The Effects of Mandatory and Free College Admission Testing on College Enrollment and Completion

Mary Vansuch

University of Chicago

May 2017

THE UNIVERSITY OF CHICAGO

THE EFFECTS OF MANDATORY AND FREE COLLEGE ADMISSION TESTING ON COLLEGE ENROLLMENT AND COMPLETION

A BACHELOR THESIS SUBMITTED TO

THE FACULTY OF THE DEPARTMENT OF ECONOMICS

FOR HONORS WITH THE DEGREE OF

BACHELOR OF THE ARTS IN ECONOMICS

BY MARY VANSUCH

CHICAGO, ILLINOIS
MAY 2017

1 Abstract

Between the years 2001 and 2015, twenty-three states and the District of Columbia implemented a policy providing mandatory and free college admission exams (ACT or SAT) to all public high school juniors. As such, the policy reduced to zero out of pocket expenses for exam fees, and likely reduced out-of-pocket expenses for exam preparation, because schools might have been induced to provide such a service in-house. The policy also reduced the time cost of test taking because the test is administered during class time and at a student's school. Because the mandatory exam is administered during the junior year, the policy may also have increased the amount of information a student has about her college prospects earlier on in her decision making process. In this paper I hypothesize that the decreased costs and increased information may induce more students to apply to and enroll in college. I use both college-level longitudinal data (IPEDS) along with cross-sectional student-level data (ACS) to test these predictions. Specifically, I exploit the fact that not all states implemented the policy and that those which did so implemented the policy at different points in time. In the college-level analysis, I find that the average college saw an increase in about 88 enrolled students and 460 applications from the policy without any effect on their graduation rates. In the individual-level analysis, I find that treated individuals have approximately 1.03 times the odds of untreated individuals of attending college. In the appendix I propose a model for the decision to apply, enroll, and complete college.

Contents

I am especially grateful to Melissa Tartari for her thoughtful mentorship and advice throughout my thesis writing process. I also thank Victor Lima and Kotaro Yoshida for their assistance during the undergraduate thesis writing workshop, as well as all of my colleagues present at the undergraduate thesis workshop.

2 Introduction

In order to examine if a student's decision to apply to and attend college is influenced by allowing her to receive more information about her own abilities earlier in the process, I examine the impact of a natural experiment wherein some states mandated a college admissions exam to comply with No Child Left Behind. No Child Left Behind (NCLB), passed in 2001, stipulated that some part of federal funding to schools is to distributed based on a state-developed measure of "Adequate Yearly Progress". That is, schools are to be rewarded for improving their students' test scores and for having all students achieve minimum proficiency standards. Some states developed their own measures of adequate yearly progress but many conveniently opted to use the ACT and SAT (either right away in 2001, or subsequently) for evaluating older students.[1] By 2012, twelve states had accordingly mandated either test to their junior students. Because colleges have used the ACT and SAT scores to determine admissions and merit-based scholarships, the tests are high-stake for both students and school districts.

In this paper, I estimate the effect of mandating the taking of one of these standardized tests ("the policy") on college application, enrollment, and completion rates. Specifically, I develop a model for a student's decision to take the standardized test, apply, enroll and complete college in the first section of the appendix. Under suitable assumptions, the model predicts that the policy would result in more students applying and enrolling in college and would not reduce completion rates. To test these predictions I use cross-sectional student-level data and panel college-level data and exploit the fact that not all states implemented the policy and that those which did so implemented it at different points in time.

The prior may be to expect that $50 is a small amount of money to influence a rational actor's decision to attend college given the large long-term benefits of a college education. Previous behavioral economics literature would suggest that small nudges can induce larger than expected behavioral changes. For example, Pallais (2013) found that when the ACT exam company increased the number of free test score reports that can be sent to colleges, the fraction of students who increased the number of reports sent far exceeded what would be expected given that the marginal cost of sending an additional test score report is merely $6. Pallais concluded that high school students may not have full information when doing the cost benefit analysis of deciding to apply to colleges since their behavior dramatically changes between offering to send

[1] No Child Left Behind was a 2001 update of the Elementary and Secondary Education Act. This is an act renewed regularly since 1965 to provide funding to schools serving low-income and special needs students. No Child Left Behind created more stipulations on federal government money flowing to states for education, including requiring schools develop exams to measure "adequate yearly progress" of students' improvements and academic proficiency. This policy affected all states' low-income schools essentially equally. The difference I use is whether or not schools happen to choose the ACT or SAT as a measurement of "Adequate Yearly Progress", or proof that students are improving their test scores and that students meet a minimum standard of knowledge. This was to be systematically recorded for both overall school statistics and for each sociodemographic subgroup. Most states use for all grades, including 11th, the passing of state-specific examinations that otherwise have no impact on an individual's future.

It should also be noted that while I make this generalization, as discussed in the appendix, some states adopted this policy instead simply as a means to encourage college attendance in their states and not to conform to NCLB standards specifically. However, in all states, schools were still evaluated in part due to their students' performance on the exam, and the earliest states to adopt the policy were doing so to adhere to NCLB standards. Therefore, this generalization is made.

a free test score report and a test score report at $6. Similarly, Mayer et al. (2015) found that substantially more parents read to their preschool children when sent a text reminder, further demonstrating that small nudges can create dramatic changes in behavior. These findings are relevant to this paper because mandating standardized tests may also be regarded as a small change in financial incentives. The policy made the tests free, waiving the relatively small $55 fee that both the ACT and SAT companies usually charge a student[2]. We may therefore expect large responses to this policy. However, analysis carried out by several researchers have produced mixed results, in that the magnitude of the effects seems not to be robust to the choice of time period and may vary depending on school's characteristics (selective versus not).

Specifically, Hurwitz et al. (2015) studied the impact of the mandatory and free SAT for the state of Maine using proprietary data for the first three years of the policy's implementation in the state. Through a difference-in-difference design, they found that college enrollment increased by 2 to 3 percent on average, and that of those students who were induced to take the SAT via the policy, enrollment increased by 10%. In order to study the effects of the program on Maine students, Hurwitz et al. merged four datasets together: the College Board[3] data from 2004-2008 regarding student performance on SAT exam and high school attended, the National Student Clearinghouse[4] data on students' college attendance, the National Center for Education Statistics data (IPEDS) on the demographics of particular high schools, and Census data on the demographics of students' schools' zip codes where high schools are located. Analysis compared the college attendance rates of a sample of Maine high school seniors to seniors from other SAT-dominant states[5]. They find that most of the increase in enrollment came from Maine's rural students in comparison to their urban or suburban ones. They further use a 2SLS approach to approximate the treatment effect on the treated, using the policy change as an instrument for the percentage of people taking the exam.

Klasik (2013) revisited Hurwitz et al.'s analysis by adding data for 2009 and the states of Colorado and Illinois, which mandated the ACT in 2001. Klasik employs a difference-in-differences approach wherein he identifies states that are similar in demographics and pre-policy college enrollment to treated states using synthetic controls. He performs separate difference-in-difference approaches analyzing data on the individual level or on the state level. Individual-level data on college-freshman aged individuals was collected from the Census's Current Population Survey. For state level data, Klasik uses the National Center for Education

[2]As of 2017, this was a $55 fee to take either test with the writing portion, $40 to take the ACT without the writing portion and $43 to take the SAT without the writing portion. Regardless of the specific exam taken, the exam fee is small.

[3]The company that produces the SAT, also the company that Hurwitz et al. work for.

[4]A private company that provides college transcript services. By matching student names from the SAT data, Hurwitz et al. know the students' majors, college attended, and degree completion status.

[5]The SAT and ACT tests are well-documented to be regionally dominated. For example, in Nebraska in 2016 (a state that does not mandate the ACT or SAT) had 18,598 students take the ACT and 604 take the SAT. Washington state had 16,652 students take the ACT but 43,783 students take the SAT. (Saget 2013)

Statistics IPEDS college enrollment data, which collects college enrollment for every college. He assumes that students generally go to college in the same state they attended high school, and thus labels any college or individual student in a state that implemented the policy as "treated". Using the state-level analysis, Klasik estimates an overall 10% drop in enrollment in Maine and documented that two-years collages experience the drop in enrollment but four-year colleges experience no change in enrollment. Klasik found no overall change in college enrollment in Colorado or Illinois. Private and public four-year colleges in Illinois saw an increase in enrollment by 12%, while in Colorado, only private colleges saw a 10% increase in enrollment. Using individual-level analysis, Klasik estimates that Colorado students were more likely to enroll in two-year colleges or full-time, but no more likely to enroll in college overall. Illinois students were 10% more likely to enroll in any college, and most likely to enroll in a four-year college. Maine students were no more likely to enroll in any college. Part of the discrepancy in the findings concerning Maine may be due to the different data sources used by Hurwitz et al. (2015) and Klasik, and the fact that Klasik uses either state-level or individual-level analysis while Hurwitz et al. use only individual-level analysis. Most importantly, Hurwitz et al. knew students's state of origin from proprietary data, while Klasik assumed students never move and that all colleges, including private or flagship universities, only enroll in-state students.

Goodman (2016) confirms Klasik (2013)'s finding of no overall impact on enrollment in in Colorado and Illinois but reports a 20% increase in selective college enrollment. Goodman estimates the percentage of students who receive "competitive" test scores who took the ACT exam because they were forced to take the ACT exam. Her assumption for a "competitive" score relies upon a figure given in a common college entrance advice book, Barron's. She uses this third party resource to divide college selectiveness into discrete categories, and then estimates changes in enrollment within each discrete category. I improve upon this assumption by not assuming that college selectiveness remains fixed over time as Goodman does. Instead, I will use the IPEDS data to test if colleges respond to an influx in applicants by changing their level of selectiveness, as measured by the twenty-fifth and seventy-fifth percentile of ACT and SAT scores. Goodman estimates that about half of students were induced to take the exam, about 40-45% of induced students received competitive scores (as per Barron's definition), and as a result, selective college enrollment (as determined by the discrete categories informed by Barron's) increased by 20%.

I contribute to the above research along several dimensions. First, I include in my analysis all the states and classify as "treated" those that made standardized tests mandatory at any point between 2001 and 2012 (11 states out of 50). Second and related, I expand the window of time which gives me more years before and after the policy change for the states that did mandate the tests. Conveniently, the long time window also enables me to relax the common trend assumption, often invoked in the difference-in-difference design. By using more states over more years, I am therefore less prone to overstate the impact of the policy due to short-term changes in student's incentives that occurred contemporaneously with the policy. Building on Goodman's insight that the impact may vary depending on college selectivity, I include college selectivity in my analysis. Additionally, I acknowledge that colleges may have responded to the policy by changing their selectiveness and test this assumption (which Goodman maintained). However, some of the discrepancy between Klasik (2013) and Hurwitz et al. (2015) also comes from the fact that Hurwitz et al. can more accurately define which students are treated, rather than making the assumption of Klasik (2013) that students never move and both private and public colleges only take in-state students. I improve upon Klasik's assumption that colleges only take in-state students by using their levels of enrollment from each state prior to the policy to create an "intensity to treat" measure. Unfortunately, I lack the proprietary data to improve the never moving assumption for individuals, and therefore may be likely to make similar errors to Klasik in this regard. Finally, I also explore the impacts on college graduation rates, which has not been seriously analyzed in past studies.

I carry out my analysis on two separate datasets. The first is the Integrated Postsecondary Education Data System survey (IPEDS, 2000-2013) and the second is the American Community Survey (ACS, 2000-2014). On any given year, IPEDS contains data on every college that received any federal funds, including for financial aid. Accordingly, the sample includes the vast majority of U.S. universities because it is illegal for any institution that receives federal funds not to report to IPEDS[6]. This data source provides excellent information to measure year-to-year changes in college enrollment and applications, as well as time invariant characteristics such as whether a collage is for-profit, private, two or four years, etc.. While IPEDS was also used by Hurwitz et al. (2015), Goodman (2016) and Klasik (2013), I choose to supplement the analysis by also employing the ACS. The ACS data is a nationally representative survey of households carried out yearly by the Census Bureau. It contains individual-level information on, for instance, current schooling status, highest degree completed, etc.. The ACS is therefore better suited to measuring changes in the proportion of college-aged individuals attending colleges. However, it does not provide information on a person's college application decision nor any detail about the college attended.

A key step in using either dataset is establishing an indicator for whether the unit of observation (a college in the IPEDS data, an individual in the ACS data) was "treated", i.e. impacted by the policy. Neither dataset

[6]It is impossible to know precisely how many institutions choose to operate without using any federal funds. Such institutions will tend to be non-accredited and most likely not have enforced standards of admissions. In other words, we don't expect such institutions to be impacted by the policy. As it is, IPEDS has data on 7,500 postsecondary institutions in the United States, and is the most comprehensive database of postsecondary institutions.

knows precisely how many students were juniors in public high schools at the time of the treatment. In the IPEDS data, I create a measure of "intensity of treatment" based upon the pre-treatment freshmen residency composition of a college. This then approximates about what percent of the untreated student body would have been treated without the mandatory college entrance exam. This has not been done in any previous analysis, which consistently assumed colleges were either completely treated or untreated based on what state they were physically located. For the ACS data, I estimate whether an individual was treated by assuming they went to high school in the state they lived one year prior to the survey. Due to this much stronger assumption, I believe that the IPEDS data more "accurately" identifies who belongs to the treated group. A common set of critiques of the IPEDS data is that it undercounts first-year, part-time students (Soldner et al. 2016) and that it undercounts online students (Straumsheim 2014). Online universities are more likely to be open enrollment, so I do not expect this to greatly impact my analysis. However, since the college-level analysis only includes full-time students, I may be understating the policy's impact if we expect that students who are impacted by the policy are more likely to enroll part-time. Since first generation college students are more likely to be part-time (Engle and Tinto 2008), and I expect that first generation students are more likely impacted by the policy than students with parents who attended college, it is reasonable to think I am somewhat underestimating the impact of the policy using the IPEDS data by not counting part-time students.

The first step I took with the IPEDS college-level analysis was to see if colleges were induced to have higher admissions due to an increase in applicants from the policy. This was approximated by testing to see if the twenty-fifth and seventy-fifth percentiles changed from the ACT or SAT scores of admitted students. Additionally, I also checked if the admission rate changed overall. I found little evidence to suggest that colleges have been modifying their admissions standards in response to more students applying to and attending college. My limited evidence may suggest that more highly-qualified students are applying to and being accepted to colleges, particularly ACT colleges, but that lower-qualified students are not being adversely effected by this shift. Enrollment at the average college increased by 443 students as if the intensity of treatment increased by 100%, or 88 at the average college for the average intensity of treatment. The number of applicants also increases by 2,378 with a 100% increase in treatment, or 460 at the average college with average treatment, albeit only significantly at the 10% level rather than the 5% level. Graduation rates are not impacted, so I have reason to believe that the increase in enrollment comes from students who are just as prepared to enter college as their untreated peers.

The individual-level analysis with the ACS data shows similar, but somewhat contradictory results.

Odds of contemporaneous enrollment changes in magnitude and direction depending on the subset of the data examined. All studied models explain very little variation in the data (less than 10% in all cases), so I am not confident these results adequately control for confounding factors. For college-aged individuals listed as dependents of their parents, the odds of enrollment only marginally increase, and are only statistically significant on the 10% level for freshmen-aged individuals. For all individuals, not controlling for parent income[7], the results are still marginal for enrollment, and only significant when examining college-aged individuals specifically. Overall, these results are contradictory and have extremely small size effects, so I conclude the policy has a marginal effect at the most on the odds of enrollment. The odds of being a college dropout or college graduate are greater for beyond college aged (twenty-three or older) individuals who received the treatment than those who did not. However, the odds of having college enrollment are no different in treated versus untreated states when including older as well as college-aged individuals in the sample. The incompatibility of these results with the increased odds of being currently enrolled in college makes me believe I have mis-approximated who is "treated" and "untreated" in the ACS data. This is particularly true because many treated states are highly rural states with fewer opportunities for college graduates, so college graduates are the least likely to be correctly labeled as living in the same state as where they grew up. These mechanisms will be further investigated.

First, I will discuss my two primary data sources, and how each can be used to see if the policy increased college enrollment and applications. Then, I will propose an econometric model for analysis. The results of the econometric analysis will be presented and analyzed. An appendix contains the beginning framework of a model to explain the economic mechanisms behind the policy's impact. This appendix also contains more details about ACT and SAT policies, including the marginal differences between states that could effect student behavior.

3 The Data

Ideally, I would have data at the individual-level wherein I knew the student's socioeconomic characteristics and whether they attended a public high school in a state with the policy. However, such data is not publicly available. Instead, in my analysis I use both college-level National Center for Education Statistics data (IPEDS) and individual-level Census American Community Survey (ACS) data. I supplement both databases with Bureau of Labor Statistics (BLS) data regarding state GDP per capita and state unemployment rates

[7]It is impossible to control for parent income when we do not have individuals listed on the survey as dependents.

to also control for the effects of state-wide characteristics that may effect the decision to go to college. The college-level analysis does not allow me to know what types of individuals are induced by the policy to attend college, but does reduce measurement error of the treatment itself. The individual-level analysis gives more detail as to what types of individuals are influenced by the policy, but has more measurement error for the treatment variable. I will first describe the college-level data, then the individual-level data.

