ON THE EFFECT OF STUDENT LOANS ON ACCESS TO HOMEOWNERSHIP*

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Abstract

We estimate the effect of student loan debt on subsequent homeownership in a uniquely constructed administrative data set for a nationally representative cohort. We instrument for the amount of individual student debt using changes to the in-state tuition rate at public 4-year colleges in the student's home state. A 10 percent increase in student loan debt causes a 1 to 2 percentage point drop in the homeownership rate for student loan borrowers during the first five years after exiting school. Validity tests suggest that the results are not confounded by local economic conditions or non-random selection into the estimation sample. (*JEL* D14, I22, R21)

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1 Introduction

Over the past ten years, the real amount of student debt owed by American households more than doubled, from about $450 billion to more than $1.1 trillion, with average real debt per borrower increasing from about $19,000 to $27,000.[1] During the same period, the U.S. homeownership rate declined markedly amidst the housing market bust and the financial crisis: from 69 percent in 2005 to 64 percent in 2014.[2] The declines in homeownership have been the largest (both in relative and absolute terms) among young households—a population segment that owes the preponderance of the outstanding student loan debt. For example, the homeownership rate for households between ages 24 and 32 declined by 9 percentage points (from 45 to 36 percent) between 2005 and 2014, nearly twice as large as the 5 percentage point drop in homeownership for the overall population. Against this backdrop, market commentary has suggested that increases in student loan debt might be a key factor pushing homeownership rates down in recent years through effects on borrowers' ability to qualify for a mortgage and their desire to take on more debt.[3] Corroborating this claim, recent surveys have found that many young individuals view student loan debt as a major impediment to home buying.[4]

Estimation of the effect of student loan debt on homeownership is complicated by the presence of other factors that influence both student loan borrowing and homeownership decisions. Researchers have previously attempted to isolate the effect by controlling for a set of observable student characteristics (Cooper and Wang (2014) and Houle and Berger

[1] Figures are based on authors' calculation from the NYFed CCP/Equifax data set. Nominal amounts are deflated by CPI-U into constant 2015:Q2 dollars.

[2] Source: Current Population Survey.

[3] For some examples, see "CFPB Director: Student Loans Are Killing the Drive to Buy Homes," *Housing Wire*, May 19, 2014; "Denied? The Impact of Student Loan Debt on the Ability to Buy a House" by J. Mishory and R. O'Sullivan at www.younginvincibles.org.

[4] See, for example, Stone et al. (2012) or "What Younger Renters Want and the Financial Constraints They See," Fannie Mae, May 2014.

(2015)). These studies found only very small negative effects. However, the covariates recorded in available data sets may not adequately control for every important omitted factor, resulting in biased estimates. For example, students preparing for a career with a high expected income might borrow more to fund their college educations and also might be more likely to own a home in the future. To address the endogeneity of student loan debt, in their study of the effects of student loan debt on the future financial stability of student loan borrowers Gicheva and Thompson (2014) use the national average levels of student loan borrowing as an instrument. They find a more meaningful effect size, but identification in their approach may be confounded by other aggregate trends.[5]

In the context of the existing literature, this paper makes two key contributions. First, we use a uniquely constructed administrative data set that combines anonymized individual credit bureau records with Pell Grant and federal student loan recipient information, records on college enrollment, graduation and major, and school characteristics. The core credit bureau data—onto which the other anonymized data sources are merged—are based on a nationally representative sample of individuals who were 23 to 31 years old in 2004 and span the period 1997-2010. The administrative nature of our data likely provides us with more accurate measures of financial variables than self-reported data sets.

Second, we exploit a quasi-natural experiment to estimate the causal effect of changes in student loan debt on the homeownership rate over the first 60 months after the final school exit (where observable factors are measured at the time of the school exit). This eliminates the bias from unobservable factors that might affect estimates identified based solely on observable characteristics. The experiment is generated by increases in average

[5]Other studies, which are mostly based on trend analysis, include Brown et al. (2013), Akers (2014), Mezza et al. (2014); and analyses by TransUnion (Kuipers and Wise (2016)) and Zillow (`http://www.zillow.com/research/student-debt-homeownership-10563/`).

in-state tuition at public 4-year universities in subjects' states of residence prior to enrolling in post-secondary education (henceforth, home states).[6] In particular, increases in in-state tuition at public 4-year universities increase the amount of student loan borrowing, as a large fraction of post-secondary students attend public universities in their home states. Moreover, since home-state tuition changes are not determined by the choices of any individual student, we claim that these tuition price changes do not affect homeownership decisions through any channel other than increases in student loan debt. This claim is supported by a number of validity tests presented in Section 4.4. Mainly, we show that the estimated effect is not due to endogeneity of the instrument to local economic conditions, and provide evidence that selection along the extensive margin of college attendance cannot explain the results.

We find that the estimated effect from the procedure based only on observable controls is negative but very small, similar to the results from existing studies. In contrast, our estimates based on the quasi-natural experiment indicate a substantially larger reduction in homeownership due to student loan debt. Namely, a 10 percent increase in student loan debt causes a decrease of about 1 to 2 percentage points in the homeownership rate of student loan borrowers immediately upon school exit, relative to a mere 0.1 percentage point decline derived from the procedure based only on observable controls. The causal effect estimated using the natural-experiment framework shows little indication of diminishing across the 60 month window, although the precision of the estimates decreases with time.

