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Essays on the Economics of Higher Education

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For Lindsey and Simon

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Essays on the Economics of Higher Education

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This dissertation contains three chapters that examine the effect of price in higher education. The first chapter considers the effect of community college tuition on college enrollment using a natural experiment in Texas where discounts for community college tuition were expanded over time and across geography. Additionally, the long-term effects of community college are examined including transfer to universities and graduation with a bachelor's degree. This chapter uses Texas administrative data from 1994-2012 on the universe of high school graduates and their college enrollment and graduation. For high school graduates, community college enrollment in the first year after high school increased by 7.1 percentage points for a $1,000 decrease in tuition. Lower tuition also increased transfer from community colleges to universities. There is also marginally statistically significant evidence that attending a community college increased the probability of earning a bachelors degree within eight years of high school graduation by 23 percentage points.

The second chapter examines whether students respond to immediate financial incentives when choosing their college major. From 2006-07 to 2010-11, low-income students in technical or foreign language majors could receive up to $8,000 in Federal Science and Mathematics Access to Retain Talent (SMART) Grants. Since income-eligibility was determined using a strict threshold, this chapter determines the causal impact of the grant on student major with a regression discontinuity design. Using administrative data from public universities in Texas, it is estimated that income-eligible students were 3.2 percentage points more likely than their ineligible peers to major in targeted fields. Brigham Young University had a larger impact of 10.1 percentage points.

The third chapter considers the effect of financial aid arising from students being declared financially independent on educational outcomes including reenrollment, credits attempted, and graduation. Students who are 24 at the end of the calender year cannot be declared dependent while students who are 23 at the end of the year can be. This sharp change in eligibility is leveraged to compare dependent students to independent students in a regression discontinuity framework. The analysis uses administrative data from from all public universities and colleges in Texas from 2003-04 to 2013-14. Financial independence is associated with modest changes in educational outcomes.

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Chapter 1

College on the Cheap: Costs and Benefits of Community College

1.1 Introduction

Understanding the decision to enroll in post-secondary education and its long-run consequences has long been a topic of interest to economists as well as policymakers. There is now much work focusing on student investment in four-year colleges; however, much less is known about investment in and consequences of community college. This is despite the fact that, in 2011, community college students represented 45 percent of all students enrolled in higher education and 42 percent of first time freshmen.[1]

Community colleges have recently received increased attention due to a proposal by the President that would make community college free. This proposal has been the subject of much debate but very little is known about the effect of community college price on the enrollment patterns of students or the impact on long term educational outcomes. This study focuses on fundamental questions about the proposal. The first question addressed is what is the effect of community

[1] I will refer to two-year schools as community colleges throughout this paper, though in principle two-year colleges can include technical schools as well as community colleges. These statistics are calculated by the American Association of Community Colleges using the 2012 NPSAS.

college price on enrollment patterns of students? The second is what is the effect of decreased tuition on the long term educational outcomes of students?

Community colleges are a large part of the United States higher education system, but very little is known about the price sensitivity of community college enrollment and long-term educational consequences of community college attendance.[2] This paper attempts to fill this void by exploring the effect of price on community college attendance using a novel identification strategy that exploits plausibly exogenous variation in community college tuition. It further explores the effects of community college attendance on educational attainment.

Community colleges differ from four-year universities in many ways. Unlike many universities, community colleges are open-enrollment which means they are open to any student who has a high school diploma or GED credential.[3] Community colleges students are more likely than four-year university students to be from backgrounds with historically lower educational attainment such as racial minorities and low-income families and are also more likely to be the first generation of college students in their family Nunez and Carrol (1998); Bailey et al. (2005). Consequently, understanding community colleges may lend new insights into understanding socioeconomic gaps in educational attainment and income.

Community colleges also stand in contrast to many other college options in that they are substantially less costly to attend. In 2010-2011, average annual

[2] Kane and Rouse (1999) provide a summary of community colleges, their history and impacts.

[3] Community colleges often offer remedial courses that enable students without a high school diploma or GED to eventually enroll in community college

community college tuition was $2,439 while average tuition at public four-year institutions was $7,136, with private four-year institutions being even more costly at $22,771. After adjusting for inflation, public four-year college tuition has risen 241 percent since 1981 while community college tuition has risen at a slower pace of 159 percent. National Center for Education Statistics (2014) Community colleges may become more attractive as four-year college costs continue to rise faster than community college costs. In fact, the net price of community college (accounting for financial aid) actually decreased from 2000 to 2009 while four-year net college price increased over the same period Gillen et al. (2011).

Estimating the effect of community college price on enrollment has been difficult for at least three reasons. The first is measurement; in most settings, the cost of community college is not observed by the researcher because tuition is paid only by students who enrolled in college. For students who do not attend community college it is not clear which price was the relevant price for their decision. I overcome this challenge by using a feature of Texas community colleges where students receive a tuition discount if they attend the local community college. This feature makes the local community college's price the relevant tuition for most students.[4] The second is identification: even in settings where the relevant community college tuition is known for each student, community college tuition may be set in ways that reflect unobserved characteristics about the community college's base of potential students. I overcome the challenge in identification by leverag-

[4]This feature also gives a rule for assigning community college price even for students who did not attend community college.

ing changes in students' eligibility for community college tuition discounts across time and geography. The third difficulty associated with estimation is the stringent data requirements–one needs data that links enrollment and tuition. I am able to use administrative records on all public high school graduates in Texas and their college enrollment.

I leverage the expansion of discounts for tuition in a differences in differences framework to examine the effect of reduced community college tuition on college enrollment. I find that a $1,000 decrease in community college tuition increases immediate enrollment in community colleges by 5.1 percentage points (pp) relative to a baseline of 26.5 pp, and also increases attendance at community college in the year after high school by 7.1 pp relative to a base of 38.4 pp.

Moreover, estimating the effect of community college on long-run educational outcomes is difficult because different types of students choose to enroll in community college (versus no college or a four-year university), and simple OLS estimates will be biased. The long-run effects of community college can be studied by finding a situation where community college enrollment is altered by a factor unrelated to unobserved student characteristics. I examine exactly such a situation using the variation in community college enrollment induced by expansions of community college tuition discounts. I find that community college attendance increases both two-year and bachelor's degree receipt. The increase in educational attainment is apparent for students who switch enrollment from universities to community colleges as well as for students who are induced to attend community college who would not have attended any college otherwise.

The paper unfolds as follows. Section 1.2 discusses the conceptual framework for enrollment responses to community college costs and the long term effects of community college enrollment. Section 1.3 describes the institutional setting explored in this paper. Section 1.4 describes the data. Section 1.5 discusses the identification strategy and results for the effect of community college price on enrollment. Section 1.6 discusses the identification strategy used to examine the longer run effects of community college as well as the estimated effects of community college on longer run outcomes. Section 1.7 discusses how the effects estimated differ by race, gender, and income. Finally, Section 1.8 concludes

1.2 Conceptual Framework

1.2.1 Costs of College

Economic theory predicts that lowering the costs of college will increase college enrollment. This common sense prediction is verified in prior work that generally finds a $1,000 decrease in college costs leads to a 2-4 pp increase in enrollment. Dynarski (2000, 2003, 2004); Scott-Clayton (2011); Castleman and Long (2013); Seftor and Turner (2002); Turner (2011).[5] However, these studies do not generally distinguish between two-year and four-year college costs because they study grants that apply to both community colleges and universities. This paper expands the work on price sensitivity of college enrollment by specifically examining the effects of community college costs on community college and university enrollment.

[5] Deming and Dynarski (2009) summarize this literature.

One might expect larger effects for changes in community college tuition than for increases in financial aid primarily used at universities for several reasons.[6] On average, community colleges serve a lower-income population that may be more price sensitive. Also, a $1,000 reduction in tuition in costs represents a substantially higher fraction of total costs at community colleges than at universities so students may have a stronger response to the same dollar amount reduction in community college costs as compared to universities. Lastly, studies using cross-state variation have found larger effects for community college price sensitivity than for universities (Kane, 1995; Rouse, 1994).[7] However, these studies should be interpreted with caution as they may capture other factors like changing policy objectives of states rather than changes in community college enrollment caused by changes in community college costs. This work expands the large literature on the price sensitivity of college enrollment by providing compelling evidence on the effect of community college prices on enrollment.

In concurrent work, Martorell et al. (2014) examine the effect of community college prices on college enrollment in Texas by leveraging variation in community college tuition induced by taxing districts. They conclude that living in community college taxing districts increases college attendance. While they use similar insti-

[6]Other costs of college have been shown to be relevant for community college enrollment including distance Jepsen and Montgomery (2009); Miller (2007) and weak labor markets Betts and McFarland (1995).

[7]However, Hilmer (1997) finds that the price elasticity for community colleges is lower than it is for universities. Nutting (2008) also examines the enrollment elasticity of community college enrollment using cross-campus, cross-year variation in community colleges in New York and finds that there is a negative relationship between community college enrollment and price. However, the estimates are not easily interpretable as rates of community college attendance.

tutional features for identification, the identifying assumptions are quite different than those used in this paper. They compare students who live on opposite sides of district boundaries who face different community college costs and argue that the students are otherwise equivalent. Martorell et al. (2014) builds on McFarlin (2007) which uses a similar strategy and administrative data in Texas. A key concern is whether students who live on opposite sides of the boundaries sort based on educational amenities. Kane et al. (2006) explores student sorting and finds that sorting across school district boundaries does occur. My paper uses variation induced by changes in these boundaries over time, thereby comparing individuals who live in the same K-12 school districts.

Moreover, the setting described in this paper allows me to identify both the own price enrollment elasticity of community college and the cross price elasticity for four-year enrollment due to precise measurement of community college tuition. Prior studies have largely focused on the effect of a $1,000 change in tuition. However, the interpretation of this parameter across time and different college settings is difficult as the value of $1,000 changes and represents a different fraction of total price. Estimating an elasticity allows a comparison across time and different settings because it is unitless.

There is also a related literature that examines the changes in enrollment patterns that occur when the costs of one sector of post secondary education are decreased and the costs of other sectors are held constant. Prior work has focused on subsidies for in-state colleges, and the present study expands that literature by focusing on a different sector–community college. Cornwell et al. (2006); Goodman

(2008); Cohodes and Goodman (2014) find that students were less likely to attend out of state colleges when scholarships that reduced the cost of attending in state were implemented. Cohodes and Goodman (2014) also document that the change in student enrollment patterns reduced graduation rates. Similarly, I examine the long term effects of a change in the relative price of community college on educational outcomes like graduation and credits attempted similar to Cohodes and Goodman (2014).

It is not clear which students will respond to decreases in the price of community college. Students who enroll in community college due to decreased costs could come from two groups: students who were planning on attending four-year universities or students who were not going to enroll in college. Knowing who responds to community college price changes is important for policymakers considering the effects of community college tuition. Existing work has not explicitly considered who is attracted to community colleges when community college price changes, and this study will be able to answer this question.

1.2.2 Educational Attainment

Increased access to community colleges has a theoretically ambiguous effect on ultimate educational attainment.[8] As articulated by Rouse (1995), there are two competing forces that affect educational attainment when there is increased access to community college: democratization and diversion. Democratization

[8]In this paper, increased access to community college will be caused by decreased community college tuition.

occurs when students switch from no college enrollment to enrollment in community college which would have positive effect on overall educational attainment. However, the diversion effect occurs when increasing access to community college diverts students from four-year universities to two-year colleges. Diversion could reduce overall educational attainment if students who switch do not go on to get a bachelor's degree. This paper will provide quasi-experimental evidence of which effect dominates.

Separating the democratization effect from the diversion effect is difficult because selection into community college is nonrandom. This study overcomes this challenge and presents quasi-experimental evidence on the effect of community college attendance on educational attainment by using variation in community college attendance caused by expansions of community college discounts over time and geography. This variation over time and geography provides "as if random" variation in community college attendance. Goodman et al. (2014) is relevant to this study as they examine SAT cutoffs for admissions to four-year universities. Failure to meet these cutoffs make students more likely to attend community college or not enroll in college. They find that switching from non-enrollment or community college attendance to university attendance increases bachelor's degree receipt, suggesting that the diversion effect dominates.[9] Moreover, McFarlin

[9]Other work has primarily used two approaches to address nonrandom selection into community college. The first is distance to college instruments Rouse (1995); Long and Kurlaender (2009) where the validity of the assumption of the exclusion restriction has been shown to be tenuous Card (2001). The second is and propensity score matching that controls for desired schooling levels and assumes that sorting into community college is random after controlling carefully for schooling intentions and other observable characteristics Reynolds (2012); Doyle (2009); Leigh and Gill

(2007) finds that initially attending community colleges decreases bachelor's degree attainment in the first five years after high school by comparing students in community college taxing districts to students not in community college taxing districts.

Additionally, Brand et al. (2014) makes it clear that choosing the comparison group is critical when examining the long-term effects of community college. In this paper, I will separately examine the long term effects of community college for students who would have attended a university but were induced to switch to community college as well as students who would not have attended any college and were induced to switch to community college.

1.3 Texas Community College System

Community colleges typically provide both academic and vocational training whereas universities focus on academic subjects. Academic training at community colleges is designed to award associates degrees and help students transition to a four-year university. Technical training typically takes the form of a certificate program and offers vocational skills.

Texas provides an ideal laboratory to study community college enrollment; there are 50 public community colleges, each serving distinct geographical areas.[10] Specific municipalities pay ad-valorem property taxes to support each commu-

(2003); Brand et al. (2014). The results from these studies are mixed with some studies suggesting democratization and others diversion.

[10] In addition to the 50 public community colleges the Texas State Technical College System and Lamar State University system also provide public, two-year college options.

nity college.[11] Students who live in municipalities that pay property taxes supporting a community college are eligible for reduced tuition at that college called "in-district" tuition; I will use this policy for identification.[12] The boundaries of community college taxing districts where students are eligible for in-district tuition is shown in Figure 1.1. For the 2014-2015 school year, community colleges in Texas will charge 63 percent more, on average, to out-of-district students relative to in-district students. This paper leverages over 20 expansions in taxing boundaries that have occurred since 1995 that induced large changes in tuition. The timing of these expansions is outlined in Table 1.1.

Importantly for my identification strategy five community colleges in Texas have expanded their taxing district through annexation of municipalities. The first annexation contained in the data occurred in 1995 and, in total, 22 municipalities joined a community college district. These expansions have increased the number of students eligible for reduced, "in-district" tuition.[13] The colleges that have expanded and are the focus of my study are Austin Community College, Lone Star College, Amarillo College, Houston Community College, and Hill College.[14] Table

[11]This in-district feature of community college tuition pricing is present in a few other states namely Arizona, Arkansas, Illinois, Maryland, Michigan, Missouri, Montana, New Jersey, New Mexico, Pennsylvania, and South Carolina. These states do not necessarily have this feature at all community colleges in the state but do at at least some community colleges. In the 2012-2013 school year nearly 70 percent of community college students in Texas were paying in-district tuition.

[12]The exception is El Paso Community which does not offer a discount to students who live in the taxing district.

[13]There has been one additional annexation at Brazosport College after the time covered by the data. Also there was an additional annexation for Austin Community College of the City of Austin in 2005, but this annexation does not map into a school district as it annexed only parts of school districts and is excluded for this reason.

[14]Lone Star College was known as North Harris Montgomery Community College District prior

1.1 lists the expansions and Figure 1.2 shows the districts annexed. These colleges represent a range of sizes and geographies with Hill College being in a rural setting and having just over 4,000 students enrolled in Fall 2013 and Lone Star College in Houston having over 61,000 students enrolled in the same year. It is the variation in community college price induced by annexations of municipalities that I will use for my identification.

In order for a tax entity to be added to the taxing district for a community college, the residents must gather signatures for a petition to vote on annexation into the community college taxing district. After a petition has a sufficient number of signatures, a vote authorizing an increase in property taxes is taken. The increase in property taxes is on the order of $.10 per $100 of property value, although it varies by college. Community colleges use the property tax revenue from their taxing district as well as other sources of revenue including state appropriations, and tuition and fees to fund their operations. As soon as a municipality approves the property tax, students begin paying in-district tuition as opposed to out-of-district tuition. The assumptions required to use these annexations as variation in community college tuition will be discussed further in Section 1.5.

Many times the vote for annexation also includes plans for new facilities being built in the annexed area. Table 1.1 contains a list of relevant campus building projects and building open dates. Additional campuses reduce the costs of attending community college and may influence both non-monetary costs like conve-

to 2007.

nience and monetary costs.[15] I will control for the presence of new campuses to isolate the change in tuition associated with annexation.

1.4 Data

The data for this project come from several sources. The primary student-level data come from the Texas Education Research Center (ERC) and cover the school years that start from 1994-2012 although the primary estimating sample will focus on 1994-2005.[16] These data contain demographic and academic performance information for all students in public K-12 schools in Texas provided by the Texas Education Agency. These records are linked to individual level enrollment, graduation, and financial aid data from all public institutions of higher education in the state of Texas as well as many private institutions using data provided by the Texas Higher Education Coordinating Board. Data on tuition comes from the Texas Association of Community Colleges and contains tuition information starting in 1992. Data on tuition is on the sticker price of attendance rather than on tuition actually paid by students. However, sticker price is particularly relevant in the community college setting and is very close to what is actually paid by students. Sticker versus actual price will be discussed further in Section 1.5. County level unemployment rates for August of each year from the Bureau of Labor Statistics are also used.

I assembled information on community college districts in Texas by visiting

[15]New campuses are often located relatively close to existing campuses and as such are unlikely to affect the decision to live at home if attending community college.

[16]For a description of these data see http://www.utaustinerc.org/

each community college's website and through conversations with administrators in cases of ambiguity.[17] Historical information for each school district's annexation history was obtained several ways. For a detailed description of determining annexation dates see Appendix A.1.1.

1.4.1 Measuring Tuition Status

Eligibility for in-district tuition depends on the taxing district of a student's residence. The ERC data do not contain precise address information or taxing district information, so in-district status for the purposes of this paper is inferred by the in-district status of a student's high school. In all instances in this study, the boundaries for community college taxing districts are defined by school districts which means eligibility is observed with smaller error than when using other geographic boundaries. However, there are several reasons for measurement error in taxing district residence including attending a high school for which the student does not live in the boundary and students who move the year after high school.

For students who attend community college, the data contain whether they paid in-district or out-of-district tuition. Panel B of Figure 1.3 shows that eligibility for in-district tuition increases sharply in the year of annexation. This figure is created using students who graduated from K-12 school districts that would experience annexation and plots the fraction who paid in-district tuition while attending community college. This figure should be interpreted with caution as annexation

[17] The information compiled from school websites for the district of each school is available upon request.

will be shown to cause students to enroll in community college, but it is useful for illustrating the discrete change in payment of in-district tuition. Ideally, the data would reveal the change in the fraction of students *eligible* for in-district tuition. However, only the change in students actually *paying* in-district tuition can be measured. In the period after annexation, some students will have their in-district status changed and other students will not. The new attendees are likely to be students who did experience a change in tuition status because those students face lower tuition costs. For this reason, the plotted or estimated change in in-district tuition payment is likely to increase more than the change in the eligibility for in-district tuition.

Prior to annexation around 15 percent of students are paying in-district tuition; after annexation the number is approximately 80 percent. In the first year of annexation there appears to be some slippage, with approximately 60 percent of annexed students paying in-district tuition. This could be explained by administrative issues in the implementation of annexation. In the data for individual K-12 districts, the first year of annexation often has a smaller fraction paying in-district tuition than subsequent years which suggests that the slippage is not due to measurement error in the annexation date. Figure 1.3 demonstrates that the annexations did affect the price paid by students for community college.

When interpreting the effects of a $1,000 change in tuition it is important to remember that tuition is assigned to change for *all* students who attended a K-12 district that was annexed. However, Table 1.4 show that among students who enrolled in community college, 55 percent of students changed from out-of-district to in-district. As previously discussed, the 55 percent estimate is likely to be an overestimate because students who are eligible for in-district tuition are more likely to attend community college and thus appear in the data than students who are not eligible for in-district tuition. To further reduce the measurement error in tuition, estimates that measure the effect of a $1,000 tuition change should be scaled up by dividing by .55 (or multiplying by 1.8). Because .55 is likely to be an overestimate of the true change in in-district eligibility, dividing by .55 will not scale up the results as much as if the coefficients were divided by the true, smaller estimate. As such, dividing by .55 is likely to be a lower bound on the effect of a $1,000 change in tuition. For this reason, results that are scaled by tuition will also be scaled by the change in in-district eligibility.

Another important consideration for interpretation is how annexation affects the net price of college. To this point, I have focused on changes in tuition but annexation could also affect grants and influence net price through changes in grant aid.[18] If decreases in tuition are offset by decreases in grant payments, then the magnitude of the change in tuition will overstate the actual change in the costs of college.

I investigate this by examining the patterns of grants received. Only students who enroll in community college are observed, and prior results show that annexation is related to additional students enrolling in community college. Be-

[18]Grants will be defined as the annual amount of Federal Pell Grants, Federal Supplemental Educational Opportunity Grants, TEXAS Grants, and Texas Public Education Grants. All of these grants are need-based but are funded by different sources. TEXAS Grants are funded by the state and Texas Public Education Grants are funded by individual colleges.

cause annexation affects enrollment, and thus the sample used in estimation, the result on grants should be viewed as descriptive rather than causal. Data on grants disbursed starts in 2001 and so results presented will be from 2001 to 2012. Column 3 of Table 1.4 examines the effect of annexation on grant aid received at community colleges and finds a statistically imprecise decrease in grant aid received of $173. When considering only students who received some grants at a community college in Column 4 of Table 1.4, the average amount of grants received went down after annexation by $286. Even after accounting for imperfect measurement of eligibility this represents roughly half of the change in tuition. However, the number of students receiving grants at community colleges during this time period is relatively small with 15-20 percent receiving non zero grants.[19] This suggests that there may be small countervailing effect of reduced grants, but this only affects a minority of high school graduates. The evidence on changes in grants suggests that the results may be biased downwards.

1.4.2 Constructing the Sample

The sample used for analysis consists of students who graduated from Texas public high schools when 17 or 18 years old between 1994 and 2005. I will first examine the immediate transition of these students to college. Studying on-time graduates of high school and their enrollment behavior in the fall after their graduation has the advantage that on-time high school graduates were unable to manip-

[19]This is likely due to issues explored in the literature on FAFSA take up and financial aid complexity Bettinger et al. (2012); Dynarski and Wiederspan (2012).

ulate the timing of their entry into college as a result of changing tax jurisdictions. This is because the annexation vote takes place during their senior year. Students who were out of high school for some time may wait to enroll in college until after a vote is taken. However, examining recent high school graduates will only capture part of the total effect of annexation and lower tuition on community college enrollment. For instance, lower tuition is also likely to attract other students to "go back" to school.

Because the sample is selected from high school graduates the estimates may be biased if annexation changes the probability of graduation from high school. This might happen if students see the opportunity for less costly post secondary schooling and change their effort. This is tested in Panel B of Table 2.2 which shows that students do not change high school graduation behavior in response to less expensive community college tuition.

For the majority of the analysis, the sample is limited to students who graduated from high school from 1994-2005. This allows an examination of graduation outcomes like bachelor's degree receipt eight years after high school. I also use students from 1994-2012 for enrollment outcomes to take advantage of additional annexations that occur from 2006-2012, and these results are discussed in Appendix A.1.2. The sample is limited to students from K-12 school districts that are part of a community college taxing district that experienced annexation from 1994 to 2005. As a result, all K-12 districts in the sample will be part of a community college taxing district by 2005. This restriction causes the sample to consist of approximately

15 percent of high school graduates in Texas during this time period.[20]

Table 3.1 contains summary statistics for the primary estimating sample which includes high school graduates from 1995 to 2006. K-12 districts that experienced annexation makes up 39 percent of the observations and post-annexation observations account for 25 percent of the observations. Over 26 percent of students attend community college immediately after high school graduation, and 24.7 percent attend public universities. Table 1.3 splits the data for the districts that experienced annexation before and after the annexation. After annexation there are increases in community college enrollment, in-district community college enrollment, payment of in-district tuition, graduation probability, and credit hours at community colleges and universities. Tuition drops from $1962 annually to $1160. These preview the results, but the patterns described here generally hold upon more precise statistical examination.

1.5 Community College Price Sensitivity

1.5.1 Identification

The first goal of this paper is to uncover the effect of community college tuition on enrollment patterns. This is difficult for a number of reasons. First, in many settings it is not clear which community college tuition is relevant for students making enrollment decisions. Second, even in settings where the relevant community college tuition is easy to assign, finding variation in costs of

[20]For analysis that includes years up to 2012 the sample is expanded to include a new community college taxing district that experienced annexation, Houston Community College.

community college unrelated to underlying student characteristics is difficult. For instance, cross sectional differences in community college tuition are likely to represent unobserved differences in the areas that support the community colleges. Temporal variation in community college price may arise from business cycle fluctuations or secular trends in college costs.

To address these issues, I exploit previously described institutional features of the Texas community college system. For the assignment of community college tuition I leverage the fact that Texas students face differential tuition depending on their residence. The system of in-district tuition creates a rule that assigns the relevant community college tuition. Namely, prior to a K-12 district's annexation the price of community college is the out-of-district price and after annexation, it is the in-district price. I also overcome the challenge of tuition being set in response to student characteristics by exploiting sharp changes in tuition within K-12 school districts over time by using taxing district annexation (which represents a substantial shock to the cost of community college for students).

To identify the causal impacts of tuition on enrollment, I implement a differences in differences estimator by comparing enrollment of annexed districts to districts already in a taxing district before and after annexation takes place. The language of a quasi-experiment will be employed with annexed K-12 districts being referred to as the treatment group and districts already included in the community college taxing districts being referred to as the control group.[21] Because the

[21] The control K-12 districts are already included in the taxing district of the college. These districts are likely to be most similar to annexed districts because they are in the same locality and they

variation in tuition occurs at the K-12 district/year level I cluster standard errors at the district level.[22] To examine the effect of annexation or treatment the following reduced form equation is estimated:

$$Y_{icdt} = \theta \cdot Annexation_{dt} + X_{idt}\alpha + W_{tc}\beta + \gamma_d + \eta_t + \tau_{tc} + \epsilon_{icdt} \tag{1.1}$$

Importantly, i indexes individuals, d indexes K-12 districts, t indexes school year, c indexes community college district, and ϵ_{icdt} represents an idiosyncratic error term. Y_{icdt} is a student enrollment outcome like attendance at community college and $Annexation_{dt}$ is an indicator for a K-12 district d that has been annexed in year t. As such, θ is parameter of interest and is the effect of annexation and the attendant reduced tuition on a student outcome. Variables that control for K-12 district characteristics that may be related to college-going are included in X_{idt} like race, gender, an indicator for economic disadvantage, and limited English proficiency.[23] X_{idt} also includes an indicator for a new campus of the community college being open in the K-12 district. W_{ct} contains covariates that control for factors affecting college attendance at the community college district level like county unemployment rates and number of high school seniors in the graduating cohort; these are only included in specifications without college/year fixed effects.[24]

have access to community college services. Choosing K-12 districts that were never treated would be problematic because the students are further away from the community college and are less likely to attend the community college under consideration. The control districts were all annexed prior to 1992 or were included initially in the formation of the taxing district.

[22] Performing the analysis on data collapsed into K-12 school district/year cells that are weighted by the number of high school graduates in the cell yields very similar results.

[23] Economic disadvantage is determined by free and reduced lunch receipt.

[24] Bound and Turner (2007) find that large cohort sizes within states lead to low educational attainment, so I control for cohort size explicitly.

In addition to district characteristics, fixed effects for K-12 district, γ_d, and year, η_t, are included. These fixed effects control for fixed observed and unobserved characteristics of K-12 districts. They also control for fixed community college characteristics as K-12 districts comprise the community college taxing district. Year fixed effects account for trends in community college enrollment and for factors common to all community college districts that change with time. In addition to year fixed effects, in some specifications time is also accounted for using community college-specific linear time trends. However, in the preferred specification, community college district-by-time fixed effects, τ_{tc}, are included to account for common trends and shocks that occur to both the treatment and control group in a community college district.

The rich set of controls and fixed effects in Equation 1.1 enable a comparison of enrollment rates *within* K-12 districts *across* cohorts who experienced lower tuition. The K-12 districts who were already part of the taxing district serve as the comparison group. These controls are in place so that θ captures only the effect of taxing district annexation after controlling for K-12 district fixed characteristics, demographic characteristics, time effects, labor market conditions, trends common to all K-12 districts in the community college district, and new campuses.[25]

[25] As an illustrative example of the spirit of the estimator, consider the annexation of Del Valle Independent School District (ISD). Dell Valle ISD was annexed into the Austin Community College taxing district in 2004 and will serve as the "treatment group". After 2004, high school graduates from Del Valle ISD experienced reduced tuition as a result of annexation into the taxing district. Austin ISD was part of the Austin Community College taxing district many years prior to the data and will serve as the "control group" because students in Austin ISD did not experience substantial changes in tuition. I compare the change in enrollment rates for Del Valle ISD before and after 2004 to changes in enrollment rates for Austin ISD before and after 2004. The difference in these differ-

Equation 1.1 captures the effect of annexation and the resulting cheaper tuition on student outcomes. However, this does not scale the effects of annexation by the change in tuition. In order to do this an instrumental variables strategy is used where listed community college tuition is instrumented for using $Annexation_{dt}$ as in the following first stage equation:

$$Tuition_{cdt} = \varsigma \cdot Annexation_{dt} + X_{dt}\phi + W_{ct}\chi + \vartheta_d + \delta_t + \omega_{ct} + \mu_{cdt} \tag{1.2}$$

The second stage equation becomes:

$$Y_{cdt} = \sigma \cdot \widehat{Tuition}_{dt} + X_{dt}\kappa + W_{ct}\rho + \pi_d + \zeta_t + \lambda_{ct} + \upsilon_{cdt} \tag{1.3}$$

$Tuition_{dt}$ is the sticker price of community college tuition and fees for two semesters of 12 credit hours measured in 1,000s of 2012 dollars. Prior to a K-12 district's annexation $Tuition_{dt}$ is the out-of-district price and after annexation, it is the in-district price. The parameter of interest is σ which is the coefficient on in-district tuition and represents the effect of a $1,000 increase in sticker tuition on enrollment outcomes. Several outcomes will be considered as Y_i including indicators for community college enrollment, enrollment in the in-district community college, four-year university enrollment, and no enrollment. This will allow an investigation of not only the own price sensitivity of community college enrollment, but also the cross price sensitivity for four-year college enrollment.

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ences is interpreted as the effect of the reduced tuition resulting from annexation on community college enrollment. The actual estimation performs this type of exercise for many treatment and control districts simultaneously while also controlling for many other factors.

Assumptions for Identification

For the identification strategy used to examine the effect of annexation on enrollment to be valid, I must assume that treatment and control K-12 districts have the same trends in college enrollment prior to treatment.[26] While this seems reasonable given that students in these K-12 districts share many common characteristics like geography, labor markets, etc., I will test this in more detail later in the paper by providing visual evidence.[27]

Another assumption is that there are no other shocks occurring at the same time as annexation that would also affect the decision to enroll. To address this issue I control for potential confounders like demographic characteristics, indicators for new community college campuses in the K-12 district, and use year-by-college fixed effects to capture shocks common to treatment and control groups. While there could still be unaccounted for shocks that occur, the shocks would have to be systematically correlated to annexation across different colleges and districts. It is worth noting that a shock to the entire community college taxing district would be experienced by both the treatment and control groups and would not be an issue except if treatment and control districts reacted to the shock differently.[28]

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[26]Formally the assumption for identification is that $E(\epsilon_{icdt}|Annexation_{dt}, X_{idt}, W_{tc}, \gamma_d, \eta_t, \tau_{tc}) = 0$.

