

Home Equity Credit and College Access: Evidence from Texas Home Lending Laws

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Abstract

This paper explores how access to credit impacts college choice through a 2003 Texas constitutional amendment that provided exogenous variation in access to home equity loans. The amendment induced large increases in home equity lines of credit, which led Texas homeowners to send their children to more selective colleges and spend \$4,500 more in net-of-aid tuition. Due to supply constraints, homeowners' increased demand for more selective institutions forced some renters to attend less selective colleges, and others to forgo college altogether. On net, the availability of home equity financing reinforced ethnic and income gaps in access to higher education.

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A growing literature suggests that college quality affects labor market earnings (e.g. Andrews, Li and Lovenheim 2012; Hoekstra 2009). This can reinforce inequality in lifetime earnings between individuals who are able to access more selective colleges and those who are not. At the same time, tuition growth at selective colleges has outpaced financial aid, and most families must borrow to finance college.¹ Thus, insufficient access to low-cost credit may constrain some students' ability to access selective colleges.

For policymakers aiming to remove barriers to college degrees that offer high returns, the key question is which barriers are most salient, and how much would it cost to reduce them? There is some evidence that access to credit is an important barrier. For instance, federal student loans—loans that are typically disbursed through colleges—have been shown to have positive impacts on college attendance in general, and at 4-year colleges in particular (Dunlop 2013; Dynarski 2002).² But there is also substantial variation in students' access to private credit through home lending markets, and comparatively little is known about the effects of this type of credit channel.³ In addition, institutions may be aware of variation in private credit supply and make strategic adjustments to financial aid that affect college access for some groups.⁴

This paper explores how constraints on credit supply can impact the level

¹The net price of attendance (tuition and fees less institutional aid) at colleges with selective admissions rose from \$4,763 in 1999-2000 to \$7,628 in 2007-08; net price at open enrollment colleges rose from \$1,132 to \$2,171. Between 2003-04 and 2007-08, the share of undergraduates who borrowed for college rose from 46% to 51%. All college aid figures are the author's calculations using the National Postsecondary Student Aid Study.

²Federal students loans are offered to eligible college students through Title IV of the Higher Education Act of 1965 (and subsequent amendments), which guarantees repayment to the lender if the student defaults.

³In the 2007 Survey of Consumer Finances, a third of homeowners with children ages 17 to 25 had loans from private borrowers secured by their home equity ("home equity loans").

⁴Long (2004) and Turner (2012) show that colleges capture a portion of federal/state aid by price-discriminating based on need-based eligibility for grant aid. This paper explores whether colleges price discriminate based on eligibility for private loans.

and distribution of higher education, including access to selective and 4-year colleges. Specifically, it estimates how enrollment decisions across all potential students respond to an increase in private credit access for one group. I exploit a sharp change in access to private credit markets in Texas; this was triggered by a 2003 constitutional amendment that allowed homeowners to secure home equity lines of credit (HELOCs). HELOCs are revolving lines of credit secured by the value of one's home equity, and are frequently utilized by homeowning families to finance college. Despite widespread use of HELOCs in the rest of the country, they were effectively unconstitutional in Texas until the passage of Proposition 16 in September of 2003.

I exploit this policy change as a source of plausibly exogenous variation in the availability of HELOCs along three key dimensions: temporal, geographic, and by homeownership status. These three dimensions define different groups that vary in their exposure to HELOC eligibility—homeowners in Texas, homeowners in other states, and renters—which allows for several counterfactual exercises. First, I use individual-level data to compare the evolution of college investment levels among homeowners (renters) in Texas to homeowners (renters) in other states using difference-in-difference (DID) methods. Second, I consider the gap in average college sticker price, for example, between homeowners and renters, and compare the evolution of this gap in Texas to other states using triple difference (DDD) methods.⁵

I rely on data from two national surveys (the Current Population Survey and National Postsecondary Aid Study) which provide repeated cross-sectional data on parents' homeownership status linked to their child's college enrollment and choice. While I cannot identify which homeowners take out HELOCs, I can estimate average impacts among all homeowners (and renters) in

⁵Sticker price is defined as tuition and fees paid; net price subtracts institutional aid.

an intent-to-treat approach. The identifying assumption is that there are no confounding shocks among Texas homeowners that are not shared with Texas renters or homeowners in control states, conditional on family income, and state-level shocks to housing prices and mortgage rates. I first draw the counterfactual from a set of control states designed to mimic the evolution of income inequality in Texas between homeowners and renters. To explore the identifying assumption, I also compare results for all states, and for a “synthetic” control group that is a weighted combination of states using the methods of Abadie, Diamond and Hainmueller (2010). An alternative explanation posits that Texas colleges faced confounding budget pressures, prompting them to raise net price and target homeowners with a greater willingness to pay; however, it is reassuring that public college funding in Texas evolves similarly to other states. I also show that impacts are concentrated among homeowners most likely to have HELOCs, and analyze impacts on non-college spending.

The first contribution of this paper is descriptive: I document how homeowning families can lower their cost of capital using HELOCs. Among the 18% of families who had HELOCs in 2007, more than three-quarters had HELOCs with nominal interest rates lower than those they faced on federal student loans.⁶ HELOC-holders can also deduct interest payments on their federal income tax return, and save on the fixed costs (time and fees) associated with arranging multiple lump-sum loans over the duration of a college degree. These observations support the longstanding view that home equity is generally the cheapest source of capital after subsidized federal loans (Dynarski 2005; Kane 1998). For the many families that secured HELOCs at the prime rate during the housing boom, switching from a fixed rate federal PLUS loans to HELOC-

⁶Seventy-eight percent had HELOCs with interest rates lower than 8.5% fixed-rate federal PLUS Loans; and 23 percent faced HELOC rates below 6.8% fixed Stafford rate.

financing for a four-year degree at a highly selective private institution could save upwards of \$10,680 in the net present value of interest payments, and an additional \$1,410 in federal income tax savings.⁷

The second contribution is to present arguably causal estimates of the effects of access to home lending markets on college enrollment, college choice, and net price. I find that despite no change in their overall enrollment rate, HELOC access increases the likelihood that homeowners enroll at 4-year colleges (7.2 percentage points relative to other states), the most selective college tier (2.1 percentage points relative to other states), and Texas flagships (3.1 percentage points relative to Texas renters).⁸ As they ascend the selectivity hierarchy, homeowners spend more on college—roughly \$4,500 per line credit.⁹

In other words, HELOCs shift out homeowner demand for better colleges. In the absence of college capacity constraints, if prices increased in response to greater demand then some renters might be priced out. However, selective colleges can price discriminate based on homeownership. Thus renters may not in fact experience higher prices, with uncertain effects on enrollment. In a more realistic model, capacity constraints limit enrollment in the 4-year college sector; Bound and Turner (2007) argue that college capacity has remained largely fixed since the 1970s, a reversal of earlier expansion during the 1950s and 1960s. Stagnant capacity coupled with a growing pool of Texas high school graduates suggests the existence of supply constraints at Texas colleges.¹⁰

⁷To obtain a rough upper bound on HELOC savings, calculations are based on the average full-time, in-state student in homeownership families at Rice University (the only Texas institution to receive Barron’s “most competitive” designation based on admissions selectivity) from 2007-08 and 2010-11, assuming ten year repayment of additional loans to finance unmet need based on federal EFC calculations. See Appendix A for further details.

⁸The University of Texas at Austin and Texas A&M are the two Texas flagships.

⁹This assumes that Texas homeowners take up HELOCs at the same rate as homeowners with college-aged children nationwide in the 2007 Survey of Consumer Finances (26.4%).

¹⁰The number of rising Texas high school graduates increased by 20.7 percent between 2000-01 and 2004-05 (in NCES’ Common Core of Data), while the total number of full-time

I show that in the presence of these capacity constraints, increased demand for selective colleges among homeowners pushes some renters down the college quality hierarchy.¹¹ I find that some renters even forgo college altogether. Specifically, the overall renter enrollment rate drops by 6 percentage points relative to other states, and conditional on attending any college their likelihood of enrollment drops at the most selective colleges by 0.6 percentage points, and by 4.6 percentage points at non-selective 4-year colleges.

The finding that some renters forgo college altogether rather than attending less selective colleges could arise for several reasons. One explanation is that the return to college may fall with reduced college selectivity more quickly for low-income students than for higher income students (Dale and Krueger 2011, 2002). It is thus possible that for some renters who are displaced from 4-year colleges, their return to a 2-year college degree may not exceed their return to full-time employment. While past research has documented positive returns to community college attendance for certain groups of students (e.g. Kane and Rouse 1995), several recent studies have found negative or insignificant returns to associate degrees or certificate programs in non-technical fields at 2-year colleges (Bahr 2014; Dadgar and Trimble 2014). An alternative explanation is that low-income renters only apply to a limited set of colleges (Hoxby and Avery 2012), and if they aren't admitted because of college supply constraints then employment may be their only option. The finding of renter crowd-out is also consistent with Zimmerman (2014), which argues that supply constraints at public colleges bind, preventing students from making investments that would have high economic returns.

enrollments at Texas institutions increased by 10.6 percent (in IPEDS).

¹¹Bound and Turner (2007) show that rising college demand owing to larger cohort sizes crowd some individuals out of college when public funding doesn't increase proportionately.

Additional results show that the most selective Texas colleges are price-discriminating based on homeownership status: they raise tuition for all students by \$2,000 (relative to colleges in other states), simultaneously re-allocating institutional aid from homeowners to renters to cover this tuition increase.¹²

The results inform our understanding of how inequality in college access is generated and transmitted from parent to child: the availability of home equity credit reinforces gaps between homeowning and renting families through two distinct mechanisms. First, constraints in credit access are relaxed for homeowners, allowing them to ascend the college quality hierarchy. Second, due to college supply constraints, the gains to homeowners crowd out some renters from making otherwise privately optimal investments. Because homeownership status is highly correlated with race and ethnicity, minority gaps in college enrollment and quality are also widening as a result of HELOC access.¹³

The rest of the paper is organized as follows. Section I describes how HELOCs impact college financing costs. Section II describes identification and a simple model to inform empirical predictions about the effects of a credit supply shock for a subset of students. Section III describes the data, empirical methods, and tests the predictions of section II. Results are presented in sections IV (households) and V (institutions) before concluding in section VII.

