges in determining their own tuition. Although this is totally plausible, it is difficult to directly compare the percentage point changes between the two price measures (tuition and fees versus room and board charges). Therefore, I make a separate category for these states with contrasting estimates and leave them as inconclusive.

So far, I have described a few common patterns in my results and demonstrated the heterogeneity of these estimates. My results are strikingly different across states and college types. In order to explain the

[14] This does not mean that colleges literally decreased student charges. The nominal value of student charges almost always goes up, but the level of increases was smaller than that in other states.

heterogeneity, I explore whether each state's merit-based aid design as well as higher education governance structures are related to colleges' responses. As seen in Tables 1 and 2, eligibility requirements and award amounts are widely varied across states. These differences in merit-based aid design could provide colleges with different incentives. I hypothesize that colleges whose states set less rigorous requirements and provide generous funding are more likely to raise their student charges and reduce the amount of institutional aid per student. In contrast, colleges whose states set more rigorous requirements and provide fewer amount of institutional aid would be less likely to raise student charges and reduce institutional aid.

In Figure 1, I locate states depending on rigorousness of academic requirements of their merit-based aid and generosity of the funding. The x-axis represents the rigorousness based on the minimum high school GPA required, while the y-axis represents the generosity in terms of the percentage of the average tuition and fees at in-state public four-year colleges covered by merit-based aid. If a state's merit-based aid program is multi-tiered (e.g., Florida's Bright Future Scholarships), I use the least rigorous standard and the minimum award amount. In Massachusetts and Michigan, scholarship eligibility has been determined by their state exam scores or standardized test scores rather than high school GPAs. Because students in these two states are required to be within the top 25% on these tests to be eligible for merit-based aid, I assume that this requirement is more rigorous than having a 3.0 in high school.

According to Figure 1, states on the top left corner (e.g., Arkansas and Louisiana) covered almost 100% of tuition and fees in public four-year colleges, while these states set the minimum requirement around a 2.5 GPA. Hence, I hypothesize that colleges in these two states would be more likely to take advantage of the system by raising student charges and/or reducing institutional aid. In contrast, states on the right bottom corner (e.g., Massachusetts and Michigan) limited the eligibility around the top 25% within their state and provided only a portion of tuition and fees. Because the number of eligible students was very limited, colleges in these states are expected not to raise student charges and/or reduce institutional aid. These hypotheses partially explain the results. Among the four states that I mentioned above, the hypotheses fit the results for Arkansas

and Michigan. Colleges in Arkansas (with large incentive) raised student charges without increasing institutional aid, while colleges in Michigan (with small incentive) decreased student charges. However, the hypotheses do not explain results for Louisiana and Massachusetts well. Although merit-based aid provided 100% tuition and fees to students with a 2.5 high school GPA, public colleges in Louisiana decreased student charges. In Massachusetts, public colleges raised all three price measures although their state merit-based aid covered tuition only, leaving a large amount of required fees unsubsidized. These results suggest that colleges do not always respond to the incentives embedded in merit-based aid programs.

Moreover, private colleges significantly changed their prices in some states even though merit-based aid in these states were not eligible for students enrolled in private colleges. Of the thirteen states examined in this study, three states (Massachusetts, New Mexico, and Nevada) limit the merit-based aid eligibility to students in their public colleges. If private colleges in these states had responded to their state's merit-based aid (or incentive generated from the aid), they would have not increased their tuition due to possible loss of students. However, private colleges in New Mexico and Nevada increased their tuition as well as the amount of institutional aid per student. These responses in private colleges also call into question whether colleges change their prices in response to merit-based aid.

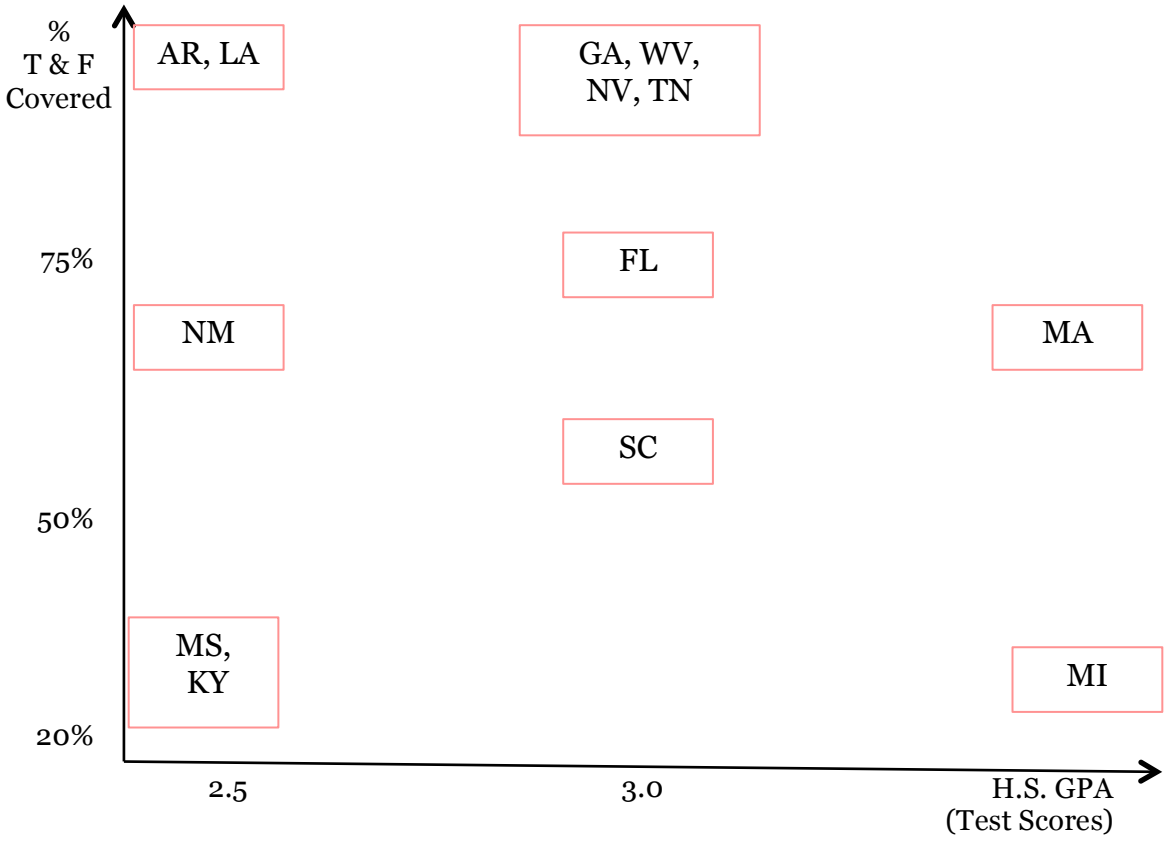

Figure 1. Academic Requirements and Award Amounts of Merit-Based Aid

Another factor that can explain the heterogeneous responses observed is whether individual institutions in a state have capability of setting their own tuition. As Long (2005) mentions, public colleges have less authority over their tuition than private colleges. Instead, their tuition levels are monitored or consulted by many stakeholders such as state legislature, state higher education agency, or system board (Bell, Carnahan, & L'Orange, 2011). Following Curs and Dar (2010), I hypothesize that the way public colleges respond to merit-based aid is different depending on the level of autonomy that individual colleges have regarding their tuition levels. In particular, colleges with less autonomy would reduce their student charges or increase the amount of institutional aid per student so that their tuition policy can be aligned with their state initiative.

Table 10. Higher Education Governance Structure

(A) Primary Authority over Tuition	
Primary Authority	States
State Legislature	FL, LA
Statewide Agency for Multiple Systems	GA, KY, MA, NM, NV
Governing Boards for Individual Systems	TN
Individual Institutions	AR, MI, MS, SC, WV
(B) Higher Education Governance Structure	
Structure	States
Consolidating Board	FL, GA, MS, NV
Regulatory Coordinating Board with Budget Authority	AR, KY, LA, MA, NM, SC, TN, WV
Planning Agency	MI

Source: Panel (A) Christal (1997); Rasmussen (2003); Bell, Carnahan, & L'Orange (2011), Panel (B) Doyle (2013).

Note: Higher education governance structure implemented at the time of adoption of merit-based aid (or the closest year, if not available) is reported.

Table 10 provides information about higher education governance structure in each state. Based on the survey conducted by State Higher Education Executive Officers, panel (A) shows the agency that had the primary authority over tuition setting within a state when its merit-based aid was adopted. The information is placed in the order of the level of centralization from the most centralized (e.g., state legislature) to the least centralized (e.g., individual institutions). Panel (B) provides the type of higher education governance structure in each state at the time of merit-based aid adoption. As Richardson et al., (1999) explain, decision-making process is more centralized in a state with a consolidating board, while individual institutions have more autonomy in a state with a coordinating board or a planning agency. According to both panels, state agencies (or external boards) appear to have more influence on public colleges in Florida, Louisiana, Georgia, Mississippi,

and Nevada than other states. Of the five states with a more centralized form of governance, public colleges in Florida, Louisiana, and Mississippi decreased student charges and sometimes increased the amount of institutional aid per student. Public colleges in the other two states (Georgia and Nevada) show mixed results. In addition, anecdotal evidence suggests that there is an initiative to keep public college tuition low in Florida and Georgia because tuition increases would directly call for more funding for merit-based aid (Rasmussen, 2003). Although higher education governance structure does not perfectly explain the way colleges respond to merit-based aid either, it explains why public colleges in some states might not be able to raise their net prices in response to their state merit-based aid.

Conclusion

My study examines whether four-year colleges changed listed tuition and fees, the amount of institutional grants per student, and room and board charges after the implementation of merit-based aid. To summarize, colleges significantly changed these prices after their states implemented merit-based aid. However, these price changes did not always increase the net price that students actually paid for their education. As opposed to the Bennett hypothesis, public colleges in some states significantly reduced tuition and fees in response to their state's merit-based aid. Although private colleges in many states significantly raised tuition and fees, many of them also increased the amount of institutional grants per student, which may offset the negative effects of tuition raises. The direction and magnitude of college responses differed across states, but each program's academic requirements and award amounts were not always related to the way colleges responded to merit-based aid. State higher education governance structure explains the way colleges responded to merit-based aid to some extent.

These results can be interpreted in two ways. First, the adoption of merit-based aid does not necessarily make our public colleges more expensive in most states. Although colleges in a few states significantly raised tuition and/or room and board charges, some of them also increased the amount of institutional aid per student.

Moreover, public colleges in many states either decreased or did not significantly change tuition and fees. Considering these results, attending public college in most merit-based-aid states appears to be more affordable for many students eligible for merit-based aid. As listed tuition and fees have remained stable or even decreased in many states, receiving merit-based aid helps students pay their tuition. If these students received other sources of financial aid (e.g., Pell Grants) in addition to their merit-based aid, the adoption of merit-based aid would significantly reduce their unmet needs. Even for students who were not eligible for the aid programs, the introduction of merit-based aid did not significantly raise the cost of attending public four-year colleges in most states. Despite these non-negative results at the institutional level, it is still important to consider that many students who are not eligible for merit-based aid come from disadvantaged backgrounds (e.g., Heller & Marin, 2002; 2004) and the adoption of merit-based aid has some negative effects on racial minority students (e.g., Dynarski, 2004).

Second, colleges may have used the additional revenue from state merit-based scholarships to subsidize their students rather than to reduce the amount of institutional grants per student and secure more revenue. Both public and private colleges in many states significantly increased the dollar value of institutional grant that each student received. Although it is not clear to whom these colleges distributed the additional institutional aid money, they might have spent it on subsidizing out-of-state students or needy students who were not eligible for state merit-based scholarships. In either case, the creation of state merit-based scholarships may have allowed these colleges to provide more in their institutional grants and enhance college affordability for their students.

Third, based on my findings, the Bennett hypothesis does not always hold in the context of state merit-based aid. The Bennett hypothesis assumes that colleges attempt to maximize their utility by raising their tuition in response to increases in government financial aid, up to the point where it does not harm their reputation. However, my study shows that colleges in many states did not significantly increase their tuition and fees. Moreover, except in a couple of states, colleges were not responsive to the incentive embedded in their state's merit-based aid programs. This result further calls into question the validity of the Bennett hypothesis in the

context of merit-based aid. The fact that some states intentionally kept college costs low at their public colleges suggests that individual institutions might not be capable of raising their tuition even if they would like to.

All in all, my study demonstrates that there is little empirical evidence to support the Bennett hypothesis. As states started new aid programs, students with solid academic records have received monetary subsidies to pay for college education. State legislatures can also see that the money they invested in merit-based aid programs does not result in unintended consequences such as tuition increases. This study contributes to the literature by testing the Bennett hypothesis using all thirteen states that have implemented merit-based aid. In particular, results from my study illustrate that academic eligibilities as well as award amounts of merit-based aid programs are widely varied across states, and so does the way colleges respond to merit-based aid. My results suggest that it is important to consider a merit-based aid program in each state separate and unique rather than treat these programs as a similar one.

Although my study adds to the literature, there are some limitations that my study does not address. First, my study does not examine why colleges respond to merit-based aid in certain ways. Although I explore possible explanations such as governance structure, testing these possibilities is out of the scope of my study. Future research that explores these potential factors will improve our understanding of the economic behavior of colleges. Second, it is possible that other factors related to tuition or college finance could have occurred in the treatment states at the same time merit-based aid was adopted. For example, a state might have adopted performance-based funding in the same year it implemented its merit-based aid program. If this were the case, it also could have affected my estimates. Third, although I find increases in institutional grants per student in many states, it is not yet clear to whom colleges in these states distributed the money. Did colleges spend their money on students who already received merit-based aid to complement their unmet needs, or on students who were not eligible for merit-based aid such as low-achieving students or nonresident students? Exploring these questions will provide another important, but mostly missing, piece to the puzzle of how colleges respond to increased government aid.

Lastly, I cannot entirely rule out the possibility that my treatment states (states that have adopted merit-based aid) tend to change their prices more than the control states regardless of their state merit-based aid policy. In Appendix A, I present the results from the falsification test that examines whether colleges in each of the treatment states significantly changed their prices more than their control groups at least six years before or after their state merit-based aid was implemented.[15] Although estimates are widely varied across states, estimates in some states are still statistically significant and consistent to the main results. These results suggest that colleges in the treatment states were more likely to raise their student charges or decrease the amount of institutional aid per student even when merit-based aid was not available, and the main results might just reflect these overall trends. The results from the falsification test again support the main conclusion of this study: the adoption of merit-based aid does not necessarily lead colleges in most states to take advantage of their state merit-based aid by raising college prices.

[15] I first set the false implementation year as a few years before the actual implementation year in each state because post-policy outcome variables could have still been affected by the policy. However, this approach does not work for states that adopted merit-based aid policy in the early 1990s (e.g., Arkansas, Georgia, etc.) due to the data availability. Therefore, I set a false implementation year that is at least six years far from the actual implementation year either before or after the policy.

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Appendix A

Table A.1. Falsification Test Results (Public 4-Year Colleges)

State (Available Years)	Control States	Tuition & Fees (1)	Institutional Grants (2)	Room & Board Charges (3)
Arkansas	RC	57%	N/A	0%
(7 years)	All	57%	N/A	0%
Florida	RC	100%	0%	N/A
(1 year)	All	N/A	0%	0%
Georgia	RC	100%	0%	80%
(5 years)	All	100%	0%	40%
Kentucky	RC	100%	N/A	N/A
(2 years)	All	100%	N/A	N/A
Louisiana	RC	N/A	N/A	0%
(1 year)	All	N/A	N/A	0%
Massachusetts	RC	13%	13%	0%
(8 years)	All	38%	88%	25%
Michigan	RC	N/A	N/A	0%
(3 years)	All	33%	N/A	0%
Mississippi	RC	100%	0%	N/A
(2 years)	All	100%	0%	N/A
Nevada	RC	0%	0%	0%
(3 years)	All	N/A	0%	0%
New Mexico	RC	N/A	N/A	0%
(1 year)	All	100%	0%	N/A
South Carolina	RC	0%	N/A	0%
(1 year)	All	0%	N/A	0%
Tennessee	RC	N/A	N/A	50%
(7 years)	All	N/A	N/A	43%
West Virginia	RC	N/A	N/A	0%
(5 years)	All	N/A	0%	0%

Note: I run the same model as specified in equation (1) for all years available at least six years far from its actual implementation year. For example, Arkansas implemented merit-based aid policy in 1991. Thus, I set its false implementation year as every year from 1998 to 2004, run my model for each of the false years, and see if the result is consistent to the main result. The percentage terms in the table indicate the percentage of false years that show consistent results among all available false years. RC means that I compare colleges in the treatment state to colleges in states that belong to the same regional compact and that had not adopted merit-based aid during the time period. N/A refers to "not applicable" because the main result is not statistically significant.

Table A.2. Falsification Test Results (Private 4-Year Colleges)

State (Available Years)	Control Groups	Tuition & Fees (1)	Institutional Grants (2)	Room & Board Charges (3)
Arkansas	RC	86%	57%	86%
(7 years)	All	86%	43%	100%
Florida	RC	N/A	0%	N/A
(1 year)	All	0%	0%	0%
Georgia	RC	40%	20%	20%
(5 years)	All	40%	20%	20%
Kentucky	RC	N/A	N/A	0%
(2 years)	All	100%	N/A	0%
Louisiana	RC	N/A	0%	N/A
(1 year)	All	0%	0%	N/A
Massachusetts	RC	N/A	N/A	N/A
(8 years)	All	63%	13%	63%
Michigan	RC	0%	100%	N/A
(3 years)	All	0%	100%	33%
Mississippi	RC	N/A	0%	100%
(2 years)	All	N/A	0%	100%
Nevada	RC	100%	100%	N/A
(3 years)	All	100%	67%	N/A
New Mexico	RC	100%	100%	0%
(1 year)	All	100%	100%	0%
South Carolina	RC	N/A	100%	0%
(1 year)	All	0%	100%	0%
Tennessee	RC	0%	N/A	57%
(7 years)	All	N/A	57%	N/A
West Virginia	RC	N/A	40%	N/A
(5 years)	All	100%	40%	N/A

Note: I run the same model as specified in equation (1) for all years available at least six years far from its actual implementation year. For example, Arkansas implemented merit-based aid policy in 1991. Thus, I set its false implementation year as every year from 1998 to 2004, run my model for each of the false years, and see if the result is consistent to the main result. The percentage terms in the table indicate the percentage of false years that show consistent results among all available false years. RC means that I compare colleges in the treatment state to colleges in states that belong to the same regional compact and that had not adopted merit-based aid during the time period. N/A refers to "not applicable" because the main result is not statistically significant.

CHAPTER III

Does Merit-Based Aid Promote Degree Attainment?

Abstract

This study examines whether receiving merit-based aid affects the probability of earning a bachelor's degree within six years. Using an example of Tennessee, I compare students who received the state's merit-based aid to students who did not. I employ the regression discontinuity method, which minimizes pre-existing differences between scholarship recipients and non-recipients. Results show that receiving merit-based aid increases the probability of earning a bachelor's degree in the fourth year. However, it does not affect whether a student obtains the degree or not in the fifth or sixth year. Possible explanations as to why merit-based aid does not affect degree attainment will be discussed.

Key words: merit-based aid, graduation, time to degree

Introduction

Since the early 1990s, more than a dozen states have adopted state-funded merit-based aid. By providing funding for merit-base aid, states aim to keep the bright and best students within their states and produce college-educated workers who are critical for their economy (Hu, Trengove, & Zhang, 2012). Many researchers agree that state-funded merit-based aid increases college enrollment for its state residents at the entry level (Cornwell, Mustard, & Sridhar, 2006; Dynarski, 2002; Goodman, 2008). However, its effect on persistence graduation has received less attention (Dynarski, 2005; Scott-Clayton, 2011).

College graduation is important not only for students but also for states. It is widely known that those with a college degree enjoy higher wages in the labor market compared to those without a college degree (Card,

1999; Kane & Rouse, 1995). From the perspective of states, it is also important to ensure that their students earn college degrees given the vast amount of state funding spent on public colleges and student aid (NASSGAP, 2011).

Despite the importance of postsecondary educational attainment, relatively little is known about the relationship between financial aid and college graduation. There is a long line of research about the effect of financial aid on either initial enrollment or year-to-year persistence (Heller, 1997; Leslie & Brinkman, 1987). However, most of the studies do not investigate if the initial enrollment or persistence translates into graduation. Moreover, there are mixed results about the effect of financial aid on persistence and graduation (Hossler et al., 2009). Some researchers find positive effects (e.g., Dynarski, 2003), while others do not find any significant impact (e.g., Castlemen, Long, & Willett, 2010).

In this study, I examine whether or not receiving merit-based aid affects graduation within six years in the case of Tennessee. In 2004, Tennessee implemented its merit-based aid, Tennessee Educational Lottery Scholarships (TELS). Among the fall 2004 entering cohort, students who scored a 19 on ACT or received a B grade in high school were eligible for the scholarships. As the scholarships were awarded based on pre-determined scores, I employ the regression discontinuity method that reduces selection bias by focusing on students near the cut-off scores. I find that receiving merit-based aid does not affect whether or not students earn a bachelor's degree anytime within six years. Instead, it appears to increase the probability of attaining the degree in the fourth year.