[27]In addition to visual tests presented later in the paper, I test for parallel trends explicitly in each case of annexation. In all but two of the annexation events, the trend for the treatment and control districts are not statistically distinguishable. When excluding these two annexations, the results do not change substantively.

[28]One potential confounder would be a change in the admissions policies of community colleges that coincided with annexation. This is a potential problem in a selective college setting, but because community colleges are open-enrollment this is not an issue. If community colleges changed in quality after annexation this increased quality would affect both the treatment and control dis-

As previously discussed, annexation is always associated with a vote approving the annexation. The assumption is that timing of a vote authorizing annexation is exogenous or unrelated to factors that may affect community college enrollment. The timing of votes cannot be related to the underlying characteristics of students or taxing district which will be tested in Table 2.2.

One way to test that annexation is unrelated to other factors is to examine whether observable characteristics of a district are related to annexation. If student observable characteristics are related to annexation, student unobservable characteristics are likely to be related as well. Table 2.2 presents these results and finds that annexation is unrelated to gender, race, economic disadvantage status, and limited English proficiency indicators. I also consider whether annexation is related to high school graduation by selecting a sample of 10th graders and find no relationship between annexation and the probability of graduating from high school in column 9 of Table 2.2.[29] Lastly, student plans for college are measured and are found to be negatively related to annexation though this result is marginally statistically significant. The implications of no change (or possibly a small negative change) in college plans will be discussed further in the results section. Overall, Table 2.2 presents evidence that student characteristics were not observably differ-

tricts.

[29] I define the annexation variable for these students as cohorts who will experience an annexation in their senior year rather than in their tenth grade year. A special consideration is that students may change their graduation plans in response to annexation. Graduation plans would be difficult for students to change as annexation is announced during a student's senior year, but I can test for this directly. The probability of graduation does not change for cohorts that will be annexed. This means that using the sample of high school graduates does not suffer from the sample selection related to annexation. Interestingly, students are asked whether they plan to attend college and this variable does not change. The implications of this finding will be discussed in the Section 1.5.2.

ent by annexation status. This evidence lends credibility to the assumption that there were no simultaneous changes at the time of annexation.

In order for the estimates of σ in Equation 3.1 to reveal the effect of community college tuition on enrollment several assumptions for instrumental variable estimation need to hold. The first is that annexation is strongly related to tuition. Annexation is a policy that intentionally changes the tuition and so this should be true. Table 1.4 examines the impact of annexation on the sticker price of tuition and finds that annexation reduces tuition by $1124. This reduction is verified visually in Panel A of Figure 1.3 where annexation results in a substantial drop in tuition by approximately 50 percent.

I must also assume that annexation is correlated with community college tuition but is not related to any other factors that would influence enrollment behavior. Ultimately this exclusion restriction is untestable, but controlling for the factors that are most likely to vary at the county/K-12 district level as previously outlined helps alleviate potential problems. One change of particular interest may be the changing of services offered by community colleges which I attempt to capture using indicators for new campuses being built.

The last required assumption for a Local Average Treatment Effect (LATE) interpretation of the instrumental variable estimation is a monotonicity assumption. The LATE interpretation implies that the parameter estimated applies to the group of students who were induced to attend community college by the instrument. The monotonicity assumption means that annexation cannot induce some students to enroll in community college and discourage some students who would have enrolled in community college from enrolling. In this context this assumption seems very reasonable as a story where less costly community college leads to decreased community college enrollment is counter-intuitive.

1.5.2 Enrollment Results

Panel A of Table 1.6 contains the reduced form estimates of the effect of annexation on immediate community college enrollment. Only the preferred specification is presented which includes year, K-12 district fixed effects, demographic characteristics, and college by year fixed effects. Results for other specifications are quantitatively and qualitatively similar and are available upon request.[30] Column 1 shows that annexation is associated with a 3.2 pp increase in community college attendance, which is a 12 percent increase over the sample average. Column 2 in Panel A of Table 1.6 examines the effect of annexation on enrollment at four-year universities. In the preferred specification there is a very small point estimate of -.05 pp that is not statistically significant suggesting no impact of annexation on public, four-year enrollment.

To test whether the local community college's price is the relevant price for community college for most students, I compare the estimated effects of enrollment in any community college in Panel A, column 1 of Table 1.6 to the effects of in-district enrollment found in Panel A, column 3 of Table 1.6.[31] If students

[30]Other specifications that do not include demographics or college/year fixed effects tend to measure effects that are slightly larger in absolute value than the results presented.

[31]For cohorts that were not in district at the time of high school graduation this is defined as the community college into which their K-12 district would eventually be annexed.

could easily switch enrollment between community colleges, annexation might have zero effect on enrollment in community college but a large increase in enrollment in-district. The estimated annexation effect is larger for enrolling in-district at 4.4 pp than for enrolling in any community college which is 3.2 pp. The discrepancy in magnitudes indicates annexation induced some students to switch enrollment in community college from out-of-district to the community college that was closest to home. Ultimately this switching should only bias the estimates of tuition's effect on community college enrollment downward as it is an indication that the local community college's tuition may not be the relevant tuition for a subset of students.

Column 4 in Panel A of Table 1.6 examines the effect of annexation on the decision to not enroll in any public college in the data.[32] High school graduates are 3.1 pp less likely to not attend college as a result of annexation–that is, students were 3.1 pp more likely to attend college with all of the increase occurring at community colleges.

Another important result for interpretation is the combination of the estimated enrollment effects and the lack of effects found on stated college intentions

[32] Students may be switching enrollment from private two-year colleges to public community colleges. Notably, Cellini (2009) finds that additional funding for public community colleges induces students to switch from proprietary schools to public community colleges. Unfortunately, data on for private two-year colleges has only recently been collected by the THECB. However, the THECB estimated that students at private two-year colleges represented just 3 percent of state college enrollment in 1999 as compared to public community colleges which represented 44 percent Texas Higher Education Coordinating Board (2001). In fact, if *all* students switched from private two-year colleges to community colleges that would only account for approximately 60 percent of the measured effect. For this reason, switching from private two-year colleges is likely to be at most a small part of the story.

in Column 9 of Table 2.2. This suggests that several students had planned on going to college who would not have enrolled except for changes in community college costs. Lowering tuition costs did not affect college plans but allowed students who had a stated interest in college attendance to enroll. This result builds on a growing body of work that suggests interventions in a student's high school career can affect student enrollment behavior Castleman and Page (2013).

Taken together, these results indicate that annexation and the reduced tuition associated with annexation resulted in students attending community college at higher rates. It also appears that lower tuition induces students who would not have attended any college to attend community college and that cheaper tuition did not induce students to switch from public four-year colleges to community colleges.

To scale the results by the changes in sticker tuition, Equation 3.1 is estimated and results are presented in Panel B of Table 1.6, where the effect of community college tuition is in $1,000s of dollars. A $1,000 increase in the annual *sticker price* of tuition decreases community college attendance by 2.8 pp. It also decreases enrollment in-district by 3.8 pp and increases the fraction of students enrolling in no college by 2.8 pp. As there are not large changes in financial aid, the change in sticker price is likely to reflect the true tuition bill for students who experienced annexation. However, sticker price is measured with error which needs to be corrected.

As previously discussed, the results should be scaled by the change in the fraction of students eligible for in-district tuition which was measured as .55. Using this information a decrease of $1,000 in tuition per semester would lead to an increase in immediate community college enrollment for high school graduates of 5.1 pp. This is slightly higher than estimates of the effect of financial aid on college attendance. There are at least two possible reasons for a slightly higher estimate. The first is that the actual change in the costs of college is observed relatively well in this study, so appropriate adjustments can be made for measurement error. The second reason is that students on the margin of attending community college may be more price sensitive than the entire population of potential college goers.

The estimates thus far have been in terms of the enrollment rate to aid comparability with prior estimates in the literature. An alternate approach is to estimate equation 3.1 but to use the natural logarithm of Y_{cdt} and $Tuition_{dt}$.[33] This specification yields estimates of the elasticity of enrollment with respect to community college tuition. An elasticity has the benefit of being unitless and allows comparisons across time and context. Panel C of Table 1.6 contains these elasticity estimates. Column 1 indicates that a 10 percent increase in community college tuition would lead to a 1.6 percent decrease in community college enrollment, or 2.9 percent if scaled by the change in in-district tuition payment. Column 2 confirms that an increase in tuition does not affect enrollment at public four-year universities. Column 3 indicates that the elasticity is higher for in-district enrollment as previously discussed. Finally, Column 4 indicates that a 10 percent increase in community college tuition increases the probability that a student is not attending any college by .98 percent, or 1.8 percent when accounting for payment of in-district

[33]When using collapsed data, the cells are weighted by the number of high school graduates.

tuition.

Overall, these results indicate that students respond to a \$1,000 decrease in community college tuition by increasing immediate community college attendance by 5.1 pp, or a 20 percent increase over the baseline. Students do not appear to switch their enrollment from universities to enroll in community college but instead switch from not enrolling in college to enrolling in community college. This finding provides evidence that access to community college in the form of cheaper tuition has a democratizing effect but no diversion effect.

Effects by Cohort Relative to Annexation

To examine the timing of these effects a model is estimated with indicator variables for cohorts relative to annexation instead of a single annexation indicator in an event study framework.[34] This gives a sense of when enrollment patterns changed and if pre-existing trends are driving the results. The coefficients are plotted in Figure 1.4 along with 95 percent confidence intervals; the omitted category is for the cohort one year prior to annexation. Prior to annexation, treatment and control groups appear to have similar trends in community college enrollment as can be seen by a flat difference in years prior to annexation. Also, in four of the five years prior to annexation, the 95 percent confidence interval contains zero which means that in those years, the difference between treatment and control groups cannot be distinguished from what it was in the year before annexation. If there

[34]Cohorts beyond five years after annexation are combined into one indicator for five years or greater. Cohorts six years or greater before annexation are similarly combined.

were differential trends the levels of the plotted coefficients would exhibit a trend. Five years before annexation there appears to be a one time deviation from a flat trend, but in the four years leading up to annexation there does not appear to be any trend.

There is a jump in the probability of attending community college in the year of annexation, and by the second cohort after annexation treated districts are statistically significantly more likely to attend community college attendance relative to the control districts. The effects are largest after three years and seem to stabilize in years 3-5 after annexation.[35] A similar exercise for enrollment in university is performed in Panel B of Figure 1.4 for enrollment in university though there does not appear to be any change in university enrollment.

Placebo

To provide an alternate measure of the probability of these estimates arising from chance, I conduct a placebo exercise. Using data from community college enrollments in 1996 I predicted whether a college ever expanded its taxing district using the fraction of male students, fraction of Hispanic students, fraction of students in technical programs, and the log number of students. The four colleges that had the highest likelihood of annexation and as such make up the "placebo data" were Dallas Community College, Tarrant County College, Tyler Junior College, and Collin County Community College. These four colleges were mapped to

[35]The gradual increase in the estimated effects of annexation could happen for a few reasons, but one potential explanation that is consistent is a salience story where students may not be entirely aware of the change in community college price but as time passes information is diffused.

the four colleges that did experience annexations prior to 2006.[36] Within matched colleges, each K-12 district in the placebo data was randomly assigned to a K-12 district in the actual data and was given the annexation dates (if any) of the district in the actual data. This assignment rule ensures the same number of treated K-12 districts and timing of simulated annexations as were contained in the original data.[37] Then the reduced form regression of the effect of annexation on community college enrollment was performed and the results were stored. This process was repeated 500 times and the results are visually summarized in Figure 1.5. The vertical line shows the coefficient estimated in the actual data and the distribution of the estimates.

In the case of enrollment in community college, there were no placebo regressions in which a larger effect was estimated. This presents strong evidence that annexation and the attendant decreases in tuition did increase community college enrollment. In contrast, the estimated effect of annexation on enrollment in a four-year college was in the 46th percentile of estimates of the placebo exercise. The estimate of the effect of annexation on enrollment at a four-year college from Table 1.6 was statistically insignificant, and the placebo exercise confirms that the enrollment in universities was not affected.

[36]This was done to make sure that the matched college had a greater or equal number of school districts that were in the taxing district as the college that actually experienced the expansion. Inherently this matched schools of roughly similar sizes. Dallas was matched with Lone Star College, Tarrant County College with Austin Community College, Tyler Junior College with Amarillo College, and Collin County Community College with Hill College.

[37]There are more control K-12 districts in the placebo data than in the original data because the four placebo community college districts had more K-12 districts than their actually-treated counterparts.

Longer Term Enrollment

To this point immediate enrollment in the fall after high school has been the focus of the estimation. However, enrollment patterns beyond the fall following high school graduation are interesting as well. When examining year two after high school, some students who did not experience reduced community college tuition directly after high school graduation had exposure to lower community college tuition two years after high school graduation. The more years pass after high school, the greater the portion of the control group that has some level of treatment increases so effects in the later years should be attenuated.

Panel A of Table 1.7 examines community college enrollment in the years after high school. The dependent variable is a binary indicator with unity if the student enrolled in community college in the 1st, 2nd, etc. calendar year after their high school graduation. In all years students are more likely to be enrolled in community college with the largest estimates being in the years directly after high school. High school graduates are more likely to respond to annexation in the entire first year as compared to just fall enrollment immediately following graduation. The estimated effect of annexation on enrollment in the calendar year after highs school is 4.5 pp as opposed to 3.2 pp when considering fall only. This translates into a 7.1 pp increase for a $1,000 decrease in community college tuition when dividing by .55. The magnitude gets smaller over time but is fairly constant at around a 10 percent increase over the baseline attendance rate in that year. Taken together these results indicate that reduced tuition induces high school graduates to attend community college immediately and continues to affect enrollment for several years after high school. The effects past the first year can come through either increased persistence in college or increased first time enrollment at older ages. Further consideration of longer term attendance is considered in A.1.3 which examines credit hours attempted.

Panel B of Table 1.7 performs a similar exercise considering enrollment at a public university in each year since high school. In the first three years after high school graduation, students do not appear to be more likely to attend a four-year university if they experience an annexation. However, starting in year four after high school, the coefficients increase in magnitude and in year six after high school the increase is statistically significant. Table 1.8 further explores this result by examining transfer from community colleges to universities. For each year after high school graduation I define transfer as if a student is enrolled in a university in the current year and had been enrolled in a community college in a prior year. In years three to six after high school, students are more likely to be at universities with prior attendance at community colleges. These results suggest that reduced tuition for community colleges induces students to initially enroll in community colleges and eventually attend four-year universities after attending community colleges.

The evidence on enrollment suggests that reduced community college tuition has a democratization effect and no diversion effect. Reduced community college tuition induced students who would not have attended college of any type to enroll in community colleges. This is compelling, quasi-experimental evidence on the effect of community college access on enrollment, and the results suggest that reduced community college tuition increases college attendance but does not reduce university enrollment.

1.6 Educational Effects of Community College

1.6.1 Identification

Knowing the relationship between community college access and long term educational outcomes is difficult because students who attend community college are likely to be unobservably different from students who do not. In order to overcome this challenge, a source of variation is needed that influences community college attendance but does not directly influence long term outcomes. For the second part of my analysis, I use community college taxing district annexations as an instrument for community college attendance to identify the effects of community college attendance on educational attainment. Annexation has been shown to strongly influence community college attendance and induces students to attend community college who would not have attended college otherwise.

For this analysis, I am estimating the following first stage equation using high school graduates from 1994-2005. The familiar indicator for annexation, $Annexation_{dt}$ is an instrument for attendance at a community college in the first year after high school $AttendCC_{dt}$:

$$AttendCC_{dt} = \varsigma \cdot Annexation_{dt} + X_{dt}\phi + W_{ct}\chi + \vartheta_d + \delta_t + \omega_{ct} + \mu_{cdt} \tag{1.4}$$

The second stage equation becomes:

$$Y_{cdt} = \aleph \cdot \widehat{AttendCC}_{dt} + X_{dt}\kappa + W_{ct}\rho + \pi_d + \zeta_t + \lambda_{ct} + \upsilon_{cdt} \tag{1.5}$$

Y_{cdt} is an education outcome like graduation from a four-year college. The indices are the same as prior estimating equations with c indexing community college taxing district, d indexing K-12 school district, and t indexing time. As before, these specifications include year fixed effects, K-12 district fixed effects, and community college district by time fixed effects as well as controls for demographic characteristics.

For this instrumental variables strategy to be valid there are several assumptions that need to be made. First, the instrument must be strongly correlated with attending community college. Section 1.5 established that annexation is strongly correlated with community college attendance. Second, the instrument must not be correlated with longer term outcomes like bachelor's degree receipt except through community college attendance.[38]

1.6.2 Educational Attainment Results

Panel A of Table 1.9 explores the effect of annexation on graduation probabilities from community college as well as universities. Column 1 of Panel A considers graduation from a community college with a degree or certificate and does not find any effect of annexation on degree or certificate receipt. Column 2 of

[38] A potential violation of this assumption is if cheaper community college tuition affects students who would have attended community college anyway by giving them access to reduced tuition. In order to test this an indicator for the cohort prior to annexation is included. These students would have access to cheaper community college tuition in all but the first year of attendance. This indicator is statistically insignificant and very small suggesting that access to cheaper community college for students who would have attended community college in the presence of higher tuition did not affect graduation probabilities. This result supports the assumption of the exclusion restriction. The full results from this exercise are available upon request.

Panel A considers graduation with a community college credential or degree after 4 years and finds no effect. Likewise, annexation is not associated with increases of bachelors' degree receipt in 4 or 6 years after high school graduation. However, for 8 years after high school graduation, there is an increase of 1.1 pp with a p-value of .11 providing marginally statistically significant evidence that annexation increased bachelor's degree receipt after eight years.

Panels C and D of Figure 1.4 consider graduation outcomes by cohort relative to annexation. In both instances graduation appears to have increased slightly in the years after annexation but not dramatically so. This confirms the results in Panel A of Table 1.9 which measured statistically imprecise increases in graduation as a result of annexation. The previously described placebo exercise is also performed and summarized in Panel C and D of Figure 1.5. The estimate for graduation from community college in 4 years is in the 10th percentile of estimates from the placebo exercise, and the estimate for bachelor's degree receipt in 8 years is in the 13th percentile. This mirrors the prior finding that attending community college appears to increase educational attainment.

To consider the effect of attending a community college on ultimate degree receipt Equation 1.5 is estimated. The results are very similar to what has been discussed previously but scales the results by the fraction of students who attended a community college in the first year after high school graduation. The results from this instrumental variables estimation are in Panel B of Table 1.9 which indicates that attending community college increases the probability of graduation with a four-year degree eight years after high school by 23 pp. This result is marginally statistically significant at the 10 percent level. This suggests that students induced to attend community college as a result of annexation are more likely to graduate with a four-year degree. These students would not have attended college otherwise, and so the decreased tuition provided a viable pathway toward bachelor's degree receipt.

1.7 Heterogeneity

This section examines the heterogeneous effects of reduced community college tuition on enrollment in addition to the heterogeneous effects of community college attendance on educational attainment by race, gender, and economic disadvantage status. Table 1.10 contains estimates for the enrollment effects as well as the reduced-form effects for educational attainment. In these analyses, I employ a fully interacted model where indicators for race, gender, or economic disadvantage status are interacted with every variable in Equation 1.1.

I will only discuss the results that have statistically different results by gender, economic disadvantage, or race while all others are statistically indistinguishable. For immediate enrollment in community college, African American students respond more strongly to annexation than white students. African American students also respond to annexation by diverting enrollment from universities to community college.

The measured diversion effect for African American students stands in contrast to the results for the whole sample where there was no switching from universities to community colleges. Interestingly, white students are more likely to receive a bachelor's degree in eight years and African American students are are not statistically any different in their bachelor's degree receipt despite being initially diverted from universities–in fact, the point estimate is positive. This suggests that for these racial groups that community colleges have a democratization effect, even for students induced to attend community college who would have attended universities.

Exploring the heterogeneous effects suggests that minority students are particularly price sensitive in their community college enrollment decision. Additionally, the results present another piece of evidence that community colleges increase overall educational attainment because students induced to attend community college at higher rates due to lowered tuition have higher probability of bachelor's degree receipt. These results also suggest that reduced community college tuition is likely to affect minority students to a greater degree and that the long term effects for minority students do not differ from white students.

The evidence in this paper finds support for a democratization effect but no support for a diversion effect of attending community colleges. This may be because the groups induced to attend community college persist at higher rates due to lower costs or a better match of an the student's needs and institutional structures. The results suggesting that bachelor's degree receipt increases even for groups of students initially diverted to community college run counter to the findings of Goodman et al. (2014). There are several reasons that these findings may be different–the first is considering the local average treatment effect in both cases.

In the present study the affected population are students who respond to price as compared to students who are constrained by low SAT scores. These groups of students need not be the same or share the same response to community college attendance. Also, students who elect to attend community college instead of a four-year university when community college tuition is decreased may respond differently to community college attendance than students who are excluded from university enrollment on academic grounds.[39] Additionally, the results in this paper find evidence in support of a democratization effect of community college for racial minorities.

1.8 Conclusion

This paper presents evidence on the price sensitivity of community college enrollment as well as the long term consequences of community college enrollment. Using variation in the price of tuition at community colleges in Texas caused by the expansion of community college taxing districts and administrative data, I find that students respond to changes in community college tuition at a higher rate than the rate at which prior studies have measured responses to grant aid. Overall, students do not switch from four-year college to community college as a result of price decreases but rather switch to attending from not enrolling in college. However, there is important heterogeneity by race in the response to reduced community college tuition with racial minorities initially diverting attendance from

[39]The differing educational contexts in Georgia versus Texas may also matter.

universities to community colleges.

For students induced to attend community college, educational attainment is increased as measured by bachelor's degree receipt and credits attempted. Increased educational attainment occurs for students who switch to community college attendance from both not enrolling in college as well as from attending a university. This paper provides quasi-experimental evidence on the democratization versus diversion effect of community college and finds evidence supporting a democratization effect for community college.

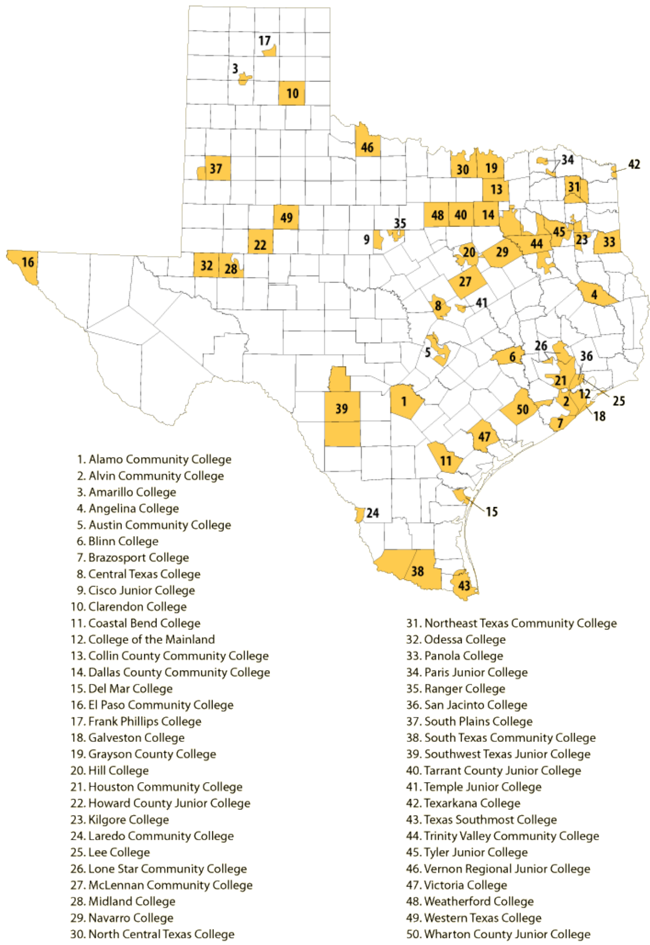
The Texas experience studied provides insight into the potential effects of reduced community college tuition on the enrollment and educational attainment of proposals that would reduce community college tuition. A $1,000 in community college tuition leads to larger increases in attendance than the same increase in financial aid primarily used at four year universities. Increasing the number of students who attend community college also increases the number of students earning bachelor's degrees. These findings help frame discussions about the merits of proposals to reduce community college tuition.

Overall, lowering community college costs provides a pathway for more students to attend college. It also has positive, longer term benefits of bachelor's degree receipt. The benefits of community college attendance may make lowering community college tuition an attractive option for policymakers seeking to increase educational attainment.

1.9 Tables and Figures

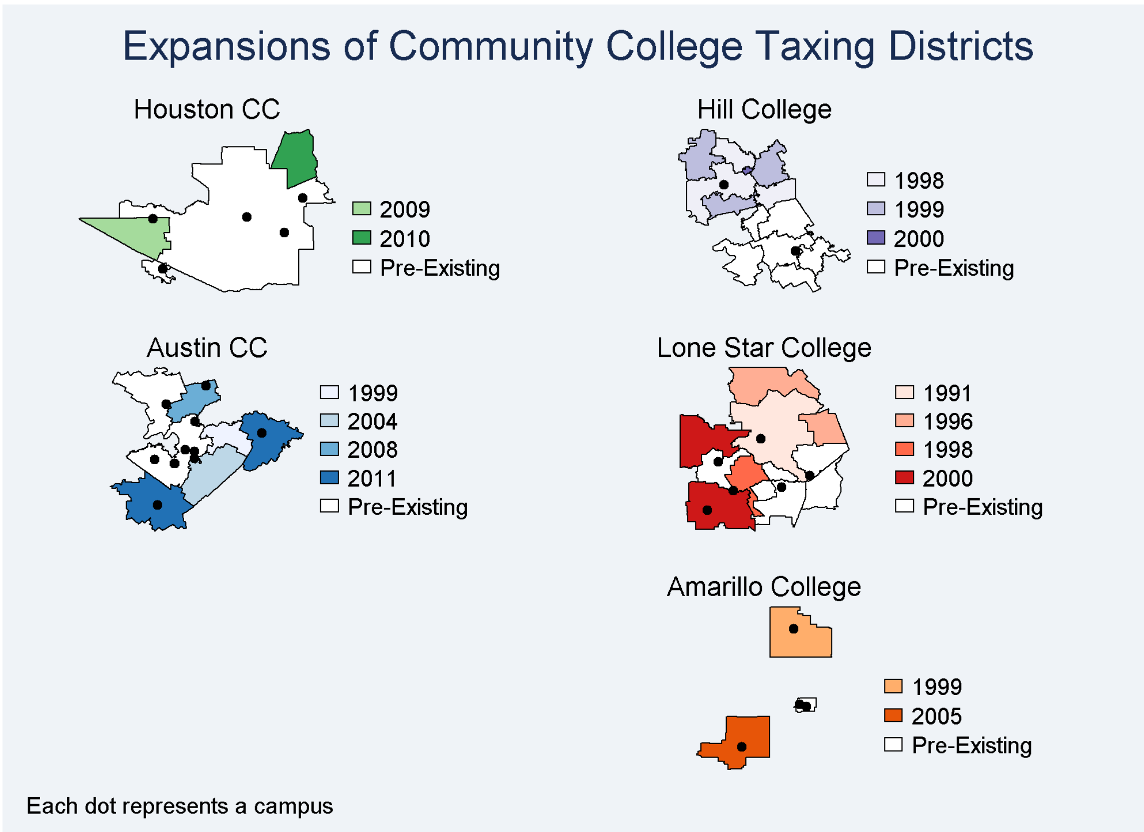
1.9.1 Figures

Figure 1.1: Texas Community College Taxing Districts



Source: Texas Association of Community Colleges, 2008. This figure highlights the areas in Texas included in a community college taxing district in 2008.

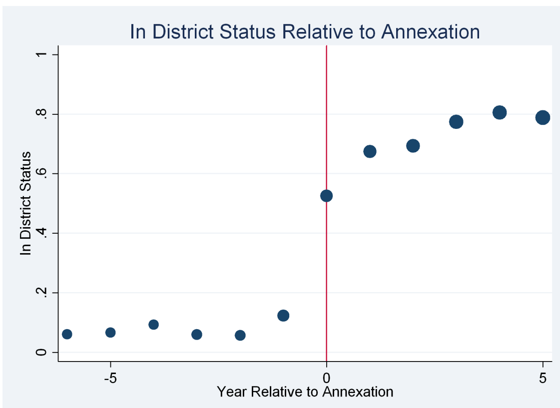
Figure 1.2: Texas Community College Expansions



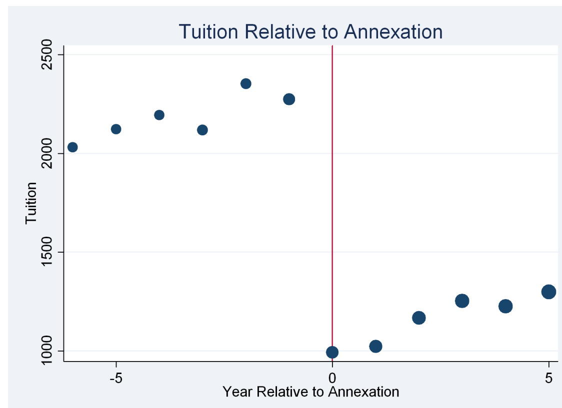
Each panel represents the taxing district of a distinct community college in Texas. The boundaries in the figures represent K-12 school district boundaries and the colors indicate when the K-12 district was annexed. K-12 districts that have no color were included in the community college taxing district prior to the start of the data.