¹²The Federal Application for Student Aid (FAFSA) does not ask students about their family's home equity, but many selective colleges rely on alternative aid calculators that incorporate home equity such as College Board's College Scholarship Service PROFILE (CSS). In Texas, Baylor University, Rice University, Southern Methodist University, Texas Christian University and Trinity University all use CSS as of the 2014-15 academic year.

¹³This paper also relates to a recent literature finding positive effects of housing price shocks on college enrollment and choice for homeowners (Lovenheim and Reynolds 2013; Lovenheim 2011). These studies exploit local housing booms that generate wealth effects in addition to easing liquidity constraints. In contrast, this paper emphasizes the effects of credit access by exploiting variation in home equity loan eligibility.

I HELOCs and the Cost of Capital

This section describes how the introduction of HELOCs lowered the cost of financing college, which can be used to identify the effects of access to cheaper credit.

A Fixed Cost Savings

HELOCs function like credit cards but with lower interest rates and different default provisions. Brito and Hartley (1995) emphasize that lines of credit can save borrowers on the fixed transaction costs of arranging loans (origination fees and time costs). Once a line of credit is secured, consumers can finance spending over time without repeatedly incurring fixed loan costs (e.g. when securing student loans every year over the duration of a 4-year college education).¹⁴ The introduction of HELOCs thus reduces college financing costs in the face of uncertain consumption or income flows and fixed loan transaction costs. Because uncertainty is greater when looking further into the future, HELOCs should lower financing costs for large investment goods spread out over several years, such as college degrees and vehicle purchases. On the other hand, small predictable purchases (e.g. food) are less sensitive to uncertainty in consumption or income. These predictions are confirmed in Appendix D.

B Tax and Interest Rate Advantages

HELOCs can offer several other advantages relative to federal student loans: (1) interest rate savings; (2) tax deductible interest payments; and (3) bankruptcy provisions that are less restrictive than on federal student loans. Federal stu-

¹⁴Marx and Turner (2015) present evidence that fixed costs have economically meaningful impacts on educational attainment and indebtedness.

dent loans consist of need-based subsidized Stafford loans up to the annual limit (the average annual limit for undergraduates is \$4,750), beyond which students can take out unsubsidized Stafford loans up to the aggregate Stafford limit of \$23,000, followed by higher rate PLUS loans up to the cost of attendance. The interest on subsidized Stafford loans is paid by the federal government while the student is in school. PLUS loans are the only federal student loans that can be taken out in either the student's or a parent's name.¹⁵

Table 1 shows that HELOCs are most common among middle and upper income quintiles. Table 2 confirms that HELOCs allow families to borrow large amounts if necessary, with a median limit of \$50,000. Among homeownership families with college-aged children (17-25 years old), 23.2% had HELOCs in the 2007 Survey of Consumer Finances (SCF), not far below the 27.2% with student loans. Table 2 also summarizes the distribution of HELOC rates across the income distribution in 2007; within every income quintile more than half of HELOC-holders have lower rates than fixed rate PLUS loans at 8.5%.¹⁶

The value of the tax deduction rises with family income. At the median household income level for HELOC-holders with college-aged children in 2007 (\$95,702, or the 78th percentile of household income in the US), deducting interest payments of \$960 (based on the median HELOC balance of \$12,000 times the median interest rate of 8 percent) for an itemizing married couple reduces the effective HELOC interest by two percentage points.¹⁷

¹⁵Since July 1, 1994, independent undergraduate students and dependent students whose parents were denied a PLUS loan were allowed an additional \$23,000 in unsubsidized Stafford loans, facing a combined aggregate Stafford limit of \$46,000. PLUS Loan rejection rates spiked in October 2012 after the Department of Education changed their underwriting standards, with loan denials disproportionately affecting students at historically black colleges (Fishman 2014). This has implications for the importance of private credit going forward.

¹⁶Since these figures include all open HELOCs, they understate the interest rate savings available to many families who were issued HELOCs during the housing boom at or near the prime rate, which has remained at 3.25 percent since December 2008.

¹⁷Federal income tax savings were computed using NBER's TAXSIM tax calculator for

II Identification

A Home Lending Reform in Texas

Texas has a history of legal restrictions on home lending dating back to the Texas Homestead Act of 1839, which exempted the family home from the claims of creditors. Article XVI, Section 50 of the Texas Constitution of 1876 protected homes from foreclosure except for failure to pay the original home purchase loan or debt incurred to finance home improvements. This effectively prohibited home lending by eliminating the collateral value of housing for creditors. Abdallah and Lastrapes (2012) explain that because this restriction was embedded in the state constitution, it has been difficult to relax, in spite of home lending innovations available to homeowners in other states.¹⁸ Homes were protected from forced sale until Proposition 8 was approved by voters in 1997, allowing lump-sum home equity loans (but not lines of credit) without restriction on how the proceeds were to be used.¹⁹ By the fall of 2002, the Texas Credit Union League (TCUL) had begun a campaign to push for further home equity reform, including lines of credit. Other features of the reform efforts included reverse mortgages, designation of a state agency to issue home equity rules and interpretations, and allowing for more flexible loan repayment options. In March of 2003, the state comptroller issued a report in support of home equity lines of credit, and the legislature passed the TCUL proposal with SJR 42 and SB 1067. The corresponding amendment, Proposition 16,

2007, a married family in Texas with one 18 year old child and other itemized deductible expenses totaling \$10,000 (exceeding the standard deduction of \$9,600).

¹⁸Section 50 was only amended prior twice before 1997, extending homestead foreclosure protections to single adults in 1973, and exempting from protection debts related to purchasing an undivided interest in the homestead (related to divorce proceedings) in 1995.

¹⁹Prior to the passage of Proposition 8, homeowners were allowed to refinance only their current loan balance, thus ruling out “cash-out” refinance.

was approved with 65 percent of the vote on September 13, 2003.²⁰

B “First-Stage” Impacts on Home Lending in Texas

The ban on HELOCs was lifted effective September 29, 2003, though subsequent administrative interpretations by the Texas Finance Commission and Credit Union Commission were adopted on December 18, 2003 and February 20, 2004. While there is no public data that reliably reports information on the number of HELOCs in Texas, regulatory data maintained by the Federal Deposit Insurance Commission can be used to track home equity lending at banks based in Texas.²¹ Figure 4 plots the sum of outstanding HELOC loans and unused commitments by quarter for Texas and the rest of the nation, indexed to pre-Proposition 16 levels (the third quarter of 2003). It is also restricted to “small” institutions (with less than \$1 billion in assets) more likely to lend in-state, though results are not very sensitive to this asset threshold. A stable pattern of HELOC growth is evident in other states over the period, while Texas exhibits a pronounced increase beginning around the second quarter of 2004 and peaking towards the end of the housing boom in 2006.

Results presented in Section IV confirm sharp changes in college enrollment patterns beginning in the 2004-05 academic year, with no anticipatory effects in earlier years. Accordingly, the remainder of the paper generally focuses on the period spanning from the 1999-2000 academic year through 2007-08, with treatment beginning in 2004-05. This allows for the analysis to begin several

²⁰Other amendments were reviewed for confounding policy changes. Subsequent amendments in 2005 and 2007 made minor changes to reverse mortgage agreements and instituted additional consumer protections, respectively.

²¹The U.S. Census Bureau asks households about home equity lines of credit in the American Housing Survey, but the data suggests there was confusion by respondents in terms of differentiating between lump-sum home equity loans and lines of credit. This view was articulated in a special report issued by the Texas comptroller’s office in March 2003.

years after lump-sum home equity loans were introduced in Texas in 1997, while largely excluding college-going decisions made after dramatic housing market changes towards the end of 2007.²²

C Proposition 16 and Identification

Before proceeding, it is important to consider whether the forces behind the policy change reflect provides plausibly exogenous variation in HELOC availability. A review of government press releases and news reports suggest that the push for home equity lines of credit—and home equity reform in general—was largely framed as an issue of consumer choice: why shouldn't Texas citizens have access to the same home lending innovations available in the other 49 states?²³ Abdallah and Lastrapes (2012) argue that the timing of home equity reform was heavily influenced by the Tax Reform Act of 1986, a 1994 circuit ruling, and growing Republican influence in Texas—none of which have any obvious relationship to college investment or local credit demand. After practical issues with lump-sum home equity loans and reverse mortgages were resolved around the end of the decade, the ban on home equity lines of credit remained as the last major home lending restriction for Texas citizens. I argue that lifting the restriction on HELOCs was in a sense the inevitable conclusion to home equity reform in Texas, irrespective of local credit demand. While it is impossible to definitively determine whether the last round of major reforms was accelerated by rising local credit demand, publicly the reform

²²It is plausible that Texas homeowners relied more heavily on lump-sum home equity loans in the absence of HELOCs prior to Proposition 16, but only two percent of families with lump-sum home equity loans nationwide cited education among the uses of borrowed funds in 1997 (Canner, Durkin and Luekett 1998). Unfortunately, timing of the available college data (described in the next section) only supports a more rigorous analysis of the effects of Proposition 16 and not Proposition 8 allowing lump-sum home equity loans.

²³See, for example, Combs (2003) and Sopensky (2003).

efforts emphasized equity in consumer choice.

D Testable Predictions

How might a credit supply shock for homeowners impact the distribution of higher education? Consider a model in which 4-year colleges price discriminate by conditioning net price on homeownership; the price elasticity of demand for homeowners exceeds that of renters, which 4-year colleges observe (but not 2-year colleges). In practice, colleges may be maximizing a quality function that takes student characteristics as inputs. For present purposes, I abstract away from the quality tradeoff and assume that colleges admit all students above some ability admissions threshold (as in Epple et al. 2013). Four-year colleges act as monopolistic competitors, segmenting the market into homeowners and renters. Marginal costs are assumed to rise with college selectivity, and are thus highest at colleges in the most selective 4-year college sector and lowest at 2-year colleges. To fix ideas, I make the simplifying assumption that there are a fixed number of enrollment slots at each 4-year college, while 2-year colleges are perfectly enrollment elastic and cannot price discriminate.²⁴

In this basic setup, the marginal cost of a student does not depend on whether the student comes from a family that owns or rents. In this case, the institution would allocate its given number of slots sequentially to the higher marginal revenue sector (own vs rent), as shown in the top panel of Figure VII. The final allocation will equalize the marginal revenue from each sector. Net prices would then be given by the respective demand curves at this allocation (NP_O^* and NP_R^*). The last step is for the institution to verify that the final MR^* equals or exceeds marginal cost at the full allocation of students. If not,

²⁴In practice, Bound and Turner (2007) argue that enrollment elasticities rise as college selectivity falls.

the institution should leave slots unfilled until this condition is satisfied.