To be sure, this paper estimates the effect of a *ceteris paribus* change in debt levels, rather than the effect of a change in access to student loan debt, on future homeownership. In particular, if student loans allow individuals to access college education—or, more broadly, acquire more of it—student loan debt could have a positive effect on homeownership, as long

[6]Relatedly, Bleemer et al. (2014) use state-level tuition measures to instrument for student debt in the context of its effect on parental co-residence.

as the return to this additional education allows individuals to sufficiently increase their future incomes. Given that changes in access to student loan debt could impact the decision to go to college, the type or quality of college attended, and the total educational attainment, such a research question is quite different from the question asked in this paper. Rather, the question we address is: "All else equal, if one were to obtain a certain level of education but at a somewhat lower price (and, consequently, with less debt), how would one's access to homeownership be affected?" Our exercise is similar in spirit to a thought experiment in which a small amount of student loan debt is forgiven upon exiting school, without any effect on individuals' past decisions on post-secondary education acquisition.

The rest of our paper is organized as follows. Section 2 briefly reviews the institutional background of the student loan market and examines the main theoretical channels through which student loan debt likely affects access to homeownership. Section 3 gives an overview of the data set and defines variables used in the analysis. Section 4 analyzes the effect of changes in student loan debt on homeownership using "selection on observables" as well as instrumental variable frameworks, and conducts several validity tests for our instrument. Section 5 interprets and caveats our main findings. Section 6 concludes.

2 Background and Mechanism

2.1 Institutional Background

Student loans are a popular way for Americans to pay the cost of college. Among bachelor's degree recipients who graduated in 2012, 70 percent had accumulated some student debt and 18 percent owed $40,000 or more.[7]

[7]SOURCE: College Board, *Trends in Higher Education: Student Aid* `http://trends.collegeboard.org/student-aid/figures-tables/cumulative-debt-bachelors-recipients-sector-time`.

Every student has access to federal student loans, which generally do not involve underwriting and can charge below market rates[8]. The amount of such loans students can borrow is capped by Congress, however. Federal student loans are also not dischargeable in bankruptcy, reducing the options of borrowers in financial distress.[9] Student borrowers frequently exhaust their available federal loans before moving on to generally more expensive private loans, often with a parent as co-signer.[10] Historically, the typical student loan is fully amortizing over a 10-year term with fixed payments. Deferments and forbearances can extend this term, as can enrollment in alternative repayment plans, such as the extended repayment plan (available for borrowers with high balances) and income-driven repayment plans (which have become more common in recent years and are available for borrowers with elevated debt-to-income ratios), and through loan consolidation.[11]

Student loan debt can impose a significant financial burden on some borrowers. Despite the inability to discharge federal loans through bankruptcy, 14 percent of recipients with outstanding federal student debt were in default as of October 2015.[12] Student borrowers are often young and at a low point in their life cycle earnings profile. The financial difficulties may be more severe for students who fail to graduate. Of the federal student loan borrowers who entered repayment in 2011-12 without a degree, 24 percent defaulted within two years.[13]

[8]Graduate students taking PLUS loans—as well as parents taking Parent PLUS loans—must pass a credit check.

[9]In 2005 the bankruptcy code was amended, making private student loans also not routinely dischargeable in bankruptcy.

[10]The share of loans with a co-signer increased significantly after the financial crisis, from 67 percent in 2008, to over 90 percent in 2011. SOURCE: CFPB, *Private Student Loans*, August, 2012.

[11]SOURCE: `https://studentaid.ed.gov/sa/repay-loans/understand/plans`.

[12]SOURCE: U.S. Department of Education, Federal Student Aid Data Center, Federal Student Loan Portfolio.

[13]SOURCE: U.S. Department of Treasury calculations based on sample data from the National Student Loan Data System.

2.2 Theoretical Mechanism

Most young home buyers must borrow the money to buy their first house. We conjecture that three underwriting factors provide a channel through which student loan debt can affect the borrower's ability to obtain a mortgage. First, the individual must meet a minimum down payment requirement that is proportional to the house value. While a 20 percent down payment is typical for many buyers, with mortgage insurance (whether purchased from a private company or a government agency such as the Federal Housing Administration (FHA)) the down payment can be significantly less.[14] Second, the individual must satisfy a maximum debt-to-income (DTI) ratio requirement, with the ratio of all her debt payments not to exceed a percentage of her income at the time the loan is originated. Third, the individual must satisfy a minimum credit score requirement. As these underwriting factors worsen for any individual (i.e. less cash available for a down payment, higher DTI ratio and lower credit score), she will be more likely to be rejected for a loan, or face a higher interest rate or mortgage insurance premium.