3.1 College-Level Data

The IPEDS database contains data on every college that receives any federal funds, including for financial aid, which is the vast majority of U.S. universities attended. This data is collected annually and is self-reported by the colleges. The longitudinal data enables me to compare year-to-year changes in enrollment and applications at the college level. It also provides information on institutional characteristics, such as highest degree offered and whether the institution is private. I use data from the year 2000 to construct base characteristics for each college. Time invariant characteristics include the college's the admissions rate, as a proxy for its selectivity, the percentage of the student body from racial and ethnic minority groups in 2000, whether or not the college is mainly applied to using the ACT or SAT test, the tuition charged in the year 2000, the highest degree level offered, the graduation rate in 2000, whether or not the college is a military academy, and whether the college is private or for-profit.

I regard years 2001 to 2013 as the treatment years[8]. Because the earliest adopted policy was in spring of 2001 by Colorado and Illinois, the earliest affected freshmen class is those entering college in the fall of 2002. Therefore, I have at least one treated and one untreated year for each state.

Not all colleges necessarily fill out the survey completely each year. Approximately 90% of public colleges and 85% of non-profit colleges have all fourteen years of data, and all colleges have at least two years of data. Therefore, since these are the schools most affected by the ACT policy, I am not concerned by the missing data of a few years. I only use schools that are not "open enrollment", i.e. I use schools that are only "competitive colleges".[9]The colleges have the pre-treatment characteristics outlined in Tables 1 and 2. I proxy the prestige and quality of the school by controlling for the highest degree offered and their pre-treatment graduation rates, as well as other pre-treatment characteristics.

[8]See appendix for when precisely each state adopted the policy.

[9]I define for now "competitive" college to mean that the college has an application process at all. If any students are able to be rejected from the school, even if only a small percentage, the college is "competitive". This is to contrast with open enrollment schools where anyone with a high school diploma or GED can attend so long as they fill out the paperwork.

Table 1: Pre-Treatment Characteristics of Colleges (Year 2000)

	Observations (Number of Colleges)	Mean	25th Percentile	50th Percentile	75th Percentile
Number of Years of Data	2,842	13 (2.83)	14	14	14
Graduation Rate, 2000	2,076	0.52 (0.22)	0.37	0.51	0.67
Admission Rate, 2001	2,388	0.74 (0.21)	0.64	0.78	0.89
Percent Black 2000	2,803	0.14 (0.21)	0.02	0.06	0.17
Percent Native American 2000	2,803	0.01 (0.05)	0.00	0.00	0.01
Percent Asian, 2000	2,803	0.04 (0.08)	0.00	0.01	0.04
Percent Hispanic 2000	2,803	0.08 (0.14)	0.01	0.03	0.08
Percent White 2000	2,803	0.67 (0.28)	0.50	0.76	0.89
Percent Unknown Race 2000	2,803	0.04 (0.11)	0.00	0.00	0.04

Standard deviation in parenthesis.

Table 2: Highest Degree Offered by College (Year 2000)

	Frequency	Percent	Cumulative Percent
Award of less than one academic year	99	3.48	3.48
At least 1, but less than 2 academic years	469	16.5	19.99
Associates degree	384	13.51	33.5
At least 2, but less than 4 academic years	275	9.68	43.17
Bachelors degree	494	17.38	60.56
Post-baccalaureate certificate	27	0.95	61.51
Masters degree	535	18.82	80.33
Post-masters certificate	155	5.45	85.78
Doctors degree	404	14.22	100
Number of Colleges	2,842	100	

As table 2 shows, a wide variety of types of institutions exist in the dataset. I use an indicator variable for each type of highest degree offered in the regressions. This, combined with data on graduation rates, is used as a proxy for "college quality". While some future analysis may use these measures to create an index of "college quality", I use indicator variables for the highest degree offered and the graduation rate in 2000 directly in my analysis to roughly control for "college quality".

When doing a few simple graphs, we see that enrollment and application numbers increased as intensity of treatment increased. These can be seen in Figures 1, 2, and 3. The admissions rate remains relatively stable over time, although it dips immediately after the recession, as seen in Figure 3. Increases in the admissions and enrollment rates coincide with increases in the proportion of treated individuals, as seen in Figures 1 and 2. While there is a consistent upward trend in applications and enrollment, the slopes become steeper as the proportion of treated students increases. This would suggest that the policy does increase applications and enrollment.

3.1.1 Treatment and Comparison Colleges

To identify treated individuals, I create an "intensity of treatment" variable based upon 2000 level of freshman undergraduate residency. This has not been previously done in the literature, which always assumed that all colleges in treated states would be equally impacted by the policy and all colleges in untreated states would be equally not impacted by the policy. Instead, I propose that the intensity of treatment at colleges is determined by the historical patterns of full-time first-time freshmen undergraduate residency[10]. For example, if fifty percent of the University of Colorado comes from Colorado in 2000 but eighty-five percent of Colorado State University comes from Colorado in 2000, then Colorado State University would be more "intensely" impacted by Colorado adopting the policy. Similarly, if fifteen percent of the University of Wyoming's freshman class comes from Colorado, but only two percent of University of Alaska's freshman class comes from Colorado, then the University of Wyoming will be more intensely treated than the University of Alaska.

[10]As previously noted in the introduction, this does not include part-time students. This has been cited as a major flaw in the IPEDS data, as many first generation college students opt to begin as part-time students (Soldner et al. 2016). This should bias our results downward, i.e. underestimate the effects of the policy, since I would suspect that first-generation college students are more likely to have their behavior changed by the policy than students who live in families with a college attendance culture.

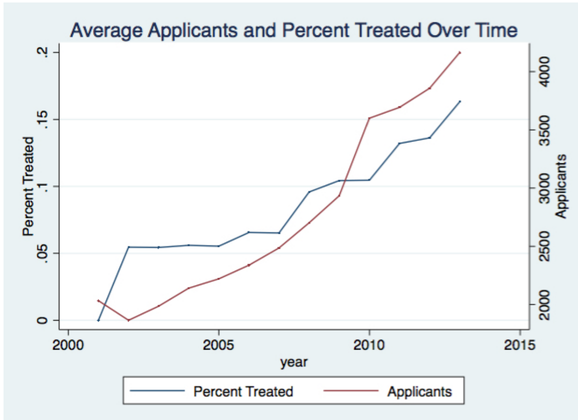

Figure 1:

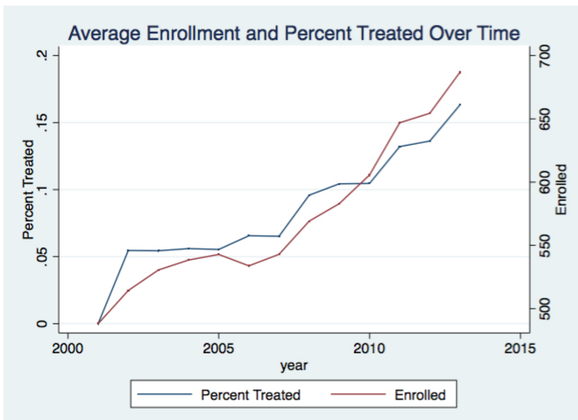

Figure 2:

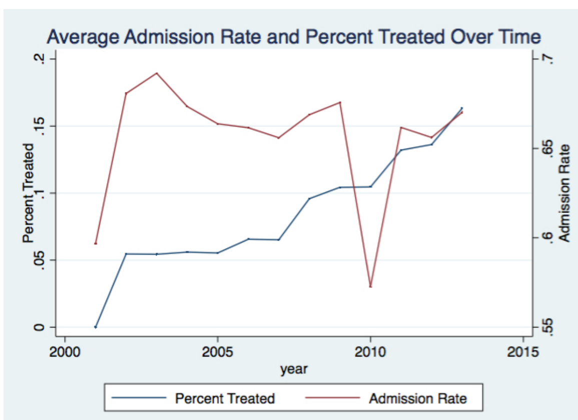

Figure 3:

Table 3: University of Michigan Ann Arbor Treatment

Year	Additional States Treated	Percent of year 2000 freshmen from these states	Intensity of Treatment for University of Michigan
2001	None	0.00%	0.00%
2002	Colorado and Illinois	4.91%	4.91%
2003	〃	〃	4.91%
2004	〃	〃	4.91%
2005	〃	〃	4.91%
2006	〃	〃	4.96%
2007	Maine and Wyoming	0.06%	4.96%
2008	Michigan	58.95%	63.92%
2009	Kentucky	0.31%	64.23%
2010	North Dakota	0.02%	64.23%
2011	Tennessee and Delaware	0.20%	64.43%
2012	〃	〃	64.45%
2013	North Carolina and Idaho	0.26%	64.71%

Table 4: Kalamazoo College Treatment

Year	Additional States Treated	Percent of year 2000 freshmen from these states	Intensity of Treatment for Kalamazoo College
2001	None	0.00%	0.00%
2002	Colorado and Illinois	3.69%	3.69%
2003	〃	〃	3.69%
2004	〃	〃	3.69%
2005	〃	〃	3.69%
2006	〃	〃	3.69%
2007	Maine and Wyoming	0.00%	3.69%
2008	Michigan	82.46%	86.15%
2009	Kentucky	0.62%	86.77%
2010	North Dakota	0.00%	86.77%
2011	Tennessee and Delaware	0.00%	86.77%
2012	〃	〃	86.77%
2013	North Carolina and Idaho	0.31%	87.08%

To demonstrate more precisely, I will outline each year's intensity of treatment at University of Michigan at Ann Arbor. In 2000, the the composition of the freshman class corresponds to intensity of treatment can be seen in table 3. We can compare this to Kalamazoo College, a private liberal arts college in the same state with a larger in-state draw in table 4. As we can see, the regional Kalamazoo College is more "intensely" impacted than University of Michigan from Michigan adopting the policy because University of Michigan draws historically less from Michigan itself. Similarly, University of Michigan is more intensely impacted when Colorado and Illinois adopted the policy because Kalamazoo College does not draw many students from out of state.

3.2 Individual-Level Data

I couple the college-level analysis with individual-level analysis of the American Community Survey (ACS). This data is better suited for measuring the changes in the proportion of college-aged individuals attending colleges, as well as parsing out what types of individuals are impacted by the policy. While the IPEDS data can measure the increases in enrollment, it cannot measure the increases in enrollment in comparison to increasing or decreasing numbers of college-aged individuals. Additionally, I can better measure how the policy impacts different socioeconomic groups.

For individuals who were college-aged at the time of the survey, the average age is 21, and for individuals post-college (but old enough to have been potentially treated during the earliest treatment, 2001), the average age is 26. Data from 2000 to 2014 is used, like with the IPEDS data. The sample size is 3,230,912, substantially larger than the colleges in the IPEDS data.

3.2.1 Treatment and Comparison Individuals

For the ACS data, I estimate whether an individual was treated by assuming they went to high school in the state they lived one year prior to the survey. Due to this much stronger assumption, I believe that the IPEDS data more accurately labels who belongs to the treated group. An individual is classified as "treated" if they lived one year ago in a treated state and were high school aged at the time of the treatment. For most years I can estimate when people were juniors in high school using quarter of birth data. I presume people born between January and September are juniors in high school sixteen years after their year of birth, and people born in the last quarter are juniors in high school seventeen years after their year of birth. For data the first four years of data we do not have quarter of birth data, so I conservatively code people as if they were born in the fourth quarter (therefore we are likely to identify high school juniors who are actually seniors, and therefore underestimate the proportion of the population who had not been treated by a marginal amount). Since this is true for earlier years in the data, I only underestimate the number treated in this regard in Illinois and Colorado.

As seen in Table 5, here are marginal differences between the treated and untreated, but in general, the treated group has more people going to and completing college. There are a total of 226,162 treated and 3,004,750 untreated individuals in the dataset. For college-aged individuals, 828,400 (42.25%) are not listed as dependents while 1,132,099 (57.75%) are.

Table 5: College Enrollment, Dropouts, and Graduates in ACS Data by Subsample

	Observations	Mean, Treated	Mean, Untreated
College Enrollment, College Aged	1,960,499	0.52	0.48
College Dropout, Post-College Aged	1,588,882	0.27	0.28
College Graduate, Post-College Aged	1,588,882	0.42	0.36
College Enrollment, Post-High School Aged	3,230,912	0.62	0.58

I use the highly flawed measure of assuming people do not move between states. For people currently in college, this is not a particularly far-fetched assumption. It has been shown in previous literature that people tend to go to college close to home (Long 2004) geographically, and this therefore will typically translate to going to college within the same state. This is especially true in my data, where most treated states are geographically large (except for Delaware). I treat people as having gone to high school in the location they lived in as of one year prior to the survey, under the assumption people rarely move, and if they move, they do not move particularly often.

This skews towards more accurately labeling people as treated if they have recently entered college or never went to college, as these are the people least likely to move out of state. Therefore, my results trying to evaluate the impact of treatment on those who are well into their twenties are most likely the least accurate for those who attended college. Nevertheless, I explore these results.

3.2.2 Individuals Listed as Dependents

For most individuals in the dataset, I do not know the income of the parents. Parental socioeconomic status is a strong predictor of college enrollment (Desilver 2014) (Delaney 1998), and would ideally be contained in all of my models. Additionally, as explained above, it is far more accurate to know the location of the parents when designating whether an individual went to high school in a treated state, as most individuals would have attended high school in the state their parents live. Therefore, to both control for parental income and more accurately designate individuals as treated, I run one sent of regressions on freshmen-aged and college-aged individuals listed as dependents in the American Community Survey [11]to predict if they are currently attending college. This will also serve as a check as to the accuracy of the "never moving" assumption for non-dependents. In Table 6, we can see that the individuals listed dependents skew towards the more wealthy, which may then understate the policy's impact.

[11]The American Community Survey can be sent to the college or parental residence of college-aged individuals. I define "dependents" as anyone who is listed as a non-spousal family member of the head of household.

Table 6: Parental Income of Dependents

Income	Frequency	Percent
Over $250,000	99,053	3.35
Between $150,000 and $250,000	205,627	6.96
Between $100,000 and $150,000	426,372	14.44
Between $75,000 and $100,000	425,202	14.40
Between $50,000 and $75,000	581,866	19.71
Between $25,000 and $50,000	675,454	22.28
Between $10,000 and $25,000	369,391	12.51
Less than $10,000	169,712	5.75
Total	2,952,677	

Source: ACS

4 The Empirical Analysis

I conduct two separate analyses:

1. College-Level Analysis: Using panel IPEDS data where each observation unit is a college, I test whether admissions standards, enrollment, and graduation rates are impacted by the policy. I employ a difference-in-difference design and inspect admissions rate, accepted students' scores, application numbers, enrollment numbers, and graduation rates.

2. Individual-Level Analysis: Using cross-sectional Census ACS data where each observation unit is an individual, I test whether enrollment and graduation rates are impacted by the policy. Each measure is a binary outcome variable indicating whether someone is enrolled in college or has completed college. I employ a difference-in-difference design and inspect college enrollment and completion within each subpopulation. I break down by subpopulation to compensate for the previously described tradeoffs between increasing sample size and increasing measurement error of the treatment itself. Measurement error is reduced the most for freshmen-aged individuals listed as dependents, and highest for those who are the longest out of high school.

4.1 College-Level Analysis

For college c in state $s$during year t

$$Y_{cst} = \beta D_{ct} + \gamma X_{cst} + \delta W_{cs} + \alpha S_{st} + \zeta_s + \pi_t + \chi_{st} + u_{cst} \tag{1}$$

Y_{cst} is the outcome variable, as described below. D_{ct} is college c's intensity of treatment at year t. Therefore, I am specifically interested in the parameter β, reflecting the amount that Y_{cst} increases if the college changes from having no students treated to all students treated that year. In order words, it is the increase in Y_{cst} for each increase in proportion of students treated by 100%. Because D_{ct} is on a scale from 0 to 1, the β can be multiplied by the proportion of students treated in college c at time t to find the amount that Y_{cst} was projected to increase due to the treatment.

X_{cst} are the time variant college characteristics for college c in state s in year t. Time variant college characteristics include in-state tuition and out-of-state tuition. W_{cs} are the time invariant college c's characteristics in state s. I consider time invariant college characteristics to be characteristics of the college as of the year 2000, a base year chosen as pre-NCLB and pre-treatment for all colleges. Time invariant college characteristics include the historical percent of freshmen from racial minority groups, historical admission rate (year 2001 instead of 2000, earliest available year), historical 6-year graduation rate[12], the highest degree offered by the institution, an indicator for if the institution is a military service academy, and an indicator for whether the school is a public, private non-profit, or private for-profit institution. S_{st} are the time variant state characteristics, including state s's GDP per capita at time t and the unemployment rate in state s at year t. ζ_s are the state indicator variables, π_t are the year indicator variables, and χ_{st} are the interactions between each state and year indicator variable.

I have a few different measures for the outcome variable for college c in state $s$during year t, Y_{cst}. In order to test the effect of the policy on the admissions standards, I use a few different measures:

1. Admission rate: Percentage of students that apply who are admitted. When Y_{cst} is the admissions rate, β reflects the increase in the percent of students admitted if the percent of treated students increases by 100%.

2. Twenty-fifth percentile score for admitted students: The 25th percentile score of admitted students to the college. This is done separately for ACT and SAT dominant college, as determined by which test is more frequently sent into the college. When Y_{cst} is the 25th percentile score of the ACT or SAT, β reflects how many points the 25th percentile of admitted students' scores increase if the percent of treated students increases by 100%.

3. Seventy-fifth percentile score for admitted students: Similarly, the 75th percentile score of admitted students to the college. This is done separately for ACT and SAT dominant college, as determined by which test is more frequently sent into the college. When Y_{cst} is the 75th percentile score of the ACT or SAT, β reflects how many points the 75th percentile of admitted students' scores increase if

[12]The IPEDS survey only reports the 6-year graduation rate, no the 4-year graduation rate.

the percent of treated students increases by 100%.