The bottom panel of Figure VII depicts the equilibrium after an outward shift in homeowner demand and marginal revenue, as the reduction in borrowing costs raises homeowners' return to college (net of financing costs). In the new equilibrium, final marginal revenue is higher as institutions charge a higher net price to both homeowners ($NP'_O > NP^*_O$) and renters ($NP'_R > NP^*_R$) and admit a greater share of homeowners ($ADMIT'_O > ADMIT^*_O$).

Renters who are displaced from the top of the hierarchy will filter down the college selectivity hierarchy. If increased homeowner demand displaces renters from the 4-year college sector altogether, this will shift out renter demand for open enrollment 2-year colleges as depicted in Figure 2. Any renters who are displaced from the 4-year sector will be accommodated in the enrollment-elastic 2-year sector as 2-year renter enrollment increases from $ADMIT^0_R$ to $ADMIT^1_R$ with no effect on net price. The impact of the credit supply shock on homeowner demand in the cheaper 2-year sector is less obvious; it depends on whether the return to these non-selective colleges (net of financing costs) rises for individuals who would otherwise enter the workforce. Homeowners thus filter upwards, with increases in homeowner enrollment at 4-year colleges and an ambiguous effect on enrollment at 2-year colleges and overall enrollment rates. These predictions are tested in the following section.

III Empirical Strategy and Data

A Data

The empirical strategy requires individual-level data on college-going decisions, including college enrollment, sticker price and net price, institutional type and

selectivity, coupled with enough family background information to determine homeownership status and state of residence.

Enrollment Data (Extensive Margin)

College enrollment data is taken from the March Supplements of the Current Population Survey (CPS), with enrollment defined as part- or full-time enrollment in any college (or completion of at least a 2-year degree program). I observe 18-22 year olds in March of every year, assigning them to cohorts based on their anticipated year of college entry: e.g. 18 year olds observed in March of 2005 are assigned to the 2004-05 college entry cohort (the first treated cohort), while 19 year olds observed in March of 2005 are assigned to the 2003-04 college entry cohort (the last pre-treatment cohort).^{25,26} The enrollment estimation sample begins with the 1999-2000 college entry cohort and ends with the 2007-08 cohort, totaling 73,355 students across all states, including 2,337 Texas homeowners and 1,743 Texas renters.

College Choice Data (Intensive Margin)

Data on college choice is taken from the National Postsecondary Aid Study (NPSAS), a national survey of enrolled college students released by the National Center for Education Statistics (NCES) every four years. NPSAS pro-

²⁵This is a conservative approach, because any impacts on the children of homeowners who were observed after the credit supply shock but turned 18 before the credit supply shock are assigned to the pre-treatment group. In other words, if a 20-year old observed in March of 2005 is enrolled it will boost pre-treatment enrollment rates, even if they were induced to enroll after the credit supply shock. The major disadvantage to this approach is that treatment status is also based on homeownership status, which may have changed between the time of the CPS observation and the timing of college enrollment. The potential for bias introduced by measurement error in homeownership is discussed further in Section IV.

²⁶The top one percent of the national income distribution is dropped in order to focus on students who are constrained in their ability to finance college out of current income.

vides a rich set of information on students' college experience and finances (including an institutional identifier), with sufficient sample size to focus on the state of Texas. The survey collects student information from a variety of institutional sources and the FAFSA. I use the 2003-04 wave as the pre-treatment period (excluding students who began college in the Spring semester of 2004 after HELOCs were available in Texas) and the 2007-08 wave for the post-treatment period (excluding children who began college before 2004).^{27,28} For dependent students, their homeownership status and state of residency is determined by their parents; for independent students their own information is used.²⁹ Because the HELOC option is only relevant for households that cannot finance entirely out of their savings and current income, I only consider students who applied for some sort of financial aid.³⁰ The analysis is also restricted to students aged 25 and younger who permanently reside in the United States or Puerto Rico.³¹ The 2007-08 NPSAS wave covers 113,500 undergraduate students in total, up from 79,900 in the 2003-04 wave, 50,000

²⁷All specifications include a control for class year to address imbalances in class composition between NPSAS waves in the analysis sample.

²⁸While it is unfortunate that NPSAS data is not available between the 2003-04 and 2007-08 waves, it is fortunate that the timing of these waves covers a period before and after Proposition 16 without being subject to the effects of confounding policy changes from the previous decade. Affirmative action in admissions was banned in 1996. Beginning in 1998, the Ten Percent Rule guaranteed admissions to all public universities for Texas high school graduates in the top ten percent of their graduating class. It was followed shortly after its adoption with various targeted scholarship and recruitment programs.

²⁹The response rate to the NPSAS homeownership question does not appear to be influenced by strategic concerns; there are no missing observations in the 2003-04 wave, and only a small number of missing observations in the 2007-08 wave for independent students only (representing 3.3% of respondents).

³⁰The general pattern of the estimates is similar when estimated over all students and not just aid applicants, but precision is reduced. Table 4 confirms that treatment status is not correlated with the financial aid decision.

³¹For federal aid purposes, all students under the age of 24 are considered dependents unless they are enrolled in graduate school, are married or have their own dependents, are an orphan, have active/veteran military status or are emancipated minors. For the final analysis sample of younger aid applicants, 82 percent are listed as dependents.

in 1999-2000 and 41,500 in 1995-96. DID and DDD models are only estimated over the 2007-08 and 2003-04 data in order to obtain sufficient sample sizes to support a rich set of fixed effects. The synthetic control group method uses data from all four NPSAS waves, aggregated to the state level.

Outcome variables derived from the NPSAS data include sticker price and net price. NPSAS also includes an institutional identifier which is used to merge additional data sources: (1) NCES' Integrated Postsecondary Education Data System (IPEDS) provides information on median composite SAT scores for an admitted class; and (2) the Barron's Selectivity Index assigns an ordinal selectivity category to 4-year institutions based on a function of SAT/ACT scores among accepted students, admission rates, and class rank and GPA required for admission.³² For the student-level college spending data, the final estimation sample of students in all states with non-missing tuition and homeownership status includes 85,460 students for the 2003-04 and 2007-08 waves (rounded to the nearest 10, per NCES requirements). This includes 2,190 Texas homeowners in 2007-08 and 2,080 in 2003-04, as well as 1,010 Texas renters in 2007-2008 and 900 Texas renters in 2003-04.

B Descriptive Results

Before describing the empirical strategy in detail, consider the empirical predictions from the stylized model of Section D. The available data supports estimation of the effect of Proposition 16 on overall enrollment rates for homeowners and renters, and measures of college choice conditional on any college enrollment. It does not, however, support estimation of 4-year (or 2-year) college enrollment rates without conditioning on any enrollment. Instead,

³²Additional details on these variables can be found in Appendix B.

sampling weights from NPSAS can be used to obtain estimates of the number of homeowners (and renters) enrolled at 4-year colleges and at 2-year colleges (in Texas and in other states). To proxy for enrollment rates, these figures are divided by the number of 18-25 year olds (in homeownership or renting families) obtained from the March Supplements of the CPS.

The top graph of Figure 1 presents evidence of a positive homeowner demand shock, showing the change in imputed enrollment rates between 2003-04 and 2007-08 at 4-year colleges by homeownership status. The homeowner 4-year college enrollment rate rises by 1.4 percentage points in Texas, while only rising by 0.04 percentage points for non-Texas homeowners. On the other hand, the renter 4-year enrollment rate fell by 1.2 percentage points in Texas, but exhibited no change in other states. These findings are consistent with renter crowd out at 4-year colleges in favor of homeowners.

The middle graph of Figure 1 presents the change in imputed enrollment rates at 2-year colleges, indicating that renters who leave the 4-year sector are not being absorbed by the 2-year sector. Renter enrollment at 2-year colleges actually falls by 1.3 percentage points at 2-year colleges (a drop of similar magnitude is observed in other states), despite previous evidence that the 2-year sector is enrollment-elastic. For homeowners, the 2-year enrollment rate drops by 5.1 percentage points in other states, but only drops by 0.5 percentage points in Texas.

Texas homeowners thus exhibit modest increases in 4-year college enrollment and smaller decreases in 2-year enrollment. The bottom graph shows that the combined effect is a small increase in the overall homeowner college enrollment rate of 0.9 percentage points. Renters exhibit a decrease in 4-year enrollment that is not offset by gains in the 2-year college sector, resulting in a decrease in the overall renter enrollment rate of 2.5 percentage points.

C Empirical Strategy

Under the assumption that Proposition 16 can be interpreted as a natural experiment and the timing of the event is well-defined, the effects of introducing HELOCs can be estimated by comparing college-going outcomes in Texas before and after the amendment to an appropriate control group.

Difference-in-Difference and Triple Difference Methods

First, I consider homeowners in other states as the counterfactual for Texas homeowners. This “Within-Owner” DID compares the before-after change in outcomes for homeownership families with college-aged children in Texas to those in other states. A “Within-Renter” DID specification can be estimated in similar fashion. Next, a DDD model is estimated to compare before-after changes in the owner-renter gap between Texas and other states (in college sticker price or net price, for example). The DDD estimates combine any gains to homeowners and losses to renters into a single “wedge” that can be interpreted as the gap between homeowners and renters induced by the policy change. In other words, the DDD approach also exploits variation in homeownership status between similarly-aged Texans exposed to the new HELOC regime. The full DDD specification is estimated with the following equation,

$$y_{ijst} = \beta_1 \text{own}_{ijst} \text{post}_t \text{texas}_s + \beta_2 \text{own}_{ijst} \text{post}_t + \beta_3 \text{post}_t \text{texas}_s + \beta_4 \text{own}_{ijst} + \beta_5 \text{post}_t \\ + \theta_s + \theta_s \text{own}_{ijst} + \theta_s \text{post}_t + \phi_j + \phi_j \text{own}_{ijst} + \psi_{sj} + \beta_6 X_{ijst} + \beta_7 Z_{st} + \sum_k \delta_k \text{class}_{ijst} + \varepsilon_{ijst} \quad (1)$$

where the subscript i denotes the individual student, j their college-entry cohort, s their permanent state of residence, and t the year of observation (from

1 to T). The dependent variable y is the outcome of interest (e.g. an enrollment dummy, the log of sticker price, or measures of college quality). The right-hand side variables for *own*, *post* and *texas* are dummy variables for coming from homeowning family, being observed in the post period, and permanently residing in Texas, respectively. The parameter β_1 is the parameter of interest, the DDD estimator. In addition to the usual DDD terms, this specification controls for: (1) time-invariant differences shared by homeowners (and renters) in a given state ($\theta_s own_{ijst}$); (2) state-period shocks ($\theta_s post_{ijst}$); (3) national shocks shared by all homeowners (and renters) in a given cohort ($\phi_j own_{ijst}$); and (4) state-cohort shocks (ψ_{sj}). Other controls include a vector of individual controls for class year ($\sum_k \delta_k class_{ijstc}$ where k indexes class year from first through fifth and unclassified) and household income (X_{ijst}); and a vector of state-level credit and house price controls (Z_{st}) including median state home mortgage rates (to control for the price of home equity credit), median state house prices and 3-year state house price growth (to control for housing wealth effects). The enrollment data also supports the inclusion of metropolitan area fixed effects to confirm that the results are not being driven by omitted time-invariant factors unique to metropolitan areas. All observations are weighted according to the individual-level weights provided in the survey data.