It is not hard to see how—all else equal—having more student loan debt can mechanically affect one's entry into homeownership through these three channels. First, a higher student loan debt payment affects the individual's ability to accumulate financial wealth that can then be used as a source of down payment. Second, a higher student loan payment increases the individual's DTI ratio, potentially making it more difficult for the borrower to qualify for a mortgage loan. Third, student loan payments can affect borrowers' credit scores. On the one hand, the effect can be positive: timely payments of student loan debt may help borrowers to improve their credit profiles. On the other hand, potential delinquencies adversely affect credit scores, thereby hampering borrowers' access to mortgage credit. At

[14]The FHA requires a down payment as low as 3.5 percent of the purchase value.

the same time, other non-underwriting factors might have effects as well. For example, from a behavioral perspective, if individuals exhibit debt aversion and wish to repay at least some of their existing debt prior to taking on new debt in the form of a mortgage, larger student loan debt burdens can further delay their entry into homeownership.

Various factors might influence how the effect of student loan debt on homeownership changes in the years after leaving school. Since cumulative balances are generally the largest immediately upon entering repayment (see Figure 15 in Looney and Yannelis (2015)), there are at least three reasons to believe that the ceteris paribus effect of higher student loan debt on homeownership access might be the largest immediately upon school exit. First, given that the income profile tends to rise over the life cycle and student loan payments are fixed, the DTI constraint should ease over time, as should the budget constraint, thereby allowing the individual to potentially accumulate assets for a down payment at a faster rate. Second, once all debt is repaid, the student loan debt component of debt payments in the DTI constraint disappears entirely. Of course, the past effects of student loan payments on accumulated assets are likely to be more persistent if student loan payments significantly impaired the individual's ability to save at a rate comparable to that of an individual with less student debt for a period of time. Third, any effect of debt aversion induced by a higher student loan debt burden at school exit should diminish over time as the balance is paid down.

However, there may also be countervailing effects. In particular, the propensity for homeownership is generally relatively low among those newly out of school and increases with age. Hence, the number of marginal home buyers may peak many years after the final school exit, suggesting that the effect of student loan debt might be increasing as the debtor ages. Also, individuals may exhibit habit formation in their housing tenure choice.

A marginal home buyer who is induced into renting by her debts may become accustomed to renting, in which case the apparent effect of student loan debt on homeownership could persist for many years.

3 Data

Our data are pooled from several sources.[15] Mezza and Sommer (2015) discusses the details of the data, checks the representativeness of the merged data set against alternative data sources, and provides caveats relevant for the analysis.

By way of summary, the data set starts with a nationally representative random sample of credit bureau records picked and provided by TransUnion, LLC, for a cohort of 34,891 young individuals who were between ages 23 and 31 in 2004, and spans the period 1997 through 2010. Individuals are followed biannually between June 1997 and June 2003, and then in December 2004, June 2007, and December 2008 and 2010. The data contain all major credit bureau variables, including credit scores, tradeline debt levels, and delinquency and severe derogatory records. In order to capture information on enrollment spells and the institutional-level characteristics associated with each spell, in the next step individual educational records through 2007 are sourced from DegreeVerify (for degrees) and Student Tracker (for enrollments) programs by the National Student Clearinghouse (NSC). Next, additional individual-level information on enrollment for spells funded by federal student loans, the amount of federal student loan borrowed during and the institutions associated with these enrollment spells, as well as information on Pell Grants received, is sourced from

[15] All the merges of individual-level information have been performed by TransUnion, LLC, in conjunction with the National Student Clearinghouse, and the Department of Education. The merges have been done based on a combination of Social Security number, date of birth, and individuals' first and last names. None of the variables used to merge individuals across sources is available in our data set.

the National Student Loan Data System (NSLDS) and merged onto the data for federal student loan and Pell Grant recipients. The NSC and NSLDS educational institution identifiers allow us to further merge institutional records from the Integrated Postsecondary Education Data System (IPEDS), such as tuition, sector (e.g., public, private for-profit and not-for-profit, open admission), and SAT and ACT scores that are summarized at a school level. Finally, information on the state of permanent residence at the time when individuals took the SAT standardized test—sourced from the College Board—is merged for the subset of individuals who took this test between 1994 and 1999, at a time when most of the individuals in our sample exited high school.[16]

Since our analysis aims to estimate the effect of a marginal change in cumulative student loan debt on future homeownership once all educational and college-funding decisions have been made, we collapse our panel into a cross-sectional data set where all explanatory variables are measured approximately at the time of the final school exit, and estimate their effect on the individuals' homeownership status observed at different time windows following school exit. An individual in our sample is thus characterized by variables such as age at the final school exit, cumulative student loan balance and credit scores at the time of that exit, highest degree ever obtained, and total days spent in school. Most variables are constructed using a single data source. However, for some variables we combine information from multiple data sources to increase measurement accuracy. In what follows, we describe how each variable used in our analysis is constructed, and discuss the final estimation data set after any sampling restrictions have been applied.

[16]The SAT test is not mandatory nor is it required by all institutions and, as such, not all potential college entrants take it.

3.1 Dependent Variable

Given that we are not able to observe the individual's homeownership status, we infer it from whether the individual has at least one open mortgage account in the TransUnion data N months following the final school exit. The credit data contains the opening and closing dates for each mortgage tradeline in a year/month format, meaning that the individual's homeownership status is observed at a monthly frequency. In our analysis, we treat the individual's homeownership status as an absorbing state, so that if an individual is observed to be a homeowner at a given point after the final school exit, the individual will be treated as a homeowner at all future times. For individuals with a mortgage tradeline prior to school exit, the binary dependent variable takes on a value of one at $N = 0$.

