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Essays on incentives, economic conditions, and educational choices

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ESSAYS ON INCENTIVES, ECONOMIC CONDITIONS, AND EDUCATIONAL CHOICES

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Abstract

In this dissertation, I offer two independent studies that each contribute to the literature on the college choices. The first paper examines the impact of Arizona's merit based scholarship on college enrollment decisions. I find that both enrollment and tuition and fees increased at Arizona's three large public institutions after the implementation of the AIMS scholarship. Enrollment effects are strongest among black and hispanic students. The second paper examines the impact of the recent 2009 housing crisis on college enrollment decisions. I find that large reductions in housing wealth lead to increases in public school enrollment and decreases in private school enrollment. Similarly, I find that increases in foreclosures in a metro area are associated with decreases in private school enrollment and increases in public school enrollment.

Chapter 1. Introduction

When I compare the generation in which I grew up to my parents' generation, there are two very distinct differences that are apparent. First, the probability that one my peers attended college is much higher than that of my parents' peers. Second, home ownership increased substantially during this generation due to the general perception that owning a home is a "good investment". Due to the recent housing collapse, many in my generation are much more skeptical about home ownership. This dissertation will discuss the relationship between these seemingly unrelated societal changes that have occurred in recent decades.

1.1 College Enrollment

College enrollment has increased drastically over the past several decades (Turner, 2004)[1], but also has the cultural expectations around college enrollment (Turner, 2004).[2] Young adults no longer consider a college education a luxury good that some will have the opportunity to consume. Instead, more and more I see my peers referring to a college degree as a birthright available for all who are fortunate enough to be born in this country. We have seen this sentiment echoed in our politics, as both the federal and state governments are spending billions each year to subsidize higher education. Here in Louisiana alone, students who meet basic GPA and ACT requirements in high school, regardless of income or any other means test, will receive a four-year, full tuition scholarship, called *TOPS*, to an in-state public institution.[3] Furthermore, low income families are eligible for additional state

[1] "Among individuals aged twenty-three in 1970, 23 percent of high school graduates had completed a BA degree, while about 51 percent had enrolled in college for one time period since high school graduation. For the same age group in 1999, the share of high school graduates who had enrolled in college at some point rose substantially, to 67 percent, while the share receiving a BA degree rose only slightly, to 24 percent of the cohort."

[2] "That a college education is more important now than ever is certainly cliché.

[3] TOPS is an acronym for "Taylor Opportunity Program for Students."

aid, called *Pelican Promise*. In addition, low income students are also eligible for Federal Pell Grants.

When my parents graduated from high school in the late 1970s, not a single state offered a *TOPS* like program and today at least fifteen states offer similar programs. Also when my parents graduated from high school, total Pell Grant funding was just shy of $5.7 billion in 2012 dollars. In 2012 over $32 billion was spent on Pell Grants.[4] Simply put, more people are attending college today than ever before and this is likely–at least partially–due to the increase in student aid at both state and federal levels.

Figure 1.1 shows the growth of college enrollment nationwide in the United States over the past 40 years alongside the Federal expenditures on Pell Grants and the number of recipients of Pell Grants.

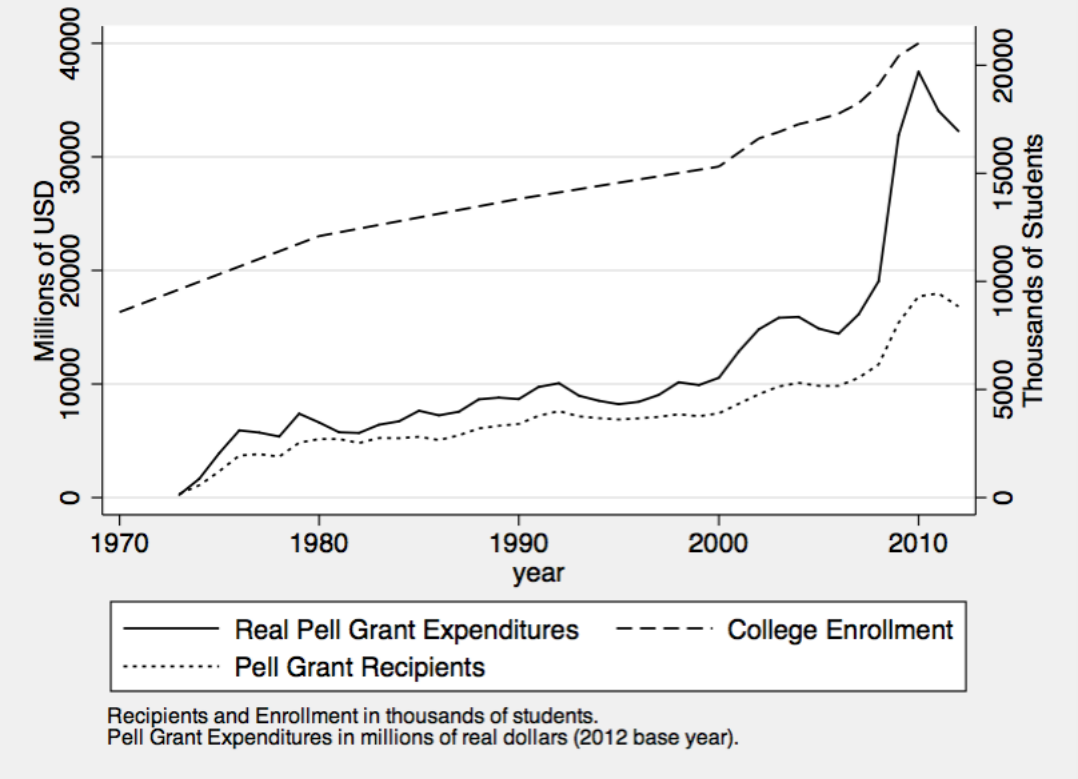


Figure 1.1: College Enrollment and Pell Grants

[4]CollegeBoard Advocacy & Policy Center.

All three of these series have increased substantially over this time period. Furthermore, there is a break from trend on all three of these series that occurs during the most recent Great Recession.

While it is impossible to deny the increasing importance that our country is placing on higher education, the reason for this cultural shift is not clear. This dissertation will discuss the growth in scholarship programs and empirically analyze one scholarship in particular–the Arizona AIMS scholarship–and how it impacted college enrollment in Arizona.

1.2 Housing Wealth

An integral part of the "American Dream" has always been home ownership. The concept of *ownership* is integral to a capitalist society. Ironically, though, the concept of home ownership has transformed into bank ownership over the past two generations. The terms of mortgages available to borrowers has increased over this past century (Hyman, 2012).[5] Furthermore, families are now able to borrow against the equity in their homes for a "Home Equity Line of Credit" (HELOC) (Hyman, 2012).[6] These factors, among others, likely contributed to the rise in real housing debt that has been documented in the United States (Mian and Sufi, 2011).

It is possible that a large increase in housing wealth also occurred during a period where housing debt increased. If real housing prices increase substantially, then housing debt too would logically rise as larger mortgages are needed to purchase the same home. While it is true that nominal housing prices have increased since the 1980s, real prices have stayed remarkably constant with the notable exception of the housing bubble and subsequent collapse that occurred during the first decade of the twenty-first century. These trends are

[5]In the 1920s, the longest possible mortgage was a 10 year mortgage–many of which had balloon payments in the tenth year. Today, the most common term of a mortgage is 30 years.

[6]P. 219. "Unlike the debt repayment of the post war period, which relied on rising income, debt repayment in the 1990s relied on rising asset prices. With rising house prices, home owners could borrow against their houses and repay their debts, even if their incomes did not rise. Everyone's house, and the U.S. economy, became a house of credit cards."

illustrated in Figure 1.2. Interestingly, in 2013, housing prices were only 25 percent higher in real dollars than in 1987.[7] This is less than a nine-tenths of one percent increase in real prices per year over this 26 year period. Therefore, the increase in housing prices does not tell the whole story about why housing debt has increased.

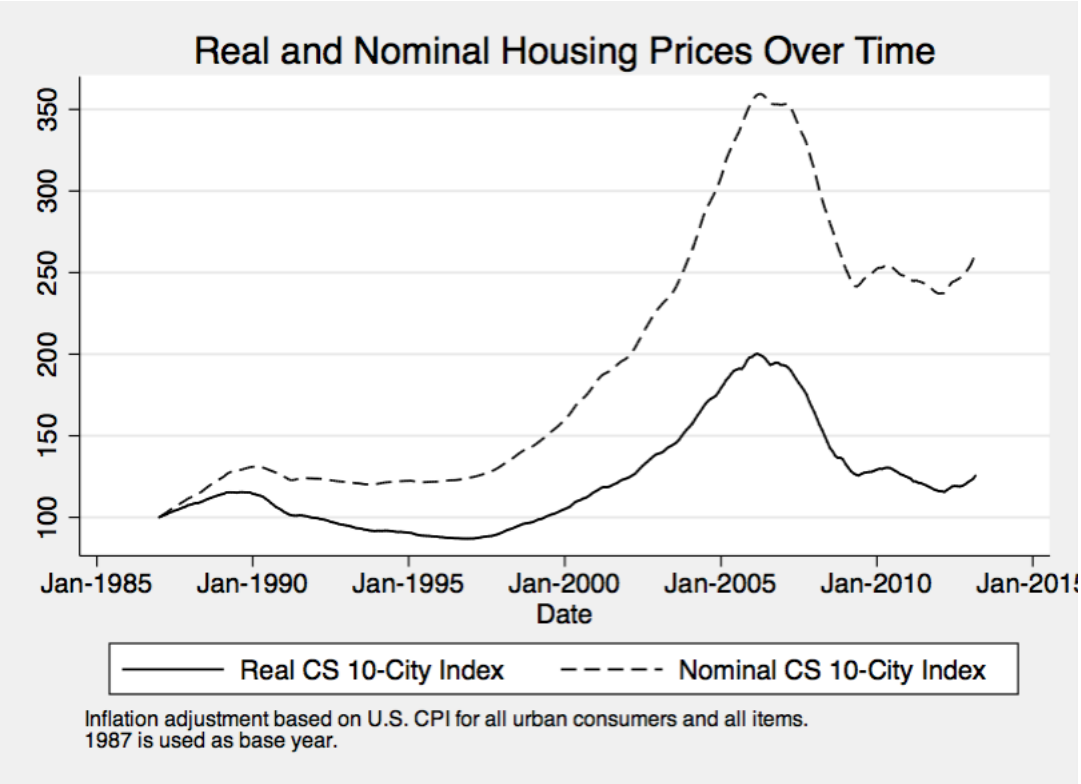


Figure 1.2: Housing Prices Over Time

Over the past century, Americans have believed that housing prices would consistently increase over long periods of time. This belief drove people to purchase larger homes and borrow more money, because after all, the price of the house would increase. This was most famously discussed by recent (2013) nobel laureate Robert Shiller (Schiller, 2005). He refers to the cultural phenomenon as "irrational exuberance" a term her borrowed from a speech made by Federal Reserve Chairman, Alan Greenspan (Greenspan, 1996). Schiller describes a feedback effect in which increased demand for housing fueled by public perception that housing is a "good investment" lead to increases in housing prices which in turn solidified

[7]Based on authors calculations.

the belief that housing prices would continue to rise and drove more demand for housing. It is this "irrational exuberance" that is at least partially responsible for the real estate bubble and crash that occurred during the Great Depression of 1929 and Great Recession of 2008.

Like the generation who lived through the great depression, my generation too will likely be more skeptical about home ownership and have doubts about whether home prices will increase substantially throughout our adult lives. While there is no doubt that home ownership will continue to be important in American culture, the recent financial crisis has dampened this excitement and made my generation more hesitant about how home ownership will play a part in our financial goals. This dissertation will discuss the drastic change in housing wealth that occurred during the 2008 financial crisis and investigate the extent to which this impacted college enrollment decisions.

1.3 Overview

This dissertation was motivated by these two observations that have manifested themselves into two essays that each contribute to the literature on college choices. In the second chapter, I investigate the impact of Arizona's AIMS scholarship on a variety of educational outcomes including college enrollment and tuition and fees at Arizona's three large public universities. In the third chapter, I analyze the impact of housing wealth on college enrollment. Taking advantage of the 2009 housing crash as an exogenous shock, I analyze the extent to which college enrollment changes in response to shocks to housing wealth.

The second essay is entitled *The Effect of Merit-Based Scholarships on Educational Outcomes: An Analysis of the Arizona AIMS Scholarship*. This paper analyzes the effect of a statewide merit-based scholarship program on educational outcomes in Arizona. It tests whether Arizona's Instrument to Measure Standards (AIMS) scholarship has had an effect on a comprehensive set of educational outcomes such as the number of applicants, student admissions, first-year first-time enrollment, ACT scores of entering freshman, retention rates,

as well as on the level of tuition and fees at the three schools targeted by the program; Arizona State University, University of Arizona and Northern Arizona University. Both difference-in-differences estimation as well as synthetic control methods shows that AIMS has an economically and statistically significant effect on many of these outcomes, primarily enrollment and tuition. Enrollment effects are greatest among African American and Hispanic students and are significant for both men and women.

The third essay is entitled *Housing Wealth and College Enrollment.* This is the first paper to explore the relationship between *local* housing market conditions and enrollment in institutions of higher education within a metropolitan area. I examine how changes in housing wealth impact college enrollment decisions within these areas. Housing wealth indicators are obtained from a large national sample of proprietary loan-level data which allows us to observe individual loan specific characteristics and outcomes such as value at origination and payment history. We find that changes in housing wealth impact private and public institutions differently. The results suggest that enrollment at public schools are negatively related to housing wealth, while private schools are positively related. Potentially students are able to substitute from public to private institutions as housing wealth increases.

The findings of these two studies are summarized in Chapter 5.

Chapter 2. The Effect of Merit-Based Scholarships on Educational Outcomes: An Analysis of the Arizona AIMS Scholarship

2.1 Introduction

In the United States, the federal and state governments spend billions of dollars on higher education each year.[1] Many of these dollars are funneled into federal programs such as Pell grants, Perkins loans, Stafford loans, and college student tax credits for higher education expenses. The effectiveness of different types of student aid on an array of outcomes has been extensively studied (Dynarski, 2004; Bettinger, 2004; Avery and Hoxby, 2004).

Over the past two decades, individual states in the U.S. have created their own statewide merit-based scholarships to encourage residents to attend colleges and universities in their home states (Dynarski, 2004).[2] While not a single statewide merit-based scholarship existed before 1991, today there are at least 15 states that provide merit-based scholarships for their residents.[3]

Many politicians have touted these programs as successful, yet there are many important questions that need to be answered.[4] Do these scholarships increase the probability that high school students will attend college? How do students respond to merit aid compared to

[1] In 2012 the U.S. Department of Education had a discretionary appropriations of $68.1 billion.

[2] These scholarships are distinctly different than need-based scholarship programs that are also prevalent in many states. Merit-based aid is available for any student who meets certain criteria laid out by the state, while need-based aid is for students who qualify based on need, primarily due to guardian's income being below a certain threshold.

[3] These states are Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Michigan, Mississippi, Nevada, New Mexico, South Carolina, Tennessee, West Virginia, Wyoming, and Arizona.

[4] In January of 2012, Louisiana Governor Bobby Jindal said the following when asked about the possibility of capping Louisiana's Taylor Opportunity Program for Students (TOPS) scholarship. "We remain opposed to any efforts to cap TOPS. We think it is an important program that has been very successful."

need-based aid? Do these scholarships result in regressive income transfers? Do merit-based scholarships increase the probability of course withdrawals? Do these scholarships cause grade inflation in high school? Do they increase enrollment at colleges and universities?

Much of the literature attempting to answer these questions has analyzed data from Georgia's Helping Outstanding Pupils Educationally (HOPE) scholarship. Dynarski (2004) investigated HOPE's effect on an array of educational outcomes in Georgia. She found that HOPE increased the probability of college attendance by about five to seven percentage points, and that HOPE was more effective than need-based aid in increasing enrollment. She also found that the scholarship shifted students toward attending four-year schools instead of two-year schools.

Cornwell and Mustard (2007) tested whether the HOPE scholarship allowed wealthier families to substitute money that they otherwise would have spent on tuition for a new vehicle. They found a significant increase in the number of vehicle registrations in Georgia after HOPE was implemented, thereby supporting their hypothesis. Cornwell, Lee, and Mustard (2005) used enrollment records of undergraduates at the University of Georgia (UGA) to estimate HOPE's effect on course taking. They concluded that HOPE increased course withdrawals among freshmen because students in danger of losing their scholarship were the most likely to withdraw from courses in the first year. The paper also found that HOPE increased the number of summer school credits earned by students. Buglar, Henry, and Rubenstein (1999) studied HOPE's effect on grade inflation in high school but found no evidence that that the implementation of HOPE created or exacerbated grade inflation in Georgia.

Cornwell, Mustard, and Sridhar (2006)–hereafter referred to as CMS–tested HOPE's impact on college enrollment in Georgia. They found that the implementation of HOPE was associated with a 5.9 percent increase in enrollment, translating into almost 2,900 additional students enrolled in Georgia colleges and universities per year. They also found that the rise in enrollment was concentrated in four-year schools–especially private colleges–and that

the effect on enrollment was smaller for white students than for other races. Finally, the scholarship led to an increase in the average SAT scores of incoming freshmen at Georgia's public colleges and universities. Zhang and Ness (2012) found that the HOPE scholarship decreases migration of the best and brightest students out of Georgia. On the other hand, Sjoquist and Winters (2013) argue that the HOPE Scholarship did not increase the percentage of students who remain in state after college because students who received the HOPE scholarship have a high probability of leaving Georgia post graduation.

Recently, there has been interest in merit-based scholarships in other states as well. Scott-Clayton (2011) tested for the impact of the West Virginia PROMISE program on "end-of-college" outcomes such as the time it took students to complete a degree. Bruce and Carruthers (2012) focused on the Tennessee HOPE scholarship and found that students are more likely to take the ACT multiple times in order to achieve the minimum ACT score needed for the scholarship. They also found that students who meet the requirements substitute away from two-year community colleges in favor of four-year colleges. Cohodes and Goodman (2013) found that Massachusetts' Adams Scholarship induces high-skilled students to substitute away from higher quality institutions to enroll in in-state public colleges, which decreases the probability that the student will graduate.

Other studies have focused on merit scholarships in general, without focusing on specific programs in specific states. For instance, both Fitzpatrick and Jones (2012) and Sjoquist and Winters (2014) found that a merit aid program increased the probability that a college attendee stayed in state post college.

While the insights obtained from these studies are important, it is also valuable to analyze the impacts of similar programs in other states because many of these programs differ substantially. This paper tests for the effect of a comprehensive set of outcomes on Arizona's Instrument to Measure Standards (AIMS) scholarship. It is unknown whether AIMS will have similar effects on many educational outcomes, as scholarships (1) have different requirements, (2) are implemented in states with very different demographics, (3) were not

enacted during the same time period,[5] and (4) are available for different subsets of schools within their respective states.

Because much of the existing literature has focused on the Georgia HOPE scholarship, I will focus on the differences between the HOPE and AIMS scholarships. For instance, HOPE and AIMS have substantially different eligibility requirements. In particular, AIMS' requirements are more stringent than HOPE's. Currently, it is unknown how much of an impact scholarship requirements have on a scholarship's effectiveness at increasing enrollment. The specific differences in the eligibility requirements between HOPE and AIMS will be discussed in a Section 2.1.1.

Another important difference between the HOPE and AIMS programs is the underlying demographics in Georgia and Arizona. For example, according to the 2010 census, Hispanics constitute about 25 percent of Arizona's population, while they only make up about 5 percent of Georgia's population. On the other hand, African Americans make up 28 percent of the population in Georgia, and only 3 percent of the population in Arizona. Merit-based scholarships are potentially more effective with different races, and if these races are more prevalent in different parts of the country, then the scholarships' effectiveness also might be different. While I will not be able to test this hypothesis formally, I will test the enrollment effects of AIMS by race.