For the CPS enrollment data, I use cohort as the time variable (t) (precluding separate fixed effects for cohort and time). This data includes annual observations, permitting estimation of an event study model that breaks up the treatment effect by cohort,

$$y_{ist} = \sum_{r \neq t_b}^T \alpha_r own_{ist} texas_s + \phi_t own_{ist} + \theta_s own_{ist} + \psi_{st} + \beta_6 X_{ist} + \beta_7 Z_{st} + \varepsilon_{ist} \quad (2)$$

where t_b is an omitted base period (the 2003-04 academic year).

Estimation proceeds using a set of control states designed to mimic the evolution of owner-renter income inequality in Texas. The restricted state control group consists of all fifteen states within a fifteen percentile band of Texas (in the population-weighted pre-post change in log owner-renter income inequality).³³ Appendix Figure 1 confirms that this restricted set of control states follows the evolution of owner-renter income inequality in Texas quite closely. Estimates are reported along with robust standard errors clustered by state, except for the Within-Texas estimates which report unclustered robust standard errors.³⁴ A more detailed discussion of alternative statistical inference methods is provided in Appendix E, which shows that state-level clustering is a more conservative approach than clustering by state-year or state-own cells.

Synthetic Control Methods

The DID and DDD methods specify a counterfactual that is effectively a population-weighted average over the fifteen states included in the control group. Ideally the control group isn't simply matched to Texas in terms of owner-renter income inequality, but other relevant determinants of college investment as well. The idea behind the synthetic control methods developed by Abadie, Diamond and Hainmueller (2010) is that a weighted combination of untreated units can provide a better comparison for the treated unit than a simple population-weighted average across untreated units. To use this method, I aggregate the microdata to the state-level and then construct

³³Texas exhibited the 8th highest increase in the owner-renter income gap between pre- and post-periods. The restricted set of control states includes: Alaska, Colorado, Connecticut, District of Columbia, Georgia, Indiana, Iowa, Kentucky, Maryland, Massachusetts, Minnesota, Mississippi, Missouri, New Mexico, and North Dakota.

³⁴Estimation of college choice impacts proceeds with a single pre-treatment period and post-treatment period, yielding consistent standard errors in the face of serially correlated outcomes even when the number of states is small; see Bertrand, Duflo and Mullainathan (2004) for a discussion of inference in the presence of serial correlation.

a “synthetic control” that resembles relevant characteristics of Texas prior to Proposition 16. The synthetic control method is described in further detail in Appendix C.

I form the synthetic control by matching on pre-treatment values of the sticker price gap, homeownership rates, Mexican-American population shares, the change in public college funding per student, and three-year changes in housing prices.³⁵ Estimation proceeds over three periods (1999-2000, 2003-04 and 2007-08). The donor pool consists of the 32 states with at least 20 renters that appear in the analysis sample for each NPSAS wave, in order to minimize measurement error from aggregating to the state level.

IV Enrollment Effects

This section estimates the effect of the introduction of HELOCs on overall college enrollment rates using the yearly event study (equation 2) and before-after mean shift specifications (equation 1).

A Event Study Estimates of Enrollment Effects

The event study estimates from Within-Owner, Within-Renter and DDD specifications (for the restricted state control group and for all states) are reported in Figure 7. For homeowners, no discernible pattern emerges between the cohort-specific estimates before and after the policy change, confirming the descriptive finding that overall homeowner enrollment in Texas is unaffected. For renters, enrollment is lower for the cohorts after the introduction of HELOCs. While the post-period cohort effects are generally not statistically

³⁵The data sources used for these variables are described in Appendix B.

significant for the restricted control states, cohort effects based on all states range from -2.8 to -10.4 percentage points after the policy change, and are statistically significant for all cohorts except 2006. Cohort effects for all states prior to the policy change are not distinguishable from zero.

A similar pattern emerges from the DDD specification of equation 2, which rules out any concerns that the results are being driven by state-level shocks shared by all Texans in the same age cohort, or nationwide shocks shared by all renters in the same age cohort. The bottom graph of Figure 7 confirms that the difference in the gap in college enrollment rates between homeowners and renters is not distinguishable from zero for unexposed cohorts, but rises by between 3.1 and 16.3 percentage points for exposed cohorts. The estimated effects are smallest for the first and last cohorts exposed to the treatment. This pattern would be consistent with homeowners taking some time to respond to HELOC marketing from creditors and incorporate newly available HELOCs into their college financing decisions, followed by a tightening of available home equity credit in 2007 as housing prices began to drop.

B Mean-Shift Estimates of Enrollment Effects

The event study allows for a counterfactual with richer dynamics than a simpler mean-shift comparison between pre- and post-periods (as in equation 1), but at the cost of reduced precision. The mean-shift results of Table 3, on the other hand, allow for greater precision and simpler interpretation. The Within-Owner estimates show an insignificant drop in the homeowner enrollment rate of just over 1 percentage point (relative to other states), while the Within-Renter estimates show a statistically significant drop in the enrollment rate of 5.7 percentage points. These findings are confirmed by the DDD

estimates of columns 3 and 4, which show that the enrollment gap between homeowners and renters in Texas widened by 6.3 to 7.7 percentage points compared to other states. Column 4 also confirms that the results are robust to metropolitan-level fixed effects that capture local labor market specialization or metropolitan variation in home prices, for example. A remaining concern is that the estimates are driven in part by local housing wealth shocks over time, but it is reassuring that the estimates are not responsive to the inclusion of controls for state housing prices and state housing price growth. Drawing the control group from all states also reveals a similar pattern of renter crowd-out but no homeowner enrollment effects (Appendix Table 1).³⁶

The results confirm that the credit supply shock was followed by a crowd-out of renters (this is explored further in section V). While these enrollment changes seem large at first glance, it is important to consider the proportion of renters among enrolled students; depending on assumptions about the distribution of crowd-out effects across age levels, the drop in renter enrollment represents between 1 out of 18 and 1 out of 35 students enrolled in 2003-04.³⁷ In contrast, if Texas homeowners with college-aged children took up HELOCs

³⁶Individual are assigned to a cohort based on year of anticipated college entry, but it is possible that homeownership status has changed between the age of 18 and the time it is observed, in ways that are related to college enrollment. This bias might work in two different ways: the less common transition from owning to renting, or the relatively more common transition from renting to homeownership. This amounts to measurement error in homeownership status that is correlated with financial distress, with mismeasurement of some of the “worst” homeowners as renters and some of the “best” renters as homeowners. To rule out the possibility that the drop in renter enrollment is driven by measurement error, I turn to the NPSAS data of enrolled students which does not suffer from the same lag in homeownership status; similar to the CPS results, the raw number of enrolled renters under the age of 26 who applied for financial aid drops by 9.7% between 2003-04 and 2007-08.

³⁷A 6 percentage point drop in renter enrollment corresponds to 2.4% of all Texas 18-22 year olds in the 2003 CPS, or 5.5% of all enrolled Texas students. Students between the ages of 18 and 22 represent 51.8% of enrolled students in the 2003-04 NPSAS. Under the extreme assumption of no effect on students older than 22, the drop in renter enrollment represents 1 in every 35 students. Under the alternative assumption that all ages face similar crowd-out effects, the drop in renter enrollment represents 1 in every 18 students.

at the same rate as in the rest of the country, it would imply that roughly 1 in 5 students enrolled in 2003-04 secured HELOC financing.³⁸ Moreover, roughly 3 out of every 5 enrolled Texas students from renter families are in the non-selective college sector and thus may have less attachment to college.

C Additional Robustness Checks

Panel A of Table 4 explores the identifying assumption by examining the relationship between treatment status and observable measures of family background for the CPS sample of college-aged individuals. The left-side of Panel A reports estimates of the DDD coefficient from a baseline specification (equation 1), but with the dependent variable replaced with selected family background measures and without individual-level controls. The last column shows DID estimates comparing the change in homeownership rates in Texas to other states. The results indicate that there is no significant relationship between treatment status and family income, mother's education, race and homeownership. While it is impossible to rule out changes in unobserved factors, the fact that treatment status is uncorrelated with family background among all 18-22 year olds helps to mitigate concerns about an unobserved shock to Texas renters. Back of the envelope calculations demonstrate that even under extreme assumptions about the enrollment behavior among marginal homeowners, any bias introduced from compositional effects owing to increasing homeownership rates in

³⁸In the 2007 SCF, 26.4% of homeownership families with college-aged children had HELOCs. Multiplying this figure times the 75.9% of enrolled students in homeownership families in the 2007-08 NPSAS wave implies that roughly 17.8% of enrolled students were in families with HELOCs.

Texas cannot explain changing enrollment patterns.^{39,40}

Taken as a whole, the enrollment results show that some renters no longer enroll in college despite only small absolute increases in homeowner enrollment (that aren't distinguishable from zero when compared to homeowners in other states). This finding is robust to: (1) identification within-renters only, and between homeowners and renters; (2) confounding shocks unique to homeowner-state, homeowner-cohort and state-cohort cells; (3) time-invariant differences across metropolitan areas; and (4) statewide shocks to the price of home mortgage credit and home prices. To confirm that the results are not driven by broad shocks to the returns to any college education, Appendix Table 2 confirms that the identifying variation is not associated with differential impacts on high school enrollment between homeowners and renters in Texas.

V College Choice Impacts

This section explores the effect of HELOC-eligibility on the intensive margin of college choice, conditional on any college enrollment. For renters (homeowners), the estimates should be interpreted as the effect of the credit supply shock on college choice for the renters (homeowners) that remain enrolled.