This paper is organized as follows. I first introduce Tennessee Educational Lottery Scholarships as background information. Then, I briefly explain the theoretical background of this study, the human capital theory, and summarize previous studies that examine the effect of financial aid (especially, merit-based aid) on degree attainment. In the next section, I describe research methods, data, and sample used in this study. Then, I present my results and conclusions.

Tennessee Educational Lottery Scholarships

In Tennessee, TELS was first awarded for the fall 2004 incoming cohort. It consists of five sub-categories based on a student's family income and academic achievement. Of the five programs, the Wilder-Naifeh program is available only for students enrolled in Tennessee Technological Centers. The other four programs are available for state residents who are enrolled at least part-time in one of the state's public or private colleges. To be eligible, students were required to have at least a 19 ACT score or a 3.0 high school GPA. An additional $1,000 was awarded for either highest-achieving students who received a 27 ACT score and a 3.75 GPA or low-income students whose annual family income was below $36,000.[16] Once students receive the scholarships, they must renew their scholarship eligibility periodically by maintaining a 3.0 cumulative GPA. Students can receive the scholarships for up to five years or 120 college credits, whichever comes first.

Compared to other states' merit-based aid, TELS is unique in terms of its initial academic requirements and income supplements. Although many states require both standardized test scores and high school grades in order for students to be eligible for their merit-based aid, students in Tennessee are required to meet only one of the standards. In the first year of implementation, the ACT cut-off score in Tennessee (19) was even lower than that in neighboring states, such as Florida (20), Louisiana (20), or West Virginia (22). In addition, even if students failed to meet the base criteria, there is a second chance for students from low-income families. The Access grant, one of the sub-categories of TELS, is only available for low-income students who received at least a score of 18 on the ACT and a 2.75 high school GPA. As its name suggests, this program aims to promote college access for low-income students by slightly lowering the academic standards required for TELS. Under the former rules, low-income students who met the base criteria (either a 19 ACT score or a 3.0 high school GPA) received the ASPIRE grant which awarded an additional $1,000 per year.

[16] The awarding rules have changed since 2005. In 2005, the ACT cut-off score was raised from 19 to 21. The additional award amount for low-income students was also increased from $1,000 to $1,500. Because I look at the fall 2004 cohort in my study, I use the former rules throughout the study.

The relatively less rigorous academic standards and the provision of income supplements are designed to address the major weakness of merit-based aid: the disproportionate distribution of the aid to students from middle- and high-income families (Heller & Marin, 2002; 2004). Of TELS recipients who entered public four-year colleges in Fall 2004, approximately 27% of students were from low-income families and received either the ASPIRE or the Access grant (THEC, 2011).[17] However, it is not clear whether these scholarships help students succeed in college. The six-year graduation rates of the ASPIRE and Access grants recipients are 44% and 30%, respectively. These are lower than the state average graduation rates by more than 10%.

Theoretical Background

The human capital theory is used as a theoretical background for this study. Based on economic theory, this framework assumes that students are rational actors who pursue maximizing their utility among different sets of choices such as whether or not to go to college. In other words, students consider benefits and costs of each alternative and choose the one that satisfies them the most. Although utility includes both monetary and non-monetary values, most research using the human capital framework focuses on monetary benefits and costs of college education. This calculation can be simplified as equation (1).

$$\sum_{t=s+1}^{t} \frac{B_t}{(1+i)^t} - \sum_{i=1}^{s} \frac{C_t}{(1+i)^t} \geq 0 \qquad (1)$$

Equation (1) illustrates future earnings and costs associated with college education. When a student has *s* years of college education, this generates direct and indirect costs of college education, C_t. Direct costs include tuition, fees, and living expenses, while indirect costs mean a student's foregone earnings, which (s)he would have earned if (s)he had a job right after graduating from high school. After *s* years of college education, (s)he expects annual earnings of B_t until (s)he retires in year *t*. Because both benefits and costs of college education

[17] The statistics in this paragraph are calculated by the author based on Table 51 in the state's annual report (THEC, 2011, p. 65).

occur in the future, both of them are discounted at a rate of *i*, which is equivalent to an interest rate. The human capital theory assumes that students can borrow money at a rate of *i* from the market to invest in their education (Schultz, 1961).

When students decide whether to pursue another year of college education, they weigh the present value of the benefits to the present value of costs associated with another year of education. Then, students decide to attend another year of education only if the benefits are larger than the costs. In this study, receiving statewide merit-based aid will increase the estimate of equation (1) by reducing the direct costs of college education (C_t). When other things are held constant, I hypothesize that receiving merit-based aid will lead students to attend another year of college.

Literature Review

There are several ways that financial aid promotes college participation and graduation. Most importantly, financial aid reduces direct costs of college education by providing students with monetary support to pay their tuition. This monetary support allows students to postpone or reduce their labor market participation and spend more time on campus. As students spend more time on campus, they can put more effort into academic activities and interact more with their peers and faculty members. Therefore, students are academically and socially integrated into their institutions, which eventually leads to their college success (Tinto, 2010). If a student received institutional aid, it would also convey the message that the student is valued in her institution. This can have positive psychological effects on the student and increase the student's loyalty to her institution (DesJardins, Ahlburg, & McCall, 2002).

In addition to the monetary, social, and psychological frameworks, Scott-Clayton (2011) demonstrates that state merit-based aid incentivizes students to work hard in college. Because most state merit-based aid programs require students to renew their scholarship eligibility by maintaining their cumulative GPA above a certain level, students put effort into getting good grades in class, which in turn leads to their persistence and

on-time graduation. However, Binder, Ganderton, and Hutchens (2002) propose that the renewal requirement might discourage students who fail to renew their eligibility from pursuing further education due either to financial or psychological costs. This is plausible especially if a state sets a relatively lower initial requirement and induces students who would not have attended college at all to attend one (or students who would have gone to two-year colleges to attend four-year colleges).

To date, there are only a few studies that directly examine the effect of merit-based aid on graduation. Scott-Clayton (2011) examines the effect of West Virginia's PROMISE scholarship on bachelor's degree attainment. Using regression discontinuity and cohort analysis methods, she finds that scholarship recipients were six to nine percent more likely to earn a bachelor's degree than non-recipients were. She argues that at least part of the positive effect can be attributed to the incentive embedded in the PROMISE scholarship. In addition to maintaining a 3.0 cumulative GPA, students are required to take 30 credits per year to renew their scholarship eligibility, and they can renew it for up to four years. If it adds up, students can complete 120 credits within four years, which is generally required to earn a bachelor's degree. As evidence of this incentive working, she shows that scholarship recipients were more likely to take 30 credits per year and maintained a 3.0 GPA than non-recipients were. However, these positive outcomes are only observed until the end of students' junior year because they could not renew their eligibility anyway after that time. Her study indicates that scholarship recipients respond to incentive generated from their merit-based aid design, and it helps them successfully graduate from college on time.

Henry, Rubenstein, and Bugler (2004) examine whether receiving HOPE scholarships affects college GPAs, the number of college credits, persistence,[18] and four-year graduation. In order to minimize selection bias, the authors focus on students whose high school GPAs were close to the cut-off GPAs required for the scholarships and match recipients with non-recipients in terms of high school GPAs and institutions they attended. The authors find that scholarship recipients earned more credits, had higher GPAs, and were more

[18] In the article, persistence is defined as being enrolled in one of the public colleges in Georgia without graduating from college within four years of initial enrollment.

likely to graduate than non-recipients. However, the positive impact on graduation disappears if students who initially received the scholarships lost them later. Results from this study show that the actual effect of state merit-based aid on graduation could be smaller than its intended effects because many students fail to renew their scholarship eligibility and lose it.

Castleman (2013) examines whether the different amount of merit-based aid makes a difference in credits completed and degree attainment. Florida's Bright Future Scholarships (BFS) is a two-tiered program that covers either 100% (the Florida Academic Scholars) or 75% (the Florida Medallion Scholars) of tuition and fees in the state's public colleges depending on students' academic achievement. Using the difference-in-differences method, the author compares the outcomes (credits completed and degree attainment) of three different cohorts who graduated from high schools in the state and attended in-state public colleges. He finds that there is almost no difference in the outcomes between non-recipients and FMS recipients even after the BFS was implemented. In contrast, FAS recipients completed more college courses and were more likely to earn a college degree than FMS recipients, after the implementation of the scholarships. That is, awarding 100% subsidy significantly promoted degree attainment, while 75% subsidy did not. The author attributes the positive outcomes observed among FAS students to the change in student composition rather than the scholarship award itself. The BFS induced FAS-eligible students who would have gone to private or out-of-state colleges to their in-state public colleges. Therefore, the increased number of highest-achieving students in the state's public colleges contributed to the improved degree attainment probabilities.

Merit-based aid appears to increase college completion rates at the state level, too. Employing the difference-in-differences method to analyze the 2000 Census data, Dynarski (2005) finds that the percentage of young adults with at least an associate's degree has increased by three to four percent since the inception of merit-based aid in Arkansas and Georgia. The positive impact is larger for women than men. Since the policy was adopted, the percentage of young adults with bachelor's degrees has increased in all racial and gender groups except white male. Zhang (2011) also uses the difference-in-differences method to examine whether

merit-based aid programs in Georgia and Florida increased the number and percentage of bachelor's degree holders, especially focusing on STEM (Science, Technology, Engineering, and Math) fields. Georgia and Florida experienced 1.8% and 11.8% increases in the number of bachelor's degree holders, respectively. In addition, both states observed increases in the number of bachelor's degree holders in STEM as well as non-STEM majors. This result raises a question on the current argument that adopting merit-based aid discourages students from choosing STEM majors, in which it is more difficult to receive good grades.

Recently, there are a growing number of studies that employ experimental or quasi-experimental methods to analyze the persistence and graduation effects of need-based grants. However, their results regarding whether need-based aid promotes student success are mixed. For example, MDRC randomly assigned need-based grants to low-income college students depending on students' academic performances[19] at seven different research sites. Although it is too early to evaluate its impact on graduation in most sites, students who received the aid were more likely to earn an associate's degree than non-recipients in Ohio (Patel, Richburg-Hayes, Campa, & Rudd, 2013).[20] In contrast, Stinebrickner and Stinebrickner (2003) argue that financial aid is not enough to improve graduation. In Berea College where all students received subsidies for full tuition and a significant portion of room and board charges, the average graduation rate[21] was as low as 47%, which was slightly lower than the national average for not-for-profit four-year private colleges. Several studies using quasi-experimental methods find the positive impact of need-based aid on degree attainment (e.g., Dynarski, 2003), while others do not (e.g., Castlemen, Long, & Willett, 2010[22]).

To summarize, merit-based aid appears to have positive effects on degree attainment. However, there are only a few studies in this area, and most of them focus only on a few states with merit-based aid. If I look at

[19] Although the specific rules differ across research sites, students are usually required to enroll in part-time and maintain a C grade in each semester. Need-based grants are awarded three times within a semester as students meet these requirements throughout the semester.

[20] The MDRC interventions also include other components (e.g., counseling) in addition to financial aid. Thus, it is not clear whether all of the positive effect observed can be attributed to the need-based grants.

[21] In the research, the authors define graduation rates as the percentage of matriculating students who returned to their school at the seventh semester. The authors explain that almost every student who came back at the seventh semester eventually graduated (Stinebrickner & Stinebrickner, 2003, p. 598).

[22] The authors use both regression discontinuity and event history models, and find the positive impact of need-based grants only among high-achieving students in the event history model.

the question more broadly including need-based aid, it is not yet clear whether financial aid promotes degree attainment. My study adds to the literature by examining the effect of merit-based aid on graduation using the example of Tennessee. Tennessee is an interesting example especially because the state sets less rigorous standards and provides more generous funding for low-income students.

Data, Sample & Methods

I use administrative data in Tennessee for the fall 2004 entering cohort. The data contains information about students who entered one of the public four-year colleges in the state in fall 2004. Because the data covers all students in the state's public four-year colleges, I can track students as long as they remain in one of the state's public four-year colleges. The data provides information on students' demographics, parental educational levels, Pell Grants eligibility[23], amount of need- and merit-based aid from the state (Tennessee), institution's name, enrollment status, high school grades, and standardized test scores. This rich set of data allows me to control for factors that are known to affect college persistence and completion at both individual and institutional levels.

Among the fall 2004 cohort, I limit my sample to first-time freshmen students who graduated from high school no earlier than 2003 and who started at one of the public four-year colleges in Tennessee in the fall of 2004. I exclude non-traditional students (adult students who delayed their college entry) because they are different from traditional students in terms of demographic, enrollment, and course-taking patterns. These different characteristics are associated with persistence and graduation (Adelman, 2006). Moreover, for the fall 2004 cohort, adult students who delayed college entry more than 16 months of high school graduation were not eligible for TELS in the first place. I also limit my sample to students who were Tennessee residents and registered for at least six credits during their first semester. These two conditions (state residency and part-time

[23] Although the data set has an annual family income variable (a continuous variable), its value is missing for 43.5% of my sample. Due to the substantial number of missing values, I will use a variable that indicates whether or not a student is eligible for Pell Grants as a proxy for family income.

status) are basic requirements to be eligible for TELS. As a result, I have 14,391 first-time freshmen students in my sample. Table 1 provides the descriptive statistics for my sample broken down by their TELS receipt status.

Table 1. Descriptive Statistics for the Sample

	Not received	Received	Total
Female	0.551	0.551	0.551
	(0.498)	(0.497)	(0.497)
Racial Minority	0.433	0.183	0.213
	(0.496)	(0.387)	(0.409)
Pell-Eligible	0.591	0.294	0.320
	(0.492)	(0.456)	(0.467)
College-Educated	0.439	0.689	0.659
Parent(s)	(0.496)	(0.463)	(0.474)
High School GPA	2.809	3.329	3.270
	(0.488)	(0.499)	(0.525)
ACT score	18.83	22.95	22.47
	(3.686)	(3.746)	(3.965)
Graduation rates	0.253	0.546	0.511
(within six years)	(0.435)	(0.498)	(0.500)
Sample size	1,722	12,669	14,391
	(11.97%)	(84.85%)	(100%)

Note: When I calculated the average high school GPA, I dropped GED students. In the data set, their high school GPAs actually mean their GED scores.

According to Table 1, approximately 85% of my sample received TELS when they entered college. Overall, more than one-half of the sample is female students. 21% of the sample is racial minority students (African-American, Hispanic, Native American, and multiracial students), and 51% of the sample graduated within six years of their initial enrollment. When I compare TELS recipients to non-recipients, the two groups are different in terms of racial composition, parental education, family income, and academic performances. On

average, the proportion of racial minority and Pell-grants-eligible students is much higher among non-recipients than recipients. Non-recipients have relatively low high school GPAs, ACT scores, and graduation rates. Because the two groups are very different in terms of family background and academic performances, the graduation rate gap between the two groups cannot be solely attributed to TELS.

Using the sample, I use logistic regression and regression discontinuity models to examine whether receiving merit-based aid affects graduation in a given year. In order to examine the effect of merit-based aid, I first compare scholarship recipients to non-recipients using a logistic regression model as specified in equation (2).

$$\Pr(y_i = 1) = \frac{1}{1 + \exp^{-(\alpha + \beta(TELS_i) + \delta X_i)}} \tag{2}$$

In equation (2), the outcome variable ($\Pr(y_i = 1)$) is the probability of a student earning a bachelor's degree within six years. In addition, I also examine if a student earned the degree at the fourth, fifth, and sixth year after their initial enrollment in order to see if merit-based aid affects the duration it takes to earn a bachelor's degree. Throughout the study, whether students graduated from their first institution does not matter for two reasons. First, a large number of students attend more than one institution throughout their college years (Adelman, 2006). Excluding these students from the sample would underestimate the number of college graduates. Second, students are eligible for TELS as long as they remain in in-state colleges. Hence, any potential effect of TELS would be observed even if students transferred to another institution in the state.

The key independent variable ($TELS_i$) takes on a value of one if a student received one of the TELS awards in his/her first year and zero otherwise. β refers to the effect of receiving the scholarships on graduation. Because some of the initial recipients lost their award after a couple of years, the estimate is an intent-to-treat estimate and could be smaller than treatment-on-the-treated estimate. I also add a vector of

covariates (X_i) to take into account effects of a student's demographics, family background, and academic performances (Adelman, 2006). Table 2 summarizes the variables used in this study.

Table 2. Descriptions of Variables Used

Type	Variable	Description
Outcome Variable	Graduation	1: If a student earns a bachelor's degree within six years of initial enrollment 0: Otherwise
Independent Variable	Receiving TELS	1: If a student received one type of the TELS awards in their first year 0: Otherwise
Assignment Variables	ACT scores	Continuous variable that indicates a student's ACT composite score (centered at 19)
	High School GPA	Continuous variable that indicates a student's cumulative high school GPA (centered at 3.0)
Control variables	Female	1: Female 0: Male
	Racial Minority	1: African-American, Hispanic, Native American, or Multiracial students 0: White or Asian students
	Pell Grants Eligibility	1: Being eligible for Pell Grants 0: Otherwise
	Parental Education	1: At least one parent has some college experience 0: Neither parents has attended college
	Institutions	A binary variable that indicates the institution in which a student was enrolled during his/her first year

Although the logistic model estimates the effect of receiving merit-based aid after taking into account a student's family background and academic achievement, the estimate still reflects unobservable differences

between recipients and non-recipients. For example, scholarship recipients with a 30 ACT score and a 4.0 high school GPA might be more motivated than non-recipients with a 16 ACT score and a 2.0 high school GPA. These students' different levels of motivation could affect their college outcomes, including graduation. Therefore, I use the regression discontinuity model that minimizes both observable and non-observable differences between the two groups by comparing students in the two groups who are close to the cut-off score.

The regression discontinuity method is used when treatment is assigned based on a pre-determined arbitrary rule, such as the TELS eligibility requirements in Tennessee (Schneider et al., 2007). In order to be eligible for TELS, students whose family income was above $36,000 had to score a 19 on the ACT exam or receive a 3.0 high school GPA. Students who did not meet the GPA standard received the scholarships if their ACT score was at or above 19. However, the cut-off score of 19 is somewhat arbitrary. In the absence of the scholarships, there is no reason to believe that students with a 19 ACT score would be considerably more likely to graduate than students with an 18 ACT score. The former would do a little better than the latter because standardized test scores indicate academic capability to some extent; however, there would not be a clear gap in graduation probabilities between the two groups in the absence of the scholarships. This continuity in an outcome variable is a key assumption of the regression discontinuity method (Imbens & Lemieux, 2008).

The continuity assumption cannot be directly tested because it is impossible to observe what would have happened in the absence of the treatment(in this study, the TELS award). Instead, researchers test whether students on both sides are similar except for the treatment, given that the cut-off score is arbitrary. If students are identical in many ways except for whether they receive the treatment, any difference in their outcome can be attributed to the treatment. However, if students are significantly different in many ways, these different characteristics will affect their outcome as well, which makes it difficult to find the net effect of the treatment. In order to test whether students are similar, I create several plots of each of covariates against each of the assignment variables (ACT score and high school GPA).

Second, when using the regression discontinuity method, it is important to ensure that students are not able to fully control their assignment variable (McCrary, 2008). For example, some students in my study who were especially determined to receive the scholarships may have put in extra effort to meet the criteria and consequently received them. If this happened, students who met the criteria by a slight margin would be different from students who just missed them. Hence, comparing these two students might overestimate the effects of the scholarships. In order to see if there is evidence of student manipulation, I run a McCrary test on both ACT scores and high school GPAs. I present these test results in the results section.

Lastly, it is important to choose an appropriate bandwidth. A narrow bandwidth minimizes bias by including students who are very similar to each other, while a wide bandwidth improves precision by increasing the sample size used in analysis. Jacob and Zhu (2012) introduce two procedures that are commonly used to find optimal bandwidths: the cross-validation procedure and the plug-in procedure. After conducting simulations, the authors report that estimates from both approaches are qualitatively similar, even if the optimal bandwidths are different. In this study, I utilize both procedures[24] and find that my estimates are similar in both approaches. Hence, I report my estimates using the optimal bandwidth calculated through the cross-validation approach. I also run my model using 50% and 200% of the optimal bandwidth to see if my estimates are sensitive to the bandwidth selection.