A third factor that might have an impact on a scholarship's effectiveness is the time period in which the scholarship is implemented. According to Snyder and Dillow (2011), there were almost 14 million students students enrolled in institutions of higher education in the fall of 1990. By 2009, this number increased to over 20 million, an increase of 48 percent. During this same time period, the inflation-adjusted average tuition at four-year institutions increased from $12,185 to $20,986.[6] This is an increase of over 72 percent. It is unknown whether the marginal contribution of a scholarship program today is more or less

[5]For instance, HOPE and AIMS were implemented 13 years apart.

[6]These costs include total tuition and room and board rates charged for full-time undergraduate students in degree-granting institutions in 2008-2009 dollars. Source: National Center for Education Statistics Fast Facts.

effective in increasing enrollment than it was several decades ago. It might be hypothesized that due to major changes in students' choices to attend college over time, the effectiveness of scholarship programs on students' decisions might also have changed over time.

The final and most notable difference between HOPE and AIMS pertains to the type of colleges at which students can take advantage of the scholarships. HOPE can be used at any eligible public or private college, university or eligible technical college in Georgia. AIMS, on the other hand, is only available to students enrolled at one of Arizona's three publicly funded universities: Arizona State University (ASU), University of Arizona (UA) and Northern Arizona University (NAU). CMS tested HOPE's effects on a wide range of colleges and universities in Georgia. This study focuses primarily on ASU, UA and NAU as the scholarship applies to these institutions only. I also investigate the impact of AIMS on other colleges and universities in Arizona as a falsification test, but I do not expect to find a positive AIMS effect on enrollment at these other institutions because their students are not eligible for the scholarship. There may, however, be a negative effect of AIMS on enrollment at non-covered universities or colleges, as students might substitute away from these institutions to attend ASU, UA, or NAU in order to take advantage of the scholarship.

In this study, I investigate the impact of the AIMS scholarship (the "AIMS Effect") on the number of student applications, number of students admitted, freshmen ACT scores, first-year first-time enrollment (by race and gender), retention rates, and in-state tuition and fees at ASU, UA and NAU. I find AIMS has an impact on both enrollment and tuition. This result is robust when both difference-in-differences (DD) and synthetic control (SC) empirical specifications are employed.

The remainder of Section 2.1 provides background information on the AIMS and HOPE scholarships. Section 2.2 describes the empirical specification and data used in this analysis. Section 2.3 presents the results in addition to a variety of robustness checks, including a synthetic control model, placebo tests and asymptotically refined confidence intervals. Finally Section 2.4 consists of conclusions and extensions for future research.

2.1.1 The AIMS Scholarship

Arizona students who graduated from high school in 2006 were the first class eligible for the AIMS scholarship. In order to be eligible, students must meet the following requirements: (a) they must complete all core high school classes with a grade of B or better, (b) they must have a 3.5 GPA on a 4-point scale in core classes or rank in the top 5% of their class, and (c) they must exceed standards on the three AIMS tests while in high school. Core courses consist of classes such as English, math, and science.[7] The AIMS tests measure student competence in reading, writing, and math. Students who meet these requirements are eligible for an in-state tuition scholarship that is valid for one year. This scholarship can be used at one of Arizona's three large public universities: Arizona State University (ASU), University of Arizona (AU) and Northern Arizona University (NAU). Upon performing adequately in college, the scholarship is renewable for a maximum of four years.[8]

Since the inception of the AIMS scholarship, the number of students who have taken advantage of the scholarship has increased substantially. In 2006, about 1,500 incoming college freshmen took advantage of AIMS. By 2009, this number increased to almost 3,000, an increase of 88 percent. Due to this large increase in the cost of the program as well as current budgetary constraints in Arizona, beginning with the high school graduating class of 2013, the AIMS scholarship will only cover 25 percent of tuition.

The AIMS requirements contrast drastically with Georgia's HOPE scholarship requirements. Georgia students who graduated from high school in 1993 were the first class eligible for the HOPE scholarship. In order to be eligible, students need to graduate from an accredited high school in Georgia with a 3.0 GPA or better. The requirements have remained relatively constant over time.[9] If met, students are eligible for an in-state tuition scholarship

[7] There are 16 "core competency" courses that include four units of English, four units of math, three units of Science, two units of social sciences, two units of foreign language, and one unit of art.

[8] Arizona Department of Education 2012.

[9] Some changes have been made to the HOPE scholarship's eligibility requirements. For example, in 1995, the $100,000 parental income cap was removed. Other minor changes have been made over the years.

that is valid for one year. Similar to AIMS, upon performing adequately in college, the scholarship is renewable for a maximum of four years. Unlike AIMS, though, HOPE can be used at any college or university in Georgia.

2.1.2 Data

I use data from the Integrated Post Secondary Education Data System (IPEDS) published by the National Center for Education Statistics (NCES) as the sole source of dependent variables. IPEDS has yearly data on over 7,400 colleges and universities throughout the United States. Independent variables used in this paper include (a) first-year first-time enrollment of freshman (by race and gender), (b) number of applicants, (c) student admissions, (d) ACT scores of entering freshman[10], (e) retention rates, and (f) in-state tuition and fees.[11]

Three control variables are employed. The first is the number of high school graduates by state from the National Center for Education Statistics (NCES).[12] It is hypothesized that as the number of high school graduates increases in a state, so too will the enrollment at that state's colleges and universities. Previous research has shown that economic conditions can affect many decisions made by students, such as whether to drop out of high school (Rees and Mocan, 1997), whether to attend or continue college (Mincer, 1974; Dellas and Sakellaris, 2003; Dellas and Koubi, 2003), and what major they should choose (Lee, 2010).[13] Therefore, two additional variables are employed to control for labor market conditions. These variables are the unemployment rate obtained from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) and the average weekly wage from Bureau of

[10]IPEDS does not provide average ACT scores, but instead provides the 25th and 75th percentile of ACT scores of incoming freshmen. This paper utilizes the average of these two.

[11]Ideally, I would have limited the analysis to only in-state students, but due to data availability, this is not possible. CMS were subject to the same data limitations, and therefore they also used all first-time freshmen as the independent variable of interest.

[12]The estimated 18-24 year old population by state from the U.S. Census Bureau was also used, but because these two variables are highly collinear, they were used interchangeably as a robustness check. The results were robust to both variables, and therefore only regressions with high school graduates will be reported.

[13]Mincer discusses the human-capital investment-model which includes cyclic labor market conditions as a potential reason to not obtain additional schooling.

Labor Statistics' Quarterly Census of Employment and Wages (QCEW).[14] Lovenheim and Reynolds (2013) and Lovenheim (2011) test the impact of housing wealth on college choices and found that increases in housing wealth can lead to an increase in college enrollment. Therefore the state-level housing price index published by the Federal Housing Finance Agency (FHFA) is also used as a covariate.[15] The data set contains state-level information on each of these variables from 2000 to 2010.

Table 2.1 presents the change in the variables of interest at the three universities analyzed in this study (ASU, UA, and NAU) between 2005 (the year before AIMS was implemented) and 2010 (the most recent year in the sample). First-year first-time enrollment of freshman increased at all three universities over this time period, with NAU's increase of 80 percent being the most notable. ASU's and UA's enrollment increased by about 12 and 18 percent respectively. While NAU experienced the largest percent increase in enrollment, it also has the smallest first-year first-time enrollment both before and after the implementation of AIMS.

Equally as noticeable as the change in enrollment is the change in tuition. Tuition increased by over 74 percent at all three institutions, with the highest increase (84.6 percent) at ASU. While these increases appear large, both tuition and enrollment are on an upward trend nation-wide as previously discussed. Figure 2.1 presents a graphical representation of the increase in both enrollment and in-state tuition and fees for the treatment and control groups. The increase in enrollment of the treatment group compared to the control group is particularly apparent after the implementation of AIMS in 2006.

[14]NAICS 23 Construction and NAICS 31-33 Manufacturing were also used to proxy for low skilled labor that is potentially available for high school graduates. When these variables are used in the place of total average weekly wages, the results are similar, and therefore regressions with the inclusion of these industry specific variables are not included in the results

[15]Specifically, the average monthly seasonally adjusted housing price index by year is used.

Table 2.1: Change in Enrollment and Tuition Over Time

	2005	2010	% Change
Arizona State University (ASU)			
Applicants	19,914	28,304	42.1%
Admissions	18,126	25,616	41.3%
Enrollment	7,396	8,261	11.7%
ACT Scores	23	23	0%
Retention	79%	81%	2.5%
Tuition + Fees	$4,406	$8,132	84.6%
University of Arizona (UA)			
Applicants	17,904	26,629	48.7%
Admissions	15,701	20,068	27.8%
Enrollment	5,785	6,804	17.6%
ACT Scores	23.5	24	2.1%
Retention	79%	78%	-1.2%
Tuition + Fees	$4,498	$8,237	83.1%
Northern Arizona University (NAU)			
Applicants	7,305	31,773	349.9%
Admissions	6,308	20,024	217.4%
Enrollment	2,105	3,789	80.0%
ACT Scores	21	22.5	4.3%
Retention	69%	72%	7.1%
Tuition + Fees	$4,393	$7,672	74.6%

Full-time first-time degree/certificate-seeking undergraduate students.
Retention rates for 2009 due to unavailability in 2010.

Table 2.2 presents the description of the variables as well as their summary statistics. All of the universities in the sample are quite large, with an average of over 21,000 applicants and 13,000 students admitted per year with incoming first-year first-time freshmen classes averaging over 5,000 students per year and the average in-state student pays about $6,500 in tuition and fees each year. Retention rates are 85 percent on average.

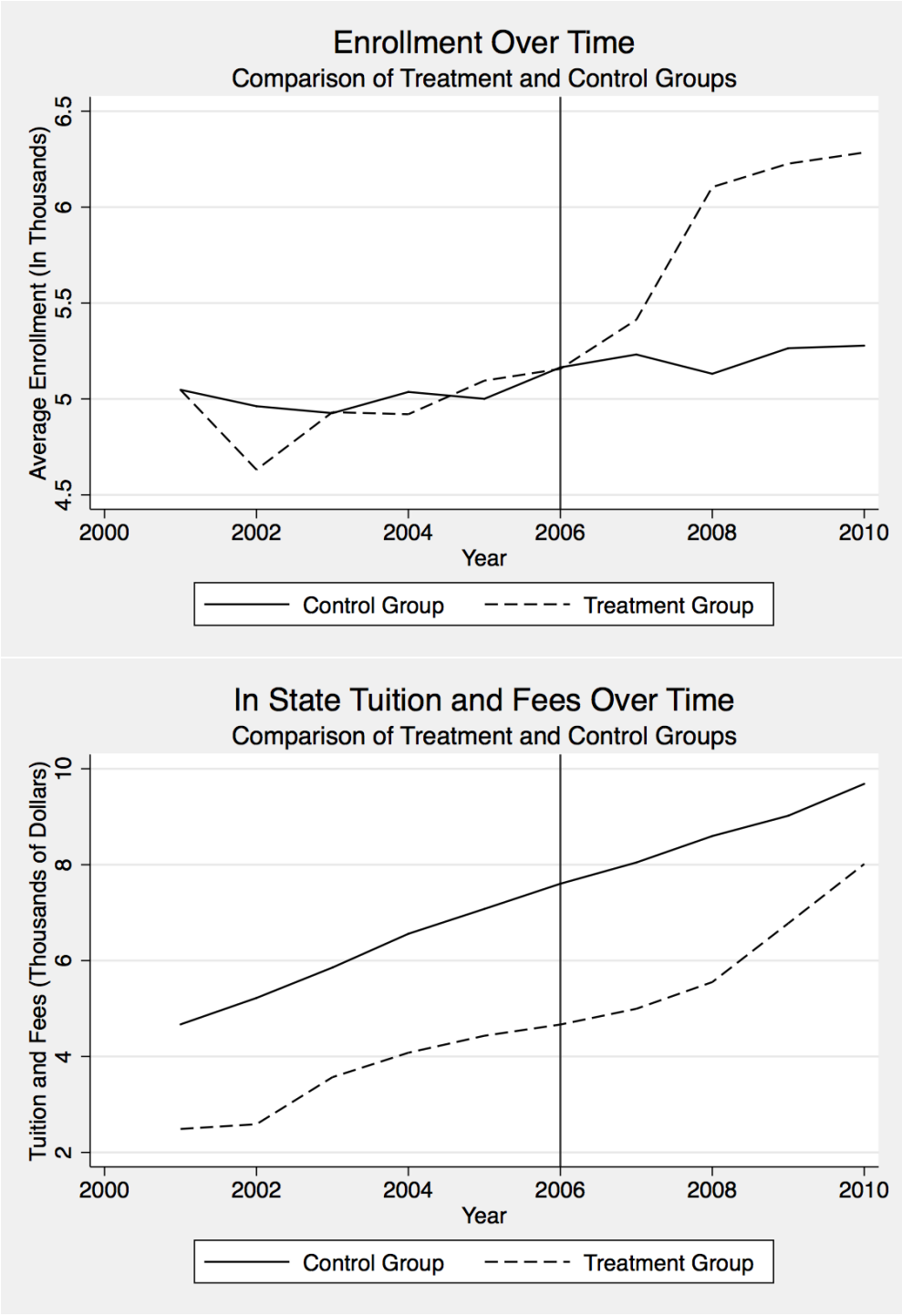


Figure 2.1: Average First-Year First-Time Enrollment and In-State Tuition and Fees: Comparison of the Treatment and Control Groups

Table 2.2: Summary Statistics and Variable Descriptions

Variable	Description	Mean	SD	N
Total Applicants	FTFY degree-seeking applicants total	21,508	9,055	170
Male Applicants	FTFY degree-seeking applicants - Men	10,062	4,252	170
Female Applicants	FTFY degree-seeking applicants - Women	11,444	4,913	170
Total Admissions	FTFY degree-seeking admissions total	12,960	4,003	170
Male Admissions	FTFY degree-seeking men admitted	5,899	1,940	170
Female Admissions	FTFY degree-seeking women admitted	7,060	2,159	170
Enrollment Total	FTFY degree-seeking enrolled full time total	5,162	1,540	170
Enrollment Men	FTFY degree-seeking men enrolled full-time	2,421	785	170
Enrollment Women	FTFY degree-seeking women enrolled full-time	2,740	798	170
ACT Total	Average of ACT Composite 25th and 75th percentile score	24.7	2.1	138
ACT English	Average of ACT English 25th and 75th percentile score	24.3	2.4	120
ACT Math	Average of ACT Math 25th and 75th percentile score	24.8	2.5	120
Tuition	Published in-state tuition and fees	$6,533	$2,628	187
Retention Rate	Full-time retention rate	85%	9.9%	115
Unemployment Rate	Statewide-Not Seasonally Adjusted	6.1%	2.1%	170
High School Graduates	Total High School Graduates	104,282	80,681	186
FHFA Index	Federal Housing Finance Agency Housing Price Index	216.8	41.04	170

"FYFT" stands for "First-Year First-Time"

2.2 Empirical Strategy

2.2.1 Difference-In-Differences (DD) Estimation

Following the empirical strategy employed by CMS to estimate the impact of HOPE on educational outcomes in Georgia, I employ Equation (2.1) to estimate the impact of AIMS on educational outcomes in Arizona. Control and treatment groups are identified, and the following empirical specification is estimated:

$$\ln(E_{ist}) = \alpha + \delta(S_{AZ} \times A_t) + X'_{st}\zeta + \gamma S_{is} + \beta_t Y_t + \varepsilon_{ist} \tag{2.1}$$

where E_{ist} is the variable of interest at school i in state s in year t.[16] S_{AZ} is an indicator variable corresponding to the treatment group: Arizona State University (ASU), University of Arizona (UA), and Northern Arizona University (NAU) and is zero for the control schools. A_t is an indicator variable that indicates the post-AIMS time period: 2006 to 2010. X_{st} is a vector of control variables that includes the number of high school graduates, unemployment rate, and average wage observed at the state level. S_{is} represents school fixed effects and Y_t is year fixed effects. The coefficient of interest is δ, as it represents the "AIMS effect."

There are 14 institutions that both ASU and UA consider to be "peer institutions." These peer institutions are Florida State University, University of Illinois at Chicago, University of Illinois at Urbana-Champaign, University of Iowa, University of Maryland-College Park, Michigan State University, University of Minnesota-Twin Cities, Rutgers University-New Brunswick, Ohio State University, University of Texas at Austin, University of Washington-Seattle Campus, and University of Wisconsin-Madison. These will be used as the control group in the differences-in-differences framework.[17]

[16] Variables of interest include student enrollment, number of applicants, student admissions, ACT scores, retention and tuition.

[17] NAU has a very different list of peer institutions due to its difference in size as compared to ASU and UA as is illustrated in Table 2.1. Instead of trying to pick and choose which institutions are appropriate peers for all three universities, I chose to use just the common peer institutions for ASU and UA. As a

Year and school level fixed effects are included in all regressions, although these coefficients are not reported. Standard errors are bootstrapped in all regressions.[18]

2.2.2 Synthetic Control (SC) Estimation

There have been substantial critiques to empirical literature that employs DD estimation (Bertrand, Duflo, and Mullainathan, 2004; Abadie, Diamond, and Hainmueller, 2010). Due to critiques primarily about non-robustness to placebo tests especially when the number of treated units is relatively small, researchers have been pushed to conduct additional robustness checks to assure that results from DD estimation are indeed valid. One underlying assumption necessary for DD estimation to be valid is that the treatment group would have been the same as the control group holding all covariates constant post treatment had the treatment not have been implemented. For this reason, choosing an appropriate control group is crucial to validity of this estimation technique. Unfortunately choosing the "right" control group is not always a straightforward or easy process. This is illustrated in this paper, as two of the three universities being analyzed are very similar (ASU and UA) but the third school (NAU) is less similar. Therefore, the use of SC methods are particularly relevant in this application.

Before creating the synthetic schools, the potential group of control schools is expanded to include all four-year public universities in the IPEDS southwest region in addition to the control schools discussed in section 2.2.1.[19] This increases the potential control group from

robustness check, I ran separate regressions using each school as the sole treatment school. These results of this specification are presented in Table 2.6 and are discussed further in the results section. Furthermore, similarly ranked institutions in the *U.S. News and World Report College Rankings* were also considered as a potential control group. In 2006, the year when AIMS was implemented, only one of the three schools, University of Arizona, received a ranking and therefore I chose to use the common peers as the control group.

[18]Cluster corrections are not used in calculating the standard errors because the number of schools is not sufficiently large compared to the number of years (17 schools over 10 years). When standard errors are clustered at the institution level, the results do not differ significantly. Not clustering is the more conservative approach, and therefore these more conservative standard errors are reported.

[19]The IPEDS southwest region includes Arizona New Mexico, Oklahoma and Texas. No other schools within Arizona are included in the expanded control group as they were potentially impacted by AIMS.

thirteen schools to thirty-two schools. Using this expanded group of potential controls, I create a synthetic control school for ASU, UA and NAU using the methods discussed in Abadie, Diamond, and Hainmueller (2010). Then, removing ASU, UA and NAU from the sample, I create synthetic schools for each of the other institutions that will be used to conduct a placebo test.

Following Abadie, Diamond, and Hainmueller (2010) synthetic control groups are made by choosing a W^* that minimizes $\sqrt{(X_1 - X_0W)'V(X_1 - X_0W)}$ where X_0 is a vector of pre-intervention characteristics for the exposed regions (or treatment group) and X_1 is a vector of pre-intervention characteristics of the non-exposed regions (or control group). W is a $(J \times 1)$ vector or positive weights that sum to one. V is some $(k \times k)$ symmetric and positive semidefinite matrix. The "synthetic schools" are created by taking a weighted average of the other schools variable of interest. The weights used come from W which was estimated econometrically.

A "synthetic school" is made to correspond with each school in the sample (both control and treatment schools) for purposes of analyzing changes in enrollment by constructing X_0 and X_1 to include the following variables: enrollment, applicants, number of high school graduates, unemployment rate, total wages, and FHFA HPI.[20] A second "synthetic school" is made to correspond with each school in the sample for purposes of analyzing changes in tuition using the following variables: tuition, enrollment, high school graduates, unemployment rate, total wages, and FHFA HPI.[21]

Table 2.3 shows the weights associated with potential control institutions to make up the "synthetic school" for ASU, UA, and NAU for both enrollment and tuition. As can be expected, the weighting vector used to create the synthetic schools for ASU and UA enrollment are very similar, thus speaking to the validity of using the same control group in the DD estimation. As is also expected, the synthetic control school for NAU is quite different, thus reaffirming the need for conducting SC estimation. Importantly, the SC

[20]The natural log of each one of these variables is used in order to be consistent with regression results.
[21]The natural log of these variables is also used in order to be consistent with regression results.

methods are consistent with colloquial knowledge of these institutions. Table 2.3 also shows that the synthetic control group used for estimating the AIMS effect on tuition is almost identical for these three schools. This is expected, as the same board of regents approves tuition increases for all three of these schools, and therefore the tuition increases at these three institutions is very consistent across the different institutions.