It is worth noting that the renters who remain enrolled after the policy

³⁹Glaeser and Shapiro (2002) show that the entire home mortgage interest deduction, which targets wealthy families who own homes, has no effect on homeownership rates.

⁴⁰The homeownership rate among Texas families with children between the ages of 18 and 22 rose 1.9 percentage points between 2003 and 2007 (from 52.2 to 54.1 percent). First, even under the assumption that these marginal Texas homeowners were among the “worst” students and enrolled at the lower enrollment rates exhibited by Texas renters (33.1% in 2007-08 compared to 49.9% among Texas homeowners), this would only account for a very small drop in homeowner enrollment equal to $(.499 - .331) \cdot (.019/.541) = 0.006$, or 0.6 percentage points. Second, even under the assumption that marginal homeowners were among the “best” students and enrolled at the higher rates exhibited by Texas homeowners, this would only account for a very small drop in renter enrollment equal to $(.499 - .331) \cdot (.019/(1 - .541)) = 0.007$, or 0.7 percentage points.

change do not look substantively different in terms of student ability and family background. Panel B of Table 4 reports estimates of the relationship between treatment status and family background measures for the NPSAS sample (household income, mother’s education, race, SAT scores, dependency status and homeownership rates). As in Panel A, none of the relationships are statistically significant and no pattern emerges. At first glance, this seems at odds with the notion that renters with less attachment to college are crowded out; however, subsequent results will show that renters who forgo college entirely are predominantly leaving from non-selective 4-year colleges, and are in fact the modal renters in terms of college selectivity.⁴¹

A Difference-in-Difference and Triple Diff. Estimates

Sticker Price and Net Price Impacts

Table 5 reports DID and DDD estimates of the effect of HELOC eligibility on the log of college sticker and net price. Columns 1 and 2 show statistically significant increases in sticker price and net price among homeowners (14.1% and 19.8% respectively). This corresponds to annual increases in sticker price and net price of roughly \$1,200, on pre-treatment means for Texas homeowners of \$6,260 and \$4,440, respectively. Columns 3 and 4 show that sticker price and net price are unaffected among renters, consistent with the notion that the drop in renter enrollment comes from modal renters (in terms of college selectivity). The DDD estimates of columns 5 and 6 echo the Within-Owner DID estimates, with significant increases in sticker price and net price gaps of

⁴¹The distribution of college selectivity at Texas colleges by homeownership status is shown in Appendix Figure 2.

14.7% and 15.2%, respectively.⁴²

The Within-Owner DID estimates are again very similar in magnitude to the DDD estimates. NPSAS does not include a finer geographic identifier than state of residence to control for local housing price growth, but once again the estimated treatment effect is virtually unaffected by the inclusion of state-level housing price controls. Moreover, the fact that the estimated treatment effect persists when renters (and renter neighborhoods) are excluded suggests that the results are not driven by confounding factors correlated with neighborhood or neighborhood type.

Table 6 presents the Within-Owner DID coefficients for the log of sticker price and net price by income quintile (computed over the analysis sample). The results show that college choice impacts are generally limited to families in the top three income quintiles of the analysis sample (with household income above \$69,000); these are the same families who are most likely to have been approved for a HELOC as in Figure 1.

College Selectivity Impacts

To the extent that sticker price is associated with institutional quality, spending increases among the children of homeowners should translate into attendance at institutions with more selective admissions criteria and higher-ability peers.⁴³ Panel A of Table 7 shows that the owner-renter gap in median peer SAT scores increased by more than twelve points, with a statistically signif-

⁴²Appendix Table 3 confirms that the results are nearly identical when measuring sticker price and net price in levels rather than logs. Expanding the control group to include all states yields similar conclusions but with slightly smaller estimates (Appendix Table 4).

⁴³Among public and private non-profit institutions, average sticker price for enrolled undergraduates rises with each Barron's selectivity tier in 2007. Once private for-profit institutions are included, monotonicity in the sticker price-selectivity gradient across selectivity tiers is violated among non-selective colleges.

icant increase of more than 26 points for homeowners.⁴⁴ Panel B of Table 7 shows the effect of HELOC-eligibility on college selectivity, using enrollment at different selectivity tiers as the dependent variable. The first column reports treatment effects from the preferred DDD specification with controls (equation 1), reflecting the change in the gap in the likelihood of enrollment between homeowners and renters; columns two and three show Within-Owner and Within-Renter DID estimates, respectively.

Several patterns emerge from Panel B. First, the enrollment gap between Texas homeowners and renters is widening at the top (most competitive colleges) and at the bottom (non-selective) of the 4-year college hierarchy.⁴⁵ Second, both of these gaps are widening due to increases in the likelihood of homeowner enrollment coupled with decreases in the likelihood of renter enrollment (relative to in other states). Third, Texas homeowners are significantly more likely to leave the 2-year college sector (relative to other states) as they ascend the college quality hierarchy into the 4-year college sector, while Texas renters are not significantly more likely to exit the 2-year sector (relative to in other states). This is consistent with the notion that the reduction in financing costs makes a 4-year college degree worthwhile for some homeowners who would otherwise enroll in less costly 2-year colleges. It is also consistent with college supply constraints for renters at non-selective 4-year colleges; enrollment in the non-selective 4-year college sector expanded by almost fifty percent from 2003-04 to 2007-08 (see Appendix Figure 3), but these colleges enroll greater numbers of homeowners (in absolute terms) while rationing supply for renters (relative to homeowners).

⁴⁴The SAT estimates are based on students in the subset of selective colleges that require and report median SAT scores for admitted students.

⁴⁵Similarly, Appendix Table 5 shows that the likelihood that homeowners are enrolled in one of Texas' two flagship universities rises by more than 3 percent relative to renters.

The interpretation of supply constraints for renters is supported by Figure 5, which shows the share of enrollment slots allocated to homeowners within each college selectivity tier and across NPSAS waves. If renters are being crowded out of most competitive and non-selective 4-year colleges, then the share of students at these colleges coming from homeowning families should increase. Indeed, while the homeowner share is rising over time within every selectivity tier in other states, the increase in Texas homeowner share is considerably larger across all 4-year college selectivity tiers, especially at the most competitive and non-selective 4-year colleges.

Thus the 4-year college prospects for Texas renters fall with the introduction of HELOCs. Between 2003-04 and 2007-08, average composite SAT scores for renter children attending an in-state selective college rises by 2.2 points in Texas, but falls by 2.4 points in other states. While these differences are not statistically significant, the pattern of these changes in student ability as measured by SAT scores suggests that some of the more able renters in Texas are still enrolling at more selective 4-year colleges, while some of the less competitive renter applicants may be filtering down the selectivity hierarchy and in some more extreme cases opting to work rather than attend a 2-year college (or non-selective 4-year college). On the other hand, the average composite SAT score for the children of homeowners attending in-state selective colleges falls by 4.5 points in Texas, but falls by only 0.8 points in other states. This is consistent with selective Texas institutions admitting and enrolling more Texas homeowners after the introduction of HELOCs, in spite of lower SAT scores for these marginal (homeowning) enrollees.

The reduction in renter enrollment at non-selective 4-year colleges is also higher for males (-0.080 , $p\text{-value} = 0.043$) than for females (-0.034 , $p\text{-value} = 0.014$). This is consistent with higher college returns for women than men

(Dougherty 2005). It also supports the notion that the discouragement effect is stronger among males who may face greater employment prospects in the construction sector in the midst of the housing boom.⁴⁶ While it is difficult to definitively determine causality between construction employment and renter discouragement, the fact that the Texas housing boom was stronger during the pre-period casts doubt on the notion of a confounding post-period shock.⁴⁷

Because homeownership status is highly correlated with race and ethnicity, most of the reduction in renter enrollment at the most selective colleges is experienced by minorities. Estimating the preferred DDD specification with minority status used in place of homeownership status implies that the gap in enrollment at the most selective colleges between minorities and non-Hispanic whites widens by 1.4 percentage points (p -value = 0.011).

Additional results show that the policy change did not induce a significant increase in the likelihood that Texas homeowners attended college out-of state. Texas homeowners were, however, more likely to attend college further away from home; estimating the DDD specification with distance between home and college as the dependent variable yields an increase in the homeowner-renter gap of 73.1 miles (p -value = 0.001).

Additional Robustness Checks

The question remains whether the drop in renter enrollment is occurring at the same colleges where homeowner enrollment is rising (consistent with renter crowd-out by homeowners), or if the drop among renters is greater at colleges where homeowner enrollment is also falling (consistent with a contraction at

⁴⁶Similarly, Charles, Hurst and Notowidigdo (2012) show that local housing booms can reduce college enrollment while increasing construction employment.

⁴⁷The three-year change in Texas housing prices peaked in 2001 (author's calculations based on the FHFA's Housing Price Index).

colleges disproportionately attended by renters). Unfortunately, raw headcounts by college are not informative in NPSAS, as there is substantial variation across survey waves in the number of sampled students at a particular college. To explore the root causes of renter displacement, consider that if the drop in renter enrollment was driven by an unobserved shock impacting the college prospects of renters, one might also expect a drop in the relative likelihood of enrolled renters attending college full-time rather than part-time. Additional results show this is not, in fact, the case (see Appendix Table 6).

In the event of an unobserved shock to renters, one might also expect a drop in college applications from renters. While application data is not disaggregated by homeownership status, I use IPEDS data to track total applications at the top 5 Texas colleges in terms of pre-treatment renter share (with a combined student body that was 45.7% renter) and pre-treatment owner share (94.7% owner). The total number of applications from the three-year period just before and just after Proposition 16 grew by 23.6% at the top renter colleges and 19.7% at the top homeowner colleges. Under the assumption that homeowner applications were not rising faster at historically renter colleges than at historically homeowner colleges, then the increase in applications at top renter colleges is inconsistent with a negative demand shock for renters.