There are two types of regression discontinuity models: a sharp regression discontinuity model and a fuzzy regression discontinuity model. In this study, I use the fuzzy regression discontinuity model. A fuzzy model is used when receiving treatment is not fully determined by assignment variables (Imbens & Lemieux, 2008). For example, it is possible, although very unlikely, that some students who met all the requirements chose not to receive the scholarships for some reason. In my study, 6% of students fall into this category. More importantly, I employ the fuzzy model because I have to exclude Access grantees from my study. As previously

[24] I follow Imbens and Lemieux (2008) for the cross-validation approach and Imbens and Kalyanaraman (2009) for the plug-in procedure. The latter is the default option for the rd command in STATA. When I use ACT scores as a forcing variable, I use a slightly larger bandwidth than the optimal bandwidths calculated using the two procedures, not only because the procedures are designed for the sharp regression discontinuity model but also because the sample size is too small within the optimal bandwidths.

mentioned, students whose annual family income was below $36,000 could be eligible for TELS if they received a 18 on the ACT exam and a 2.75 high school GPA. I first attempt to compare these students to other low-income students who just missed the Access grant. Unfortunately, the family income variable is missing for 43% of my sample, which makes it difficult to identify the counterfactual (students who would have received the Access grant if they had met the academic criteria). Moreover, there are only 53 students who actually received the Access grant among the fall 2004 cohort. This number amounts to 0.37% of my sample. Therefore, it appears that excluding the Access grantees from my study does not significantly change the estimates. After excluding these students, there are still a few students who received the scholarships even though they failed to meet the academic criteria. The fuzzy model is also appropriate when there are non-compliance cases such as these students.

The fuzzy model assumes that the academic criteria (ACT score and high school GPA) predict whether students received one of the awards, but do not perfectly determine it. The fuzzy model is estimated using the two-stage least squares model that is mathematically equivalent to the instrumental variable model (Imbens & Lemieux, 2008). In the first stage, as specified in equation (3), I predict D_i, which indicates whether students received one of the TELS awards (excluding the Access grant). This is predicted by whether students met either one of the academic requirements ($Above_i$). The case of Tennessee is somewhat complicated because students were required to meet either one of the criteria, not both. Hence, I limit my sample to students who failed to meet one requirement (e.g., GPA) and then look at students who met or missed on the other requirement (e.g., ACT) by a slight margin. In case the relationship between the running variables and the outcome variable is quadratic rather than linear, I include the square term of each of the running variables in the model. Lastly, I include a set of covariates (X_i) that are included in the logistic model as in equation (2). Although adding covariates does not change the point estimate, it generally increases precision of the estimates (Imbens & Lemieux, 2008).

Equation (4) shows the second-stage model. In the second stage, I use the predicted value of receiving the scholarships ($\hat{D}_i$) and estimate whether receiving the scholarships has any impact on graduation (Y_i). If receiving the scholarships has a positive influence on graduation, α_1 will be positive and significant. If it does not have an impact, α_1 will not be statistically different from zero. Again, I include my running variables and a set of covariates (X_i) that take into account the effects of student and institutional characteristics.

$$D_i = B_0 + B_1(Above_i) + X_i\delta + \varepsilon_i \quad (3)$$

$$Y_i = \alpha_0 + \alpha_1(\hat{D}_i) + X_i\gamma + u_i \quad (4)$$

There are several limitations of my study. Above all, estimates from this study may not be interpreted as causal, due to the difference between scholarship recipients and non-recipients. As I will explain in the next section, there is evidence of student manipulation in my data. This indicates that students on either side of the threshold could be different in terms of unobservable factors. In addition, I also find that racial minority students and low-income students are better represented among non-recipients than recipients. To the extent that these two groups are different, the regression discontinuity model in my study significantly reduces, but does not entirely remove, potential selection bias. Second, results from this study may not be applicable to students at the very top or bottom of the ACT score (or high school GPA) distribution. Because the regression discontinuity model focuses on a narrow range of students who met or missed the academic criteria by a slight margin, my results may not be generalizable to students who do very well or poorly in school. Third, there is a measurement error in the key independent variable. Although it is fairly accurate for most students, I find that some students who are coded as non-recipients lose their aid after their first year. I treat these students as scholarship recipients because it is impossible to lose a scholarship unless they have already received it. If my identification of scholarship recipients was incorrect, my estimates may have been biased downward. Lastly, this study focuses on the effect of receiving a specific financial aid program, TELS, on graduation rather than considering

other relevant predictors such as students' academic or social integration to their institutions. However, even when students received the scholarships, their college experiences could be very different from each other, and their different experiences could lead to different graduation outcomes. Due to data availability, I leave the effect of these relevant predictors as a limitation of this study.

Results

I first present estimates from the logistic regression model, and then proceed to results from the regression discontinuity model. Table 3 presents my results from the logistic regression model. In order to compare coefficients from the logistic regression model to the ones from the regression discontinuity model, I present raw coefficients in Table 3.[25] According to model 1, receiving TELS is positively associated with graduating any time within six years. Although the effect is large and statistically significant, it still reflects observable differences between recipients and non-recipients. To control for their observable differences, I add a variety of covariates in my model, as in model 5. In model 5, even after taking into account the observable differences between the two groups, receiving the scholarships is positively and significantly associated with graduating from college within six years. Although the magnitude of the coefficient shrinks in half, it is still statistically significant. That is, based on the logistic regression model, students who received TELS had higher probability of earning a bachelor's degree within six years than students who did not. This positive effect of receiving the scholarships is consistent even when I break down the graduation variable into fourth-year, fifth-year, and sixth-year graduation. The results for these three outcome variables are available upon request.

In addition to the key independent variable, other student characteristics are also significantly related to college graduation. Female students, students with at least one college-educated parent, and students with higher test scores or high school grades have higher probabilities of graduation than male students, first-generation students, and students with lower academic performances. Being racial minority or eligible for Pell

[25] For ease of interpretation, I also provide the same results in odds-ratio in Appendix B.

Grants are negatively associated with graduation. These results are consistent with previous studies (e.g., Adelman, 2006).

Table 3. Logistic Regression Results

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
TELS	1.272***	1.150***	1.221***	0.645***	0.645***
	(0.058)	(0.059)	(0.075)	(0.082)	(0.083)
Female		0.314***	0.365***	0.236***	0.270***
		(0.035)	(0.036)	(0.039)	(0.039)
Minority		-0.593***	-0.432***	-0.109*	-0.126*
		(0.044)	(0.047)	(0.051)	(0.054)
Pell Eligible			-0.560***	-0.524***	-0.525***
			(0.041)	(0.043)	(0.043)
Parental Education			0.364***	0.332***	0.307***
			(0.039)	(0.041)	(0.041)
HS GPA				1.074***	1.044***
				(0.042)	(0.042)
ACT Score				0.046***	0.040***
				(0.006)	(0.006)
Institution Dummy	No	No	No	No	Yes

Note: p-value: *: <0.05, **: <0.01, ***: <0.001
Note: 95% Confidence Intervals are reported in parentheses.

Although logistic regression results consistently report the positive impact of receiving TELS, the estimates still reflect unobservable differences (e.g., motivation) between recipients and non-recipients. Hence, I run the regression discontinuity model that focus on students near the cut-off score with an assumption that these students are very similar except whether they received the scholarships. Given the assumption, any difference observed in the outcome variable can be attributed to the scholarships.

Before presenting the regression discontinuity estimates, I examine if my data meets the key assumptions of the regression discontinuity method. First, I check whether the scholarships were awarded based on the rules. Because I limit my sample to students who met all basic eligibility requirements (e.g., residency) for TELS, the only reason for not receiving the scholarships should be either their test scores or high school GPAs. Overall, about 6% of eligible students did not receive the scholarships, and about 10% of students who failed to meet the academic requirements received them anyway. Considering that there is almost no way to receive the aid without satisfying the academic requirements, some of these students might be falsely identified due to measurement error that I discussed previously.

I then check to see if there is a significant break in the probability of receiving TELS near the cut-off scores. If the scholarships were awarded based on the eligibility requirements, the probability should jump from zero to one at the cut-off scores. Figures 1 and 2 provide that this is not always the case. Figure 1 shows the probability of receiving TELS among students who did not meet the high school GPA requirement. Because they failed to meet the GPA requirement, they had to score at least a 19 on the ACT exam to be eligible for TELS. In Figure 1, almost every student who met the ACT standard received the award. The probability is near one on the right side of the figure. In contrast, there are still many students who received the award anyway without satisfying the requirement. Although I see a big and clear gap in the probability at the cut-off score, it is still puzzling to see many non-compliance cases.

Figure 2 shows the probability of receiving TELS among students who failed to meet the ACT requirement. Again, because these students did not meet the ACT requirement, they had to have at least a 3.0 high school GPA to be eligible. Unfortunately, the non-compliance issue appears more serious in this case. Although there is a gap in the probability around the 3.0 cut-off point, many students below the threshold still received the aid.[26] Due to these non-compliance cases, I use the fuzzy discontinuity model. In addition,

[26] While I acknowledge the issue of non-compliance, it is noteworthy that the small sample size on the left side (ineligible students) makes the issue look more serious. Because it is expressed in terms of probabilities, only a couple of non-compliance cases out of a few students could result in high probabilities. This also partially explains why the non-compliance issue looks more serious in Figure

throughout the study, I focus on the model using ACT scores as an assignment variable than the one using high school grades because the former is more accurate in predicting whether students received the scholarships. Results from the latter model will be presented as complementary.

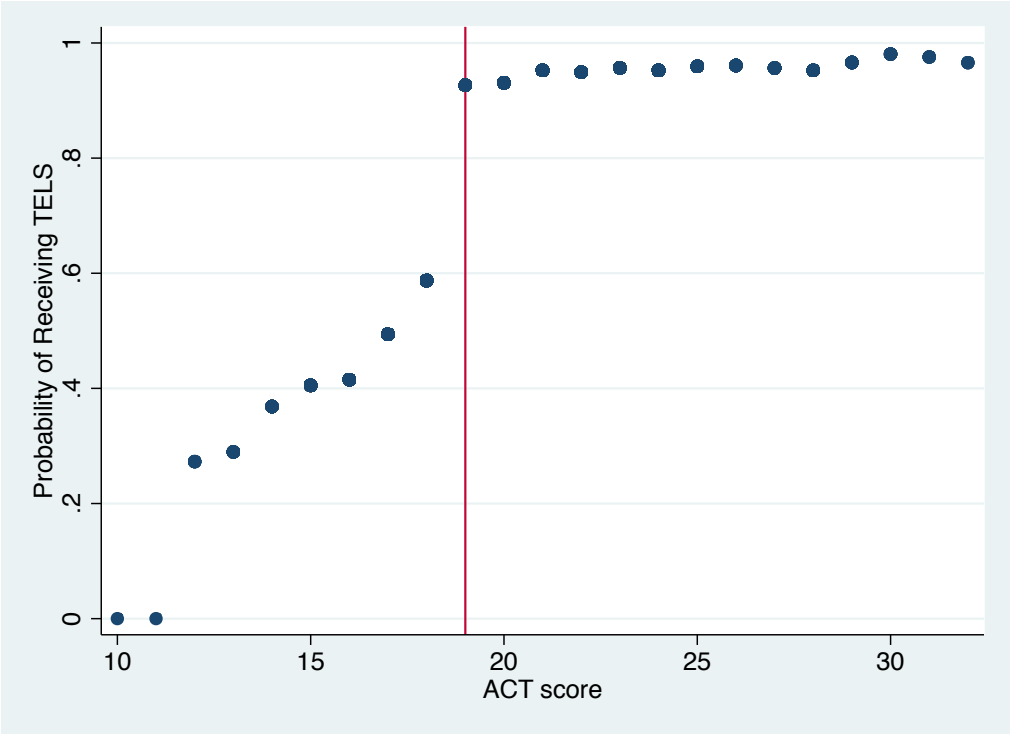

Figure 1. Probability of Receiving TELS based on ACT scores
(among students who did not meet the GPA requirement)

2. Because the unit of GPA is much smaller than that of ACT scores, the number of students in one unit of GPA (0.01) is much smaller than that of ACT scores (1 point).

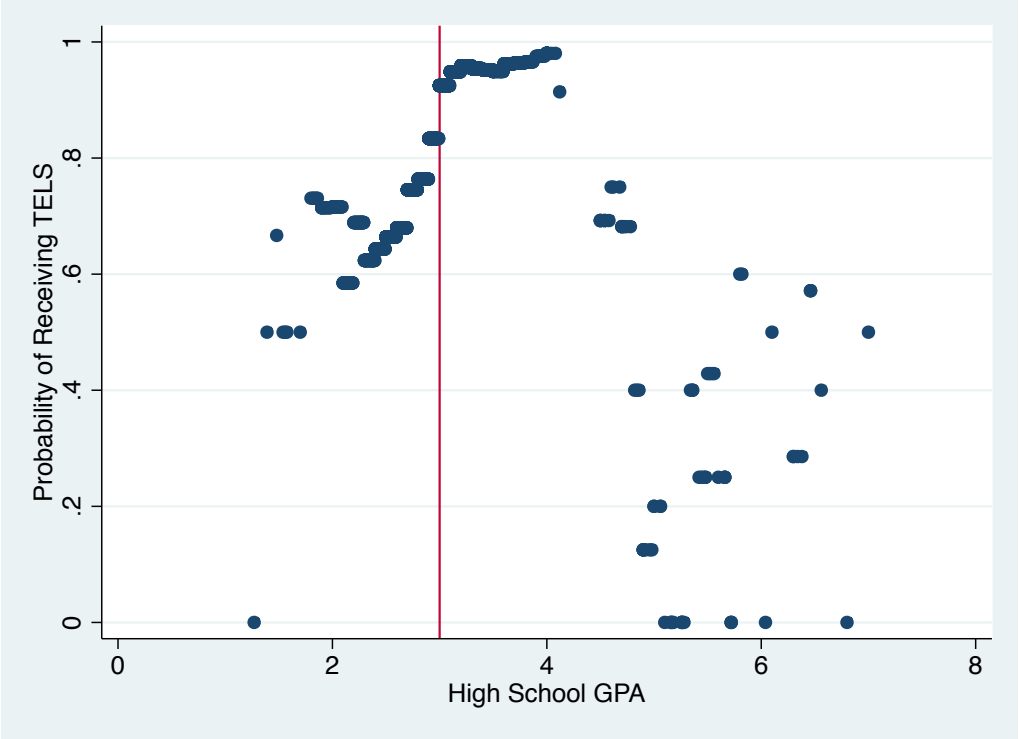

Figure 2. Probability of Receiving TELS based on High School GPAs
(among students who did not meet the ACT requirement)

Next, I also examine if there is evidence of student manipulation. I first present a density plot of ACT scores and high school GPAs as seen in Figures 3 and 4. In both cases, the density is approximately normally distributed, while it is peaked near the cut-off scores. In order to see if the density significantly jumps at the cut-off scores, I run the McCrary test (McCrary, 2008). Figures 5 and 6 as well as Table 4 suggest that the number of students suddenly increases right after the cut-off scores, which suggests possible student manipulation.

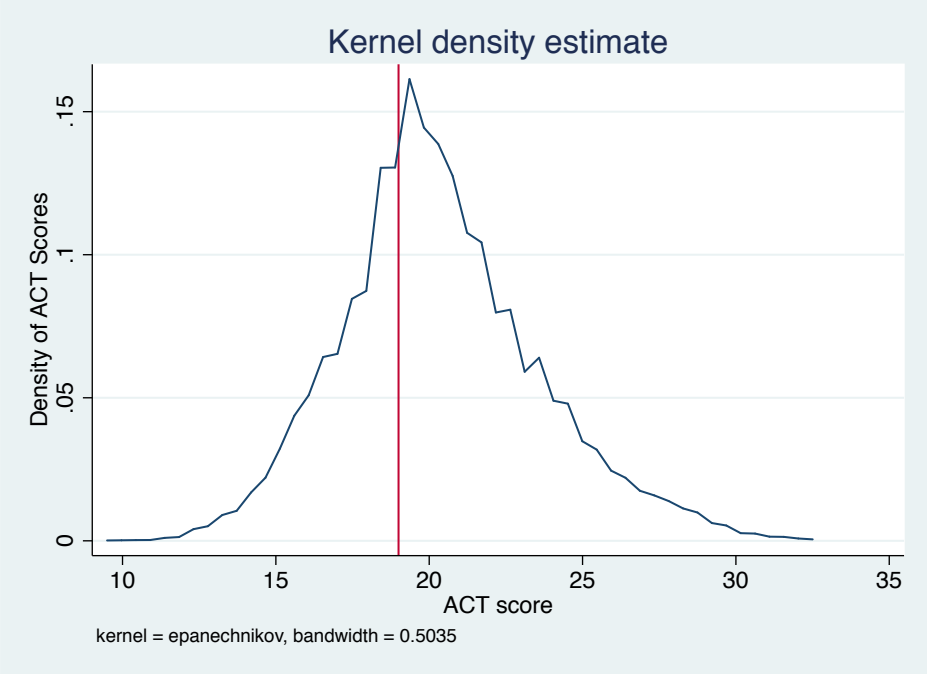

Figure 3. Density of ACT Scores
(among students who did not meet the GPA requirement)

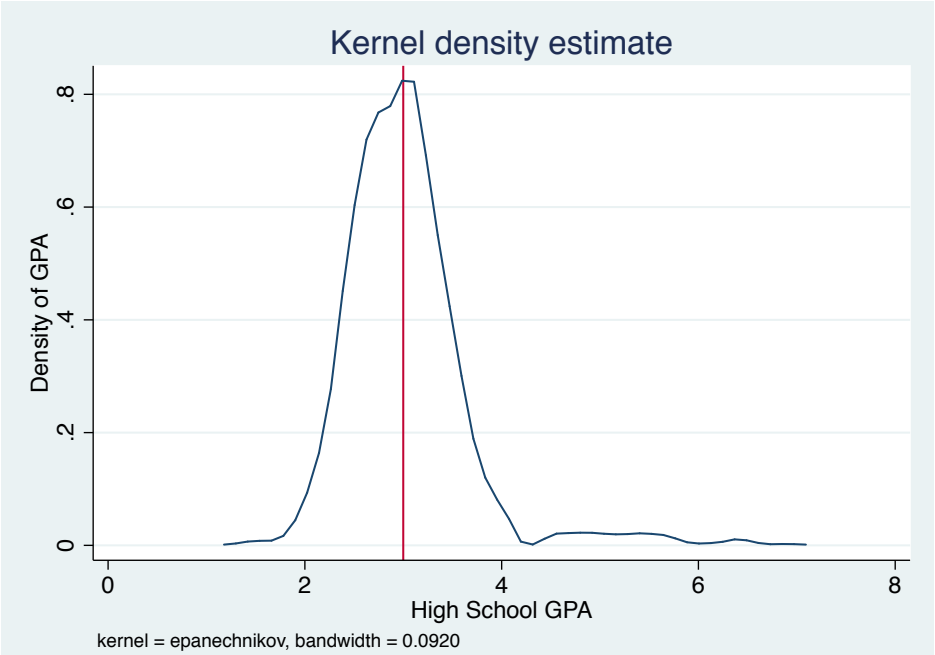

Figure 4. Density of High School GPA
(among students who did not meet the ACT requirement)

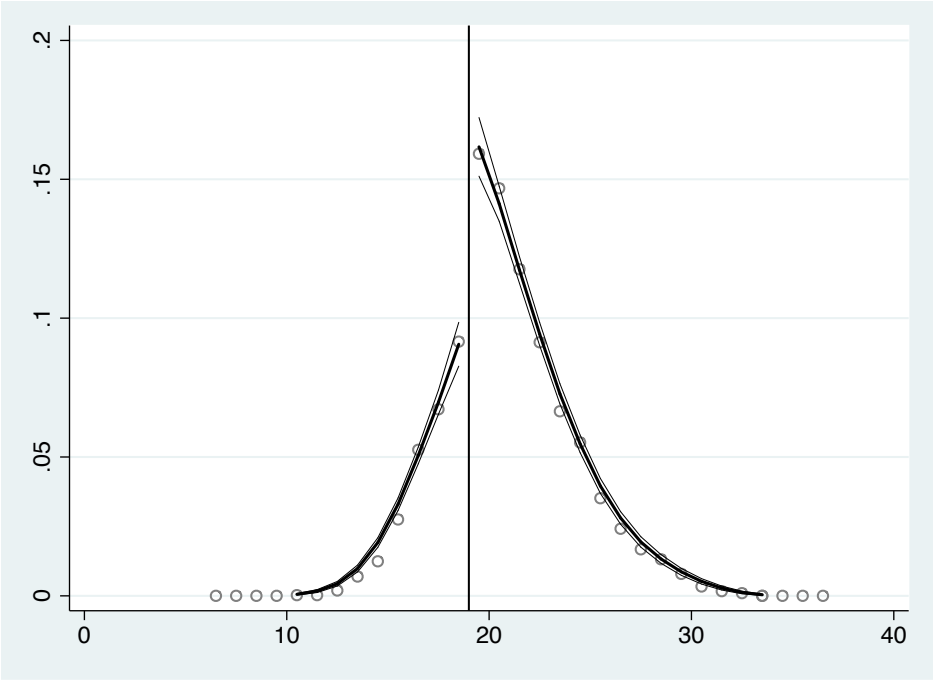

Figure 5. Density of ACT scores (McCrary test)
(among students who did not meet the GPA requirement)

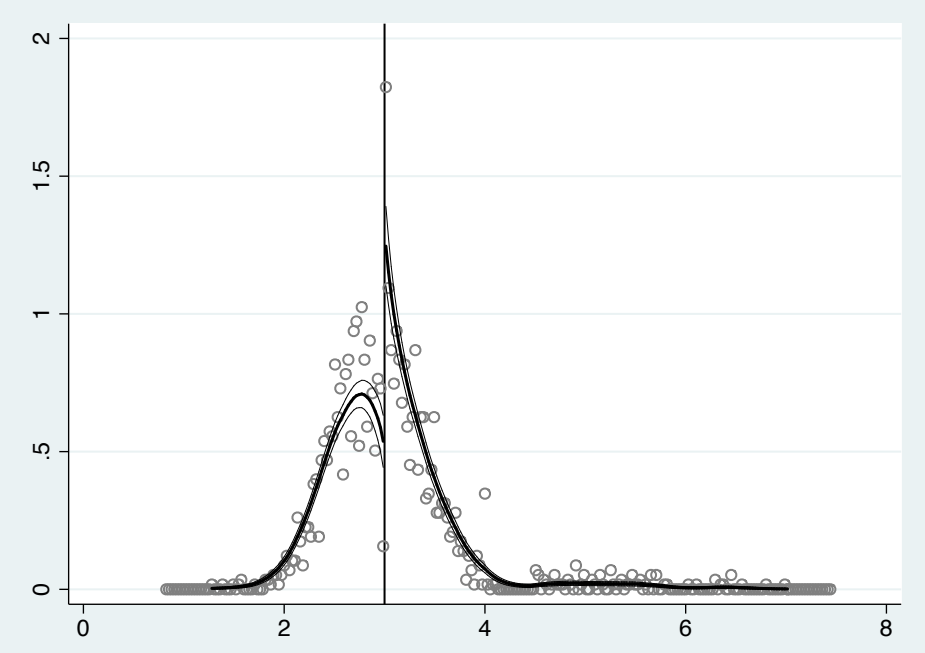

Figure 6. Density of High School GPA (McCrary test)
(among students who did not meet the ACT requirement)

Table 4. McCrary Test Results

Running variables	Discontinuity Height	Std. Error	t-value
ACT score	0.523	0.074	7.068***
High school GPA	0.915	0.116	7.888***

Although passing the McCrary test is "neither necessary nor sufficient"(McCrary, 2008, p. 701) for causal interpretation, I cannot rule out the possibility that my estimates could be confounded by differences between scholarship recipients and non-recipients. The issue of student manipulation is difficult to avoid if the assignment rules are publicly known, and the treatment is beneficial for agents, such as merit-based aid (McCrary, 2008). Although the TELS bill was passed just a few months before students in my sample entered college (January, 2004), it was still possible for some of them to take the ACT exam multiple times until they passed the threshold. Therefore, the regression discontinuity estimates of my study could substantially reduce selection bias, but not entirely remove it.