Figure 1.3: Change In Cost

(a) In-District Status



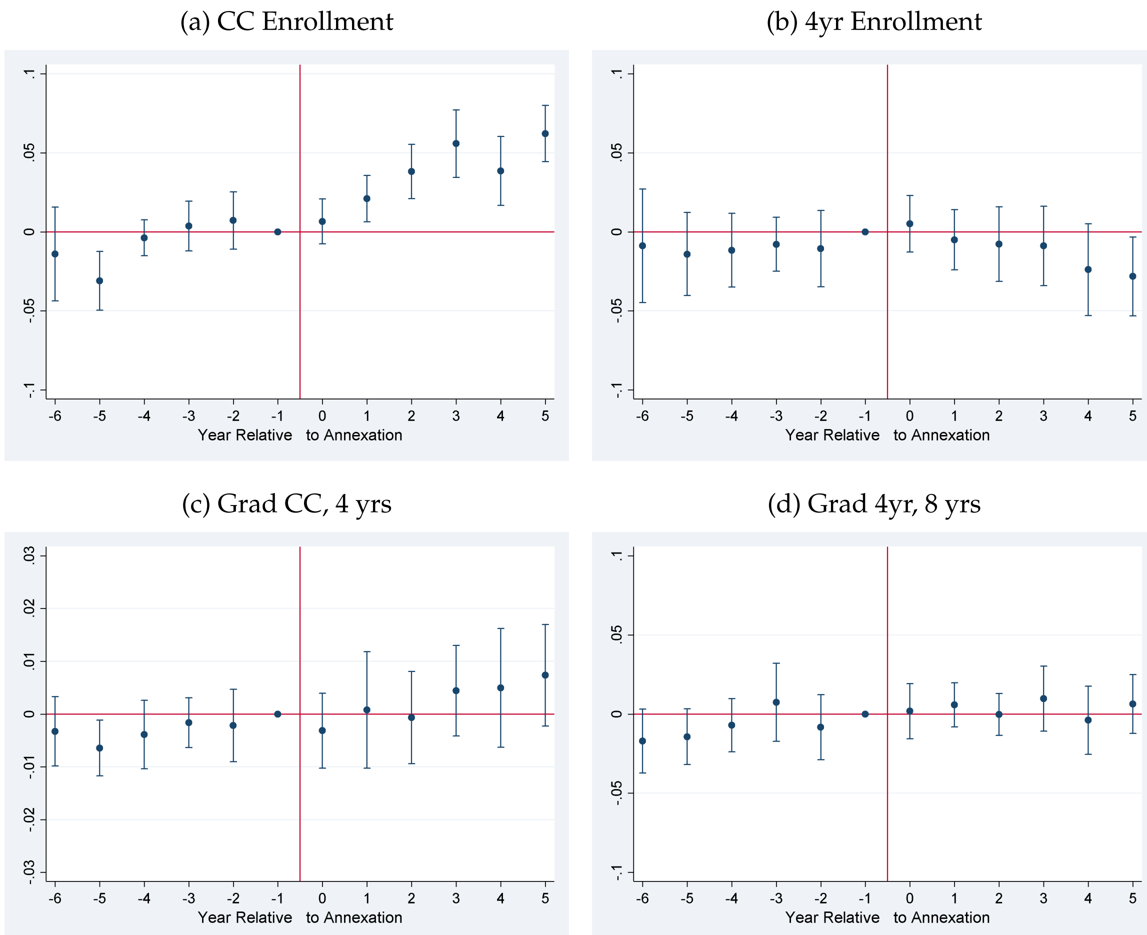
(b) Tuition



Panel A plots the fraction of students in a K-12 cohort paying in-district tuition at the local community college among students who attended community college. Each dot represents a cohort re-centered by its annexation date. The size of the dot is proportional to the number of students attending community college in that re-centered year. Only K-12 districts that experience an annexation are included in this figure.

Panel B is a plot of the tuition and fees for two semesters of 12 credits paid by student at the local community college relative to annexation. For comparability, only schools that had five years prior to annexation and five years after were included.

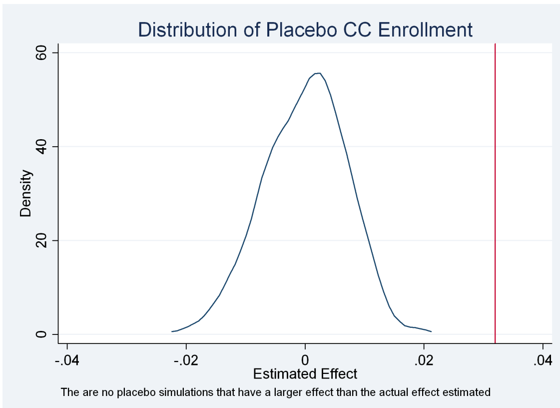
Figure 1.4: Event Studies for Annexation



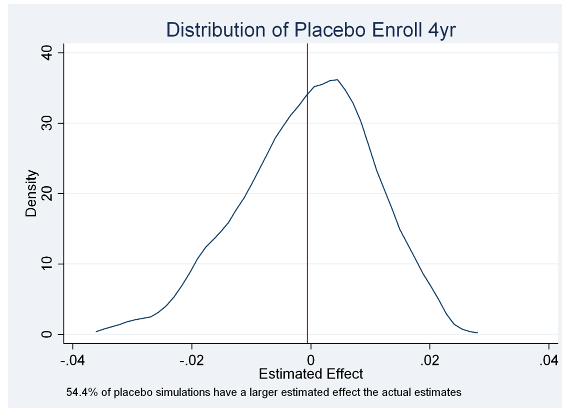
These figure plots the coefficients of a regression that compares yearly differences in student outcomes between annexed districts and districts already part of the taxing district. Panel A considers immediate enrollment in community college, Panel B considers immediate enrollment at a university, Panel C examines receiving a degree or certificate from a community college in 4 years, and Panel D examines receiving a bachelor's degree within 4 years. The regression that produces these differences also controls for demographic characteristics, year fixed effects, K-12 district fixed effects, college-by-year fixed effects, as well as the building of a new campus.

Figure 1.5: Placebo Regressions

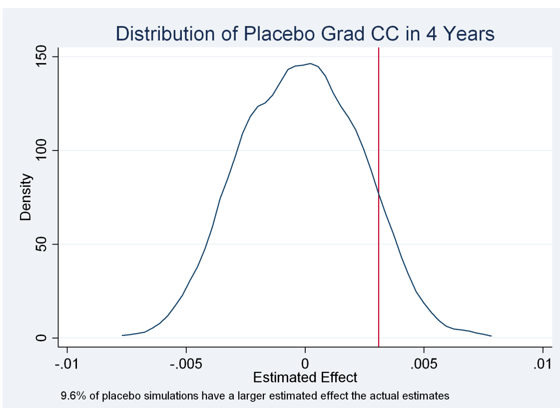
(a) CC Enrollment



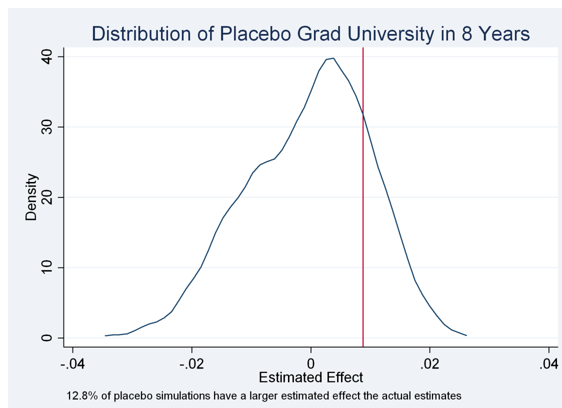
(b) 4yr Enrollment



(c) Grad CC, 4 yrs



(d) Grad 4yr, 8 yrs



The above figures represent the results of a placebo test describe in Section 1.5.2 for various student outcomes. Panel A examines enrollment in community college, panel B examines enrollment in universities, panel C examines graduation from community college within 4 years, and panel D examines bachelor's degree receipt within 8 years. The plots display the distribution of estimated treatment effects using data from other community college districts that did not experience annexation. The vertical line represents the treatment effect measured in the actual data.

1.9.2 Tables

Table 1.1: Expansions of Community College Taxing Districts

Austin Community College		
District	*Expansion of Taxing District*	*New Building*
Manor ISD	1999	1999
Del Valle ISD	2004	
Round Rock ISD	2008	2010
Elgin ISD	2011	2013
Hays ISD	2011	2014
Lone Star College		
District	*Expansion of Taxing District*	*New Building*
Conroe	1991	1995
Willis	1996	
Splendora	1996	
Klein	1998	2011
Cypress-Fairbanks	2000	2003
Magnolia	2000	
Amarillo College		
District	*Expansion of Taxing District*	*New Building*
Hereford	2005	2005
Dumas	1999	2001
Hill College		
District	*Expansion of Taxing District*	*New Building*
Rio Vista	1999	2000
Keene	2000	2000
Joshua	1998	2000
Grandview	1998	2000
Godley	1999	2000
Cleburn	1998	2000
Alvarado	1999	2000
Houston Community College		
District	*Expansion of Taxing District*	*New Building*
Alief	2009	2008
North Forest	2010	

This table outlines the expansions to the five community colleges that experience annexations of municipalities into taxing districts during the time contained in the data. Each row contains a K-12 District, the year of annexation and the year of building a new campus (if any). See Appendix A.1.1 for details on the collection of these dates.

Table 1.2: Summary Statistics

	Mean	SD	N
Enrolled in CC, Fall	0.265	0.441	206375
Enrolled in 4yr, Fall	0.247	0.431	206375
Enrolled In-District, Fall	0.211	0.408	206375
Enrolled in CC, 1 Year after HS	0.384	0.486	206375
Enrolled in 4yr, 1 Year after HS	0.232	0.422	206375
Pays In District Tuition	0.715	0.452	54658
Ever Annexed	0.391	0.488	206375
Post Annexation	0.250	0.433	206375
Building	0.180	0.384	206375
Did not Enroll	0.491	0.500	206375
Grad with 4yr Degree in 4 Years	0.077	0.266	206375
Grad with 4yr Degree in 6 Years	0.212	0.409	206375
Grad with 2yr Degree in 2 Years	0.011	0.106	206375
Grad with 2yr Degree in 4 Years	0.041	0.199	206375
Asian	0.043	0.203	206375
Black	0.112	0.315	206375
Hispanic	0.192	0.394	206375
White	0.651	0.477	206375
Male	0.512	0.500	206375
Economically Disadvantaged	0.152	0.359	206375
Limited English Proficiency	0.013	0.112	206375
Sticker Tuition	1266.2	390.7	206375
Grants	213.9	939.7	120580

This table is constructed using ERC and Texas Association of Community College data and includes students from 1994-2005 who live K-12 Districts that are part of community college taxing districts that experience any annexation from 1994-2005. This includes Austin Community College, Amarillo Community College, Hill Community College, and Lone Star Community College.

Table 1.3: Summary Statistics, Before and After Annexation

	Pre		Post	
	Mean	N	Mean	N
Enrolled in CC	0.230	29032	0.278	51680
Enrolled in 4yr	0.279	29032	0.279	51680
Enrolled In-District	0.143	29032	0.206	51680
Did not Enroll	0.493	29032	0.448	51680
Theoretical Tuition	1.962	29032	1.160	51680
Pays In District Tuition	0.109	6664	0.724	14390
Building	0.000	29032	0.359	51680
Grad with 4yr Degree in 4 Years	0.075	29032	0.095	51680
Grad with 4yr Degree in 6 Years	0.227	29032	0.245	51680
Grad with 2yr Degree in 2 Years	0.007	29032	0.013	51680
Grad with 2yr Degree in 4 Years	0.024	29032	0.050	51680

This table is constructed using ERC and Texas Association of Community College data and includes students from 1994-2005 living in K-12 districts that experienced annexation. The data are split before and after annexation. This includes Austin Community College, Amarillo Community College, Hill Community College, and Lone Star Community College.

Table 1.4: Changes in Cost

	(1) CC Tuition	(2) In District	(3) Grants	(4) Grants, No Zero
Annexation	-1.124***	0.55***	-173.1	-286.8***
	(0.0627)	(0.021)	(125.0)	(80.2)
Mean of Dep Var	1.266	0.71	322.3	3593.5
N, Students	206,375	206,375	274,739	24,639
Year and District FE	X	X	X	X
Demographics	X	X	X	X
College/Year FE	X	X	X	X

This table considers the changes in cost associated with annexation. CC tuition is the amount paid in tuition for two, 12 credit hour semesters in $1000s of 2012 dollars. In District is an indicator for whether a student pays in district tuition among community college attendees. For both tuition and in-district status, high school graduates from 1994-2005 are considered. Grants consider the amount of grants received at community colleges for high school graduates from 2001-2012. The rows at the bottom indicate inclusion of controls for year and district fixed effects, demographic characteristics including race and gender, and college by year fixed effects. Standard errors are clustered at the K-12 district level and are in parentheses with $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

Table 1.5: Student Characteristics

	(1) Asian	(2) Black	(3) Hispanic	(4) White	(5) Male	(6) Econ. Disadv.	(7) Limited Engl.	(8) College Plans	(9) Grad HS
Annexation	0.0029	-0.0057	-0.010	0.013	-0.0022	-0.036	-0.0031	-0.041*	-0.00844
	(0.0032)	(0.012)	(0.015)	(0.020)	(0.0043)	(0.027)	(0.0023)	(0.022)	(0.0141)
Year and District FE	X	X	X	X	X	X	X	X	X
College/Year FE	X	X	X	X	X	X	X	X	X
New Campuses	X	X	X	X	X	X	X	X	X
Mean of Dep Var	0.043	0.11	0.19	0.65	0.51	0.15	0.013	0.77	0.705
N	206370	206370	206370	206370	206370	206370	206370	206370	232689

This table considers how student characteristics vary with annexation. Results in columns 1 to 8 use high school graduates from 1994-2005. Column 9 examines graduation behavior for cohorts that will be annexed in the future by examining 10th graders from the 1996-2005 graduating classes. The columns at the bottom indicate inclusion of controls for year and district fixed effects, demographic characteristics including race and gender, college by year fixed effects, and an indicators for new campuses. Standard errors are clustered at the K-12 district level are in parentheses with $^{*}p < .1,^{**}p < .05,^{***}p < .01$.

Table 1.6: Immediate Enrollment Effects

A. Reduced Form				
Immediate Enrollment	CC	4yr	In. Dist	Nowhere
Annexation	0.032***	-0.00057	0.044***	-0.031***
	(0.0059)	(0.0095)	(0.0096)	(0.0086)
Mean of Dep Var	0.26	0.25	0.21	0.49
N	206370	206370	206370	206370

B. Per $1000 Dollars				
Immediate Enrollment	CC	4yr	In. Dist	Nowhere
Annexation	-0.028***	0.00050	-0.039***	0.028***
	(0.0042)	(0.0040)	(0.0038)	(0.0046)
Mean of Dep Var	0.26	0.25	0.49	0.21
N	206370	206370	206370	206370

C. Elasticity	Log	Log	Log	Log
Immediate Enrollment	CC	4yr	In Dist.	None
Log Tuition	-0.16***	0.0016	-0.36***	0.097***
	(0.035)	(0.033)	(0.066)	(0.021)
N	372	372	372	372
Year and District FE	X	X	X	X
Demographics	X	X	X	X
College/Year FE	X	X	X	X
New Campuses	X	X	X	X

This table considers enrollment in the fall immediately after high school graduation. Panel A considers the reduced form effect of annexation on enrollment and Panel B instruments for changes in tuition with annexation. Standard errors are clustered at the K-12 district level are in parentheses with $^{*}p < .1,^{**} p < .05,^{***} p < .01$. Panel C collapses the data into K-12 District/Year cells and considers log outcomes and log tuition with tuition instrumented for using annexation.

Table 1.7: Enrollment in CC by Years after HS Graduation

A. Enrollment in CC	(1)	(2)	(3)	(4)	(5)	(6)
	1 year	2 years	3 years	4 years	5 years	6 years
Annexation	0.045***	0.035***	0.020***	0.012***	0.0095***	0.0087**
	(0.0084)	(0.0043)	(0.0051)	(0.0036)	(0.0025)	(0.0035)
Mean of Dep Var	0.38	0.25	0.18	0.14	0.11	0.089
N	206370	206370	206370	206370	206370	206370
A. Enrollment in 4yr	1 year	2 years	3 years	4 years	5 years	6 years
Annexation	-0.00036	0.00038	0.0038	0.0089	0.0044	0.0070***
	(0.011)	(0.012)	(0.012)	(0.0096)	(0.0037)	(0.0025)
Mean of Dep Var	0.23	0.25	0.25	0.20	0.12	0.089
N	206370	206370	206370	206370	206370	206370
Year and District FE	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
College/Year FE	X	X	X	X	X	X
New Campuses	X	X	X	X	X	X

This table considers longer term enrollment patterns of annexation. Each column is a separate regression containing an indicator for if a student enrolled in the X^{th} year after high school graduation. For year 1, this would be if a student enrolls in the Fall, Spring, or Summer semester immediate after their high school graduation. The rows at the bottom indicate inclusion of controls for year and district fixed effects, demographic characteristics including race and gender, college by year fixed effects, and indicators for new campuses. All results use high school graduates from 1994-2005. Standard errors are clustered at the K-12 district level and are in parentheses with $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

Table 1.8: Transfer

Transfer	(1) Year 2	(2) Year 3	(3) Year 4	(4) Year 5	(5) Year 6
Annexation	0.011	0.014*	0.015**	0.0070***	0.0083***
	(0.0067)	(0.0073)	(0.0056)	(0.0021)	(0.0018)
Year and District FE	X	X	X	X	X
Demographics	X	X	X	X	X
College/Year FE	X	X	X	X	X
New Campuses	X	X	X	X	X
Mean of Dep Var	0.13	0.17	0.14	0.097	0.071
N	206370	206370	206370	206370	206370

This table considers student transfer behavior. Transfer is defined as attending a university in the Xth year when having attended a community college in a prior year. The rows at the bottom indicate inclusion of controls for year and district fixed effects, demographic characteristics including race and gender, college by year fixed effects, and indicators for new campuses. All results use high school graduates from 1994-2005. Standard errors are clustered at the K-12 district level and are in parentheses with $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

Table 1.9: Community College Effect on Educational Attainment

A. Reduced Form	(1) Grad CC in 2 yrs	(2) Grad CC in 4 yrs	(3) Grad 4yr in 4yrs	(4) Grad 4yr in 6yrs	(5) Grad 4yr in 8yrs
Annexation	-0.0023	0.00331	0.0015	0.0061	0.011
	(0.0015)	(0.00287)	(0.0040)	(0.0075)	(0.0070)

B. Instrumental Variables	Grad CC in 2 yrs	Grad CC in 4 yrs	Grad 4yr in 4yrs	Grad 4yr in 6yrs	Grad 4yr in 8yrs
Attend CC	-0.048	0.070	0.032	0.13	0.23*
	(0.029)	(0.061)	(0.079)	(0.14)	(0.12)
Year and District FE	X	X	X	X	X
Demographics	X	X	X	X	X
College/Year FE	X	X	X	X	X
New Campuses	X	X	X	X	X
Mean of Dep Var	0.011	0.041	0.077	0.21	0.25
N	206370	206370	206370	206370	206370

This table considers the effect of community college attendance on educational attainment from 1994-2005. Panel A considers the reduced form effect of annexation on graduation outcomes and Panel B instruments for community college attendance within the first year after high school graduation using an indicator for annexation. The rows at the bottom indicate inclusion of controls for year and district fixed effects, new campuses, demographic characteristics including race and gender, and college by year fixed effects. Standard errors are clustered at the K-12 district level and are in parentheses with $^{*}p < .1,^{**}p < .05,^{***}p < .01$.

Table 1.10: Heterogeneous effects

	(1) Enroll CC	(2) Enroll 4yr	(3) Enroll Nowhere	(4) Grad CC in 4 years	(5) Grad 4yr in 8 years
A. Econ. Dis.					
Annexation	0.031***	0.0072	-0.037***	0.0027	0.012
	(0.0079)	(0.010)	(0.11)	(0.0029)	(0.0094)
Annexation*Econ Dis.	0.019	-0.035	0.016	0.00013	-0.0096
	(0.027)	(0.022)	(0.023)	(0.010)	(0.014)
B. Race					
Annexation	0.027***	0.014	-0.040***	0.00053	0.015*
	(0.0069)	(0.010)	(0.011)	(0.0032)	(0.0083)
Annexation*Black	0.024**	-0.044***	0.020*	0.0084**	0.0080
	(0.0090)	(0.0083)	(0.010)	(0.0040)	(0.0068)
Annexation*Hispanic	0.015	-0.018	.00049	0.0051	-0.0093
	(0.019)	(0.013)	(.015)	(0.0055)	(0.0061)
C. Gender					
	0.029***	0.0087	-.037***	-0.00044	0.014*
Annexation	(0.010)	(0.0077)	(0.0085)	(0.0030)	(0.0072)
	0.0099	-0.0080	-0.0014	0.0078***	0.00017
Annexation*Male	(0.0096)	(0.0085)	(0.013)	(0.0025)	(0.0067)
Mean of Dep Var	0.26	0.25	.049	0.041	0.25
N	206370	206370	206370	206370	206370
Year and District FE	X	X	X	X	X
Demographics	X	X	X	X	X
College/Year FE	X	X	X	X	X
New Campuses	X	X	X	X	X

Table 1.10: (cont.)

This table considers the effect of annexation separately by different student characteristics. Each column represents a new outcome. Panel A contains results that fully interact the model with indicators fully for economic disadvantage. Panel B contains results that fully interact the model with indicators fully for race. Panel C contains results that fully interact the model with indicators for gender.

Chapter 2

Was That SMART? Institutional Financial Incentives and Field of Study

With Patrick Turley, Harvard University

2.1 Introduction

Choosing a college major is perhaps the most important decision students make in their college years, potentially influencing the jobs they are offered, their future earnings, and their contribution to society. Due to the perception that choice of major can have long-term impacts, both individually and collectively, policymakers have proposed several policies to influence this choice. Many policymakers and researchers have paid particular attention to science, technology, engineering, and mathematics (STEM) fields due to their high income potential and societal externalities. In this paper, we explore how students choose their major by investigating whether students respond to direct financial incentives when choosing their major. We do so by examining the National Science and Mathematics Access to Retain Talent (SMART) Grant, which offered financial awards to eligible students who majored in qualified technical fields.

Often, schooling is discussed as homogeneous when the type of training received can be quite heterogeneous. We explore how students choose among many types of human capital when making decisions about college major. Typically, economists have modeled choice among heterogeneous types of human capital (like college major) as agents weighing the costs and benefits of potential options. However, there may be other factors that matter like the how the major is structured, the composition of potential peers, or behavioral factors. Our study shows that small changes in the relative prices of different types of human capital can have relatively large effects on human capital acquisition.[1] Our work also suggests suggests that simple financial incentives can alter the skill composition of the work force.

On an individual level, there is evidence that college major can have significant labor market impacts. (Arcidiacono, 2004; Arcidiacono et al., 2012). For instance, in the 2009 and 2010 American Community Survey, college graduates with fine art degrees had an unemployment rate of 11.1% and an average salary of about $30,000; college graduates with engineering degrees had an unemployment rate of 7.5% and an average salary of about $55,000 (Carnevale et al., 2012). However, differences in labor market outcomes cannot be solely attributed to different returns to college majors due to selection into majors and subsequent selection into the labor force.[2] It is interesting to note, however, that with or without a degree in a STEM field, acquiring technical skills (e.g taking more math courses in

[1]The changes in incentives examined in this study are much smaller than average differences in earnings across these fields.

[2]Hamermesh and Donald (2008) find that the earnings gap across majors decreases when controlling for hours worked and selection into the labor force.

high school) may lead to a wage premium of as high as 20-25 percent (Joensen and Nielsen, 2009). While there appear to be private benefits to majoring in STEM fields, there is also evidence of externalities, suggesting a justification for policy intervention.[3]

The US Department of Education operated the SMART Grant program between the fall of 2006 and the summer of 2011 in an effort to direct college students into–and retain them in–certain fields. In particular, this program gave up to $8,000 to juniors and seniors who met a variety of criteria including majoring in technical fields or critical foreign languages, qualifying for Pell Grants (a federal needs-based grant program for college students), and having a GPA above 3.0. This program awarded $195 million in grants in the 2006-2007 school year (United States Department of Education, 2007) and over $432 million in grants for the 2010-2011 school year (Office of Postsecondary Education, 2011).

This paper investigates the effect of the SMART Grant program using student-level, administrative data from all public universities in Texas and from Brigham Young University (BYU), a large private university in Utah that received the largest amount of SMART Grants of any school in the nation in the first year of the program. By examining this program we hope to gain important insights into how students choose their major and the role that policy can play in the types of hu-

[3]For instance, Murphy et al. (1991) show that the economy of countries with a higher fraction of engineering majors grows more quickly than the economy of countries with more law concentrators. The choice of major is a significant source of interest for the Federal Government of the United States. In fact, the U.S. government has claimed, "In the case of technical fields, these majors will benefit both national and individual competitiveness, increasing the nation's economic security." (United States Department of Education, 2006).

man capital acquired. Our research design takes advantage of a discontinuity in the Pell Grant eligibility criteria and uses a regression discontinuity design to uncover the causal impact of the program on various measures of student major. Our data include students who attended these schools from the year 2000-01 to 2011-12, which allows us to conduct a robustness test of this discontinuity in the years before the grant existed as well as for one year after the grant ended. Our results show that SMART Grants did induce students to major in STEM fields as juniors and seniors who would not have done so otherwise. We also provide suggestive evidence that this response operates more strongly through encouraging students already in SMART-eligible majors to persist their major than through pushing student in non-eligible majors to switch into an eligible field. The overall estimated effect is over twice as large at BYU as at public universities in Texas. Our results suggest that programs can have very different results in different settings and over time. We explore these differences and find that the differences are consistent with salience being an important determinant of the effect of a program.

It may seem surprising that students could react to incentives that are small relative to the average wage differentials between these fields. However, large effects may exist if students are myopic, misinformed about future earnings, or credit-constrained. Credit constraints may be particularly relevant in the case of SMART Grants since the grant was only available to low-income students. The responsiveness to relatively small amounts of financial incentives suggests that behavioral factors or market failures are likely to play a significant role in the acquisition of human capital.

Our work is part of a large literature on how students choose their college major. Previous research has identified many factors that appear to play a role in this choice, including tastes and ability (Wiswall and Zafar, 2011; Stinebrickner and Stinebrickner, 2011), career risk (Saks and Shore, 2005), future earnings (Berger, 1988; Wiswall and Zafar, 2011; Beffy et al., 2012), credit constraints (Rothstein and Rouse, 2011), career opportunities (Eide and Waehrer, 1998), differential tuition (Stange, 2015), and financial aid (Evans, 2012; Sjoquist and Winters, ming).[4] Our paper contributes to this literature by providing the strongest evidence to date that even small direct financial incentives can have large impacts on a student's major.

Of the above papers, only two consider how direct financial incentives may motivate students to graduate in targeted fields. Stange (2015) uses university-level data to perform a difference-in-difference analysis of the roll-out of differential tuition programs across the country. He finds that increasing the tuition of particular majors decreased the number of students graduating in some fields, but increased it in others. He explains that the increase is likely because he is unable to decompose this effect into a response due to a price change and a change in the quality or capacity of departments who expand with the additional tuition money.

[4]Many studies have found that merit-based financial aid programs have increased college enrollment (Kane, 2003; Dynarski, 2004; Cornwell et al., 2005), decreased college dropout rates (Dynarski, 2008), and raised GPAs (Scott-Clayton, 2011). However, the evidence on how these programs impact course taking is mixed, with papers that report that merit-based aid programs increase, decrease, and have no effect on course credit accumulation (Scott-Clayton, 2011; Brock and Richburg-Hayes, 2006; Angrist et al., 2009; Cornwell et al., 2005). Turner (2014) illustrates that grant aid can be captured by the institution rather than fully realized by the student. Turner finds that 11% of Pell aid is captured by universities though the estimate is smaller at 4.9% for public universities. We proceed with our analysis noting that some of the aid disbursed may be captured by universities but that that amount is likely to be small.

In contrast, we use individual-level data, which allows us to compare students within the same institution who qualify for direct financial incentives to those who do not.

Concurrent with our study, a working paper by Evans (2012) considers the impact of SMART Grants at Ohio pubic universities and finds little evidence suggesting that the SMART Grant program increased the number of students graduating in STEM fields. There are several key differences in our paper which will be discussed in detail later in the paper. However, he is limited in that he only has data from 2006 to 2010. He finds little evidence suggesting that the SMART Grant program increased the number of students graduating in STEM fields. We attribute the difference in our results to a variety of factors. Importantly, we have access to more years of data, we use a different method of sample selection, and we have a more diverse sample of universities. In the National Postsecondary Student Aid Survey administered in 2008, there is evidence that there is little knowledge of the program nationally, which means it should be unsurprising that no impact can be measured in early years. When we replicate Evans' methodology and data restrictions but using our data, we similarly find no significant impact of SMART Grants on students' choice of major.

The rest of the paper is organized as follows. Section 2 gives details of the SMART Grant program. Section 3 describes the data used. Section 4 discusses the econometric identification. Section 5 presents the results and Section 6 concludes.

2.2 The SMART Grant Program

The U.S. Federal Government operated the SMART grant program from the fall of 2006 until the summer of 2011 with the purpose of increasing the number of students who were studying STEM fields and critical languages. This federal program was designed to complement the existing Pell Grant program. Students who were eligible for the Grant received up to $2,000 per semester in their junior and senior year for a maximum benefit of $8,000[5]. In order to be eligible for a SMART Grant a student was required to:

- be a U.S. citizen;
- be Pell Grant-eligible during the award semester;
- be majoring in physical, life or computer science, engineering, mathematics, technology, or critical foreign language fields–hereafter "SMART fields" or "SMART majors;"[6]
- be a junior or senior (or fifth year student in a five year program) as defined by credit hours;
- be enrolled as a full time student;[7]

[5] The award amount could not exceed the cost of attendance less Pell Grant receipts

[6] In practice, this was all foreign language majors in the later years. We use the definitions from 2011 to define which majors are SMART eligible.

[7] Starting in 2009 a pro-rated award was available to students who were enrolled in at least 6 credits.

- have at least a 3.0 GPA on a 4.0 scale; [8]

To be Pell-eligible a student must submit a Free Application for Federal Student Aid (FAFSA). The FAFSA is used to compute an Expected Family Contribution (EFC) which is a score that represents how much a student's family can afford to contribute to the student's post-secondary education. This EFC determines what federal grant and loan programs a student is eligible for. The threshold that defined whether a student was eligible for Pell Grants increased gradually throughout the time frame of this study. In the 2006-2007 school year the EFC cutoff for Pell Grants was 4,110 and by 2010-2011 the EFC cutoff for Pell grants had risen to 5,273.

Students with an EFC below the Pell Grant threshold in a particular year received the full amount of the SMART Grant in that year, while any student above the threshold received no SMART Grant money that year.[9] As a result, students local to the threshold were very similar in family income, but they may have differed in their incentives to major in eligible fields by up to $4,000 per year[10]. Our identification strategy will take advantage of this large discontinuity in incentives.

An additional issue that also may affect the efficacy of the SMART Grant program is how informed students were about the existence of the grant. Bettinger

[8]Officially this was 3.0 for course work required for the major. In practice, some school websites listed the requirement as 3.0 cumulative GPA .

[9]Provided they were not already receiving other sources of aid that was not greater than the Cost of Attendance. In practice nearly all students received the full amount of the SMART Grant for a given semester.

[10]EFC is computed yearly and so an eligible junior in fall semester would receive $4,000 more than an ineligible student.

et al. (2012) highlight how the salience and simplicity of federal grant and scholarship programs can have first-order impacts on program take-up. According to the National Post Secondary Aid Survey, only 6.8% of Pell recipients in 2007-2008 knew about the SMART Grant program. Of the relatively few students who had heard of the program and were declared in SMART majors, 4.7% said SMART Grants had affected their choice of major. Of those who had heard of SMART Grants and who were undeclared, 19.1% said that the grants would "definitely or probably affect their choice of major. Of the students who were declared in non SMART majors and had heard of SMART Grants, 16.8% said they would "definitely or probably [11]. This survey suggests that among students who knew about them, SMART Grants had the potential of influencing choice of major, but given that so few students knew of the programs existence by 2007-2008, the measured impact of SMART Grants may be small or undetectable in its early years, which is consistent with what is seen in the data. One reason that the program may not have been well known is that students did not have to file additional forms when applying for the SMART Grant beyond the FAFSA. Rather, the SMART Grant was automatically added on to financial aid packages if the student was eligible.