Table 2.3: Synthetic Control Group Weights

	ASU	UA	NAU
Enrollment			
Florida State University	57.1%	57.2%	-
Michigan State University	26.3%	30.3%	-
University of Minnesota-Twin Cities	11.2%	8.2%	21.1%
University of New Mexico-Main Campus	4.2%	-	-
University of Washington-Seattle Campus	-	-	21.8%
University of Wisconsin Colleges	-	-	47.4%
Tuition			
Michigan State University	16.5%	16.5%	16.5%
University of Minnesota-Twin Cities	32.9%	32.9%	32.8%
University of New Mexico-Main Campus	10%%	10.1%	10.1%
New Mexico State University-Main Campus	10%%	10%	10.1%
University of Wisconsin Colleges	30.5%%	30.5%	30.5%

2.3 Results

Table 2.4 presents the estimated impact of AIMS on applicants at ASU, UA, and NAU. The estimated AIMS effect on applicants is quite large, about 20 percent for both men and women, although it is not statistically significantly different than zero. This increase can also be seen graphically in Figure 2.2, which shows an increase in the average number of applicants in the treatment group compared to the control group after the implementation of AIMS in 2006. While the estimated impact is large, results are not statistically significant. This result can be explained if students "cast a wide net" when applying for colleges, and therefore apply in either the presence or absence of AIMS.

Table 2.4: Estimated AIMS Effect on Applicants and Admissions

	Applicants			Admissions					
	(1) Total	(2) Men	(3) Women	(4) Total	(5) Total	(6) Men	(7) Men	(8) Women	(9) Women
AIMS Effect	0.202	0.207	0.196	0.128	-0.0356	0.134	-0.0330	0.123	-0.0361
	(0.197)	(0.200)	(0.199)	(0.101)	(0.134)	(0.0978)	(0.104)	(0.0936)	(0.130)
Total					0.800***				
					(0.252)				
Men							0.801***		
							(0.224)		
Women									0.799***
									(0.204)
Ln(H.S. Grad)	-0.216	-0.202	-0.227	0.195	0.367	0.194	0.355	0.190	0.371
	(0.409)	(0.390)	(0.398)	(0.296)	(0.361)	(0.297)	(0.320)	(0.341)	(0.312)
Ln(Unemp)	-0.00871	-0.00418	-0.0136						
	(0.197)	(0.183)	(0.235)						
Ln(Total Wages)	2.075*	1.828*	2.302*						
	(1.087)	(0.991)	(1.246)						
Ln(FHFA HPI Index)	-0.332	-0.313	-0.354						
	(0.218)	(0.198)	(0.252)						
Observations	169	169	169	169	169	169	169	169	169

Bootstrapped standard errors are reported in parentheses. School and year fixed effects are included in all regressions. Treatment Group includes UA, ASU and NAU. Control Group includes peer institutions.

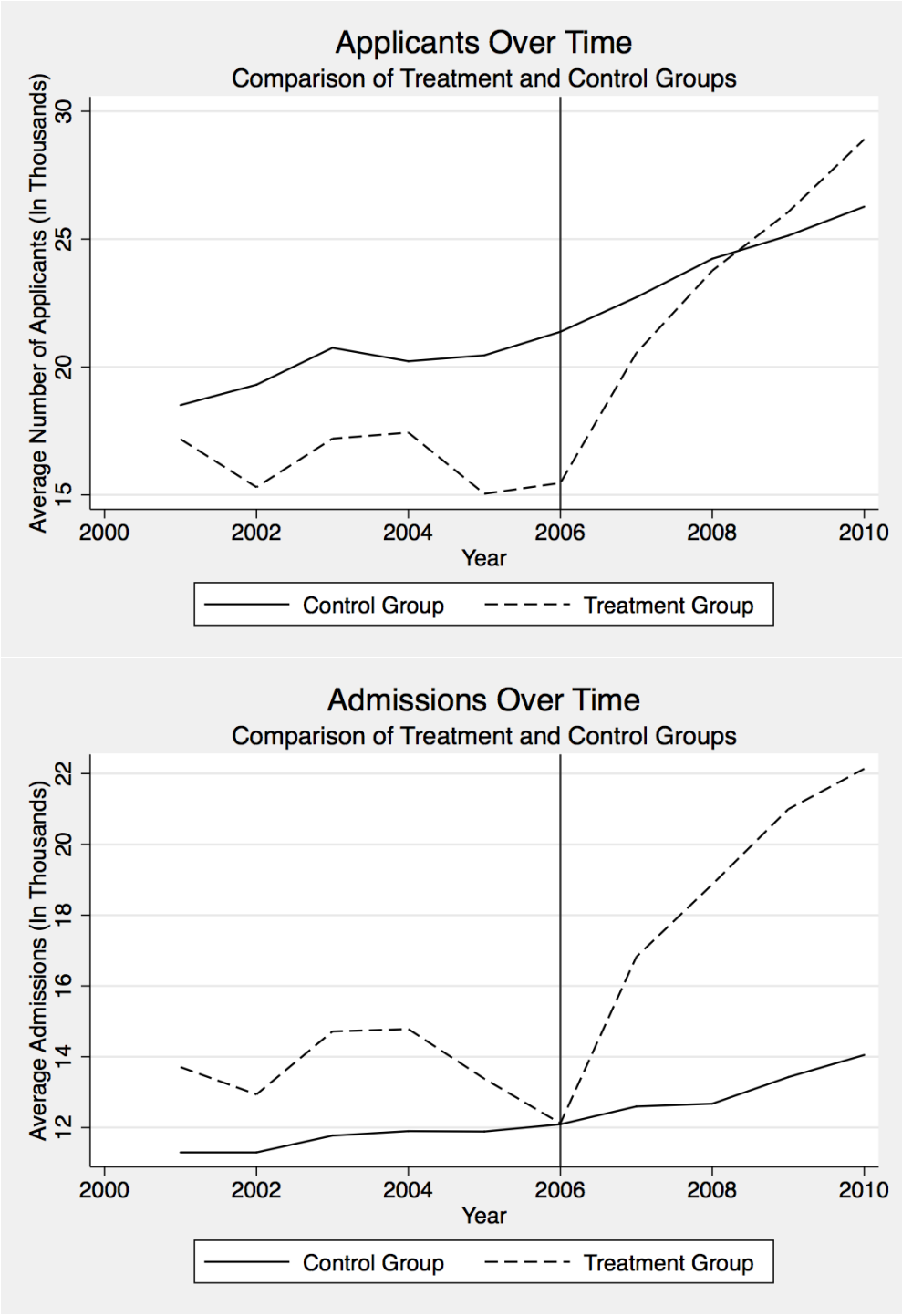


Figure 2.2: Average Number of Applicants and Student Admissions: Comparison of the Treatment and Control Groups

Table 2.4 presents the findings on the number of students who are admitted each year to ASU, UA, and NAU. These results serve two purposes. First, this tests whether more students were admitted after the implementation of AIMS. Secondly, it provides insight to whether the schools themselves changed their admissions policies due to the scholarship. As seen in Table 2.4, when the number of applicants is not controlled for, the estimated magnitude of the AIMS effect on student admissions is quite large, about 12 to 13 percent (although not statistically significant), but when the number of applicants is controlled for, the estimated AIMS effect attenuates and actually becomes slightly negative, but is still statistically insignificant. Figure 2.2 illustrates this increase in admissions in the treatment group compared to the control group at these universities after 2006. Again, though, due to the statistical insignificance of this effect, I do not have sufficient evidence to conclude that more students were admitted due to the scholarship. Equally as important, I find no evidence that the schools have changed their admittance decisions due to the scholarship, as the number of students admitted, holding applicants constant, also did not change.

Table 2.5 presents the estimated enrollment effects and shows a statistically significant overall AIMS effect as well as an impact for both men and women separately. A 15 percent increase in overall first-year first-time enrollment–hereafter simply referred to as "enrollment"–is estimated using this framework. The AIMS effect is estimated to be higher for women than for men, about 16-17 percent for women as compared to 14 percent for men. These results are robust when the unemployment rate, wages, and housing prices are used as covariates. I find no impact of either the unemployment rate or average weekly wages on enrollment. Because no AIMS effect was observed on either applicants or admissions, this increase in enrollment is most likely attributable to students' choices of where to attend college.

At first glance, this estimated increase in enrollment appears large compared to CMS who estimated a 5.9 percent increase in Georgia enrollment due to the HOPE scholarship. But upon further consideration, it is not at all surprising as the HOPE scholarship is available to students who attend any public college or university in Georgia, while AIMS is only

Table 2.5: Estimated AIMS Effect on Enrollment of First-Year First-Time Degree Seeking Freshmen

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
AIMS Effect	0.150***	0.152***	0.139***	0.139***	0.165***	0.170***
	(0.0422)	(0.0410)	(0.0469)	(0.0465)	(0.0490)	(0.0519)
Ln(H.S. Grad)	0.120	0.126	0.148	0.162	0.0916	0.0902
	(0.109)	(0.103)	(0.153)	(0.143)	(0.118)	(0.142)
Ln(Unemp)		-0.0242		-0.0635		0.00997
		(0.137)		(0.202)		(0.0593)
Ln(Total Wages)		0.571		0.334		0.767*
		(0.447)		(0.478)		(0.426)
Ln(FHFA HPI Index)		-0.137		-0.0888		-0.190***
		(0.0914)		(0.152)		(0.0638)
Observations	169	169	169	169	169	169
R^2	0.391	0.407	0.413	0.417	0.299	0.337

Bootstrapped standard errors are reported in parentheses. School and year fixed effects are included in all regressions. Treatment Group includes UA, ASU and NAU. Control Group includes peer institutions.

available to three in-state institutions. Therefore, it is not surprising that these institutions see a relatively large percentage increase in enrollment compared to the overall percentage increase that all colleges and universities in Georgia received.

Next, Table 2.6 presents estimates of the AIMS effect on enrollment for each university separately. The enrollment effects for ASU and UA are roughly the same, between 10 and 11 percent. The enrollment effect for NAU is much larger: about 24-25 percent. While the percent increase in enrollment for NAU is much larger, NAU is a much smaller school and therefore the actual increase in the number of students is not necessarily larger. For instance, in 2005, the year before AIMS was implemented, NAU enrolled 2,105 students, while ASU and UA enrolled 7,396 and 5,785 students respectively. Therefore, while the percent increases for NAU is larger, the actual magnitude of the increase in number of students is similar. This estimated increase in the number of students enrolled is discussed in detail in Section 2.3.2.

Table 2.6: Estimated AIMS Effect on the number of First Year First Time Degree Seeking Freshmen By School

	ASU		UA		NAU	
	(1)	(2)	(3)	(4)	(5)	(6)
AIMS Effect	0.113***	0.111***	0.108***	0.107***	0.246***	0.247***
	(0.0260)	(0.0364)	(0.0222)	(0.0257)	(0.0206)	(0.0289)
Ln(H.S. Grad)	0.118	0.135	0.0175	0.0424	0.0756	0.0955
	(0.156)	(0.200)	(0.126)	(0.124)	(0.110)	(0.175)
Ln(Unemp)		-0.0224		-0.0519		-0.0131
		(0.0870)		(0.131)		(0.131)
Ln(Total Wages)		0.471		0.309		0.569
		(0.480)		(0.556)		(0.432)
Ln(FHFA HPI Index)		-0.0810		-0.0727		-0.144
		(0.0765)		(0.110)		(0.0908)
Observations	149	149	149	149	149	149
R^2	0.281	0.292	0.284	0.292	0.374	0.394

Bootstrapped standard errors are reported in parentheses. School and year fixed effects are included in all regressions. Treatment Group includes UA, ASU and NAU. Control Group includes peer institutions.

As presented in Table 2.1, ASU experienced an 11.7 percent increase in enrollment from 2005 to 2010, and Table 2.6 presents the estimated AIMS effect on enrollment at ASU to be approximately 11 percent. UA and NAU, on the other hand, experienced an approximately 18 and 80 percent increase in enrollment respectively over this same time period, but the estimated impact of AIMS on enrollment is about 11 and 25 percent respectively. According to these results, almost all of the increase in enrollment seen at ASU after the implementation of AIMS can be attributed to the AIMS scholarship, while only 61 and 31 percent respectively of the increases in enrollment at UA and NAU can be associated with AIMS. Less formally, enrollment would have increased at UA and NAU regardless of whether AIMS was implemented, but enrollment increased more than it otherwise would have due to the implementation of AIMS.

Table 2.7 presents a statistically significant impact of AIMS on enrollment for white students: approximately a 6 to 8 percent increase. The estimated AIMS effect is larger for women than for men and the estimated enrollment effect is not statistically significant for men. It should also be noted that the estimated AIMS effect on white students is significantly lower than the overall AIMS effect presented in Table 2.5. This indicates that the AIMS effect is likely larger for minorities. This result is consistent with CMS's findings on the impact of HOPE on enrollment in Georgia. Table 2.7 presents coefficient estimates for enrollment effects on black students. These point estimates are much larger than the estimated AIMS effect for white students. Overall AIMS has increased enrollment of black students by about 25 percent. The estimated AIMS effect on enrollment for black men is 32 percent–higher than for any other group. While the AIMS effect was estimated to be slightly larger for white women than for white men, the converse is observed with black women and men as the AIMS effect for black men is about 13 percentage points larger than for black women.

Table 2.7 shows the estimated AIMS effect on Hispanic enrollment. These point estimates are on average larger than the overall estimated AIMS effects, but not as large in magnitude as the coefficients estimating black enrollment effects. Specifically, a 21 to 23 percent enrollment effect is estimated. The estimated effect is similar for Hispanic men and women. Again these results are robust to the inclusion of covariates that control for economic conditions.[22] Consistent with results on the Georgia HOPE scholarship, the AIMS scholarship has a larger impact on minority enrollment than white student enrollment. This result is intuitive, as minority students, on average, might be subject to budget constraints to a larger extent than their white counterparts, and therefore a scholarship program can be more effective at increasing enrollment.

[22]Results for the AIMS effect on American Indian and Asian student enrollment is also available upon request. These results are not statistically significant and therefore regression tables are not included.

Table 2.7: Estimated AIMS Effect on Enrollment by Race

	White			Black			Hispanic		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Men	Women	All	Men	Women	All	Men	Women
AIMS Effect	0.0710***	0.0586*	0.0867**	0.250**	0.321**	0.193**	0.211***	0.211***	0.211***
	(0.0251)	(0.0336)	(0.0387)	(0.106)	(0.141)	(0.0888)	(0.0300)	(0.0608)	(0.0358)
Ln(Unemp)	-0.100	-0.0431	-0.162	-0.396	-0.146	-0.593**	-0.317**	-0.214	-0.411**
	(0.167)	(0.160)	(0.183)	(0.247)	(0.168)	(0.258)	(0.128)	(0.224)	(0.185)
Ln(Total Wages)	-0.345	-0.552	-0.153	2.138	1.406	2.576	0.952	1.200	0.814
	(0.519)	(0.473)	(0.579)	(1.859)	(2.013)	(1.884)	(1.355)	(1.774)	(1.245)
Ln(FHFA HPI Index)	-0.0484	0.0792	-0.179	-0.420	-0.224	-0.565	-0.0373	0.0325	-0.103
	(0.148)	(0.140)	(0.176)	(0.368)	(0.359)	(0.387)	(0.138)	(0.172)	(0.165)
Observations	143	143	143	143	143	143	143	143	143
R^2	0.051	0.093	0.098	0.249	0.229	0.245	0.653	0.609	0.562

Bootstrapped standard errors are reported in parentheses. School and year fixed effects are included in all regressions. Treatment Group includes UA, ASU and NAU. Control Group includes peer institutions.

The AIMS scholarship can only be used at one of the three schools being analyzed in the previous regressions: ASU, UA, and NAU. For this reason, it is not surprising that AIMS had an impact on enrollment at these schools, but it is unknown what these new enrollees would have done had they not attended one of these three universities. First, these students may not have attended college at all if AIMS was not available. Because AIMS has very stringent requirements (a 3.5 high school GPA as well as a passing score on three exams) it seems unlikely that these students would not attend college in the absence of AIMS. A second possibility is that these students are substituting away from other schools in Arizona to attend ASU, UA, or NAU due to the scholarship. Third, the students may substitute away from out-of-state colleges and universities and instead choose to stay in-state. I explore these three possibilities.

First, I test whether students are substituting away from other in-state schools to attend ASU, UA, and NAU. Table 2.8 presents the estimated enrollment effects of AIMS at other colleges and universities in Arizona. Specifically, in these regressions the treatment group contains all Arizona schools except ASU, UA and, NAU, while the control group contains all schools in in New Mexico, Texas, and Oklahoma. I estimate separate regressions for (a) all colleges and universities in Arizona, (b) all four-year (public and private) colleges and universities, as well as (c) two-year institutions. The results presented in Table 2.8 serve two purposes. First, they test to see if AIMS had a negative impact on enrollment at other institutions in the state. If a negative relationship is found, then it provides evidence that students are substituting away from other schools in Arizona. It also serves as a falsification test for previous results. Because AIMS is not available to students who attend these other in-state schools, if a positive significant AIMS effect on enrollment is found, this will be a warning that previous results are potentially problematic. As can be seen in Table 2.8, no AIMS effect is observed on enrollment at any institution and therefore the substitution hypothesis is not supported.[23]

[23]Separate analysis was conducted analyzing the impact of the AIMS scholarship on Maricopa Community College system's enrollment. Maricopa Community College has seven separate schools located near Phoenix

Table 2.8: Enrollment Effects of AIMS at Arizona Colleges and Universities

	All		4-Year		2-Year	
	(1)	(2)	(3)	(4)	(5)	(6)
AIMS Effect	0.145	0.0804	0.189	0.122	0.248	0.136
	(0.218)	(0.186)	(0.216)	(0.187)	(0.200)	(0.193)
Ln(H.S. Grad)	0.404	0.280	0.408	0.139	0.325	0.156
	(0.302)	(0.361)	(0.345)	(0.367)	(0.483)	(0.410)
Ln(Unemp)		0.275		0.260		0.503
		(0.237)		(0.243)		(0.380)
Ln(Total Wages)		-1.023		-2.005		-1.356
		(1.385)		(1.423)		(2.833)
Ln(FHFA HPI Index)		0.284		0.341		0.460
		(0.300)		(0.297)		(0.401)
Observations	1010	1010	890	890	540	540
R^2	0.028	0.030	0.053	0.057	0.053	0.057

Bootstrapped standard errors are reported. School and year fixed effects are included in all regressions. Treatment group includes colleges and universities in Arizona. Control group includes collegs and universities in NM, TX, and OK. ASU, UA, and NAU are not included in any of these regressions.

Due to the lack of evidence that students are substituting away from other colleges and universities in Arizona to attend either ASU, UA, or NAU, I explore the other two possible explanations for these new students. First, these new students may otherwise not attend college if AIMS were not available. Second, students might attend schools out of state if not for AIMS. While I cannot directly test which of these effects dominates, I can test whether the quality of students at ASU, UA and NAU changed due to the AIMS scholarship using two different measures: incoming freshmen American College Testing (ACT) scores and retention rates. Students who are on the margin of whether to attend college may have lower credentials (such as ACT scores) upon entering college and will potentially be more

Arizona. I find no impact of AIMS on these colleges, and therefore I find no evidence to support the hypothesis that students are substituting away from these local community colleges to attend ASU, UA or NAU. These results are available upon request.

likely to drop out. If the quality of students does not change or increases due to AIMS, then it is unlikely that these new students were on the margin of whether to attend college.