As a way to check if recipients are different from non-recipients, it is recommended that researchers create a plot of each of the covariates against a running variable, a so-called "balance test" (Imbens & Lemieux, 2008; van der Klaauw, 2012). If the plot of a covariate is discontinuous at the cut-off point, it indicates that recipients are different from non-recipients in terms of that covariate. However, even if researchers found a balance between two groups, it would be still possible that the two groups are different in unobservable factors.

Figures 7 and 8 show the plots against ACT scores and high school GPAs, respectively. The point zero on the x-axis indicates the cut-off scores. Assuming that students on both sides are quite similar, I expect to see that the plot does not suddenly jump or drop at the threshold. In Figure 7, I observe that the proportion of female students and that of the average high school GPA tend to be balanced around the threshold. However, the proportion of racial minority students and the proportion of Pell-eligible students decrease in a small step function. In terms of having a college-educated parent, the proportion increases continuously, but the slope is

rather steep. As a result, the proportion of students who have a college-educated parent seems quite different between recipients and non-recipients. Therefore, these three factors (racial minority, Pell Grants eligibility, and having a parent with college education) need to be controlled for in the discontinuity model.

In Figure 8, each of the covariates tends to change continuously and be balanced on either side of the cut-off point, except for the proportion of female students. Compared to Figure 7, the sample is more balanced in Figure 8 where the assignment variable is high school grades. This might be explained by the narrow optimal bandwidth with a high school GPA as a forcing variable. In Figure 8, the proportion of female students suddenly increases right after the cut-off point. This suggests that female students who failed to meet the ACT requirement might try hard to make the GPA requirement.

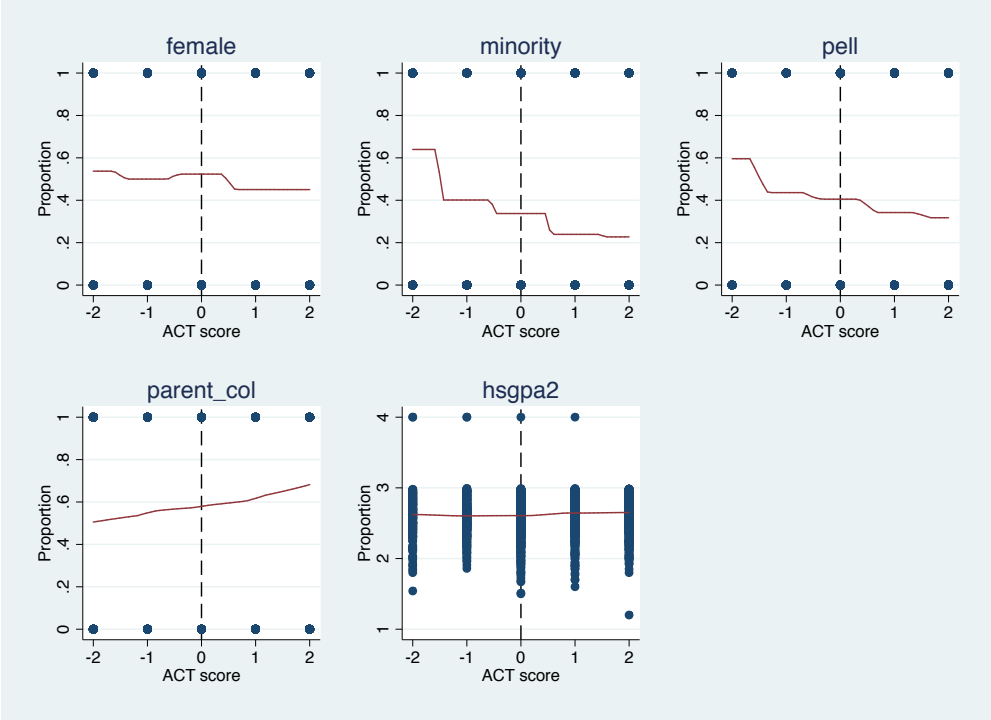

Figure 7. Discontinuity in covariates (ACT)

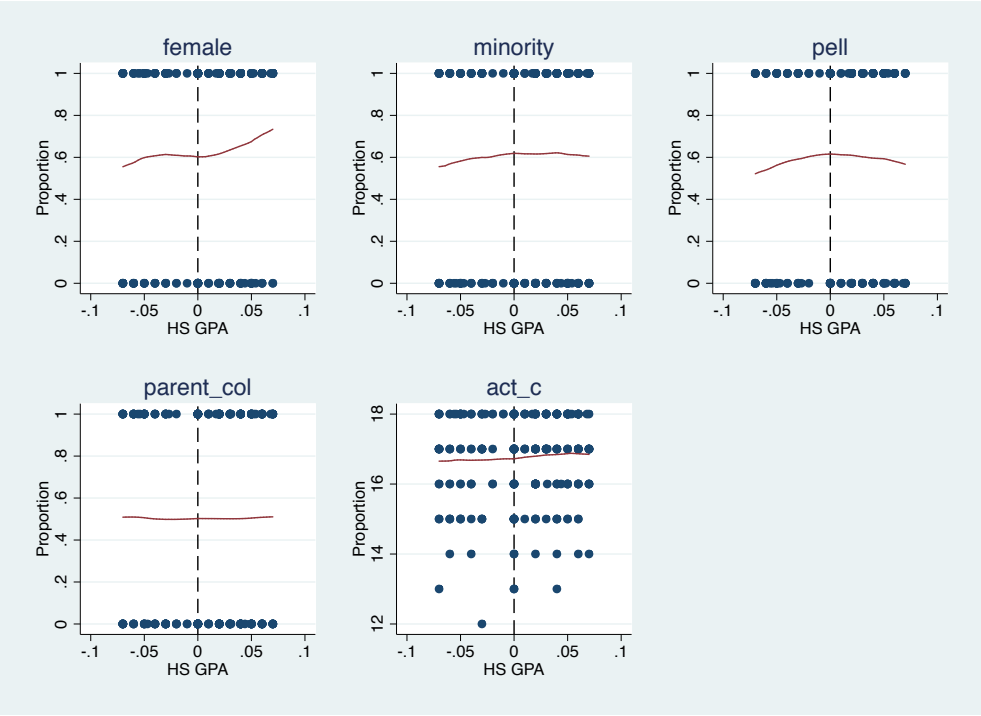

Figure 8. Discontinuity in covariates (GPA)

Lastly, I explore whether there is a sudden jump in the probability of graduation right at the cut-off scores. If receiving scholarships increased the graduation probabilities, then it would be expected to see a sudden increase in the probabilities around the cut-off scores. In Figures 9 and 10, I plot the graduation probabilities against each of the running variables. Although the average graduation rate is much higher for recipients than non-recipients (55% for recipients as opposed to 25% for non-recipients), the graduation probability does not seem to jump at the cut-off score in both figures. Instead, the graduation probabilities seem to be higher on average among scholarship recipients (students on the right side of the cut-off scores) than non-recipients (students on the left side).

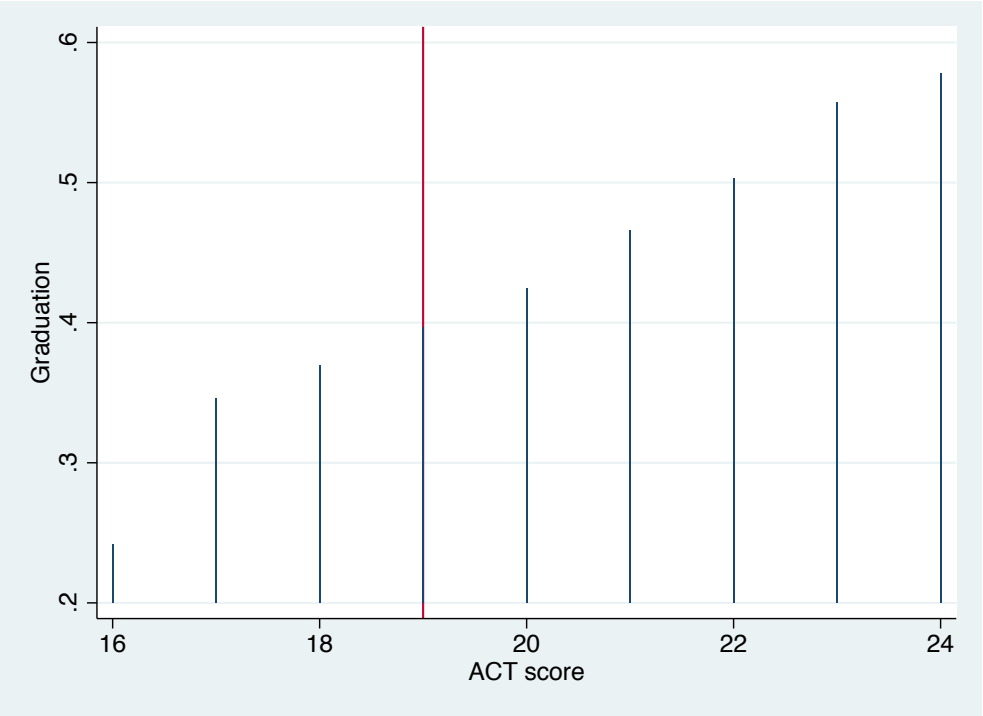

Figure 9. Graduation probability as a function of ACT scores
(among students who did not meet the GPA requirement)

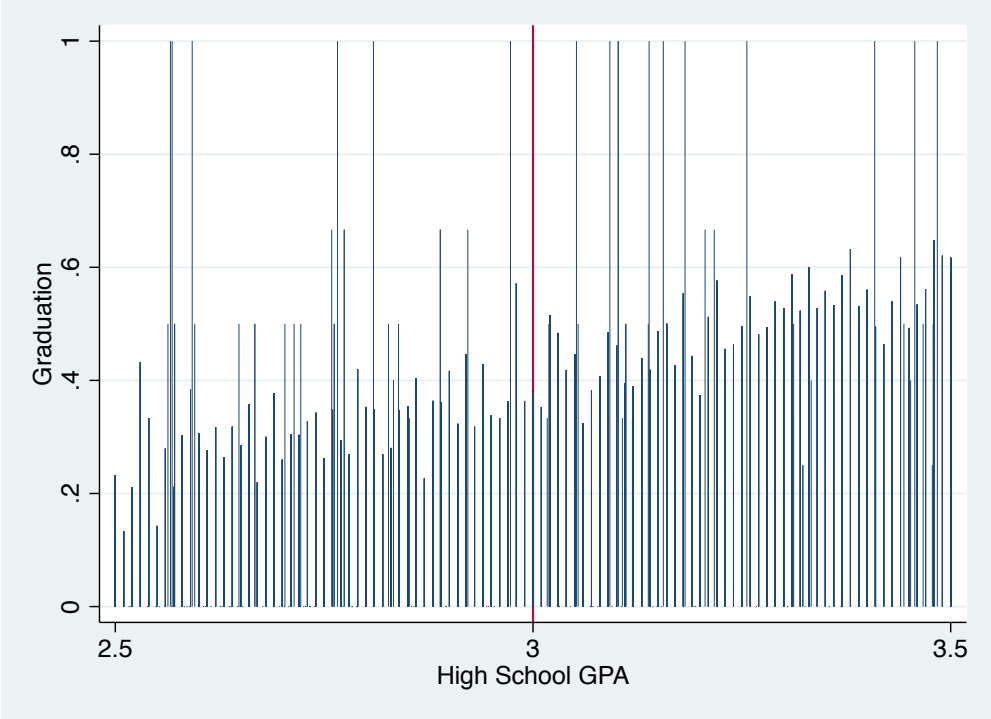

Figure 10. Graduation probability as a function of high school GPA
(among students who did not meet the ACT requirement)

From now on, I present my regression discontinuity estimates using simple mean differences and fuzzy regression discontinuity models. In Table 5, I compare the average rate of earning a bachelor's degree anytime within six years between scholarship recipients and non-recipients within a set of bandwidths. Regardless of forcing variables and bandwidths used, the graduation rates are always higher for recipients than non-recipients. For example, scholarship recipients who scored between 19 and 20 on the ACT exam were 6.3% more likely to earn a bachelor's degree within six years compared to non-recipients who scored between 17 and 18 on the exam. The difference between the two groups is statistically significant except for the 50% bandwidth around the ACT threshold. Similarly, the average degree attainment rates are always higher for scholarship recipients than non-recipients when the forcing variable is high school GPAs. The difference is statistically significant in all bandwidths.

Table 5. Simple Mean Difference Between Recipients and Non-Recipients (Graduation)

Bandwidths	Recipients	Non-Recipients	Mean diff.	t-value	*df*
Assignment variable: ACT scores					
100 %	0.311	0.249	0.063	3.079**	2,400
(2 points)	(0.011)	(0.016)			
50%	0.302	0.266	0.036	1.385	1,624
(1 point)	(0.013)	(0.022)			
200%	0.319	0.237	0.082	4.869***	3,405
(4 points)	(0.010)	(0.013)			
Assignment variable: High School GPAs					
100 %	0.365	0.238	0.128	2.064*	275
(0.07 GPA)	(0.034)	(0.048)			
50%	0.427	0.2	0.227	2.301*	138
(0.03 GPA)	(0.047)	(0.074)			
200%	0.359	0.269	0.090	1.922+	463
(0.14 GPA)	(0.027)	(0.037)			

Note: standard error in parentheses. p-value: *: <0.05, **: <0.01, ***: <0.001

Although the graduation rates are always higher for scholarship recipients, most estimates become statistically not significant in the full regression discontinuity models as shown in Table 6. When I use the ACT score as a forcing variable, scholarship recipients are still more likely to earn their degree within six years than non-recipients. However, the positive estimates are marginally significant only in the 200% bandwidth. These results suggest that the positive outcomes observed in Table 5 are largely attributable to observable differences between recipients and non-recipients. Among covariates, being female, being ineligible for Pell Grants (which indicates being financially better-off), having a college-educated parent, and having a higher high school GPA are positively associated with graduation.

Table 6. Regression Discontinuity Estimates (ACT scores, Graduation)

Bandwidth	100% (2 ACT)	50% (1 ACT)	200% (4 ACT)
Received TELS	0 .038	0 .041	0.046+
	(0.028)	(0.036)	(0.024)
Female	0.035+	0.046+	0.056**
	(0.019)	(0.023)	(0.016)
Racial Minority	-0.006	-0.013	-0.024
	(0.024)	(0.028)	(0.020)
Pell Grants Eligible	-0.082***	-0.081**	-0.078***
	(0.021)	(0.025)	(0.017)
Parental Education	0.061**	0.040	0.049**
	(0.020)	(0.024)	(0.017)
High School GPA	0.153***	0 .138**	0.152***
	(0.037)	(0.046)	(0.029)
Institution Dummy	Yes	Yes	Yes
Sample Size	2,199	1,498	3,132

Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001

In Table 7, I break down the graduation variable depending on the duration taken to earn a bachelor's degree in the fourth, fifth, or sixth year only. Because the scholarships are renewable up to five years or 120 college credits, whichever comes first, it may have incentivized students to finish their education within five years. According to Table 7, receiving the scholarships significantly increases the probability of graduating in the fourth year only. The estimates are positive, statistically significant, and robust across different bandwidths. However, receiving the scholarships does not have a significant impact on graduating in the fifth or sixth year. To summarize, receiving TELS does not change whether or not students earn a bachelor's degree within six years. Instead, it significantly increases the probability of earning a bachelor's degree in the fourth year.

Table 7. Regression Discontinuity Estimates (ACT Scores, graduation in a given year)

Bandwidth	100%	50%	200%
Fourth-year Graduation	0.039*	0.051**	0.044**
	(0.019)	(0.019)	(0.014)
Fifth-year Graduation	-0.018	-0.020	-0.010
	(0.022)	(0.029)	(0.019)
Sixth-year Graduation	0.017	0.011	0.013
	(0.016)	(0.020)	(0.013)

Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001

Tables 8 and 9 present fuzzy discontinuity estimates when a forcing variable is high school GPAs. Similar to the previous results, receiving TELS does not affect whether student earn a bachelor's degree anytime within six years. According to Table 8, the estimates on the TELS variable are all positive, but none of them is statistically significant at the conventional level. In Table 8, the only factor that has a significant effect on graduation is being racial minority. Even when I break down the outcome variable into graduation in the fourth year, fifth year, and sixth year as in Table 9, receiving the scholarships does not have a significant impact on any of these outcomes in all bandwidths.

It is noteworthy that the results are sensitive to assignment variables used. Receiving TELS increases the probability of graduating in the fourth year in the first model (ACT scores as an assignment variable), while it does not in the second model (GPAs as an assignment variable). This is partially because the bandwidth is smaller for the second model, as of 0.07 point. Due to the narrow bandwidth, the sample size used in the second model is fewer, which leads to large standard errors.

Table 8. Regression Discontinuity Estimates (High School GPA, Graduation)

Bandwidth	100% (0.07 GPA)	50% (0.03 GPA)	200% (0.14 GPA)
Received TELS	0.089	0.191	0.061
	(0.130)	(0.284)	(0.082)
Female	0.060	0.020	0.087+
	(0.064)	(0.095)	(0.048)
Racial Minority	-0.158*	-0.104	-0.154**
	(0.080)	(0.116)	(0.059)
Pell Grants Eligible	-0.073	-0.205+	-0.071
	(0.066)	(0.108)	(0.050)
Parental Education	-0.022	0.027	-0.001
	(0.062)	(0.093)	(0.046)
ACT Scores	0.005	-0.020	0.004
	(0.026)	(0.037)	(0.019)
Institution Dummy	Yes	Yes	Yes
Sample Size	257	130	432

Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001

Table 9. Regression Discontinuity Estimates (High School GPA, Graduation in a given year)

Bandwidth	100%	50%	200%
Fourth-year Graduation	0.006	0.087	-0.236
	(0.070)	(0.154)	(0.046)
Fifth-year Graduation	-0.047	-0.139	-0.016
	(0.102)	(0.233)	(0.064)
Sixth-year Graduation	0.131	0.243	0.101+
	(0.086)	(0.217)	(0.053)

Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001

Conclusion

This study examines whether receiving Tennessee Educational Lottery Scholarships has an influence on the probability of earning a bachelor's degree within six years as well as earning a bachelor's degree in the fourth, fifth, or sixth year. In Tennessee, state residents who immediately went to in-state colleges after high school graduation were eligible for the scholarships if they met either the ACT or high school GPA requirement. Because the ACT/GPA cut-off scores are rather arbitrary, I employ the regression discontinuity method that assumes students near the cut-off scores are almost identical except for whether they are eligible for merit-based aid. I find that receiving TELS does not significantly affect whether students earn a bachelor's degree anytime within six years. Instead, receiving the scholarships significantly increases the probability of students earning their degree in the fourth year after they enter college. I draw several conclusions, especially based on the results from regression discontinuity models.