2.3 Data

The data come from two administrative data sets. The first data set was assembled for the purposes of this study by the Texas Higher Education Coordi-

[11] These statistics from the NPSAS are the authors' calculations.

nating Board (THECB). The Texas data contain information on every student who enrolled in Texas public universities from 2000-2001 to 2011-2012, providing a diverse set of public institutions and a large number of students enrolled in higher education. The data include Expected Family Contribution (EFC) from every student who submitted a FAFSA and subsequently enrolled. It also includes information on a student's declared major in every semester they enrolled, degrees received, parent's education, student race, student full time/part time status, cost of attendance, Texas residency and student gender[12]. For this study we consider only students who are attending full time because SMART Grants were available only to full time students for the majority of the life of the grant. We also restrict the sample to students for whom the cost of attendance was high enough to enable the maximum Pell Grant in a given year.[13]

The second data set includes very similar information for Brigham Young University starting in 2001-2002. The biggest difference in the BYU data set is additional information about classes taken for all students at BYU. The BYU data also includes additional demographic variables, namely ACT/SAT Score, and the high school rank of a student (which we express as a percentile) and lacks information about parental education. Unfortunately ACT/SAT score and class rank variables are not available for every student, but we only use them as covariates in our regression specification. Our results are robust to specifications that do and do not

[12]Administrators at THECB feel most confident about the accuracy of the financial aid data starting in 2005. The only substantive variable we use from before that time is EFC, and it appears to follow similar patterns to the data from post 2005 so we feel confident using these data.

[13]This restriction does not affect many students but simplifies the calculation of the cutoff for SMART Grants.

include these variables.[14]

Summary statistics for Texas students with an EFC within 2,000 units of the eligibility threshold are presented in Table 2.1; a similar table for BYU is also presented in Table 2.1 but with a window of 3,000; these windows roughly correspond to the largest window chosen when estimating with the respective data sets. The Texas sample from 2006-07 to 2010-11 is majority female and 30% Hispanic. Many students in Texas have parents who did not attend college. At public universities in Texas, 19.2% of juniors are declared in SMART eligible majors. Less than one percent of these are declared in language majors; the majority being in STEM majors.

For BYU, the summary statistics reveal that the student body in this EFC window is 52% male and predominantly white. The fraction of students with SMART eligible majors in their junior year is higher than the fraction for schools in Texas as well with 27% of students declared in eligible majors, with a small fraction in language. This is much larger than the fraction of students in SMART eligible major in Texas schools in this period. We note though that before 2006, the fraction of SMART majors at BYU is more similar to schools in Texas at about 22%. The divergence between Texas and BYU in the years following the grant's implementation is consistent with the results found in our analysis, which find much larger effects of the grant at BYU than in Texas.

[14]We use a mean value imputation when high school percentile or ACT score is missing along with a dummy variable for a missing observation, this mean value imputation does not change the results significantly.

BYU is unique in that it distributed more SMART Grants than any school in the nation in the first year of the program. (McArdle et al., 2007) In fact, 4.17% of students at BYU in our data received a SMART Grant in 2006-2007. By the end of the program in 2010-2011 6.2% of the student body were receiving SMART Grants. The reason for this large number of SMART Grant recipients is likely because BYU has a very high fraction of students receiving Pell Grants. Over 30% of BYU's student body received Pell Grants in 2001 which is one of the highest proportions of Pell recipients among comparable institutions in the nation. (Heller, 2004)

While BYU's position as the top distributor of SMART Grants may give cause to question the external validity of any estimates using data from BYU, it may still provide insights of the impact of these grants in a population that was likely to be aware of the grant. During this time frame around 5% of all BYU students were receiving SMART Grants, which means that many students were likely to have heard about the program through informal channels. In fact, some majors at BYU publicly advertised at orientation meetings that choosing their major could result in up to an additional $8,000 in grants. Public universities in Texas, however, seem to resemble more closely national patterns for the fraction of students receiving SMART Grants. In the Texas data there were 2,808 SMART Grants awarded in the 2006-07 school year, and 6,496 were awarded in 2010-11 .[15]

[15]Some of this increase is likely due to relaxing the requirements for the grant, but some is also likely to represent real growth in SMART Grants distributed.

2.4 Identification

2.4.1 Background

When a student completes the FAFSA, their EFC is computed from information about family income, assets, and number of dependent children in a student's family. This EFC determines eligibility for a host of federal grant and loan programs like Pell Grants, SMART Grants, subsidized student loans, etc. Each year a minimum Pell Grant and an EFC threshold are set. If a student's EFC is below the EFC threshold, then the amount of a student's Pell Grant will be equal to a decreasing function in EFC that equals the minimum Pell Grant at the threshold and is zero for all values above the threshold.[16] This means that if the student's EFC is above the EFC threshold, no Pell Grant is received. Although the amount of a student's Pell Grant is a function of their EFC, students receive the whole SMART Grant if their EFC is below the threshold that qualifies them for a Pell Grant of any size. Thus, this discrete cutoff in Pell eligibility serves as a discrete cutoff in SMART Grant eligibility and facilitates a fuzzy regression discontinuity design. The identification comes from the fact that students barely on one side of the Pell eligibility cutoff are similar to students on the other side in both observable and unobservable ways, but they differ in their eligibility for SMART Grants. Estimates for the impact of the program are all local to the margin of eligibility; namely, students with families who are just barely eligible. Roughly, these are students with family incomes from $40,000 to $60,000 in 2010 dollars (Office of Postsecondary

[16]This function is a step function. In general, the function takes on the minimum Pell amount for a few hundred EFC units below the EFC threshold, though this varies from year to year.

Education, 2011).

Since the threshold for eligibility for SMART Grants is the same as the threshold for Pell Grant eligibility, using this threshold may conflate the effect of SMART Grants and the effect of Pell Grants. We address this by performing the same analysis on the Pell Grant eligibility threshold in the years before SMART Grants were implemented and find that Pell Grant eligibility had no impact on the outcomes of interest in those years. We also perform the analysis for the one year in the data after the grant program ended and again find no effect. The likely reason for this null finding is that the Pell Grant for this marginal group was only \$400 per year in 2006 and grew to \$976 per year in 2010. This amount is small relative to SMART Grants, which paid \$4,000 per year.[17] Additionally, the Pell grant offers no price incentives for major and would be operating through an income effect which is less likely to affect SMART major participation. Additionally, we will later show that the largest responses measured were not in years with the largest minimum Pell Grants.

2.4.2 Estimation

The basic estimating equation that takes advantage of this discontinuity in EFC eligibility is:

$$Y = f(\widetilde{EFC}) + \theta \cdot 1(\widetilde{EFC} < 0) + X\beta + \eta_u + \epsilon \quad for|\widetilde{EFC}| < h \tag{2.1}$$

[17] During the summers of 2009, 2010, and 2011 students were eligible for a "third semester" of Pell Grants. Notably students were also eligible for an additional semester of SMART Grants during this time.

where Y is the outcome of interest, $f(\widetilde{EFC})$ is a flexible function of junior-year re-centered EFC where EFC is re-centered so that $\widetilde{EFC} = (EFC - MaxEFC)/1000$ and $MaxEFC$ is the maximum EFC in a given year that is allowable to qualify for Pell Grants. This re-centering means that $\widetilde{EFC}$ being 0 or negative indicates a person was eligible for a Pell Grant. X is a vector of covariates including indicators for student race (African American, Hispanic, Asian, missing race, with White omitted), and parent's highest educational attainment indicators.[18] University fixed effects, η_u are included when using the Texas data. [19]

In some instances, the above equation is estimated but $f(\widetilde{EFC})$ and $1(\widetilde{EFC} < 0)$ are interacted with indicators for student characteristics. This allows a comparison of the discontinuities for two groups of students and also accommodates the implementation of a Regression Discontinuity Difference estimator and compares the discontinuity in the years of the program to the discontinuity in the years before the program.

Choice of Student Classification for EFC

In order to be eligible for federal aid–and in many cases any financial aid–students must submit a FAFSA every year. The EFC calculated from the informa-

[18] At BYU this also includes information about ACT/SAT score as well as high school percentile and does not include parental education indicators.

[19] As in Imbens and Lemieux (2008) and Lee and Lemieux (2010), estimating $f(\widetilde{EFC})$ using kernel regression with a rectangular kernel yields the same results as a linear regression on a local subsample allowing the slopes to vary on either side of the cutoff; as such, we estimate this equation using Ordinary Least Squares. The covariates are only included to increase precision and are not necessary for identification.

tion on the FAFSA applies from the semester that the FAFSA is submitted until the following Fall semester.[20] As a result, our data potentially contain several measures of EFC for each student. Many factors impact which EFC measure is the appropriate one for our analysis. On one hand, using the EFC from the students' junior or senior year may be best since those are the measures that actually determine SMART Grant eligibility. On the other hand, using an earlier year might be best since many students likely choose their major before their junior year. Ultimately, we use the EFC from the students' junior year for several reasons.

It is critical for the research design that students are able to respond to their eligibility for SMART Grants by altering their choice of major. Because freshman or sophomore EFC do not convey information about SMART Grant eligibility we opt not to use freshman or sophomore measures.

In contrast, in most instances juniors will know their precise eligibility for SMART Grants before making choices about their college major in their first semester as a junior. Returning students typically file the FAFSA in the spring before the school year. This allows students to know their Pell (and SMART) eligibility before making choices about their major in the next year. Figure 2.1 depicts the fraction of eventual junior FAFSA filers that have filed their FAFSA by a particular date in the 2007-08 National Postsecondary Aid Survey cohort. This figure shows that 67% of juniors who eventually will file a FAFSA have submitted their FAFSA by the end

[20]If a student has a life event that would change their EFC after their FAFSA has been submitted, a student may amend their FAFSA and receive Federal Grant money for the semester in which they submit the amendment if they then qualify.

of May and 83% by the end of July. Consequently, most juniors will know their eligibility for the SMART Grant when deciding what major they should pursue in their junior year. This is especially true when a student's first semester as a junior occurs in Spring or Summer semesters due to the extra time to file the FAFSA.

The timing of the federal financial aid process and the rules of the SMART Grant made it so that most students would know their eligibility for the SMART Grant before their first semester classified as a junior. However, students near the eligibility threshold would not know about their eligibility more than a year in advance. We use junior EFC as the running variable because it determines eligibility for the program and is known by most students far enough in advance to affect their behavior.

2.4.3 Choice of Years to Include in Estimation

Choosing which years to include in the analysis is an important consideration for several reasons. Our goal in choosing the years is to identify the students who were "treated" by the SMART Grant program. For this reason we focus on a student's junior year as an indicator for treatment because it is the first year a student can actually receive funding from the grant. In both the BYU and the Texas data we start by including students who were juniors in 2006-2007. The number of SMART Grants awarded increased throughout the life of the grant, and it is likely that salience increased as well. Additionally, students in their junior year at the beginning of the program would likely have higher switching costs because they had already invested time in their chosen major. Students who were juniors later would have potentially known about the program for the entirety of their college career, and as such, would be more likely to respond to the grant. For this reason we also present analysis for students who were juniors in the last three years of the program, in the 2008-2009 to 2010-2011 school years, and expect there to be bigger effects in those years.

Choosing the last year for treatment is done under the goal of maximizing the number of students treated. The SMART Grant program started in the Fall of 2006 and was discontinued in the Summer of 2011. For this reason, we use all students who were juniors during the life of the program. We also examine the years before the SMART Grant and the one year afterwards as placebo exercises.

2.4.4 Assumptions for Regression Discontinuity

One assumption of the regression discontinuity estimator is that students are not able to precisely manipulate their EFC to gain access to the grant. If students in SMART Grant eligible majors precisely manipulate their EFC to be eligible for Pell Grants or were more likely to submit a FAFSA conditional on being Pell eligible, the distribution of EFC would have a discontinuity at the eligibility threshold with additional weight to the left of the threshold (just-eligible students). Fortunately for our identification, the formula for determining EFC is complicated and opaque, using a large number of current and historical factors, making it difficult to manipulate EFC precisely. We test manipulation and selection by analyzing the distribution of EFC around the threshold. Figure 2.2 displays the density of EFC reported in both the BYU and THECB data and it does not show evidence of manipulation. Oddly, in both data sets, it appears that there are actually fewer students to the left of the threshold that to the right, which is the opposite of what would be expected if there were manipulation of EFCs or differential reporting. In formal testing of this manipulation as outlined in McCrary (2008), the discontinuity is significant in Texas when considering students who were juniors from 2006-2011, but the discontinuity drops in magnitude and is no longer statistically significant when considering juniors from 2008-2011. At BYU, the manipulation is never statistically significant but again goes in the direction of students moving out of eligibility. In other samples Turner (2014) and Evans (2012) find these same visually suggestive but statistically insignificant distributional attributes. Given all this evidence, we believe this form of selection bias is likely to be negligible.

Another assumption is that observed and unobserved student characteristics do not vary discretely at the EFC eligibility threshold. We test that observed student characteristics do not vary by estimating Equation 1 with the outcome variable being student characteristics, and results are presented in Table 2.2. We also test that school characteristics do not change by checking to see if school characteristics such as the fraction of SMART majors or Pell Eligible students at a university changes at the threshold. For all Texas schools there are 14 covariates considered, and in the time frame from in both 2006-2011 and 2008-2011, there is never any statistically significant discontinuity in covariates. For the 11 coefficients at BYU from 2006-2011 there are no statistically significant differences at the 5% level. Similarly for 2008-2011 at BYU only one coefficient is significant at the 5% level. Given that we are testing for discontinuities in 24 covariates in two time frames, finding only one that appears significant at the 5% level is what we would expect under that hypothesis that student characteristics are smooth through the threshold. Overall there is evidence that observable student characteristics do not vary discretely at the threshold for Pell/SMART eligibility, increasing our confidence in the causal estimates found below.

Outcome Variables

The primary outcomes considered are being declared in a SMART eligible major at the beginning of a student's junior or senior year or earning a SMART eligible degree. Specifically, the junior major variable is a binary variable that indicates if a student is declared in a SMART eligible major in the first semester that they are classified as a junior. This variable is only defined for students whom we observe in their junior year. The senior major variable is defined as unity if the student is declared a SMART major in the first semester of their senior year and 0 if they are declared in a non SMART major in their senior year or do not appear as seniors in the data.

The degree outcome is a binary variable that indicates if a student receives a degree in a SMART Grant qualified major. This variable is only defined for all students who have a valid EFC measurement as a junior and is a one if a student receives a diploma in any field in the time-frame studied and a 0 if the student receives a degree in a non SMART field or does not receive a degree. Because many students in the last years of our data will not have had sufficient time to graduate, the fraction of students graduating will be lower than it would be if we had additional years of data. At BYU we have data on coursework, so we also consider the fraction of credits earned that are in SMART fields in a student's junior or senior year. Students who are not observed taking courses as seniors have will have the fraction of their courses in SMART fields coded as 0.

To confirm that the grant was administered in a discontinuous way, we consider actual receipt of the grant as an outcome as well. We express this as the total amount of SMART Grant dollars ever received as well as an indicator for whether a student ever receives SMART Grant money to provide evidence that there was a discontinuity in SMART Grant receipt. We perform this analysis separately for students who were declared as SMART majors as juniors, as well as for students who were not declared as SMART majors as juniors.

The optimal bandwidth, h, was chosen using the optimal bandwidth rule of thumb (Imbens and Kalyanaraman, 2012) and is roughly 2.0 for the BYU data and 1.0 for the Texas data, although the actual optimum varies by outcome.[21] We show later, however, that our results are not sensitive to our choice of bandwidth. Standard errors are corrected for heteroskedasticity in all specifications.

In all specifications, the parameter θ from equation 1 is the coefficient of interest. It represents the average effect of a student becoming EFC-eligible for a SMART Grant in their junior year. That is, a student could receive the grant if they were eligible in other ways (e.g. major in an appropriate field, have a high enough GPA, etc.) Since students may be eligible by EFC but not be eligible by

[21] For degrees the bandwidth is 1.2, total SMART Grant received has a bandwidth of 1.6, and ever received a SMART Grant uses a bandwidth of .9.

other criteria (other than major), θ may be considered a lower bound on the impact of otherwise eligible students.

2.5 Results

2.5.1 Grant Receipt

As discussed above, using a single year's EFC is not a perfect way to separate eligible and ineligible groups since students who are eligible in their junior year may no longer be eligible in their senior year. In the extreme, this could mean that students local to the eligibility threshold may all receive similar amounts of SMART Grant money on average, regardless of which side of the threshold they are on in their junior year. If this effect is so exaggerated that there is no measurable discontinuity in grant money received at the eligibility threshold, then a regression discontinuity design would not be appropriate since there is no discontinuity in treatment.

We test for a discontinuity in SMART Grant receipt with a regression discontinuity analysis of total SMART Grant awards. The total SMART Grant award variable is the sum of all of the SMART Grants received. We conduct this analysis separately for students who are declared as SMART majors in their junior year as well as for students declared in any other majors. Graphical results based on these regressions are found in Figures 2.3 and 2.4 and the estimates from these regressions are found in Table 2.3 .

These regressions highlight several important considerations in our analysis. We see in the figures that there is a clear and unambiguous discontinuity at the threshold for students declared in SMART majors. However, for students not declared in SMART majors there is not discontinuity in terms of grant eligibility as would be expected. In Table 2.3 all of these discontinuities are significant at the 1% level for students in SMART majors and zero for students not in SMART majors. In the SMART Grant amount regressions in the 2008-2011, we estimate at discontinuity for students declared in SMART majors of of about $589 for Texas students and $1,772 for BYU students. These measurements are all slightly smaller when we use the 2006-07 to 2010-11 samples. There is no discontinuity in SMART Grant dollars for students in non SMART majors as would be expected.

The magnitude of these discontinuities give a sense of how binding other conditions of the grant are for students. In Texas the estimate is substantially smaller than the estimate at BYU suggesting that other factors (like GPA) play a larger role in determining eligibility at Texas universities than at BYU.

A second thing that can be learned from the figures is that we are measuring eligibility at one point in time while eligibility will be determined several times. That is, if students' eligibility was entirely determined by their junior year EFC, we would expect the level on the right (corresponding to ineligible students) to be zero. The positive values for ineligible juniors give a sense of the fraction of students who are ineligible in their junior year but are eligible in later semesters. In Figure 2.4, there are non-negligible positive values to the right of the threshold. For instance, of the just-ineligible students at Texas public universities in eligible fields, about 15 percent eventually receive money in a later year. This contrasts with the approximately 30 percent of students who receive SMART Grant money who barely meet the EFC criterion in their junior year. At BYU, approximately 30% of EFC-ineligible junior students in SMART majors eventually receive SMART Grant money relative to 70% for EFC-eligible junior SMART majors.

2.5.2 Student Outcomes

Majors, Diplomas, and Courses To test the impact of SMART Grants on student major, we look at a variety of outcomes. In both the Texas and BYU data, we have information on the declared majors of junior and senior students and also information on the diploma they eventually received. In the BYU data, we additionally have information on the fraction of classes that were taken in SMART eligible fields. We conduct our analysis with a 2006-07 to 2010-11 subsample and a 2008-09 to 2010-11 subsample, but for these regressions we also measure the discontinuity for students who were juniors before 2006 as a robustness check. Results from regressions are in Table 2.4. Graphical evidence is presented on junior major in Figure 2.5, on senior major in Figure 2.6, degrees granted in Figure 2.7, and courses taken at BYU are found in Figure 2.8.

Figure 2.5 contains plots of the estimated regression lines superimposed over a bin-scatter plot for all of our specifications corresponding to the junior major outcome variable. In the Texas plots, a small but clear discontinuity can be seen at the threshold in the 2006-2011 data and an even larger discontinuity can be seen in the 2008-2011 data. In the BYU plots, the discontinuity is much larger. Figure 2.6 gives parallel figures but for the senior declared major outcome.

The estimates from these regression in Table 2.4 tell the same story as can be seen in the figures. In Texas in the 2006-2011 sample for both junior and senior major, a positive but insignificant effect of about 1.5 percentage points is measured. When we restrict our sample to only students who were juniors from 2008-09 to 2010-11, the magnitude of the effect in both regressions doubles to 3.27 percentage points for junior and 3.18 percentage points for senior major , and both are significant at the 5% level. This discontinuity indicates that roughly 3% students responded to the incentives of the grant and adjusted or persisted in their choice of major.

This is consistent with students who are already several years into the university studies either being unaware of the program in its early years or for the switching costs of changing into a qualified major being too high to motivate a large number of students to switch their major. Including these early students attenuates our measure to insignificant levels. This 3 percentage point increase is over a baseline SMART participation rate of 18% which is 17 percent increase over the baseline.

At BYU in the 2006-07 to 2010-11 sample, we measure a larger effect of almost 7 percentage points for both junior major, but this effect is only significant at the 10% level. For senior major the effect is larger at 8 percentage points and is significantly different from zero at the 5% level. Similar to the Texas data, when we restrict our sample to the 2008-09 to 2010-11 sample, we measure an impact of 10 percentage points with 95% confidence. This gives further evidence of an increasing impact in later years of the program. This increase is over a baseline of 22.4% which is a 45 percent increase over the baseline

As discussed above, we attribute a large portion of the magnitude differences between Texas and BYU to the greater salience of the program.[22] Since a much larger of fraction of BYU students are eligible for Pell Grants, more students would have heard of the SMART Grant program through informal channels, making it more likely that this program could have an effect. It is also possible, however, that other characteristics of the student body or universities accounts for this heterogeneity, such as different policies for declaring majors, differential response to the incentives across schools, or that income-marginal students in Texas may be less likely to be qualified along other margins such as GPA or citizenship. Anecdotally, we know that some BYU departments used the SMART Grant to recruit students into certain majors.[23]

In Texas, we are unable to detect an impact of SMART Grants on the number of diplomas awarded in SMART eligible fields. This is seen in Figure 2.7. There is no apparent discontinuity in the Texas plots, and in the BYU plots, the estimated discontinuity is obscured by a lack of precision in the data. The regression results in Table 2.4 confirm what we see in the figures: the impact of the grant on eligible degrees granted at Texas public universities is virtually zero, and the 6.6 percentage point effects measured at BYU is marginally statistically significant. This is likely because the data only contain degrees for students who have finished by 2012. Many students who were juniors during the life of the program had not

[22]This difference is also likely due to differences in eligibility on other dimensions like GPA.

[23]We reached out to all Texas public universities to try to examine if similar advertising was done but only received a handful of responses. All respondents indicated that they had not done any recruiting using the SMART Grant.

graduated by 2012 and therefore are treated as if the grant had no impact on their diploma in our data. In a few years when these students have graduated, it may be possible to measure the impact of SMART Grants on diplomas awarded. Since our data suggest that students at BYU responded more strongly and earlier to this program, it is unsurprising that a small impact on diplomas awarded can already be detected even with our limited data. However, students at Texas responded most strongly in the last year of the program. As a results, even those students who eventually graduated in SMART field as a result of the grant would only be coded as having responded if they graduated in no more than one year after they were first classified as a junior. This is uncommon, suggesting that a more accurate measure for this particular outcome would be possible to obtain if more years of data were available.

At BYU, we also have data on the specific courses students are taking.[24] This allows us to test whether students are "gaming" the program by signing up for eligible majors to receive the SMART Grant money but not taking courses in the major since they never intend to complete it. We attempt to identify this by measuring the discontinuity as before, but using as the outcome variable the fraction of courses that a student takes in SMART eligible departments. Despite a small sample size, we see that both the point estimates for the fraction of courses taken by juniors and seniors class taking are positive and are marginally statistically significant. These results give credence to the claim that the measured impacts on con-

[24]The THECB only recently started collected course-level data, so we could not conduct this analysis with their larger data set.

temporary major are a result of students adjusting their actual major in response to the program rather than students gaming the system.

We also conduct a placebo test by performing the same regressions for students in the years before the SMART Grant was instituted. With one exception, each of these regression coefficients are close to zero and statistically insignificant. The exceptional case is the effect on junior major at Texas, for which we measure a small but marginally significant impact of 1.7 percentage points. Since we only measure an impact as large as 3 percentage points in our 2008-2011 Texas regression, this placebo estimate cannot be statistically separated from the measured impact in the years the grant was operating. This may raise concerns that the effect we measure in our main specifications are not due to SMART Grant but rather due to other factors that existed before the SMART Grant program. Several things, however, make us believe that this placebo estimate should not be so concerning. First, this oddity disappears in the senior major placebo regression, which includes the same students but measured a year closer to graduation. Also, this junior year placebo test oddity is not present in the BYU data. Additionally, in the year after the program there is no effect on student major declaration in the junior or senior year at Texas or at BYU which can be seen in Table 2.5. This evidence suggests that the effect we measure is actually the impact of SMART Grants rather than Pell Grants or other programs that might discretely vary across the Pell Grant eligibility threshold.

We formally estimate the difference in the pre period vs. 2008-09 to 2010-11 in Table 2.4 using a Regression Discontinuity Difference estimator. In Texas there is always a positive effect measured though it is only statistically different for senior major. At BYU the results are similar to estimates using data only from 2008-09 to 2010-11 though the results are slightly less precise with only junior major being marginally statistically significant.

Effects by Year The regressions above suggest that there may be a large amount of heterogeneity across time, and more specifically, the impact of SMART Grants grew over the lifetime of the grant program. To examine the heterogeneity of the effect across time we estimate the discontinuity separately by pairs of years except for the last year for which we have data. Specifically, the regressions estimate the discontinuity for students who were juniors in the school years beginning in 2001-02 to 2002-03 . These estimates are plotted with their 95% confidence intervals in Figure 2.9. The actual regression results are found in Table 2.5.

Clearly, reducing the sample in each of these regressions drastically reduces our ability to precisely measure the yearly impact. Several patterns emerge from these regressions nonetheless. First, in all of the sets of regressions, the only regressions reaching any level of significance are those corresponding to the 2009-10 to 2010-11 junior cohort. We note that the regressions meet 90% confidence at BYU for both junior and senior major, and meet 95% confidence in the in Texas for junior and senior major. The magnitude of these regressions is slightly larger than the 2008-2011 estimates reported before. Second, in every regression corresponding to years before SMART Grants were being distributed, the estimates are insignificant and effectively zero in magnitude.

The measured discontinuity sharply drops for junior and senior major when the grant expires in the 2011-12 school year in both the BYU and the Texas data. This is an additional falsification test in addition to using previous years. The estimated zero effect reinforce the idea that the measured discontinuities are related directly to the SMART Grant incentives rather than other changes occurring (e.g. Pell Grant) at the discontinuity.

We interpret these patterns as reinforcing our previous result that the impact of SMART Grants were small or absent in early years but that the impact of the grant grew over time. There are several reasons this pattern could emerge but two seem most likely: first, students needed time to adjust their plans so that the first cohorts of students were less likely to adjust their major; and second, salience is likely to have increased throughout the life of the grant. This second point gives further merit to the hypothesis of increased salience at BYU due to a higher fraction of Pell-eligible students.

The difference in salience in early years between BYU and public Texas universities may also explain the heterogeneity of the impact of SMART Grants on degrees granted. The estimate for Texas degrees was estimated as zero while a moderately-sized, imprecise impact was measured in the BYU data. In Texas public universities, the impact of this grant in early years seems negligible or non existent, while the effect at BYU can been seen in the first years that the grant was available. If the grant had no impact on declared majors in Texas in its early years, we would not expect to see any impact on diplomas granted for these same students. Alternatively, since students responded earlier at BYU, some effect may be seen in the time-frame for which we have data.

Specific Majors Since SMART Grants gave incentives for several classes of major, there is also interest in decomposing the effect into the impact on each of these smaller classes. Of particular interest would be a decomposition into the impact on STEM majors and language majors. We do this by running separate regressions using a binary variable for the applicable subgroups. These results can be found in Table 2.6.

In Texas, we see that there was a 3.08% increase in junior STEM majors and 0.4% increase in junior language majors. The magnitudes are similar for senior majors as well. All of these measures sit very close to the 95% confidence level. This suggests that for junior major, the impact on STEM fields accounts for 87% of the total impact, and for senior majors, it accounts for 80%. The increase in language majors is notable because it is a 0.4 percentage point increase over a baseline of 0.7% for juniors and a 0.64 percentage point increase over a baseline of .9% for seniors. The results at BYU are too noisy to make any strong claims about the decomposition, but they again show that the bulk of the effect was in STEM fields. Ultimately it appears that while language majors are different from STEM majors in many ways, financial incentives increased the number of declared majors in both cases.

We hoped to measure which majors these new students were coming from by examining other classes of majors in a similar manner, but our results suffered from a lack of statistical precision, and no consistent patterns emerged.[25] While some of the regressions passed low levels of significance, none of them were strong enough to convincingly rule out significance purely due to multiple testing.

2.5.3 Contrast with Evans (2012) and Replication

As mentioned above, Evans (2012) also examines SMART Grants using data from Ohio universities of students who entered college in 2006-2007 and following them through spring 2010. Using a similar Regression Discontinuity design on Ohio data, he finds no evidence of an impact of SMART Grants on students majoring in STEM fields.

Several of our results lend insight into why our estimates differ. Primarily, Evans has less statistical power that we do and is trying to measure an impact that is smaller than what we are measuring. First, our analysis measures the impact of SMART Grants on all eligible majors while Evans only considered STEM majors. Since it appears that language majors make up about 20% of the effect, he is trying to measure a smaller value than we are. Second, Evans' sample is much smaller than ours since he restricts it only to students who enter college in 2006-2007 while we use all students who are juniors or seniors during the lifetime of the grant. That is, our analysis includes all students who Evans would include and also students who start earlier and progress more slowly to graduation or who start later and progress more quickly. Third, Evans doesn't include data for the last year that the SMART Grant program existed. Our analysis suggests that there was an increas-

[25] These results are available upon request.

ing impact of the program over time, meaning that Evans is trying to measure a smaller impact that what we measure in our analysis. Of course, it is possible that the measured difference is simply due to geographic heterogeneity, and the program had a larger impact in Texas than in Ohio. This would be consistent with the measured differences we observe between public universities in Texas and at BYU. However, the Texas and Ohio data both include primarily large public universities that are similar in observable characteristics, making it seem more likely that we would observe similar effects in each sample.

An additional relative strength of our data set is we observe grant receipt directly and can measure the size of the discontinuity in grant receipt. The final difference is that we are able to examine the years prior to the grant as a placebo test. This provides a valuable falsification test that allows us to attribute our estimates to the grant program as opposed to chance or effects from the Pell Grant program.

To test if Evans' data restrictions and outcome variable are sufficient to account for his lack of a measured impact, we restrict our sample in the same way, only including students entering college in 2006-2007 and removing all data after spring 2010, and measure the impact of SMART Grants on STEM majors. These replication results, which are found in Table 2.7, fail to be statistically significant much like Evans (2012), although the magnitude of the estimates are similar to those in our main analysis.[26] We believe that this is evidence that a significant

[26]We use the optimal bandwidth in the Texas data using the data restrictions described for the replication exercise because bandwidth selection is dependent on the data set used.

portion of the difference between Evans' and our estimates is purely due to the empirical issues described above. We can't measure, however, to what degree, if any, heterogeneity accounts for the remaining measured difference.

2.5.4 Heterogeneity/Robustness

There is significant interest nationally to increase the number of women and minorities in STEM fields. One might be interested, therefore, if SMART Grants had a differential impact on these groups. To test this, we run extended models that include interaction terms between the group we are examining (e.g. gender) and the slope and discontinuity terms. The coefficient associated with the interaction between the discontinuity variable and the group indicator would identify any between-group heterogeneity. Unfortunately, in each of these specifications, no significant differences could be identified. Given that our samples are only barely large enough to measure the main effect in many cases, this lack of result may simply be due to lack of power.