Table 2.9 tests for the impact of AIMS on ACT scores.[24] I find a decrease in composite ACT test scores by less than 1 point (.768 points). While this result is statistically significant, it is not economically significant, as the ACT has a maximum score of 36 points, with a mean of 18 and standard deviation of 6. Therefore, I estimate a decrease of less than 15 percent of one standard deviation. Furthermore, when separate regressions are run on English or math scores, no statistically significant impact is found. Therefore, the quality of incoming freshmen as measured by ACT scores has not declined. One potential explanation for the very slight drop in ACT scores might have to do with the AIMS scholarship itself. Because AIMS does not require that students perform well on the ACT, but rather on the three AIMS exams in high school, students may substitute studying away from the ACT toward the AIMS exams. This result contrasts with CMS, who found that HOPE led to an increase in SAT scores.[25]

Table 2.9 also tests for quality of student by estimating AIMS' impact on retention rates. Retention rates are the proportion of students enrolled in one semester that are still enrolled the next semester, excluding students who graduate. These regressions estimate that AIMS increased retention rates by less than 2 percent, but these are not statistically significant at any level.[26] Therefore, there is no evidence to conclude that AIMS had a negative impact on retention rates at ASU, UA and NAU. These results are robust when economic conditions are controlled for. Because I find no evidence that the quality of students at these universities decreased after the implementation of AIMS and no evidence that students are substituting

[24]University of Maryland –College Park and University of Connecticut are excluded from these regressions as applicants at these schools primarily take the SAT, unlike the other schools in the sample who are primarily interested in ACT scores.

[25]The SAT test is similar to the ACT test in that it is a standardized test that is taken before applying to college. SAT was previously an acronym for "Scholastic Assessment Test" but is now an empty acronym.

[26]It should be noted that data is not available on retention rates of just first year students, therefore this is not a perfect measure of retention of students exposed to AIMS.

away from other in-state colleges, the most likely explanation is that students are substituting away from out of state colleges to stay in Arizona.[27]

Table 2.9: Estimated AIMS Effect on ACT Scores and Retention Rates

	ACT Scores			Retention
	(1) Composite	(2) English	(3) Math	(4)
AIMS Effect	-0.700**	-0.315	-0.433	0.194
	(0.342)	(0.408)	(0.529)	(0.438)
Ln(Unemp)	0.0736	0.328	0.995	-0.885
	(0.351)	(0.574)	(0.628)	(1.179)
Ln(Total Wages)	2.784	4.310	8.238*	-15.99
	(4.451)	(5.157)	(4.905)	(12.42)
Ln(FHFA HPI Index)	-0.376	-0.220	0.0920	0.191
	(0.485)	(0.691)	(0.694)	(1.369)
Observations	132	120	120	115
R^2	0.584	0.578	0.602	0.424

Bootstrapped standard errors are reported in parentheses. School and year fixed effects are included in all regressions. Treatment Group includes UA, ASU and NAU. Control Group includes peer institutions.

Finally, Table 2.10 presents estimates of the AIMS effect on in-state tuition and fees. I find a statistically significant and large impact of a 16 to 20 percent increase in tuition and fees due to the AIMS scholarship. Table 2.1 shows a much larger change in tuition from 2005 to 2010–approximately a 75 to 85 percent increase. These results suggest that there would have been an increase in tuition regardless of whether AIMS was implemented. However, tuition increased more than it otherwise would have due to AIMS. This result is intuitive. Because some students' education is subsidized, the price sensitivity is reduced. Therefore, universities have greater liberty to increase tuition without compromising enrollment.

Next, the main result of the paper is replicated using SC methods. In particular, the overall AIMS effect on enrollment and tuition is estimated. The difference between the

[27]Retention rates are for all students, not just students who drop out after their first year. For this reason, this is an imperfect measure of retention rates of the impacted students.

Table 2.10: Estimated AIMS Effect on In State Tuition and Fees

	ASU		UA		NAU	
	(1)	(2)	(3)	(4)	(5)	(6)
AIMS Effect	0.192***	0.187***	0.190***	0.185***	0.165***	0.158***
	(0.0245)	(0.0251)	(0.0202)	(0.0297)	(0.0214)	(0.0325)
Ln(H.S. Grad)		0.179		0.181		0.196
		(0.130)		(0.120)		(0.146)
Ln(Unemp)		-0.180		-0.183*		-0.184
		(0.114)		(0.111)		(0.116)
Ln(Total Wages)		1.940***		1.937*		1.910**
		(0.581)		(1.018)		(0.916)
Ln(FHFA HPI Index)		-0.553***		-0.552***		-0.546***
		(0.138)		(0.120)		(0.112)
Observations	150	149	150	149	150	149
R^2	0.917	0.940	0.917	0.939	0.918	0.941

Bootstrapped standard errors are reported in parentheses. School and year fixed effects are included in all regressions. Treatment Group includes UA, ASU and NAU. Control Group includes peer institutions.

synthetic schools' enrollment and actual enrollment as well as the difference between the synthetic schools' tuition and actual tuition is presented in Figure 2.3. As can be seen, the difference between the treated schools and their respective synthetic schools increased after AIMS was implemented. Furthermore, the difference between the other controls and their synthetic schools did not change before and after the treatment.

Figure 2.4 illustrates these results further by showing a histogram of the estimated treatment effect using only each school and its synthetic control. Equations 2.2 and 2.3 illustrate the DD estimation used to estimate the AIMS effect on enrollment and tuition where there is only one treatment school and one control school (the synthetic control). Each school in the sample is "treated" and compared to its synthetic school. If the previous results in this paper are robust, then δ will be approximately zero for the non-Arizona schools and δ will be similar to the point estimates presented in Tables 2.6 and 2.10 for the Arizona schools.

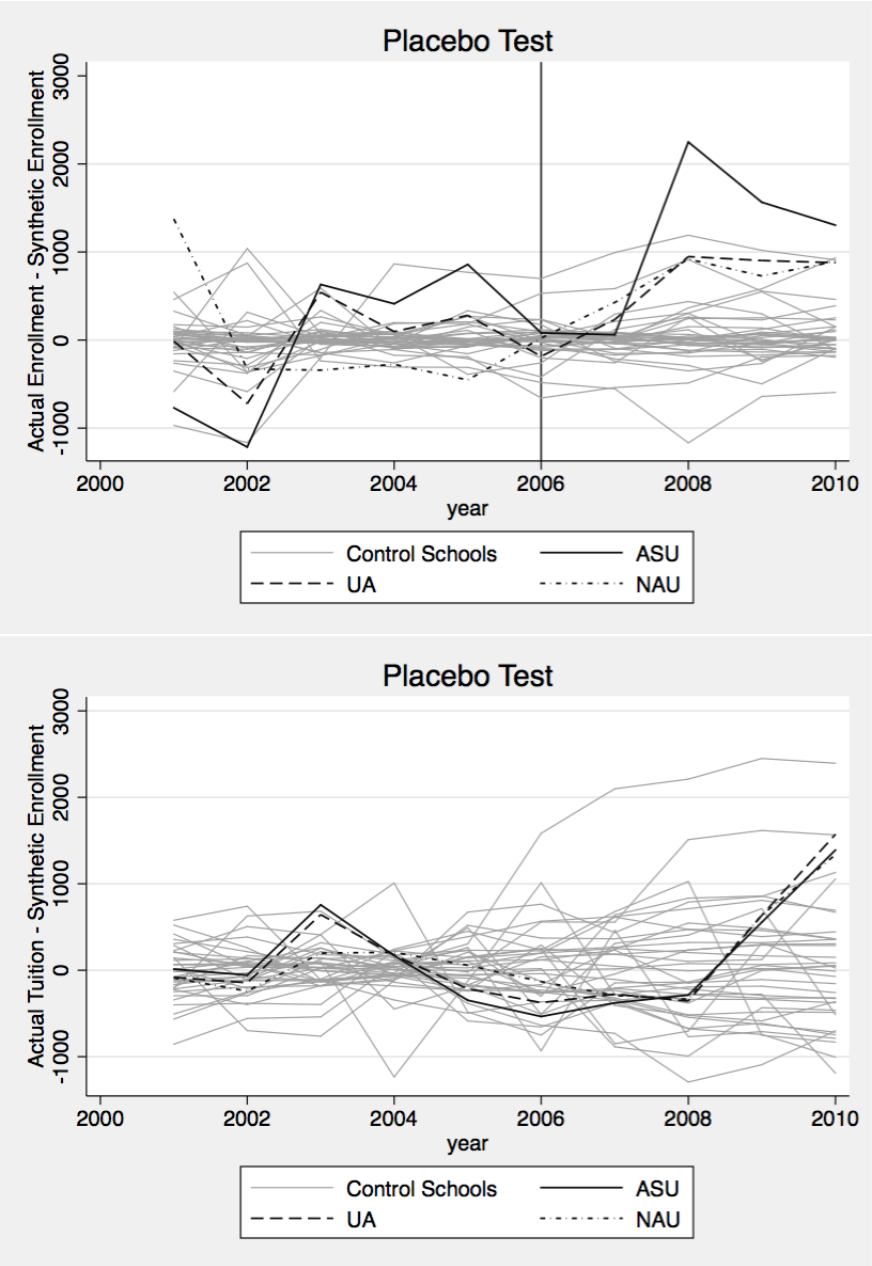


Figure 2.3: Enrollment and Tuition Gaps at Arizona Schools and Placebo Gaps in Non-Arizona Schools.

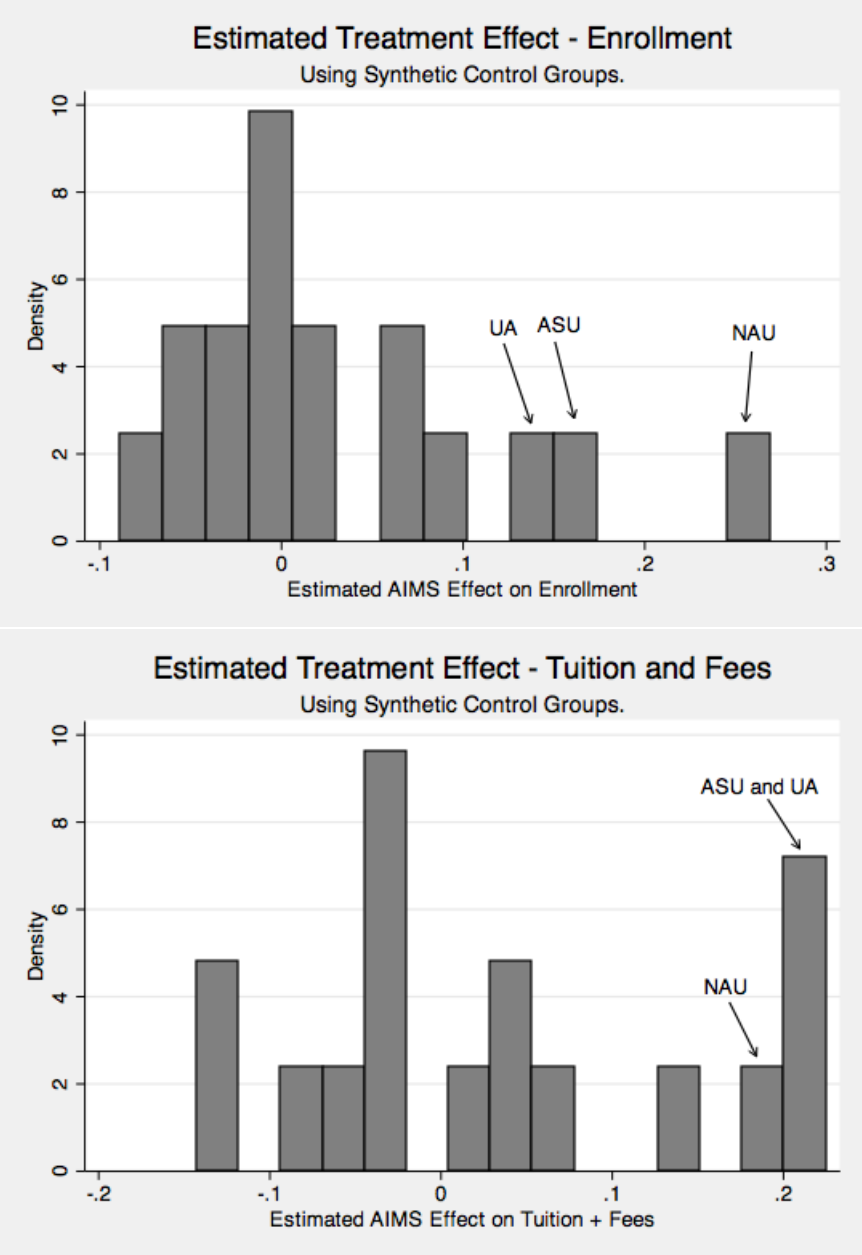


Figure 2.4: Histograms of estimated treatment effects for Arizona schools and non-Arizona placebo schools.

$$ln(E_{ist}) = \alpha + \delta(S_T \times A_t) + \gamma S_{is} + \beta_t Y_t + \varepsilon_{ist} \tag{2.2}$$

$$ln(T_{ist}) = \alpha + \delta(S_T \times A_t) + \gamma S_{is} + \beta_t Y_t + \varepsilon_{ist} \tag{2.3}$$

The estimated AIMS Effect on Enrollment for ASU, UA and NAU using this synthetic control group is 16.8%, 14.9% and 26.83% respectively. Note that these estimates are actually slightly larger than the results presented in Table 2.6, thus giving evidence that if anything, DD results are downward biased. The estimated AIMS effect on tuition and fees using the SC group for ASU, UA and NAU are 21.2%, 20.9% and 18.5% respectively. These results are similar to the estimated AIMS effect of 16-19.5% presented in Table 2.10. A comparison of the results from the DD and SC methods are presented in Table 2.11.

Table 2.11: Comparison of DD and SC Results

	DD	SC	Observed Δ
Enrollment			
ASU	11.1%	15.7%	11.7%
UA	10.8%	13.7%	17.6%
NAU	24.6%	26.9%	80%
Tuition + Fees			
ASU	18.7%	22.5%	84.6%
UA	18.5%	22.3%	83.1%
NAU	15.8%	19.8%	74.6%

Next, I implement a placebo test by estimating the AIMS effect for every other school in the sample relative to its synthetic school. Figure 2.4 illustrates that the estimated AIMS effect on both enrollment and tuition are large compared to the estimated effects for the other schools that were used as a placebo test. In fact, all of the other schools' estimated AIMS effects are centered around zero. The estimated AIMS effects on tuition for ASU, UA and NAU are three of the top four and the estimated AIMS effect on tuition of ASU, UA and NUA are the three highest. This speaks to the robustness of these results.

2.3.1 Additional Robustness Checks

There have been substantial critiques to difference-in-differences estimation (Bertrand, Duflo, and Mullainathan, 2004; Abadie, Diamond, and Hainmueller, 2010). Due to critiques primarily about potential overestimation of standard errors, non-robustness to placebo tests, and improper controls, researchers have been pushed to conduct additional robustness checks to assure that results from difference-in-differences estimation are indeed valid. These critiques are especially important when there is relatively small number of units being observed, N, relative to the number of time periods, T, which is certainly the case in this research. For these reasons, I have conducted additional robustness checks that together illustrate that it is very unlikely that the results in this paper are due to misspecification of standard errors, serial correlation, or an inappropriate control group.

One well-know problem with difference-in-differences estimation is the potential downward bias in the standard errors (Bertrand, Duflo, and Mullainathan, 2004). This can be especially true if the number of groups, N, is not sufficiently large compared to the number of time periods, T. In order to account for this potential issue, all standard errors presented in the previous results have been bootstrapped.[28] While bootstrapping does mitigate concerns about the large-N-small-T problem, it does not improve estimation in small samples.

In order to mitigate concerns about the validity of confidence intervals due to the small number of observations used in this research, I calculate asymptotically refined confidence intervals that are then compared the bootstrapped standard errors for the main specification. The advantage to using such asymptotically refined confidence intervals is that they obtain an approximation error that disappears at a rate faster than the standard t-test statistic used to test statistical significance with the standard errors obtained through bootstrapping

[28]When the same regressions are run with standard errors clustered at the institution level, results are similar. Bootstrapped standard errors are the most conservative, as cluster corrections are are only asymptotically valid when the number of groups in the panel data context is sufficiently large, which is certainly not the case in this application.

(Cameron and Trivedi, 2009). These asymptotically refined critical values will produce a more conservative estimate than the standard t-distribution.

I calculate asymptotically refined critical values for the main results of this paper which are presented in Tables 2.5 through 2.10. For example, asymptotically refined critical values for regression 1 in Table 2.5 are 2.35, 2.93, and 4.18 (p=.10, p=.05 and p=.01 respectively). The test static had a value of 3.47 for this regression, and thus statistical significance decreases from significant at p=.01 to p=.05. While the level of statistical significance changes in some regressions in Tables 2.5 and 2.10, the overall statistical significance of the result does not. Thus, using conservative bootrapped standard errors as well as conservative asymptotically refined confidence intervals does not change the main result.[29]

Another well-known problem with difference-in-differences estimation is the fact that it has not been found to be robust to serial correlation in many applications. For instance, Bertrand, Duflo, and Mullainathan (2004) finds that serial correlation is a potential problem in a survey of papers using difference-in-differences estimation due to the relatively large number of time periods, as the average in their sample of papers is 16.5 periods. Furthermore, the most commonly used dependent variables in the surveyed papers are shown to be highly serially correlated. In response to this well known critique, the next robustness check employed controls for serial correlation that can potentially cause bias in estimating the impact of the AIMS scholarship on enrollment.

Two tests are conducted in order to control for potential bias in the estimation due to serial correlation. First, lags of the two main dependent variables in this paper–enrollment and tuition and fees–are used. These results are presented in Tables 2.12 and 2.13. The estimated coefficient for the AIMS effect on enrollment remains virtually unchanged when one lag of the dependent variable is included. When two and three lags are included, the coefficient attenuates from about 15 percent to about 12 percent, but the results remains statistically significant even when covariates are used. The impact of lagged dependent

[29]Please contact me for complete asymptotically refined confidence intervals.

variables attenuates results more significantly for tuition and fees. The estimated AIMS effect on tuition and fees declines from 17 percent to about 7 percent when lagged dependent variables are employed.

In order to estimate the AIMS effect adjusting for serial correlation, the Arellano-Bond (AB) estimator (Arellano and Bond, 1991; Windmeijer, 2005) is also employed using one, two, and three lags. These results are presented in Tables 2.12 and 2.13. The AB estimator is used as OLS when lagged dependent variables and serial correlated errors can lead to inconsistent parameter estimates. This additional robustness check will add one more level of stringency. When the AB estimator is employed using one lag, the AIMS effect on enrollment is still statistically significant at p=.05, but the magnitude decreases from about 15 percent (when this estimator is not employed) to 11 percent. The result is similar when two lags are employed. When the third lag is employed, the statistical significance attenuates completely and the estimated AIMS effect attenuates to 7 percent. The estimated AIMS effect on tuition and fees also decreases substantially when the AB estimator is used–from 17 percent to about 5 percent when one lag is used and 3 percent when two and three lags are employed.

2.3.2 Percent of Scholarship Dollars Spent on Marginal Students

I have shown that AIMS has led to an increase in first-year first-time freshmen enrollment at ASU, UA, and NAU. In this section, I consider the following question: What percent of the scholarship recipients would not have attended one of the three treatment schools if they were not eligible for AIMS? Conversely, what percent of the students who receive AIMS would attend one of these three schools even if they were not eligible for AIMS (i.e., their college decision is not altered by the scholarship)? I consider the following:

$$E_i^a = (1 + \hat{\delta}_{i,1})E_i^c \tag{2.4}$$

Table 2.12: Estimated AIMS Effect on First-Year First-Time Enrollment Robustness Checks

	(1) No Lag	(2) 1 Lag	(3) 2 Lag	(4) 3 Lag	(5) AB 1 Lag	(6) AB 2 Lag	(7) AB 3 Lag
AIMS Effect	0.152***	0.151***	0.124***	0.118***	0.114	0.105	0.0731
	(0.0334)	(0.0317)	(0.0345)	(0.0364)	(0.0778)	(0.0754)	(0.0666)
Ln(Unemp)	-0.0242	-0.110	-0.159*	-0.132	-0.156	-0.120	-0.133
	(0.0795)	(0.0818)	(0.0906)	(0.0989)	(0.137)	(0.159)	(0.190)
Ln(Total Wages)	0.571	0.375	0.706	0.693	1.654**	1.433***	1.390***
	(0.476)	(0.453)	(0.484)	(0.579)	(0.710)	(0.519)	(0.514)
Ln(FHFA HPI Index)	-0.137	-0.102	-0.176*	-0.164	-0.139	-0.127	-0.125
	(0.0831)	(0.0865)	(0.0946)	(0.113)	(0.0905)	(0.0999)	(0.132)
Ln(Total Enrollment)$_{t-1}$		0.198**	0.344***	0.285***	0.410***	0.439***	0.365***
		(0.0790)	(0.0941)	(0.103)	(0.154)	(0.145)	(0.129)
Ln(Total Enrollment)$_{t-2}$			-0.00759	0.155		0.0396	0.220**
			(0.0816)	(0.103)		(0.0596)	(0.112)
Ln(Total Enrollment)$_{t-3}$				-0.108			-0.0307
				(0.0941)			(0.0384)
Observations	169	152	135	119	134	118	102
R^2	0.407	0.521	0.536	0.518			

Arellano-Bond (AB) estimation used in regressions 5-7.