First, receiving Tennessee Educational Lottery Scholarships increases the probability of earning a bachelor's degree in the fourth year, but not in other years. This can be explained in two ways which are somewhat contrary to each other. One way to explain this result is that the scholarships have a net positive effect on degree attainment; however, as many students fail to renew their scholarship eligibility, they do not fully benefit from the aid. Consistent with the human capital theory, TELS reduces direct costs of college

education and mitigates the financial burden of students. This may allow students to spend more time in academic and extracurricular activities on campus, which in turn leads to persistence and degree attainment. It is also possible that the renewal requirement, maintaining a 3.0 college GPA, gives its recipients an incentive to work hard in class. If this were true, TELS would provide students with monetary incentive to keep on track to earn a college degree. As Scott-Clayton (2011) observes in the case of West Virginia, merit-based aid has both financial and academic components that help students persist and graduate.

Considering that the regression discontinuity estimates in this study are intent-to-treat estimates, the actual effect of TELS could be larger than the estimates. As mentioned previously, approximately 50% of scholarship recipients lost their scholarships after a couple of years because they failed to meet the renewal requirement. Supposing that receiving the scholarships have a positive impact on degree attainment, if all recipients in my sample had maintained their scholarships, then their graduation outcomes might have been largely improved (Henry, Rubenstein, & Bugler, 2004).

Another way to explain the result is that there could be confounding factors that affect both receiving the scholarships and degree attainment. Although I find a positive impact of the scholarships on degree attainment, it is not yet conclusive whether TELS is the sole factor. Above all, results from the McCrary test and the balanced test suggest that scholarship recipients are different from non-recipients to some extent. The difference between the two groups might have affected the graduation outcomes in my study. For example, scholarship recipients were more likely to graduate in the fourth year than non-recipients not only because they received the scholarships but also because they were more motivated in the first place. Although my regression discontinuity estimates substantially reduce the difference between the two groups by limiting my sample to students near the cut-off scores and including several covariates, the difference is not entirely removed.

At this point, there is not enough evidence to determine which explanation fits my results better. More importantly, both explanations are not necessarily mutually exclusive. It is possible that scholarship recipients were more likely to graduate than non-recipients in the first place, and then providing recipients with the

scholarships helped them attain their degree faster compared to non-recipients. Future research needs to employ a research design that disentangles the net impact of financial aid.

Second, I find that the number of students who barely made the eligibility requirements is much higher than the number of students who barely missed them. This suggests that some students might have tried hard to satisfy the requirements by taking the ACT exam multiple times or pushing for better grades. This is not surprising given that manipulation is plausible when the treatment (merit-based aid in this case) is beneficial for agents, its assignment rules are widely known to the public, and agents have control over the assignment rules to some extent (McCrary, 2008). Although students in my sample had only a few months before entering college, some students still seemed to work through meeting the requirements and receive the scholarships. As van der Klaauw (2008) points out, there can be factors other than the treatment that incentivize students to make the cut-off scores. For example, students might need to have a 3.0 high school GPA not only for being eligible for TELS but also for getting accepted to selective colleges. Whatever the reason is, it is still plausible that these students who just made the cut-off scores are more motivated and persistent than students who failed to do so.

Researchers have developed several strategies to deal with potential manipulation. Acknowledging the possibility of manipulation among students who took the SAT multiple times, Zhang et al., (2013) focus on SAT scores from the *first* attempt of each student. Cohodes and Goodman (2013) use the regression discontinuity method in the case of Massachusetts, where the state's merit-based aid is awarded to students whose state exam scores put them on the 75^{th} percentile or higher within their district. The authors argue that it is impossible for students to game the system because scholarship eligibility in Massachusetts is determined in relative terms after students take the test. Lastly, Castelman (2013) uses the difference-in-differences method because he is aware of the manipulation problem in his data. Because my data lack information about the frequency of tests taken by students or about previous cohorts, I leave this issue of manipulation as a limitation of this study.

Lastly, given the evidence of possible student manipulation, TELS appears to incentivize high school students to earn a better grade or a higher test score at least up to the point where they can secure the aid. This is consistent with previous studies which show that the average SAT/ACT scores of high school students have significantly gone up after a state adopted merit-based aid (Cornwell, Mustard, and Sridhar, 2006; Henry & Rubenstein, 2002; Pallais, 2009). The improvement in standardized test scores is encouraging, especially if the test scores accurately reflect student ability. However, it is also possible that students merely take advantage of the system by taking the tests multiple times, and the improved test scores do not translate into better outcomes in college. If this were the case, students' money and effort spent on improving their test scores would be socially inefficient.

This study adds to the existing literature by demonstrating the impact of receiving merit-based aid on degree attainment in Tennessee. This study significantly reduces selection bias by employing the regression discontinuity method. Results from this study show that receiving merit-based aid promotes degree attainment in the fourth year, even though a substantial number of scholarship recipients lose their scholarships a couple of years later.

There are some recommendations for future research. First, future research that uses the regression discontinuity method to analyze the effect of merit-based aid needs to address potential manipulation issue. This issue would be plausible in many states given that eligibility requirements of most merit-based aid programs are publicly known and controllable by students. Future research that adequately addresses this issue will provide more precise evidence regarding whether merit-based aid affects graduation. Second, future research needs to consider how receiving merit-based aid in the first year affects a student's college pathway to a degree. Recently, researchers have recognized that merit-based aid could affect college choice and examined how college choices affect graduation (Cohodes & Goodman, 2013; Castleman, 2013). In addition to these efforts, I also recommend examining whether receiving merit-based aid affects student enrollment patterns, year-to-year persistence, or transfer, all of which are related to their degree attainment.

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Appendix B

Table. B.1. Logistic Regression Results (in Odds-Ratio)

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
TELS	3.569***	3.159***	3.390***	1.906***	1.905***
	(3.184, 4.001)	(2.812, 3.548)	(2.928, 3.924)	(1.623, 2.239)	(1.620, 2.240)
Female		1.368***	1.440***	1.266***	1.310***
		(1.278, 1.465)	(1.342, 1.546)	(1.174, 1.366)	(1.213, 1.414)
Minority		0.553***	0.649***	0.897*	0.881*
		(0.507, 0.602)	(0.592, 0.711)	(0.812, 0.991)	(0.793, 0.980)
Pell Eligible			0.571***	0.592***	0.592***
			(0.527, 0.618)	(0.545, 0.644)	(0.544, 0.643)
Parental			1.439***	1.394***	1.359***
Education			(1.332, 1.554)	(1.286, 1.510)	(1.253, 1.474)
HS GPA				2.928***	2.842***
				(2.696, 3.179)	(2.615, 3.089)
ACT Score				1.047***	1.040***
				(1.035, 1.059)	(1.028, 1.053)
Institution	No	No	No	No	Yes
Dummy					

Note: p-value: *: <0.05, **: <0.01, ***: <0.001

Note: 95% Confidence Intervals are reported in parentheses.

CHAPTER IV

Effect of Losing Scholarships on Persistence and Degree Attainment

Abstract

This study examines the effect of losing Tennessee Educational Lottery Scholarships on college persistence and degree attainment. In Tennessee, approximately 50% of students who receive their state merit-based aid lose it after their first year. In order to see whether losing the scholarships affects student success, I use the event history model and compare the hazard of re-enrolling and earning a bachelor's degree within six years across scholarship keepers, losers, and non-recipients. Results show that students who maintained their scholarships were much more likely to re-enroll and graduate from college than the other two groups. However, scholarship losers were slightly more likely to re-enroll and graduate than non-recipients for a limited time. Results from this study suggest that merit-based aid has a positive effect on persistence and degree attainment only when students maintain their scholarships until graduation.

Key words: merit-based aid, scholarship loss, persistence, degree attainment

Introduction

During the last three decades, a growing number of students entered college. The percentage of students who attended college within 12 months of their high school graduation increased from 49.3% in 1980 to 68.2% in 2011 (NCES, 2012).[27] However, college graduation rates never exceeded 60%. Of first-time freshmen students who entered four-year colleges in 2005, only 38.6% and 58.3% earned their bachelors' degrees within

[27] NCES (2012). Digest of Education.

four years and six years, respectively. When we look at two-year colleges, the percentage of students who earned a credential within 150% of the normal duration is even lower, as low as 30%.

Why are we losing so many students on their way to degree? Traditionally, researchers explain that students drop out of college because they are not academically prepared, not academically or socially engaged in their institutions, or cannot afford college prices (Melguizo, 2011). The interaction between student and institutional factors makes the matter more complicated. According to Bound, Lovenheim, and Turner (2009), the low graduation rate can be attributed to the interaction between student and college factors because a large number of academically underprepared students are concentrated in less selective colleges where resources to help these students are scarce. There have been many policies and programs that are intended to help students stay in college and earn their degrees. Among them, financial aid may be the most direct and short-term intervention that gives students an incentive to postpone their labor market participation and pursue college education.

Whether financial aid promotes persistence and graduation has been assessed in many studies. Most of the previous studies ask whether receiving financial aid (or being eligible for financial aid) affects a student's college success (e.g., Alon, 2007; 2011, Bettinger, 2010; Chen & DesJardins, 2008; DesJardins, Ahlburg, & McCall, 2002; Goldrick-Rab, Harris, Benson, & Kelchen, 2011). Although these studies provide important insights regarding whether and how financial aid works, they have not reached a consensus about whether financial aid promotes college success. In addition, most of the previous studies focus on the effects of receiving need-based grants at one time period on student success later. However, Alon (2011) and Chen and DesJardins (2008) demonstrate that the effect of financial aid can be different depending on aid type, student income, and time period. Compared to need-based grants, merit-based aid serves different student population (generally more affluent and academically prepared students), and students must renew their scholarship eligibility every year. These characteristics suggest that merit-based aid might have a different impact on

student success compared to need-based aid. In this study, I examine whether merit-based aid promotes persistence[28] and graduation, especially as a result of a year-to-year change in scholarship renewal status.

Since the early 1990s, more than a dozen states have introduced merit-based aid whose eligibility is solely determined by a student's test score and GPA (Dynarski, 2002). A unique feature of this type of aid is that students must renew their eligibility by maintaining their cumulative GPA above a certain point. If students fail to meet the standard, they lose the aid. Previous studies demonstrate that this risk of losing financial aid is real to many students, and students employ a variety of strategies not to lose it (Zhang et al., 2006). Despite their efforts, if they lose their aid in college, what happens to these students? Are they more likely to drop out of college than students who retain their aid because they cannot pay tuitions? Despite the financial hardship, do they still perform better than students who never receive the aid in the first place because they already finish at least one year of education?

In this study, I answer these questions in the case of Tennessee. Tennessee implemented its state-funded merit-based aid in the fall of 2004. I divide the fall 2004 first-time freshman cohort enrolled in public four-year colleges into the three groups of scholarship keepers, scholarship losers, and non-recipients based on their scholarship receipt status. Then, I compare their probabilities of persistence and graduation up to six years after their initial enrollment using the event history model and the regression discontinuity method. I find that students who received and maintained merit-based aid were more likely to persist and graduate. However, these positive effects on scholarship keepers are not observed when I focus on students who met or missed renewal eligibility by a slight margin. Overall, students who lost their scholarships tended to persist and graduate at a higher rate than non-recipients for a limited time period.

[28] In this study, I measure student persistence in terms of stop-out. Stop-out occurs when a student stops enrolling in any of the public four-year colleges in Tennessee for the first time. Regarding the definition, it is necessary to make sure that these students who once stop out can return to one of these institutions later after a couple of semesters. For example, some students who lose their scholarships and stop attending college might come back to their college after they earn sufficient money to pay their tuition. Or, it is also possible that students who lose their scholarships transfer to community colleges, which are usually more affordable to attend. In either case, these students are coded as stop-out. This operationalization of stop-out might have made the stop-out problem in this study look worse than it actually is.

This study analyzes the impact of *losing* merit-based aid on student outcomes such as persistence and graduation. In contrast to abundant evidence on receiving a scholarship, the effect of losing a scholarship has been rarely examined in the literature. Because a large number of students lose their scholarships each year, it is important to look at how losing scholarships affects these students and consider possible policy tools to help them. In addition, this study reflects the temporal element of scholarship renewal status, persistence, and graduation by employing an event history model. In other words, my study not only examines whether losing scholarships affects student success but also considers the effect of the timing of losing scholarships on student success.

Tennessee Educational Lottery Scholarships

Tennessee adopted its merit-based aid program, Tennessee Educational Lottery Scholarships (TELS) in 2004. It consists of five sub-programs, each of which has a different award amount and eligibility. Of the five programs, the Wilder-Naifeh program is available only for students enrolled in one of the Tennessee Technological Centers. The other four programs are available for state residents who are enrolled at least part-time in one of the state's public and private colleges. To be eligible, students must have at least a 21 ACT score or a 3.0 high school GPA. An additional $1,000 to $1,500 is awarded for either highest-achieving students with a 27 ACT score and a 3.75 GPA or students whose family income is below $36,000, respectively.[29]

Once students receive one of the scholarships, they must renew eligibility periodically by maintaining a 3.0 cumulative GPA.[30] These check points come up when students attempted 24, 48, 72, 96, and 120 college credits including credits from remediation courses. For example, full-time students typically face their first checkpoint at the end of their second semester. If their cumulative GPA is at or above 3.0 at that time, they maintain their scholarships. If the GPA is below 3.0, they lose the scholarships in the next regular semester.

[29] When students in my sample entered college, the initial requirement was either a 19 on the ACT exam or a 3.0 high school GPA. The additional amount for low-achieving students was $1,000 at the time.

[30] Since 2008, the renewal threshold has been lowered to a 2.75 for students who attempt 24 and 48 credits. The 3.0 cumulative GPA is still required for students who attempt 72, 96, and 120 credits. Because my study uses the fall 2004 cohort, who went through the first and second checkpoints before 2008, I use the 3.0 GPA standard for all check points analyzed in this study.

Students who lose their scholarships can regain eligibility one time if their cumulative GPA at the next checkpoint is at or above 3.0.[31] However, there are only a few students who have actually regained their financial aid.[32]

According to the state's annual report, approximately 50% of the fall 2004 cohort who received the scholarships maintained them at the end of their first year (THEC, 2009). After the second year, only 37% of initial recipients kept their scholarships. These renewal rates suggest that a majority of TELS recipients lost their scholarships within two years. The renewal rates differ based on which initial eligibility criterion students have satisfied. For example, the renewal rate is the highest for students who met both ACT and high school GPA criteria. In contrast, students who met either one of the criteria had much lower renewal rates. These statistics suggest that a majority of scholarship recipients lost their aid. In addition, it appears that whether students lost their scholarships is related to their academic performances prior to college attendance (e.g., ACT scores and high school GPAs) to some extent.

Conceptual Framework

To date, there has not been much work that theoretically explains the effect of losing a scholarship. Therefore, I make several propositions based on the human capital theory, which is widely used as a theoretical background for financial aid research. Although hypotheses of my study are built upon the human capital theory, results from my study should not be used as either supporting or opposing evidence of the theory.

Based on economic theory, the human capital theory assumes that students are rational actors who pursue maximizing their utility among different sets of choices such as whether or not to go to college. In other

[31] To my knowledge, this is the only way for students who lose their scholarships due to the GPA requirement to regain their eligibility. According to the THEC website, students who lose their scholarships can appeal to either their institutional board or to the Tennessee Student Assistance Corporation review panel only if they lose their scholarships for non-academic reasons (e.g., leaving an institution for personal or medical reasons; changing their enrollment status from full-time to part-time in the middle of a semester, etc.). The web site explicitly mentions that neither a student's institution nor the TSAC panel "have the authority to rule on appeal from students due to a final high school GPA below the minimum required for initial eligibility or a lottery scholarship college GPA below the minimum required for continuation". For more information regarding appeal process, visit https://www.tn.gov/CollegePays/mon_college/tels_appeals_exception.html.

[32] This statement is based on my personal communication with a former THEC staff.

words, students consider benefits and costs of each alternative and choose the one that satisfies them the most. Although utility includes both monetary and non-monetary values, most research using the human capital framework focuses on monetary benefits and costs of college education. This calculation can be simplified as equation (1).

$$\sum_{t=s+1}^{t} \frac{B_t}{(1+i)^t} - \sum_{i=1}^{s} \frac{C_t}{(1+i)^t} \geq 0 \quad (1)$$

Equation (1) illustrates future earnings and costs associated with college education. When a student has *s* years of college education, this generates direct and indirect costs of college education, C_t. Direct costs include tuition, fees, and living expenses, while indirect costs mean a student's foregone earnings, which (s)he would have earned if (s)he had a job right after graduating from high school. After *s* years of college education, (s)he expects annual earnings of B_t until (s)he retires in year *t*. Because both benefits and costs of college education occur in the future, both of them are discounted at a rate of *i*, which is equivalent to an interest rate. The human capital theory assumes that students can borrow money at a rate of *i* from the market to invest in their education (Schultz, 1961).

When students decide whether to pursue another year of college education, they weigh the present value of the benefits to the present value of costs associated with another year of education. Then, students decide to attend another year of college only if the benefits are larger than the costs. In this study, receiving merit-based aid will increase the estimate of equation (1) by reducing direct costs of college education (C_t). When other things are held constant, I hypothesize that receiving merit-based aid will encourage students to attend another year of college, which subsequently leads them to earn a bachelor's degree. In contrast, losing merit-based aid will decrease the estimate of equation (1) by increasing the portion of direct costs of education (C_t) that students have to pay out of their pocket. When other things are held constant, I hypothesize that students who

lose their scholarships will have a lower probability of re-enrolling in college, which also leads to a lower probability of earning a bachelor's degree.

Literature Review

There is not a large literature that explains how *losing* a scholarship affects persistence and graduation. Instead, several researchers demonstrate that students strategically choose courses and majors so that they can maintain good grades and keep their scholarships. Because students must maintain a certain GPA, they avoid difficult courses and take fewer credits per semester. In particular, these strategic behaviors are frequently observed among students who major in science, technology, engineering, and math (STEM). Dee and Jackson (1999) find that students who entered Georgia Tech in 1996 were more likely to lose their HOPE scholarship if they majored in science, computer science, and engineering compared to their peers in humanities and social science majors, even after taking into account their SAT scores.

In a similar vein, Zhang et al., (2006) and Mobley, Brawner, and Ohland (2009) illustrate enrollment behaviors of merit-based aid recipients in the case of Florida and South Carolina, respectively. These authors find that scholarship recipients tended to take fewer credits per semester, enroll in summer or leisure courses, leave their majors, withdraw from classes in which they anticipated to earn low grades, and re-take a course if they actually received low grades. In Georgia, Cornwell, Lee, and Mustard (2005) observe similar behaviors most frequently among students whose GPAs were at the margin of the renewal threshold. These studies show that scholarship recipients are concerned about the risk of losing their scholarships and change their behavior to avoid the possibility. Despite the evidence, only a few studies investigate the effects of losing merit-based aid on college outcomes.

Henry, Rubenstein, and Bugler (2004) compare college outcomes among three groups: students who received and maintained the HOPE scholarships, students who initially received but lost the scholarships, and students who did not receive the scholarships in the first place. In order to minimize selection bias, they limit

their sample to students whose high school GPA was close to the cut-off GPA. They also match recipients with non-recipients based on students' GPAs and institutions. The authors find that scholarship recipients earned more cumulative credits and had higher college GPAs than non-recipients. However, even with the same number of credits earned and GPAs, scholarship recipients who lost their scholarships were not more likely to graduate within four years than scholarship non-recipients, while scholarship recipients who maintained their scholarships were more likely to graduate than non-recipients. This is one of the few studies that demonstrate that losing merit-based aid decreases graduation probabilities. However, the authors do not explain why the graduation outcome differs between scholarship keepers and losers, especially when their credits earned and grades are equivalent.

Other than Henry, Rubenstein, and Bugler (2004), little is known about the effect of losing a scholarship on persistence and graduation. Most studies that examine whether receiving merit-based aid affects degree attainment tend to focus on the difference between scholarship recipients and non-recipients, not paying much attention to scholarship losers. In her regression discontinuity model, Scott-Clayton (2011) defines both scholarship keepers and losers as scholarship recipients and compares college outcomes between the two groups. Similarly, researchers using the difference-in-differences method have not considered whether individual students lost their scholarships (Dynarski, 2005; Zhang, 2011). Instead, they focus on the before- and after-merit-aid-policy difference in the average graduation rates between states with and without merit-based aid. Given that all three previous studies find significant and positive effects of (receiving) merit-based aid on degree attainment in several states, receiving merit-based aid seems to have a large positive effect on degree attainment, which is not canceled out by possible negative effects of losing scholarships. This study directly tests if the positive effect of receiving merit-based aid on degree attainment holds true by comparing persistence and graduation outcomes of scholarship losers to those of scholarship keepers and non-recipients.