As a final robustness check, we test how sensitive our results are to the choice of bandwidth. We this by repeating our junior and senior declared major regressions with the Texas and BYU data, but with various bandwidths in a 500 EFC-unit neighborhood of the optimal one. We also include examine bandwidths that are 1,000 or 2,000 EFC units than the optimal bandwidths. The coefficients of these regressions and their 95% confidence intervals are plotted in Figure 2.10 and reported in Table 2.8. The figure shows that our estimates are quite stable for all bandwidths tested. Generally, the wider bandwidths produce slightly smaller estimates which we attribute to increasing bias associated with larger bandwidths. The ideal comparison in a regression discontinuity setting is the students just above and below the cutoff. As data further from the discontinuity is used, the modeled relationship between EFC and major choice becomes more reliant on students who are increasingly dissimilar in family income. As a result, estimates using data closer to the cutoff is likely to be less biased but less precise. As an additional check on the functional form of $f(x)$, we use quadratic in recentered EFC that is allowed to be different on each side of the threshold. These results are also presented in Table 2.8 and the results are qualitatively very similar to the local linear results presented before with the Texas estimates being slightly smaller and the BYU estimates being slightly larger.

There is still the question of whether the impact of SMART Grants operates primarily through persistence in SMART fields or through switching into SMART fields from ineligible fields. We examine this question by interacting the running variable and discontinuity with an indicator for being declared as a SMART major in a student's sophomore year. The results are presented in Table 2.9 where we detect no significant differences in the discontinuities for students declared in SMART majors as sophomores in any of these regressions. However, the point estimates suggest that if anything. the effects are concentrated among students who were declared in SMART majors as sophomores. Many students leave STEM majors as they advance through college and it appears that the SMART Grant may have partially mitigated this flow from STEM fields. Students already in SMART majors as sophomores would have another potential avenue for information about the grant. These students may have filed the FAFSA and found out about the existence of the grant because they were awarded it. Students who received the SMART Grant would then be able to alter their plan to switch from STEM but students who did not receive the grant would have no such incentives.[27]

2.6 Conclusion

This analysis of the SMART Grant Program provides evidence that students respond to direct financial incentives when choosing their major. This is the first evidence that a student's major can be influenced by targeted grants and represents an interesting policy intervention. The magnitude of the impact is of note given that the mean differences in earnings in eligible and ineligible fields are much larger than the $4,000 a year offered by the SMART Grant program. These relatively small financial incentives may have had an effect because students are credit constrained, uniformed about differences in earnings, or are myopic. Alternatively, it could be that these mean differences across fields do not reflect well the true counter-factuals in earnings for these students. This result, however, is consistent with the larger body of literature that finds that financial aid can have significant impacts on student outcomes.

These results also show that there can be a high level of heterogeneity in the impact over time and geographically. For instance, in Texas it appears that effect is too small to be measured in early years of the grant but that it grew to nearly 4

[27] The heterogeneity results are available from the authors upon request.

percentage points from a base-line of 18%. Some of this growth may be a result of early students being too invested in their pre-SMART Grant majors to switch as a result of the grant, but we believe that it was likely due to increasing salience of the program as students learned it existed. Similarly, BYU saw growth over time, but at much higher levels. Since BYU has a much higher fraction of students who qualify for Pell Grants and up to 5% of the students received SMART Grants at some point in college, it is likely that many student had heard about the grants from other students.

Our results also indicate that there is a differential impact across fields of study. While only 20% of the impact of the SMART Grant program was in language fields, the much lower baseline means that SMART Grants nearly doubled the fraction of students going into language fields. Our further analysis was unable to detect which majors these additional students in STEM and language came from.

We also attempt to decompose what fraction of our measured effect is due to students persisting in eligible fields versus switching to eligible fields. We are not well-powered enough to disentangle these two mechanisms, though our point estimates suggest that persistence may play a more significant role in this program than switching.

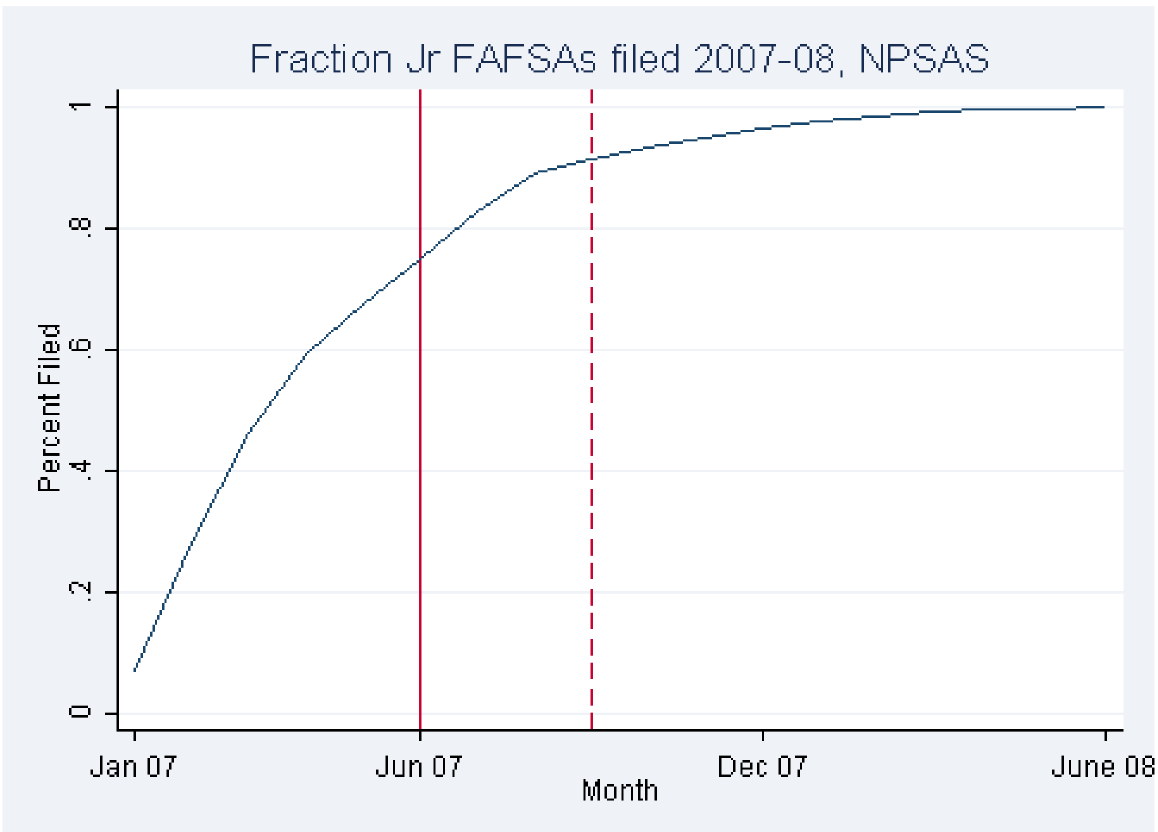
Because the grant was discontinued and because the students from the end of the program's life only had one year to complete their degree by the time our data concludes, it is difficult to determine if the number of SMART degrees awarded changed. In the future, we hope to extend this research by collecting more years of data, allowing us to measure the impact of the SMART Grant program on actual diplomas received and the number of people eventually entering STEM fields. We also hope to use the SMART Grant as an instrument to measure the impact of majoring in a STEM field on various employment outcomes such as employment, employment in a STEM field, and earnings.

Several lessons emerge from this program. First, policy makers can influence the choice of major using targeted financial incentives. Second, students choices among heterogeneous human capital investments are affected by factors outside of long term costs and benefits. Third, programs that target student major may need a longer time frame because juniors or seniors may be unlikely to adjust their decision about field of study. Lastly, salience plays a fundamental role in the success of these sorts of programs; unadvertised and unknown programs can be expensive and have little impact on outcomes of interest.

2.7 Figures & Tables

2.7.1 Figures

Figure 2.1: Timing of FAFSA Submission

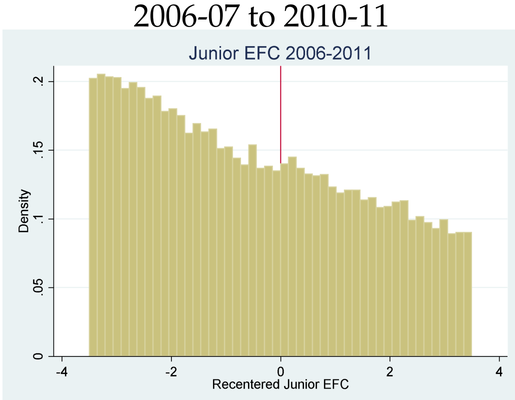


This figure represents the CDF of FAFSA filing for juniors in the 2007-08 NPSAS. The school year starts in August 2007 and is represented by the dashed line. The vertical line at June 07 is to highlight that the bulk of FAFSA submissions occurs several months prior to the school year starting.

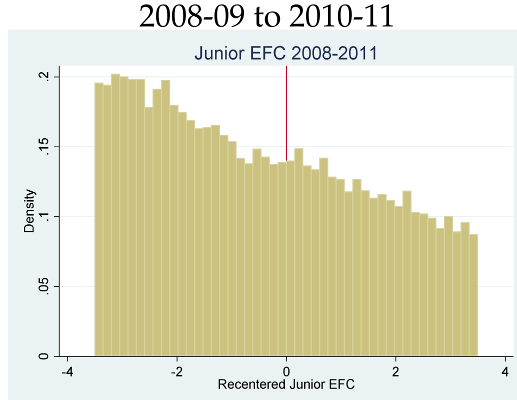
Figure 2.2: Density of Jr EFC

Texas

2006-07 to 2010-11

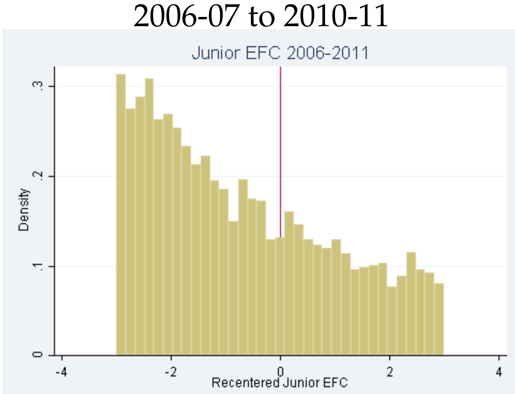


2008-09 to 2010-11

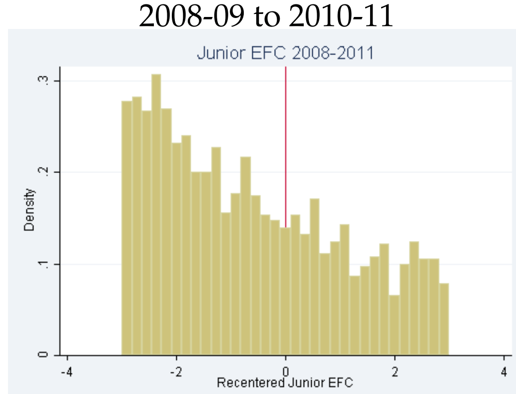


BYU

2006-07 to 2010-11



2008-09 to 2010-11



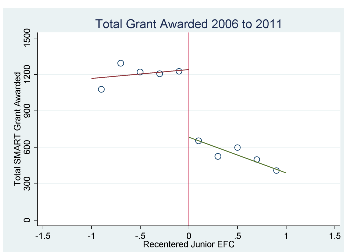
These figures depict the density of recentered EFC in the first semester a student is classified as a junior. EFC is recentered so that SMART eligibility occurs to the left of 0 and EFC is divided by 1,000.

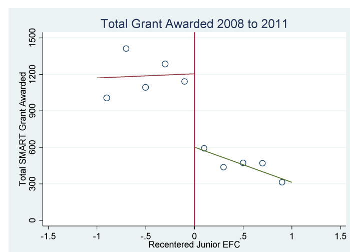
Figure 2.3: Total SMART Grant

Texas

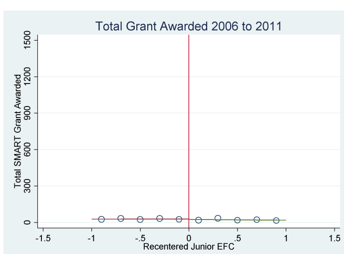
2006-07 to 2010-11 | 2008-09 to 2010-11

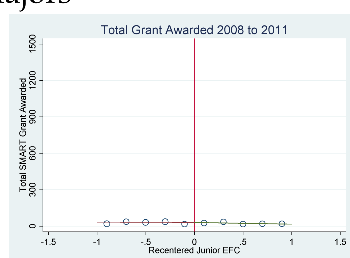
SMART Majors





Not SMART Majors

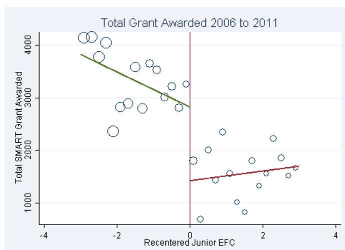


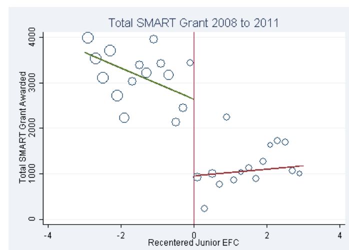


BYU

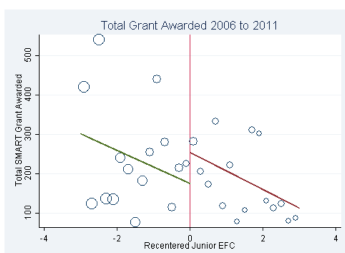
2006-07 to 2010-11 2008-09 to 2010-11

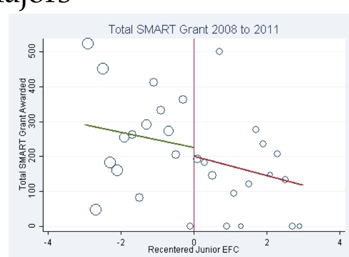
SMART Majors





Not SMART Majors





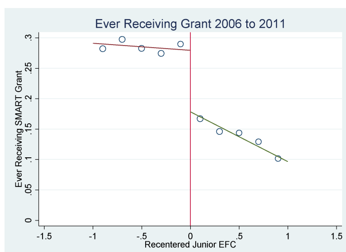
The average amount of the SMART Grants received is plotted against recentered junior EFC. Each dot represents the average for students in a bin of 200 EFC. EFC is recentered so that SMART eligibility occurs to the left of 0 and EFC is divided by 1,000. The size of the dot is proportional to the number of observations included in the average. The lines represent linear predictions allowed to vary on each side of the cutoff. The bandwidth used at Texas 1.0 and the bandwidth at BYU is 2.0.

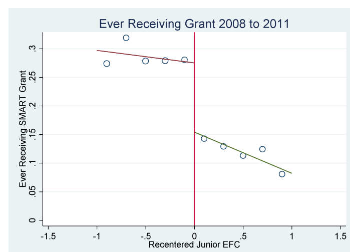
Figure 2.4: Ever Receive SMART Grant

Texas

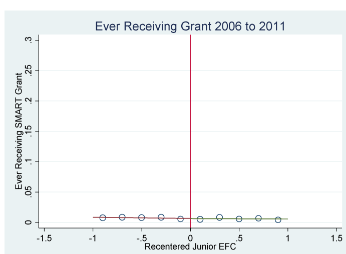
2006-07 to 2010-11 | 2008-09 to 2010-11

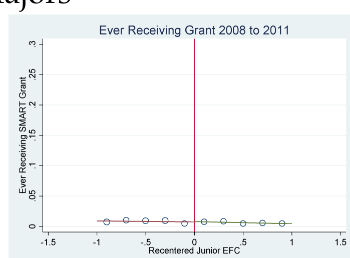
SMART Majors





Not SMART Majors

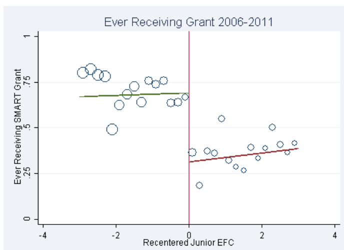


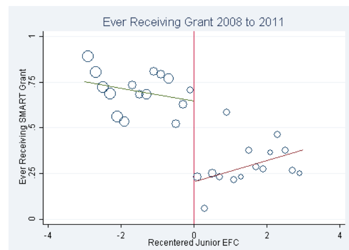


BYU

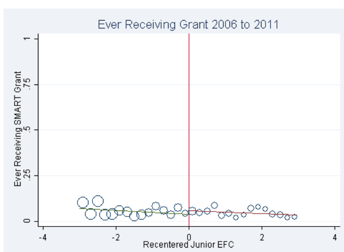
2006-07 to 2010-11 2008-09 to 2010-11

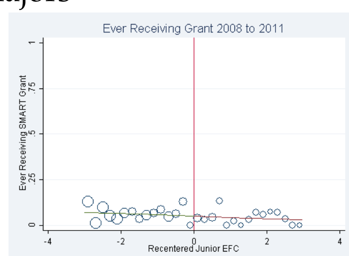
SMART Majors





Not SMART Majors



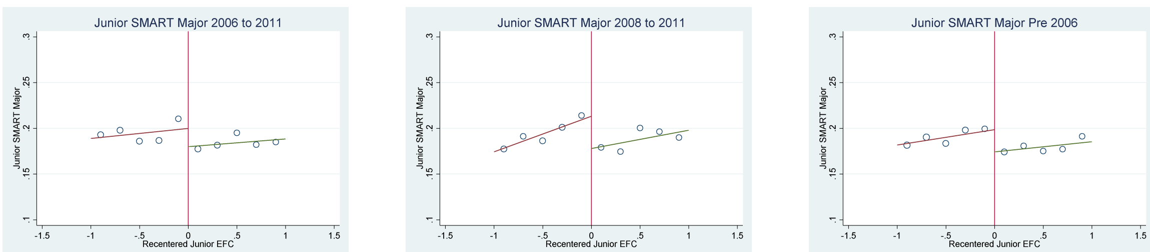


The probability of ever receiving a SMART Grant is plotted against recentered junior EFC. Each dot represents the average for students in a bin of 200 EFC. EFC is recentered so that SMART eligibility occurs to the left of 0 and EFC is divided by 1,000. The size of the dot is proportional to the number of observations included in the average. The lines represent linear predictions allowed to vary on each side of the cutoff. The bandwidth used at Texas 1.0 and the bandwidth at BYU is 3.6.

Figure 2.5: SMART Major in Jr Year

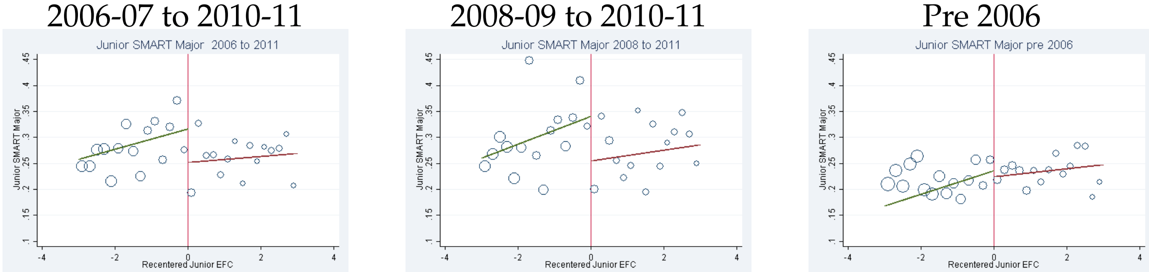
Texas

2006-07 to 2010-11 | 2008-09 to 2010-11 | Pre 2006



BYU

2006-07 to 2010-11 | 2008-09 to 2010-11 | Pre 2006

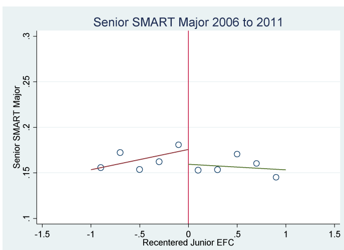


The probability of having a SMART major declared in the first semester of a student's junior year is plotted against recentered junior EFC. Each dot represents the average for students in a bin of 200 EFC. EFC is recentered so that SMART eligibility occurs to the left of 0 and EFC is divided by 1,000. The size of the dot is proportional to the number of observations included in the average. The lines represent linear predictions allowed to vary on each side of the cutoff. The bandwidth used at Texas 1.0 and the bandwidth at BYU is 2.0.

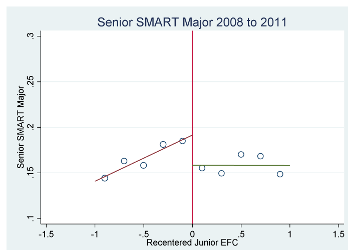
Figure 2.6: SMART Major in Sr Year

All Texas Schools

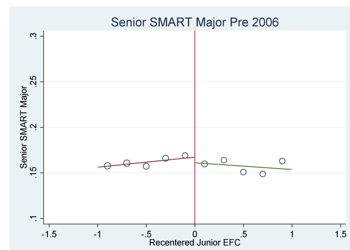
2006-07 to 2010-11



2008-09 to 2010-11

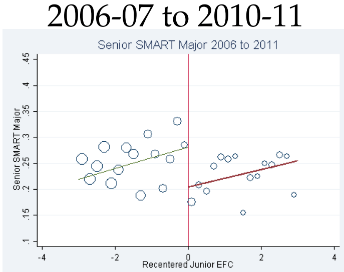


Pre 2006

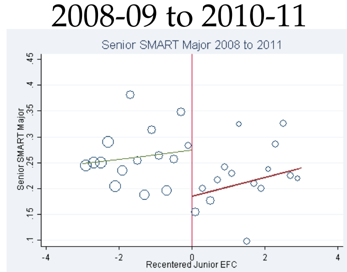


BYU

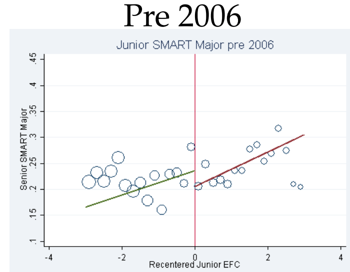
2006-07 to 2010-11



2008-09 to 2010-11



Pre 2006

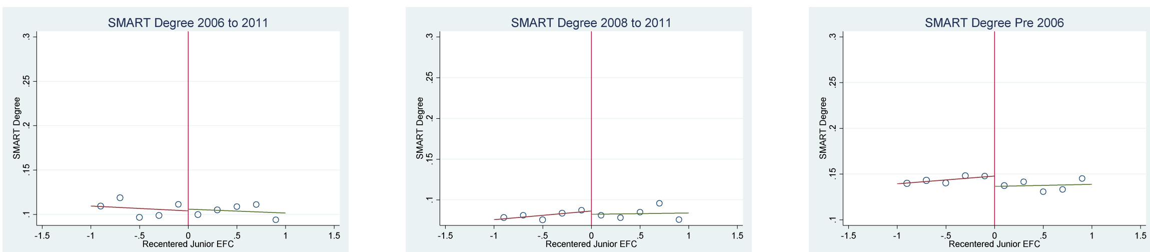


The probability of having a SMART major declared in the first semester of a student's senior year is plotted against recentered junior EFC. Each dot represents the average for students in a bin of 200 EFC. EFC is recentered so that SMART eligibility occurs to the left of 0 and EFC is divided by 1,000.The size of the dot is proportional to the number of observations included in the average. The lines represent linear predictions allowed to vary on each side of the cutoff. The bandwidth used at Texas 1.0 and the bandwidth at BYU is 2.0.

Figure 2.7: SMART Degree

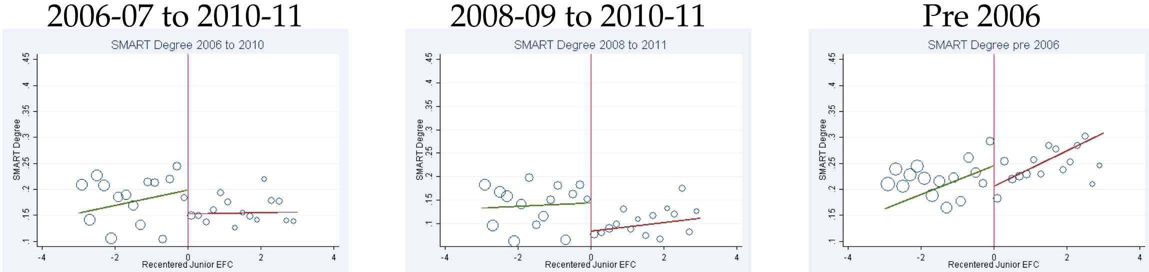
Texas

2006-07 to 2010-11 | 2008-09 to 2010-11 | Pre 2006



BYU

2006-07 to 2010-11 | 2008-09 to 2010-11 | Pre 2006

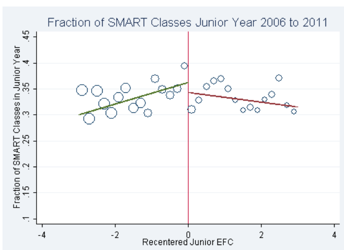


The probability of receiving a SMART degree is plotted against recentered junior EFC. Each dot represents the average for students in a bin of .2 scaled EFC. The size of the dot is proportional to the number of observations included in the average. The lines represent linear predictions allowed to vary on each side of the cutoff. The bandwidth used at Texas 1.0 and the bandwidth at BYU is 2.0.

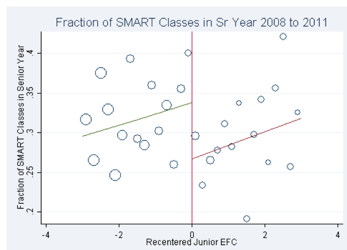
Figure 2.8: Fraction SMART Classes–BYU Only

Jr. Year

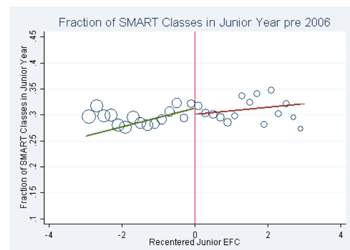
2006-07 to 2010-11



2008-09 to 2010-11

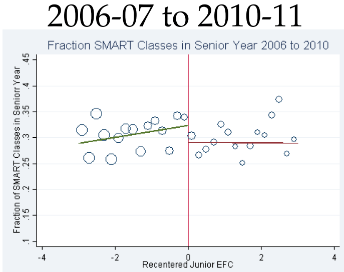


Pre 2006

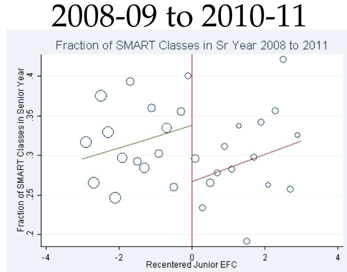


Sr. Year

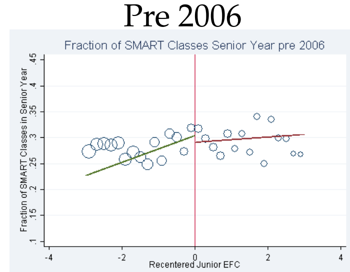
2006-07 to 2010-11



2008-09 to 2010-11



Pre 2006

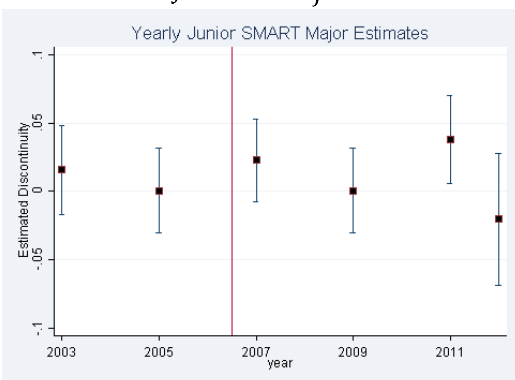


The fraction of classes taken in SMART fields is plotted against recentered junior EFC. Each dot represents the average for students in a bin of 200 EFC. EFC is recentered so that SMART eligibility occurs to the left of 0 and EFC is divided by 1,000. The size of the dot is proportional to the number of observations included in the average. The lines represent linear predictions allowed to vary on each side of the cutoff. The bandwidth used for juniors is 2.0 and the bandwidth for seniors is 1.8.

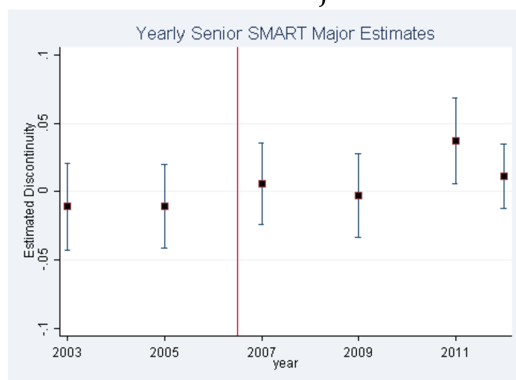
Figure 2.9: Estimates by Year

Texas

Junior Major

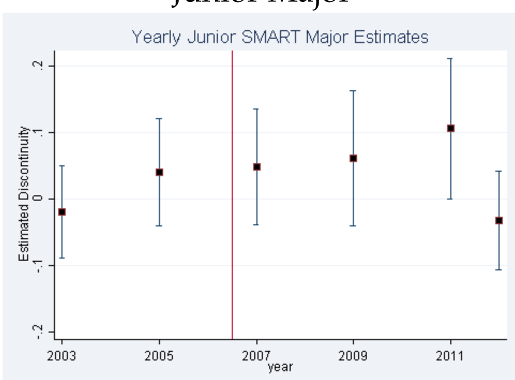


Senior Major

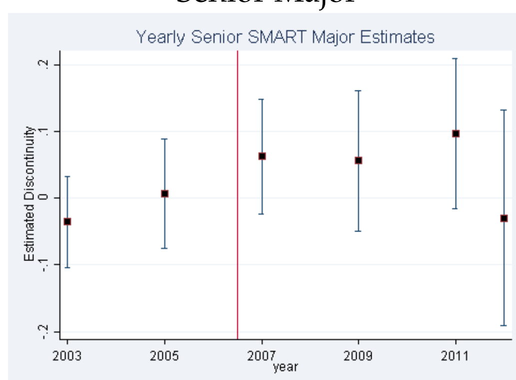


BYU

Junior Major



Senior Major

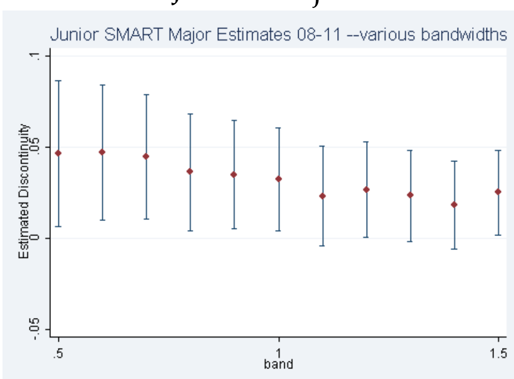


The estimated discontinuity for the impact of SMART Grants on majors is plotted along with 95% confidence intervals. The years represent the end of a school year and the preceding two school years (e.g. 2003 is the 2001-02 and 2002-03 school year). The exception is in 2012 which is only estimated using data from the 2011-12 school year A bandwidth of 1.1 is used for Texas and 2.5 is used for BYU.