This is to see if by virtue of more students applying to college due to the policy, colleges become more competitive to compensate for the surge in applicants. Alternatively, I can see if colleges lower their admissions standards by accepting students with lower ACT or SAT scores due to the increase in less qualified people applying for colleges.

I then use the same framework to instead test the policy's impact on the student's application, enrollment, and graduation decision. This is measured by:

1. Applicants: The number of undergraduate applications received by the college
2. Enrollment: The number of freshmen enrolling in the college
3. Graduation rate: The 6-year graduation rate for the college

My college-level analysis depends on the β estimate not being contaminated by u_{cst}, that is, hidden college-level characteristics that are not captured by time variant or time invariant college-level characteristics, time variant state characteristics, state fixed effects, time fixed effects, or state-time fixed effects. That is, I expect that $E[u_{cst}|X_{cst}, W_{cs}, S_{st}, \xi_s, \pi_t, \chi_{st}] = 0$. Therefore, $E[Y_{cst}^{D_{cst}=1} - Y_{cst}^{D_{cst}=0}|X_{cst}, W_{cs}, S_{st}, \xi_s, \pi_t, \chi_{st}] = \beta$, and β reflects a change from a college being 0% treated to 100% treated.

4.2 Individual-Level Analysis

In order to approximate the effect of the treatment on college enrollment and completion rates, I use a few different possible outcome variables:

1. College enrollment: Either being currently enrolled in college at the time of the survey or having attended college in the past.
2. College graduate: Possessing a college degree, whether an associate's or a bachelor's degree.
3. College dropout: Having attended but not completed college.

Since all of these outcomes are binary, I use a logistic regression. Suppose that $\pi(Y_{ist})$ is the probability of binary outcome Y_{ist} for person i in state s during junior year of high school t(the outcome being any of the three previously mentioned options). The logistic regression then measures the *odds ratio* of this outcome, as measured by $\log(\frac{\pi(Y_{ist})}{1-\pi(Y_{ist})})$. The regression therefore is:

$$\log(\frac{\pi(Y_{ist})}{1-\pi(Y_{ist})}) = Odds\ Ratio\ of\ Y_{ist} = \beta_0 + \beta_1 D_{st} + \gamma I_{ist} + \alpha S_{st} + \mu t + \lambda t^2 + \zeta_s + \tau\zeta_s t + \eta\zeta_s t^2 + u_{ist} \quad (2)$$

To clarify, if D_{st} is the binary treatment of state $s$during junior year of high school t, then β_1 reflects the odds ratio between those who received the treatment and those who did not. If β_1 is 1.5 and Y_{ist} is college enrollment, this would mean that students who resided in treated states at time $t$have 1.5 times the odds of being enrolled in college as those who did not receive the treatment. If β_1 is 0.5, then students who resided in treated states at time t have 0.5 times the odds of being enrolled in college as those who did not receive the treatment. Any odds ratio above 1 is a positive effect and below 1 is a negative effect. I am mostly interested in the coefficient of β_1, as this is the effect of the treatment controlling for other factors. I also interact D_{st} with some individual characteristics, namely parental income and race, to test if the policy has a different impact on different populations.

In the same model, I_{ist} contains individual characteristics of person i in state $s$who was a junior at time t. This includes race, hispanic origin, physical and cognitive disability, gender, and citizenship status. These are all observable characteristics expected to impact the individual's decision to attend college and expected to differ by state. When the sample is restricted to young adults listed on the survey as dependents of their parents, family income is also included. For individuals not listed as dependents, parental income is not possible to ascertain, so it is not included. S_{st} are characteristics of state s at time t, time t being the year the individual was a junior in high school. This includes GDP per capita, average in-state tuition at a bachelor's degree-granting institution, and consumption per capita as an approximation of cost of living. This is to approximate the resources the state has to invest in education and the costs of obtaining a bachelor's degree for the typical student. ζ_s contains the state indicator variables. Unlike the college-level regressions, since D_{st} is directly measured by the state and year a student was a junior in high school, we cannot include year indicator variables. This would create identification issues. Instead, $t$reflects the cumulative number of years since the individual was a junior in high school, and t^2 is the number of years squared. This makes theoretical sense because throughout the 2000s, I expect overall state quality in education to progressively change, particularly as states adhere to more NCLB standards (implemented in 2001). I include a quadratic term in case this change is non-linear. Although this strategy of approximating changes within states across time is not as flexible as though we had used state-year dummies, it is the best model under the constraints that state-year dummies are simultaneously used to identify treated and control groups.

I run the same model on a number of different population sub-groups:

1. Dependents: Individuals listed in the ACS survey as dependents of an older family member, typically a parent.
2. Freshmen-Aged: Individuals who have been out of high school 0-1 years at the time of the survey. This implicitly makes the assumption students are traditional students, but we expect our policy to effect traditional students more as well.
3. College-Aged: Individuals who have been out of high school 0-4 years at the time of the survey.
4. Past College-Aged: Individuals who have been out of high school at least 4 years at the time of the survey.
5. Past High School-Aged: Individuals who are out of high school at the time of the survey.

In order to be part of the studied subsample, individuals must have been a junior in high school between the years of 2000 and 2012. The earliest treated individuals were high school juniors in 2001 in either Illinois or Colorado.

I cannot see both treated and control individuals from the same state and the same time, so I capture state effects with state dummies and time effects using a continuous time variable to indicate the time in which the survey was taken both nationwide and within each state, and can average out that $E[u_i] - E[u_{i'}] = 0$ and $E[I_i] - E[I_{i'}] = 0$. Therefore, $E[Y_{ist}^{D=1} - Y_{ist}^{D=0} | \, junior \ in \ HS \ between \ 2000 \ and \ 2012] = \beta$, and β reflects the average treatment effect from the policy. I expect unobservables to be randomly distributed across individuals, or to be able to be captured by either state fixed effects or continuous time variables. Since I have restricted our sample to people who were high school juniors between the years of 2000 and 2012, I am identifying the effect of taking a mandatory, free ACT or SAT test on college enrollment as opposed to needing to elect to take the test in order to attend college. I similarly identify effects on college attainment.

5 Results

5.1 College-Level Results

Using college-level IPEDS data, I examine whether admissions standards are stronger after the implementation of the policy as well as whether application, enrollment, and graduation rates are effected by the policy. Since β reflects the change from 0% treated to 100% treated, but the average college that is treated is treated at 20%, the average treatment effect is 20% of the "intensity of treatment" value. This is reported in a row labeled "Average Treatment" without t-test statistics. *To clarify, "Average Treatment" is not a separate regressor*; it is merely more easily reporting the typical treatment at the typical college from the policy. Tables 7-9 contain the β values of model 1 showing how the admissions standards are impacted by the policy and Table 10 contains the β values showing how enrollment and graduation patterns are impacted by the policy. The other controls outlined in model 1 are present in these models, and more detailed results can be found in the appendix.

In table 7, I examine the impact of the policy on the accepted ACT or SAT scores. It should be noted that I can only view the twenty-fifth and seventy-fifth percentile scores, rather than the true distribution of scores accepted. This limits my distribution by only being able to approximate how the average lower-tier and upper-tier score changes with the policy's introduction. In columns one, three, and four, we can see that the twenty-fifth percentile score does not change with the introduction of the policy. However, for both the ACT and SAT, the seventy-fifth percentiles score changes. This indicates more highly qualified applicants are being accepted to colleges with the introduction of the policy. Specifically, the seventy-fifth percentile of the ACT is increased by 1.2 points, or approximately five percentile points, by a 100% increase in the treated population. The typical increase from the policy, reflecting that the average college is 20% treated, is only 0.25 points, or approximately a one percentile increase in ACT scores. The policy induces an increase of 27 to 36 points on each section of the SAT for students in the seventy-fifth percentile, or roughly eight percentile points, by a 100% increase in treated individuals. The typical treated school sees a one to two percentile point increase in each of the two sections of the SAT, or two to four percentile points overall. The increase in seventy-fifth percentile scores but not twenty-fifth percentile scores can indicate that more highly qualified applicants are applying to colleges, shifting the mean and median to be higher without changing the lower tail of the distribution. Alternatively, the college could still be accepting the same quality of lower-qualified students who otherwise have special traits, such as being athletes or having the income to pay full tuition, but may be increasing the standards for other students applying. The full distribution of scores would be needed to parse out these effects.

Since the change in ACT scores is different than the change in SAT scores, I inspect if the effect on the admissions rate is different for ACT or SAT dominant schools in table 8. I may expect that if the SAT's scores are changed more by the policy than the ACT's, that SAT-dominant schools will have increased admissions standards more than ACT-dominant schools. I define a school as being dominated by the ACT if more students send in an ACT score than an SAT score and vice versa. I find that regardless of the subsample studied, there is no statistically significant change in the admissions rate. However, there is some evidence that more highly qualified students are applying for colleges due to the policy, as reflected by the admitted SAT scores increasing and the seventy-fifth percentile of both ACT and SAT scores increasing when all schools are considered. The policy may be improving students' scores themselves, but the ability for less qualified students to get into colleges has not be adversely effected. Colleges may accept more highly qualified students with the expectation few will matriculate, making the application process for college still the same level of competitiveness. Merit-Based scholarships, however, may then be impacted by the policy if more highly-qualified students are applying when treated.

Table 7: Admissions Standards: ACT and SAT Scores, All Colleges, IPEDS

	ACT Composite 25th Percentile	ACT Composite 75th Percentile	SAT Maths 25th Percentile	SAT Verbal 25th Percentile	SAT Maths 75th Percentile	SAT Verbal 75th Percentile
Intensity of Treatment	0.799	1.257*	15.09	22.40	26.80*	34.51*
	(1.34)	(2.21)	(1.13)	(1.41)	(2.14)	(2.17)
Average Treatment	0.1598	0.2514*	3.018	4.48	5.36*	7.102*
R^2	0.715	0.692	0.739	0.714	0.713	0.675
Adjusted R^2	0.700	0.674	0.724	0.698	0.697	0.657
Observations	12956	12950	13089	13006	13087	13007

t statistics in parentheses. "Average Treatment" is not an additional regressor.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

Table 8: Admissions Standards: Admissions Rate, Various Subsamples, IPEDS

	Admissions Rate Rate	Admissions Rate, ACT Dominant	Admissions Rate, SAT Dominant	Admissions Rate, Not Missing	Admissions Rate, Not Missing	Admissions Rate, Missing ACT and SAT

	All	Colleges	Colleges	ACT Data	SAT Data	Data
Intensity of Treatment	0.0358	0.0555	-0.0766	0.0471	0.0306	0.0306
	(1.06)	(1.30)	(-1.01)	(1.38)	(0.83)	(0.83)
Average Treatment	0.00716	0.0111	-0.01532	0.00942	0.00612	0.00612
R^2	0.379	0.330	0.551	0.475	0.497	0.497
Adjusted R^2	0.356	0.282	0.527	0.446	0.469	0.469
Observations	19140	6506	9692	12958	13092	13092

t statistics in parentheses. "Average Treatment" is not an additional regressor.

$^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

However, rather than measuring the change in the ACT or SAT exam by a singular measure of the ACT and SAT policy combined, I can separately measure the ACT or SAT policy. Each intensity of treatment variable is identical except it only adds up the students historically from ACT-treated states or SAT-treated states, respectively. Table 9 displays these results, wherein the effect of the SAT policy disappears, being small in magnitude and insignificant. Neither policy has a statistically significant on admissions rates. The 75th percentile of the ACT scores remains statistically significant and still about 1.4 ACT points (about 10 percentile points) by a 100% increase in the treated population, while the 25th percentile score is insignificant and small in magnitude. This is only about a two percentile score increase in the upper tail of the distribution for the ACT for the average college, the average college being 20% treated. These results suggest that the previously found results regarding the SAT were most likely due to the fact that the SAT policy is only implemented in Maine in 2006, Delaware in 2010, Idaho in 2011, and North Carolina in 2012. Therefore, most of the states implemented the ACT in the remaining years, skewing the results of the previously run regressions. Since the most-qualified ACT scores of admitted students still increases with the policy why the SAT does not, the previous results suggesting that the ACT and SAT colleges may be different in nature still holds.

My results suggest that colleges did not become substantially harder to be admitted to due to the policy, as I find no evidence of admissions rates changing. However, there is some evidence that more highly qualified students are applying for colleges due to the policy, as reflected by the admitted SAT scores increasing and the seventy-fifth percentile of both ACT and SAT scores increasing when all schools are considered. The policy may be improving students' scores themselves, but the ability for less qualified students to get into colleges has not be adversely effected. Colleges may accept more highly qualified students with the expectation few will matriculate, making the application process for college still the same level of competitiveness. Merit-Based scholarships, however, may then be impacted by the policy if more highly-qualified students are applying when treated.

Table 9: Admissions Standards: Separate ACT and SAT Treatments, IPEDS Results

	Admission Rate (Percent)	ACT Composite 25th Percentile	ACT Composite 75th Percentile	SAT Maths 25th Percentile	SAT Verbal 25th Percentile	SAT Maths 75th Percentile	SAT Verbal 75th Percentile
Intensity to Treat ACT	0.0428	0.872	1.361*				
	(1.13)	(1.37)	(2.19)				
Average Treatment ACT	0.00856	0.1744	0.2722*				
Intensity to Treat SAT	-0.00571			-2.979	-2.910	-8.450	-3.535
	(-0.09)			(-0.13)	(-0.10)	(-0.38)	(-0.13)
Average Treatment SAT				-0.5958	-0.582	-1.69	-0.707
R^2	0.379	0.715	0.692	0.738	0.713	0.712	0.674
Adjusted R^2	0.356	0.700	0.674	0.724	0.698	0.696	0.656
Observations	19140	12956	12950	13089	13006	13087	13007

t statistics in parentheses. "Average Treatment" is not an additional regressor.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

Since perhaps ACT and SAT states are different in nature, and therefore I control for whether or not a college is an ACT or SAT dominant school in analysis of application, enrollment, and graduation trends (Table 10). It should be noted that this parameter is never statistically significant nor large in magnitude.

As seen in table 10, enrollment did increase as colleges had more treated students, on average by about 443 enrolled students if the percent treated increases by 100%. For the typical college, this is an increase in 88 students. Considering the average number of students is 401, this is a substantial increase. However, our data is extremely skewed. While the mean number of enrolled freshmen students is 401, the standard deviation is 785. The effect on the applicants, while only statistically significant at the 10% rather than 5% level, also increased by an average of 2,378 students per college if the percent treated increases by 100%. For the typical college, this is an increase in 475 applicants. The average number of applicants to a college is 2,586, so this is also a substantial increase. Similarly the applicants are skewed as well, with a standard deviation of 5,407 in comparison to its mean of 2,586. Since applicants increased substantially more than enrollment numbers, it is also likely students may have applied to a wider variety of schools rather than only more students applying to college. However, given the data is at the college-level, it is difficult to parse out if more students were applying to colleges but choosing not to go or if students were applying to more colleges.

Table 10: Applicantion, Enrollment, and Completion, IPEDS Results

	Applicants	Enrolled Students	Graduation Rate
Intensity of Treatment	2377.8^{+}	442.7^{*}	0.0374
	(1.68)	(2.04)	(0.91)
Average Treatment	475.56	88.54	0.00748
R^2	0.553	0.601	0.728
Adjusted R^2	0.533	0.583	0.716
Observations	16192	16175	9987

t statistics in parentheses. "Average Treatment" is not an additional regressor.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

5.2 Individual-Level Results

In Table 11, I examine differing estimations of the treatment effect by changing the subsample studied. Strangely, the significance of the effect is highly dependent on what subsample is studied. If only freshmen-aged individuals (0 to 2 years out of high school) are examined, the effect is only significant at the 10% level if individuals listed as dependents are studied. The effect size is also rather small; if an individual lived in a treated state when they are a junior in high school ("is treated"), they have 1.03 times the odds of attending college than an individual who was not treated. Once people who are not listed as dependents in the survey are included in this sample, the effect size and significance disappears. Contrastingly, when examining all college-aged individuals, there is no statistically significant effect of treatment on dependents. However, when examining all college-aged individuals, treated individuals have 1.03 times the odds of attending college as non-treated individuals. When examining all people who are 18 or older, there is no effect of the treatment in magnitude or significance on college attendance.

In Table 12, I examine college attainment outcomes after restricting the sample size to individuals of at least the age to have completed a college degree in four years after high school graduation. Interestingly, the treatment increases both the odds of being a college dropout and college graduate, and increases the odds of being a college dropout more. Treated individuals have 1.09 times the odds of being a college dropout and 1.07 times the odds of being a college graduate as non-treated individuals. This is hard to reconcile with the previous results that suggested that if college attendance was increased by the policy, it was marginally at best. All college-bound individuals can only either dropout or graduate from college, so these effect sizes should agree with the result that there is no overall effect on being enrolled in college when including all individuals in the sample.

While it is ideal to analyze the effects on the individual level, the nature of our data makes it too difficult to control for enough factors to isolate the effects of the policy. I cannot even observe the income of the individual's parents (unless they are listed as dependents, as is true in the first two columns of Table 11), what type of neighborhood they come from, or any other personal characteristics beyond the very basic demographic factors of ethnicity, disability, citizenship, and gender. While standard controls, these basic social characteristics hardly adequately capture the complex factors influencing an individual's decision to attend college. Additionally, college attendance in competitive and non-competitive college is not differentiated, so it is impossible to see if people are going to competitive colleges more but non-competitive colleges less. The previously discussed issues of identifying if individuals are treated in reality are much larger for the individual-level data than the college-level data, and thus the measurement errors in approximating the treatment may be too large to isolate any effects.