Robust standard errors are reported. School and year fixed effects are included in all regressions.

Table 2.13: Estimated AIMS Effect on Full Time Tuition and Fees Robustness Checks

	(1) No Lag	(2) 1 Lag	(3) 2 Lag	(4) 3 Lag	(5) AB 1 Lag	(6) AB 2 Lag	(7) AB 3 Lag
AIMS Effect	0.169***	0.0717**	0.0606**	0.0707***	0.0468*	0.0325	0.0315**
	(0.0323)	(0.0284)	(0.0268)	(0.0244)	(0.0261)	(0.0213)	(0.0158)
Ln(Unemp)	-0.203***	-0.0964	-0.0344	0.0635	-0.102	-0.0464	0.0272
	(0.0768)	(0.0727)	(0.0682)	(0.0640)	(0.0860)	(0.0483)	(0.0681)
Ln(Total Wages)	2.113***	1.566***	1.384***	1.384***	1.927***	1.471***	1.636***
	(0.460)	(0.401)	(0.367)	(0.346)	(0.566)	(0.340)	(0.572)
Ln(FHFA HPI Index)	-0.582***	-0.388***	-0.400***	-0.280***	-0.392***	-0.427***	-0.311***
	(0.0803)	(0.0788)	(0.0707)	(0.0687)	(0.0891)	(0.0584)	(0.0902)
$\text{Ln(Tuition and Fees)}_{t-1}$		0.601***	0.748***	0.809***	0.654***	0.717***	0.642***
		(0.0719)	(0.0877)	(0.0818)	(0.119)	(0.0929)	(0.135)
$\text{Ln(Tuition and Fees)}_{t-2}$			-0.189**	-0.141		-0.112	-0.0667
			(0.0903)	(0.0942)		(0.0878)	(0.115)
$\text{Ln(Tuition and Fees)}_{t-3}$				-0.0576			-0.0174
				(0.0813)			(0.0944)
Observations	169	152	135	119	134	118	102
R^2	0.945	0.954	0.954	0.959			

Arellano-Bond (AB) estimation used in regressions 5-7.

Bootstrapped standard errors are reported. School and year fixed effects are included in all regressions.

Where E_i^a is the actual number of students who were enrolled in school i in 2010 and E_i^c is the "counterfactual" number of students school i, or the number of students who would be enrolled if AIMS were not implemented. Solving for $E_i^a - E_i^c$, I get the following

$$E_i^a - E_i^c = E_i^a\Big(\frac{\hat{\delta}_{i,1}}{1+\hat{\delta}_{i,1}}\Big) \tag{2.5}$$

This equation provides the estimated difference in the number of students who actually attended one of the treatment schools that would not have attended if AIMS were not available. Table 2.1 shows the actual 2010 enrollment at each of these three schools. Table 2.6 shows two estimates of $\hat{\delta}_{i,1}$ for each school. For these calculations, I used the estimated coefficients in regressions (2), (4), and (6) as they include the full list of covariates. Using these inputs in Equation 2.5, I find that enrollment increased by 825, 663, and 748 students at ASU, UA, and NAU respectively.[30]

These are the estimated number of students who attended ASU, UA and NAU that would not have otherwise done so without AIMS. I do not know the number of students who actually received the scholarship at each of these schools, but I do know that 2,935 students in total utilized the scholarship in 2009.[31] Summing the additional students from (4)-(6) yields 2,217 students. Therefore, I estimate that 718 of the 2,935 students, or 24 percent, who received the scholarship in 2009 would have attended one of these schools regardless of whether they were eligible for AIMS. Therefore, conversely, 76 percent of the students who received the AIMS scholarship would not have attended ASU, UA, or NAU if the scholarship was not available. They would have (a) not attended college at all (b) attended a school out of Arizona or (c) attended another school in state. I am unable to empirically examine the magnitude of each of these alternatives, but as discussed previously, it appears that (b) is the most likely explanation for these additional students.

[30] $E_{ASU}^a - E_{ASU}^c = 8,261\Big(\frac{.111}{1+.111}\Big) = 825$ Students, $E_{UA}^a - E_{UA}^c = 6,804\Big(\frac{.108}{1+.108}\Big) = 663$ Students, $E_{NAU}^a - E_{NAU}^c = 3,789\Big(\frac{.246}{1+.246}\Big) = 748$ Students

[31] This data has not been released for 2010.

Using these calculations, I can also obtain a rough estimate of the dollar value of the transfer to students who would attend ASU, UA, or NAU regardless of whether the scholarship was available. Multiplying a simple weighted average of tuition in 2010 from Table 2.1 ($8,788) by the 718 students who would have attended one of these universities regardless of whether the scholarship was available, yields an estimated $5.8 million subsidy to incoming freshmen that did not affect enrollment decisions.

2.4 Conclusions

This paper finds strong evidence of an AIMS effect on enrollment at Arizona's three large in-state universities: Arizona State University (ASU), University of Arizona (UA) and Northern Arizona University (NAU). These enrollment effects are strongest for black and Hispanic students. I do not find evidence that AIMS impacts the quality of students at these universities as measured by ACT scores of incoming freshmen and retention rates. I do, however, find that AIMS led to an increase in tuition and fees. Results obtained from DD estimation as well as SC groups are consistent. Furthermore, the specification is robust to placebo tests.

While these results are quite consistent with CMS's analysis of the HOPE scholarship, there are several differences between AIMS and HOPE that can provide insight about statewide merit-based scholarships. First, the scholarships were implemented in different time periods. HOPE was implemented in 1993 while AIMS began in 2006. Because a growing number of students are attending college every year, the marginal contribution of any program aimed at increasing enrollment might be hypothesized to be lower today than it was in 1993. Therefore, analyzing a scholarship that was implemented in 1993 might not be relevant for a policy maker wanting to create or change a policy today. My research shows that AIMS was effective at increasing enrollment in 2006.

The second obvious difference between the programs is the geographic region of the country where the scholarships were implemented. If these programs are more effective for a

particular demographic of students, and that demographic is more common in Georgia than Arizona, for instance, then this might explain the difference in outcomes. In particular, I find that the scholarship is most effective among black and Hispanic students. Given these results, it is not surprising that both AIMS and HOPE were effective in their respective states whose populations are comprised of a large percent of Hispanics and blacks respectively.

Finally, it is possible that the requirements of obtaining a scholarship might have an impact that scholarship's effectiveness. In particular, the AIMS scholarship has much stricter requirements than does HOPE, but nonetheless the program was still effective at increasing enrollment. It must be noted that Georgia's HOPE scholarship can be used at any approved school in Georgia, while AIMS is restricted only to Arizona's three largest state schools (ASU, UA, and NAU). It is unknown whether AIMS would have had an impact on enrollment at other schools in Arizona if it were available.

While we have learned a great deal about merit-based scholarship programs, there are still many questions that have not been answered. First, I have estimated that merit-based scholarship programs can lead to an increase in tuition. This increase in tuition, though can have a negative impact on enrollment (Berger & Kosta 2002, Dellas & Sakellaris 2003). This paper is only able to test for the net of the increase in enrollment due to the scholarship's availability to some students and the decrease in enrollment associated with the increase in tuition. Future research might be interested in testing for each effect separately.[32]

While similar, the effects of HOPE and AIMS on many of the variables of interest are not identical. I am only able to hypothesize on why these differences exist. Future research might be interested in the relationship between enrollment effects of a scholarship and the minimum requirements necessary to be eligible for the scholarship, for instance. It can be hypothesized that more stringent scholarships will have smaller impacts on enrollment, but the difference in magnitudes of these effects are unknown. This research will be useful to

[32]It is possible to use tuition as a control variable in the estimation of the AIMS effect on enrollment, but the inclusion of this variable will create an endogeneity problem, as tuition and enrollment are jointly determined by both the supply and demand for college. For this reason, tuition was not used as a control variable in these regressions.

policy makers when constructing or changing scholarships.

Future research might also be interested in whether scholarships have become less effective as time has progressed. This might be the case if less students today are "on the margin" of whether or not to attend college. If this is the case, then a scholarship program might have a smaller effect today than it did when HOPE was implemented. While this paper shows that AIMS was effective at increasing enrollment, it is unknown how much of an impact the program would have had on enrollment if it were implemented a decade earlier.

The possibilities for future research on state merit-based scholarship programs are vast, and a better understanding of these programs will allow policy makers to design programs that maximize outcomes of interest while minimizing the overall cost of the program. This research will add to the understanding of the AIMS scholarship in particular as well as to the broader literature on scholarship programs.

Chapter 3. Housing Wealth and College Enrollment

3.1 Introduction

As discussed in Chapter 2, college enrollment has increased dramatically in the United States since the 1970s. In 1970, 5.8 million students were attending college full time. By 2010, this number increased to 14.7 million, an increase of over 150 percent (Snyder and Dillow, 2011). It is therefore no surprise that a vast literature on college attendance has emerged within the last several decades. For instance, it is well documented that students respond to financial aid by increasing enrollment (Dynarski, 2004; Bettinger, 2004; Avery and Hoxby, 2004; Cornwell, Lee, and Mustard, 2005; Cornwell, Mustard, and Sridhar, 2006; Upton, 2014) and that increases in college tuition and fees can lead to a decrease in college enrollment (Jackson and Weathersby, 1975; John, 1990; Kane, 1994, 1995; Heller, 1997; Hemelt and Marcotte, 2011).

The relationship between economic conditions and college enrollment has been found to be countercyclical at both the graduate (Bedard and Herman, 2008) and undergraduate levels (Dellas and Koubi, 2003; Dellas and Sakellaris, 2003; Berger and Kostal, 2002; Black and Sufi, 2002; Card and Lemieux, 2000; Light, 1996; Betts and McFarland, 1995; Kane, 1994; Corman, 1983; Gustman and Steineier, 1981). While this has been observed empirically, theoretically the cyclicality of schooling is ambiguous. Low skilled wages decrease during a recession and therefore the opportunity cost of going to college is low. Furthermore, during a recession families' ability to help finance their children's college expenses decreases. More recent literature (Long, 2014) explored the net effect of the most recent 2008 Great Recession on college enrollment. Using states with large increases in unemployment and large reductions in home prices as the treatment group and states with relatively small changes

in unemployment and housing prices as the control, Long (2014) found that the net effect of the recession on college enrollment is positive, corroborating past research which found that college enrollment is countercyclical.

There has also been a vast literature on the impact of housing wealth on an assortment of economic outcomes. For instance, increases in housing wealth can lead to increases in personal consumption (Gan, 2010; Slacalek, 2009) as accessing home equity can be used as a means to smooth consumption over time (Hurst and Stafford, 2004). There is also suggestive evidence that increases in housing wealth can help to ease individual liquidity constraints as households with higher levels of housing wealth are more likely to own businesses or engage in other entrepreneurial activities (Hurst and Lusardi, 2004). Housing wealth can even impact the age at which people retire (Lusardi and Mitchell, 2007).

Merging these two strands of literature, some researchers have investigated the impact of housing markets on college enrollment. Housing wealth is a potential source of credit for families that are attempting to finance their childrens' education, as families can choose to borrow against their home's value to obtain a loan for their childrens' college education (Bennet, Peach, and Peristianai, 2001; Deep and Domanski, 2002; Greenspan and Kennedy, 2005; Doms and Krainer, 2007). Housing wealth is a very plausible source of financing for many college students as 85 percent of college attendees come from families who own a home and, for all but the wealthiest families, their home is their only major financial asset (Lovenheim, 2011). For these reasons, Lovenheim and Reynolds (2013) and Lovenheim (2011) tested for the impact of housing wealth on college choices and found that increases in housing wealth can lead to an increase in college enrollment and allows students to substitute away from two-year schools towards flagship four-year public universities.

I am able to improve on the existing literature that analyzes the impact of housing wealth on college decisions in several ways. First, I employ a large and detailed proprietary data set not previously used in this literature, BlackBox Logic (BBx), which provides information on over 90 percent of the privately securitized mortgages in the United States since the

year 1999 which includes about 21 million unique loans.[1] I have information on the amount of each individual loan at origination as well as detailed monthly payment histories of each mortgage. I know if and when a household has missed a mortgage payment or if they decided to make extra payments above the contractually obligated payments and therefore I know the amount outstanding on the loan. I aggregate individual loans up to the metropolitan areas where these homes are located for our measures of housing wealth. The data are described in detail in Section 3.2.1.

Second, Lovenheim and Reynolds (2013) and Lovenheim (2011) focused on the housing bubble that occurred in the late 1990s and early 2000s–extending their analysis to only 2005. I focus my attention on both the bubble and subsequent financial crisis by utilizing data from 2001 to 2010. I argue that the "shock" that occurred during the financial crisis to housing wealth is exogenous to college enrollment, therefore mitigating the need for instrumental variable estimation. If the severity of the housing crisis in an area was impacted by the college enrollment decisions within an area, then my exogeneity assumption will not be valid. I provide evidence that my assumption of exogeneity is plausible.

Thirdly, I am able to estimate the effect of housing wealth on college enrollment at private and public schools separately. I find that the housing crisis lead to an increase in enrollment at public institutions, and a decrease in enrollment in private institutions. Long (2014) found that college enrollment increased in areas where housing prices fell drastically. I expand on this result finding that students substitute away from more expensive private colleges to more affordable public institutions when their families experience a reduction in their housing wealth.

I am also able to extend my analysis to beyond housing wealth itself. I test if an increase in mortgage delinquencies and foreclosures lead to a decline in college enrollment. As households face severe financial distress and have fewer resources to pay for educational

[1]This paper focuses on data from 2001 to 2010.

expenses, this will potentially alter students' educational decisions. To date, no research has specifically tested the impact of foreclosures on college enrollment.

In Section 3.2.1 I describe the data used. I provide details on the construction of the housing wealth measures used in my models in 3.2.2-3.2.4. I briefly discuss some summary statistics in Section 3.2.5. Finally, in Section 3.2.6 I present the regression specifications utilized in my analysis.

3.2 Empirical Strategy

3.2.1 Data

Five datasets are employed in this paper. I use the Integrated Post Secondary Education Data System (IPEDS) published by the National Center for Education Statistics (NCES) for the independent variable of interest, college enrollment. IPEDS has yearly data on over 7,400 colleges and universities throughout the United States. The independent variable used in this paper is the first-year first-time enrollment of freshman for full time students.

Data on local housing prices comes from the seasonally adjusted Federal Housing Finance Agency house price index (FHFA-HPI). I use quarterly observations for the 75 metro areas tracked by the FHFA-HPI. I will show that results are robust when the S&P/Case-Shiller house price index (CS-HPI) is used. Loan-level data is from BlackBox Logic, LLC (BBx).[2] This database covers over 90 percent of non-agency residential securitizied mortgages including prime, alt-a, and subprime loans. BBx has detailed mortgage contract information at the point of loan origination as well as monthly records of mortgage payment information. For this study, only first-lien, single family owner-occupied properties located in a FHFA-HPI city are used. BBx contains information on over 21 million loans and includes over 800 million monthly remittance records as of December 2012. Details on variables utilized from

[2]Detailed BBx data information is available at http://www.bbxlogic.com/data.htm.

this dataset are discussed in the subsequent section. Data on local economic characteristics come from the Quarterly Workforce Indicators (QWI) data from the U.S. Census Bureau. QWI is a unique dataset in that it provides average wages by education level at the metro and micropolitan area levels in the United States.

3.2.2 Housing Leverage Measures

One of the main contributions of this paper is the detailed estimate of the housing wealth within a metropolitan area. I use three groups of housing wealth indicators (HWI), the first of which examines borrower leverage. I calculate an estimate of the current loan to value (CLTV) ratio for individual mortgage remittance report observations and calculate the average CLTV ratio for a given metro area for a given year. The following methodology is used to calculate estimated CLTV ratios, for each loan, l, at each monthly observation, t=m. This variable is constructed with five key data points: the loan to value ratio at origination, which includes the mortgage balance at origination, ($MB_{t=0}$), and value of the home at time of origination ($V_{t=0}$), mortgage balance outstanding at time of observation, ($MB_{t=m}$), the house price index level for a given city, i, at origination ($HPI^{i}_{t=0}$), and the house price index level for that city, i, at the time of the observation ($HPI^{i}_{t=m}$). I only use observations for loans on properties located in the 75 cities tracked by the FHFA-HPI.

Loan to value (LTV) ratios at origination are calculated using information from the BBx dataset. The loan to value ratio at origination expresses a borrower's leverage at the beginning of the life of the loan. For example, if a borrower makes a 20 percent down payment, the LTV ratio at origination will be 0.8. A LTV ratio of 1 indicates the home purchase is 100 percent financed, and a ratio of greater than 1 indicates the loan amount is greater than the housing value at origination.

$$\mathrm{LTV}_{t=0} = \frac{\mathrm{MB}_{t=0}}{\mathrm{V}_{t=0}} \tag{3.1}$$

The denominator of this ratio, property value, is known precisely only at the time of sale because it is the total purchase price of the home. Appraisals are typically conducted as part of the sale process and may be conducted at other points during the tenure of the homeowner; for example, an appraisal may be conducted at the point of mortgage refinance.[3] Although exact housing prices are only known at the point of sale, housing appraisals for loans originated as refinances give an approximation of expected house selling price; the appraiser, who observes many transactions, can help reduce the uncertainty in the estimated value of property (Quan and Quigley, 1991). Since property appraisals themselves are estimates and are conducted infrequently and at irregular intervals, a precise monthly measure of the LTV ratio after the point of origination is not feasible. Since LTV ratios are not updated continuously, the estimate of the CLTV ratio measure provides a reasonable proxy for expressing the current residential housing market environment individual borrowers face.

Monthly payment records for each loan are recorded in the BBx database. The sum of all payments from origination to point of observation gives the reduction of the mortgage balance outstanding from *t=0* to *t=m*. In calculating the CLTV ratio, the numerator will be the origination mortgage balance, $MB_{t=0}$, adjusted for the change in mortgage balance outstanding since origination. Since current value of the home is not precisely known, I use a change in the HPI level for city i from *t=0* to *t=m* to approximate for the changes in the local housing market since origination. The current loan to value estimate (CLTV) is given by:

$$\text{CLTV}_{it} = \left(\frac{(\text{MB}_{t=0} + \Delta\text{MB})}{(\text{V}_{t=0}) \cdot (1 + \Delta\text{HPI}^{i})} \right) \tag{3.2}$$

where ΔMB equals the change in mortgage balance from *t=0* to *t=m*, ΔHPI^{i} is the percentage change in the house price index for city i from *t=0* to *t=m* and $\text{V}_{t=0}$ is the property value at origination. I calculate average CLTV ratios for each metro area for each school year.[4] Since LTV ratios are a measure of borrower leverage, I would expect borrowers with

[3]The sample includes refinanced loans. The denominator of the LTV ratio at origination for these loans is the most recent appraisal value of the property.

[4]School year is measured as August of a given calendar year to July of the following calendar year.

higher current leverage would be more financially constrained and this may negatively impact college enrollment choices.