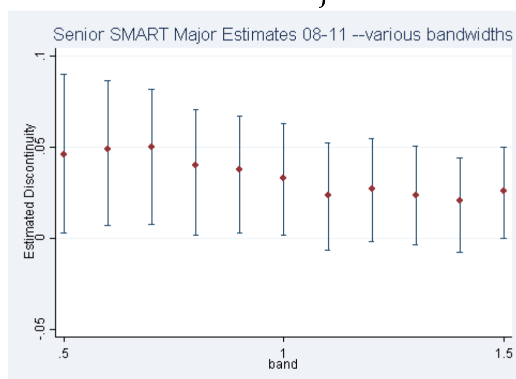
Figure 2.10: Various Bandwidths

Texas

Junior Major



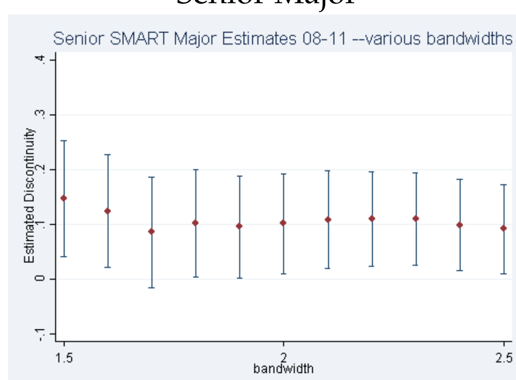
Senior Major



BYU

Junior Major

Senior Major



Estimates of the impact of SMART Grants on majors is plotted for various bandwidths. The bandwidths vary by +/-.5 around the optimal bandwidth. These estimates are for the 2008-09 to 2010-11 school years.

2.7.2 Tables

Table 2.1: Summary Statistics
Texas

	2000/01 - 2005/06		2006-07 to 2010-11	
	mean	sd	mean	sd
Junior Language Major	0.008	0.092	0.007	0.085
Junior Technical Major	0.179	0.383	0.185	0.388
Jr SMART Major	0.188	0.390	0.192	0.394
Senior Language Major	0.010	0.097	0.008	0.088
Senior Technical Major	0.153	0.360	0.154	0.361
Senior SMART Major	0.163	0.369	0.162	0.368
Technical Degree	0.134	0.341	0.099	0.299
Language Degree	0.010	0.101	0.008	0.088
SMART Degree	0.144	0.351	0.107	0.309
SMART Grant Amount	34.073	384.670	190.280	949.427
Ever Receive SMART Grant	0.009	0.094	0.047	0.212
Pell Eligible	0.579	0.494	0.552	0.497
Junior EFC	-0.221	1.149	-0.152	1.149
Male	0.404	0.491	0.428	0.495
Black	0.127	0.333	0.147	0.354
White	0.515	0.500	0.475	0.499
Asian	0.070	0.254	0.078	0.268
Missing Race	0.016	0.124	0.072	0.258
Hispanic	0.273	0.445	0.300	0.458
Mother Ed <= HS	0.533	0.499	0.515	0.500
Father Ed <= HS	0.501	0.500	0.517	0.500
Texas Resident	0.969	0.174	0.967	0.179
College Father	0.370	0.483	0.376	0.484
College Mother	0.374	0.484	0.416	0.493
Missing Mother Ed	0.093	0.291	0.070	0.254
Missing Father Ed	0.129	0.335	0.108	0.310
Hispanic	0.273	0.445	0.300	0.458
Observations	37754		45189	

Table 2.1: (cont.)
BYU

	2001-02 to 2005-06		2006-07 to 2010-11	
	mean	sd	mean	sd
Male	0.533	0.499	0.524	0.499
White	0.813	0.390	0.877	0.328
Black	0.007	0.083	0.006	0.078
Missing Race	0.025	0.157	0.017	0.130
Hispanic	0.040	0.196	0.048	0.214
Asian	0.017	0.129	0.024	0.154
ACT Score	25.892	3.382	26.036	3.501
HS Percentile	0.165	0.125	0.173	0.141
Missing ACT	0.113	0.317	0.100	0.300
Missing HS Percentile	0.297	0.457	0.223	0.416
Junior EFC	-0.861	1.634	-0.693	1.672
Pell Eligible	0.709	0.454	0.670	0.470
Frac. SMART classes Jr	0.299	0.267	0.333	0.264
Frac. SMART classes Sr	0.283	0.296	0.303	0.315
Total SMART Grant	217.109	992.427	923.318	2027.641
Ever Receive SMART	0.052	0.223	0.201	0.401
Jr SMART Major	0.224	0.417	0.271	0.445
Sr SMART Major	0.225	0.418	0.244	0.430
SMART Degree	0.226	0.418	0.174	0.379
Jr Tech. Major	0.203	0.402	0.245	0.430
Sr Tech. Major	0.203	0.402	0.217	0.412
Tech Degree	0.182	0.386	0.135	0.342
Jr Lang. Major	0.021	0.142	0.027	0.162
Sr Lang. Major	0.022	0.148	0.027	0.163
Lang. Degree	0.030	0.169	0.028	0.165
Observations	6994		3754	

Table 2.1: (cont.)

These summary statistics are produced from data provided by the Texas Higher Education Coordinating Board and Brigham Young University. Each observation represents a student with a valid EFC measurement in their junior year and the data is restricted to a window around the Pell/SMART Eligibility threshold of 2,000 EFC and 3,000 EFC at BYU.

Table 2.2: Covariate Checks
Texas

Covariates
2005/06-2010/11

	Male	Black	Asian	Hispanic	Missing Race	Missing Mot. Ed.	Missing Fat. Ed.
Discon.	0.00823	0.00473	0.00341	-0.00666	0.00456	-0.00187	0.0121
SE	(0.0136)	(0.00875)	(0.00714)	(0.0113)	(0.00713)	(0.00689)	(0.00849)
N	20594	20594	20594	20594	20594	20594	20594

	Mot. Ed <= HS	Fat. Ed. <= HS	Fat. College	Mot. College	Texas Res	Sch. Frac. SMART	Sch. Frac Pell
Discon.	-0.00544	-0.00203	-0.0101	0.00731	-0.00271	0.00293	-0.00445
SE	(0.0139)	(0.0139)	(0.0134)	(0.0137)	(0.00488)	(0.00251)	(0.00303)
N	20594	20594	20594	20594	20594	20571	20571

Table 2.2: (cont.)
Covariates
2008/09-2010/11

	Male	Black	Asian	Hispanic	Missing Race	Missing Mot. Ed.	Missing Fat. Ed.
Discon.	0.00369	-0.000658	0.00367	-0.0158	0.00869	-0.00478	0.00529
SE	(0.0177)	(0.0115)	(0.00948)	(0.0148)	(0.0111)	(0.00891)	(0.0108)
N	12242	12242	12242	12242	12242	12242	12242

	Mot. Ed <= HS	Fat. Ed. <= HS	Fat. College	Mot. College	Texas Res	Sch. Frac. SMART	Sch. Frac Pell
Discon.	-0.00414	-0.000514	-0.00478	0.00892	-0.00546	0.00121	-0.00535
SE	(0.0180)	(0.0180)	(0.0175)	(0.0178)	(0.00636)	(0.00328)	(0.00383)
N	12242	12242	12242	12242	12242	12219	12219

Table 2.2: (cont.)
BYU

Covariates
2006/07-2010/11

	ACT Score	Male	White	Black	Miss. HS. Pctile	Miss. ACT	Miss. Race
Discon.	0.305	-0.0236	0.0482*	-0.00676	-0.00970	-0.00509	0.0155
SE	(0.302)	(0.0425)	(0.0282)	(0.00702)	(0.0355)	(0.0253)	(0.0109)
N	2,332	2,332	2,332	2,332	2,332	2,332	2,332

	Hispanic	Asian	HS Pctile	SMART Frac. Cour.
Discon.	-0.0317*	-0.0161	-0.00118	-0.00309
SE	(0.0187)	(0.0128)	(0.0120)	(0.0188)
N	2,332	2,332	2,332	2,332

Table 2.2: (cont.)
Covariates
2008/09-2010/11

	ACT Score	Male	White	Black	Miss. HS. Pctile	Miss. ACT	Miss. Race
Discon.	0.326	-0.0527	0.0448	0.00268	0.0237	-0.00379	0.0307**
SE	(0.415)	(0.0566)	(0.0373)	(0.00941)	(0.0469)	(0.0322)	(0.0125)
N	1,297	1,297	1,297	1,297	1,297	1,297	1,297

	Hispanic	Asian	HS Pctile	SMART Frac. Cour.
Discon.	-0.0279	-0.0161	-0.00353	0.0299
SE	(0.0259)	(0.0153)	(0.0160)	(0.0254)
N	1,297	1,297	1,297	1,297

Each column represents the estimated discontinuity in covariates at the EFC threshold for SMART Grant eligibility. The discontinuity is estimated using local linear regression and uses the optimal bandwidth.

Table 2.3: SMART Grant Receipt

Texas	SMART Amount 2006-07 to 2010-11		SMART Amount 2008-09 to 2010-11	
Discontinuity	534.6***	2.589	589.0***	-3.746
	(97.96)	(8.129)	(120.2)	(10.76)
School FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
SMART Major	Yes	No	Yes	No
Observations	5780	24338	3491	14436

	Ever SMART 2006-07 to 2010-11		Ever SMART 2008-09 to 2010-11	
Discontinuity	0.121***	0.000709	0.129***	-0.00144
	(0.0285)	(0.00282)	(0.0357)	(0.00382)
School FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
SMART Major	Yes	No	Yes	No
Observations	3180	13709	1920	8122

Table 2.3: (cont.)

BYU

	SMART Amount 2006-07 to 2010-11		SMART Amount 2008-09 to 2010-11	
Discontinuity	1467.9***	-71.15	1772.8***	-2.988
	(313.1)	(76.74)	(381.3)	(102.8)
Covariates	Yes	Yes	Yes	Yes
SMART Major	Yes	No	Yes	No
Observations	1273	3458	750	1848

	Ever SMART 2006-07 to 2010-11		Ever SMART 2008-09 to 2010-11	
Discontinuity	0.418***	0.00296	0.542***	0.0164
	(0.0761)	(0.0217)	(0.0958)	(0.0302)
Covariates	Yes	Yes	Yes	Yes
SMART Major	Yes	No	Yes	No
Observations	654	1678	380	917

These regressions estimate the discontinuity in amount of SMART Grants disbursed or probability of ever receiving a SMART Grant as a result of the EFC eligibility discontinuity. The estimation is performed separately for SMART Majors and non SMART Majors.

Table 2.4: Effects on Major

Texas

	Junior Major			Senior Major		
	07-11	09-11	01-06	07-11	09-11	01-06
Discontinuity	0.0158	0.0327**	0.0173*	0.0133	0.0318**	0.0000240
	(0.0110)	(0.0143)	(0.0101)	(0.0104)	(0.0135)	(0.00954)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18746	11161	22004	18746	11161	22004

	SMART Degree		
	07-11	09-11	01-06
Discontinuity	-0.00211	0.00186	-0.00288
	(0.00812)	(0.00948)	(0.00983)
School FE	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Observations	22422	13347	21852

Table 2.4: (cont.)
BYU

	Junior Major			Senior Major		
	07-11	09-11	01-06	07-11	09-11	01-06
Discontinuity	0.0676*	0.102**	0.000395	0.0804**	0.101**	0.0182
	(0.0365)	(0.0493)	(0.0258)	(0.0350)	(0.0468)	(0.0258)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,332	1,297	4,219	2,332	1,297	4,219

	SMART Degree			Fraction SMART Jr Classes		
	07-11	09-11	01-06	07-11	09-11	01-06
Discontinuity	0.0479	0.0665*	0.0293	0.0207	0.0595*	0.00658
	(0.0312)	(0.0366)	(0.0259)	(0.0220)	(0.0307)	(0.0168)
Covariates	Yes	Yes	Yes			
Observations	2,332	1,297	4,219	2,332	1,297	4,219

	Fraction SMART Sr Classes		
	07-11	09-11	01-06
Discontinuity	0.0354	0.0774**	-0.000300
	(0.0262)	(0.0365)	(0.0188)
Observations	2,332	1,297	4,219

Table 2.4: (cont.)
Regression Discontinuity Difference

	Texas			BYU		
	(1)	(2)	(3)	(1)	(2)	(3)
	Jr Major	Sr Major	Degree	Jr Major	Sr Major	Degree
Later Discon.	0.0222	0.0409**	0.00399	0.0937*	0.0774	0.0342
	(0.0181)	(0.0171)	(0.0151)	(0.0526)	(0.0519)	(0.0497)
Discontinuity	0.00983	-0.00944	-0.0000911	0.000305	0.0187	0.0308
	(0.0112)	(0.0106)	(0.00936)	(0.0265)	(0.0261)	(0.0250)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	29320	29320	29320	5516	5516	5516

These tables represent the effect income eligibility for a SMART Grant in a student's junior year on declared major, degrees granted, or fraction of courses taken. Declared major is a 1 if declared in the relevant year and 0 otherwise (including not being observed graduating or as a senior). The years listed are the last years of the school year (e.g. 2007 is the 2006-07 school year. The regression discontinuity difference estimator compares the discontinuity in 2008-09 to 2010-11 to the discontinuity prior to 2006-07.

Table 2.5: Yearly Discontinuities

Texas

Jr. Major

	01	02-03	04-05	06-07	08-09	10-11	12
Discontinuity	-0.00874	0.0158	0.000612	0.0230	0.000625	0.0382**	-0.0203
	(0.0227)	(0.0166)	(0.0159)	(0.0155)	(0.0158)	(0.0164)	(0.0247)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4493	8193	9114	9309	9162	8571	4054

Sr. Major

	01	02-03	04-05	06-07	08-09	10-11	12
Discontinuity	-0.0198	-0.0110	-0.0108	0.00559	-0.00274	0.0372**	0.0114
	(0.0228)	(0.0164)	(0.0156)	(0.0153)	(0.0156)	(0.0161)	(0.0122)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4118	7515	8375	8577	8382	7874	3721

Table 2.5: (cont.)

BYU

	Sr. Major					
	02-03	04-05	06-07	08-09	10-11	12
Discontinuity	-0.0200	0.0397	0.0480	0.0611	0.105*	-0.0334
	(0.0351)	(0.0413)	(0.0443)	(0.0518)	(0.0536)	(0.0380)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,748	1,920	1,711	1,188	1,057	506

	Sr. Major					
	02-03	04-05	06-07	08-09	10-11	12
Discontinuity	-0.0357	0.00734	0.0627	0.0558	0.0963*	-0.0297
	(0.0350)	(0.0418)	(0.0438)	(0.0538)	(0.0574)	(0.0822)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,748	1,920	1,711	1,188	1,057	506

These tables estimate the discontinuity separately for different groups of years. The years listed are the last years of the school year (e.g. 2007 is the 2006-07 school year.)

Table 2.6: STEM and Language Outcomes

Texas

	Jr STEM	Jr Language	Sr STEM	Sr Language
Discontinuity				
	0.0308**	0.00456*	0.0268*	0.00636**
	(0.0147)	(0.00277)		(0.00317)
Covariates	Yes	Yes	Yes	Yes
Observations	11161	11161	11161	11161

BYU

	Jr STEM	Jr Language	Sr STEM	Sr Language
Discontinuity				
	0.105**	-0.0157	0.0645	0.0189
	(0.0528)	(0.0221)	(0.0138)	(0.00317)
Covariates	Yes	Yes	Yes	Yes
Observations	1,145	1,145	1,145	1,145

This table considers the effects separately STEM and Language majors. The years used in estimation are 2008-09 to 2010-11. The discontinuity at the SMART Grant EFC eligibility is presented.

Table 2.7: Evans Replication

	(1)	(2)	(3)	(4)	(5)
	STEM Major	SMART Amount	Ever SMART	SMART Maj. Sr.	SMART Degree
Discontinuity	0.0407	87.44	0.0263*	0.0407	0.0216
	(0.0274)	(54.80)	(0.0159)	(0.0267)	(0.0246)
Observations	3281	6736	3281	3281	3281

This table tries to replicate the sample conditions of Evans (2012) using the Texas data. Data from before the 2010 school year is used and only students who entered in 2006-07 or 2007-08 school year are included.

Table 2.8: Bandwidth Sensitivity

Texas

Junior Major

Bandwidth	0.5	0.6	0.7	0.8	0.9	1	1.1
Discontinuity	0.0464**	0.0470**	0.0447**	0.0362**	0.0350**	0.0324**	0.0230*
	(0.0205)	(0.0189)	(0.0175)	(0.0163)	(0.0153)	(0.0145)	(0.0140)
Observations	5641	6743	7802	8946	10030	11161	12242

Bandwidth	1.2	1.3	1.4	2	3
Discontinuity	0.0265**	0.0234*	0.0184	0.0132	0.0113
	(0.0134)	(0.0128)	(0.0123)	(0.0103)	(0.00834)
Observations	13347	14498	15645	22421	34162

Senior Major

Bandwidth	0.5	0.6	0.7	0.8	0.9	1	1.1
Discontinuity	0.0461**	0.0488**	0.0501***	0.0402**	0.0375**	0.0331**	0.0237
	(0.0222)	(0.0203)	(0.0189)	(0.0175)	(0.0165)	(0.0156)	(0.0150)
Observations	4884	5817	6719	7698	8607	9591	10505

Bandwidth	1.2	1.3	1.4	2	3
Discontinuity	0.0273*	0.0235*	0.0205	0.0129*	0.0136*
	(0.0144)	(0.0138)	(0.0132)	(0.00735)	(0.00780)
Observations	11457	12440	13430	37740	34162

Table 2.8: (cont.)

BYU

Junior Major

	1.5	1.6	1.7	1.8	1.9	2	2.1
Discontinuity	0.131**	0.132**	0.103*	0.113**	0.102**	0.102**	0.108**
	(0.0575)	(0.0552)	(0.0541)	(0.0528)	(0.0504)	(0.0493)	(0.0480)
Observations	958	1,026	1,082	1,145	1,236	1,297	1,363

	2.2	2.3	2.4	3	4
Discontinuity	0.112**	0.110**	0.105**	0.0971**	0.0550
	(0.0466)	(0.0455)	(0.0445)	(0.0394)	(0.0339)
Observations	1,448	1,524	1,604	2082	2972

Senior Major

	1.5	1.6	1.7	1.8	1.9	2	2.1
Discontinuity	0.147***	0.124**	0.0857*	0.102**	0.0950**	0.101**	0.109**
	(0.0546)	(0.0527)	(0.0517)	(0.0502)	(0.0479)	(0.0468)	(0.0457)
Observations	958	1,026	1,082	1,145	1,236	1,297	1,363

	2.2	2.3	2.4	3	4
Discontinuity	0.109**	0.110**	0.0983**	0.0884**	0.0702**
	(0.0442)	(0.0433)	(0.0424)	(0.0346)	(0.0325)
Observations	1,448	1,524	1,604	2519	2972

Table 2.8: (cont.)

Texas

	Jr Major–Quadratic			
	2.5	3	3.5	4
Discontinuity	0.0255*	0.0169	0.0110	0.0220**
	(0.0136)	(0.0125)	(0.0116)	(0.0109)
Observations	28351	34162	39933	46245

	Sr Major-Quadratic			
	2.5	3	3.5	4
Discontinuity	0.0269**	0.0187	0.0146	0.0216**
	(0.0128)	(0.0117)	(0.0108)	(0.0101)
Observations	28351	34162	39933	46245

	Degree Quadratic			
Discontinuity	0.000317	-0.00195	-0.00276	0.00611
	(0.00975)	(0.00892)	(0.00829)	(0.00779)
Observations	28351	34162	39933	46245

Table 2.8: (cont.)

BYU

	Jr Major–Quadratic			
	2.5	3	3.5	4
Discontinuity	0.153**	0.125**	0.129**	0.143***
	(0.0665)	(0.0600)	(0.0547)	(0.0512)
Observations	1685	2082	2519	2972

	Sr Major-Quadratic			
	2.5	3	3.5	4
Discontinuity	0.150**	0.126**	0.126**	0.136***
	(0.0634)	(0.0574)	(0.0525)	(0.0491)
Observations	1685	2082	2519	2972

	Degree Quadratic			
	2.5	3	3.5	4
Discontinuity	0.105**	0.0975**	0.103**	0.0807**
	(0.0499)	(0.0455)	(0.0418)	(0.0394)
Observations	1685	2082	2519	2972

This table estimates the discontinuity by varying the bandwidth and using a quadratic running variable allowed to vary on each side of the cutoff. Students from 2008-09 to 2010-11 are used in estimation.

Table 2.9: Heterogeneity By Sophomore Major

	Jr SMART	
	Texas	BYU
Soph. Non SMART	0.00392	-0.0127
	(0.00681)	(0.0383)
Soph. SMART Majors	0.0188	0.107
	(0.0262)	(0.0735)
Observations	17104	1771

	Jr SMART
Below Median Soph. SMART Classes	0.0179
	(0.0776)
Above Median Soph. SMART Classes	0.127
	(0.0978)
Observations	1297

This table examines heterogeneity by sophomore major or class taking. The running variable and discontinuity are allowed to vary by sophomore major or class taking. Students from 2008-09 to 2010-11 are used in estimation.

Chapter 3

All Grown Up? The Effects of Financial Aid on Enrolled Students

3.1 Introduction

Attending college can have large impacts on students' earnings as well as many other dimensions of students' life (Oreopoulos and Salvanes, 2011; Zimmerman, 2014; Hoekstra, 2009). Moreover, students who *complete* college have substantially higher wages than those who do not (Oreopoulos and Petronijevic, 2013). Because college attendance and completion can have such large impacts, understanding how students make decisions about college becomes critical. This paper explores explore how students who have already enrolled in college are affected by additional financial resources. I will consider how students' persistence, credits attempted, and graduation is affected by financial aid.

The price of college has been shown to affect student enrollment in numerous studies (Deming and Dynarski, 2009). The primary focus of these studies has been students deciding to enroll in college for the first time. However, far fewer studies have examined how financial aid made available to students while in college affects student decisions.[1] While enrolling in college is a key step in the pro-

[1] These studies will be discussed in detail in section 3.2.

cess of human capital acquisition, *completing* college predicts additional earnings gains over simply attending. College enrollment rates have grown since 1970 but college completion rates have stagnated (Turner, 2004). Financial aid could play a key role in student graduation and this paper will examine the role it plays for older students.

To examine the effect of financial aid for already-enrolled students, I examine changes in the dependent status of students. Financial independence from parents can induce large changes in federal financial aid such as Pell Grants and federal subsidized loans. This paper first documents the changes in financial aid that occurs with financial independence and then links changes in financial aid to changes in student outcomes. Financial independence will be shown to have heterogeneous impacts on student financial aid outcomes depending on the income of the students' family as well as the type of institution the student is attending. I leverage Texas administrative data from 2002-03 to 2013-14 to examine these effects separately for university and community college students.[2] I also consider heterogeneity by student family income as measured by Pell receipt in the prior year. Ultimately, I find that sizable changes in financial aid have small impacts on student outcomes in college.

Older students constitute a large fraction of the college going population and will be the focus of this study. In the nationally representative 2012 National Postsecondary Aid Survey, 51.3% of all undergraduate students were classified as

[2]Community college and four year students differ along many dimensions, including age and price responsiveness (Denning, 2015)

independents and 43.8% were 24 years or older (U.S. Department of Education, 2013). Not only are older students a large part of the college going population, they are an increasing share. In 1970 students 25 and older constituted 27.7% of all undergraduate enrollment and by 2010 they accounted for 42.6% (National Center for Education Statistics, 2014). Since 1990, over 40% of students enrolled as undergraduates were 25 years old or older. This study sheds new insight into this large group of older students and how the federal financial aid system affects them.

The primary changes observed in financial aid in this study will be on federal financial aid. Federal financial aid is substantial with over $171 billion disbursed in 2012-13 of which $33 billion was allocated to Pell Grants (CollegeBoard, 2014). Independent students made up nearly 60% of Pell Grant recipients in 2010-11. (Department of Education, 2013). Despite the majority of Pell recipients being independent, very little is known about the consequences of classifying students as independent or the effect of additional grant aid on already-enrolled students.

This study makes several contributions to the existing literature. It examines the effect of additional financial aid for existing students on outcomes such as persistence, credit taking, and graduation. Second, it considers the effect of need-based aid on student outcomes for older students who are increasingly important in higher education. Lastly, it is the first study of which I am aware to document the empirical changes in federal financial aid occurring when students become independent.

3.2 Conceptual Framework

Reducing the price of college is politically popular and has been considered in various forms.[3] One of the potential benefits of reducing the price of college is affecting students who are already enrolled but now are paying less to go to college. Reducing the price of college may affect student's number of credits attempted, the probability of reenrollment, and graduation among other things. This study will examine whether increased financial aid affects inframarginal students and their college decisions. Understanding the effect of financial aid on already-enrolled students is critical for understanding the entire effect of financial aid policy.

Persistence

Several studies have focused on the effect of price on initial enrollment in college but far fewer have examined the effect of grants on student persistence.[4] The studies that have considered reenrollment have found that additional grants increase reenrollment for some groups of students (Goldrick-Rab et al., 2011; Bettinger, 2004; Castleman and Long, 2013). These studies have focused on recent high school graduates or first year students while the present will focus on older students. The current study also examines a one time increase rather than changes

[3] Hansen (1983) and Kane (1995) examine changes in enrollment as a result of the implementation of the Pell grant but do not consider graduation or persistence. Both studies do not find any evidence that college enrollment increased as a result of the grant. In contrast, Seftor and Turner (2002) finds that older students did respond to the implementation of the Pell Grant by increasing enrollment. Other studies have examined the effect of grants on post-enrollment behavior but they often focus on merit-based grants with specific incentives (Scott-Clayton, 2011; Cornwell et al., 2005; Cohodes and Goodman, 2014; Denning and Turley, 2015).

[4] See Deming and Dynarski (2009) for a summary of the research on financial aid.

in financial aid that increase in every year the student is enrolled. In contrast to Bettinger (2004), this study also examines graduation outcomes and credits attempted to further understand the effects of need-based aid and the mechanisms through which it works.

Course Taking

Additional need-based grant aid may affect student course taking in several ways. Grants could act as a substitute for working during college and as such students might devote more time to their studies. Increased time focused on studies would allow students to progress through college more quickly. If grant aid displaces parental aid there is no clear impact on a student's decision to take courses. Grant aid may also decrease credit hours taken in a semester or increase time spent in college if there is consumption value to time in college (Jacob et al., 2013). Ultimately the impact on course taking is ambiguous. Overall, there is no strong prediction for the effect of grant aid on course taking.

Graduation

Students may respond to changing college price by adjusting the time it takes to graduate. Garibaldi et al. (2012) shows that at a university in Italy students speed up graduation when college is more expensive in the next year. The present study uses a similar increase in the cost of continuing college in the United States to examine reenrollment and graduation behavior. Though the present work considers changes in financial independence and financial aid as opposed to changes in tuition, both of these papers examine changes in the net cost of college.[5]

3.2.1 Financial Independence

Seftor and Turner (2002) examine the effect of financial independence on student enrollment and finds that financial independence increases student enrollment. The present study also studies financial independence and focuses on outcomes beyond enrollment and precisely measures both the change in aid and student outcomes as a result of detailed administrative data. Seftor and Turner (2002) use a differences-in-differences framework to examine the impact of the change the age at which students were classified as independent. The relevant policy changes were most likely to affect single heads of households and so they focus on single students with married heads of households as controls. They also focus on students who they predict would lose eligibility under the new rules. They find that decreased access to federal financial aid significantly decreased college enrollment of older students using CPS data. Their findings suggest that federal financial aid policy determining independence can have large effects. One potential shortcoming of their paper is that the result may be driven by different trends in enrollment for the groups they expect to be affected. Additionally, they use uses one change in policy which may lead to a biased estimate, particularly if other changes oc-

[5]Another approach that examines the impact of types of financial aid on outcomes has been to use hazard models to estimate the effect of type of student aid on persistence and graduation. these studies generally find that the type of student aid matters for persistence and graduation (DesJardins et al., 2002; Glocker, 2011).

cur contemporaneously. (Conley and Taber, 2011).[6] The present studies examines similar issues but uses a different source of variation–exact birth date.

3.3 Background

Financial Independence The federal government has several financial programs that are designed to help students pay for college. The first set of programs is administered by the U.S. Department of Education which I will refer to as "federal financial aid." The second is a part of the United States Tax Code and I will refer to it as "tax aid". I will discuss these programs in the following section and how financial independence impacts aid receipt from both sources.

3.3.1 Federal Financial Aid

Federal financial aid consists of the federal grants, student loans, and work study. The largest federal grant program is the Pell Grant which is targeted toward low income students. In the 2013 fiscal year the Pell Grant cost over $33 billion and provided aid to over 9 million students. Various federal student loans are also available to students and low-income students may take out loans at subsidized interest rates. In order to be eligible for need-based financial aid students must file a Free Application for Federal Student Aid (FAFSA).

The FAFSA uses information about student income and assets as well as family income and assets and demographic information (such as the number of

[6] Seftor and Turner (2002) also examine the effect on students ages 21 to 23 where the present study focuses on students ages 23 and 24.

siblings in college) to compute an Expected Family Contribution (EFC).[7] This EFC determines eligibility for need-based federal programs with lower values leading to eligibility for more grants and subsidized loans. In general, the federal financial aid awards are calculated yearly. If a life event occurs that would change a student's EFC, students can amend their FAFSA to reflect the new information and possibly change their eligibility for Pell Grants. During the period studied, students could receive the Pell Grant for up to 18 semesters.

Students must include parent information on their FAFSA as long as they are considered dependent. Undergraduate students may be classified as independents for several reasons including being over 24 years old as of January 1st of the school year, being married, having dependent children, or for a few other reasons.[8] When students are independent, parental financial information is not considered and student aid edibility increases as a result. All else equal, independent students qualify for larger grant awards than dependent students. Independent students also qualify for larger amounts of government loans on average.[9]

Independent status is determined once per year. Students who are 24 or older as of January 1st will be independent for the entire school year. Students who are 23 years and 364 days old and younger that meet the other conditions for dependent status will be declared dependent for the entire year. This discontinu-

[7] Bettinger et al. (2012) examine the complexity of the FAFSA's effect on student filing and enrollment

[8] See http://studentaid.ed.gov/fafsa/filling-out/dependency for all conditions that determine independent status.

[9] Parents may longer be qualified for certain loans like parent PLUS loans–a more complete examination of these loans will be considered in future drafts.

ity creates a situation where students are very similar in age but are treated very differently in terms of their eligibility for federal financial aid.

3.3.2 Tax Aid

The United States Tax Code gives special treatment to dependent children. Children can be claimed as dependents as long as they are younger than 19 at the end of the year. If a child is a full time student they may be claimed as a dependent from if they are younger than 24 at the end of the year and meet certain conditions. Those conditions are that the child must be a full time student for at least 5 months in a year, they must live with their parents for at least 6 months of the year, and must receive more than half of their financial support from their parents. If these conditions are met parents may claim their student children as dependents and receive exemptions and tax credits that reduce taxable income. Additionally, dependent students may qualify the taxpayer for tax credits like the American Opportunity Credit, the Lifetime Learning Credit, and the Earned Income Tax Credit. During the time period studied the Hope Tax Credit and Tuition Deduction could also be used. For an extremely thorough treatment of the effect of tax credits on college enrollment see Bulman and Hoxby (2015) who conclude that tax credits for college have essentially no effect on student enrollment patterns.

Tax aid will change at the same January 1st threshold for some students. In particular, students who are living at home for at least 6 months and providing less than half of their support, tax filing status is likely to have changed for students turning 24 before January 1st. The number of enrolled students living with their parents during the school year is available using the ACS from 2005-2011. In Texas, 42.16% of students in the cohort that turns 24 during the school year are living with their parents. This number is an upper bound on the number of students affected by the change in tax status as some of those who live at home may receive less than half of their support from their parents. Unfortunately the ACS does not have information whether students are attending a four year or two year college. However, the 2007-2008 National Post Secondary Aid Survey (NPSAS) contains information about residence with parents for students while they are enrolled. Students at four year schools in Texas who are from 24 to 24.3 on January 1st live with their parents 15.9% of the time while two year students are more than twice as likely with 35.7% living with their parents while enrolled (U.S. Department of Education, 2013). The fraction of students with a change in tax status is no more than 42% is likely to be smaller at four year schools than at two year schools.