Note these are odds ratios interpretations in tables 11 and 12 (we have already converted log odds ratios to odds ratios). For example, the odds of any college-aged student being enrolled in college given a student takes the free ACT or SAT is 1.03 times the odds that a student is contemporaneously enrolled in college that must elect to take the ACT or SAT.

Additionally, it is useful to see if different populations are differently effected by the policy. More

Table 11: Enrollment, ACS Data

	Freshmen-Aged Dependents Enrollment	Freshmen-Aged All Enrollment	College-Aged Dependents Enrollment	College-Aged All Enrollment	Post-High School Aged Enrollment
Treated	1.0262^{+}	0.9930	1.0140	1.0318^{**}	1.0016
	(0.0141)	(0.0120)	(0.0102)	(0.0086)	(0.0071)
Observations	575995	890902	1132099	1960499	3230912
Pseudo R^2	0.053	0.062	0.056	0.044	0.068

Exponentiated coefficients; Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 12: College Attainment, ACS Data

	College Dropout Post-College Age	College Graduate Post-College Age
Treated	1.0915^{**}	1.0744^{**}
	(0.0131)	(0.0124)
Observations	1588882	1588882
Pseudo R^2	0.014	0.084

Exponentiated coefficients; Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

specifically, I would suspect that if offering the ACT or SAT for free was driving change in behavior, i.e. that students could not afford to take the exam, poorer students would be more effected by the policy. I test this by interacting treatment with race and parental income. When I add these interaction terms, I surprisingly find that there's essentially no statistically significant impact of parental income interacted with the policy. The policy has the same impact on wealthy and poor students. At the 10% level, it is possible that the policy interacted with being upper middle income ($50,000-$75,000) increases the odds of attending college in comparison to being extremely wealthy (over $250,000 income) for college-aged individuals (but not freshmen-aged). Beyond this, none of the interacted income terms have a statistically significant impact.

The interacted effects of race are similar. For freshmen-aged individuals, there are only statistically significant impacts on individuals who are Japanese or "Other Race". The effects of being Japanese are due to the extremely small sample size of Japanese individuals, wherein only one-tenth of a percent of treated individuals are Japanese. Students who are other race and treated are less likely to attend college than white individuals who are treated. What groups other race precisely comprises of is unclear, since multiracial is an option on the survey. Therefore, these effects may be picking up more on the uniqueness of individual who distrusts the Census and chooses not to report their race. Additionally, when inspecting the regressions for college-aged rather than only freshmen-aged individuals, Black individuals are also less likely to attend college when treated than White individuals who are treated. These results suggest that the policy may not be targeting racial minorities effectively, despite NCLB's and the policy's hopes of impacting minorities who are less likely to attend college. However, evidence for this is extremely limited, and it seems more likely the policy roughly equally impacted different socioeconomic statuses.

Table 13: Enrollment, ACS Data, Interacted

	Freshmen Aged Dependents	Freshmen Aged Dependents Interact Income	Freshmen Aged Dependents Interact Race	College Aged Dependents	College Aged Dependents Interact Income	College Aged Dependents Interact Race
Treated	1.0226	0.9881	1.0426**	1.0116	0.9477	1.0353**
	(0.0142)	(0.0652)	(0.0157)	(0.0103)	(0.0468)	(0.0115)
White	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)	(.)
Black	0.8278**	0.8278**	0.8325**	0.8325**	0.8325**	0.8396**
	(0.0100)	(0.0100)	(0.0105)	(0.0069)	(0.0069)	(0.0072)
Native American	0.6116**	0.6116**	0.6048**	0.5896**	0.5896**	0.5857**
	(0.0233)	(0.0233)	(0.0237)	(0.0158)	(0.0158)	(0.0161)
Chinese	2.8415**	2.8414**	2.8653**	3.0861**	3.0861**	3.1074**
	(0.1177)	(0.1178)	(0.1222)	(0.0870)	(0.0870)	(0.0897)
Japanese	1.8353**	1.8360**	1.9772**	1.9700**	1.9697**	2.0419**
	(0.1918)	(0.1917)	(0.2124)	(0.1369)	(0.1368)	(0.1448)
Other Asian	1.9875**	1.9879**	1.9972**	2.0003**	2.0003**	1.9968**
	(0.0437)	(0.0437)	(0.0452)	(0.0295)	(0.0295)	(0.0302)
Other race, nec	0.9177**	0.9177**	0.9293**	0.9160**	0.9160**	0.9269**
	(0.0167)	(0.0167)	(0.0173)	(0.0115)	(0.0115)	(0.0119)

Two major races	0.9564*	0.9563*	0.9558*	1.0045	1.0045	1.0047
	(0.0202)	(0.0202)	(0.0210)	(0.0150)	(0.0150)	(0.0156)
Three or more major races	0.9160	0.9159	0.9139	0.9656	0.9655	0.9588
	(0.0585)	(0.0585)	(0.0601)	(0.0428)	(0.0428)	(0.0436)
Family Income Over $250,000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)	(.)
Family Income Under $250,000,	1.0614*	1.0669**	1.0615*	0.9490**	0.9470**	0.9487**
Over $150,000	(0.0248)	(0.0262)	(0.0248)	(0.0158)	(0.0165)	(0.0158)
Family Income Under $150,000,	0.8903**	0.8861**	0.8902**	0.7654**	0.7607**	0.7652**
Over $100,000	(0.0190)	(0.0199)	(0.0190)	(0.0117)	(0.0121)	(0.0117)
Family Income Under $100,000,	0.7141**	0.7121**	0.7141**	0.5996**	0.5967**	0.5995**
Over $75,000	(0.0153)	(0.0160)	(0.0153)	(0.0092)	(0.0096)	(0.0092)
Family Income Under $75,000,	0.5696**	0.5670**	0.5696**	0.4813**	0.4775**	0.4813**
Over $50,000	(0.0120)	(0.0125)	(0.0120)	(0.0073)	(0.0075)	(0.0073)
Family Income Under $50,000,	0.4341**	0.4328**	0.4340**	0.3748**	0.3733**	0.3747**
Over $25,000	(0.0092)	(0.0096)	(0.0092)	(0.0057)	(0.0059)	(0.0057)
Family Income Under $25,000,	0.3270**	0.3245**	0.3269**	0.2891**	0.2871**	0.2891**
over $10,000	(0.0074)	(0.0077)	(0.0074)	(0.0047)	(0.0048)	(0.0047)
Family Income Under $10,000	0.2948**	0.2956**	0.2949**	0.2672**	0.2662**	0.2673**
	(0.0077)	(0.0081)	(0.0077)	(0.0049)	(0.0051)	(0.0049)
Family Income Over $250,000		1.0000			1.0000	
× Treated		(.)			(.)	
Family Income Under $250,000,		0.9471			1.0272	
Over $150,000 × Treated		(0.0756)			(0.0604)	

Family Income Under \$150,000,	1.0531		1.0777	
Over \$100,000 × Treated	(0.0759)		(0.0579)	
Family Income Under \$100,000,	1.0315		1.0612	
Over \$75,000 × Treated	(0.0752)		(0.0576)	
Family Income Under \$75,000,	1.0528		1.1047^{+}	
Over \$50,000 × Treated	(0.0754)		(0.0590)	
Family Income Under \$50,000,	1.0305		1.0481	
Over \$25,000 × Treated	(0.0738)		(0.0559)	
Family Income Under \$25,000,	1.0961		1.0903	
Over \$10,000 × Treated	(0.0841)		(0.0625)	
Family Income Under \$10,000	0.9636		1.0419	
× Treated	(0.0847)		(0.0681)	
White		1.0000		1.0000
× Treated		(.)		(.)
Black		0.9353		0.8966**
× Treated		(0.0385)		(0.0268)
Native American		1.3192		1.2133
× Treated		(0.2236)		(0.1516)
Chinese		0.8758		0.8874
× Treated		(0.1555)		(0.1171)
Japanese		0.2083**		0.4075*
× Treated		(0.0915)		(0.1464)
Other Asian		0.9346		1.0531
× Treated		(0.0804)		(0.0643)

Other race, nec			0.8347**			0.8405**
× Treated			(0.0513)			(0.0365)
Two major races			1.0136			1.0031
× Treated			(0.0777)			(0.0565)
Three or more major races			1.0738			1.1590
× Treated			(0.2873)			(0.2316)
Observations	575995	575995	575995	1132099	1132099	1132099
Pseudo R^2	0.062	0.062	0.062	0.063	0.063	0.063

Exponentiated coefficients; Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

6 Conclusion

6.1 Summary

Based upon the more reliable college-level results, there is some evidence that enrollment and applications to college increased without an adverse effects on graduation rates. Students are likely underestimating their own abilities to apply to and succeed in college, deciding to not even take the exam early in the process. A 2004 survey showing that 32% of urban seniors had taken either the ACT or SAT, while 98% of suburban students had (Avery and Kane 2004). This suggests that many students in areas that do not have a strong college culture do not believe they are capable of succeeding on the exam well enough to be competitive in the college admissions process. My results suggest this is true, as not only the application to but also enrollment in college increases from the increase in applicants taking a mandatory ACT or SAT exam. Furthermore, there is little evidence to suggest that colleges have been modifying their admissions standards in response to more students applying to and attending college. My limited evidence may suggest that more highly-qualified students are applying to and being accepted to colleges, particularly ACT colleges, but that lower-qualified students are not being adversely effected by this shift. This means that any students who are induced to take the exam via the policy are not entering into a substantially more difficult college competition process than students in untreated states.

From a policy standpoint, this means the tests likely worked in the way they were intended: increasing college applications and enrollment without harming students' chances to be competitive in the admissions process or succeed in college. Additionally, my results add further evidence to the literature that students often have imperfect information and assumptions when navigating the college admissions process. If students were better able to gage their own abilities to be competitive in the college application process, forcing them to take the ACT or SAT exam for free should have little to no effect on college application rates because the instantaneous costs should not dramatically change the students' decision making process. Future education policy reforms can take this possibility into account more, seeking out ways to either give students more information or nudge their behavior with small gestures like a free college admissions exam.

6.2 Future Analysis

As previously discussed, my individual-level analysis has many issues with measurement accuracy and adequate control variables. Since college-level analysis cannot tell what types of students are being induced to attend colleges, it would be useful if future analysis could more accurately use individual data. This could be gained from proprietary data that tracks student outcomes for those who take the exam. Additionally, while my analysis suggested there was a difference between SAT and ACT states, it did not adequately explore this difference further or the differences between individual state policies. While some of these differences are discussed in the appendix, an empirical analysis of the differences would be useful to parse out the different effects between mandating the ACT or SAT and offering it for free. For example, the inclusion or exclusion of pre-exams in students' freshmen and sophomore years would be an additional modification to the student's decision model, as they would have data on their likelihood to be qualified to attend college much earlier in their high school career. This creates a college centric culture much earlier in the student's life, as well as gives them the opportunity to change their habits if they are not performing as well as they would like as early as freshman year. High school-level data, or exploiting the differences between the pre-tests mandated by state policies, would be useful to do this type of analysis. My analysis suggest that while the majority of the college application process requires initiative on the part of the student , students do this without necessarily understanding the full cost-benefits analysis of applying. More rigorously analyzing differences between schools or states is necessary to conclude if students behavior is being primarily changed by additional information earlier in their high school career or by waiving the test fee itself.

7 References

"Average ACT Scores by Grade at Time of Testing." 2015. ACT Research & Policy. http://www.act.org/content/-dam/act/unsecured/documents/Info-Brief-2014-21.pdf.

Avery, Christopher, and Thomas J. Kane. 2004. "Student Perceptions of College Opportunities. The Boston COACH Program." NBER, September, 355–94.

Carl Straumsheim. 2017. "IPEDS Survey Misses Thousands of Online Students, but Often Counts Too Many." Inside Higher Ed. Accessed March 28. https://www.insidehighered.com/news/2014/09/26/ipeds-survey-misses-thousands-online-students-often-counts-too-many.

Carnevale, Anthony P, Stephen J Rose, and Ban Cheah. 2011. The College Payoff: Education, Occupations, Lifetime Earnings. Georgetown University Center on Education and the Workforce. https://eric.ed.gov/?id=ED524299.

Cohn, D'Vera, and Rich Morin. 2008. "Who Moves? Who Stays Put? Where's Home?" Pew Research Center's Social & Demographic Trends Project. December 17. http://www.pewsocialtrends.org/2008/12/17/who-moves-who-stays-put-wheres-home/.

Delaney, Anne Marie. 1998. Parental Income and Students' College Choice Process [Microform] : Research Findings To Guide Recruitment Strategies. AIR 1998 Annual Forum Paper / Anne Marie Delaney. [Washington D.C.]: Distributed by ERIC Clearinghouse. http://www.eric.ed.gov/contentdelivery/servlet/-ERICServlet?accno=ED422810.

Desilver, Drew. 2014. "College Enrollment among Low-Income Students Still Trails Richer Groups." Pew Research Center. January 15. http://www.pewresearch.org/fact-tank/2014/01/15/college-enrollment-among-low-income-students-still-trails-richer-groups/.

Engle, Jennifer, and Vincent Tinto. 2008. Moving Beyond Access: College Success for Low-Income, First-Generation Students. Pell Institute for the Study of Opportunity in Higher Education. https://eric.ed.gov/?id=ED504448.

"Executive Summary of the No Child Left Behind Act of 2001." 2007. Legislative Materials; Abstracts. November 20. https://www2.ed.gov/nclb/overview/intro/execsumm.html.

Gebeloff, Gregor Aisch, Robert, and Kevin Quealy. 2014. "Where We Came From and Where We Went, State by State." The New York Times, August 14. https://www.nytimes.com/interactive/2014/08/13/upshot/where-people-in-each-state-were-born.html.

Goodman, Sarena. 2016. "Learning from the Test: Raising Selective College Enrollment by Providing Information." The Review of Economics and Statistics 98 (4): 671–84.

Huchton, Marie. 2011. "Predicting 11th Grade Colorado ACT Performance Using 9th and 10th Grade CSAP Results." https://www.cde.state.co.us/sites/default/files/documents/research/download/pdf/predictingactusingcsap_

Hurwitz, Michael, Jonathan Smith, Sunny Xinchun Niu, and Jessica Howell. 2015. "The Maine Question: How Is Four-Year College Enrollment Affected by Mandatory College Entrance Exams?" SSRN Scholarly Paper ID 2757209. Rochester, NY: Social Science Research Network. https://papers.ssrn.com/abstract=2757209.

Klasik, Daniel. 2016. "The ACT of Enrollment." Educational Researcher, June. doi:10.3102/0013189X12474065.

Lewin, Tamar. 2013. "More Students Are Taking Both the ACT and SAT." The New York Times, August 2. http://www.nytimes.com/2013/08/04/education/edlife/more-students-are-taking-both-the-act-and-sat.html.

Long, Bridget. 2004. "How Have College Decisions Changed over Time? An Application of the Conditional Logistic Choice Model." Journal of Econometrics 121 (1–2): 271–96.

Mayer, Susan E., Ariel Kalil, Philip Oreopoulos, and Sebastian Gallegos. 2015. "Using Behavioral Insights to Increase Parental Engagement: The Parents and Children Together (PACT) Intervention." Working Paper 21602. National Bureau of Economic Research. doi:10.3386/w21602.

National Center for Education Statistics. 2017. "IPEDS : Integrated Postsecondary Education Data System." https://nces.ed.gov/ipeds/.

Pallais, Amanda. 2013. "Small Differences That Matter: Mistakes in Applying to College." Working Paper 19480. National Bureau of Economic Research. http://www.nber.org/papers/w19480.

"Published Prices — National for Trends in College Pricing - Trends in Higher Education - The College Board." 2017. Accessed March 29. https://trends.collegeboard.org/college-pricing/figures-tables/published-prices-national.

Saget, Bedel. 2013. "Where the SAT and ACT Dominate." The New York Times. http://www.nytimes.com/-interactive/2013/08/04/education/edlife/where-the-sat-and-act-dominate.html.

Shaul, Marnie S. |Ganson. 2005. "The No Child Left Behind Act of 2001: The Federal Government's Role in Strengthening Accountability for Student Performance." Review of Research in Education 29: 151–65.

Soldner, Matthew, Cameron Smither, Kelle Parsons, and Audrey Peek. 2016. "Toward Improved Measurements of Student Persistence and Completion." Text. American Institutes for Research. May 16. http://www.air.org/resource/toward-improved-measurements-student-persistence-and-completion.

"State Reports – 2016 SAT Suite Program Results – The College Board." 2017. Accessed March 28. https://reports.collegeboard.org/sat-suite-program-results/class-of-2016-results/state-reports.

Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. 2015. "Integrated Public Use Microdata Series: Version 6.0 [Dataset]." Minneapolis, University of Minnesota. doi:http://doi.org/-10.18128/D010.V6.0.

The College Board. 2017. "Published Prices: National for Trends in College Pricing, Trends in Higher Education." Accessed February 13. https://trends.collegeboard.org/college-pricing/figures-tables/published-prices-national.

"The Condition of College and Career Readiness 2016." 2017. ACT. Accessed March 28. http://www.act.org/-content/act/en/research/condition-of-college-and-career-readiness-2016.html.

US Department of Commerce, B. E. A. 2017. "Bureau of Economic Analysis." Accessed February 13. https://www.bea.gov/regional/.