Next I create a variable to measure changes in borrower financial constraint over time. Using the original LTV ratio from Equation 3.1 and the estimate of the CLTV ratio from Equation 3.2 I calculate the estimated change in equity from origination to the point of observation. Equity is 1 minus the loan to value ratio.[5] The change in equity for a loan located in city i observed at time t is given by:

$$\Delta\text{Equity}_{it} = (1 - \text{CLTV}_{it}) - (1 - \text{LTV}_{it}) \tag{3.3}$$

I construct a variable that measures the prevalence of positive equity changes for a given geographic area. I create an indicator variable that takes the value of 1 if an individual has experienced a strictly positive change in equity since origination, and a value of 0 if the individual has experienced an equity change of less than or equal to zero since origination. I then calculate the percentage of borrowers in a given metro area for a given year that have had positive equity change since origination. All else equal, I anticipate that as the percentage of borrowers in a metro area that have had a positive change of equity since origination increases, so will the likelihood of enrollment at more expensive private universities.

3.2.3 Access to Home Equity

I use three variables related to accessibility of home equity. One common method through which borrowers can utilize existing equity is home equity lines of credit (HELOCs). A necessary, but not sufficient, condition to be able to access home equity through a HELOC is the loan not being "underwater." An underwater mortgage is a loan where the borrower currently owes more on the mortgage balance outstanding than the home is currently estimated to be

[5] A borrower's change in home equity is a function of monthly payment choices by the borrower, given by monthly remittance records, as well as changes in the value of the property, as measured by the FHFA-HPI.

worth. In other words, the borrower has negative equity. Using the value from Equation 3.2, if the CLTV ratio is greater than 1 then the indicator variable *underwater* takes the value 1. Otherwise, *underwater* equals 0. I then calculate the percentage of borrowers in a given metro area for a given school year that have underwater mortgages. All else equal, I would anticipate a higher percentage of underwater borrowers would result in a decrease in private school enrollment.

Next, I determine if a borrower would be likely to be eligible for a HELOC, and if so, approximately how much she would be able to borrow. To determine HELOC eligibility and amount, I first use the estimated current equity, expressed as a percentage of original loan amount:

$$\text{CurrentEquityLevel}_{it} = (1 - \text{CLTV}_{it}) \tag{3.4}$$

Next I use the original property value, $V_{t=0}$, adjusted for the change in value since origination by the HPI, to calculate the current equity amount in dollars.

$$\text{EquityAmount}_{it} = \text{CurrentEquityLevel}_{it} \cdot (V_{t=0} \cdot (1 + \Delta\text{HPI}^{i})) \tag{3.5}$$

If *EquityAmount* is greater than zero and the loan is current in payments at the time of the observation[6] then I anticipate that loan will be eligible for a HELOC. If the loan has a value of zero or less for *EquityAmount* or a payment status indicator of anything other than "current" then I designate that loan as currently ineligible for a HELOC. If a loan is not eligible for a HELOC at the time of loan observation it will take a value of $0 for the two HELOC amount variables used in this analysis.

If a borrower is eligible for a HELOC, the maximum amount they could borrow is a function of the amount of equity they currently have in the property. Rarely are borrowers allowed by banks to borrow 100% of estimated available equity. The percentage of equity individuals are allowed to borrow varies across lenders, time, and other borrower risk factors

[6]Loan payment status from BBx database and is discussed in greater detail in Section 3.2.4.

that I cannot directly observe, but maximum HELOC amount typically ranges between 70% and 85% of current equity amount. Therefore, I create two variables, a low estimate for maximum HELOC and a high estimate for maximum HELOC to encapsulate the range of possible maximum home equity loan amounts possible for borrowers.

$$\text{HELOCamount(low)}_{it} = \text{EquityAmount}_{it} \cdot 0.70 \tag{3.6}$$

$$\text{HELOCamount(high)}_{it} = \text{EquityAmount}_{it} \cdot 0.85 \tag{3.7}$$

I then find an average low and high HELOC eligibility amount for each metro area for each school year. All else equal, I anticipate as the amount of home equity accessible to borrowers increases, enrollment in private universities will increase. For ease of interpretation, I scale this variable by $10,000s.

3.2.4 Foreclosures

I use three variables related to measures of mortgage delinquency and foreclosure. First I examine the effect of delinquency in payments. In the BBx dataset, each remittance report includes a description of the current payment status of the loan. Loans that are identified as being either 30 or 60 days late in payments at the time of the observation are considered to be delinquent. This level of delinquency is commonly termed a “mild default." I find that the percentage of loans within a given metro area for a given year are non-current in payments, but not in foreclosure at the time the loan is observed. Delinquency is a measure of mild financial distress; some non-current borrowers recover and regain their current loan status, but others continue down the path of missed payments and eventually face foreclosure. All else equal, I would expect a rise in delinquencies to have a negative impact on college enrollment decisions.

Within the BBx dataset there are several indicators of severe loan distress. Non-current payment status indicators that are stronger signals of distress than the previously defined mild delinquencies include 90 or more days late, loan in judicial foreclosure proceedings, loan liquidation, real estate-owned (bank-owned property awaiting sale), or borrower bankruptcy filing. Even though these categories of delinquency have different legal implications, they are all indicators of severe loan distress. For the purposes of this analysis, a loan with any of these payment status indicators is broadly classified as a property in foreclosure. Again, I calculate the percentage of loans within a given metro area for a given year that have any of these foreclosure indicators at the point of observation. Although some borrowers may recover to current status either on their own or through loan modification programs, the lack of payment for at least three months on a home mortgage, all else equal, suggests the borrower has a high level of financial constraint or distress. Similar to the delinquency variable, I would expect a rise in foreclosures to negatively impact enrollment decisions, but I would expect the effect to be stronger than for foreclosures than delinquencies.

Finally, I measure the impact of having any negative payment event in the past twelve months. For each school year, I select a single month, June, to observe all mortgages active at that date. Then I examine the past twelve months of payment history for each loan. In addition to the delinquency and foreclosure-related values of the payment status mentioned above, the payment status indicator also could indicate that the loan is current, the loan has been prepaid in full, or there is no record of the loan for a given month.[7] If a loan has any payment status indicators for one or more of the previous 12 months other than current, prepaid, or no record then the indicator variable for loan distress in the past year equals 1. All else equal, I would expect as percentage of loans that have some kind of negative payment indicator in the past year for a given city, enrollment choices for the following school year would be negatively impacted.

[7]Since our database shows remittance records for securitized loans, there will be no observation values for points in time after the loan was originated, but before the loan was securitized, which is typically 3-6 months.

3.2.5 Summary Statistics

Table 3.1 shows the average LTV for mortgages is 68 percent. This means that the loan amount is 68 percent of the value of the home on average across all years 2001-2010. The average percent with a positive equity change is approximately 81 percent over the full sample period. The average amount that a lender would likely be willing to provide as a HELOC is between $43,700 and $53,100 and there are about 9 percent of borrowers with zero or negative equity in their homes. Concerning missed payments and foreclosure activities, 30 percent of borrowers were delinquent at some point over the last 12 months, 8 percent are currently in mild default, and 12 percent are currently facing foreclosure activity. Three variables that are employed in the robustness checks are also listed. The average change in the LTV ratio between 2005 and 2009 was 10.7 percent. The number of the employees of all colleges and universities within a metro area as a ratio of the total workforce in 2005 (pre-recession) is 2.5 percent while the number of incoming freshmen at all colleges and universities as a ratio of the size of the total workforce is 1.4 percent. These variables will be discussed in more detail in section 3.4.

3.2.6 Empirical Model

Following Long (2014), I first test for the net effect of the housing market downturn on college enrollment using a difference-in-differences estimation technique presented in Equation (3.8) where the treated schools are those in metropolitan areas that were severely impacted by the housing crisis and the "control" schools are those that were in areas that were not severely impacted by the housing crisis. Consistent with Long (2014), the treatment begins in 2007 and extends to the 2010, the final year in our sample. I selected the treatment and control groups from the population of the 75 metro areas tracked by the Federal Housing Finance Agency's Housing Price Index (FHFA-HPI). Taking the change in the FHFA-HPI for each

Table 3.1: Summary Statistics

	Sample Average	Std. Dev.	N
College Level Variables			
First-Year First-Time Enrollment			
Public Schools	1,712	1,457	4,920
Private Schools	430	515	10,955
In State Tuition and Fees			
Public Schools	$5,042	$2,486	5,973
Private Schools	$16,768	8,057	14,142
Real Estate Variables			
Average Current LTV	67.9%	20.8%	561
Percent with Positive Equity Change	80.6%	30.1%	561
HELOC Amount (High)	5.31	3.49	975
HELOC Amount (Low)	4.37	2.88	975
Percent of Borrowers with No Equity	8.9%	16.9%	975
Percent with delinquency in past 12 months	30.3%	13.1%	561
Percent with delinquency in past 90 days	8.2%	2.5%	561
Percent with Current Foreclosure Activity	11.8%	8.9%	561
Economic and Demographic Variables			
Wages of Workers with High School Educ.	$2,382	$393	7,321
Percent White	86.2%	12.5%	7,475
Percent Black	9.2%	11.2%	7,475
Percent Hispanic	4.5%	13.0%	7,475
Percent With College Degree	20.3%	5.0%	7,321
Percent With Some College	32.6%	1.8%	7,261
Percent With High School Degree	33.7%	4.6%	7,321
Metro Level Characteristics			
Change FHFA-HPI from 2005 2009	10.7%	18.1%	48
College Employees Total Workforce	2.5%	1.1%	48
Incoming Freshmen Total Workforce	1.4%	0.8%	48

LTV ≡ "Loan to Value", HELOC ≡ Home Equity Line of Credit. HELOC amounts are in tens of thousands of dollars. Wages of Workers with High School Education are average monthly earnings.

city from 4th quarter of 2005 to 4th quarter of 2009, I identify the 10 metro areas that had the most favorable outcomes[8] (least affected by the crisis) as the control group and the 10 metro areas that had the largest price declines as the treatment group[9].

[8]Control group is comprised of Austin, TX, Houston, TX, Raleigh, NC, Buffalo, NY, Tulsa, OK, Oklahoma City, OK, Pittsburgh, PA, Charlotte, NC, San Antonio, TX, and Dallas, TX.

[9]Treatment group is comprised of Detroit, MI, Phoenix, AZ, Fresno, CA, West Palm Beach, FL, Oakland, CA, Ft. Lauderdale, FL, Sacramento, CA, Bakersfield, CA, Riverside, CA, and Las Vegas, NV.

I estimate Equation (3.8) below.

$$\ln(\text{Enrollment}_{i,t}) = \alpha + \beta_1\Big(\text{Bust}_{i,m} \times \text{After}_t\Big) + \beta_2\text{Bust}_{i,m} + \beta_3\text{After}_t + \beta_5\text{H.S. wage}_{mt} + \theta T + \epsilon \tag{3.8}$$

Where $\text{Bust}_{i,m}$ is school i in metro area m where the housing market downturn was severe and zero in areas where the housing crisis was was relatively not severe and After_t is a dummy for 2007 to 2010, after the crisis hit. H.S. wage_{mt} is the average earnings of a worker with high school degree in metropolitan area m. I employ this specification with (a) all 4-year schools pooled together; (b) only 4-year public schools; and (c) only 4-year private schools to see if the housing crisis impacted different types of institutions differently.

Next, I extend our analysis to all schools, not just schools in the areas that were heavily impacted and lightly impacted by the housing crisis.[10] Equation 3.9 describes the main empirical relationship that is being analyzed where $\text{Enrollment}_{i,t}$ is first-year first-time enrollment in college i in year t, $HWI_{m,t-1}$ is a housing wealth indicator in metropolitan area m in year $t-1$. I employ multiple measures of housing wealth including current loan to value ratio, change in equity from origination, percentage of borrowers with underwater mortgages, and potential access to home equity as discussed in Section 3.2.1. H.S. wage_{mt} is the average wage of a worker with a high school diploma (and no additional education) in metro area m and $X'_{m,t}$ includes a group of covariates that include local metro level characteristics such as the percent of workers with college degrees and percent of workers that are white. A yearly time trend, T, and school level fixed effects D_m are also included in all empirical specifications.

$$\ln(\text{Enrollment}_{i,t}) = \alpha + \beta_1 \ln\Big(HWI_{m,t-1}\Big) + \beta_2\text{H.S. wage}_{mt} + \gamma X'_{i,t} + \theta T + \rho D_m + \epsilon \tag{3.9}$$

All housing wealth indicators are compiled for August-July, and are therefore on a "school

[10]This paper focuses exclusively on all four-year private schools and four-year public schools in metro areas where QWI wage data as well as FHFA or Case-Shiller housing price indexes are available. Due to these data constraints not all schools country-wide can be included.

year" instead of a traditional calendar year. Our specification tests for the impact of the HWI in the previous school year $(t-1)$ on enrollment the current school year (t).

Next, I will analyze the impact of housing foreclosures on college enrollment. Equation 3.10 describes this empirical relationship.

$$\ln(\text{Enrollment}_{i,t}) = \alpha + \beta \ln\left(F_{m,t-1}\right) + \gamma X'_{i,t} + \theta D_t + \rho D_m + \epsilon \tag{3.10}$$

Where $F_{m,t-1}$ are foreclosures in $t-1$. Similar to HWI, this allows us to test for the impact of foreclosures in $t-1$ on college enrollment in year t. I will use three measures of foreclosure activity including the percent of mortgages that are currently in mild delinquency, the percent with current foreclosure or severe delinquency activity (90 or more days delinquent) and the percent that have been delinquent for any amount of time at least once within the last 12 months.

Our empirical specification assumes that the housing crisis is exogenous both over time and across geographic areas to college enrollment. In normal times, assuming that changes in the HWI is exogenous both over time and geographic area to college enrollment may be considered implausible. However, I specifically target the first decade of the 21st century, as the housing downturn occurred within this time frame. Housing wealth will be endogenous to college enrollment if (a) areas that have more college enrollment produce citizens with higher incomes who therefore accumulate more housing wealth or (b) unobserved local area economic conditions impact both people's housing wealth as well as their college enrollment decisions.

To illustrate this point, Figure 3.1 shows the average housing prices in using the FHFA-HPI National Composite over this time period as well as the percentage of mortgage balances outstanding 90 or more days delinquent as given by the Federal Reserve Bank of New York quarterly report on household debt and credit. The housing price index peaks in 2006 and then experiences a drastic fall from 2007 to 2010. I also see a large increase in foreclosures

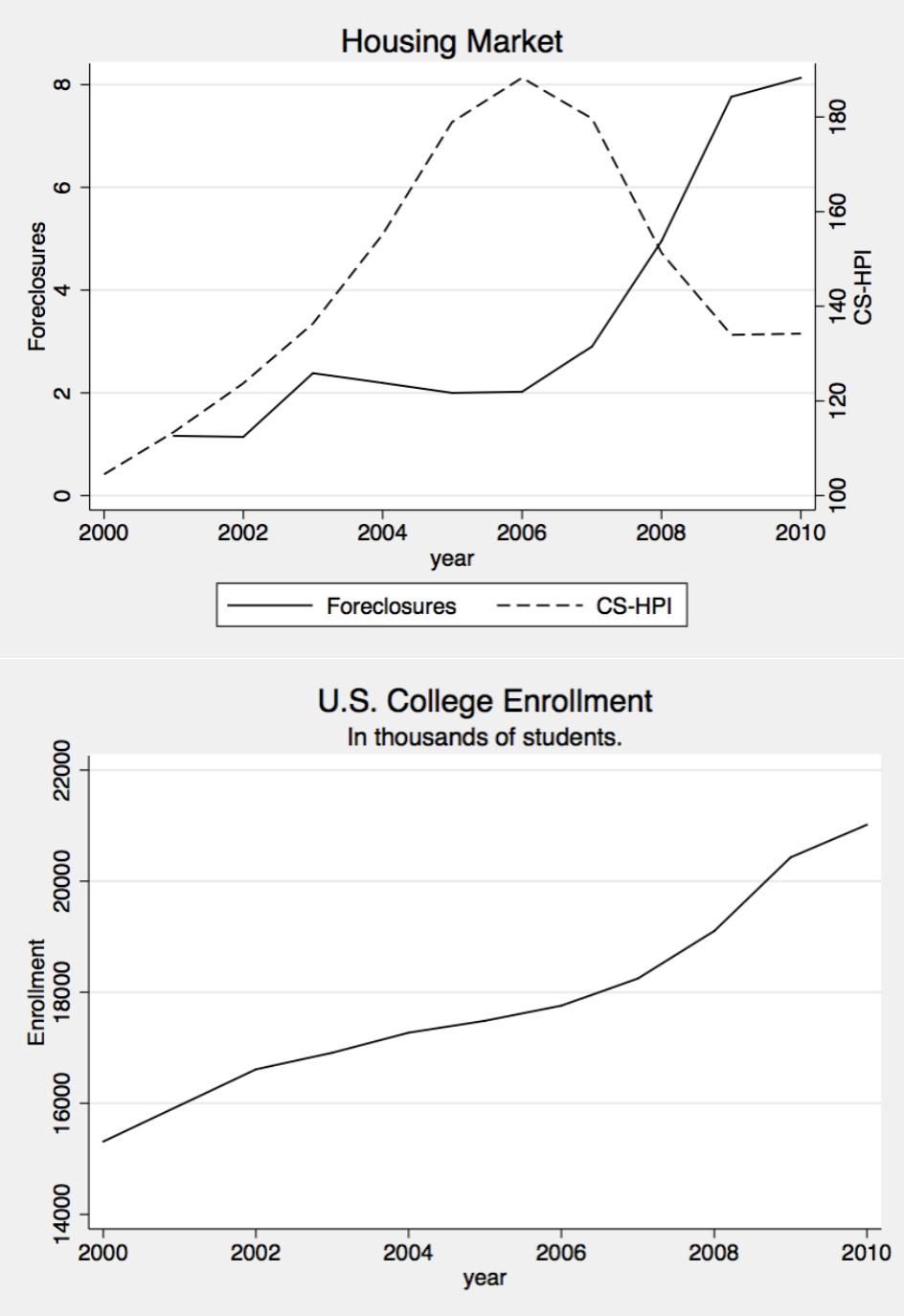


Figure 3.1: College Enrollment, Housing Prices, and Foreclosure National Trends

that lags behind the change in housing prices. This is not surprising seeing that as housing prices fall drastically, more people will be "underwater" on their mortgage and therefore decide (or are forced) to foreclose. What is important, is that these events occurred due to the financial crisis and subsequent global recession and not due to college enrollment decisions in the United States.

Figure 3.2 further illustrates the extent to which the housing crisis was indeed an exogenous shock. The average current loan to value (CLTV) ratio increased rapidly during the recession which lasted from December of 2007 to June of 2009.[11] Simultaneously, there was a large drop in leverage since origination. Both of these graphs show a drastic jump in these indicators that occur during the time period in which the Great Recession occurred.

While the housing crisis was certainly a nation-wide event, it did not impact the entire United States equally. Figure 3.3 shows the change in the FHFA housing price index from 2004–2006 and 2007–2009. Some states, such as Nevada, Florida, Arizona, and California suffered very severe housing price declines, while others such as Texas, Oklahoma, North Dakota, and South Dakota actually saw mild growth throughout the crisis period. In our regression specifications I use school level fixed effects and control for local demographic characteristics and yearly wage levels to help reduce the effects of omitted variables that could be correlated with the severity of the housing crisis in a local area. I argue that the difference in the severity of this housing crisis across the United States was not impacted by college enrollment and is thus exogenous to college enrollment in our empirical specifications. This allows us to unbiasedly estimate the impact of housing wealth and foreclosures on college enrollment using a panel specification, taking advantage of variation over time as well across metropolitan areas within the United States.

The argument could be made that economic conditions during the financial crisis period within our sample made it more difficult for individuals to get credit to finance college. Therefore, any decline in college enrollment might be due to credit constraints, not necessarily

[11]Source: NBER Business Cycle Expansions and Contractions.