The obvious limitation of using mortgage tradeline information to infer the individual's homeownership status is that we will not be able to identify homeowners who are either cash-buyers or have already paid off their mortgage loans in full prior to June 1997.[17] However, given that individuals in our sample are between ages 23 and 31 in 2004, the population of such "unidentified" homeowners in our sample is likely to be small.

3.2 Independent Variables

Student loan balances: Ideally, student loan balances would be measured at the time of the final school exit. Unfortunately, given that TransUnion data are available only at particular points in time, generally this will not be the case unless school exit coincides with the exact dates for which we observe TransUnion records. Theoretically, after the final school exit, the cumulative student loan balance should increase only if accrued interest on the outstanding

[17] Individuals who have had a loan between June 1997—the earliest wave of TransUnion data available to us—and school exit will be treated as homeowners in our analysis.

balances or penalties (such as debt collection fees) exceed payments.[18] Practically, in our data, the student loan balance observed in the TransUnion wave just after the final school exit could be higher than that measured in the wave just before school exit because a borrower might have accumulated more student loan debt between the wave preceding the school exit and the school exit itself. Thus, to measure the balance at school exit as closely as possible, we use the maximum level of student loan debt observed in TransUnion in the waves immediately adjacent to the final school exit.[19]

Credit scores, credit card debt, and auto debt: Similarly to student loan balances, generally these variables are not observed at the exact time of the final school exit.[20] To avoid reverse causality, we use their most recent value observed in TransUnion before the final school exit.

Missing credit score: This indicator variable takes on a value of one if the individual does not have a credit score reported in TransUnion in the wave preceding the final school exit; zero otherwise.

Credit lag: This variable measures how many days before the final school exit the lagged credit variables—credit score, credit card, and auto debts—were measured in TransUnion.

Ever Pell and cumulative Pell Grants received: These variables indicate whether the individual ever received Pell grants to finance their post-secondary education and the total amount received, respectively.

Highest degree attained: We construct a set of seven mutually exclusive binary indicators

[18]For example, student loan borrowers do not have to start repaying their student loans right away after school exit, in general. The waiting period after school exit and before repayment begins is known as the grace period, and typically lasts six months. Subsidized Stafford federal loans do not accrue interest over the grace period, whereas other types of federal student loans do.

[19]As such, student loan balances are expected to be measured with error. The instrumental variable approach should deal with this measurement problem. Results are robust to whether we use the maximum value or a lagged value of student loan balances.

[20]The credit score used in this analysis is the TU TransRisk AM Score and it ranges from 270 to 900 points

for the highest degree ever attained. We group degrees into the following categories: (1) dropouts (i.e, those with at least some college but no attained degree), (2) associate's or certificate degree holders, (3) bachelor's degree holders, and (4) holders of a master's degree or more. Moreover, for some individuals, we observe a certain degree (such as a bachelor's degree) that is followed by another degree of unknown type. In such instances, when an associate's degree/Certificate or bachelor's degree are observed and are followed by a degree of unknown type, we assign individuals into the categories (5) at least an associate's degree or a certificate and (6) at least a bachelor's degree, respectively. Finally, those with just a degree of unknown type are grouped into a category (7) with a degree of unknown type.[21]

Majors: College majors are available only for those with completed degrees. We aggregate them into 15 different categories, described in detail in Mezza and Sommer (2015). If a major is missing but a degree was received, a "missing major" indicator takes on a value of one; zero otherwise.

School sectors: We construct a set of five non-mutually exclusive binary indicators capturing all school sectors with which an individual was ever associated while in school: (1) public 4-year, (2) public 2-year, (3) private 4-year not-for-profit, (4) private 2-year not-for-profit, and (5) private for-profit. To determine the school sectors in our data set, we need unique school level identifiers associated with each enrollment spell observed for a given individual in the sample. In theory, the NSC enrollment records should be sufficient to identify all enrollment spells and, consequently, allow us to observe all sectors attended. In practice, the NSC coverage is not perfect, largely due to school non-participation in the NSC Student Tracker and DegreeVerify programs (for detailed discussion, see Mezza and Sommer (2015)

[21]The NSC collects the graduation date and degree information from schools that report into the DegreeVerify program. Unfortunately, some graduation dates are reported without the type of degree associated with it. When a degree of unknown type is observed in the NSC, but borrowing from the federal government for a subsequent degree is observed in the NSLDS, we use this additional information to infer the degree.

or Dynarski et al. (2013)). Hence, in order to supplement the NSC enrollment data, we use enrollment information from the NSLDS for enrollment spells funded by federal student loans.

Age at final school exit: This variable captures the individual's age when the person exited school for the last time. To construct this variable, we use the maximum age at school exit based on the NSC and the NSLDS data.

Cumulative days in school: This variable counts the total days of enrollment in post-secondary education. The cumulative time spent in school is derived from enrollment histories that are constructed using the combined NSC and NSLDS enrollment records.