8 Appendix

8.1 A Model for the College Application and Enrollment Decisions of a Student

I use a variation of a worker's learning model to describe how the student makes her college attainment decisions. The crux of the model is that the student may be uncertain of her true ability and her test score in a college-entry test and the colleges' acceptance decisions may provide her more information about her unknown true ability. The status quo pre-adoption of the policy is that students must opt to take a college entrance exam, requiring they already have the notion that they may be successful enough in a competitive college[13] to make taking the exam worth the physical, opportunity, and psychological costs. Under the policy, the student knows more information from the exam's score about her abilities to better optimize her college decision. Additionally, the policy reduces the physical costs associated with taking the exam are reduced to zero, decreasing the costs associated with test taking. Thirdly, the policy may induce schools to teach-to-the-test, evidence for which is presented in section 8.2.1, reducing the student's costs in exam preparation and potentially improving her scores. Lastly, the policy may impact colleges acceptance probabilities because it may change the characteristics and size of the applicant pool.

I will start by drawing a time line of the student's decision problem which unfolds over a finite horizon. I then present the student's decision problem in each period.

8.1.1 The Student's Decision Time Line

The student's decision timeline without the policy in place is outlined in figure 4, and the modification of her decision is illustrated in figure 5. The timeline for her college decision process begins in the spring of her junior year or fall of her senior year, denoted as period one ($t = 1$). This is when she chooses whether to take a college entry exam. If she takes the exam, she *learns* about her own abilities from her standardized test score (denoted s). In mid to late fall senior year, period two ($t = 2$), she decides whether to apply to a competitive college, noncompetitive college, or no college. A competitive college is defined as any college that requires a college entrance exam, so if she did not decide to take a college entrance exam, this option is not available to her. If the student applies to a competitive college during period 2, she gains *learns* about her abilities again from the colleges' acceptance decision early spring senior year. Accordingly, period three ($t = 3$) correspond to the spring of her senior when she deicides whether to enroll in college or go to the workforce. The final period ($t = 4$, not shown) in the student's horizon comprises of the post-high school

[13]Again, competitive college reflects colleges requiring the ACT or SAT to be admitted versus being open enrollment.

years.

8.1.2 The Student's Initial Conditions

At the start of her decision horizon (spring of junior year, early fall of senior year) a student is characterized by her innate ability (denoted by θ), her perceptions of her ability (denoted by $f_0^{\theta}(.)$), and her parent's characteristics (denoted by I) which include the student's state of residence during her high school years. Both innate ability and parent's characteristics are assumed to be time invariant; the former is not known to the student while the latter is. That is, all the student knows is that her ability is a draw from the probability distribution function (pdf) $f_0^{\theta}(.)$ where the subscript "0" reflects the fact that this pdf captures the student's beliefs at the start of $t = 1$. These beliefs may evolve over time as information is received.

8.1.3 The Student's Decision Problem

Consider a student at the start of period $t = 1$. Her information set contains $\Omega_0 = \left(I, f_{\theta}^{0}(.)\right)$ and the indicator D which denotes the policy scenario: $D = 1$ when the college-entry test is free and mandatory and zero otherwise. The student makes decision sequentially over time. Likewise information is revealed over time depending on the choices made by the student and the colleges.

The Student's Test Taking Decision at the Start of Period $t = 1$ In the spring of junior year / early fall of senior year ($t = 1$), a student chooses whether to take a college-entry test in order to maximize her life-time utility: $d^s = 1$ if she takes the test, zero otherwise. Formally, given (Ω_0, D), the student solves this problem:

$$\max_{d^s \in \{0,1\}} u_1\left(d^s; D\right) + W_1\left(\Omega_0, d^s; D\right) \tag{3}$$

where $u_1\left(d^s; D\right)$ and $W_1\left(d^s, \Omega_0; D\right)$ are, respectively, the instantaneous (indirect) utility and the continuation utility values associated with choice alternative d^s under policy regime D. When the entry-college test is not free ($D = 0$), $u_1\left(d^s; 1\right)$ captures the monetary, time, and psychological costs of taking the test: any charge connected with and time spent preparing for the test (e.g. tutorials), the fees charged by the company that administers the test, the monetary and time expenses to go to the testing location, the fear of receiving a poor score, etc.. When taking the test is free and mandatory ($D = 1$), I let $d^{s*} = 1$ for all students. This reflect that taking the test is no longer a choice. The monetary costs of the exam itself have been reduced to zero, and many extraneous costs such as studying have likely also been supplemented by the school.

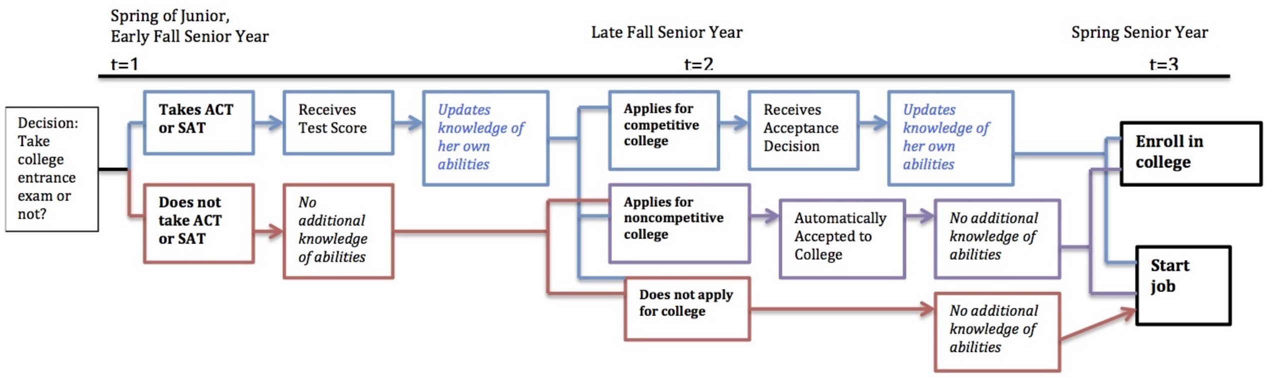

Figure 4:
Student's Decision Without Policy

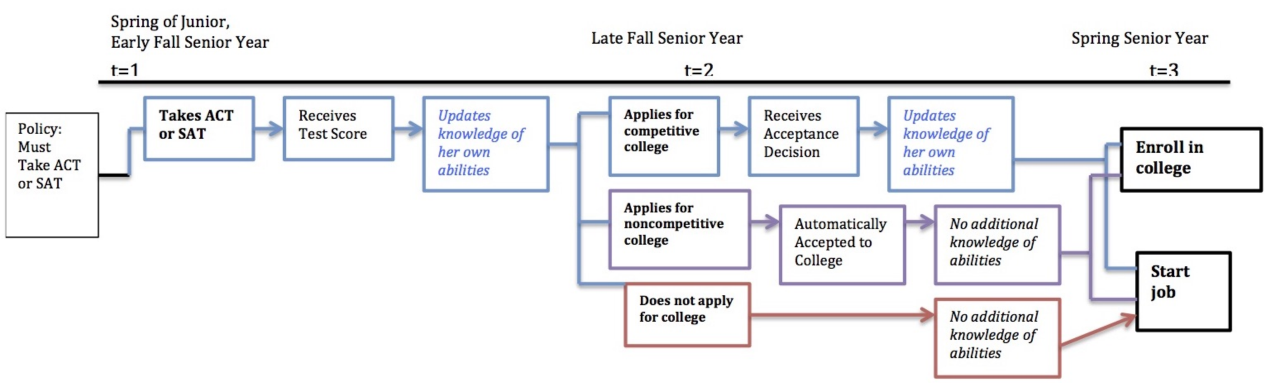

Figure 5:
Student's Decision With Policy

The Student's Test Score Realization at the End of Period $t = 1$ At the end of period $t = 1$ a student who chose to take the college-entry test learns her test score. The test score is a noisy measure of the student's ability:

$$s = \mu + \varepsilon \tag{4}$$

where ε is the noise. The student observes the realization of T but does not know either μ or ε, that is, ε is a random variable. Because the test score depends on μ, it contains information about a student innate ability. Accordingly, the student uses such information to update her beliefs about her ability. If the student presumes that the policy itself improves her test score in the form of teaching to the test, then the test score is an even noisier measure of her true ability:

$$s = \mu + \alpha D + \varepsilon \tag{5}$$

where α is the change in test score, for given student's ability, due to the school's response to the policy. I assume that α is known to the student.

Regardless if she chooses to take the exam or takes it due to the policy, she updates her beliefs accordingly. Therefore, we have that $f_1^\theta(.) \neq f_0^\theta(.)$ if and only if she took the college entrance exam and therefore knows more information regarding her true abilities. Otherwise, perception of her abilities remains unchanged.

The Student's College Application Decision at the Start of Period $t = 2$ At the start of period $t = 2$ the information set of a student contains $\Omega_1 = \left(I, f_1^\theta(.), d^s, s\right)$ and D. Let d^p denote the student's college application choice, which has three options: apply to competitive colleges ($d^p = 2$), non-competitive colleges ($d^p = 1$), or not to apply to college ($d^p = 0$). A student who did not take the college entry test ($d^s = 0$) can only apply to non-competitive colleges or choose not to apply to college by definition. A student optimizes:

$$\max_{d^p \in \{0,1,2\}} u_2(d^p) + E\left[W_2(\Omega_2; D) | \Omega_1, d^p\right] \tag{6}$$

where $u_2(d^p)$ and $E\left[W_2(\Omega_2; D) | \Omega_1, d^p\right]$ are, respectively, the instantaneous and continuation utility values associated with choice alternative d^p under policy regime D, and $\Omega_2 \equiv (\Omega_1, d^p, d^a)$. I think of $u_2(1)$ as capturing the financial and time costs associated with applying to colleges (e.g. fees, preparing a personal statement, polishing one's CV, visiting campuses etc.). Accordingly, I normalize $u_2(0)$ to zero. Observe that these costs are policy-invariant, that is, D is not an argument of $u_2(.)$. The expectation term is present to reflect that at the time when the student applies to colleges she does not know whether her application will be accepted ($d^a = 1$) or rejected ($d^a = 0$), as outlined in the next paragraph.

The College's Acceptance Decision at the end of Period $t = 2$ I let d^a denote the college's acceptance choice, such that the college can either accept the student's application ($d^a = 1$) or reject it ($d^a = 0$). If a student applies to a non-competitive college, $d^a = 1$ by definition. Therefore, a college's admissions decision is represented as follows:

$$\Pr(d^a = 1 | s, f_1^\theta(.), d^p; D) = \begin{cases} 1 & \text{if } d^p = 1 \\ p^a\left(s, f_1^\theta(.); D\right) & \text{if } d^p = 2 \end{cases} \tag{7}$$

where $p^a\left(s, f_1^\theta(.); D\right)$ is the probability (possibly less than one) that a student with test scores s is admitted by a competitive college under policy regime D. I can write out the continuation value $E\left[W_2(\Omega_2; D) | \Omega_1, d^p\right]$as follows:

$$E\left[W_2(\Omega_2; D) | \Omega_1, d^p\right] = \begin{cases} W_2(\Omega_1, d^p, 0; D) & \text{if } d^p = 0 \\ W_2(\Omega_1, d^p, 1; D) & \text{if } d^p = 1 \\ \begin{array}{c} p^a\left(s, f_1^\theta(.); D\right) W_2(\Omega_1, d^p, 1; D) + \\ \left(1 - p^a\left(s, f_1^\theta(.); D\right)\right) W_2(\Omega_1, d^p, 0; D) \end{array} & \text{if } d^p = 2 \end{cases} \tag{8}$$

The second row of equation 8 reflects the fact that admission is guaranteed to a student who applies to a non-competitive college. Thus, the continuation value associated with applying to college is that associated with being admitted to such college (which is why the last argument of $W_2(.,.,.; D)$ is set to zero). The third and last row of equation 8 reflects the fact that when a student applies to a competitive college she is accepted with probability $p^a\left(s, f_1^\theta(.); D\right)$. Thus, the continuation value of this choice alternative is a weighted average of the value from being admitted (namely $W_2(\Omega_1, d^p, 1; D)$) and of not being admitted (namely $W_2(\Omega_1, d^p, 0; D)$) where the weights are the acceptance (namely $p^a\left(s, f_1^\theta(.); D\right)$) and rejection probabilities (namely $\left(1 - p^a\left(s, f_1^\theta(.); D\right)\right)$). She learns additional information about her abilities if and only if $d^p = 2$ because the acceptance decision only varies if the student is applying to a competitive college. Therefore, this is the only instance where $f_2^\theta(.) \neq f_1^\theta(.)$. Otherwise, perception of her abilities remains unchanged.

The College Enrollment Decision at $t = 3$ At the start of period $t = 3$ (the spring of the senior year) the information set of a student is $\Omega_3 = \left(I, f_2^\theta(.), d^s, s, d^p, d^a\right)$. Let d^e denote the student's college enrollment choice, where she can either enroll ($d^e = 1$) or not ($d^e = 0$). Trivially, if $d^p = 0$, then $d^e = 0$.

Consider next a student who applied to a college ($d^{p*} \in \{1, 2\}$) and was accepted ($d^{a*} = 1$). Given (Ω_3, D) she chooses whether to enroll as to maximize her life-time utility. Formally, she solves the following problem:

$$\max_{d^e \in \{0,1\}} u_3 (d^e) + W_3 (\Omega_3, d^e; D) \tag{9}$$

where $u_3 (d^e)$ and $W_3 (\Omega_3, d^e; D)$ are, respectively, the instantaneous and continuation utility values associated with choice alternative d^p under policy regime D. I think of $u_3 (1)$ as related to psychic costs (e.g. from not attending college when the students peers do so) or up-front monetary costs related to going to college (e.g. moving costs). I accordingly normalize $u_3 (0)$ to zero.

The Post-High School Continuation Value at $t = 4$ At the start of period $t = 4$ (the end of high school) the information set of a student contains $\Omega_4 = (\Omega_3, d^e)$ and D. I do not explicitly model the continuation value post-high school. Instead, for a student of ability μ I simply represent it with

$$V_4 \left(d^e, f_2^\theta (.), \mu; D\right) \tag{10}$$

which is to be interpreted as the utility value that a student with characteristics $\left(d^e, f_2^\theta (.), \mu\right)$ commands at the time she graduates from high school under policy regime D. The dependence of this utility value on $\left(d^e, f_2^\theta (.), \mu\right)$ reflects the following pathways. The dependence on d^e captures the fact that the returns to college in terms of earnings may be positive and differentiated based on the competitiveness of the college attended, that competitive and non-competitive colleges charge a (possibly differentiated) tuition, and that by attending college a student forgoes labor market earnings for a few years. The dependence on $f_2^\theta (.)$ captures the fact that a student may choose different occupations based on her perceived ability, and returns to education (hence earnings) may differ by occupation. The dependence on true ability reflects the fact that it is a determinant of the student's productivity in the labor market hence of her life-time earnings. Her ability to complete her education is also a function of her ability, impacting her long-term labor earnings. Finally, the dependence on D reflects any general equilibrium effects of the policy. For instance, if more students are induced to go to college the supply of college-educated workers increases and this may affect earnings. The student does not know μ, thus, standing at the start of period $t = 4$, her expected future utility is the expected value of $V_4 \left(d^e, f_2^\theta (.), \mu; D\right)$ taken with respect to true ability, that is, using $f_2^\theta (.)$ as the pdf for the integration:

$$W_3 (\Omega_3, d^e; D) \equiv E \left[V_4 \left(d^e, f_2^\theta (.), \mu; D\right) | \Omega_4\right] \tag{11}$$

8.1.4 The Model Solution

A model solution needs to be constructed in future study. Given parametric and functional form assumptions, the above model can be solved by backward recursion. The model provides a conceptual framework for how I expect the policy to impact college application and enrollment. I expect that a student gains information regarding her own ability by being forced to take the exam, giving her the option to apply for and attend competitive university. Additionally, I expect that more students will be substituting away from non-competitive college to attend competitive college than substituting away from the workforce to attend competitive college. This creates problems when trying to measure competitive college attendance with a measure that includes non-competitive college attendance, as is true in the individual-level empirical analysis.

The likelihood of more students enrolling in colleges depends on whether the information given by the exam induces more students to accurately believe they are more prepared for college than they would have without taking the exam. If students under-predict their own abilities, and the noise on the SAT or ACT as predictors of college success is minimal, then the policy should inform more students that they are qualified to attend college. However, if students are do not under-predict their own abilities, then the SAT or ACT would minimally change their self-perception. Additionally, if the SAT or ACT are extremely noisy to the extent they tell students their abilities are less than the student's prior, then the policy would not induce more students to go to college.

8.2 Additional Policy and Data Information

8.2.1 Detailed Information on Policies

Between the years 2001 and 2015, twenty-three states and the District of Columbia implemented a policy providing mandatory and free college admissions exams to all public high school juniors during the school day (typically a Wednesday in April or May of their junior year). These policies do not apply to students in private schools, but since approximately 90% of the US K-12 population throughout the 2000s has attended public K-12 education, we will treat all individuals as though they went to public school. Furthermore, since we are primarily concerned with the effects on low income or rural students, these populations tend to attend public school, and thus are more likely to be accurately deemed as treated. Students attending private schools are relatively likely to take the ACT and attend university irrespective of treatment, so we do not expect change in their numbers.