Data & Sample

In this study, I use administrative data that is collected and managed by the Tennessee Higher Education Commission (THEC). Every postsecondary education institution in Tennessee is required to send its student information to THEC. Of the data, I have access to students who entered one of the state's public four-year colleges in 2004 fall. Then, I further limit my sample to students who met all of the initial eligibility requirements except for standardized test scores and high school grades so that I can minimize possible selection bias.[33] As a result, my sample consists of first-time freshman students who enrolled at least part-time in one of the state's public four-year colleges. The data allows me to track these students as long as they remain in one of the state's public four-year colleges.

Table 1 provides the descriptive statistics of my sample. Female students are more represented than male students. In terms of race and ethnicity, white or Asian students make up 79% of the sample, while African-American, Hispanic, American Indian, and multi-racial students are 21% of the sample. Approximately 65% of the sample has at least one college-educated parent. Approximately 33% of students were eligible for Pell Grants, which is used as a proxy for low-income status in this study.[34]

In my sample, 87% received one type of the TELS awards when they entered college, while 13% of students did not. The percentage of scholarship recipients is much higher than that among population because I exclude many students who did not satisfy basic eligibility requirements (e.g., state residency) from my sample. Of the 87% of students who initially received the scholarships, 52% of them lost their scholarships. Of all students in my sample, 58% of my sample has stopped out of their college at least once, while the remaining 42% continuously enrolled in college until graduation. In terms of graduation, 47% of my sample earned their bachelor's degree within six years.

[33] More specifically, I exclude students from my sample if they 1) were not the state residents at the time of college application 2) entered college after sixteen months of high school graduation or 3) enrolled fewer than 6 credits during their first semester.
[34] My data reports family income information, but the missing rate is as high as 43%. Therefore, I use the Pell-eligibility as a proxy for family income because it indicates the extent to family resources which can be used for a student's college education.

Table 1. Descriptive Statistics of the Sample

Characteristics	Categories	Percentages
Gender	Male	45.34%
	Female	54.66%
Race	White/Asian	78.90%
	Racial minorities	21.10%
Parental Education	Some college	65.25%
(At least one parent)	No college	34.75%
Pell Grants Eligibility	Eligible	32.51%
	Ineligible	67.49%
TELS Status	Keep	35.20%
	Lose (ever)	51.76%
	Never receive	13.04%
Stop Out (ever)	Yes	58.21%
	No	41.79%
BA Attainment (ever)	Yes	47.46%
	No	52.54%
Variables	Mean	Std. Deviation
ACT composite	22.384	3.944
High School GPA	3.275	0.586

Methods

This study examines two research questions. First, were students who lost their scholarships more likely to stop out than their peers who either maintained their scholarships or did not receive one at all? Second, were students who lost their scholarships more likely to earn a bachelor's degree within six years than their peers who either maintained their scholarships or did not receive one at all?

To answer these questions, I mainly use the event history model. Event history models not only examine whether an event of interest occurs but also investigate how long it takes for the event to occur (Box-Steffensmeier & Jones, 2004). More specifically, an event history model estimates the probability that a person experiences an event of interest at time *t*, conditional on that the person has not experienced it before. This is called a hazard function, $h(t)$. An event history model estimates the hazard function of an event of interest as well as how covariates affect the hazard function. For example, results from the event history model in my study will show the probability of stopping out at each semester as well as how the stop-out probability changes depending on covariates.

In this study, I mainly use the Cox hazard model. The Cox model is preferred when a researcher does not have an assumption about the functional form of the baseline hazard (Cleves, Gould, Gutierrez, & Marchenko, 2010). Because I do not have a prior knowledge about the baseline hazard function of both outcomes (stop-out and graduation), I first employ the Cox model, as specified in equation (2).

$$h(t \mid x_{it}) = h_0(t)\exp(x_{it}B) = h_0(t)\exp(B_1x_{1t} + B_2x_{2t} + ... + B_kx_{kt}) \quad (2)$$

The left side of equation (2) shows that this model estimates the hazard function of experiencing each of the events of interest (i.e., stop-out and graduation). In my study, the stop-out variable takes on the value of 1

when a student stops enrolling in their college for the first time.[35] If the student is enrolled, the stop-out variable has a value of 0. Another outcome variable of this study is whether a student earned his/her bachelor's degree at time *t*. Again, the graduation variable takes on the value of 1 if a student earned a bachelor's degree at time *t*, and 0 otherwise. I track students up until six years after they entered college because six-year graduation is a common benchmark to evaluate student success. Students who did not earn a bachelor's degree and were still enrolled at the last semester of the sixth year are treated as right-censored cases.[36]

The middle and right sides of equation (2) show that the model estimates the hazard function of first stop-out (or degree attainment) at time *t* conditional on covariates (x_{it}). I leave the baseline hazard function ($h_0(t)$) unspecified because it is not necessary, when using the Cox model. Because the baseline hazard function is the same for students at the same time period, it is independent variables that determine different hazard functions across students. The key independent variables that I am interested in are indicators that show if students kept, lost, or never received the TELS award. In my model, the reference group is those who never received the scholarships in the first place. I compare these non-recipients to students who kept their scholarships until graduation and students who lost their scholarships in college. This way, I can see if receiving TELS and losing it later is any better than never receiving it. The model also allows me to examine if losing TELS is significantly worse than keeping the aid in terms of student persistence and graduation. In addition to these key independent variables, I include a student's demographic characteristics, family background, and prior academic achievement in the model. Table 2 describes these independent and outcome variables used in my study.

[35] These students could have transferred to private or out-of-state colleges. Due to the data availability, I assume that these students have stopped enrolling in college.

[36] Right-censored cases are those who have not experienced an event of interest, but who still show up in data. It is possible that limiting the time period of my study to six years will lose some students who do eventually graduate. In my sample, there is only one student who enrolled more than six years and did eventually earn a bachelor's degree.

Table 2. Description of Variables

Categories	Variables	Description
Outcome Variables	Stop-out	1: if students stop being enrolled in the next regular semester (e.g., fall to spring, spring to fall) for the first time 0: if students continuously enrolled
	Graduation	1: if students earned a bachelor's degree within 6 years 0: otherwise
Independent Variables	TELS	Two dummy variables, one of which indicates students who received and maintained TELS (scholarship keepers), and another indicates students who received and lost TELS (scholarship losers). The reference group is students who never received TELS in the first place.
Control Variables	Female	0: male, 1: female
	Minority	0: white or Asian, 1: African-American, Hispanic, others
	Pell	0: Pell grants ineligible, 1: Pell grants eligible
	Parent College	0: Neither of parents has college experience 1: At least one parent has enrolled at least one semester in college
	GPA	A student's high school GPA (continuous)
	ACT	A student's ACT composite score (continuous)
	College Majors	Two dummy variables that indicate whether students major in one of the STEM fields (stem) or have not decided their majors (undecided). The reference group is students in non-stem majors

It is necessary to take into account institutional effects on persistence and graduation. As Pascarella and Terenzini (2005) conclude, institutional characteristics (e.g., institutional control, selectivity, etc.) are associated with these outcomes, even after student characteristics are controlled for. Students in my sample started at one of nine public four-year colleges in Tennessee,[37] and students enrolled in the same college may share common culture and learning environments.

[37] University of Memphis, Austin Peay State University, East Tennessee State University, Middle Tennessee State University, University of Tennessee at Knoxville, University of Tennessee at Martin, Tennessee State University, Tennessee Technological University, and University of Tennessee at Chattanooga.

In order to take into account within-college correlation, I try three different approaches: 1) cluster-robust standard error, 2) institutional fixed effects, and 3) stratified models (Cleves et al., 2010).[38] The first approach fixes standard errors without changing the point estimates. The second approach directly estimates the effect of each institution, which will shift the baseline hazard line up or down. This approach is appropriate when a researcher is interested in the sample itself rather than making an inference about the population. The third approach estimates a separate baseline hazard for each of the colleges in the sample. This is contrasting to the second approach, which assumes that the shape of the baseline hazard is the same for different groups (colleges, in this study). When I run my model using these three approaches, results are qualitatively the same in most cases. Therefore, I report my results using the third approach and mention results from the other two estimates only if they are substantially different across the three approaches.

Lastly, when using an event history model, the issue of "tied failures" is commonly observed. Because most data are measured in a discrete interval (e.g., days, months, years, etc.), it is possible that at least two subjects experience an event of interest at the same interval. For example, in my study, a substantial number of students did not come back (re-enroll in) their third semester (the fall semester of their sophomore year). Some of them might have stopped attending college in the middle of their second semester, while others have stopped out after completing the second semester. Because it is not possible to measure the exact date of their stop-out, there are several ways to handle these tied failure cases. Of these procedures, I use the efron approximation, which is not too computationally intensive but reflect the number of students who are at risk of experiencing an outcome more accurately than the breslow approximation (Cleves et al., 2010).

In addition to the Cox model, I also run the Weibull and discrete-time models to see if my estimates are consistent across these models. The difference among these three models is the way each model assumes the baseline hazard function. The Weibull model assumes a monotonically increasing or decreasing baseline, and

[38] Another way to handle within-college correlation is to include institutional characteristic variables in my model. However, nine colleges in my sample are all public four-year colleges located in the same state, which means that they already share the same features in terms of institutional control, institutional type, and location. Hence, I choose to use more indirect ways of controlling for within-college correlation such as adjusting standard error or including institutional fixed effects.

the discrete-time model allows the baseline to differ at each semester. I look at these models as well for the following reasons. First, the Weibull model (or other parametric models) provides more efficient estimates if the model's assumption is met. Second, the discrete-time analysis model is used because both of the outcome variables (stop-out and graduation) are measured at discrete intervals (Allison, 1982). Lastly, I use these two models as a sensitivity check because my data violates the proportional hazards assumption, which is critical for using the Cox model (Steele, 2005). According to the proportional hazards assumption, the effects of a covariate remain constant at all time periods. If the magnitude or direction of the effects varies over time, it indicates a violation of this assumption. Because my data, especially the key independent variables that indicate a student's scholarship renewal status, violates the assumption, I use the discrete-time model, which "allows for non-proportional hazards" (Steele, 2005, p.8).

An event history model addresses several challenges that traditional regression models face (Box-Steffensmeier & Jones, 2004). First, an event history model accommodates a skewed distribution, which is often the case when an outcome variable is the duration of time for an event to happen. The model allows researchers to choose an appropriate functional form that fits the distribution of their outcome variable. Second, an event history model differentiates between missing and censored data.[39] An OLS model treats both cases as missing, which leads to biased estimates. Lastly, an event history model allows time-varying variables, while an OLS model does not. Because the key independent variables of this study (scholarship renewal status) can vary each semester, it is important to reflect the temporal element of the variables.

Despite these advantages, an event history model is not very powerful in terms of reducing selection bias. That is, except for the factors controlled in the model, unobservable differences among scholarship keepers, losers, and non-recipients still remain. To complement the limitation, I run a regression discontinuity model at each of the five renewal checkpoints. In the regression discontinuity model, I compare scholarship

[39] There are two types of censored cases: right-censored and left-truncated. Right-censored cases are observed when an event of interest does not occur until the last period of observation. In this research, students who did not graduate from college within six years of initial enrollment and still enrolled in college can be an example of right-censored cases. Left-truncated cases are those who experienced the event of interest even before they were observed by researchers. Regardless of the type, censored cases are different from missing observations, so they need to be treated differently (Box-Steffensmeier & Jones, 2004).

losers to scholarship keepers in terms of their stop-out and graduation, as specified in equation (3). In equation (3), Y_i represents whether or not a student ever stopped out (or earned a bachelor's degree) within six years of their initial enrollment. The key independent variable is $(lose_{it})$, which indicates whether or not a student lost his/her scholarship at checkpoint *t*. Once a student lost scholarship at a checkpoint, I exclude the student from analysis for subsequent checkpoints. In addition, I add my running variable (GPA_{it}) and a vector of covariates that are also included in the event history models above (as summarized in Table 2).[40] Adding covariates in the model does not address biased estimates, but improves precision of my estimates.

$$Y_{it} = \alpha + \beta_1(GPA_{it}) + \delta(lose_{it}) + \phi X_{it} + \varepsilon_{it} \quad (3)$$

Results

Stop-Out

In this section, I first present the results about stop-out, and then proceed to results about graduation. Table 3 and Figure 1 show how the hazard of experiencing first stop-out changes each semester (fall, spring, and summer). Because it is optional to enroll in a summer semester, a student is regarded re-enrolled (persisted) as long as he or she is enrolled in a regular semester (fall or spring). According to Table 3, a large number of students first stopped out after their first academic year. The fifth column of Table 3 shows the survivor function of students. The survivor function goes down to 91.07% and 76.44% after the first and second semester, respectively. That is, 23.56% of students stopped out during the first academic year. After the first academic year, the survivor function decreases at a slow rate. This pattern is illustrated in Figure 1. In Figure 1, each of the red dashed vertical lines indicates the end of an academic year. As can be seen, the survivor function sharply drops at the first red line (at the end of the first academic year) and then gradually decreases.

[40] In the regression discontinuity model, I also add two more terms: 1) the square term of the running variable and 2) the interaction term between the key independent variable and the running variable. The square term is added in case the relationship between the running variable and the outcome variable is quadratic. The interaction term allows the effect (slope) of the running variable differs for scholarship keepers and losers.

Table 3. Life Table of Students Experiencing First Stop-Out

Semester	Students at Risk	Students Stopped	Students Censored	Survivor Function	Std. Error
2004 F	14,394	1,285	0	0.9107	0.0024
2005 S	13,109	2,106	0	0.7644	0.0035
2005 SU	11,003	71	1	0.7595	0.0036
2005 F	10,931	881	0	0.6983	0.0038
2006 S	10,050	942	1	0.6328	0.0040
2006 SU	9,107	71	5	0.6279	0.0040
2006 F	9,031	521	57	0.5917	0.0041
2007 S	8,453	451	24	0.5601	0.0041
2007 SU	7,978	46	218	0.5569	0.0041
2007 F	7,714	284	2,338	0.5364	0.0042
2008 S	5,092	261	494	0.5089	0.0043
2008 SU	4,337	53	1,328	0.5027	0.0043
2008 F	2,956	177	1,265	0.4726	0.0046
2009 S	1,514	123	245	0.4342	0.0054
2009 SU	1,146	24	344	0.4251	0.0056
2009 F	778	74	254	0.3846	0.0067
2010 S	450	0	450	0.3846	0.0067

Note: F means fall, S means spring, and SU means summer.

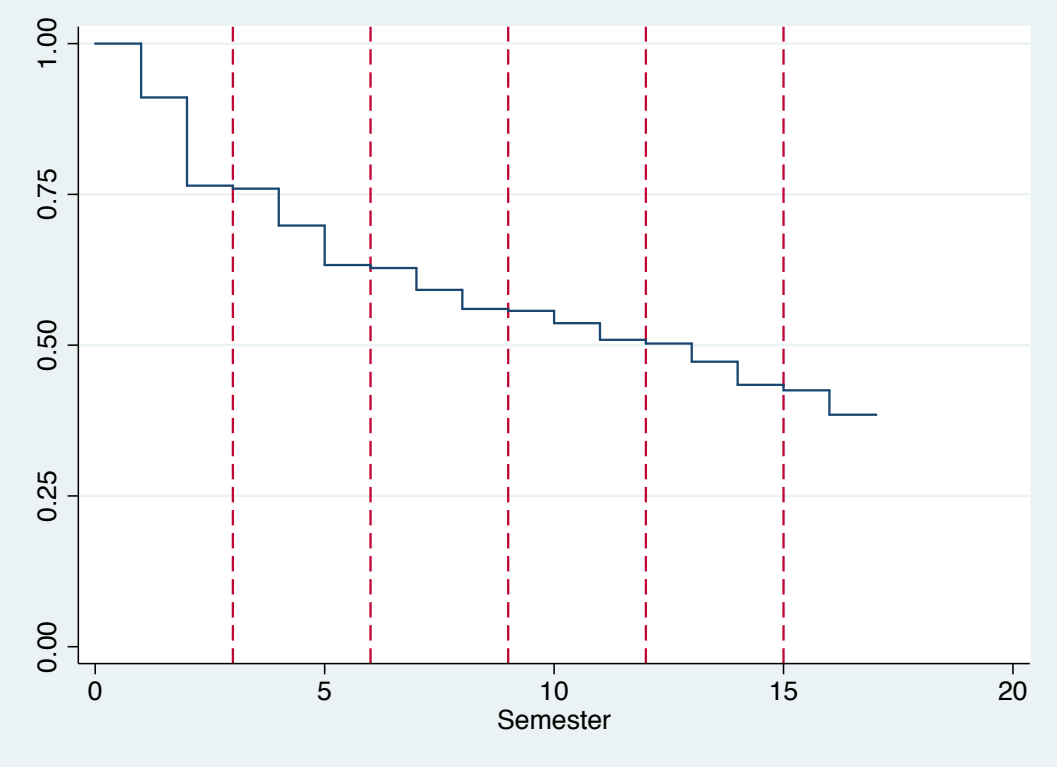

Figure 1. Survivor Function of Student Stop-Out

In Figure 2, I look at how the survivor function differs depending on scholarship renewal status. According to the figure, it seems clear that the probability of re-enrolling (persistence) is the highest among scholarship keepers, followed by scholarship losers and non-recipients. Except after the first semester, the survivor function is always the highest among students who maintained their scholarships.

I confirm these trends with the statistical test. The equality test of survivor functions examines whether survivor functions among different groups are statistically same. Using the log-rank test, I find that the functions are significantly different across the three groups (χ^2=1026.90, p-value=0.000). These results suggest that the scholarship renewal status is significantly associated with stop-out behavior.

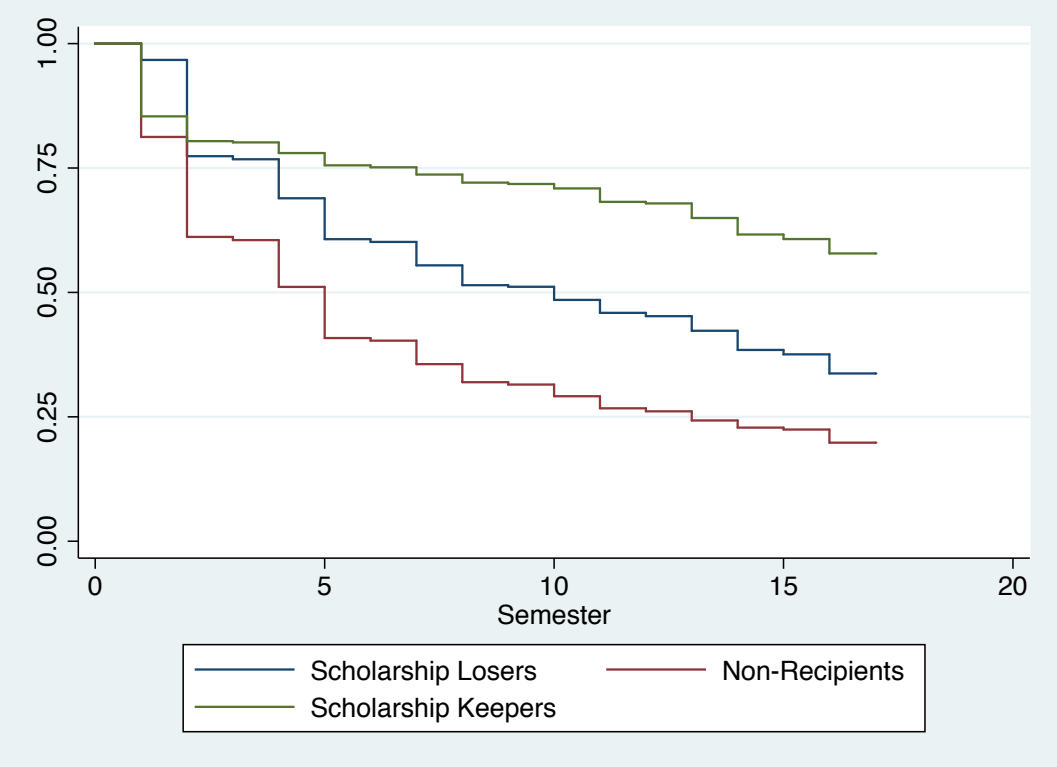

Figure 2. Survivor Function of Student Stop-Out by Scholarship Status

Although the survivor function and log-rank test results show that stop-out probabilities significantly differ based on scholarship renewal status, these results still reflect observable and unobservable differences among scholarship keepers, losers, and non-recipients. In order to take into account observable differences, I run event history models that include covariates. Table 4 shows estimates from these models.

In the first column of Table 4, I present the estimates from the Cox model in terms of the hazard ratio.[41] In parentheses, I present the 95% confidence interval of hazard ratio estimates. In this model, I find that the hazard of stopping out is lower for both scholarship keepers and losers in comparison to non-recipients. Compared to non-recipients, the hazard of stop-out is 3% lower for scholarship losers and 14% lower for scholarship keepers. However, the difference is statistically significant only for scholarship keepers. In other words, the stop-out hazard is not statistically different between scholarship losers and non-recipients. In

[41] The hazard ratio can be calculated by taking exponential on a raw coefficient.

addition to the scholarship renewal status, racial minority students, students whose parent(s) has some college experience, and students with higher ACT scores or high school grades were less likely to stop out. In contrast, students who were eligible for Pell Grants, students who majored in one of the STEM fields, or students who have not decided their majors were more likely to stop out than students who were not eligible for Pell Grants and students in non-STEM majors. There is no gender difference in terms of stop-out behavior. The effects of these covariates are consistent in other models.