Pre-college state of residence (or home state): To construct the state of residence prior to the first postsecondary enrollment spell, we proceed in three steps. First, for individuals who took the standardized SAT test, we use these individuals' state of legal residence at the time when they took the test, reported in the College Board data. In our sample, 31 percent of students have their home state identified in this manner. Second, for individuals for whom this information is not available, we use the first state of residence observed in the TransUnion credit records as long as this information is available for the period that precedes the first college enrollment observed in the sample. An additional 12 percent have their home state identified this way. Finally, for the remainder of the sample, we impute the home state using data on the state in which the school associated with the first enrollment spell is located.

This last step can certainly appear problematic given that it could reflect an endogenous location choice associated with state-level college cost or college quality. However, a case can be made for why the state of the first college attended might be highly correlated with the individuals' pre-college state of residence. In particular, in the nationally representative

2003-04 Beginning Postsecondary Students Longitudinal Study, only 11 percent of first-time, non-foreign college entrants attended a post-secondary institution not in their state of legal residence, with the state of legal residence defined as the student's true, fixed, and permanent home.[22] Under this definition, if the student moved into a state for the sole purpose of attending a school, that state does not count as the student's legal residence. In our sample, 26 percent of students whose home state was identified by the SAT or their credit record attended an out of state school.[23] These students represent 11 percent of our total sample, accounting for the entire expected population of out-of-state students, and suggesting that among the remaining students the state of first college attendance is extremely likely to be their home state. We therefore do not believe that misidentification of home state is a significant issue.

Home state and year fixed effects: The state controls are associated with the home state described above while the time controls are associated with the year in which the person left school for the last time.

Unemployment rate, average weekly wages and house prices at the state level: The unemployment rate is sourced from the yearly Local Area Unemployment Statistics series by the Bureau of Labor Statistics (BLS) and captures the unemployment rate in the individual's home state for the year when the person exited school for the last time. The average weekly wages are sourced from the Quarterly Census of Employment and Wages by the BLS and capture the average weekly wages in the home state for the quarter when the person exited school for the last time. Finally, the house value index is sourced from Zillow and captures the median house value (measured in dollars) in the home state for the month when the

[22]Source for the definition: https://fafsa.ed.gov/fotw1415/help/fahelp46.htm.

[23]While the College Board data for SAT-takers is available only for a subsample of our total population, its coverage is likely skewed toward higher academically achieving individuals who are more likely to attend out-of-state selective institutions.

person exited school for the last time.

3.3 Candidate Instrumental Variable

Our candidate instrument for cumulative student loan balances at the final school exit is based on the average in-state tuition at public 4-year schools in the state where an individual lived before enrolling in college for the first time.[24] To construct the instrument, we proceed in three steps. First, we count the number of days that the individual spent enrolled in school per academic year.[25] Second, we assume that individuals pay the average in-state tuition at public 4-year institutions associated with the state of their pre-college residence (defined in Section 3.2), proportionally adjusted for the number of days spent in school in that academic year. Third, we add up these tuition costs across time—up until the final school exit in the sample—to capture the student loan balance accumulation over the course of post-secondary studies. Importantly, given that the time spent in school can also be correlated with omitted variables that might be associated with the homeownership decisions, we control for cumulative time spent in school separately.[26]

3.4 Estimation Data Set

In this subsection, we describe the final subsample used in the analysis. First, we focus on the population of college-going individuals with existing NSC enrollment records: 18,748 individuals. Second, to estimate the effect of student loan debt on homeownership once

[24]The data on the average in-state tuition at public 4-year school by state and academic year (starting with the academic year 1993-94) are available on the NCES's *Digest of Education Statistics* website: https://nces.ed.gov/programs/digest/. Average in-state tuition reflects the average undergraduate tuition and required fees.

[25]The academic year is assumed to start in July of a given year and end in June of the subsequent year.

[26]To give a concrete example, consider an individual who enrolls in college in July 1995 and stays enrolled until June 1996, and re-enrolls in school in July 1998 until March 1999. For this individual, the value of the instrument will be given by the summation of the in-state tuition in the academic years 1995-96 and 1998-99, proportionally adjusted by the fraction of the academic year spent in school each year.

education decisions have been made, we concentrate on individuals who have likely finished all of their post-secondary education. Given that enrollment spells sourced from the NSC and NSLDS are available to us only up to early 2008, we drop 5,383 individuals who were still in school after 2005.[27] Additionally, given that we only observe student loan balances (as well as other debt holdings and credit scores) starting in June 1997, we drop 1,008 individuals who left school prior to that date. Moreover, we drop 4 individuals who were not residing in any of the 50 U.S. states or the District of Columbia before starting college, as well as additional 150 individuals whose home states cannot be determined based on our methodology described in Section 3.2. Next, we drop 172 individuals whose earliest enrollment record corresponds to the date a degree was obtained, rather than an actual enrollment record.[28] Furthermore, we drop 403 individuals who had open student loans at the moment they exited school for the last time but whose balances were missing in their credit records at that time. Finally, we drop 443 individuals who last exited school in 2005 but whose homeownership status cannot be determined with certainty 60 months after their final school exit because their credit files are not available to us in 2010. This leaves 11,185 individuals, of whom 5,610 had non-zero student loan balances and comprise our estimation sample. Summary statistics of this estimation sample are reported in Table 1.