I will be treating the ACT and SAT tests as the same treatment, unlike many previous studies have done. While we list states that adopted the policy post-2012 since these states could be incorporated in further analysis with more data, only states with at least two years of the policy (thus allowing juniors to complete their senior year and be in at least their first year of college) are included in our sample.

Taking a pre-college entrance exam in the student's freshman or sophomore year is likely to help improve their score and give the student more information about their true ability earlier on in their high school career. Furthermore, the pre-SAT (PSAT) is also a scholarship competition, so any students doing exceptionally well will be further incentivized by cheaper college.

The individual states' policies in detail:

Illinois: Beginning in spring of 2001, all Illinois juniors take the ACT exam in school. Students are not charged to take the exam. Starting in 2008, Illinois started offering the preparatory pre-tests to the ACT (EXPLORE and PLAN tests) to all high school freshmen and sophomores, respectively, free of charge. This increases student preparation for the exam. Many districts offered these exams before the state policy. As the ACT was used as a component of No Child Left Behind mandates, districts are evaluated based upon their act pass rates. In 2015, the policy was modified to be the SAT instead of ACT, but since our data does not extend this far, it does not effect our analysis.

Colorado: Beginning in spring of 2001, all Colorado juniors take the ACT exam in school. Students are not charged to take the exam. Many districts offer the EXPLORE and Plan exam, but there is no official state policy. This is explicitly because the Colorado Department of Education found that state-created exams were no worse at predicting ACT performance than the ACT company's EXPLORE or Plan exam (Huchton 2011), and thus found no need to purchase additional exams. As the ACT was used as a component of No Child Left Behind mandates, districts are evaluated based upon their act pass rates. In 2015, the policy was modified to be the SAT instead of ACT.

Maine: Beginning in spring 2006, all Maine juniors take the SAT for free. This replaced the previous school assessment test under No Child Left Behind, and thus Maine high schools were evaluated based upon their students' performance on the exam. Additionally, the PSAT (akin to the Explore/Plan test, a pre-SAT test given to sophomores) became state-provided and mandatory for all high school sophomores in autumn 2006. Thus, the first test taken by those who had taken both the PSAT and SAT was in spring 2007. The policy has since been revoked as of 2015, but since my data does not extend this far, it is irrelevant to my analysis.

Wyoming: Beginning in spring 2007, all Wyoming juniors take the ACT. Students also take the EX-

PLORE test in the ninth grade and PLAN test in the tenth to prepare for the ACT. The ACT and pre-ACT exams replace state exams, and thus are used to fulfill NCLB standards of adequate yearly progress.

Michigan: Beginning in spring 2007, all Michigan juniors take the ACT. This replaced the previous school assessment test under No Child Left Behind, and thus Michigan high schools were evaluated based upon their students' performance on the exam. Michigan did not choose to use the EXPLORE or Plan exam until 2012. In 2012, a pilot project testing the EXPLORE and Plan tests was implemented, but these students would be too young to be present in our dataset regardless. The EXPLORE test is also taken only in the eighth grade instead of the ninth grade.

Kentucky: Beginning in spring 2008, all Kentucky juniors take the ACT for free. This policy was accompanied by the mandatory and free explore and plan tests. Thus, beginning in spring 2008, all eighth graders and freshmen take the EXPLORE test, all sophomores take the PLAN tests, and all juniors the ACT. This replaced the previous school assessment test under No Child Left Behind, and thus Kentucky high schools were evaluated based upon their students' performance on the exam. Additionally, if students taking the test do not reach "college readiness" benchmarks, then they are given the opportunity to have additional learning and re-take the test for free as a senior. This is importantly not true in other states.

North Dakota: Beginning in spring 2010, all North Dakota juniors take the ACT for free. Neither the EXPLORE nor Plan exam are required, though some individual districts may still use them.

Tennessee: Beginning in spring 2010, all Tennessee juniors take the ACT for free. Additionally, all freshmen take the EXPLORE test and all sophomores the Plan test.

Delaware: Beginning in spring 2011, all Delaware juniors take the SAT for free. The PSAT is taken in the tenth grade.

Idaho: Beginning in spring 2012, all high school juniors can take the SAT for free. To graduate, they must take either the ACT or SAT, but the SAT is free and in-school. Those who wish to can take the ACT on their own time instead and use it in lieu of the SAT. The PSAT is not mandated.

North Carolina: Beginning in spring 2012, all North Carolina juniors take the ACT for free. The EXPLORE and Plan tests are also taken.

More states began offering the policy after spring of 2013, but I am unable to study them.

Therefore, a condensed list from which we can look at both "potentially currently enrolled people" and "potential college graduates" from each state after treatment for individual-level data:

Table 14: Policy Timing in Each State

State	Type of Exam	Policy Year	Observe Freshmen Enrollment Numbers in IPEDS	Observe Graduation Numbers (within 6 years) in IPEDS	First Year to Identify if Enrolled in University in ACS	First Year to Identify if Graduated from University in ACS
Illinois	ACT	2001	2002	2008	2003	2007
Colorado	ACT	2001	2002	2008	2003	2007
Maine	SAT	2006	2007	2013	2008	2012
Wyoming	ACT	2007	2008	N/A	2009	2013
Michigan	ACT	2007	2008	N/A	2009	2013
Kentucky	ACT	2008	2009	N/A	2010	2014
North Dakota	ACT	2010	2011	N/A	2012	N/A
Tennessee	ACT	2010	2011	N/A	2012	N/A
Delaware	SAT	2011	2012	N/A	2013	N/A
Idaho	SAT	2012	2013	N/A	2014	N/A
North Carolina	SAT	2012	2013	N/A	2014	N/A

8.2.2 Other potential policies not included

In addition to the previously described states, Arkansas offered a free but not mandatory ACT exam starting in spring of 2009. However, because of my own hypothesis that the mandatory part is even more important than the free part of the policy, I opt not to include Arkansas in my analysis. This would be an interesting state to add for future analysis as a state where the students' costs have been reduced to zero but students but still choose to take the exam.

Additionally, while I researched for state-wide initiatives to implement this policy, some individual school districts choose to implement similar policies as well. While it is impossible to know without large amounts of primary source research precisely how many school districts choose to mandate the ACT or SAT. For example, Portland Public Schools requires the ACT, even though other school districts in the Portland metro area and Oregon as a whole do not. This is again not included in my analysis because it would be impossible to know which students specifically graduated from Portland Public Schools.

Due to the decentralized nature of the American public education system, these policies take on different names and can exist in any school district despite overall state policy. Given enough time and resources one could research if every school district decided to adopt a similar policy, but this would require significant time to thoroughly examine all school systems. There may be some students mis-labeled as not treated under both the college and individual-level analysis as a result, but the effect should be minimal because the majority of school districts in the United States did not opt to develop the policy on their own without a state mandate. This is particularly true when considering that the majority of states developed this policy in response to NCLB, wherein all states must develop a statewide measure of adequate yearly progress. Individual school districts cannot develop their own measure, so any school districts who adopt the policy in untreated states are doing so because they believe it is valuable to their students and not to fulfill statewide testing mandates. Therefore, there is less of an incentive beyond philosophical beliefs for individual school districts to adopt a relatively costly policy, since they must not only pay for every student to take the exam but also opt for a day of instruction to be spent taking the exam, so we expect that few school districts in untreated states adopted the policy on their own.

8.2.3 Strength of the Never Moving Assumption

For the individual-level analysis, I treat people as having gone to high school in the location they lived in as of one year prior to the survey, under the assumption people rarely move, and if they move, they do not move particularly often. As of 2011, we have data as to what percentage of college students attended college in-state, and as of 2012, the percentage of people in a state who never move (more details below):

Table 15: Strength of Never Moving Assumption

State	Percent of College Goers to Stay In State	Percent of People Who Stay In State Born In
Illinois	65%	65%
Colorado	75%	64%
Maine	71%	63%
Wyoming	96%	42%
Michigan	81%	72%
Kentucky	80%	69%
North Dakota	86%	47%
Tennessee	70%	71%
Delaware	73%	61%
Idaho	75%	59%
North Carolina	83%	75%

(Data from: Gebeloff, Aisch and Quealy, 2014, summarizing Census data)

Therefore, in all states studied, the vast majority of college students attend college in-state, and the vast majority of adults still stay in the state they were born in. However, college-educated individuals are still the most likely to move from their home state, and these numbers are still low enough to make me doubt that I am correctly identifying if individuals were treated. Specifically, 60% of college graduates have lived in more than one state nationwide while only 34% of those with a high school diploma or less have (Cohn and Morin 2008) according to a 2008 Pew Research poll. This means college graduates specifically are the individuals we are most likely to mis-label as growing up in the state they currently reside in.

8.2.4 Missing Data in IPEDS

As can be easily noticed in the regression results that for several outcome variables, the sample size changes depending on the outcome variable studied. Graduation rate decreases in sample size because the froward-looking 6-year graduation rate is the outcome variable, and this is only available for years 2008 and prior. This can be easily seen by examining table 22, which includes complete regression results minus state effects. The percentage of students who choose to send in ACT or SAT scores is missing for 14,287 observations (when year + school are considered unique identifiers of an observation). Those with missing data skews towards for profit institutions, with 68% of the observations with missing data being for-profit institutions while only 29% of our competitive colleges[14] are for-profit institutions. Most of our missing data comes not from missing outcome variables, however, because applicants and enrollment are only missing for 975 and 748 observations, respectively. Instead, our pre-treatment characteristics are missing for a large portion of the data, with 7,293 observations not having pre-treatment racial characteristics information, for example. I sacrifice sample size in order to maintain the data on pre-treatment characteristics of the colleges. Like the missing ACT and SAT data, colleges that are missing pre-treatment characteristics are substantially more likely to be for profit (53%) and private (88%). Since these are the colleges I expect to be the least impacted by the policy, I believe the effects of the missing data should be minimal. Future analysis should use more complete college-level data for pre-treatment characteristics, or find another way to estimate "static" college characteristics that are not biased by missing data.

8.3 Additional Empirical Results

8.3.1 College-Level Data

More detailed empirical results. I omit state and state times year fixed effects for space considerations.

Table 16: Admissions Standards: ACT and SAT Scores, All Colleges, IPEDS

	ACT Composite	ACT Composite	SAT Maths	SAT Verbal	SAT Maths	SAT Verbal

[14] As a reminder, "competitive college" is defined as non-open admissions college

	25th Percentile	75th Percentile	25th Percentile	25th Percentile	75th Percentile	75th Percentile
Intensity of Treatment	0.799	1.257^{*}	15.09	22.40	26.80^{*}	34.51^{*}
	(1.34)	(2.21)	(1.13)	(1.41)	(2.14)	(2.17)
Admissions Rt, 2001	-3.272^{***}	-2.491^{***}	-81.05^{***}	-79.70^{***}	-71.50^{***}	-74.71^{***}
	(-8.14)	(-6.60)	(-9.48)	(-9.74)	(-9.19)	(-9.20)
Pct Black, 2000	-4.282^{***}	-5.363^{***}	-95.76^{***}	-90.42^{***}	-104.0^{***}	-105.1^{***}
	(-13.42)	(-12.99)	(-13.41)	(-13.08)	(-12.58)	(-12.69)
Pct Native Am, 2000	-2.925^{**}	-4.336^{**}	-53.82^{+}	-47.59	-64.93	-89.96^{+}
	(-3.05)	(-2.97)	(-1.89)	(-1.56)	(-1.40)	(-1.76)
Pct Hispanic, 2000	-5.934^{***}	-6.953^{***}	-119.9^{***}	-103.6^{***}	-124.0^{***}	-117.5^{***}
	(-5.47)	(-7.23)	(-6.05)	(-5.44)	(-6.77)	(-6.46)
Pct Asian, 2000	4.681^{***}	5.420^{***}	143.6^{***}	49.38^{*}	156.7^{***}	65.10^{*}
	(3.93)	(4.57)	(5.94)	(2.44)	(5.49)	(2.33)
Pct Unknwn Race, 2000	-0.559	-0.921	-20.37	2.863	-21.46	-0.216
	(-0.54)	(-1.07)	(-1.01)	(0.13)	(-1.23)	(-0.01)
Tuition in-state	0.000109^{**}	0.0000493	0.00222^{**}	0.00269^{**}	0.00123^{+}	0.00170^{*}
	(2.99)	(1.35)	(2.94)	(3.82)	(1.71)	(2.25)
Tuition out-of-state	0.000221^{***}	0.000225^{***}	0.00436^{***}	0.00352^{***}	0.00430^{***}	0.00370^{***}
	(5.58)	(5.71)	(5.36)	(4.57)	(5.52)	(4.55)
Associates degree	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
2-4 Academic Years	0.0911	-0.192	-21.02	-13.25	-30.24^{**}	-10.87
	(0.23)	(-0.40)	(-1.46)	(-1.48)	(-3.05)	(-0.85)
Bachelors degree	0.791^{**}	1.216^{***}	16.53^{**}	17.26^{**}	17.10^{**}	16.79^{*}

	(3.16)	(4.24)	(2.80)	(2.88)	(3.07)	(2.57)
Postbaccalaureate	0.549	0.677	12.39	19.02	11.87	21.90
	(1.10)	(1.30)	(1.00)	(1.46)	(1.05)	(1.51)
Masters degree	0.694**	1.199***	16.44**	15.92**	15.87**	14.17*
	(2.99)	(4.41)	(2.90)	(2.77)	(2.94)	(2.25)
Post-masters	0.555*	0.895**	17.00**	14.83*	12.99*	8.379
	(2.14)	(3.07)	(2.82)	(2.47)	(2.24)	(1.26)
Doctors degree	1.385***	1.854***	32.64***	27.53***	31.89***	23.67**
	(5.68)	(6.58)	(5.57)	(4.66)	(5.72)	(3.67)
Degree-granting	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Nondegree-granting	0.533	0.112	33.34+	24.23	29.59+	7.959
	(0.49)	(0.11)	(1.72)	(1.55)	(1.88)	(0.45)
Grad Rt, 2000	6.688***	6.225***	142.6***	134.3***	122.8***	120.8***
	(11.87)	(11.51)	(13.50)	(12.47)	(12.00)	(11.31)
Military Academy	6.304***	6.196***	90.86***	59.26**	80.59***	54.61***
	(9.06)	(9.51)	(6.56)	(3.30)	(6.95)	(4.13)
Public	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Non-Profit Private	-2.095***	-1.143***	-53.88***	-47.94***	-39.68***	-34.04***
	(-7.93)	(-4.31)	(-9.44)	(-8.52)	(-7.17)	(-5.72)
For-Profit Private	-3.623***	-2.572***	-87.78***	-78.57***	-76.98***	-59.37***
	(-5.99)	(-5.20)	(-7.07)	(-8.52)	(-7.19)	(-6.77)
Unemployment	0.371	-0.456	19.17	-2.925	-28.77	3.215

	(0.28)	(-0.25)	(0.26)	(-0.05)	(-0.47)	(0.05)
GDP Per Capita	0.000342	-0.000263	0.00315	-0.00432	-0.0156	-0.00281
	(0.65)	(-0.36)	(0.12)	(-0.20)	(-0.69)	(-0.12)
year=2001	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
year=2002	-1.178	1.203	-21.45	13.39	76.13	-21.04
	(-0.49)	(0.35)	(-0.17)	(0.13)	(0.72)	(-0.18)
year=2003	-1.097	0.0827	-0.598	6.627	69.62	-5.175
	(-0.47)	(0.02)	(-0.00)	(0.06)	(0.67)	(-0.05)
year=2004	-1.662	0.723	-35.96	-4.735	119.8	22.55
	(-0.45)	(0.14)	(-0.19)	(-0.03)	(0.77)	(0.13)
year=2005	-1.580	0.708	15.28	42.14	107.0	36.86
	(-0.56)	(0.17)	(0.11)	(0.35)	(0.88)	(0.29)
year=2006	-1.218	0.140	33.82	34.49	80.68	40.25
	(-0.75)	(0.06)	(0.51)	(0.55)	(1.25)	(0.61)
year=2007	-1.228	0.125	22.35	10.18	57.85	36.39
	(-0.94)	(0.07)	(0.47)	(0.21)	(1.25)	(0.81)
year=2008	-1.258	0.279	-6.291	9.716	64.93	42.75
	(-0.69)	(0.11)	(-0.08)	(0.13)	(0.87)	(0.55)
year=2009	-2.449	2.653	-75.25	21.05	167.1	4.321
	(-0.35)	(0.27)	(-0.20)	(0.07)	(0.53)	(0.01)
year=2010	-3.021	4.394	-118.1	33.80	256.7	-10.62
	(-0.28)	(0.29)	(-0.20)	(0.07)	(0.52)	(-0.02)
year=2011	-2.696	3.572	-105.4	10.75	225.7	-21.92

	(-0.30)	(0.28)	(-0.22)	(0.03)	(0.56)	(-0.05)
year=2012	-2.013	2.575	-59.08	17.45	163.8	-3.676
	(-0.32)	(0.29)	(-0.18)	(0.06)	(0.57)	(-0.01)
year=2013	-1.886	2.705	-50.92	21.11	151.6	1.719
	(-0.32)	(0.33)	(-0.17)	(0.08)	(0.58)	(0.01)
R^2	0.715	0.692	0.739	0.714	0.713	0.675
Adjusted R^2	0.700	0.674	0.724	0.698	0.697	0.657
Observations	12956	12950	13089	13006	13087	13007