One remaining threat to identification concerns the coincidental timing of tuition deregulation at public Texas colleges.⁴⁸ While Appendix Figure 4

⁴⁸Prior to 2003, public undergraduate institutions in Texas charged statutory and designated tuition components that were set by the state legislature, and were generally identical across institutions. Public institutions were, however, able to set mandatory and course fees at the discretion of their own governing board, using these fees to maintain substantial variation in the net cost of tuition plus fees across public institutions. In 2003 the Texas Legislature passed a tuition deregulation bill (HB 3015) that allowed governing boards of public institutions to set their own designated tuition rates, effective in the spring semester of 2004. Because public institutions could still use fees to impact net price prior to tuition deregulation, it is not obvious what causal impact, if any, this change may have had on student costs inclusive of fees.

shows that tuition and fees at 4-year public colleges in Texas do not exhibit any pronounced break from trend over this period, one cannot rule out the possibility that tuition deregulation allowed greater tuition increases at colleges disproportionately attended by homeowners. To confirm that estimated sticker price impacts for homeowners are not just the result of rising tuition at colleges disproportionately attended by homeowners, I estimate the preferred DDD specification using the log of sticker price held constant at pre-treatment levels as the dependent variable.⁴⁹ The estimate is 13.1 percentage points (p – $value = 0.037$), only slightly smaller than the actual sticker price estimate of 14.7 percentage points; homeowners were thus induced to attend colleges that were more costly before Proposition 16 took effect.

In order to rule out any concerns that the results are driven by policy changes at public colleges (e.g. lingering effects of the Texas ten percent rule), I show that the preferred DDD specification yields similar results when estimated over students enrolled at private colleges (18.2 percentage points, p – $value = 0.018$).⁵⁰ Appendix Figure 5 also confirms that the level of state and local funding to public colleges (appropriations and grants) evolved similarly in Texas as in other states.

Lastly, what of the prediction from Section 2 that a line of credit can increase spending on large multi-period expenditures? Appendix Figure 5 confirms that increases in college spending by homeowners are echoed by increased vehicle purchases but not on predictable food-related expenditures.

⁴⁹This uses published tuition for the 2003-04 year based on full-time and residency status.

⁵⁰Long, Saenz and Tienda (2010) show that enrollment at the state flagship University of Texas-Austin among previously underrepresented students from rural high schools rose steadily from 2001 through 2007. If rural enrollees are more likely to come from homeowning families than non-rural enrollees, this could bias the present estimates upwards.

B Synthetic Control Methods

The synthetic control method relies on an alternative counterfactual constructed to match relevant pre-treatment characteristics in Texas. The optimal weights place zero weight on all states except Arizona, California, Colorado, North Carolina and Washington. Figure 6 plots the evolution of the actual owner-renter college sticker price gap in Texas to its synthetic control, yielding a treatment effect of \$1,317 (close to the implied estimate of \$1,200 from the individual-level analysis).⁵¹ Inference is based on the placebo study outlined in Abadie, Diamond and Hainmueller (2010): the synthetic control method is applied to every potential control state, as if each state were subject to a similar intervention. Consider the pre-intervention root mean square prediction error (RMSPE) for Texas (the average of the squared discrepancies between Texas and synthetic Texas prior to Proposition 16). If synthetic Texas were poorly fitted prior to Proposition 16, then the post-2003 gap may be artificially generated by a lack of fit rather than by the effect of Proposition 16. To limit the risk of drawing conclusions that are influenced by poor fit in the pre-period, I consider the ratio of RMSPE in the post-period to the pre-period for each state as a measure of the relative rarity of observing a large post-period gap. Figure 6 also shows the distribution of these ratios for all 32 states in the donor pool. No state achieves a ratio as large as Texas. If the intervention were randomly assigned to another state in the data, the probability of obtaining a pre/post-RMSPE ratio as large as the one obtained for Texas would be $1/33 = 0.03$.

⁵¹The synthetic control method can also be applied in similar fashion to perform the analogous Within-Owner comparisons between homeowners in Texas and in other states, yielding an estimate of \$967. Additional results are available upon request.

VI Strategic Institutional Responses

What of the possibility that some institutions are aware of the increase in private credit supply and are making strategic adjustments to their own tuition prices or financial aid offers? While it is impossible to definitively distinguish between tuition deregulation and home equity reform as the root cause, this section will specifically explore whether colleges are treating homeowners differently. To investigate, variants of the aforementioned DID and DDD specifications are estimated with fixed effects included for each institution. This serves to identify the effects of the policy change conditional on college choice, isolating changes within institutions over time. Two outcomes are considered: sticker price and institutional aid (including merit aid, non-merit aid, tuition waivers and work-study). The sample is restricted to only include students attending college in-state and not attending exclusively part-time. The in-state restriction abstracts away from potential cross-subsidies between in-state and out-of-state students, while still focusing on the majority of college students.⁵² The restriction on attendance intensity abstracts away from differences in the mix of part- and full-time students across institutions.

First consider a DID specification estimating the difference in the pre-post change between (in-state) students attending Texas institutions and those attending institutions in other states:

$$y_{ijstc} = \beta_1 post_t texas_s + \beta_2 post_t + \beta_3 EFC_{ijstc} + \beta_4 SAT_{ijstc} + \phi_j + \theta_s + \varphi_c + \sum_k \delta_k class_{ijstc} + \varepsilon_{ijstc} \quad (3)$$

where the subscript c denotes institution, with institution fixed effects (φ_c) included along with cohort (ϕ_j) and state (θ_s) fixed effects. In addition to

⁵²More than 90% of college students with Texas residency attend in-state (NPSAS).

class year dummies, expected family contribution (*EFC*) is included as a single measure of an individual’s financial need, and composite SAT scores are included to proxy for student ability (for selective colleges only). The coefficient β_1 gives the difference in the average change in in-state sticker price (or institutional aid) between Texas institutions and institutions in other states.

Note that because homeowners and renters are filtering through the college selectivity hierarchy, the relevant characteristics of homeowners and renters may be changing even conditional on college choice, *EFC* and SAT scores.⁵³ Thus equation 4 identifies the effect of the policy change on average price levels within colleges, but it does not speak to the question of whether a given Texas student is treated differently in the post-period.

In order to investigate any changes in the allocation of institutional aid by homeownership status, I extend this DID model into a DDD model (similar to equation 1) that incorporates variation between homeowners and renters:

$$y_{ijstc} = \beta_1 own_{ijstc} post_t texas_s + \beta_2 own_{ijstc} post_t + \beta_3 post_t texas_s + \beta_4 own_{ijstc} + \beta_5 post_t + \varphi_c + \theta_s + \theta_s own_{ijstc} + \theta_s post_t + \phi_j + \phi_j own_{ijstc} + \psi_{sj} + \beta_6 EFC_{ijstc} + \beta_7 SAT_{ijstc} + \sum_k \delta_k class_{ijstc} + \varepsilon_{ijstc} \quad (4)$$

The coefficient β_1 gives the difference between Texas colleges and colleges in other states in the average increase in institutional aid (or sticker price) provided to homeowning families relative to renters.

Table 8 presents estimates of β_1 for equations 4 and 5, broken down by level of institutional selectivity (the top three selectivity tiers are labeled as “more selective,” the bottom four tiers are labeled as “less selective”) and with stan-

⁵³Conditioning on *EFC* sweeps away differences in need-based aid across students due to the average effect of financial need across all colleges. Conditioning on SAT scores controls for average effects on merit-based aid across all colleges.

standard errors clustered at the institution level. The DID estimates of columns 1 and 2 show a statistically significant increase in tuition at the more selective colleges of more than \$2,000, but no tuition effects at less selective colleges. Column 3 shows a smaller statistically significant increase in the overall amount of aid (\$960) among students at more selective Texas colleges relative to colleges in other states, with a small and insignificant decrease at less selective colleges in Column 4 (\$200). Columns 5 and 6 confirm that colleges don't use tuition and fees to price discriminate between homeowners and renters. Instead colleges price discriminate using institutional aid. Despite only modest changes in the overall amount of aid at more selective Texas colleges, there is a large shift in the recipients of institutional aid. Column 7 shows that renters at more selective Texas colleges experience a statistically significant increase in institutional aid of more than \$2,400 relative to homeowners, effectively offsetting the tuition increase. For less selective colleges, on the other hand, there is a much smaller decrease in the amount of institutional aid that renters receive relative to homeowners of \$613; this may be because homeownership status is proxying for program or major choice within college.

VII Conclusion

Researchers have debated the importance of borrowing constraints on college investment (e.g. Carneiro and Heckman 2002). While insufficient access to credit may prevent college enrollment altogether in extreme cases (especially for lower income families), this paper demonstrates how higher borrowing costs may also lead families to enroll in less expensive and less selective colleges (even for more affluent homeownership families). Because more costly and selective colleges are associated with higher lifetime earnings (e.g. Hoekstra 2009),

inequality in access to credit is likely transmitting inequality across generations through differences in lifetime earnings.

The findings of this paper also demonstrate how expanded credit access for a subset of the population can trigger gains in college access for this group at the expense of reduced access for other groups. While the available data does not allow for the imputation of any foregone earnings of those renters displaced from college, previous research suggests that as important as these effects may be, the foregone earnings of those renters that remain enrolled but at lower quality colleges may be just as important (e.g. Saavedra 2008). Andrews, Li and Lovenheim (2012) and Dale and Krueger (2011) argue that these college choice effects are larger for minorities and disadvantaged families who may not otherwise have access to social networks; not only are minorities more likely to be displaced from the most selective colleges, but they may also have the most to lose from displacement in terms of foregone earnings.

From a policy perspective, it would not be very costly to target federal student aid in such a way that provides renters with a similarly low cost of capital as homeowners, and then let colleges decide which students to admit (irrespective of financing costs). It is also possible that the benefits of targeting public funds to expand capacity at 4-year colleges outweigh the costs, as Zimmerman (2014) argues. This paper also demonstrates how the more selective colleges are able to capture some of the gains from cheaper private credit by price-discriminating and re-allocating slots from renters to homeowners. This may have important implications for President Obama's plans to link federal aid to college value: if selective, high-value institutions respond to federal aid in a similar manner as they do to increases in private credit supply, they may raise tuition and re-allocate institutional aid in ways that improve accessibility for some groups at the expense of others.