[27] While individuals who left school during or before 2005 could still go back to school after 2008, we ignore this possibility.

[28] Some schools participate in the NSC DegreeVerify program, but not in the Student Tracker program. Additionally, schools participating in both programs usually report graduation dates retroactively (frequently reporting back several years prior to their enrollment in the DegreeVerify), but report enrollment spells starting from the moment they enroll in the Student Tracker program (or just a few months prior).

4 Estimation

Student loan debt is correlated with homeownership, but this relationship is not stable over time following school exit. Figure 1 plots the probability of ever having taken on a mortgage loan against the number of months since school exit for different debt levels. In the top left panel, we compare students who attended college without taking on debt to those who did borrow. Debt-free individuals have higher homeownership rates directly out of school, but are overtaken by students who borrowed within three years after school exit. In the bottom left panel of Figure 1, we refine student borrowers into three categories based on amount borrowed: less than $15,000, between $15,000 and $30,000, and between $30,000 and $50,000. Comparing these groups, we can see that students who borrow the most are always most likely to be homeowners. Students who borrow moderate amounts start off less likely to own than non-borrowers, but eventually catch up. From these plots one might be tempted to conclude that, at least in the medium run, higher student loan debt leads to higher homeownership rates.

Determining how student loan debt affects homeownership is not so straight forward, however. Individuals with differing amounts of student loan debt may also differ in other important ways. Notably, they may have different levels of education, which is itself highly correlated with homeownership (possibly through an effect on income). The top right panel of Figure 1 restricts the sample to individuals with a bachelor's degree. Within this group, those without student loan debt always have higher homeownership rates than borrowers, and this difference is increasing with time since exiting school. In the bottom right panel, we can see that splitting the sample of borrowers further into groups by amount borrowed presents a similar picture. Students who borrowed more than $15,000 had the highest homeownership

rates among the general college going population five years out of school, but have the lowest rates among the subset with a bachelor's degree. As such, simple correlations clearly do not capture the whole picture.

4.1 Selection on Observables

Further factors that are correlated with both student loan debt and homeownership (and may be driving the observed relationship between these two variables of primary interest) include the type of school attended, the use of Pell grants, and the individual's credit history, for example. We attempt to identify the causal effect by regressing an indicator for homeownership on log student loan debt, controlling for a rich set of credit bureau and education variables, including state and year fixed effects. Results for the OLS and probit estimators are presented in Tables 2 and 3. Across both linear probability and probit models—and in line with results from Cooper and Wang (2014) and Houle and Berger (2015)—we find a very small but statistically significant effect, with a one percent increase in student loan debt leading to an approximately 0.02 percentage point decrease in the probability of homeownership 24 months out of school. Estimates are similar across the range of specifications in columns 1-5 in Tables 2 and 3.

Figure 2 plots estimates of the marginal effect of student loan debt against the number of months since the Final School Exit for the linear probability and probit models, respectively. These estimates are derived from the regressions using the vector of controls in columns 3 in Tables 2 and 3 for the OLS and probit specifications, respectively. Interestingly, nearly the full strength of the effect is apparent immediately. A one percent increase in student loan debt is associated with a reduction of approximately 0.01-0.015 percentage points in the probability of homeownership in the same month the individual is recorded as leaving school.

Moreover, the estimated effect is relatively stable within the 60-month window, though the precision of the estimated effect decreases over time and becomes insignificantly different from zero 45 months out of school.

4.2 Instrumental Variable Estimation

While the estimators used above control for some important covariates, there may still be unobservable variables biasing the results. The quality of school the student attended, the amount of parental contributions, and the individual's expected future income could all influence both student loan borrowing and the probability of future homeownership. The covariates we have may not adequately control for these or other omitted factors. To reliably identify the causal effect of student loan debt, we need a source of variation that is exogenous to all other determinants of homeownership.

We propose that the average tuition paid by in-state students at public, 4-year universities in the subject's home state provides quasi-experimental variation in eventual student loan balances. This variable cannot be affected by choices the individual subjects make. Rather, changes in the tuition rate depend in part on political battles over funding and expenditure decisions by the state universities. A large fraction of students attend public universities in their home state, so the loan amounts they require to cover costs vary directly with this price.[29]

Changes in tuition are not truly randomly assigned, however. A potential concern with the validity of this variable as an instrument is correlation with changes in state level economic conditions. Demand for secondary education or the supply of government subsidies

[29] As discussion in Section 3, 89 percent of first-time, non-foreign college entrants attended a post-secondary institution in their home state. In our estimation sample, 65 percent of students attend a public 4-year university at some point during their education (see Table 1).

may be related to local shocks that influence home purchase decisions. Later we will show that the results are robust to the inclusion of labor market and housing market controls. We also test whether the instrument is correlated with homeownership for individuals who did not attend college, as these individuals should therefore be unaffected by the instrument if it is valid. We find no evidence of any relationship for this group, suggesting that the main results are isolating a causal effect of tuition changes.

Another potential concern with the instrument is that it may affect homeownership through channels other than student loan debt. As the price of education changes, students may demand more or less of it. We will show, however, that effects of the instrument on the extensive margin of college attendance or borrowing are unlikely to be driving the main results.