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

Table 17: Admissions Standards: Admissions Rate, Various Subsamples, IPEDS

	Admissions Rate Rate All	Admissions Rate, ACT Dominant Colleges	Admissions Rate, SAT Dominant Colleges	Admissions Rate, Not Missing ACT Data	Admissions Rate, Not Missing SAT Data	Admissions Rate, Missing ACT and SAT Data
Intensity of Treatment	0.0358	0.0555	-0.0766	0.0471	0.0306	0.0306
	(1.06)	(1.30)	(-1.01)	(1.38)	(0.83)	(0.83)
Admissions Rt, 2001	0.493***	0.363***	0.635***	0.517***	0.565***	0.565***
	(22.63)	(10.40)	(22.07)	(20.71)	(22.74)	(22.74)
Pct Black, 2000	-0.109***	-0.197***	-0.0674*	-0.114***	-0.0918**	-0.0918**
	(-5.02)	(-5.51)	(-2.25)	(-4.56)	(-3.52)	(-3.52)
Pct Native Am, 2000	-0.0638	-0.269*	-0.108	-0.111	-0.0852	-0.0852
	(-0.78)	(-2.04)	(-0.34)	(-1.25)	(-0.63)	(-0.63)
Pct Hispanic, 2000	-0.0635	-0.117	-0.00926	0.0359	-0.0163	-0.0163

	(-1.34)	(-1.15)	(-0.14)	(0.65)	(-0.28)	(-0.28)
Pct Asian, 2000	-0.202*	-0.378**	-0.152+	-0.276***	-0.263***	-0.263***
	(-2.17)	(-2.66)	(-1.79)	(-4.44)	(-3.95)	(-3.95)
Pct Unkwn Race, 2000	-0.120**	0.0689	-0.0477	0.0156	-0.0178	-0.0178
	(-3.47)	(1.39)	(-1.26)	(0.39)	(-0.50)	(-0.50)
Tuition in-state	-0.00000225	5.16e-08	-0.00000514+	-0.00000347	-0.00000534*	-0.00000534*
	(-0.93)	(0.02)	(-1.91)	(-1.54)	(-2.36)	(-2.36)
Tuition out-of-state	-0.00000191	-0.000000282	0.00000102	-0.00000218	0.000000157	0.000000157
	(-0.77)	(-0.09)	(0.36)	(-0.96)	(0.07)	(0.07)
Less than 2 Years Deg	0		0			
	(.)		(.)			
Associates degree	-0.0539	0	-0.171	0	0	0
	(-0.86)	(.)	(-1.20)	(.)	(.)	(.)
2-4 Academic Years	-0.0383	0.0419	-0.235**	0.0828	-0.0160	-0.0160
	(-0.70)	(0.76)	(-2.97)	(0.52)	(-0.14)	(-0.14)
Bachelors degree	-0.0652	-0.0380	-0.158	-0.00522	0.00948	0.00948
	(-1.04)	(-1.23)	(-1.10)	(-0.27)	(0.53)	(0.53)
Postbaccalaureate	-0.0687	-0.0706	-0.189	-0.0431	-0.00125	-0.00125
	(-0.90)	(-1.16)	(-1.23)	(-0.83)	(-0.02)	(-0.02)
Masters degree	-0.0440	-0.0329	-0.158	-0.00312	0.0105	0.0105
	(-0.70)	(-1.09)	(-1.10)	(-0.17)	(0.62)	(0.62)
Post-masters	-0.0433	-0.0156	-0.169	0.0131	0.0167	0.0167
	(-0.68)	(-0.50)	(-1.17)	(0.68)	(0.91)	(0.91)
Doctors degree	-0.0447	-0.0160	-0.165	0.00547	0.0203	0.0203

	(-0.70)	(-0.53)	(-1.14)	(0.29)	(1.17)	(1.17)
Degree-granting	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Nondegree-granting	-0.124**	-0.335**	-0.0480	-0.325+	-0.183	-0.183
	(-3.11)	(-3.76)	(-0.39)	(-1.89)	(-1.50)	(-1.50)
Grad Rt, 2000	-0.0725**	-0.0335	-0.0614*	-0.0710**	-0.0666**	-0.0666**
	(-3.37)	(-0.89)	(-2.32)	(-3.05)	(-2.97)	(-2.97)
Military Academy	-0.280***	0	-0.159**	-0.245***	-0.239***	-0.239***
	(-9.27)	(.)	(-2.94)	(-5.72)	(-7.40)	(-7.40)
Public	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Non-Profit Private	0.00402	-0.0444*	0.0323	0.00772	0.0345*	0.0345*
	(0.24)	(-2.01)	(1.55)	(0.47)	(2.04)	(2.04)
For-Profit Private	-0.0229	-0.0330	0.0455	-0.0115	0.0848*	0.0848*
	(-1.16)	(-0.69)	(0.84)	(-0.28)	(2.01)	(2.01)
Unemployment	0.00448	-0.0575	0.766***	-0.0122	-0.0528	-0.0528
	(0.03)	(-0.41)	(8.48)	(-0.09)	(-0.36)	(-0.36)
GDP Per Capita	0.00000776	-0.0000199	0.000139***	9.93e-08	-5.39e-08	-5.39e-08
	(0.14)	(-0.35)	(4.06)	(0.00)	(-0.00)	(-0.00)
year=2001	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
year=2002	-0.0383	0.0960	-0.507***	-0.000252	0.0439	0.0439
	(-0.16)	(0.40)	(-5.53)	(-0.00)	(0.17)	(0.17)
year=2003	-0.0725	0.0511	-1.122***	-0.0375	-0.0144	-0.0144

	(-0.31)	(0.21)	(-8.42)	(-0.17)	(-0.06)	(-0.06)
year=2004	-0.108	0.0845	-1.345***	-0.0624	-0.0120	-0.0120
	(-0.30)	(0.23)	(-7.69)	(-0.18)	(-0.03)	(-0.03)
year=2005	-0.0985	0.0712	-1.006***	-0.0504	-0.0414	-0.0414
	(-0.34)	(0.25)	(-6.61)	(-0.19)	(-0.14)	(-0.14)
year=2006	-0.130	-0.0183	-1.305***	-0.0950	-0.143	-0.143
	(-0.85)	(-0.12)	(-5.40)	(-0.67)	(-0.96)	(-0.96)
year=2007	-0.167	-0.0952	-1.220***	-0.155	-0.159	-0.159
	(-1.45)	(-0.78)	(-4.28)	(-1.40)	(-1.41)	(-1.41)
year=2008	-0.130	-0.0469	-1.329***	-0.125	-0.181	-0.181
	(-0.72)	(-0.25)	(-4.72)	(-0.73)	(-1.05)	(-1.05)
year=2009	-0.133	0.193	-2.358***	-0.0606	0.0687	0.0687
	(-0.18)	(0.26)	(-5.13)	(-0.09)	(0.09)	(0.09)
year=2010	-0.178	0.328	-3.014***	-0.0569	0.191	0.191
	(-0.16)	(0.29)	(-7.09)	(-0.05)	(0.16)	(0.16)
year=2011	-0.153	0.255	-2.997***	-0.0672	0.142	0.142
	(-0.16)	(0.27)	(-6.84)	(-0.08)	(0.15)	(0.15)
year=2012	-0.130	0.155	-2.674***	-0.0749	0.0404	0.0404
	(-0.20)	(0.23)	(-5.64)	(-0.12)	(0.06)	(0.06)
year=2013	-0.110	0.176	-1.995***	-0.0359	0.0272	0.0272
	(-0.18)	(0.28)	(-6.75)	(-0.06)	(0.04)	(0.04)
R^2	0.379	0.330	0.551	0.475	0.497	0.497
Adjusted R^2	0.356	0.282	0.527	0.446	0.469	0.469
Observations	19140	6506	9692	12958	13092	13092

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

Table 18: Admissions Standards: Separate ACT and SAT Treatments, IPEDS Results

	Admission Rate (Percent)	ACT Composite 25th Percentile	ACT Composite 75th Percentile	SAT Maths 25th Percentile	SAT Verbal 25th Percentile	SAT Maths 75th Percentile	SAT Verbal 75th Percentile
Intensity to Treat ACT	0.0428	0.872	1.361*				
	(1.13)	(1.37)	(2.19)				
Intensity to Treat SAT	-0.00571			-2.979	-2.910	-8.450	-3.535
	(-0.09)			(-0.13)	(-0.10)	(-0.38)	(-0.13)
Admissions Rt, 2001	0.493***	-3.267***	-2.484***	-80.98***	-79.60***	-71.35***	-74.56***
	(22.62)	(-8.13)	(-6.58)	(-9.46)	(-9.71)	(-9.15)	(-9.14)
Pct Black, 2000	-0.110***	-4.286***	-5.370***	-95.57***	-90.14***	-103.7***	-104.7***
	(-5.03)	(-13.45)	(-13.02)	(-13.35)	(-12.99)	(-12.47)	(-12.56)
Pct Native Am, 2000	-0.0631	-2.911**	-4.311**	-54.74^{+}	-48.91	-66.61	-92.11^{+}
	(-0.77)	(-3.04)	(-2.97)	(-1.92)	(-1.61)	(-1.46)	(-1.83)
Pct Hispanic, 2000	-0.0641	-5.948***	-6.975***	-120.0***	-103.6***	-124.1***	-117.5***
	(-1.35)	(-5.48)	(-7.24)	(-6.06)	(-5.45)	(-6.77)	(-6.46)
Pct Asian, 2000	-0.202*	4.674***	5.409***	142.7***	47.99*	155.0***	62.97*
	(-2.17)	(3.93)	(4.56)	(5.91)	(2.37)	(5.42)	(2.24)
Pct Unkwn Race, 2000	-0.121**	-0.562	-0.924	-20.45	2.737	-21.62	-0.407
	(-3.49)	(-0.54)	(-1.07)	(-1.01)	(0.13)	(-1.24)	(-0.02)
Tuition in-state	-0.00000224	0.000108**	0.0000483	0.00223**	0.00270**	0.00124^{+}	0.00171*

	(-0.92)	(2.97)	(1.32)	(2.94)	(3.81)	(1.71)	(2.25)
Tuition out-of-state	-0.00000192	0.000222***	0.000226***	0.00436***	0.00351***	0.00430***	0.00370***
	(-0.78)	(5.59)	(5.74)	(5.35)	(4.55)	(5.49)	(4.50)
Less than 2 Years Deg	0						
	(.)						
Associates degree	-0.0549	0	0	0	0	0	0
	(-0.88)	(.)	(.)	(.)	(.)	(.)	(.)
2-4 Academic Years	-0.0385	0.0945	-0.187	-21.22	-13.54	-30.60**	-11.30
	(-0.70)	(0.24)	(-0.39)	(-1.47)	(-1.52)	(-3.09)	(-0.89)
Bachelors degree	-0.0664	0.791**	1.216***	16.55**	17.30**	17.14**	16.85*
	(-1.06)	(3.16)	(4.24)	(2.79)	(2.87)	(3.05)	(2.56)
Postbaccalaureate	-0.0699	0.549	0.678	12.45	19.13	11.98	22.06
	(-0.92)	(1.10)	(1.30)	(1.00)	(1.46)	(1.04)	(1.51)
Masters degree	-0.0451	0.697**	1.203***	16.37**	15.81**	15.74**	14.01*
	(-0.71)	(3.00)	(4.42)	(2.87)	(2.74)	(2.89)	(2.21)
Post-masters	-0.0445	0.557*	0.897**	16.85**	14.62*	12.73*	8.063
	(-0.70)	(2.15)	(3.08)	(2.78)	(2.43)	(2.18)	(1.21)
Doctors degree	-0.0458	1.388***	1.859***	32.62***	27.50***	31.86***	23.63**
	(-0.72)	(5.70)	(6.60)	(5.54)	(4.62)	(5.66)	(3.65)
Degree-granting	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Nondegree-granting	-0.125**	0.533	0.113	33.31+	24.19	29.55+	7.901
	(-3.12)	(0.50)	(0.11)	(1.71)	(1.55)	(1.87)	(0.44)
Grad Rt, 2000	-0.0725**	6.682***	6.215***	142.7***	134.4***	122.9***	121.0***

	(-3.37)	(11.86)	(11.49)	(13.49)	(12.45)	(11.97)	(11.27)
Military Academy	-0.278***	6.365***	6.284***	87.01***	53.54**	73.79***	45.79***
	(-8.94)	(8.83)	(9.24)	(7.31)	(3.86)	(7.13)	(4.16)
Public	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Non-Profit Private	0.00395	-2.092***	-1.138***	-53.89***	-47.96***	-39.69***	-34.08***
	(0.23)	(-7.92)	(-4.29)	(-9.42)	(-8.49)	(-7.14)	(-5.69)
For-Profit Private	-0.0229	-3.622***	-2.570***	-87.50***	-78.16***	-76.46***	-58.74***
	(-1.17)	(-5.99)	(-5.18)	(-7.04)	(-8.51)	(-7.09)	(-6.59)
Unemployment	0.00443	0.364	-0.467	20.36	-1.141	-26.70	5.977
	(0.03)	(0.28)	(-0.25)	(0.27)	(-0.02)	(-0.43)	(0.09)
GDP Per Capita	0.00000667	0.000328	-0.000284	0.00574	-0.000464	-0.0111	0.00313
	(0.12)	(0.62)	(-0.38)	(0.22)	(-0.02)	(-0.49)	(0.13)
year=2001	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
year=2002	-0.0374	-1.158	1.235	-25.03	8.052	69.85	-29.30
	(-0.16)	(-0.48)	(0.36)	(-0.20)	(0.08)	(0.66)	(-0.25)
year=2003	-0.0708	-1.070	0.124	-5.512	-0.705	60.97	-16.49
	(-0.30)	(-0.46)	(0.04)	(-0.04)	(-0.01)	(0.59)	(-0.15)
year=2004	-0.105	-1.609	0.803	-45.82	-19.44	102.4	-0.136
	(-0.29)	(-0.44)	(0.16)	(-0.25)	(-0.12)	(0.66)	(-0.00)
year=2005	-0.0942	-1.523	0.793	4.646	26.31	88.22	12.45
	(-0.33)	(-0.54)	(0.20)	(0.03)	(0.22)	(0.73)	(0.10)
year=2006	-0.126	-1.166	0.215	24.34	20.40	63.89	18.52

	(-0.82)	(-0.72)	(0.09)	(0.37)	(0.33)	(1.00)	(0.28)
year=2007	-0.163	-1.181	0.193	13.87	-2.429	42.82	16.96
	(-1.41)	(-0.91)	(0.11)	(0.30)	(-0.05)	(0.94)	(0.39)
year=2008	-0.127	-1.213	0.345	-14.32	-2.232	50.73	24.32
	(-0.70)	(-0.66)	(0.13)	(-0.18)	(-0.03)	(0.68)	(0.31)
year=2009	-0.131	-2.397	2.736	-84.37	7.393	151.1	-16.79
	(-0.18)	(-0.34)	(0.28)	(-0.22)	(0.02)	(0.48)	(-0.05)
year=2010	-0.175	-2.943	4.520	-132.0	13.04	232.5	-42.72
	(-0.16)	(-0.28)	(0.30)	(-0.22)	(0.03)	(0.47)	(-0.08)
year=2011	-0.150	-2.627	3.683	-117.8	-7.678	204.1	-50.40
	(-0.16)	(-0.30)	(0.29)	(-0.24)	(-0.02)	(0.51)	(-0.12)
year=2012	-0.127	-1.954	2.668	-69.43	2.095	145.6	-27.40
	(-0.20)	(-0.31)	(0.30)	(-0.21)	(0.01)	(0.51)	(-0.09)
year=2013	-0.106	-1.820	2.807	-61.57	5.191	132.9	-22.87
	(-0.17)	(-0.31)	(0.34)	(-0.20)	(0.02)	(0.52)	(-0.08)
R^2	0.379	0.715	0.692	0.738	0.713	0.712	0.674
Adjusted R^2	0.356	0.700	0.674	0.724	0.698	0.696	0.656
Observations	19140	12956	12950	13089	13006	13087	13007

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Errors Clustered by College

Table 19: Applicantion, Enrollment, and Completion, IPEDS Results

	Applicants	Enrolled Undergraduates	Graduation Rate
Intensity of Treatment	2377.8^{+}	442.7*	0.0374

	(1.68)	(2.04)	(0.91)
Admissions Rt, 2001	-4584.2***	-5.753	-0.128***
	(-5.44)	(-0.05)	(-7.08)
Pct Black, 2000	239.1	-297.4*	-0.153***
	(0.38)	(-2.50)	(-9.00)
Pct Native Am, 2000	-2949.3	-1447.4*	-0.123
	(-1.56)	(-2.40)	(-1.63)
Pct Hispanic, 2000	1810.2	88.05	-0.220***
	(0.74)	(0.22)	(-4.78)
Pct Asian, 2000	19480.5**	1165.5*	0.0432
	(3.43)	(2.36)	(0.87)
Pct Unkwn Race, 2000	371.5	-302.4	-0.104**
	(0.18)	(-0.83)	(-3.38)
ACT Dominant	-864.3	-83.49	0.0264
	(-0.98)	(-0.63)	(1.50)
Tuition in-state	-1.067***	-0.216***	-0.00000464*
	(-6.73)	(-7.45)	(-2.27)
Tuition out-of-state	1.098***	0.209***	0.0000110***
	(7.16)	(6.80)	(4.98)
Less than 2 Years Deg	0	0	0
	(.)	(.)	(.)
Associates degree	-708.5	-543.7	0.182*
	(-0.32)	(-1.48)	(2.26)
2-4 Academic Years	-1736.0	-612.0*	0.224**