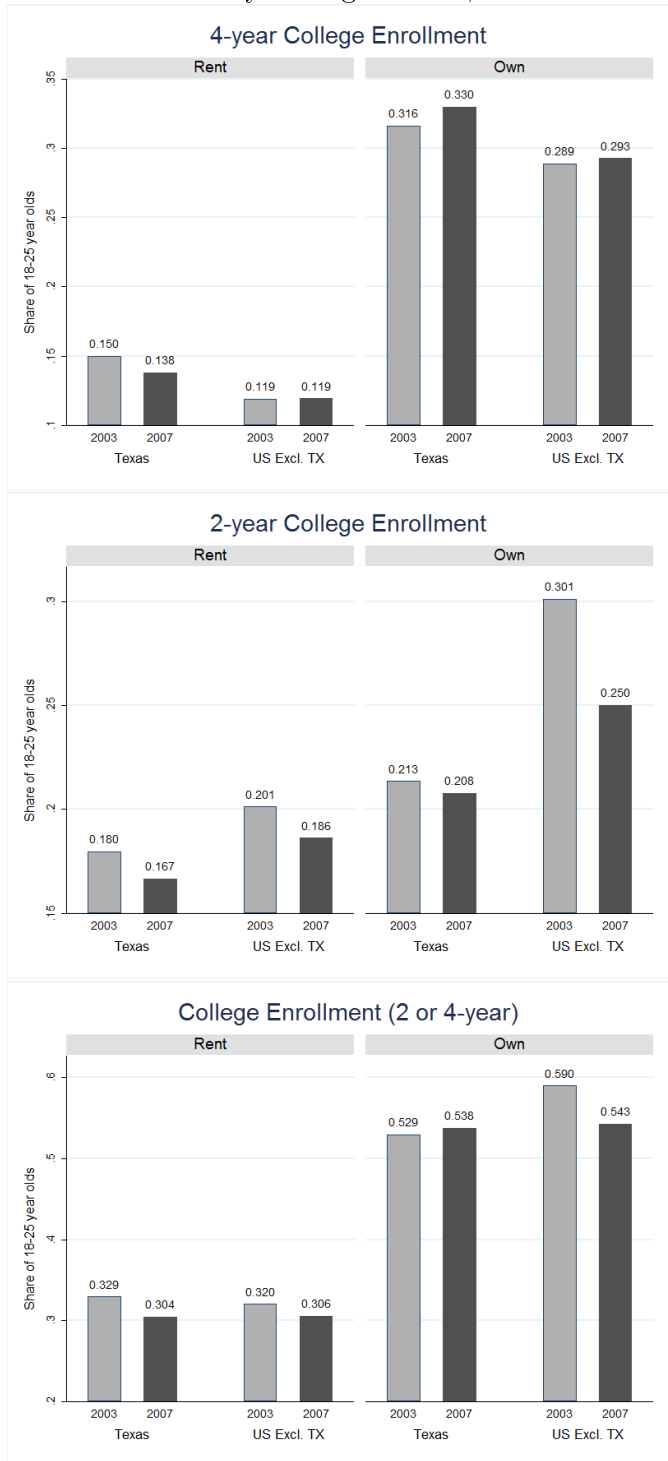
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Figure 1: Enrollment Rates by College Sector, Homeownership and State



Source: Author's calculations based on weighted calculations from NPSAS and the CPS.

Figure 2: Effect of Credit Supply Shock in Most Selective 4-Year College Sector

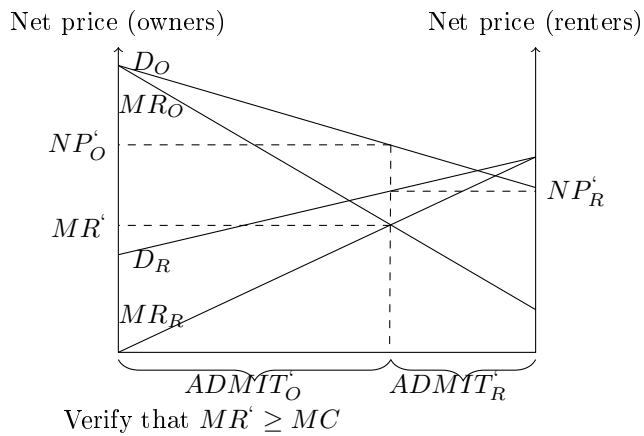
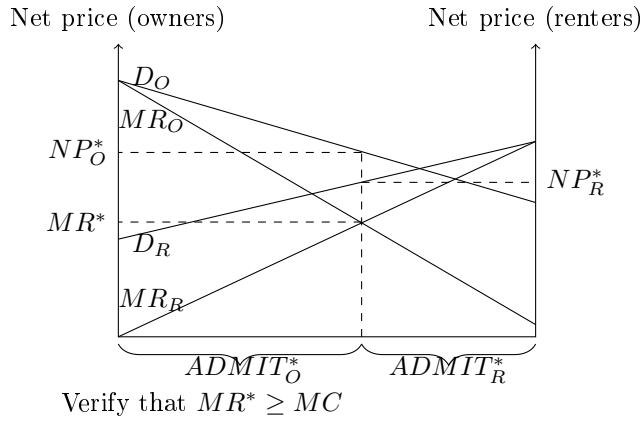


Figure 3: The Effect of a Credit Supply Shock in the 2-Year College Sector

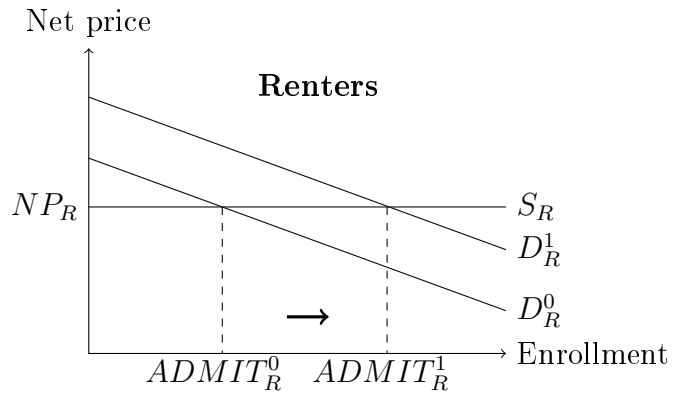
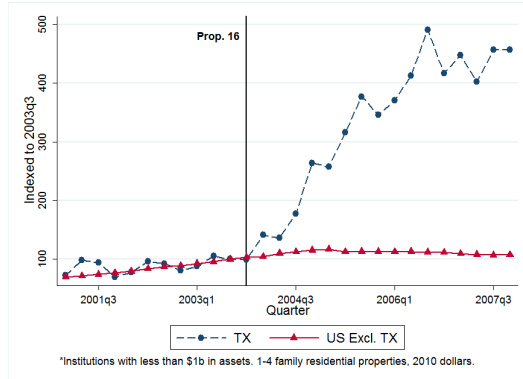
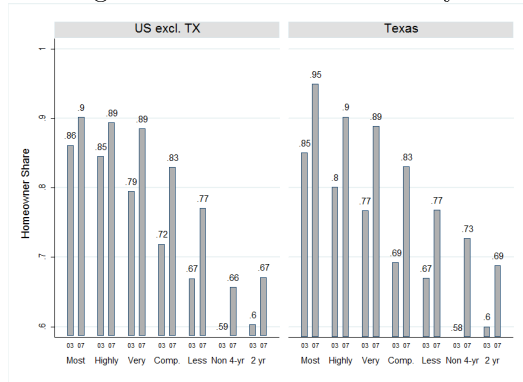


Figure 4: Amount of HELOCs Issued by Small Institutions



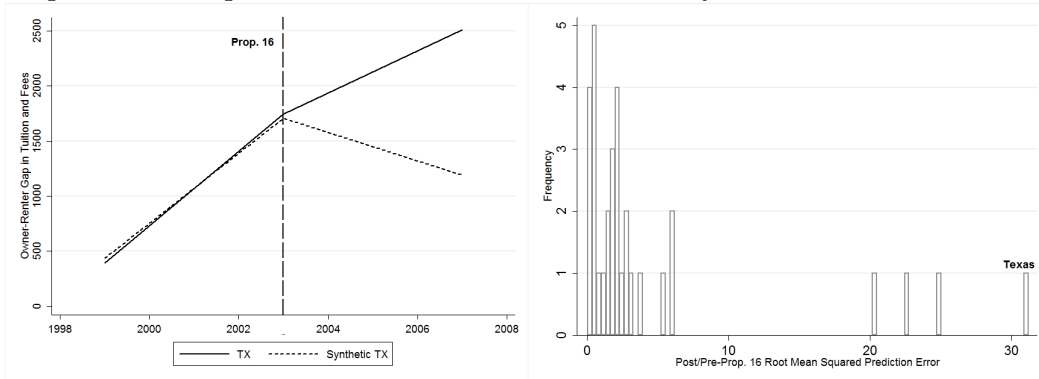
Source: Author's calculations using Call Report data from the FFIEC.

Figure 5: The Change in Homeowner Share by College Selectivity



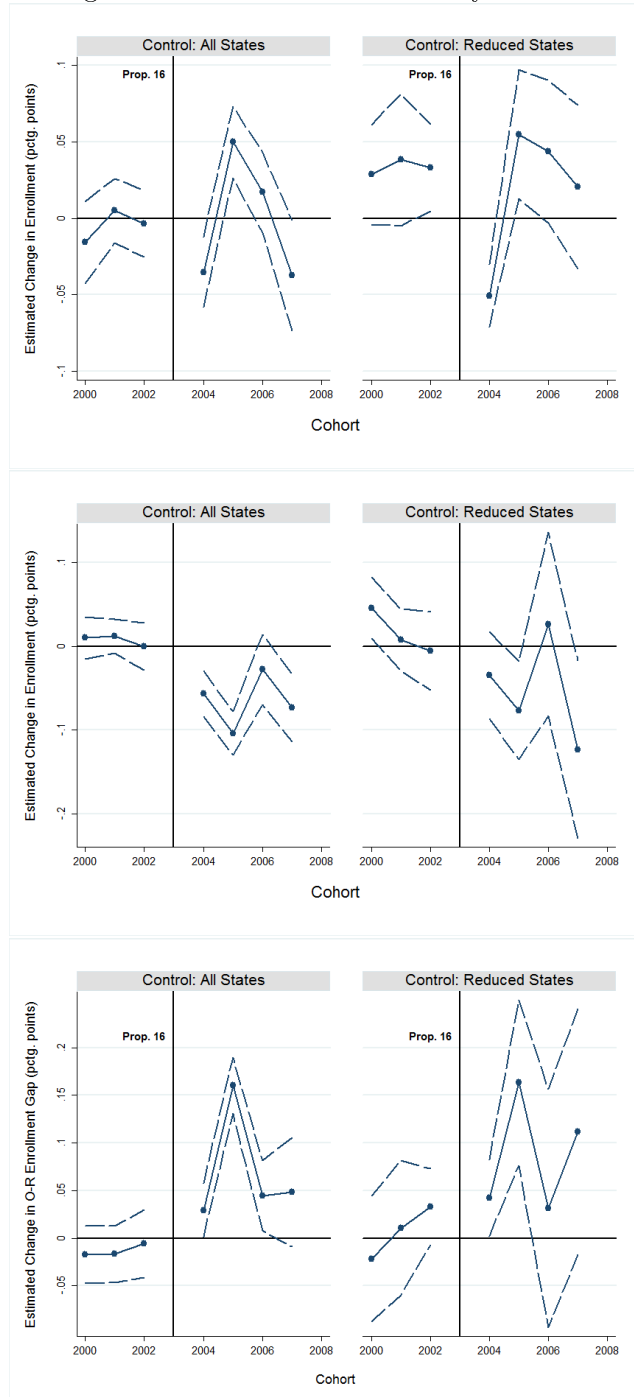
Source: Author's calculations based on NPSAS data.