4.3 Instrumental Variable Estimation Results

As mentioned in Section 3, we construct the instrument as the log of yearly in-state tuition at public 4-year universities, weighted by the number of days each academic year the subject spent attending school. We additionally control for the log of the number of days spent attending school, so the identifying variation comes entirely from the change in price. First stage results from regressing log student debt on the instrument and other controls are presented in Table 4. Across specifications, a one percent increase in the tuition measure is associated with an approximately 1.3 percent increase in student loan debt. The estimates are strongly statistically significant.

Turning now to the second stage, we find a considerably stronger effect of student loan debt on homeownership than in the earlier specifications without the instrument. Results for the 2-Stage Least Squares (2SLS) and IV-Probit estimators are presented in Tables 5

and 6. Across both linear probability and probit models, we find a statistically significant effect, with a one percent increase in student loan debt leading to an approximately 0.1-0.2 percentage point decrease in the probability of homeownership 24 months out of school.

Figure 3 plots estimates of the marginal effect of student loan debt against the number of months since the Final School Exit for the 2SLS and IV-probit models, respectively. These estimates are derived from the instrumental variable regressions using the vector of controls reported in columns 3 in Tables 5 and 6. As in the previous estimates, the full effect is evident immediately upon school exit. A one percent increase in student loan debt is associated with a reduction of approximately 0.16-0.23 percentage points in the probability of homeownership in the same month the individual is recorded as leaving school.

4.4 Validity Tests

Our identifying assumption that the instrument is exogenous to unobserved determinants of homeownership is not directly testable. We can, however, test for some plausible sources of endogeneity. For example, in-state tuition rates may be correlated with local housing and labor market conditions, which in turn affect homeownership rates. To see that such omitted variables are unlikely to bias our estimates, compare columns 4 and 5 in Table 6. Column 5 includes yearly home-state level economic controls: namely, the unemployment rate, log of average weekly wages and log median house price from the subject's home state measured at the time of the Final School Exit. Column 4 omits these local controls, but includes only observations for which these variables are available to facilitate comparison. The estimated coefficient on student loan debt is stable across the two specifications, suggesting that local economic conditions are not driving the results. However, there could be some other unobserved home state-level variation that is correlated with both homeowner-

ship rates and changes in tuition. In this case, homeownership rates should be correlated with tuition changes even among individuals who did not attend college. If the instrument is valid, in contrast, it should have no estimated effect on the homeownership rates of college non-attendees.

To test validity along these lines, we estimate the effect of the log of yearly in-state tuition on the probability of ever having owned a home each year from age 18 to 30 for individuals who never attended college. The relevant tuition figure is taken from the first academic year in which the individual had turned 18 by the preceding July. Results are presented in Table 7. We cannot reject the hypothesis that there is no partial correlation between the tuition measure and probability of homeownership for this group. Since the instrument only affects outcomes for college attendees, this rules out a certain class of arguments against validity.

Another potential challenge to validity comes from the use of a log specification in student loan debt—students without any debt are excluded from the regressions. If tuition increases affect the extensive margin of student loan debt and if these marginal borrowers have a notably different propensity to own than the inframarginal individuals, then the results may be contaminated by this non-random selection. Data on education variables, including the instrument itself, is more likely to be mismeasured for students without any loans, so we cannot estimate a consistent relationship between the instrument and a dummy for the presence of any student loans.[30] However, Table 8 shows that the reduced form effect of the instrument on homeownership is strongly negative and significant even with the full sample of college attendees, leading us to conclude that bias due to selection along the extensive

[30] As described in Section 3.2, records on degrees, school sectors, and enrollment spells are more accurately measured for student loan borrowers, as their NSC records are augmented with corresponding NSLDS data. By definition, for those who did not borrow, the NSLDS data do not exist.

margin of student loans is not driving our findings.

A further issue of sample selection is that tuition rates may affect the relationship between debt and homeownership through the composition of the student population. Table 9 presents some evidence that college attendance rates fall with increases in tuition in our data. In particular, the table shows the results of regressing the probability of attending college against the log of yearly in-state tuition, taken from the fall semester after the individual turned 18. We find a statistically significant effect, with a one percent increase in tuition causing a 0.08 percentage point decrease in college attendance. This could be a concern if the instrument is inducing a compositional shift in the estimation sample—i.e., if individuals on the margin of college attendance have very different propensities to become homeowners than inframarginal individuals. However, the estimated effect of the instrument on attendance is not large enough to explain the main result, no matter the propensities of the marginal group. As seen in Table 8, a one percent increase in tuition (conservatively) reduces the homeowning population by approximately 28 individuals per 10,000 college goers two years after exiting school. About 40 percent of the sample population does not attend college, so this translates to a reduction of approximately 17 individual homeowners per 10,000 individuals in the general cohort. The same increase in tuition reduces college attendance by only 8 individuals per 10,000, so even in the most extreme possible scenario in which all marginal attendees have a 100 percent probability of homeownership (rather than the 12 percent seen directly out of school in our sample), endogenous sample selection cannot explain the findings. Rather, because marginal college attendees tend to have lower ability and socioeconomic status than the average student, we would expect their propensity to own to be slightly lower than the general population of students. If anything, this suggests compositional effects may be biasing our estimates toward zero.