In the second column of Table 4, I add interaction terms between scholarship renewal status and time periods in addition to model 1. As previously mentioned, my data does not pass the proportional hazards assumption test. This suggests that the effects of independent variables are not constant over time. By adding the interaction terms between key independent variables and time period, I could see whether and how the effects of key independent variables change over time.

In case of scholarship losers, the hazard of stopping out is lower than that of non-recipients in the beginning. The main coefficient for scholarship losers is 0.738 in the form of hazard ratio. However, when I consider coefficients on the main and interaction terms, the hazard for scholarship losers gradually increases. As a result, the hazard of stopping out for scholarship losers becomes higher than that for non-recipients since the sixth semester. In other words, losing TELS can have worse consequences even compared to never receiving it after a few semesters. Figure 3 illustrates how the hazard of stopping out changes over time in comparison to non-recipients (the red horizontal line).

Table 4. Event History Model Estimates (Outcome: Stop-Out)

Variables	Model (1) Cox	Model (2) Cox with interaction	Model (3) Weibull	Model (4) Discrete-time
Lose	0.972	0.738***	0.893**	0.544***
	(0.903, 1.046)	(0.659, 0.828)	(0.830, 0.962)	(0.475, 0.622)
Keep	0.860**	1.345***	0.787***	1.277**
	(0.784, 0.944)	(1.176, 1.538)	(0.717, 0.865)	(1.097, 1.485)
Female	1.001	1.000	1.011	0.895**
	(0.944, 1.061)	(0.943, 1.060)	(0.953, 1.072)	(0.837, 0.957)
Minority	0.919*	0.911*	0.909*	0.889**
	(0.852, 0.991)	(0.845, 0.982)	(0.843, 0.979)	(0.815, 0.969)
Pell eligibility	1.276***	1.271***	1.260***	1.379***
	(1.200, 1.357)	(1.195, 1.351)	(1.185, 1.339)	(1.285, 1.480)
Parent College	0.890***	0.890***	0.886***	0.858***
	(0.839, 0.944)	(0.839, .0944)	(0.835, 0.940)	(0.801, 0.917)
High School GPA	0.525***	0.517***	0.522***	0.506***
	(0.492, 0.560)	(0.484, 0.551)	(0.490, 0.557)	(0.470, 0.545)
ACT score	0.970***	0.970***	0.973***	0.966***
	(0.960, 0.979)	(0.960, 0.979)	(0.964, 0.983)	(0.956, 0.977)
STEM major	1.126**	1.136**	1.201***	1.067
	(1.034, 1.225)	(1.045, 1.237)	(1.104, 1.307)	(0.970, 1.173)
Undecided major	1.781***	1.772***	2.253***	1.767***
	(1.658, 1.913)	(1.650, 1.903)	(2.101, 2.417)	(1.627, 1.919)
Lose*Time	N/A	1.062***	N/A	1.097***
		(1.037, 1.087)		(1.069, 1.125)
Keep*Time	N/A	0.887***	N/A	0.891***
		(0.861, 0.915)		(0.863, 0.920)
Number of students	12,626	12,626	12,626	73,402
Institutional FE	Yes	Yes	Yes	Yes
Semester FE	No	No	Yes	Yes

Note: hazard ratios (models 1, 2, and 3) or odds-ratio (model 4) as coefficients, 95% CI in parentheses.
Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001

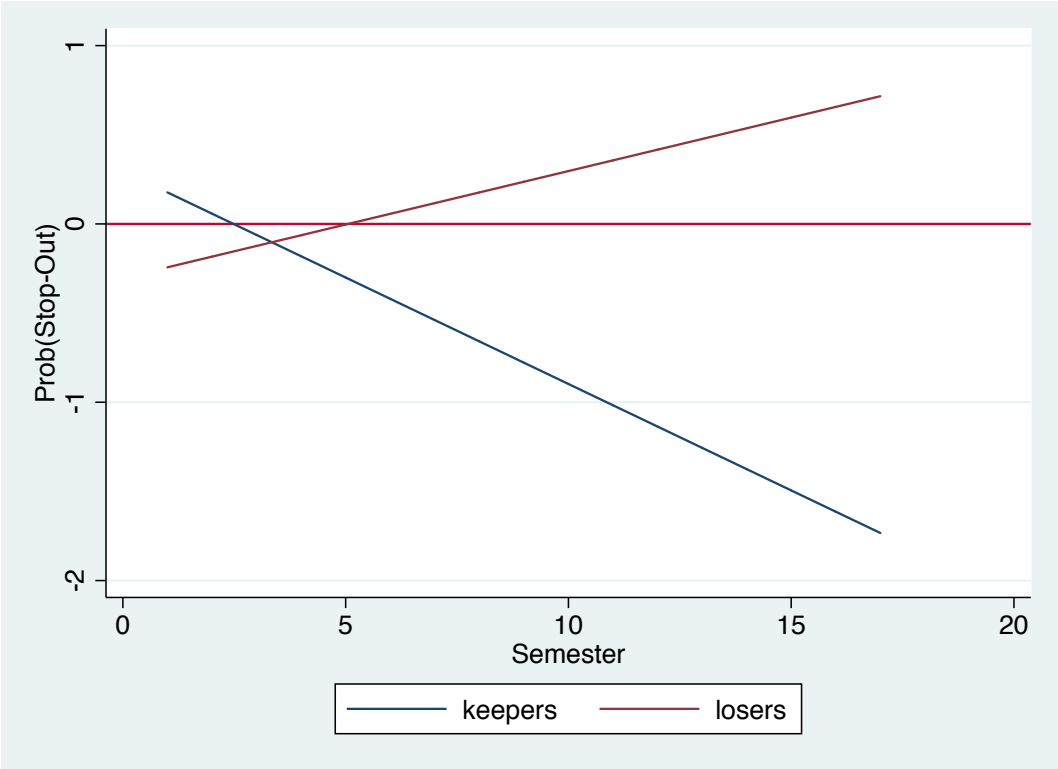

Figure 3. Probability of Stop-Out (Ref: Non-Recipients)

In the third and fourth columns of Table 4, I present estimates from the Weibull and discrete-time analysis models, respectively. As can be seen, the results are qualitatively the same across different models. In the third column, I present the estimates from the Weibull model without the interaction terms. This model is equivalent to model (1) except that it has a different assumption about the baseline hazard. Without the interaction terms, the hazard of stopping out is lower for both scholarship losers and keepers compared to non-recipients. When I estimate the discrete-time model without interaction terms (not presented in Table 4), the estimates are similar to the ones from models (1) and (3). When I include interaction terms in model 4, the results are consistent to the one from model (2). The hazard of stopping out for scholarship keepers is higher only for the first two semesters, and then becomes lower. In contrast, the hazard for scholarship losers is lower for the first six semesters, and then starts to be higher than non-recipients.

To summarize, after controlling for students' demographic characteristics and academic preparation, scholarship keepers still face a lower hazard of stopping out compared to non-recipients at almost all time periods. This appears reasonable given that scholarship recipients were academically better prepared and had more monetary resources. However, scholarship losers face a higher hazard of stopping out than non-recipients after the first two to three years. Given that many students in the sample lost their scholarships within the first two years of their enrollment, this result may suggest that losing the scholarships have negative effects on student persistence after losing their aid.

Graduation

In this section, I present the effect of keeping, losing, or never receiving TELS on degree attainment within six years. Table 5 shows that a large number of students graduated from college in their senior year. In the fifth column of Table 5, the survivor function sharply decreases from 0.97 to 0.74 in Fall 2007 (the beginning of their senior year). It again suddenly drops from 0.53 to 0.38 in Fall 2008 (the beginning of their fifth year). In other words, more than one-half of the sample graduated within four years of their initial enrollment. However, about 23% of the sample neither graduated nor dropped out of college even after the six years. This pattern is also illustrated in Figure 4. In the figure, each of the red dashed vertical lines indicates the end of the third, fourth, and fifth academic year. As can be seen, very few students graduated up until the end of the ninth semester (their third year), and then the survivor function sharply decreases within the next year.

Table 5. Life Table of Students Experiencing Degree Attainment

Semester	Students at Risk	Students Stopped	Students Censored	Survivor Function	Std. Error
2006 SU	10,952	3	49	0.9997	0.0002
2006 F	10,900	54	363	0.9948	0.0007
2007 S	10,483	22	376	0.9927	0.0008
2007 SU	10,085	214	44	0.9716	0.0016
2007 F	9,827	2352	322	0.7391	0.0044
2008 S	7,153	507	317	0.6867	0.0046
2008 SU	6,329	1408	65	0.5339	0.0051
2008 F	4,856	1388	281	0.3813	0.0050
2009 S	3,187	304	269	0.3449	0.0050
2009 SU	2,614	453	69	0.2852	0.0048
2009 F	2,092	408	265	0.2295	0.0046
2010 S	1,419	0	1415	0.2295	0.0046

Note: F means fall, S means spring, and SU means summer.

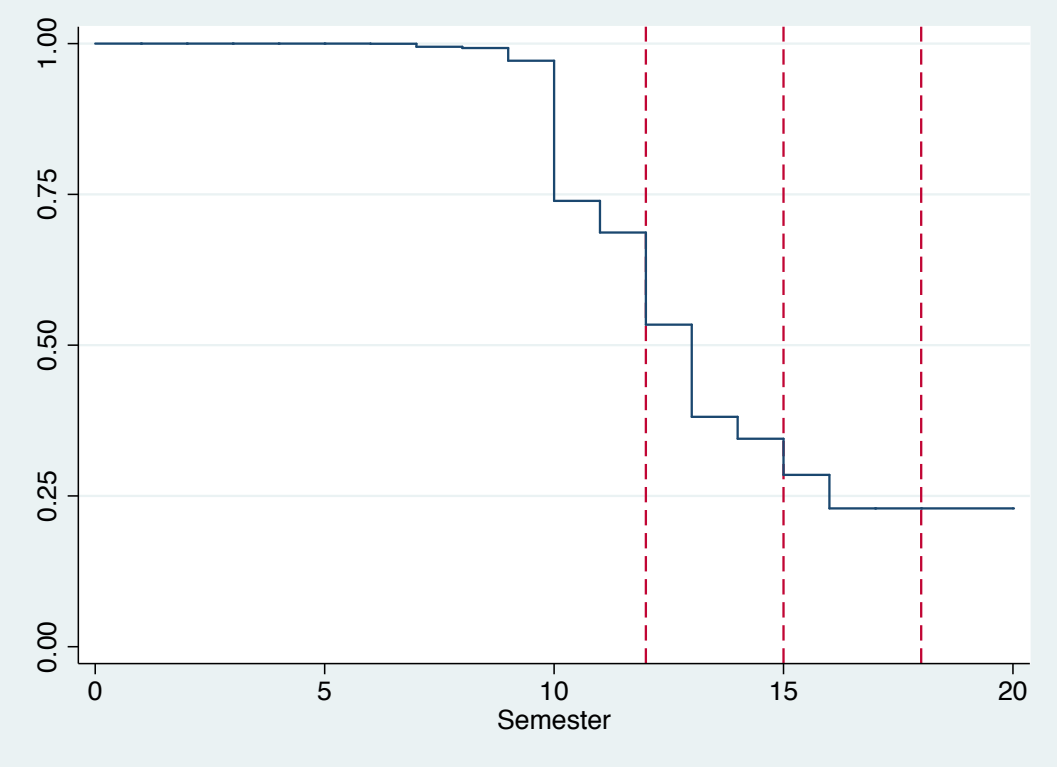

Figure 4. Survivor Function of Degree Attainment

When I look at the survivor function based on scholarship renewal status, it is clear that students who received and kept TELS were the most likely to earn a bachelor's degree within six years. Figure 5 shows that the survivor function is always lower for scholarship keepers than for the other two groups, which indicates that scholarship keepers were the most likely to graduate at every semester. Between students who lost the scholarships and students who never received one, the survivor function is always lower for those who lost the scholarships. In other words, in terms of the average graduation rates, the graduation rates are the highest for scholarship keepers and the lowest for non-recipients. However, again, the average rates do not take into account any differences among these groups.

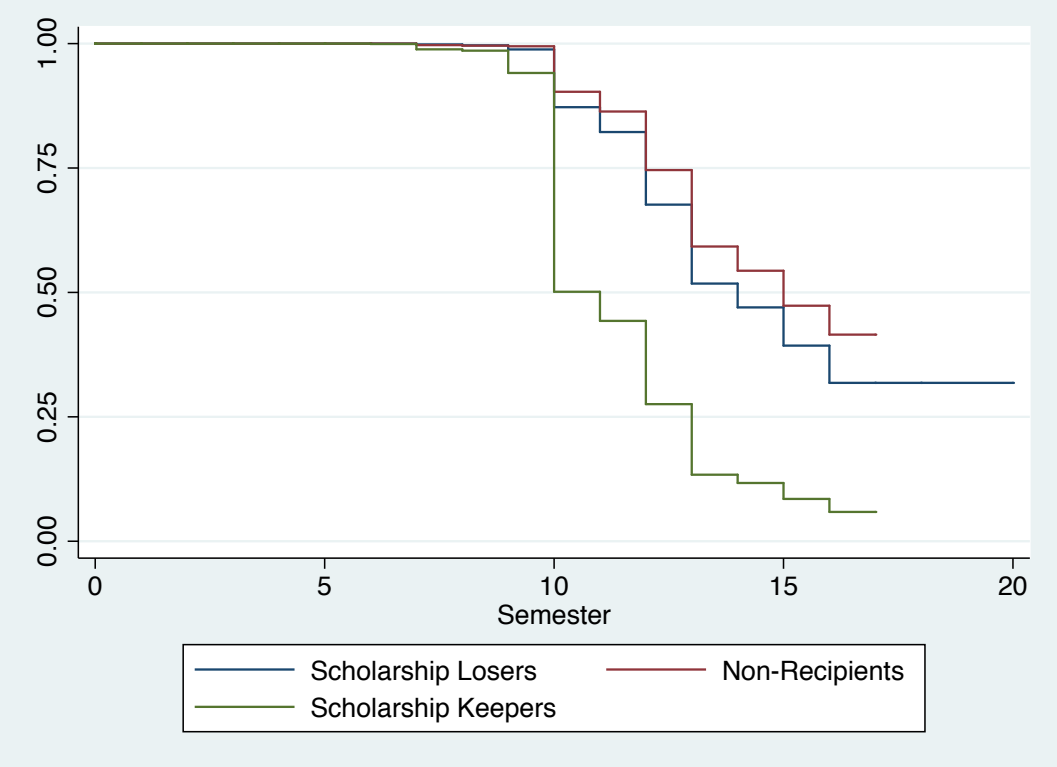

Figure 5. Survivor Function of Degree Attainment by TELS Status

Table 6 shows the estimates from event history models that include covariates. The first model in Table 6 presents estimates from the Cox model without the interaction terms between key independent variables and time periods. This model indicates that scholarship keepers have a higher hazard of graduation by 91% than non-recipients do. The hazard for scholarship losers is 14% lower than that for non-recipients, and the difference between the two groups is marginally significant. In addition to the key variables, being female, being minority, and having a higher ACT score or high school grade increase the hazard of graduation, while being eligible for Pell Grants, majoring in STEM, or having not decided their majors significantly decrease the hazard. After demographic characteristics and academic performances are controlled for, parental education levels are not significantly associated with graduation.

Table 6. Event History Model Estimates (Outcome: Graduation)

Variables	Model (1) Cox	Model (2) Cox with interaction	Model (3) Weibull	Model (4) Discrete-time
Lose	0.856+	0.166***	0.925	0.755***
	(0.727, 1.008)	(0.061, 0.452)	(0.787, 1.088)	(0.698, 0.816)
Keep	1.905***	3.461*	2.283***	2.743***
	(1.601, 2.267)	(1.263, 9.484)	(1.920, 2.714)	(2.509, 2.998)
Female	1.330***	1.325***	1.312***	1.135***
	(1.229, 1.440)	(1.224, 1.433)	(1.213, 1.420)	(1.096, 1.176)
Minority	1.102+	1.090	1.074	0.836***
	(0.992, 1.225)	(0.981, 1.211)	(0.967, 1.193)	(0.799, 0.874)
Pell eligibility	0.375***	0.374***	0.247***	0.608***
	(0.340, 0.414)	(0.339, 0.413)	(0.224, 0.273)	(0.586, 0.631)
Parental college	1.018	1.013	1.024	1.194***
	(0.937, 1.105)	(0.933, 1.100)	(0.943, 1.111)	(1.152, 1.238)
High School GPA	1.458***	1.455***	1.495***	1.933***
	(1.334, 1.594)	(1.330, 1.590)	(1.368, 1.633)	(1.858, 2.010)
ACT score	1.013*	1.011+	1.009	0.996
	(1.000, 1.025)	(0.999, 1.024)	(0.996, 1.021)	(0.991, 1.002)
STEM Majors	0.749***	0.749***	0.744***	0.667***
	(0.679, 0.826)	(0.679, 0.826)	(0.675, 0.821)	(0.637, 0.699)
Undecided Majors	0.081***	0.081***	0.082***	0.454***
	(0.052, 0.124)	(0.052, 0.124)	(0.053, 0.126)	(0.434, 0.476)
Lose*Time	N/A	1.141**	N/A	1.035***
		(1.052, 1.237)		(1.023, 1.048)
Keep*Time	N/A	0.945	N/A	1.035***
		(0.869, 1.027)		(1.021, 1.050)
Number of students	12,861	12,861	12,861	82,956
Institutional FE	Yes	Yes	Yes	Yes
Semester FE	No	No	Yes	Yes

Note: hazard ratios (models 1, 2, and 3) or odds-ratio (model 4) as coefficients, 95% CI in parentheses.
Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001

In model 2, I add the interaction terms between scholarship renewal status and time periods. When including the interaction terms, scholarship keepers still have a higher hazard of graduation from the beginning. And, the coefficient on the interaction term between the scholarship keeper indicator and time periods is not statistically significant. That is, the effect of keeping the scholarships on graduation does not change over time.

In contrast, the effect of losing the scholarships increases over time. At the beginning, the hazard of graduation is much lower for scholarship losers than non-recipients. However, given that no one graduates within first two years, this coefficient alone is not meaningful. When considering the coefficient on the interaction term, the hazard of graduation for scholarship losers gradually increases, and then it becomes higher for scholarship losers from the spring semester of the fifth year.

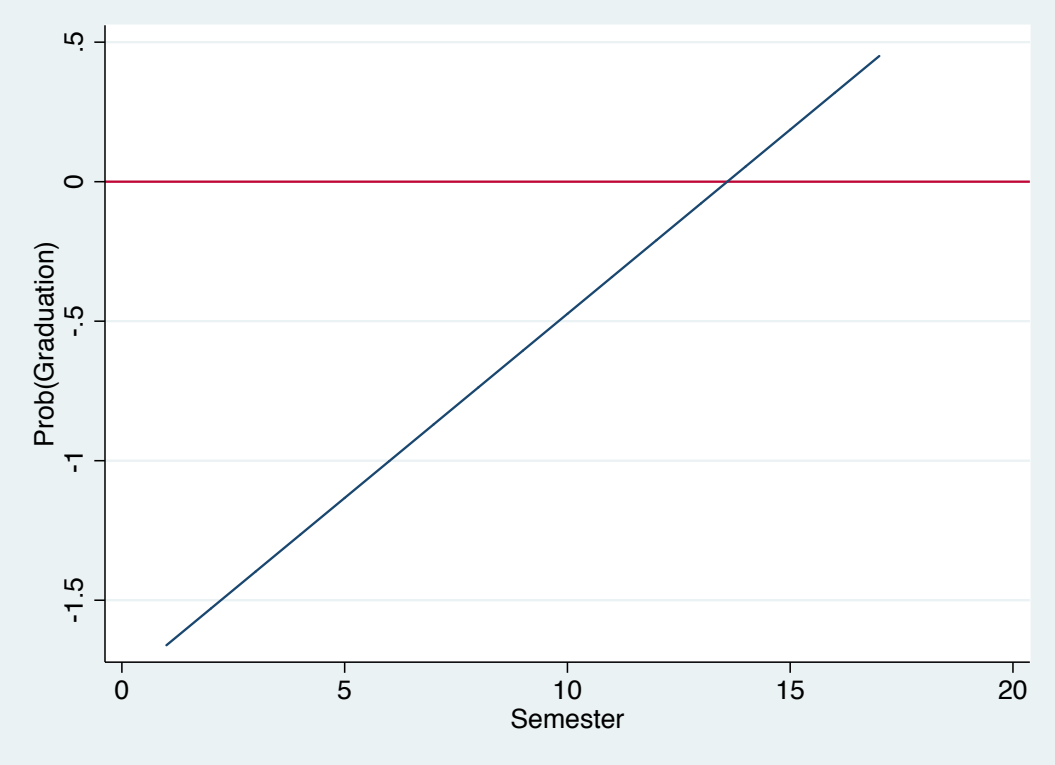

Figure 6. Probability of Graduation for Scholarship Losers (Ref: Non-Recipients)

In models 3 and 4, I run the Weibull and discrete-time analysis models, respectively. These results are consistent to the results observed in models 1 and 2 in Table 6. The only difference is that there are a few more coefficients that become statistically significant in model 4. These significant results can be attributed to the operationalization of the outcome variable in model 4. While the outcome variable in models 1 and 3 is whether a student earned a bachelor's degree in each semester (time-varying), the outcome variable in model 4 is time-

constant variable that indicates whether a student earned a bachelor's degree anytime within six years. If we ignore the timing of graduation, a few more covariates are significantly associated with graduation. Other than this, my results are robust across different model specifications.