Figure 6: College Sticker Price Estimates Under Synthetic Control Method



Source: Author's calculations using data from NPSAS, IPEDS, CPS, and the 2000 Census.

Figure 7: Enrollment Effects by Cohort



Source: Author's calculations based on the CPS analysis sample.
 Notes: Omitted base year is 2003, dashed lines represent 90% CIs.

Table 1: Loan Frequency

All Homeowners	Income Quintile (Analysis Sample)					All
	1	2	3	4	5	
Upper Income Bound	20,214	41,114	69,034	104,310	–	–
Proportion with Loans						
HELOCs	6.1%	12.6%	16.2%	20.9%	29.1%	18.4%
Education Loans	5.7%	5.8%	13.7%	19.5%	15.7%	12.7%
<i>N</i>	1,232	2,054	2,506	2,357	8,306	16,455

Source: Author's calculations based on the 2007 Survey of Consumer Finance using supplied sampling weights.

Notes: Income quintiles are determined from the NPSAS analysis sample.

Table 2: HELOC Characteristics

HELOC-Holders	Income Quintile (Analysis Sample)					All
	1	2	3	4	5	
Upper Income Bound	20,214	41,114	69,034	104,310	–	–
Limit (median)	70,000	30,000	40,000	50,000	80,000	50,000
Utilization (median)	17.9%	25.0%	31.3%	25.0%	22.0%	24.0%
HELOC Interest Rate Percentile						
10	5.9	6.0	6.0	6.0	6.0	6.0
50	8.5	8.0	8.0	8.0	7.5	8.0
90	12.0	9.8	10.0	11.8	9.0	10.0
Median Effective Interest Rate	8.0	7.3	6.8	6.5	6.0	6.8
Percentage of Families w/HELOC Rates Below						
6.8% Stafford Loan Rate	14	22	16	23	27	23
8.5% PLUS Loan Rate	60	81	70	74	85	78
<i>N</i> (Interest Rate Sample)	69	202	286	336	1,204	2,097

Source: Author's calculations based on the 2007 Survey of Consumer Finance using supplied sampling weights.

Notes: Income quintiles are computed from the NPSAS analysis sample. Median effective interest rates are calculated as the overall median HELOC interest rate (8 percent) minus the value of the tax deduction on interest payments for the median HELOC balance of \$12,000 at different points in the income distribution. The value of the tax deduction is estimated as the savings in 2007 federal income tax liability when adding \$960 of interest payments ($.08 \times \$12,000$) to another \$10,000 in itemized deductions for a married Texas family with one 18 year old child at the mean household income within each income quintile.

Table 3: The Effect of HELOC-Eligibility on College Enrollment

	Within Owner DID	Within Renter DID	DDD	
	(1)	(2)	(3)	(4)
Post*TX	-0.012 (0.011)	-0.057** (0.027)		
Post*TX*Own			0.077*** (0.023)	0.063** (0.024)
Fixed Effects:				
Cohort	X	X		
State	X	X		
State*Cohort			X	X
State*Own			X	X
Cohort*Own			X	X
MSA				X
N	12,694	8,202	20,896	19,350
R-squared	0.064	0.022	0.082	0.105

Source: Author's calculations using the CPS analysis sample.

Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is a dummy variable for college enrollment or any degree completion. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered by state, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 4: Treatment Status and Family Background

Dependent var.	Income	Mom BA	White	SAT	Dependent	Apply for Aid	Own Home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: CPS Sample</i>							
coef.	0.033	-0.022	0.007				0.021
se	(0.036)	(0.024)	(0.028)				(0.013)
N	24,934	13,944	24,934				24,934
<i>Panel B: NPSAS Sample</i>							
coef.	0.027	-0.022	0.028	5.30	0.006	-0.022	0.003
se	(0.069)	(0.021)	(0.030)	(12.96)	(0.016)	(0.023)	(0.006)
N	28,750	27,510	28,750	20,910	28,750	33,100	28,750

Source: Panel A uses the CPS analysis sample with survey year minus one as the time variable. Panel B uses the NPSAS analysis sample (column 6 includes non-aid applicants). Notes: ***Indicates significance at the 1% level, **5%, and *10%. Columns 1–6 report estimates for the *post*own*texas* coefficient from a DDD specification without individual controls and with fixed effects for state-time, state-own and time-own cells (Panel B includes fixed effects for state-time, state-cohort, own-cohort and state-own cells). Column 7 reports estimates for the *post*texas* coefficient in a DID specification with state and time fixed effects (plus class year dummies for Panel B). Robust standard errors are in parentheses, clustered by state. Observations are weighted by the CPS person-level supplement weight or the NPSAS study weight (normalized to sum to one in each wave).

Table 5: The Effect of HELOC-Eligibility on College Sticker/Net Price

	DID: Within Owner		DID: Within Renter		DDD	
	Sticker	Net	Sticker	Net	Sticker	Net
	(1)	(2)	(2)	(3)	(4)	(5)
Post*TX	0.141** (0.061)	0.198*** (0.043)	-0.030 (0.139)	0.037 (0.124)		
Post*TX*Own					0.147*** (0.048)	0.152*** (0.053)
Fixed Effects:						
Cohort	X	X	X	X		
State	X	X	X	X		
State*Time					X	X
State*Cohort					X	X
State*Own					X	X
Cohort*Own					X	X
<i>N</i>	21,550	21,550	7,210	7,210	28,750	28,750
R-squared	0.307	0.151	0.206	0.119	0.314	0.168

Source: Author's calculations using the NPSAS analysis sample.

Notes: ***Indicates significance at the 1% level, **5%, and *10%. The dependent variable is the log of sticker price or net price. All specifications include controls for the log of household income, state mortgage rates, the log of state housing prices and the 3-year change in state housing prices. Robust standard errors, clustered by state, are in parentheses. Observations are weighted by the CPS person-level supplement weight.

Table 6: The Effect of HELOC-Eligibility by Income Quintile

Quintile	Upper Bound	<i>N</i>	Tuition	Net Price
	(1)	(2)	(3)	(4)
1	\$20,214	2,430	-0.029 (0.088)	-0.064 (0.047)
2	\$41,114	4,040	-0.034 (0.054)	-0.209** (0.086)
3	\$69,034	4,900	0.308** (0.107)	0.438*** (0.090)
4	\$104,310	4,890	0.126* (0.059)	0.157*** (0.053)
5	—	5,300	0.145*** (0.045)	0.317*** (0.056)

Source: Author's calculations using the NPSAS analysis sample.

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Coefficients in columns 3 and 4 are from the Within-Owner DID specification with the full set of controls and fixed effects for state and birth cohort, and the log of tuition and net price as the dependent variable, respectively. Income quintiles are computed over the analysis sample in the restricted set of control states. Robust standard errors are clustered by state. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.

Table 7: The Effect of HELOC-Eligibility on College Quality

	DDD	DID: Owners	DID: Renters
<i>Panel A: Peer Ability</i>			
Median SAT (Admitted Class)	12.37 (10.21)	26.31** (9.15)	-4.52 (21.06)
<i>N</i>	12,720	10,600	2,090
<i>Panel B: College Selectivity</i>			
Most Competitive	0.016** (0.007)	0.021*** (0.003)	-0.006** (0.003)
Highly Competitive	0.008 (0.015)	0.019 (0.012)	0.013 (0.009)
Very Competitive	-0.018 (0.014)	-0.006 (0.003)	0.019 (0.021)
Competitive	0.005 (0.023)	0.024 (0.036)	0.038 (0.046)
Less Competitive	0.015* (0.007)	0.001 (0.017)	0.010 (0.025)
Non-Competitive 4-year	0.033** (0.013)	0.011 (0.007)	-0.046*** (0.014)
2-year	-0.059** (0.029)	-0.069*** (0.020)	-0.028 (0.058)
<i>N</i>	28,400	21,280	7,120

Source: Author's calculations using the NPSAS analysis sample.

Notes: ***Indicates significance at the 1% level, **5%, and *10%. Estimates are from the preferred specifications with full set of controls and fixed effects. Median SAT scores are estimated as the midpoint between the 25th and 75th percentiles. Robust standard errors, clustered at the state-own level, are in parentheses. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.

Table 8: Institutional Responses

Dependent Var. Selectivity	DID				DDD			
	Tuition		Institutional Aid		Tuition		Institutional Aid	
	More (1)	Less (2)	More (3)	Less (4)	More (5)	Less (6)	More (7)	Less (8)
Coefficient	2,046.7*** (615.2)	123.5 (251.1)	959.5* (576.2)	-200.0 (149.8)	-774.9 (754.7)	61.1 (226.7)	-2,421.1** (1,124.4)	613.0*** (175.0)
Fixed Effects:								
Institution	X	X	X	X	X	X	X	X
Cohort	X	X	X	X				
State	X	X	X	X				
State*Time					X	X	X	X
State*Cohort					X	X	X	X
State*Own					X	X	X	X
Cohort*Own					X	X	X	X
N	4,990	16,450	4,990	16,450	4,990	16,450	4,990	16,450
R-squared	0.952	0.904	0.540	0.488	0.961	0.906	0.577	0.502
2003-04 TX Mean	13,330	4,351	4,563	801				

Source: Author's calculations using the NPSAS analysis sample.

Notes: ***Indicates significance at the 1% level, **5%, and *10%. More selective institutions include the top three tiers of Barron's selectivity, less selective includes all other institutions. The sample includes students of all ages attending college in-state who applied for financial aid and are not missing imputed tuition, and excludes exclusively part-time students. Controls include EFC (for all colleges) and composite SAT score (for selective colleges only). Robust standard errors clustered at the institution level, are in parentheses. Observations are weighted by the NPSAS study weight, normalized to sum to one in each study wave.