If a student is declared independent, all else equal, a the parent's tax liability will increase as they no longer can claim a dependent exemption or any of the education tax credits. If parents were eligible for the Earned Income Tax Credit, as the number of eligible children will be reduced. The student will have their personal tax liability decrease as they will be able to use the education tax credits on their tax return instead of parents using the education tax credits. In general the family's total tax liability will increase as credits and/or deductions are shifted from parents with relatively high marginal tax rates to students with relatively low marginal tax rates.[10]

[10]For very high income families who are not eligible for education tax credits, the total tax burden

Financial independence is associated with fewer family resources but increased student resources. How this affects total resources toward college depends on how parents and older students split changes in wealth from marginal tax changes. I am not aware of any studies that examine how families split such tax changes and data on within-family transfers would be required to answer the question. Tax aid is never "disbursed" per se and households may differ in the timing of realizing tax benefits. Tax aid is generally realized at the time of tax filing/tax returns which happens at some time in the second half of the school year. Tax aid is likely to be of lesser impact than financial aid because it is disbursed after many of the costs of attending college occur.

Overall, tax credits for college will have changed for a minority of students enrolled. Moreover, tax credits for college have been shown to not have any effect on enrollment in Bulman and Hoxby (2015). For these reasons, the reduced form effect of financial independence is likely to be largely driven by changes in federal financial aid rather than changes in tax aid. This is particularly true at four year schools where students are not living with their parents as often. For this reason, interpreting the effects of financial independence in this paper will focus on the effects on federal financial aid. Future work will carefully identify which students were most likely to be affected by changes in tax aid and will estimate the effect of financial independence separately for these students.

may decrease.

3.4 Data

The data for this project comes from the Texas Higher Education Coordinating Board and contain the universe of students who were enrolled in public colleges and universities in the state of Texas from 2002-03 to 2012-13. The data contain demographic information about the students including race, gender, and birth date. They also contain records on student enrollment and credits attempted. Importantly, all financial aid disbursed by the university is also contained in the data. Additionally, many of the fields from the Free Application for Student Aid (FAFSA) are available including including dependency status and in later years, parent and student Adjusted Gross Income.

The data is restricted to students with a valid Social Security Number (for matching purposes) and information is matched across the various files and years to create the variables of interest. All variables created deal with a academic year which starts in the fall of one year and extends through the end of summer of the next. The following variables are created:

Enrolled Next Fall (Community College/4yr) A dichotomous variable indicating that students appear in the data enrolled at a community college or university in the next year

Graduated In X Years Indicates that a student received a degree this year or the next.

Credits Attempted The total number of credit hours attempted in the school year.

Loans Includes all federal loans the student takes out.

Table 3.1 contains summary statistics. The sample is split by whether the 23 year old student was attending a university or community college (with students attending both omitted) and by whether the student received a Pell Grant in the year they turned 23. I will consider the effect of financial independence separately for these four groups of students. Community college and four year students will be considered separately because the experiences of these students are quite different.[11] Students who received Pell Grants in the year they turned 23 will be considered separately from students who did not receive Pell Grants. Students who did not receive a Pell Grant in the year they were 23 experienced a substantially larger change in the amount of grants received as a result of financial independence. This heterogeneity in the impact of financial independence yields insights into the effect of increased grants on educational outcomes.

Some notable features of the data are apparent in the summary statistics. Pell students are more likely to be racial minorities than non Pell students. Additionally, community college students are more likely to be racial minorities than university students. Approximately 50% of students reenroll in the next year. Also students who receive a Pell Grant as 23 year olds have higher amounts of grants and loans in the next year.

[11] See Kane and Rouse (1999) for an overview of community colleges.

3.5 Identification

As previously discussed, students who are 23 years old at the end of the year are counted as dependent for the entire previous year if they meet other conditions. However, students who are 24 at the end of the year are dependent for the prior year. This rule means that students who are nearly identical in age are treated differently for financial aid and tax purposes for an entire year. I leverage the discrete nature of the change in classification to examine the effect of additional financial aid arising from being declared independent on student outcomes using a regression discontinuity framework.

The sample will consist of students who are attending a public college or university in Texas in the year that they turn 23. The outcomes considered will be in the next year and include reenrollment, graduation, credits attempted, and financial aid. Outcomes in the next year will be considered because students in the year they turn 23 will not have experienced different financial aid or tax treatment as a result of their age. However, in the year students turn 24, there may be enrollment responses to financial independence and so the sample may be changing in response to differential incentives.

The estimating equation becomes:

$$Y_{it} = f(\widetilde{age_{it}}) + \theta \cdot 1(Ind > 0) + X_{it} + \mu_t + \epsilon_{it},\ for\ |\widetilde{age_i}| < j \tag{3.1}$$

Where i indexes students and t indexes school year. Y_{its} is a student outcome like enrollment, credits attempted, or graduation, $f(\widetilde{age_{it}})$ is a flexible function of a student's age as of January 1st, θ is the parameter of interest and is the effect of the additional financial aid arising from students being declared financially independent in the next year. X_{it} contains control variables like race and gender and μ_t are year fixed effects. Finally, ϵ_{it} is an idiosyncratic error term. This equation is estimated on a subset of the data to compare students who are similar ages and in the preferred, local linear specification j is chosen using the procedure outlined in Imbens and Kalyanaraman (2012). This equation will be estimated separately for students at community colleges and universities and by whether they received a Pell Grant during the year they turned 23.

Assumptions for Identification

In any regression discontinuity estimation several assumptions are made in order to assure that the estimates obtained reflect the effect of treatment. The first is that the running variable, in this case birth date, cannot be manipulated to gain access to treatment. Obviously a student's true birth date is not manipulable by the student before or after birth. Students do have incentives to misreport their birth date to gain additional dollars but the reported birth date is verified by comparison with Social Security Administration records. In this sense, birth date is an ideal running variable because it is not determined by the student and is not misreported.

However, there is evidence that birth dates are manipulated in response to tax incentives (Schulkind and Shapiro, 2014; LaLumia et al., ming). These studies found that there is a small amount of manipulation in response to tax incentives that is less than half the amount of re-timing of births that is typically seen on a weekend. A $1,000 change in taxes leads to about 1% of births being re-timed.[12] This may be a concern for identification if children of parents who re-time their births in response to tax incentives produce children who systematically respond differently to financial independence 24 years later. It is not obvious how these students would differ systematically but it is a possibility.

To explore the amount of re-timing of births that occurs Panels A and B of Figures 3.1, 3.3, 3.5, and 3.7 plot the number of students with each birthday among students turning 23 in a given school year. The panels on the left (A) include all students and there are additional students born just before January 1st as has been documented elsewhere. Panels on the right remove students who were born within four days of January 1st and the distribution is much more smooth through the cutoff. There is some re-timing evident for university students who are not receiving a Pell grant around Christmas but otherwise the distributions appear to be smooth after omitting 4 days on either side of January 1st.

Because the manipulation is likely to be small and is done for reasons unrelated to gaining access to independent status, the preferred specification uses students close to the cutoff. However, as a robustness check a regression discontinuity donut estimator is used where born within four days of January 1st are excluded. This excludes students whose births were re-timed (up to 4 days) for tax purposes. These results are qualitatively and quantitatively very similar and are available upon request.

[12] Schulkind and Shapiro (2014) find that the manipulation is due to increased cesarean rates before January 1.

Another assumption of the regression discontinuity estimator is that students on one side of the cutoff are similar to students on the other side in observable and unobservable ways.[13] I test for observable differences by looking for discontinuities in predetermined characteristics like race and gender to test the plausibility of this assumption. Results from these checks for balance of the covariates are found in Panel A of Tables 3.2, 3.3, 3.4, and 3.5. In these regressions there are 24 discontinuities considered and no estimates are statistically significant at the 5% level. However, there are two estimates for students who received Pell in the year they turn 23 that are significant at the 10% level. This number of marginally statistically significant results is roughly what would be expected by chance given that 24 coefficients are being considered. Overall, there is strong evidence student characteristics are not discretely changing at the threshold for eligibility.

Given that students are unable to manipulate their date of birth and that observed covariates do not vary discretely by eligibility status, the testable assumptions of the regression discontinuity estimator are met and the following results can be interpreted as causal.

[13]There may be unobserved variables that also differ on each side of the age cutoff. One example that may be relevant is insurance coverage. In the state of Texas during this employers were required to cover dependent children on health for health insurance until age 25 so insurance status is not likely to vary discretely at this threshold (Dillender, 2014). For 2011 and 2012, the Affordable Care Act mandated that all children under the age of 26 be eligible for inclusion on their parent's plans which would not affect the identification strategy of this paper.

3.6 Results

The results will proceed by first characterizing the effect of financial independence on student financial aid received. As previously mentioned, the effect of financial independence depends on the student's family characteristics as well as the institution attended. For this reason the results will be considered separately for four groups. The first division compares university to community college students. The second compares students who received a Pell Grant in the year they turned 23 to students who did not. The effects on financial aid are quite different for these groups of students. The effects on educational outcomes will then be discussed and linked to changes in financial aid. Ultimately all four groups will a similar story of very small impacts of financial independence on student outcomes.

3.6.1 University Students, No Pell when 23

The regression results for students at four year institutions who did not receive a Pell Grant appear in Table 3.2 which corresponds to Figures 3.1 and 3.2. Panel A of Table 3.2 shows that observed student covariates do not vary discretely at the cutoff. Panel B characterizes the change in financial outcomes for students. Each of the columns is a different financial outcome in the next year and the estimates represent the discontinuity in the outcome for 23 year old students. Figure 3.1 presents some of these results visually. Students who enroll in the next year and are declared financially independent receive \$837 additional dollars in Pell Grants and \$934 dollars in all grants. If enrollment in the next year is related to additional grant money, then these estimates are likely to be biased upwards. Reenrollment rates will be shown to be insensitive to the change in dependent status and so these estimates are likely to be minimally biased. For a more cautious estimate, students who do not enroll in the next year are included in the estimation and the discontinuities are slightly smaller at $419 and $469 respectively. Federal student loans also increase by $313 among enrolled students and $177 if all students are included. These results suggest that there is a large discontinuity in grant aid of nearly $1,000 for students who enroll in the next year.

Student outcomes are considered in Panel C of the Table and students are no more likely to reenroll in the next year. The point estimate for reenrollment at universities is .4 percentage points and the top of the 95% confidence interval is 1.3 percentage points. These results suggest that an additional $1,000 does not have large reenrollment effects for this group of students.

Student graduation may be affected by additional grant money. Students who expect to pay relatively less in the next year may decide to wait an additional year to graduate as in Garibaldi et al. (2012). This is explored in columns 6 where graduation this year is considered. If the additional money is expected and slows graduation plans, then eligible students would be less likely to graduate in the current year. The point estimate is very small at -.2 percentage points and statistically indistinguishable from zero. Students do not seem to respond to changes in prices for the next school year by adjusting their graduation. This may be because they are not anticipated or because graduation is insensitive to price changes. Similarly there is no effect on graduation in the next year as seen in column 7. Students in this year actually do receive additional grant money of nearly $1,000 for enrolled students but the increased financial aid does not change the graduation probability.

The last student outcome to be considered is the total number of credits attempted in the next year. This is in column 1 and is shown to increase by .28 credits. Because students have not changed their enrollment probability, this increase represents a change in credits attempted for inframarginal students. An additional $1,000 of aid increases credits attempted by .6. This modest increase in credits attempted is the only educational outcome that seems to be affected for students who experience a change in dependent status.

Several lessons emerge from university students who did not receive a Pell Grant when 23. Financial independence and the associated increases in grants ($976) and federal loans ($313) does not affect reenrollment or graduation probabilities. If financial independence is not well understood than students may not change reenrollment rates or graduation in the year they turn 23. However, even after enrolled students actually receive the additional financial support, there is no change in graduation probabilities and a very small change in the number of credits attempted. Unconditional grant money and additional federal student loans do not seem to effect reenrollment or graduation and have small effects on credits attempted for this group of students.

3.6.2 University Students, Pell when 23

University students who received a Pell Grant when 23 are considered in Table 3.3 and Figures 3.3 and 3.4. University students who received a Pell Grant when 23 receive larger amounts of grant aid in the next year than students who did not receive Pell Grants when 23. This is to be expected as financial need across school years is very persistent. However, the measured discontinuity for independent students is significantly smaller than for students who did not receive Pell Grants. This is likely because Pell Grant students' parents have lower incomes and so excluding them from the calculation of federal financial aid does not have as large an impact. Among enrolled students Pell Grants increase $259 at the age threshold, federal loans increase by $749 and all grants increase by $312. If students who do not enroll are included those estimates are reduced by roughly half. Despite a lower discontinuity in grant aid, these students' discontinuity in loans is twice as large as students who did not receive a Pell Grant at age 23. This suggests that poorer students have higher demand for federal loans than richer students. Unfortunately the data do not contain information on non federal loans and so it is not clear if this is displacing private loans or if it represents new borrowing.

Panel C of Table 3.3 examines educational outcomes. The general finding is that enrollment in the next year is unaffected by the additional grants and loans. Credits attempted is also not affected. However, graduation in the year that the students receive the additional grants and loans they are 1.6 percentage points more likely to graduate. This is somewhat puzzling because there is no change in credits attempted. However, the additional grants and federal loans could help with student performance in classes and thus help students receive credits and graduate.

3.6.3 Community College Students, No Pell When 23

Community college students who did not receive a Pell Grant when 23 are considered in Table 3.4 and in Figures 3.5 and 3.6. Panel B of Table 3.4 estimates the changes in financial aid received. The discontinuity for students who enroll in the next year is $288 in Pell Grants, $329 in all grants, and $156 in loans. The discontinuities for community college students are significantly smaller than for university students[14]. When considering unconditional aid to avoid issues with differential enrollment, the the amounts are reduced by about one third.

Panel C shows that educational outcomes were largely unaffected by the change in financial aid. There estimates of the discontinuities in reenrollment and graduation are very close to zero and are not statistically different. There is an increase in the number of credits attempted with .369 more credits attempted as a result of financial independence. This happens despite there being no change in enrollment suggesting that the additional money induced additional classes attempted on the intensive margin rather than the extensive margin. With the (tenuous) assumption that all of the change in class taking is due to increased grants, these estimates imply a 1.82 credit hour increase for a $1,000 increase in grants. This is about three times as high as the similar estimates for students who did not receive a Pell Grant when 23 years old at universities.

[14] This could be due to lower rates of FAFSA completion

3.6.4 Community College Students, Pell when 23

Community college students who received a Pell Grant when 23 years old are considered in Table 3.5 and in Figures 3.7 and 3.8. Panel B documents the change in financial aid in the next year and finds that there were largely no changes in financial aid received. The exception is that there may have been modest increases in federal loans accrued though this estimate is marginally statistically significant. Unsurprisingly, there are also no changes in educational outcomes as seen in Panel C where graduation, enrollment, and credits attempted are insensitive to changes in financial dependency status.

Community college students who received a Pell Grant at age 23 may not have had any discontinuity because their parents may not have made enough money to factor into grant calculations. Whatever the reasons, it is reassuring that there is no change in student outcomes when there is no change in financial outcomes. In some ways the estimates for this group of students can be seen as a placebo test to explore if their are inherent changes in educational outcomes for students across this birth date threshold.

3.7 Conclusion

Financial independence increases financial aid to students. These increases can be quite large. For four year students not receiving a Pell Grant in the previous year the change is substantial with an increase in grants of nearly $1000. Despite this sharp change in financial aid, educational outcomes do not seem to be affected.

For students who did not receive a Pell grant in the year they turn 23, financial independence increased credit taking by small amounts at both community colleges and universities. Interestingly, community college students responded to smaller changes in financial aid with larger increases in credit taking. These results taken together suggest that grant money is not likely to affect graduation or credit taking for higher income students.

For poorer students, the change in financial aid associated with financial independence is smaller than for richer students. Students who had previously received Pell Grants at universities do increase the federal loans they take out in addition and see increases in grant money. These changes lead to faster graduation. This suggests that additional financial aid for low-income, older students students may increase graduation rates.

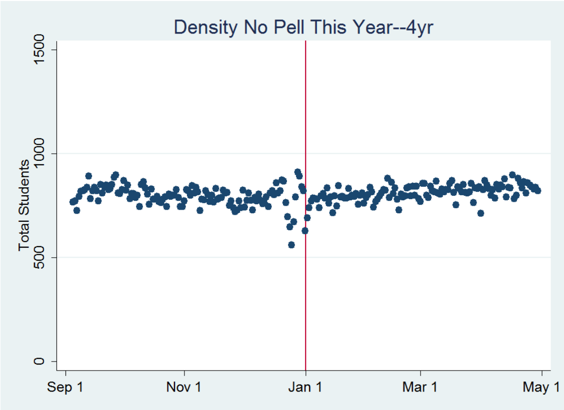
Taking all the evidence together, financial independence increases financial aid received–particularly for wealthier students. This has modest impacts on credits attempted for wealthier students. However, for poorer students, financial independence has a smaller change on grants but increases federal loan utilization and can affect graduation rates for students.

Given the modest impacts on educational outcomes policymakers may want to reconsider how financial aid for students over 24 is determined. There are large changes in financial aid without accompanying large changes in student outcomes. The results of this study suggest that raising the age of financial independence would not affect student reenrollment and would also not change credits attempted or graduation substantially.

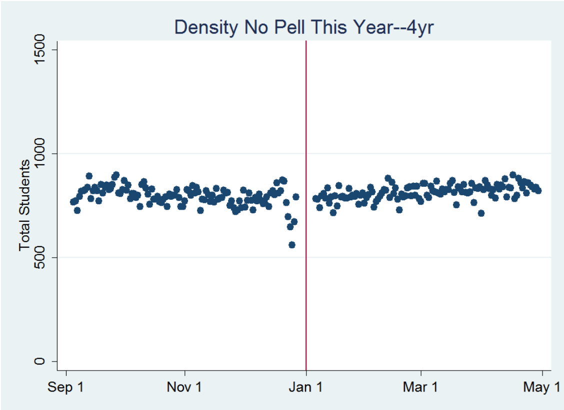
3.8 Figures and Tables

Figure 3.1: University Students–No Pell This Year, Density and Financial

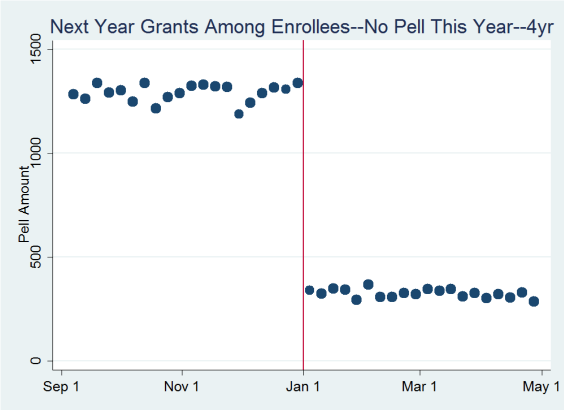
(a) Density



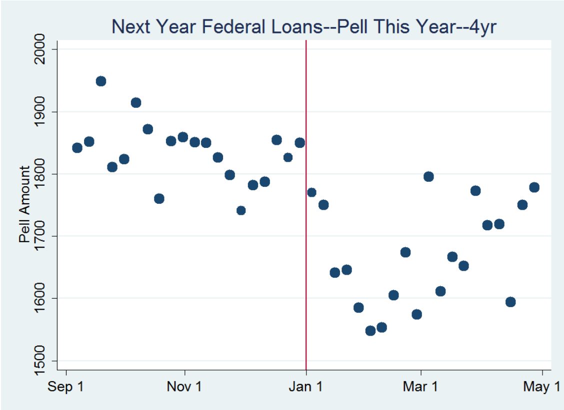
(b) Density Donut



(c) Total Grants Among Enrolled



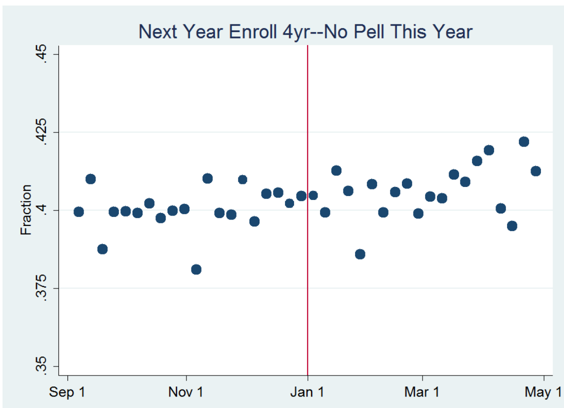
(d) Federal Loans



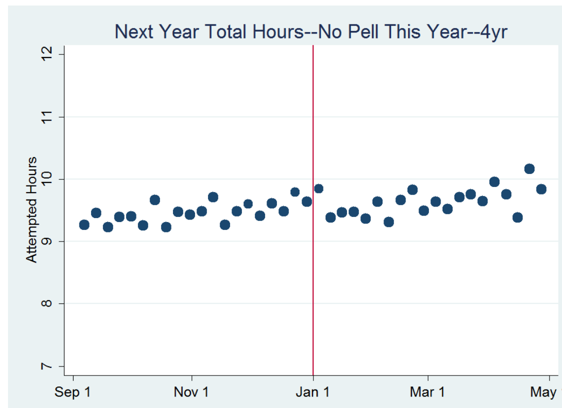
Panels A and B plot the number of students in the sample born on each day of the year. Panel B excludes students born within 4 days of January 1st. Panel C presents the total grants received by students in the next year among students who enrolled. Panel D presents the amount of federal loans taken out by the students with students not enrolling being coded as zero. In panels C and D each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Figure 3.2: University Students–No Pell This Year, Educational Outcomes

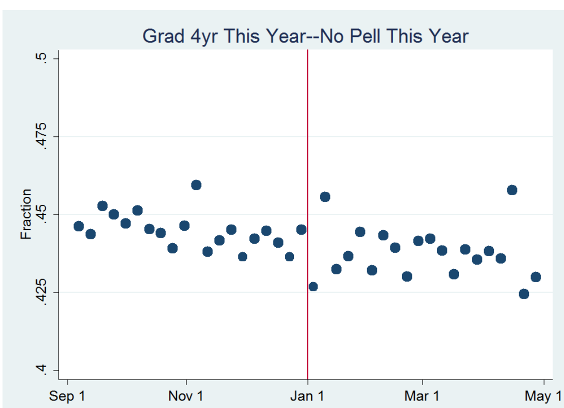
(a) reenroll



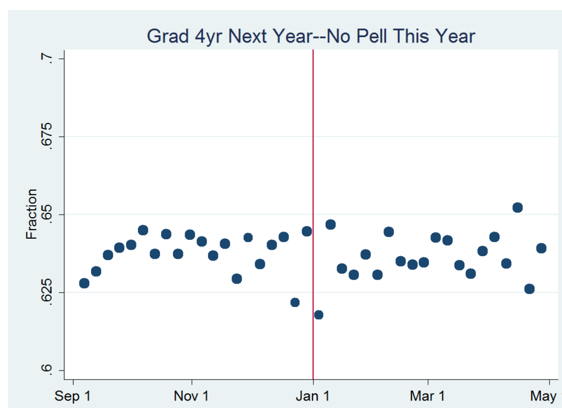
(b) Hours Attempted



(c) Graduate This Year, 4yr



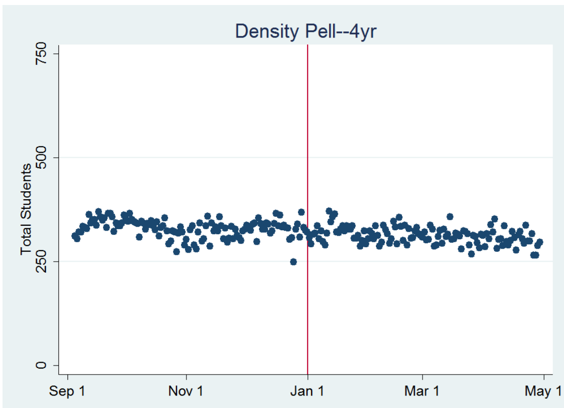
(d) Graduate Next Year, 4yr



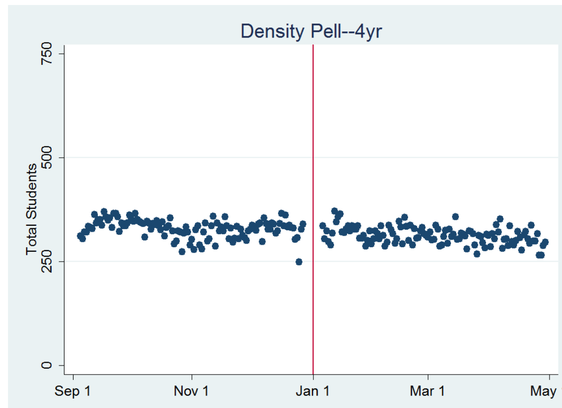
These panels present student educational outcomes by birth date. Each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Figure 3.3: University Students–Pell This Year, Density and Financial

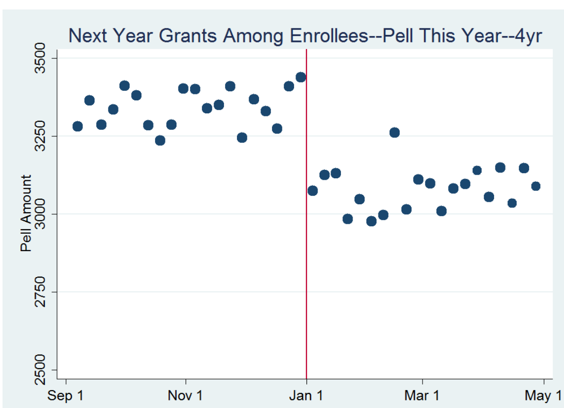
(a) Density



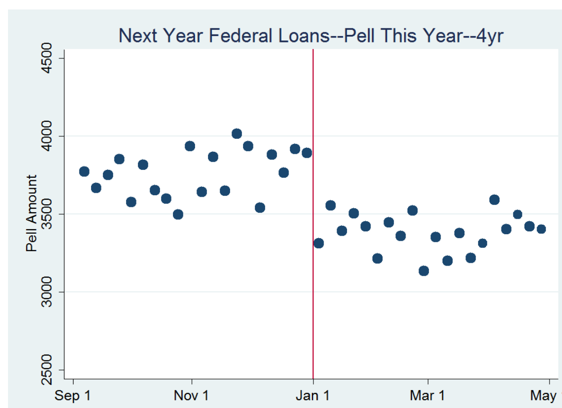
(b) Density Donut



(c) Total Grants Among Enrolled



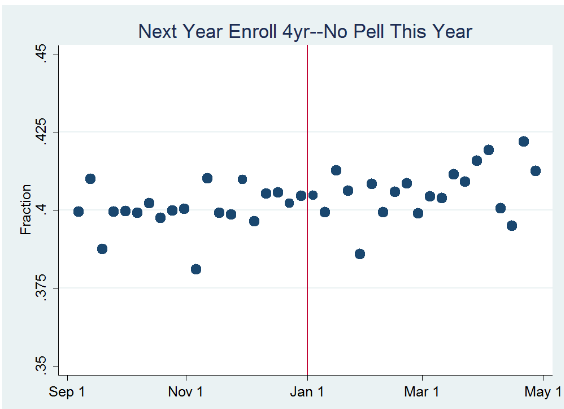
(d) Federal Loans



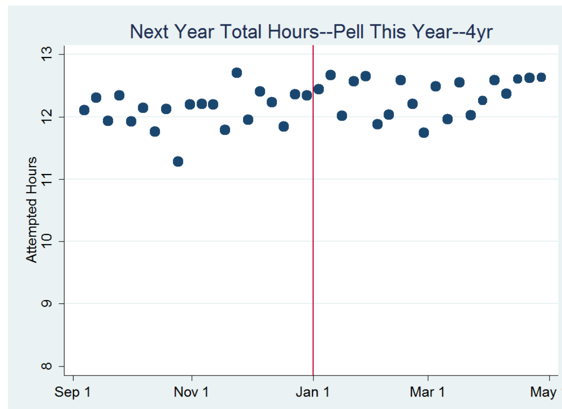
Panels A and B plot the number of students in the sample born on each day of the year. Panel B excludes students born within 4 days of January 1st. Panel C presents the total grants received by students in the next year among students who enrolled. Panel D presents the amount of federal loans taken out by the students with students not enrolling being coded as zero. In panels C and D each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Figure 3.4: University Students-Pell This Year, Educational Outcomes

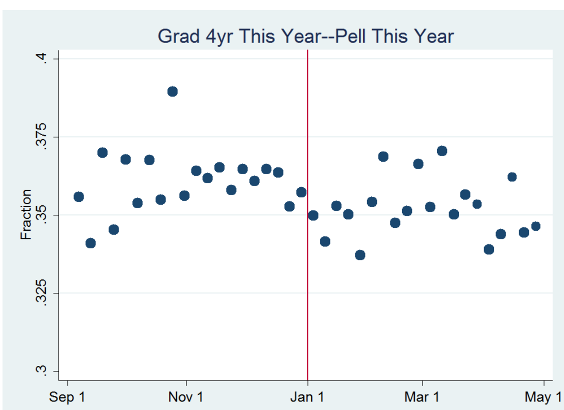
(a) reenroll, 4yr $\rightarrow$ 4yr



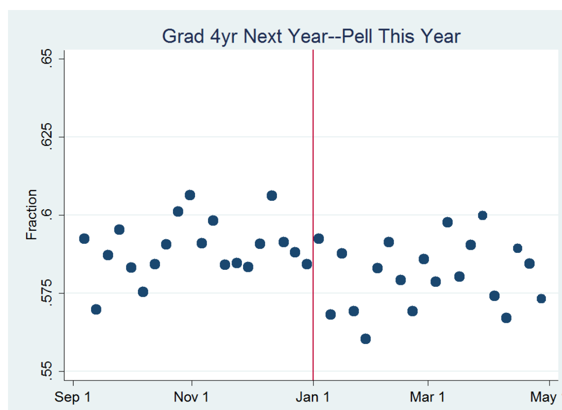
(b) Hours Attempted



(c) Graduate This Year, 4yr



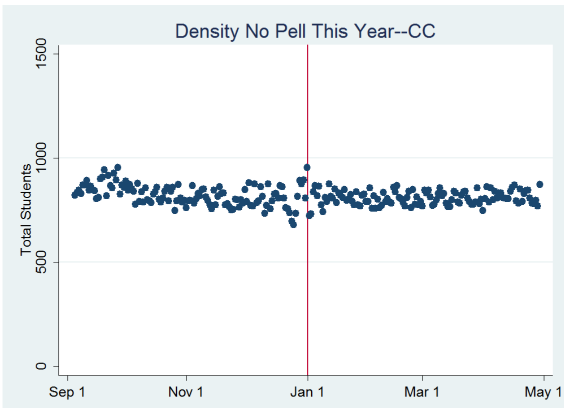
(d) Graduate Next Year, 4yr



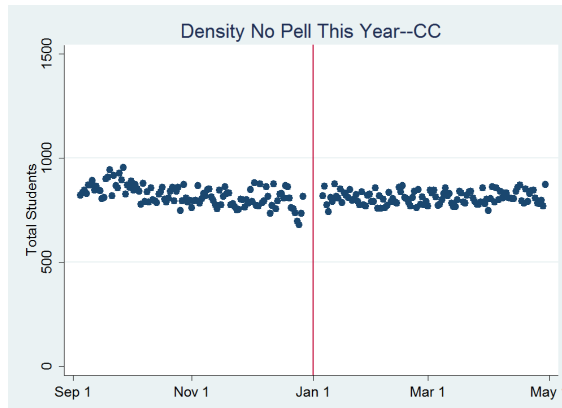
These panels present student educational outcomes by birth date. Each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Figure 3.5: Community College Students–No Pell This Year, Density and Financial