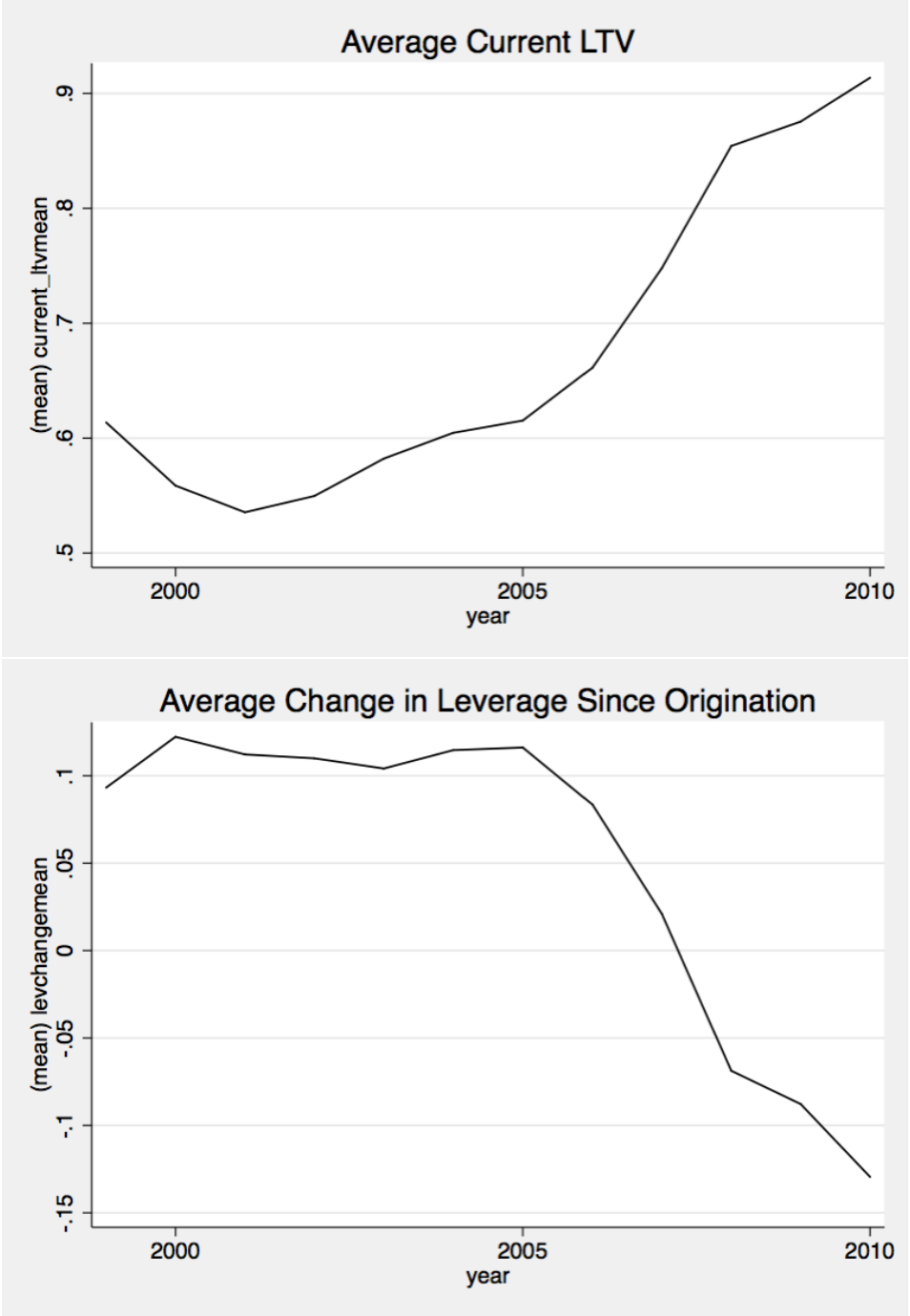
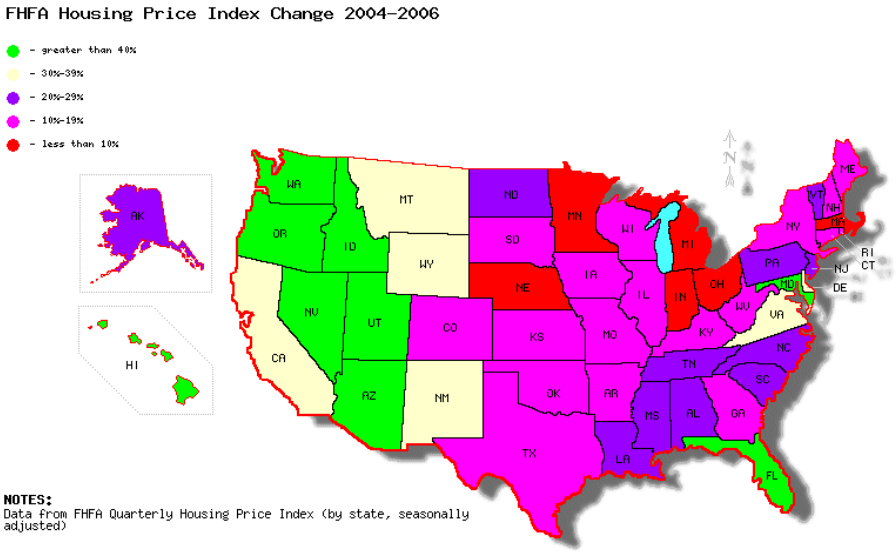


Figure 3.2: Changes in Housing Wealth Indicators During Great Recession



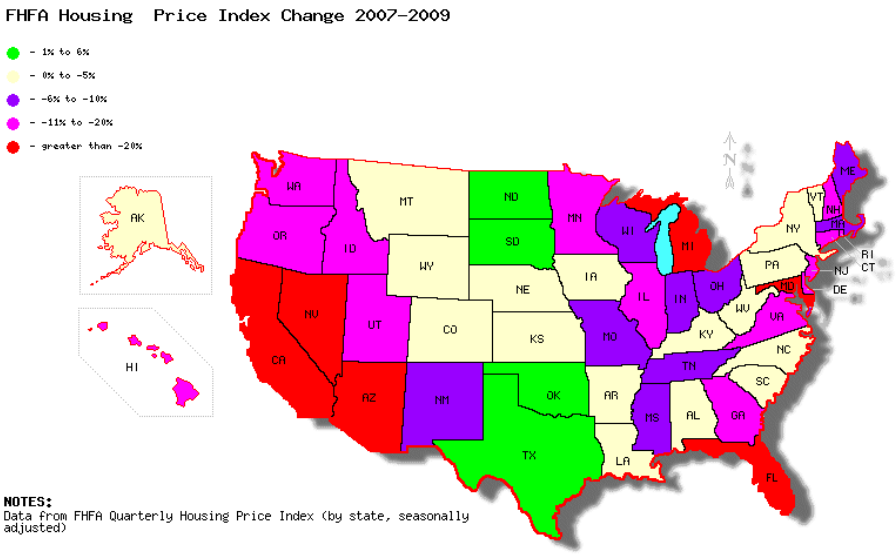


Figure 3.3: State Level Change in Housing Price Index

housing markets themselves. I find this story implausible, as households facing negative economic events, such as loss of a job, would have increased access to credit through the gaining eligibility for larger dollar amounts of federal student aid. I address other potential problems with treating the housing crisis as exogenous to college enrollment in section 3.4.

3.3 Results

3.3.1 Net Effect on Enrollment

First, I estimate the net effect of the 2008 recession as measured by the severity of the housing collapse in a metro area on college enrollment.[12] Table 3.2 presents these results. Overall, I estimate a 6.2 percent decrease in college enrollment in schools that were heavily impacted by the housing crisis. This result differs from Long (2014) who finds that the great recession had a net positive impact on college enrollment. While our point estimate is in the opposite direction, our estimated net effect is not statistically significant. Next, I break this effect into public and private schools separately. I find a statistically significant and positive impact of the housing crisis on public college enrollment as the housing crisis led to a 14.5 percent increase in public enrollment. On the other hand, I find a negative impact of the housing crisis on four-year private school enrollment. While the point estimate for four-year private enrollment is not statistically significant, I estimate that the housing crisis led to a 11.8 percent decrease in enrollment–a change in the opposite direction as public enrollment.

The main difference between the interpretation of our result and the result of Long (2014) is that Long (2014) estimates the net effect of the great recession, and is therefore not controlling for wages or any other variables that might have also been impacted by the recession. In my specification, I control for low-skilled wages, which represent the opportunity cost of attending college, in order to estimate the effect of the housing market downturn.

[12] I use logged values for enrollment in all specifications.

Table 3.2: The Net Effect of the Housing Crisis on College Enrollment

	All	4 Yr Public	4 Yr Private
Treatment Effect	-0.0623	0.145***	-0.118
	(0.0980)	(0.0304)	(0.123)
Recession High	0.0133	0.970**	-0.720**
	(0.259)	(0.380)	(0.320)
Post Recession	0.0583	0.0316	0.0276
	(0.0389)	(0.0499)	(0.0731)
Ln Avg Wage	-0.738	-0.386	-0.716
Unskilled Workers	(0.636)	(0.618)	(0.596)
Year Trend	0.0209	0.0211	0.0426**
	(0.0211)	(0.0225)	(0.0199)
Observations	1119	265	591

All Standard errors clustered at CBSA level. Treated schools are located in areas that were heavily impacted by the housing crisis. Control schools are located in areas were impacted relatively less harshly. The post treatment time period is 2007 to 2010.

This result presented in Table 3.2 is intuitive. The average in-state tuition and fees from Table 3.1 for private schools in our sample is almost $17,000, while average tuition and fees at public universities is less than one third of the price, at about $5,000. When the housing market collapse hit, families likely substituted away from more expensive private schools to more affordable four-year public institutions.

3.3.2 Leverage and Access to Home Equity

The impact of housing leverage and leverage changes over time is presented in Table 3.3. A 10% increase in the CLTV (current loan to value) ratio in associated with a 7.3% increase in college enrollment at public schools, but associated with a 12.2% decrease in private enrollment. This result is consistent with the economic intuition that higher levels of leverage in a household's largest asset–their home–reduces the financial flexibility that may be necessary to choose to send a child to a private university over a public university.

Table 3.3: The Impact of Current Mortgage Leverage on College Enrollment.

	Public		Private	
	(1)	(2)	(3)	(4)
Ln(Average Current LTV ratio)$_{t-1}$	0.0730		-0.122	
	(0.0678)		(0.0783)	
Percent with Postive Equity Change$_{t-1}$		-0.0594		0.0934**
		(0.0437)		(0.0433)
Ln Avg Wage Unskilled Workers$_t$	-0.771*	-0.701	-0.166	-0.298
	(0.397)	(0.427)	(0.397)	(0.393)
Year Trend	0.0243**	0.0211	0.0286**	0.0347***
	(0.0113)	(0.0126)	(0.0107)	(0.0110)
Observations	680	680	1664	1664

All Standard errors clustered at CBSA level. School level FE and metro specific percent of workers with college degree and percent of workers that are white are used in all regressions. Coefficients not shown. All schools that did not report enrollment for all years between 2001 and 2010 and schools with less than 50 first year-first time students are excluded.

To further examine this relationship between leverage and enrollment, I consider the change in housing wealth since origination of the loan. While the CLTV ratio is simply a point estimate of leverage for a given year, equity change since origination measures if the household experienced an increase or decrease in housing wealth over the life of the loan.[13] As a larger percentage of borrowers have seen their wealth increase (as measured by positive equity change from origination) and are better able to afford more expensive education options for their children, I would expect this to lead to an increase in private school enrollment and a decrease in public school enrollment.

As in all specifications, enrollment is in log form; positive equity change from origination is expressed as a percentage for a given metro area. Therefore, for a 10 percentage point increase in borrowers with a positive equity change in a FHFA metro area, I estimate this will lead to a 5.9% decrease in public school enrollment and a 9.3% increase in private enrollment.

[13]I include refinanced properties in our sample. For those loans the direction of the change in wealth is measured since refinance, not the original purchase of the property.

Another facet of housing leverage, HELOC eligibility, is examined in Table 3.4. First I examine the impact of being underwater, and therefore ineligible to borrow against housing wealth, on college enrollment. The percentage of borrowers in a given metro area that have no home equity at the end of a given school year negatively impacts private school enrollment, but positively impacts public school enrollment in the following academic year. For a 10 percentage point increase in borrowers who have no equity, I estimate this leads to a 1.3% increase in public enrollment and a 2.9% decrease in private enrollment. This is consistent with the story presented in Tables 3.2 and 3.3 that higher concentrations of more financially constrained borrowers (with lower housing wealth) leads to a substitution away from private institutions to public institutions.

To further quantify the financial relationship between changes in housing wealth and college enrollment decisions, I estimate the effect of changes in HELOC eligibility amounts. For a borrower with positive equity, all else equal, the borrower will likely be able to borrow between 70 and 85 percent of his current estimated equity in the form of a HELOC loan. There are two mechanisms through which home equity can increase: (1) the borrower makes regular monthly payments;[14] or (2) the property value increases.[15]

For ease of interpretation, I examine the impact of a $10,000 increase in HELOC eligibility on enrollment decisions. A $10,000 increase in accessible housing wealth is estimated to lead to between a 2.4% and 2.9% increase in private enrollment and no effect on public enrollment. This is intuitive as the tuition and fees are more expensive compared to public institutions. Therefore, it is plausible that a $10,000 increase in accessible housing wealth may influence some substitution away from public to private schools as households are better able to afford the more expensive tuition and fees charged by a private school.

[14]Borrower may also make larger than contractually required payments, leading to an even greater increase in equity, all else equal.

[15]For borrowers with some exotic mortgage types such as interest only or negative amortization, housing wealth will only increase as the house value increases.

Table 3.4: The Impact of HELOC Potential on Full Time Enrollment.

	Public			Private		
	(1)	(2)	(3)	(4)	(5)	(6)
Percent of	0.103			-0.287*		
Borrowers With No Equity$_{t-1}$	(0.0893)			(0.164)		
Mean potential		-0.00446			0.0240***	
HELOC amount (high)$_{t-1}$		(0.00748)			(0.00722)	
Mean potential			-0.00542			0.0292***
HELOC amount (low)$_{t-1}$			(0.00908)			(0.00877)
Ln Avg Wage	-0.750*	-0.743	-0.743	-0.245	-0.420	-0.420
Unskilled Workers$_t$	(0.438)	(0.497)	(0.497)	(0.404)	(0.390)	(0.390)
Year Trend	0.0245*	0.0254*	0.0254*	0.0306***	0.0311***	0.0311***
	(0.0128)	(0.0142)	(0.0142)	(0.0112)	(0.00980)	(0.00980)
Observations	680	680	680	1664	1664	1664

All Standard errors clustered at CBSA level. School level FE and metro specific percent of workers with college degree and percent of workers that are white are used in all regressions. Coefficients not shown. All schools that did not report enrollment for all years between 2001 and 2010 and schools with less than 50 first year-first time students are excluded.

3.3.3 Foreclosures

Our third set of results examines the impact of household financial distress, as measured by delinquency or foreclosure in mortgages, on college enrollment decisions. These results are presented in Table 3.5. These results provide different insights than the housing wealth measures in Tables 3.3 and 3.4 because these measures examine the impacts of household financial constraints as measured by borrowers that are currently in delinquency or foreclosure, or that missed at least one payment in the past year. Again, I see dichotomous results between school types. Increased levels of mortgage delinquency or foreclosure in a given metro area negatively impact private school enrollment, but lead to an increase in public enrollment the following school year. As borrowers become unable to afford their homes, they are likely unable to afford other major expenditures, including assisting their children in financing their college educations. Therefore, they will substitute away from expensive private schools to relatively inexpensive public schools.

3.3.4 Two-Year Schools

In all of the above specifications, I have examined the impact of housing market characteristics on only 4-year colleges and universities. The reason for focusing on these institutions is intuitive–the housing wealth channel explored in this paper will only impact college enrollment decisions if the parents of college age students own houses to begin with. If an effect is observed for students whose parents are not plausible homeowners on average, then it is likely not a housing wealth effect that is being observed, but instead an effect of the business cycle that is correlated with housing wealth within an area.

Table 3.5: The Impact of Foreclosure on Full Time Enrollment.

	Public			Private		
Percent with current foreclosure activity$_{t-1}$	0.0525			-0.432**		
	(0.166)			(0.206)		
Percent with delinquency in past 12 months$_{t-1}$		0.0666			-0.301**	
		(0.118)			(0.141)	
Percent with delinquency less than 90 days$_{t-1}$			0.306			-1.514**
			(0.625)			(0.616)
Ln Avg Wage Unskilled Workers$_t$	-0.785	-0.764	-0.784*	-0.362	-0.316	-0.207
	(0.472)	(0.462)	(0.439)	(0.411)	(0.403)	(0.396)
Year Trend	0.0255	0.0235	0.0243	0.0384***	0.0400***	0.0355***
	(0.0154)	(0.0157)	(0.0153)	(0.0130)	(0.0137)	(0.0119)
Observations	680	680	680	1664	1664	1664

All Standard errors clustered at CBSA level. School level FE and metro specific percent of workers with college degree and percent of workers that are white are used in all regressions. Coefficients not shown. All schools that did not report enrollment for all years between 2001 and 2010 and schools with less than 50 first year-first time students are excluded.

As stated earlier, housing wealth is a very plausible source of financing for many college students as 85 percent of college attendees come from families who own a home and, for all but the wealthiest families, their home is their only major financial asset (Lovenheim, 2011). While this seems intuitive for students who are attending 4-year colleges and universities, it is less obvious that students attending two-year schools, such as local community colleges, will be impacted by this housing wealth channel.

Table 3.6 shows the estimated impact of all of the housing wealth and mortgage delinquency variables used in Tables 3.3-3.5 focusing specifically on two-year schools only.[16] As can be seen, of the eight housing market characteristics presented, only one has a statistically significant effect at the 10 percent level. In other words, there is not sufficient evidence to conclude that changes in the housing market impacted two-year college enrollment. These results are consistent with intuition. The housing wealth channel does not appear to have an impact on two-year enrollment, as many of these students' parents are plausibly less likely to be homeowners.

3.3.5 Low-Skilled Wages and College Enrollment

What is consistently observed in Tables 3.2–3.6 is a negative relationship between the wage of individuals within the metro area with a high school degree and college enrollment. This is consistent with previous research (Dellas and Koubi, 2003; Dellas and Sakellaris, 2003; Berger and Kostal, 2002; Black and Sufi, 2002; Card and Lemieux, 2000; Light, 1996; Betts and McFarland, 1995; Kane, 1994; Corman, 1983; Gustman and Steineier, 1981) and is theoretically sound. As the opportunity cost of going to college decreases (such as during a recession), people will be more likely to attend college. The low-skilled wage is estimated to have a larger impact on public school enrollment than private school enrollment, as the average of the point estimates from Tables 3.3–3.5 suggest a 10 percent increase in unskilled

[16] This includes all public and private schools that do not offer any degree higher than a two-year associates degree.

Table 3.6: Housing Wealth and Two-Year College Enrollment.

Ln(Average Current LTV ratio)$_{t-1}$	-0.507							
	(0.486)							
Percent with Postive Equity Change$_{t-1}$		0.234						
		(0.216)						
Percent of Borrowers With No Equity$_{t-1}$			-2.164					
			(1.652)					
Mean potential HELOC amount (high)$_{t-1}$				-0.0321				
				(0.0744)				
Mean potential HELOC amount (low)$_{t-1}$					-0.0390			
					(0.0903)			
Percent with current foreclosure activity$_{t-1}$						-1.628*		
						(0.876)		
Percent with delinquency in past 12 months$_{t-1}$							-0.942	
							(0.573)	
Percent with delinquency less than 90 days$_{t-1}$								-4.026
								(2.542)
Ln Avg Wage Unskilled Workers$_t$	-1.814	-1.946**	-2.196**	-1.121	-1.121	-2.028**	-1.877*	-1.703
	(1.092)	(0.894)	(0.910)	(0.753)	(0.753)	(0.903)	(0.948)	(1.043)
Year Trend	0.0814*	0.0832**	0.0983**	0.0528*	0.0528*	0.0993***	0.101***	0.0896**
	(0.0406)	(0.0317)	(0.0365)	(0.0278)	(0.0278)	(0.0294)	(0.0344)	(0.0368)
Observations	403	403	403	403	403	403	403	403

All Standard errors clustered at CBSA level. School level FE and metro specific percent of workers with college degree and percent of workers that are white are used in all regressions. Coefficients not shown. All schools that did not report enrollment for all years between 2001 and 2010 and schools with less than 50 first year-first time students are excluded.

wages in a metro area leads to a 7.5% decrease in public school enrollment and a 3% decrease in private enrollment.