Sensitivity Check

Lastly, I run a regression discontinuity model as specified in equation (3) to see if my results are consistent across different model specifications. As mentioned earlier, an event history model does not control for unobservable differences among scholarship keepers, losers, and non-recipients. However, a regression discontinuity model is as good as a random experiment in terms of dealing with selection bias when its key assumptions are met.

Unfortunately, regression discontinuity estimates in this study do not entirely remove selection bias although they substantially reduce it. In order to check if my data meets the key assumption of the regression discontinuity method, I run the McCrary test at each of five checkpoints. According to Figure 7, the McCrary test shows that the number of students who barely met the renewal requirement at their first check point (24 credits attempted) is much higher than the number of students who barely missed it at that point. Although not presented, the McCrary test results are consistent for all five checkpoints. These results suggest that scholarship keepers who barely met the renewal requirement were somewhat different from scholarship losers who barely missed it. Students in the former group seemed to be more desperate and motivated to renew scholarship eligibility than students in the latter group. The possible difference between these two groups challenges the critical assumption of the regression discontinuity model. Despite this limitation, I present the estimates from the regression discontinuity model because they could reflect the net impact of losing merit-based aid more accurately by focusing on a more homogeneous group of students around the renewal threshold compared to estimates from the event history models.

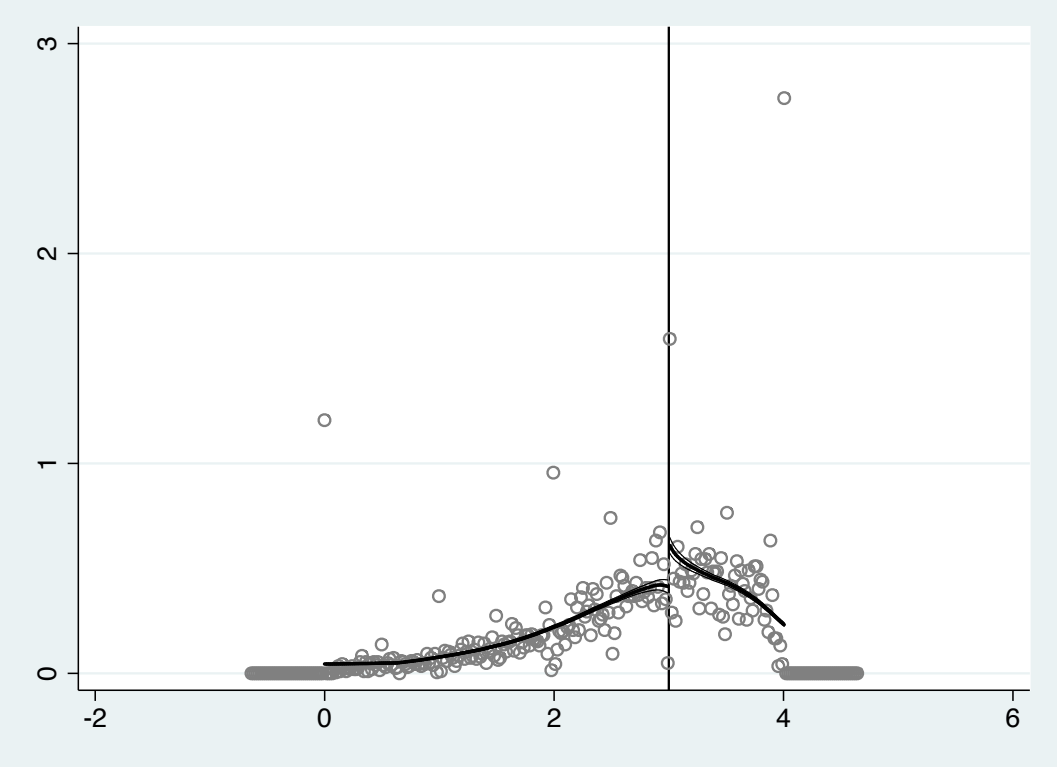

Figure 7. McCrary Test Results (First Check Point)

Table 7 shows results from the regression discontinuity model at each of the five checkpoints. In the table, I present only the coefficient on the key independent variable (δ from equation 3), which represents the effect of losing merit-based aid by failing to meet the renewal requirement by a slight margin. Surprisingly, the regression discontinuity estimates are not consistent with the results from event history models. At the first two checkpoints, students who lost their scholarships tend to have a lower probability of stop-out and a higher probability of graduation. As I include more students by slightly increasing the bandwidth, these estimates become not statistically significant.[42] However, the simple mean difference in the outcome variables between scholarship keepers and losers still indicates that scholarship losers tend to do better (fewer stop-outs and more graduates) than scholarship keepers when I limit my sample to students near the cut-off GPA. This counter-

[42] The bandwidth used in Table 7 is 0.04. When I run the same model using a variety of bandwidths from 0.02 to 0.13, I find that the coefficient becomes not statistically significant although the sign of estimates is consistent. For stop-out outcome, estimates are statistically significant for smaller bandwidths from 0.02 to 0.06. For graduation outcome, estimates are statistically significant only when the bandwidth is 0.02 or 0.04.

intuitive result seems to reflect that there are many more scholarship keepers than losers within the bandwidth rather than the beneficial impact of losing merit-based aid. Among many students who met their renewal eligibility by a slightest margin, some of them might have earned their GPA through asking their professors for better grades or taking easier courses. If this were true, their GPA might not truly measure their academic ability.

Table 7. Regression Discontinuity Estimates

Checkpoint	1	2	3	4	5
Drop-Out	-0.637**	-0.228+	-0.092	0.115	0.069
	(0.202)	(0.128)	(0.094)	(0.092)	(0.109)
Graduation	0.374* (0.190)	0.226*	0.098	-0.134+	0.078
		(0.111)	(0.082)	(0.073)	(0.079)

Note: Estimates are the coefficients on the key independent variable that indicates whether students lost their scholarship at each checkpoint. Once students lost the scholarships, I exclude these students for analysis at subsequent checkpoints. Full results are available upon request.
Note: p-value: +: <0.10, *: <0.05, **: <0.01, ***: <0.001, Standard errors are reported in parentheses.

To summarize, results from the regression discontinuity model shows that there is no significant consequences of losing merit-based aid on both persistence and graduation at least for students whose GPA is very close to the cut-off point. In other words, when students lose their scholarships by a slightest margin, it does not significantly harm their probability of re-enrolling and graduating from college. This result is somewhat contrary to the findings from event history models where I find that scholarship keepers persisted and graduated at a much higher rate than non-recipients, while scholarship losers did not. These contrasting results between the two models might suggest that the positive impact of keeping scholarships observed in the event history models occurred among highest-achieving students whose cumulative GPA was far right from the renewal threshold. It can be interpreted as either no real impact of receiving scholarships after the first year or heterogenous effects of receiving scholarships after the first year depending on students' academic

performances (e.g., a large impact on highest-achieving students as opposed to a marginal impact on average students).

Conclusion

During the last two decades, more than a dozen states adopted merit-based aid. Merit-based aid is unique in that students must renew their scholarship eligibility periodically by maintaining their cumulative GPAs above a certain point. Due to this renewal requirement, a number of students lose their merit-based aid after a couple of years. This study examines whether losing merit-based aid affects student persistence and graduation. Using administrative data from Tennessee, I compare the probability of stop-out and graduation among scholarship keepers, losers, and non-recipients with event history models. I also employ the regression discontinuity method to minimize selection bias between scholarship keepers and losers by focusing on the two groups of students who received or lost their scholarships by a slight margin. I draw several conclusions based on my findings.

First, receiving and maintaining merit-based aid is positively associated with persistence and graduation. Based on the results from event history analysis, scholarship keepers have higher probabilities of re-enrolling and earning a bachelor's degree at almost every semester compared to non-recipients. This is consistent with previous studies (Henry, Rubenstein, & Bugler, 2004; Scott-Clayton, 2011) and encouraging in that the huge investment in merit-based aid actually has helped students succeed.

However, it is not yet clear if this positive relationship is causal. As I focus on students who renewed or failed to renew scholarship eligibility by a slightest margin, the positive impact of renewing the scholarships becomes not statistically significant. Even more puzzling, students who lost their scholarships at the first two check points (at the end of the semesters in which they attempted 24 and 48 credits) by a slight margin had better college outcomes than students who barely kept their scholarships during the same period.

These contrasting results across different statistical models can be interpreted in two ways. First, these results indicate that the positive relationship between merit-based aid and college success is correlational rather than causal. Although I control for many covariates, some unobservable factors still remain and affect both merit-based aid eligibility and college success. If this were the case, merit-based aid might help students who are already doing well succeed in college by lightening their financial burden and rewarding their hard work. However, even in the absence of merit-based aid, these students would have succeeded anyway. This raises a question if providing these students with merit-based aid is efficient from the perspective of society (and states).

Another way to interpret the contrasting results is that receiving merit-based aid at the time of college entry matters, but its impact is much smaller once students entered college. This view is similar to Hossler et al., (2009), who conclude that financial aid has a null or relatively small impact on college persistence than it has on initial enrollment. For example, students might find a way to finance their education by getting an on-campus job or having better knowledge about financial aid once they attend college. If this were the case, states might want to reconsider their merit-based aid design and change it into a form of front-loading.

Second, losing scholarships has a slightly better effect on persistence and graduation than never receiving them. Based on the estimates from event history models, scholarship losers were less likely to stop out for the first two years and more likely to graduate after the fourth year. At other time periods, scholarship losers did not do better than non-recipients. These results seem plausible considering that students began to lose their scholarships after their second semester. For the first two years when many scholarship losers had received their aid, they persisted at a higher rate than non-recipients who did not receive the aid at the time. However, as a growing number of students lost their scholarships later, scholarship losers started stopping out of college and taking more time to graduate. Of course, this is one possible interpretation, and the mechanism needs to be further examined.

This study contributes to the literature by examining college outcomes of students who once received merit-based aid, but lost it later. As merit-based aid in most states requires students to renew their scholarship

eligibility, a number of students lost their scholarships across states. However, few studies differentiate this group of students from initial recipients and explore their college outcomes. My study helps to fill the gap in the literature by investigating the effect of losing merit-based aid at different semesters on student persistence and graduation.

In order to better understand the effect of losing scholarships on student success, there are some recommendations for future research. First, future research needs to examine whether the effect of losing merit-based aid differs across different income groups. A student's family income level not only affects her college outcomes but also influences the way she responds to financial aid. Given that low-income students are more responsive to price changes (e.g., Alon, 2011), losing merit-based aid might have different effects between low-income and middle-income students. Due to data availability, this study could not take into account the effect of family income but Pell Grants eligibility. Future research with detailed family income information can explore the proportion of income groups within scholarship loser population as well as heterogeneous responses of scholarship losers based on family income.

Second, future research needs to explore how students who lose their merit-based aid finance their education after they lose it. Do they find a part-time job, borrow more money, or take a year off to afford their college education? Financial aid literature has long focused on the effects of receiving a specific financial aid program, but it has rarely examined how students cover their unmet needs. Exploring this question will help better understand the mechanism of the relationship between losing a scholarship and student success.

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CHAPTER V

CONCLUSION

My dissertation examines whether merit-based aid affects college affordability, student persistence, and degree attainment. Overall, my dissertation finds little evidence of unintended consequences of merit-based aid on college prices. However, neither does it provide strong evidence that merit-based aid has a causal impact on student persistence and graduation. I present several conclusions derived from my findings.

In the first study of my dissertation, although estimates are widely varied across states, there is limited evidence that colleges raised student charges or reduced the amount of institutional aid in response to the creation of merit-based aid. Colleges in many states either decreased or maintained their student charges, in some cases, even with increases in the amount of institutional aid. These results call the Bennett hypothesis into question, which argues that colleges take advantage of increased federal student aid by raising their tuition. After all, colleges do not seem to take advantage of this newly created financial aid.

Despite the encouraging findings, one question remains: why do colleges in different states respond differently to merit-based aid? In the dissertation, I explore if incentive from a merit-based aid program or each state's higher education governance structure can explain the differences across states. However, these two factors just partially explain the results. In order to better understand the way colleges respond to merit-based aid, future research needs to consider the overall context of each state such as the availability of other state grants, state tuition policy, or demand for higher education.

The second study of this dissertation shows that receiving merit-based aid does not change whether a student earns a bachelor's degree or not. Instead, it reduces the time taken to obtain the degree. In other words, once enrolled in college, receiving merit-based aid does not make a big difference in terms of six-year

graduation. The null effect of merit-based aid on degree attainment is a bit surprising, especially given that recipients come from advantaged backgrounds at a higher rate than non-recipients.

The null effect on degree attainment can be explained in two ways. First, as Hossler et al., (2009) conclude, financial aid may have a small or insignificant impact on persistence, in contrast to its significant and positive impact on enrollment. That is, once students enrolled in college, financial aid might not be a driving predictor of persistence and graduation. Second, it is possible that financial aid actually matters to persistence and degree attainment. However, the amount of financial aid received can be even out between merit-based aid recipients and non-recipients for two different reasons. Even if students were not eligible for merit-based aid, they could have received financial aid from other sources (e.g., federal aid, state need-based grants, or institutional aid). As a result, they might not have hard time financing their education and re-enrolled at the same rate as scholarship recipients did. Or, it is also possible that students who received their merit-based aid lost their scholarships after a couple of semesters. Without additional aid from other sources, initial recipients who lost their aid experienced the same financial hardship as students who never received it. My data allows me to examine this last possibility in the third study of my dissertation.

In the third study of my dissertation, I find that losing scholarships is negatively associated with persistence and degree attainment. When comparing scholarship keepers, losers, and non-recipients, scholarship keepers had the highest probability of re-enrolling in college and earning a bachelor's degree. Compared to scholarship keepers, both scholarship losers and non-recipients did not fare well in terms of the two outcomes. However, it is not clear if losing scholarships has as negative impact as never receiving it. These results indicate that the impact of merit-based aid is not large enough to affect degree attainment when students lost their aid a couple of semesters later.

As a whole, my dissertation asks whether merit-based aid has some unintended and negative consequences on college affordability and student success. Despite its popularity among the public as well as in the research circle for the last two decades, a majority of previous studies focus on its effects at the college

entry level (e.g., college enrollment or access) without paying much attention to other areas (e.g., scholarship loss). Overall, this dissertation demonstrates that the availability of merit-based aid helps students who are eligible for the aid to pay their tuition without accompanying additional tuition increases or institutional aid decreases. However, it is not yet clear if this financial support from their states translates into their persistence and degree attainment. This conclusion is concerning considering that the ultimate goal of giving financial aid and making college education affordable is helping students go through the pipeline to earn their college degree. Facing the bad economy as well as other areas that need public subsidies (e.g., K-12 education, health care, etc.), many states may not be able to afford investing their money in higher education if their investment is not cost-effective. However, before making any major decision, it is necessary to explore other options as well as learn more about the effect of merit-based aid. In particular, I think that more work, both policy and research, needs to be done regarding the issue of losing merit-based aid in college.

In terms of policy, I suggest that more emphasis should be on students' effort in college rather than their academic preparation prior to college attendance. High standardized test scores and high school grades reflect the academic capability and effort of students in high school, which are expected to continue in college. However, once in college, many students face difficulties in navigating and making sense of their college system. Even though students know that they can pay their tuition with the state money as long as their cumulative GPA is above a 3.0, they might not know much about how to make that happen. If students were given more specific and timely guidance regarding how to do well in college, it could make a substantial difference in their persistence and graduation outcomes. This idea of combining counseling service with financial aid is currently studied, and its preliminary results seem promising (e.g., Patel, Richburg-Hayes, Campa, & Rudd, 2013).

When it comes to research, more research needs to work on predictors and effects of losing merit-based aid. First of all, has the availability of merit-based aid attracted academically underprepared students to public four-year colleges and contributed their failure to renew their scholarship eligibility? Answering this question

not only sheds light on the effect of merit-based aid but also contributes to the literature about mismatch between students and college (Bowen, Bok, & Loury, 2000; Sander & Taylor, 2012). Second, it is also important to know what happens to students once they lose their merit-based aid. How do these students finance their education, and does it affect their persistence or degree attainment? Does losing merit-based aid affect their social and psychological well-being in college, which might be related to their persistence and degree attainment? Pursuing these questions will bridge the gap between the two important areas of persistence research, each of which has been examined separately: financial and social aspects of student persistence (Nora, Barlow, & Crisp, 2006). Lastly, as a way to test the effect of losing merit-based aid on student persistence and degree attainment, it would be interesting to examine whether lowering the renewal GPA threshold affects student persistence and degree attainment. In Tennessee, since 2008, the renewal GPA requirement for the first two checkpoints has lowered from a 3.0 to a 2.75. If maintaining merit-based aid has a net and positive effect on degree attainment, this policy change would have increased the probability of earning a college degree among its recipients. In contrast, if receiving merit-based aid does not have a net effect, the policy change would not have made a substantial difference. Exploring these questions will shed light on a relatively unexplored area of financial aid research, the effect of losing financial aid, as well as provide more concrete evidence about whether and how merit-based aid works.

In addition, following my dissertation, I will continue to examine the effect of higher education policies on student access and success. Of many research ideas, I present four research plans below. The first two ideas are extended from my dissertation, while the latter two explore the issues in college access and success more broadly.

Firstly, I will explore how colleges use their institutional aid, and its impact on student access and success. Institutional aid has long been used as a tool for enrollment management. Colleges use institutional aid not only to recruit a sufficient number of students but also to attract students with desirable attributes. I am interested in whether there is a change in the recipients of institutional aid as many states have adopted

statewide merit-based aid. With a large number of students eligible for merit-based aid, students will need a smaller amount of institutional aid. This will lead to a surplus of institutional aid unless colleges reduce the total amount of institutional aid offered. How do colleges distribute this surplus? What kind of students do receive the money? Answering these questions will have implications for college access as an increasing number of colleges seek students with solid academic records, who can also pay full tuition rather than students from disadvantaged backgrounds (Woo & Choy, 2011).

Secondly, when examining the impact of merit-based aid on student success, I will consider its impact on student composition (college choice) in addition to its direct impact on student success. The adoption of statewide merit-based aid could affect the student body composition in in-state public four-year colleges. Depending on their admission policies and the eligibility requirement of merit-based aid, merit-based aid can attract either highest-achieving students (e.g., Castelman, 2013) or academically underprepared students who would have gone to community colleges without the aid (e.g., Binder, Ganderton, & Hutchens, 2002). Either way, the change in student body would have an impact on student success. In a future study that examines the effect of merit-based aid on graduation, I plan to use a model that reflects the change in student body. If the latter effect is larger than the former effect, we might need to reconsider the efficiency of the current design of merit-based aid, which gives monetary support for a large number of students with solid academic records.

Thirdly, I plan to empirically test a key assumption of the human capital theory: students make a college decision after weighing the benefits and costs associated with college education. Although the theory has been universally grounded in financial aid research, critics point out that high school students are not as rational as they are assumed to be in the theory (DesJardins & Toutkoushian, 2005). In future research, I will first explore if high school students have accurate information regarding the benefits and costs associated with college education. Then, I will examine if the perceived benefits and costs are related to students' college enrollment decisions such as attendance and choice. Results from this study will not only provide evidence to test the theory but also have an implication about college access for students from disadvantaged backgrounds. Except

for a few previous studies, little is known about how high school students actually perceive college benefits and costs (Beattie, 2002; Dominitz & Manski, 1996; Kim, DesJardins, & McCall, 2009; Perna, 2008). Demonstrating perceived benefits and costs of students would help policy makers find which information they need to provide to students for their informed decisions.

Lastly, more broadly, I am interested in the long-term process of how students decide to attend college. At this point, the three central predictors of college attendance are academic preparation, financial resources, and information about college education (or application process). Although my dissertation focuses on the role of the second factor, these three factors are closely related to each other. I would like to know how students develop these three factors over time, and if possible, which policy component can help students from disadvantaged backgrounds improve their outcomes on the three factors.

College education may not be for everyone. Although college education has been a critical step to middle-class life, many people might find their happiness outside college. Hence, it might not be the best way to spend limited resources on ensuring that all students are college-ready. However, once students decide to go to college, they should be given an opportunity to attend college and succeed on their pathway to a degree. I hope my research can contribute to design, implement, and evaluate educational policies that help students' journeys.

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