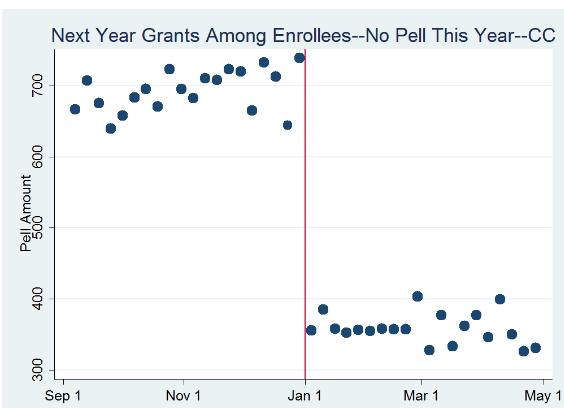
(a) Density



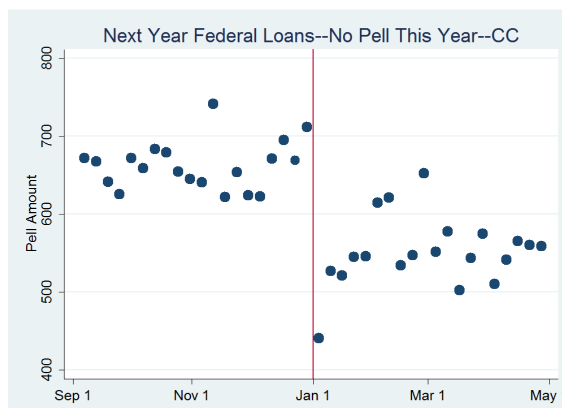
(b) Density Donut



(c) Total Grants Among Enrolled



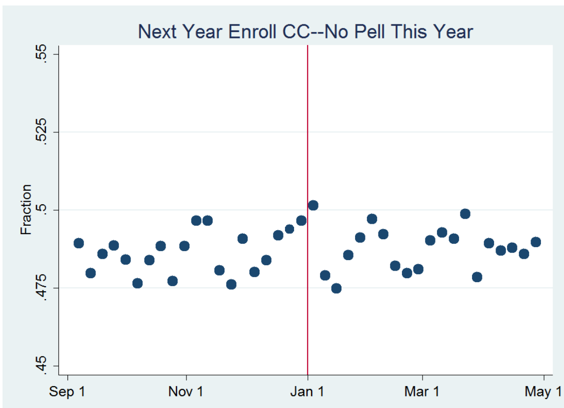
(d) Federal Loans



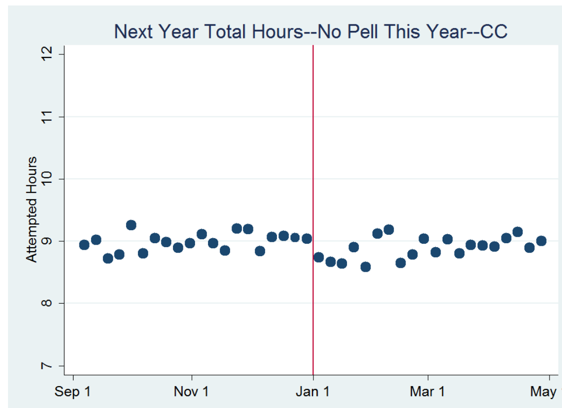
Panels A and B plot the number of students in the sample born on each day of the year. Panel B excludes students born within 4 days of January 1st. Panel C presents the total grants received by students in the next year among students who enrolled. Panel D presents the amount of federal loans taken out by the students with students not enrolling being coded as zero. In panels C and D each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Figure 3.6: Community College Students–No Pell This Year, Educational Outcomes

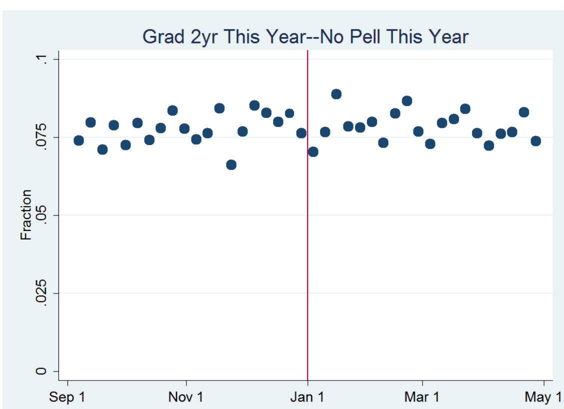
(a) Reenroll



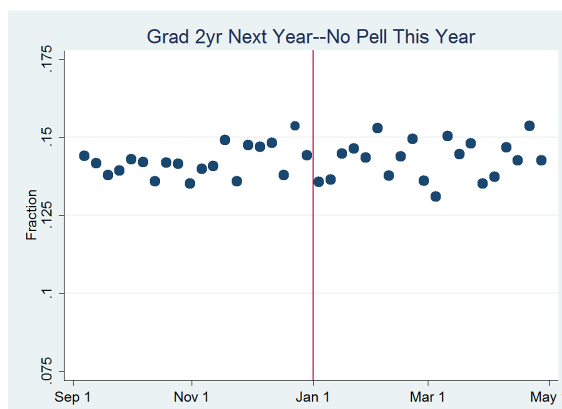
(b) Hours Attempted



(c) Graduate This Year, 2yr



(d) Graduate Next Year, 2yr



These panels present student educational outcomes by birth date. Each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

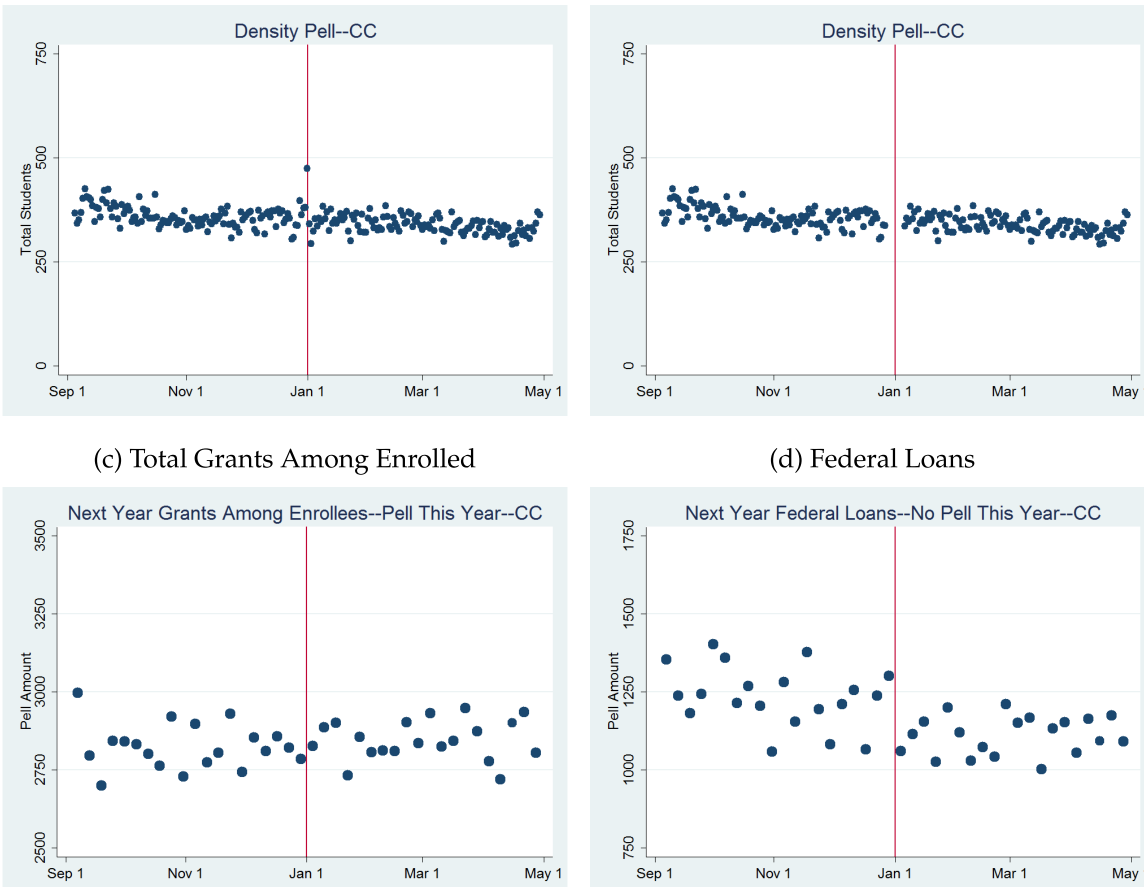
Figure 3.7: Community College Students–Pell This Year, Density and Financial

(a) Density

(b) Density Donut

(c) Total Grants Among Enrolled

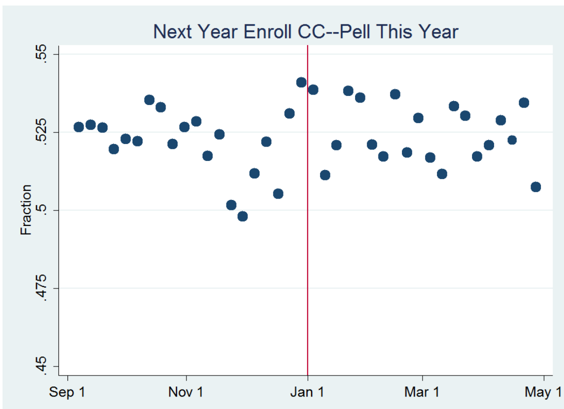
(d) Federal Loans



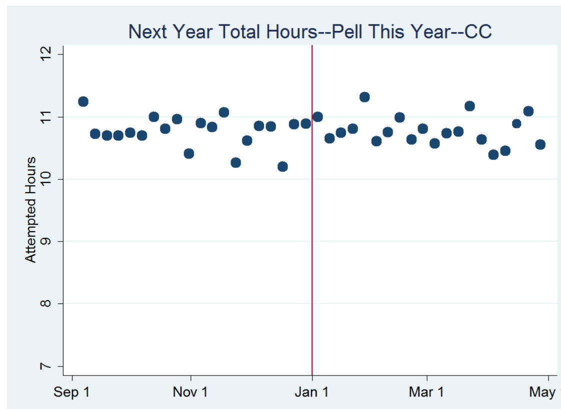
Panels A and B plot the number of students in the sample born on each day of the year. Panel B excludes students born within 4 days of January 1st. Panel C presents the total grants received by students in the next year among students who enrolled. Panel D presents the amount of federal loans taken out by the students with students not enrolling being coded as zero. In panels C and D each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Figure 3.8: Community College Students–Pell This Year, Educational Outcomes

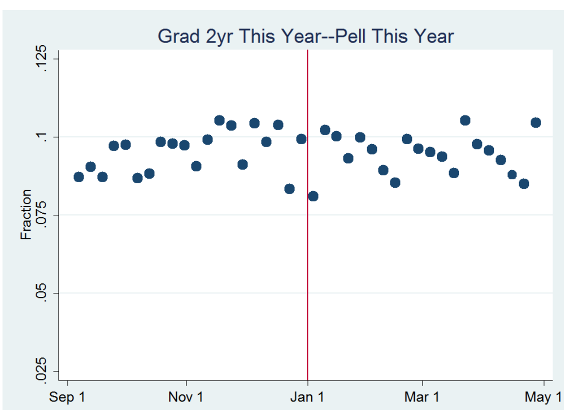
(a) Reenroll



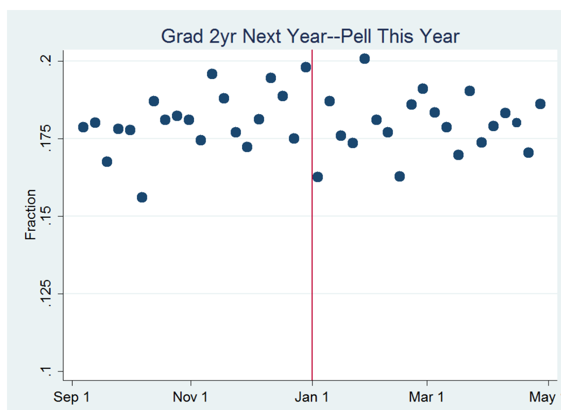
(b) Hours Attempted



(c) Graduate This Year, 2yr



(d) Graduate Next Year, 2yr



These panels present student educational outcomes by birth date. Each dot represents the average financial aid for students in a six day bin and the size of the dot is proportional to the number of students used to compute the average.

Table 3.1: Summary Stats, by Enrollment/Pell Receipt in Year Turning 23

	No Pell, CC			No Pell, 4yr		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Pell	194994	0.00	0.00	192009	0.00	0.00
White	194994	0.35	0.48	192009	0.47	0.50
Black	194994	0.09	0.28	192009	0.07	0.25
Asian	194994	0.03	0.18	192009	0.04	0.19
Hispanic	194994	0.22	0.41	192009	0.13	0.33
Male	194994	0.49	0.50	192009	0.52	0.50
Next Year Enr CC Only	194994	0.49	0.50	192009	0.02	0.15
Next Year Enr 4yr Only	194994	0.04	0.20	192009	0.40	0.49
Next Year Enr Both	194994	0.04	0.20	192009	0.03	0.18
Next Year Pell (Unc.)	194994	259.48	965.56	192009	304.33	1055.40
Next Year Tot. Grants (Unc.)	194994	308.54	1106.38	192009	397.68	1280.05
Next Year Hours Att. (Unc.)	194994	8.94	10.63	192009	9.55	12.54
Next Year Loans (Unc.)	194994	607.19	2652.88	192009	1751.85	4405.42
Next Year Loans (Enr)	114280	1036.05	3400.61	95841	3509.67	5719.48
Next Year Pell (Enr.)	114280	442.75	1228.68	95841	609.69	1430.16
Next Year Total Grants (Enr.)	114280	526.45	1404.96	95841	796.71	1721.84

Table 3.1: (cont.)

	Pell, CC			Pell, 4yr		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Pell	83506	1.00	0.00	76708	1.00	0.00
White	83506	0.20	0.40	76708	0.20	0.40
Black	83506	0.19	0.39	76708	0.21	0.41
Asian	83506	0.02	0.14	76708	0.05	0.21
Hispanic	83506	0.28	0.45	76708	0.25	0.43
Male	83506	0.32	0.47	76708	0.43	0.49
Next Year Enr CC Only	83506	0.52	0.50	76708	0.02	0.16
Next Year Enr 4yr Only	83506	0.04	0.20	76708	0.50	0.50
Next Year Enr Both	83506	0.04	0.19	76708	0.04	0.20
Next Year Pell (Unc.)	83506	1621.56	1989.56	76708	1597.56	2047.19
Next Year Tot. Grants (Unc.)	83506	1788.29	2232.61	76708	2001.42	2610.06
Next Year Hours Att. (Unc.)	83506	10.77	11.35	76708	12.23	13.13
Next Year Loans (Unc.)	83506	1172.95	2834.51	76708	3574.35	5107.01
Next Year Loans (Enr)	52696	1858.74	3384.87	47755	5741.42	5427.00
Next Year Pell (Enr.)	52696	2569.65	1958.68	47755	2566.13	2060.69
Next Year Total Grants (Enr.)	52696	2833.85	2221.69	47755	3214.85	2653.61

This table presents summary statistics for the data split by student pell receipt in the year they turn 23 and institution type. Data come from the Texas Higher Education Coordinating Board and include students from 2002-03 to 2012-13.

Table 3.2: University Students, No Pell This Year

A. Covar.	(1) Male	(2) White	(3) Black	(4) Hispanic	(5) Asian	(6) Cal. Year	(7)
Disc.	-0.00761	0.00294	0.000853	-0.000228	0.000661	0.0188	
	(0.00506)	(0.00505)	(0.00259)	(0.00340)	(0.00195)	(0.0319)	
N.	158926	158926	158926	158926	158926	158926	

B. Fin. Outcomes	Pell (Enr.)	Loans (Enr.)	All Grants (Enr.)	Loans (Uncond.)	Pell (Uncond.)	All Grants (Uncond.)	
Disc.	837.0***	313.1***	934.0***	176.8***	419.4***	469.2***	
	(17.60)	(73.81)	(21.39)	(40.41)	(9.476)	(11.54)	
N	95841	95841	95841	192009	192009	192009	

C. Educ. Outcomes	Next Tot Hours	Enr CC	Enr 4yr	Grad CC in 0y	Grad CC in 1y	Grad 4yr in 0y	Grad 4yr in 1y
Disc.	0.283**	0.000301	0.00451	-0.000428	-0.000107	-0.00209	0.000494
	(0.115)	(0.00138)	(0.00451)	(0.000739)	(0.00110)	(0.00522)	(0.00505)
N	192009	192009	192009	111603	111603	143841	143841

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

This table considers outcomes for students who were enrolled in a university in the year they turned 23. Panel A checks for balance of student characterisitics across the threshold. Panel B checks for discontinuities in financial aid received by students in the next year. Some of the estimates in Panel B are conditional on enrolling in the next year (Enr.) and others are unconditional and have zeroes for students who did not enroll. Panel C checks for changes in educational outcomes.

Table 3.3: University Students, Pell This Year

A. Covar.	(1) Male	(2) White	(3) Black	(4) Hispanic	(5) Asian	(6) Cal. Year	(7)
Disc.	0.00999	-0.00162	0.0000615	-0.00278	-0.00140	0.0869*	
	(0.00713)	(0.00578)	(0.00584)	(0.00620)	(0.00306)	(0.0463)	
N	76708	76708	76708	76708	76708	76708	

B. Fin. Outcomes	Pell (Enr.)	Loans (Enr.)	All Grants (Enr.)	Loans (Uncond.)	Pell (Uncond.)	All Grants (Uncond.)	
Disc.	259.9***	749.0***	312.1***	444.7***	150.1***	182.1***	
	(36.35)	(96.56)	(47.57)	(72.84)	(29.10)	(37.32)	
N	47755	47755	47755	76708	76708	76708	

C. Educ. Outcomes	Next Tot Hours	Enr CC	Enr 4yr	Grad CC in 0y	Grad CC in 1y	Grad 4yr in 0y	Grad 4yr in 1y
Disc.	-0.00153	-0.000714	-0.00192	0.000158	-0.000161	0.00910	0.0167**
	(0.189)	(0.00225)	(0.00719)	(0.00156)	(0.00214)	(0.00730)	(0.00786)
N	76708	76708	76708	45096	45096	67804	61273

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

This table considers outcomes for students who were enrolled in a university in the year they turned 23. Panel A checks for balance of student characterisitics across the threshold. Panel B checks for discontinuities in financial aid received by students in the next year. Some of the estimates in Panel B are conditional on enrolling in the next year (Enr.) and others are unconditional and have zeroes for students who did not enroll. Panel C checks for changes in educational outcomes.

Table 3.4: Community College Students, No Pell This Year

A. Covar.	(1) Male	(2) White	(3) Black	(4) Hispanic	(5) Asian	(6) Cal. Year	(7)
Disc.	0.00249	0.00348	-0.00537	-0.00722	0.00133	0.0346	
	(0.00645)	(0.00547)	(0.00544)	(0.00620)	(0.00195)	(0.0444)	
N	83506	83506	83506	83506	83506	83506	

B. Fin. Outcomes	Pell (Enr.)	Loans (Enr.)	All Grants (Enr.)	Loans (Uncond.)	Pell (Uncond.)	All Grants (Uncond.)	
Disc.	288.7***	156.0***	329.9***	107.6***	176.9***	202.3***	
	(14.33)	(40.18)	(16.41)	(24.05)	(8.690)	(9.966)	
N	114280	114280	114280	194994	194994	194994	

C. Educ. Outcomes	Next Tot Hours	Enr CC	Enr 4yr	Grad CC in 0y	Grad CC in 1y	Grad 4yr in 0y	Grad 4yr in 1y
Disc.	0.369***	0.00716	0.00220	-0.000267	0.00264	-0.000667	-0.000937
	(0.0964)	(0.00454)	(0.00221)	(0.00267)	(0.00347)	(0.000464)	(0.00102)
N	194994	194994	128311	161470	161470	229166	63542

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

This table considers outcomes for students who were enrolled in a community college in the year they turned 23. Panel A checks for balance of student characterisitics across the threshold. Panel B checks for discontinuities in financial aid received by students in the next year. Some of the estimates in Panel B are conditional on enrolling in the next year (Enr.) and others are unconditional and have zeroes for students who did not enroll. Panel C checks for changes in educational outcomes.

Table 3.5: Community College Students, Pell This Year

A. Covar.	(1) Male	(2) White	(3) Black	(4) Hispanic	(5) Asian	(6) Cal. Year	(7)
Disc.	-0.000982	0.00781*	0.00182	-0.00623	-0.00108	-0.0421	
	(0.00475)	(0.00454)	(0.00269)	(0.00393)	(0.00174)	(0.0298)	
N	178355	178355	178355	178355	178355	178355	

B. Fin. Outcomes	Pell (Enr.)	Loans (Enr.)	All Grants (Enr.)	Loans (Uncond.)	Pell (Uncond.)	All Grants (Uncond.)	
Disc.	-43.90	103.2*	-25.64	64.76*	-22.64	-11.01	
	(32.80)	(57.84)	(37.64)	(38.82)	(27.00)	(30.43)	
N	52696	52696	52696	83506	83506	83506	

C. Educ. Outcomes	Next Tot Hours	Enr CC	Enr 4yr	Grad CC in 0y	Grad CC in 1y	Grad 4yr in 0y	Grad 4yr in 1y
Discon.	-0.0928	-0.00602	0.00444	0.00219	0.00699	0.000230	-0.000184
	(0.156)	(0.00687)	(0.00342)	(0.00444)	(0.00580)	(0.000245)	(0.000706)
N	83506	83506	55407	69314	69314	97747	27658

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

This table considers outcomes for students who were enrolled in a community college in the year they turned 23. Panel A checks for balance of student charac-terisitics across the threshold. Panel B checks for discontinuities in financial aid received by students in the next year. Some of the estimates in Panel B are conditional on enrolling in the next year (Enr.) and others are unconditional and have zeroes for students who did not enroll. Panel C checks for changes in educational outcomes.

Appendices

Appendix A

College on the Cheap

A.1 College on the Cheap

A.1.1 Annexation/Campus Data Collection

Data on the dates of annexation was obtained in three ways. The first is through information posted online on community college websites that detailed historical annexations. The second is by using archives of newspapers covering the votes on annexation. The third is by examining patterns of students payment of in-district tuition. For each annexation. The ERC data provides information on whether enrolled students paid in-district tuition. From this data I identified years in which the fraction of students paying in-district tuition jumped substantially in a K-12 district. These changes were then verified using news reports when possible. For additional information on the source for each annexation and campus building date see this online spreadsheet: http://goo.gl/6sjDvz.

In order to assign opening dates for new campuses, I collected information on existing campuses at the five community college taxing districts studied and determined when they were opened using information from the community college websites. I then used latitude and longitude data on campuses and school districts to map campuses to K-12 school districts.

A.1.2 Additional years of data

To take advantage of additional variation in community college tuition caused by annexation, I estimate the effect of annexation on enrollment for 1995 to 2012. These results are in Table A.1 and include college/year fixed effects. In Column 1, annexation is associated with a slightly smaller increase in sticker price of tuition. The effect of annexation on community college enrollment is slightly larger with the estimate being 3.7 pp as opposed to 3.2 pp. The effects for enrolling in district and enrolling in no college are also larger than previous estimates but are still highly statistically significant. However, there is still no measured effect of annexation on enrollment at four-year colleges. The results for building a new campus are similar when using all data but slightly attenuated. These results suggest that the findings on enrollment are robust to using additional variation.[1]

A.1.3 Hours attempted

Another measure of educational attainment is the number of college credit hours accumulated. The data contain information on the number of credit hours attempted, which I will use as another measure of attainment. Unfortunately the data do not contain information on credit hours passed during the relevant time frame but credit hours attempted serves as a good intermediate indicator of credits accumulated.

Panel A of Table A.2 shows that reduced tuition resulting from annexation

[1] Specifically, there was one additional community college that had any annexations and five additional annexations from 2006-2012.

Table A.1: Enrollment, All Years

	(1) Tuition	(2) Enr. CC	(3) Enr. In. Dist	(4) Enr. 4yr	(5) Enr None
Annexation	-1.13***	0.037***	0.050***	-0.0019	-0.035***
	(0.073)	(0.0067)	(0.0081)	(0.013)	(0.011)
Year and District FE	X	X	X	X	X
Demographics	X	X	X	X	X
College/Year FE	X	X	X	X	X
New Campuses	X	X	X	X	X
Mean of Dep Var	1.33	0.27	0.22	0.24	0.49
N	390237	390237	390237	390237	390237

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

This table considers the effect of annexation on immediate college enrollment patterns using data from 1994-2012. The CC column examines enrollment in a community college, 4yr considers enrollment in public universities, In Dist. considers enrollment at the in-district community college, and Nowhere is an indicator for not enrolling in any public colleges or universities. The rows at the bottom indicate inclusion of controls for year and district fixed effects, demographic characteristics including race and gender, and college by year fixed effects. Standard errors are clustered at the K-12 District level and are in parentheses with $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

increased hours attempted at community colleges. After four years, annexation had increased average credits attempted by 2 credit hours. There point estimates on the increases in university credits are positive but are not statistically significant. Unfortunately, the data on credits attempted does not extend far enough to consider credits attempted at universities after 8 years which would give students more time to transfer to community colleges.

Panel B of Table A.2 uses annexation as an instrument for attending a community college. The results have a similar pattern to Panel B of Table A.2 but scale the coefficients by the number of students induced to attend community college. Students induced to attend community college as a result of annexation increased the number of credits attempted at community colleges after 6 years by 47.6 and the overall number of credits by 58.9. These results suggest that reduced community college tuition increased community college attendance and the students who attended were engaged nearly enough credit hours for an associate's degree.

Table A.2: Hours Attempted

	(1)	(2)	(3)	(4)	(5)
A. Reduced Form	Univ. Credits after 4 yrs	Univ. Credits after 6 yrs	CC Credits after 4 yrs	CC Credits after 6 yrs	All Credits after 6 yrs
Annexation	0.25	0.51	2.00***	2.15***	2.66*
	(1.25)	(1.34)	(0.24)	(0.25)	(1.40)
B. Instrumental Variables	Univ. Credits after 4 yrs	Univ. Credits after 6 yrs	CC Credits after 4 yrs	CC Credits after 6 yrs	All Credits after 6 yrs
Attend CC	5.56	11.3	44.3***	47.6***	58.9**
	(26.7)	(28.0)	(5.66)	(6.32)	(24.6)
Year and District FE	X	X	X	X	X
Demographics	X	X	X	X	X
College/Year FE	X	X	X	X	X
New Campuses	X	X	X	X	X
Mean of Dep Var	24.4	28.8	14.1	16.5	45.3
N	206370	206370	206370	206370	206370

This table considers the sum of hours attempted at community colleges and universities after four and six years. Panel A presents the reduced form effect of annexation on credits attempted and Panel B instruments for community college attendance using annexation. Each column is a separate regression considering the effect in the X^{th} year after high school. The rows at the bottom indicate inclusion of controls for year and district fixed effects, the building of new campuses, demographic characteristics including race and gender, and college by year fixed effects. Standard errors are clustered at the K-12 district level and are in parentheses with $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

Appendix B

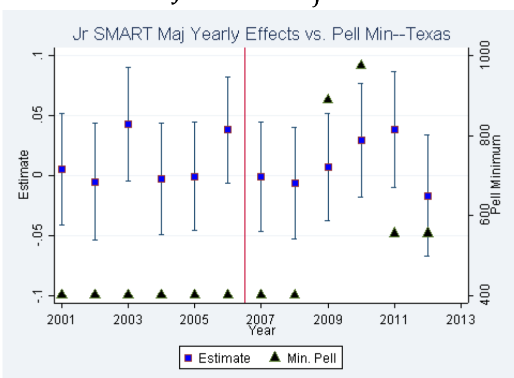
Was That SMART?

The amount of the Pell Grant that students received at the discontinuity changed over time. The largest amount was in the 2009-2010 school year of $976. One concern is the change in majors occurring at the threshold may be due to increased Pell Grants instead of incentives from the SMART Grant. Figure 11 shows the estimates for junior and senior major at BYU and Texas as compared to the minimum Pell Grant in that year. In every instance the largest discontinuity is in the 2010-11 school despite the minimum Pell Grant falling to $555. In the year with the largest effects, the Pell Grant is relatively modest at $555.

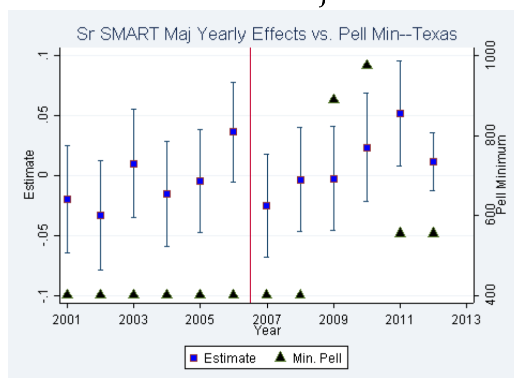
Figure B.1: Estimates by Year vs Pell Grant

Texas

Junior Major

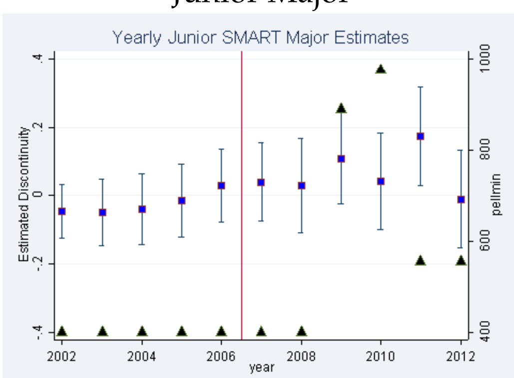


Senior Major

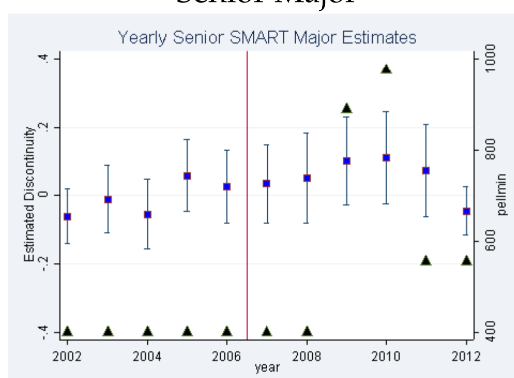


BYU

Junior Major



Senior Major



The estimated discontinuity for the impact of SMART Grants on majors is plotted along with 95% confidence intervals. The years represent the end of a school year. The triangles represent the A bandwidth of 1.1 is used for Texas and 2.5 is used for BYU.

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