Interestingly, two-year school enrollment is estimated to be impacted the most by the low-skilled wage. The magnitude of these estimates are orders of magnitudes larger than estimates in results for four-year institutions. I estimate that a 10 percent increase in the unskilled wage leads to an 11 to 22 percent decrease in two-year school enrollment. Magnitudes for four-year schools ranged from about 3 to 7.5 percent. While I do not find evidence that the housing market bubble and subsequent collapse impacted two-year school enrollment, the low skilled wage has a relatively large impact on two-year enrollment.

3.4 Robustness

3.4.1 Case-Shiller Index

All previous housing wealth indicators were obtained using the FHFA 75 city composite index. Another commonly used housing price index is the Case-Shiller 20 City Composite Home Price Index (CS). CS and FHFA are similar in that they both use repeat-sales valuations in creating their indexes. While similar, they are different in that CS is value weighted, and therefore more expensive homes have a larger weight in the index, while FHFA is equally weighted and therefore lower priced homes have a relatively larger weight compared to CS. Another difference is that CS only incorporates houses that actually changed owners, while FHFA uses transactions in addition to appraisals from refinancing activities and therefore is based on more observations. FHFA and CS also consider data from varying sources, as CS uses public records data from county assessor offices while FHFA's source of data is from Freddie Mac and Fannie Mae and therefore only includes conventional loans. One major disadvantage–and the reason why the FHFA index was chosen for all results in our main

specification–is that the Case-Shiller Index is only available for 20 U.S. cities, as opposed to the 75 cities that are covered by the FHFA index.

Table 3.7 shows results using the CS index instead of the FHFA index as used in the main results. The sample size is considerably smaller because there are only 20 cities tracked in the CS index compared to the 75 cities in the FHFA index, but the results are similar. Current LTV ratio is positively related to public school enrollment, and negatively related to private school enrollment. Similarly, the percent of borrowers with no equity is positively related to public enrollment and negatively related to private. While these results are not statistically significant, the point estimates are in the expected direction and magnitudes are very similar.

Also consistent with previous results, eligibility for HELOCs is positively related to private school enrollment and negatively related to public school enrollment. Therefore, the intuitive results are the same: when homeowners experience negative housing wealth shocks, students are likely to substitute away from more expensive private institutions to less expensive public institutions. This result is consistent across multiple measures of housing wealth using multiple housing price indices.

3.4.2 IV Estimation

Lovenheim (2011) utilized Panel Study of Income Dynamics (PSID) to test the impact of housing wealth on college enrollment. Because Lovenheim (2011) utilized micro-data, he correctly assumed that housing wealth is not exogenous to enrollment decisions, as unobservable family characteristics will impact both the enrollment decision of the children as well as the housing decisions. In order to address this issue, Lovenheim (2011) used a household's short run housing wealth change, which occurs due to the overall housing market and is therefore considered exogenous, as an instrument for its home equity level. While I will not discuss

Table 3.7: Robustness Check Using Case-Shiller Index.

	Current LTV		Percent No Equity		HELOC High		HELOC Low	
	Public	Private	Public	Private	Public	Private	Public	Private
Ln(CS Average Current LTV ratio)$_{t-1}$	0.0540	-0.0314						
	(0.0961)	(0.147)						
CS Percent of Borrowers With No Equity$_{t-1}$			0.114	-0.208				
			(0.135)	(0.227)				
CS Mean potential HELOC amount (high)$_{t-1}$					-0.0123*	0.000365		
					(0.00577)	(0.0114)		
CS Mean potential HELOC amount (low)$_{t-1}$							-0.0149*	0.000443
							(0.00701)	(0.0138)
Ln Avg Wage Unskilled Workers$_t$	-0.636	-0.829	-0.570	-1.062	-0.343	-0.802	-0.343	-0.802
	(0.691)	(1.443)	(0.747)	(1.201)	(0.772)	(1.388)	(0.772)	(1.388)
Year Trend	0.0233	0.0539	0.0197	0.0661*	0.0194	0.0519	0.0194	0.0519
	(0.0285)	(0.0433)	(0.0316)	(0.0348)	(0.0276)	(0.0426)	(0.0276)	(0.0426)
Observations	232	958	232	958	232	958	232	958

All Standard errors clustered at CBSA level. School level FE and metro specific percent of workers with college degree and percent of workers that are white are used in all regressions. Coefficients not shown. All schools that did not report enrollment for all years between 2001 and 2010 and schools with less than 50 first year-first time students are excluded.

the rational for such an instrument here, I do argue that the use of such an instrument is not appropriate in the context of this paper for two reasons. First, I am not utilizing household level data for enrollment, but instead observing enrollment at colleges and universities. Therefore, if endogeneity biases our estimates, it is not for the same reasons as Lovenheim (2011), but instead due to endogeniety at the metro level. Second, I argue that the magnitude of the housing crash in a metro area is not determined by educational attainment in that area as is discussed below. In other words, the severity of the housing crash is exogenous to college enrollment at the metro area. For these reasons I do not find it necessary to employ IV estimation, and therefore, our OLS estimates present an unbiased estimate of the relationship between housing market characteristics and college enrollment decisions.

3.4.3 Exogeneity of Housing Wealth on College Enrollment

For all empirical specifications, I assume that the magnitude of the change in housing wealth during the financial crisis is exogenous to college enrollment in a metro area. In other words, I assume that the number of students enrolled at colleges or universities within a metro area is not related to whether or how severely an area was hit by the crisis. If areas with relatively higher college enrollment levels and a larger workforce associated with the local colleges and universities are impacted differently than areas with relatively less college enrollment, then the magnitude of the shock to housing wealth might be impacted by the level of enrollment in an area. In other words, the shock to housing wealth might not be exogenous to college enrollment in a metro area. This can be thought of as the "college town effect," where potentially college towns that have a large number of students and university faculty might not be impacted as severely during a real estate crash due to the presence of the large universities.

While I am not aware of any literature that links whether a metro area is a "college town" to the severity of the housing market collapse, I examine the relationship between the ratio

of the number of college/university employees to total employment in the metro area pre-housing market collapse and the magnitude of the collapse. I also examine the relationship between the ratio of enrollment[17] to total employment in the metro area to the magnitude of the collapse. I would hypothesize that "college towns" that have a large number of faculty and students compared to the total workforce might be impacted less severely by the crisis. While there are 75 cities tracked by the FHFA index, not all of these metro areas have available data during this time period in QWI. Therefore, only 48 cities are included in this analysis. As an additional robustness check, I test whether the percent of workers within the metro area that have a college degree pre-collapse is a predictor of the magnitude of the crash. Figure 3.4 shows the graphical relationship between the saturation of workforce that

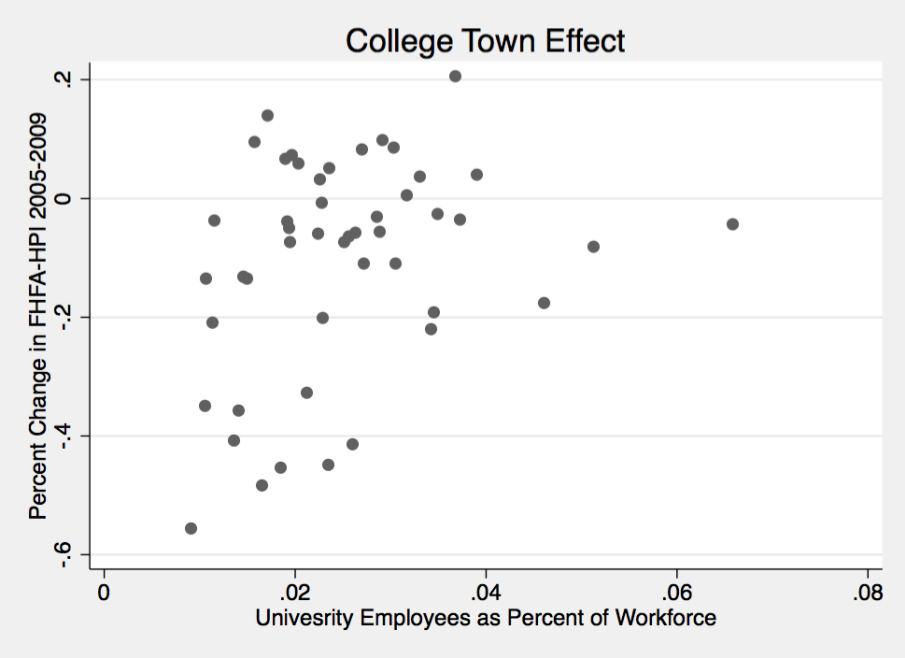


Figure 3.4: The Relationship Between the Percent Change in FHFA-HPI and Percent of Workforce Employed by Colleges and Universities.

is employed by colleges and universities and the change in HPI during the housing crash. There appears to be a positive, but relatively weak relationship, indicating that college towns

[17]I use the number of the first-year first-time freshmen consistent with previous results as a percent of the total workforce pre-collapse and the magnitude of the collapse as measured by the change in the FHFA-HPI.

might have been impacted less heavily than towns in which colleges and universities make up a smaller portion of the workforce in the area.

Table 3.8 presents empirical results. As can be seen, there is a positive, but not statistically significant relationship between the percent of workers in an area that are employed by colleges and universities pre-recession and the magnitude of the crash. This is not surprising given the graphical relationship presented in Figure 3.4. I estimate that a 1 percentage point increase in university employees as a percent of the workforce is associated with a 3.7 percentage point smaller change in FHFA-HPI in that area. To put this into perspective, the average college/university employees as a percent of total workforce is 2.5 percent.[18] So, a 1 percentage point increase from 2 percent to 3 percent, per say, is very large. Furthermore, the average change in HPI in these cities was about 10.7 percent, with a relatively large standard deviation of 18.1 percent. Therefore, a 1 percentage point increase in the college/university workforce as a percent of total workforce in a metro area is associated with about .35 of a standard deviation in the change in HPI.

Table 3.8: Pre-recession Education Levels and the Change in LTV Ratio.

	Change HPI	Change HPI	Change HPI
University	3.704		
Employees as Percent of Workforce	(2.277)		
Enrollment as		3.638	
Percent of Workforce		(3.137)	
Percent College			-0.177
Degree			(0.437)
Observations	48	48	48

Heteroskedasticity robust standard errors shown. Demographic controls including the percent white, black and Hispanic are included in all regressions but coefficients not shown.

As also presented in Table 3.8, there is a positive but statistically insignificant relationship between the size of the first-year first-time college enrollees in a metro area and the change in the FHFA-HPI. Again, this result is not statistically significant, but the economic significance

[18] As shown in Table 3.1

can be debated. The average college enrollment as a percent of workforce in a metro area is approximately 1.4 percent, thus a 1 percentage point change is again very large. This magnitude and interpretation is very similar to the previous result. Table 3.8 also estimates the relationship between the percent of the workforce with a college degree pre-collapse and the magnitude of the change in HPI. This coefficient is not statistically significant and is relatively close to zero.

Because there are only 48 cities for which both the FHFA-HPI is available and QWI labor market and demographic data are available, the lack of statistical significance of these estimates is not surprising. I am aware of no previous research that has estimated whether metro areas that are "college towns" are less susceptible to housing crashes, and thus this would be an interesting topic for future research. The point estimates here are in the direction that is expected: college towns were less impacted by the crash, but this analysis provides essentially no statistical power.

The question that is relevant for this research though, is whether this potential "college town" effect can lead to bias in the results. I argue that this is unlikely for two reasons. First, the potential source of endogeneity here is on total enrollment throughout a metro area. My analysis, though, focuses specifically on schools located within these metro areas. Furthermore, I exploit the variation between public and private schools that are located within the same metro area. Therefore, while the magnitude of the housing crash might be impacted by the total college presence in an area, I am still observing different types institutions (i.e. private and public) within the same metro area, and these institutions are consistently impacted differently. If the college town effect is strong, it cannot explain why I observe different effects on different types of institutions.

Second, according to the college town effect hypothesis the level of total enrollment or total college/university employment can impact the severity of the housing crisis. It does not, however, make a prediction about the relationship between change in enrollment and change in housing wealth within an area. All empirical specifications incorporate school-level

fixed effects, and so any correlation between the total enrollment in an area and the change in housing wealth are accounted for in the empirical specification.

Table 3.9 shows the list of metro areas that are included in this analysis. These cities are all large or midsize cities. Therefore, while some likely have a large college presence, none of these cities are primarily composed of a college or university. For instance, the housing market might be impacted heavily by the presence of a college in a very small town with a large university. For example, the "college town effect" might be very strong in a city such as Manhattan Kansas that has less than 60,000 permanent residence but is home of Kansas State University which has a student body of over 27,000 students. These relatively small towns are not covered by the FHFA index and therefore are not included in my sample.

Future research might find that the magnitude of the housing crisis was impacted by whether a metro area was saturated with colleges and universities, but it is unlikely that this leads to bias in my results.

3.5 Conclusions

I find that the housing crisis impacted private and public schools in very different ways. I find that increases in different measures of housing wealth within a metropolitan area are associated with increases in college enrollment at private schools, whereas declines in housing wealth are associated with increases in public school enrollment. These results are robust over multiple measures of housing wealth including loan to value ratios, eligibility for home equity line of credits, as well as measures of financial distress such as mortgage delinquency and foreclosure.

Table 3.9: Metro Areas and Percent Change in FHFA HPI

City	State	% Δ HPI	City	State	% Δ HPI
Albany-Schenectady-Troy	NY	3.8%	Minneapolis-St. Paul-Bloomington	MN-WI	-19.3%
Albuquerque	NM	3.0%	Nashville-Davidson–Murfreesboro	TN	4.9%
Atlanta-Sandy Springs-Marietta	GA	-13.3%	New Haven-Milford	CT	-11.2%
Austin-Round Rock-San Marco	TX	20.4%	New Orleans-Metairie-Kenner	LA	-5.1%
Bakersfield-Delano	CA	-45.4%	Oklahoma City	OK	8.5%
Baltimore-Towson	MD	-11.1%	Omaha-Council Bluffs	NE-IA	-4.08%
Birmingham-Hoover	AL	5.8%	Orlando-Kissimmee-Sanford	FL	-35.8%
Bridgeport-Stamford-Norwalk	CT	-13.6%	Phoenix-Mesa-Glendale	AZ	-40.9%
Buffalo-Niagara Falls	NY	9.7%	Pittsburgh	PA	8.2%
Charlotte-Gastonia-Rock Hill	NC-SC	7.5%	Portland-Vancouver-Hillsboro	OR-WA	-2.8%
Cincinnati-Middletown	OH-KY-IN	4.5%	Providence-New Bedford-Fall River	RI-MA	-17.7%
Cleveland-Elyria-Mentor	OH	-13.6%	Raleigh-Cary	NC	10.4%
Columbus	OH	-6.1%	Richmond	VA	-3.9%
Dayton	OH	-5.7%	Riverside-San Bernardino-Ontario	CA	-48.5%
Fresno	CA	-41.5%	Rochester	NY	3.6%
Hartford-West/East Hartford	CT	-6.5%	Sacramento-Arden-Arcade-Roseville	CA	-44.5%
Honolulu,	HI	-3.2%	St. Louis	MO-IL	-3.6%
Houston-Sugar Land-Baytown	TX	13.7%	Salt Lake City	UT	6.6%
Indianapolis-Carmel	IN	-3.8%	San Antonio-New Braunfels	TX	7.1%
Jacksonville	FL	-21.0%	San Diego-Carlsbad-San Marcos	CA	-32.7%
Kansas City	MS	-5.9%	San Jose-Sunnyvale-Santa Clara	CA	-20.2%
Las Vegas-Paradise	NV	-55.8%	Tampa-St. Petersburg-Clearwater	FL	-35.1%
Louisville-Jefferson County	KY	0.04%	Tucson	AZ	-22.1%
Memphis	TN-MS	-8.3%	Tulsa	OK	9.4%
Milwaukee-Waukesha-West Allis	WI	-7.5%	Virginia Beach-Norfolk-Newport News	VA-NC	-7.5%

% Δ HPI is the percent change in the FHFA Housing Price Index between 2005 and 2009 (pre and post crash). Large negative numbers indicate that a metro area was impacted heavily by the housing crash.

The results of this research are of particular interest when compared to the literature on the cyclicality of schooling (Dellas and Koubi, 2003; Dellas and Sakellaris, 2003; Berger and Kostal, 2002; Black and Sufi, 2002; Card and Lemieux, 2000; Light, 1996; Betts and McFarland, 1995; Kane, 1994; Corman, 1983; Gustman and Steineier, 1981). These studies have found that college enrollment is counter-cyclical; i.e. that recessions lead to *increases* in college enrollment due to the decreased opportunity cost of attending college. In this paper, I find that the housing crisis, although associated with a time period where net college enrollment increased, created substitution from public to private schools, and therefore actually lead to an increase in enrollment to public schools, likely at the expense of a decrease in enrollment at more expensive private schools. A likely mechanism for this substitution is due to changes in the housing wealth of parents who might provide financial support for their children's educational expenses.

Chapter 4. Summary and Conclusions

4.1 The Effect of Merit-Based Scholarships on Educational Outcomes: An Analysis of the Arizona AIMS Scholarship

The second chapter of this dissertation presents estimates of the impact of the Arizona AIMS scholarship on a variety of educational outcomes at Arizona's three largest in state public institutions: Arizona State University, University of Arizona, and Northern Arizona University. I find that first-year first-time freshmen enrollment increased by about 15 percent. Enrollment effects were largest for Northern Arizona University which saw an estimated 25 percent increase in enrollment due to the scholarship. Enrollment effects were larger for women than for men and were particularly strong for African American and Hispanic students as enrollment increased by 25 percent and 21 percent respectively.

While enrollment increased due to the implementation of the scholarship, tuition and fees also increased. I find an estimated 16-19 percent increase in tuition and fees at Arizona's three universities due to the scholarship. This result is intuitive: because some students' education is heavily subsidized, the aggregate price sensitivity is reduced.

Results are robust to both a difference-in-differences specification and the use of synthetic control groups. Additional robustness checks are presented to account for serial correlation and asymptotically refined standard errors are employed to mitigate concerns about small sample size.

4.2 Housing Wealth and College Enrollment

The third chapter of this dissertation takes advantage of the financial crisis which created an exogenous shock to housing wealth. Cities which had large decreases in housing wealth

after the financial crisis experienced a decrease in enrollment at private institutions compared to similar cities that were not impacted heavily by the crisis. On the other hand, cities that were impacted heavily by the crisis experienced increases in enrollment at public institutions. Thus, students likely substituted away from more expensive private schools as their parents' experienced negative shocks to housing wealth.

I extend this analysis to a number of housing wealth indicators including loan to value ratios and access to home equity lines of credit as well as foreclosures and consistently find that negative shocks to housing markets are associated with increases in public enrollment at the expense of private enrollment even after controlling for average low skilled wages and other metro level characteristics.

4.3 Concluding Remarks

It is clear that there has been an increase in societal preference for higher education, and that this has lead to both increases in enrollment as well as increases in federal and state subsidizes for college. These subsidies, in return, have led to an increase in enrollment and an increase in tuition and fees. This feedback effect likely cannot continue forever.

This dissertation also shows that increases in home ownership–and consequently housing wealth–can also lead to an increase in college enrollment. This was likely also a contributing factor to the increase in demand for higher education observed over the last generation. As a result of these facts, college enrollment and tuition and fees has increased steadily over the past 30 years.

As tuition and fees continue to increase, and more students graduate from college with substantial student loans, it is unclear whether this new generation will continue to insist that its children obtain a college education at any cost. Will the United States experience a change in public perception about the importance of a college education? Furthermore, after the recent real estate bubble and collapse, families are less certain about the value of their

home. Will this make families less likely to leverage housing wealth to fund their children's education?

It is unclear to me whether college enrollment as a percent of college age students will continue to rise in the United States. Over the next 30 years we will observe whether we have entered into a new era of history where college enrollment is considered a cornerstone of a successful life or if the generation in which I grew up will be known as the "college generation."

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Vita

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