	(-0.93)	(-2.00)	(3.58)
Bachelors degree	-2093.9	-709.3*	0.258**
	(-0.97)	(-1.99)	(3.21)
Postbaccalaureate	-2914.9	-659.9+	0.255**
	(-1.26)	(-1.80)	(2.88)
Masters degree	-1713.9	-645.0+	0.266**
	(-0.79)	(-1.82)	(3.30)
Post-masters	-1482.1	-608.4+	0.285**
	(-0.68)	(-1.71)	(3.57)
Doctors degree	1677.2	99.67	0.305**
	(0.76)	(0.28)	(3.79)
Degree-granting	0	0	0
	(.)	(.)	(.)
Nondegree-granting	-2266.5+	-390.0+	0.0830
	(-1.72)	(-1.82)	(1.06)
Grad Rt, 2000	8013.9***	1473.2***	0.554***
	(7.92)	(7.52)	(15.83)
Military Academy	8798.5***	714.4*	0.193***
	(6.59)	(2.39)	(4.45)
Public	0	0	0
	(.)	(.)	(.)
Non-Profit Private	1022.4	-119.1	0.0147
	(1.23)	(-0.81)	(0.98)
For-Profit Private	-898.5	-136.5	-0.145***

	(-0.85)	(-0.67)	(-3.90)
Unemployment	-6286.2^{+}	-1086.0^{+}	0.00745
	(-1.69)	(-1.76)	(0.28)
GDP Per Capita	-2.665^{+}	-0.453^{+}	-0.00000377
	(-1.82)	(-1.81)	(-0.96)
year=2001	0	0	0
	(.)	(.)	(.)
year=2002	11151.8^{+}	1937.6^{+}	-0.00221
	(1.76)	(1.80)	(-0.05)
year=2003	11438.3^{+}	1946.9^{+}	0.0141
	(1.84)	(1.81)	(0.31)
year=2004	17410.1^{+}	2987.1^{+}	0.0147
	(1.81)	(1.79)	(0.28)
year=2005	14123.2^{+}	2407.6^{+}	0.0112
	(1.91)	(1.84)	(0.32)
year=2006	8202.0*	1392.9^{+}	0.0329*
	(2.10)	(1.87)	(2.16)
year=2007	6286.2*	1052.8^{+}	0.00348
	(2.17)	(1.88)	(0.19)
year=2008	9111.2*	1531.4^{+}	0.0118
	(1.97)	(1.81)	(0.49)
year=2009	33462.1^{+}	5752.9^{+}	
	(1.73)	(1.75)	
year=2010	51719.9^{+}	8877.0^{+}	

	(1.72)	(1.75)	
year=2011	43352.2^{+}	7375.3^{+}	
	(1.74)	(1.74)	
year=2012	31248.5^{+}	5235.1^{+}	
	(1.76)	(1.72)	
year=2013	29143.3^{+}	4813.1^{+}	
	(1.77)	(1.73)	
R^2	0.553	0.601	0.728
Adjusted R^2	0.533	0.583	0.716
Observations	16192	16175	9987

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0001$, Standard Erros Clustered by College

8.3.2 Individual-Level Data

Table 20: Enrollment, ACS Data

	Freshmen-Aged Dependents Enrollment	Freshmen-Aged All Enrollment	College-Aged Dependents Enrollment	College-Aged All Enrollment	Post-High School Aged Enrollment
Treated	1.0262^{+}	0.9930	1.0140	1.0318**	1.0016
	(0.0141)	(0.0120)	(0.0102)	(0.0086)	(0.0071)
GDP Per Capita	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
In-State Tuition	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Consumption Per Capita	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**

	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
White	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)
Black	0.7503**	0.5729**	0.7701**	0.6129**	0.5774**
	(0.0089)	(0.0054)	(0.0063)	(0.0037)	(0.0027)
Native American	0.5722**	0.4338**	0.5585**	0.4441**	0.4239**
	(0.0216)	(0.0139)	(0.0149)	(0.0094)	(0.0067)
Chinese	2.6820**	3.6146**	2.9339**	3.4992**	5.1185**
	(0.1092)	(0.1245)	(0.0813)	(0.0713)	(0.1040)
Japanese	1.8890**	3.0896**	2.0183**	3.4286**	3.9684**
	(0.1955)	(0.2757)	(0.1399)	(0.1830)	(0.1982)
Other Asian	1.9538**	1.8586**	1.9721**	1.8630**	2.2131**
	(0.0423)	(0.0342)	(0.0287)	(0.0215)	(0.0219)
Other race, nec	0.8972**	0.8348**	0.8994**	0.8299**	0.7997**
	(0.0161)	(0.0129)	(0.0111)	(0.0083)	(0.0061)
Two major races	0.9389**	0.8793**	0.9894	0.9476**	0.9233**
	(0.0196)	(0.0153)	(0.0147)	(0.0110)	(0.0087)
Three or more major races	0.9066	0.8762*	0.9598	0.9430+	0.9462*
	(0.0574)	(0.0452)	(0.0423)	(0.0324)	(0.0265)
Cognitive Disability	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)
Not Hispanic	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)
Mexican	0.8331**	0.5488**	0.7946**	0.5393**	0.4379**

	(0.0112)	(0.0063)	(0.0073)	(0.0040)	(0.0025)
Puerto Rican	0.7041**	0.4835**	0.7141**	0.5150**	0.4768**
	(0.0207)	(0.0121)	(0.0147)	(0.0084)	(0.0059)
Cuban	1.5706**	1.2807**	1.4653**	1.2379**	1.1224**
	(0.0768)	(0.0545)	(0.0480)	(0.0329)	(0.0235)
Other	1.1376**	0.8312**	1.1420**	0.8468**	0.7123**
	(0.0230)	(0.0142)	(0.0158)	(0.0092)	(0.0060)
Physical Disability	0.3880**	0.3487**	0.3700**	0.3549**	0.3117**
	(0.0091)	(0.0067)	(0.0059)	(0.0044)	(0.0028)
Family Income	1.0000**		1.0000**		
	(0.0000)		(0.0000)		
Male	1.6369**	1.5645**	1.6147**	1.4681**	1.6081**
	(0.0120)	(0.0094)	(0.0082)	(0.0057)	(0.0050)
Born in US (Citizen)	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)
Born abroad of American parents	1.1507**	1.1825**	1.2352**	1.2255**	1.3084**
	(0.0416)	(0.0358)	(0.0310)	(0.0236)	(0.0207)
Naturalized citizen	1.3065**	1.2124**	1.4016**	1.2364**	1.3557**
	(0.0333)	(0.0272)	(0.0230)	(0.0167)	(0.0140)
Not a citizen	0.6209**	0.5096**	0.6488**	0.5294**	0.5010**
	(0.0115)	(0.0075)	(0.0079)	(0.0047)	(0.0032)
Years Accumulated	0.8660**	0.9404**	0.9107**	0.9700**	1.0485**
	(0.0057)	(0.0057)	(0.0049)	(0.0047)	(0.0044)
Years Accumulated Sq	1.0063**	1.0028**	1.0038**	1.0012**	0.9986**

	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
Observations	575995	890902	1132099	1960499	3230912
Pseudo R^2	0.053	0.062	0.056	0.044	0.068

Exponentiated coefficients; Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 21: College Attainment, ACS Data

	College Dropout Post-College Age	College Graduate Post-College Age
Treated	1.0915**	1.0744**
	(0.0131)	(0.0124)
GDP Per Capita	1.0000**	1.0000**
	(0.0000)	(0.0000)
In-State Tuition	1.0000*	1.0000^{+}
	(0.0000)	(0.0000)
Consumption Per Capita	1.0000**	1.0000**
	(0.0000)	(0.0000)
White	1.0000	1.0000
	(.)	(.)
Black	1.3379**	0.3934**
	(0.0093)	(0.0031)
Native American	1.0461*	0.3232**
	(0.0240)	(0.0097)
Chinese	0.7583**	3.0537**
	(0.0177)	(0.0592)

Japanese	1.0345	2.1721**
	(0.0582)	(0.1073)
Other Asian	1.0798**	1.6453**
	(0.0135)	(0.0189)
Other race, nec	0.9795+	0.6838**
	(0.0114)	(0.0094)
Two major races	1.2322**	0.8225**
	(0.0170)	(0.0116)
Three or more major races	1.3454**	0.8119**
	(0.0524)	(0.0340)
Cognitive Disability	1.0000	1.0000
	(.)	(.)
Not Hispanic	1.0000	1.0000
	(.)	(.)
Mexican	1.0056	0.2981**
	(0.0087)	(0.0029)
Puerto Rican	1.1208**	0.4011**
	(0.0205)	(0.0080)
Cuban	1.0937**	0.8739**
	(0.0339)	(0.0250)
Other	1.2045**	0.5200**
	(0.0154)	(0.0070)
Physical Disability	0.7117**	0.2824**
	(0.0094)	(0.0046)

Sex	1.0135**	1.6044**
	(0.0046)	(0.0072)
Born in US (Citizen)	1.0000	1.0000
	(.)	(.)
Born abroad of American parents	1.0826**	1.1717**
	(0.0231)	(0.0257)
Years Accumulated	0.9297**	1.3689**
	(0.0111)	(0.0170)
Years Accumulated Sq	1.0026**	0.9877**
	(0.0006)	(0.0006)
Observations	1588882	1588882
Pseudo R^2	0.014	0.084

Exponentiated coefficients; Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 22: Enrollment, ACS Data, Interacted

	Freshmen Aged Dependents	Freshmen Aged Dependents Interact Income	Freshmen Aged Dependents Interact Race	College Aged Dependents	College Aged Dependents Interact Income	College Aged Dependents Interact Race
Treated	1.0226	0.9881	1.0426**	1.0116	0.9477	1.0353**
	(0.0142)	(0.0652)	(0.0157)	(0.0103)	(0.0468)	(0.0115)
GDP Per Capita	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
In-State Tuition	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**

	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Consumption Per Capita	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**	1.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
White	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)	(.)
Black	0.8278**	0.8278**	0.8325**	0.8325**	0.8325**	0.8396**
	(0.0100)	(0.0100)	(0.0105)	(0.0069)	(0.0069)	(0.0072)
Native American	0.6116**	0.6116**	0.6048**	0.5896**	0.5896**	0.5857**
	(0.0233)	(0.0233)	(0.0237)	(0.0158)	(0.0158)	(0.0161)
Chinese	2.8415**	2.8414**	2.8653**	3.0861**	3.0861**	3.1074**
	(0.1177)	(0.1178)	(0.1222)	(0.0870)	(0.0870)	(0.0897)
Japanese	1.8353**	1.8360**	1.9772**	1.9700**	1.9697**	2.0419**
	(0.1918)	(0.1917)	(0.2124)	(0.1369)	(0.1368)	(0.1448)
Other Asian	1.9875**	1.9879**	1.9972**	2.0003**	2.0003**	1.9968**
	(0.0437)	(0.0437)	(0.0452)	(0.0295)	(0.0295)	(0.0302)
Other race, nec	0.9177**	0.9177**	0.9293**	0.9160**	0.9160**	0.9269**
	(0.0167)	(0.0167)	(0.0173)	(0.0115)	(0.0115)	(0.0119)
Two major races	0.9564*	0.9563*	0.9558*	1.0045	1.0045	1.0047
	(0.0202)	(0.0202)	(0.0210)	(0.0150)	(0.0150)	(0.0156)
Three or more major races	0.9160	0.9159	0.9139	0.9656	0.9655	0.9588
	(0.0585)	(0.0585)	(0.0601)	(0.0428)	(0.0428)	(0.0436)
Cognitive Disability	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)	(.)
Not Hispanic	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000

	(.)	(.)	(.)	(.)	(.)	(.)
Mexican	0.8788**	0.8790**	0.8798**	0.8284**	0.8284**	0.8293**
	(0.0119)	(0.0119)	(0.0120)	(0.0077)	(0.0077)	(0.0077)
Puerto Rican	0.7624**	0.7627**	0.7623**	0.7594**	0.7594**	0.7594**
	(0.0226)	(0.0226)	(0.0226)	(0.0157)	(0.0157)	(0.0157)
Cuban	1.6412**	1.6418**	1.6434**	1.5205**	1.5207**	1.5233**
	(0.0812)	(0.0812)	(0.0813)	(0.0502)	(0.0502)	(0.0503)
Other	1.2018**	1.2022**	1.2008**	1.1962**	1.1963**	1.1953**
	(0.0247)	(0.0247)	(0.0247)	(0.0167)	(0.0167)	(0.0167)
Physical Disability	0.4026**	0.4025**	0.4026**	0.3821**	0.3820**	0.3821**
	(0.0095)	(0.0095)	(0.0095)	(0.0061)	(0.0061)	(0.0061)
Family Income Over $250,000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)	(.)
Family Income Under $250,000,	1.0614*	1.0669**	1.0615*	0.9490**	0.9470**	0.9487**
Over $150,000	(0.0248)	(0.0262)	(0.0248)	(0.0158)	(0.0165)	(0.0158)
Family Income Under $150,000,	0.8903**	0.8861**	0.8902**	0.7654**	0.7607**	0.7652**
Over $100,000	(0.0190)	(0.0199)	(0.0190)	(0.0117)	(0.0121)	(0.0117)
Family Income Under $100,000,	0.7141**	0.7121**	0.7141**	0.5996**	0.5967**	0.5995**
Over $75,000	(0.0153)	(0.0160)	(0.0153)	(0.0092)	(0.0096)	(0.0092)
Family Income Under $75,000,	0.5696**	0.5670**	0.5696**	0.4813**	0.4775**	0.4813**
Over $50,000	(0.0120)	(0.0125)	(0.0120)	(0.0073)	(0.0075)	(0.0073)
Family Income Under $50,000,	0.4341**	0.4328**	0.4340**	0.3748**	0.3733**	0.3747**
Over $25,000	(0.0092)	(0.0096)	(0.0092)	(0.0057)	(0.0059)	(0.0057)
Family Income Under $25,000,	0.3270**	0.3245**	0.3269**	0.2891**	0.2871**	0.2891**

over $10,000	(0.0074)	(0.0077)	(0.0074)	(0.0047)	(0.0048)	(0.0047)
Family Income Under $10,000	0.2948**	0.2956**	0.2949**	0.2672**	0.2662**	0.2673**
	(0.0077)	(0.0081)	(0.0077)	(0.0049)	(0.0051)	(0.0049)
Male	1.6458**	1.6457**	1.6457**	1.6205**	1.6205**	1.6205**
	(0.0121)	(0.0121)	(0.0121)	(0.0083)	(0.0083)	(0.0083)
Born in US (Citizen)	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(.)	(.)	(.)	(.)	(.)	(.)
Born abroad of American parents	1.1170**	1.1169**	1.1171**	1.2089**	1.2089**	1.2087**
	(0.0408)	(0.0408)	(0.0408)	(0.0307)	(0.0307)	(0.0307)
Naturalized citizen	1.3243**	1.3243**	1.3242**	1.4186**	1.4186**	1.4176**
	(0.0342)	(0.0342)	(0.0342)	(0.0236)	(0.0236)	(0.0236)
Not a citizen	0.6558**	0.6557**	0.6560**	0.6793**	0.6793**	0.6792**
	(0.0123)	(0.0123)	(0.0123)	(0.0084)	(0.0084)	(0.0084)
Years Accumulated	0.8647**	0.8646**	0.8648**	0.9088**	0.9088**	0.9088**
	(0.0057)	(0.0057)	(0.0057)	(0.0049)	(0.0049)	(0.0049)
Years Accumulated Squared	1.0065**	1.0065**	1.0065**	1.0040**	1.0040**	1.0040**
	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)
Family Income Over $250,000		1.0000			1.0000	
× Treated		(.)			(.)	
Family Income Under $250,000,		0.9471			1.0272	
Over $150,000 × Treated		(0.0756)			(0.0604)	
Family Income Under $150,000,		1.0531			1.0777	
Over $100,000 × Treated		(0.0759)			(0.0579)	
Family Income Under $100,000,		1.0315			1.0612	

Over \$75,000 × Treated	(0.0752)		(0.0576)	
Family Income Under \$75,000,	1.0528		1.1047^{+}	
Over \$50,000 × Treated	(0.0754)		(0.0590)	
Family Income Under \$50,000,	1.0305		1.0481	
Over \$25,000 × Treated	(0.0738)		(0.0559)	
Family Income Under \$25,000,	1.0961		1.0903	
Over \$10,000 × Treated	(0.0841)		(0.0625)	
Family Income Under \$10,000	0.9636		1.0419	
× Treated	(0.0847)		(0.0681)	
White		1.0000		1.0000
× Treated		(.)		(.)
Black		0.9353		0.8966^{**}
× Treated		(0.0385)		(0.0268)
Native American		1.3192		1.2133
× Treated		(0.2236)		(0.1516)
Chinese		0.8758		0.8874
× Treated		(0.1555)		(0.1171)
Japanese		0.2083^{**}		0.4075^{*}
× Treated		(0.0915)		(0.1464)
Other Asian		0.9346		1.0531
× Treated		(0.0804)		(0.0643)
Other race, nec		0.8347^{**}		0.8405^{**}
× Treated		(0.0513)		(0.0365)
Two major races		1.0136		1.0031

× Treated			(0.0777)			(0.0565)
Three or more major races			1.0738			1.1590
× Treated			(0.2873)			(0.2316)
Observations	575995	575995	575995	1132099	1132099	1132099
Pseudo R^2	0.062	0.062	0.062	0.063	0.063	0.063

Exponentiated coefficients; Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$