We noted previously that the full effect of student loan debt on homeownership was apparent immediately upon exiting school. Approximately 10 percent of the sample bought a home before leaving school, so it is reasonable to expect an effect even while the student is still attending. If student loan debt poses a credit constraint, however, we would expect to observe a particular pattern. Whether the main channel is through DTI ratios, credit scores or down payments, the constraint should become more binding as the student progresses through her education and debt accumulates. Similarly, the influence of debt aversion should grow. Therefore, the earlier in a student's educational career we look, the weaker the estimated effect of her (eventual) student loan debt on homeownership should be.

To test if this expected pattern holds in the data, we plot the estimated marginal effect of student loan debt against the number of months prior to Final School Exit for the 2SLS and IV-probit models in Figure 4. For the period approximately two to three years prior to school exit, there is not a statistically distinguishable effect of student loan debt on homeownership. With about a year to go, depending on the model, a significant effect becomes apparent and continues to strengthen until the student finishes school. This fits the intuitive story that the effect should be small or zero when debt balances are.

4.5 Additional Outcomes

Student loan borrowers may experience the burden of their debt in other areas than the binary outcome of homeownership. If DTI ratios or down payment constraints are binding, borrowers may substitute toward smaller mortgages in response to higher student debt levels. Alternatively, if student debt delays the home purchase decision to a point in the life cycle at which the borrower has a greater demand for housing, mortgage balances could conceivably rise with student debt. In the first column of table 10, we present the results

from regressing the (logged) loan amount of the first mortgage we observe for each individual against their student loan debts and the usual vector of controls. Only borrowers who obtain a mortgage within 5 years of leaving school are included in this regression. The estimated partial correlation is positive and statistically significant, implying a 10 percent increase in student loan debt is associated with approximately 0.3 percent higher mortgage balances.

This naive estimate is likely to be biased by omitted variables similar to those that bias estimates of the effect of student loan debt on homeownership. We apply the same instrumental variable solution, and present results in the second column of Table 10. This point estimate suggests that student loan debt causes lower average mortgage balances among the population of homeowners. The standard errors are very large, however, and the result is not close to statistically significant. Because mortgages are substantially larger than student loan balances (approximately 8 times larger in our sample) we cannot rule out meaningful effect sizes in either the positive or negative direction.

One channel through which we hypothesize student loan debt could affect homeownership is through the borrower's credit score. Increased debt balances can worsen credit scores directly, as well as potentially lead to delinquencies which have a further derogatory effect. The sign of the effect is ambiguous, however, as taking out and subsequently repaying student loans may help some borrowers establish a good credit history and thus improve their scores.

Borrowers whose credit scores place them in the subprime category, traditionally defined as those with a credit score below 620, may be more likely to have their loan applications denied. However, some subprime borrowers may still be able to obtain credit with FHA or other mortgage insurance. Prior to 2008, the first percentile of FICO scores was approximately 500 on FHA insured loans.[31] Borrowers with a score below this level would have

[31]SOURCE: Data provided by McDash Analytics, calculation by authors.

tremendous difficulty getting approved for a loan (in 2010, the Department of Housing and Urban Development set the minimum credit score for FHA loans at 500 by rule). We therefore define a further "deep subprime" category of borrowers whose scores are low enough to effectively deny them access to FHA loans.

In the first and third columns of Table 11 we present the results of a probit estimation, regressing borrowers' probability of falling into the subprime and deep subprime categories against their student debt and the usual vector of controls. We use cutoffs in the individual's TU TransRisk AM Score of 620 and 500 to define the categories. The sample is limited to observations between 0 and 60 months after school exit, with multiple observations per individual included. The second and fourth columns present the results from the IV regression. In both cases the instrumented estimates are larger than those from the simple regression, suggesting that a 10 percent increase in student loan debt causes over a 0.6 percentage point increase in the probability a borrower falls into the subprime category, and over an 0.8 percentage point increase in the probability of being deeply subprime. The estimated effect on becoming subprime is significant only at the 10 percent level, while the effect on becoming deeply subprime is significant at the traditional 5 percent level. While the effect of student loan debt on risk category is meaningful in magnitude, it is unlikely to be enough to fully explain the 1 to 2 percentage point decrease in homeownership the same increase in student loan debt implies.

The finding that increased student loan debt raises the probability of having very poor credit suggests that the burden of debt may be causing some borrowers to become delinquent on their loans. We estimate the effect of student loan debt on the probability the borrower is reported 90 days or more delinquent on a student loan payment. Results are presented in Table 12. The simple probit estimate in the first column suggests a 10 percent increase in

debt is associated with a 0.1 percentage point increase in delinquencies, similar in magnitude to the probit estimates on credit categories. In the second column, we use the instrument to deal with endogeneity of student loan debt and find a larger effect—a 10 percent increase in debt is estimated to raise delinquency rates by over 0.7 percentage points.

A caveat with the findings on risk categories and mortgage amounts is that these outcomes are not independent of each other or of the homeownership decision. For example, some of the estimated effect on subprime status may occur through the channel of lowered homeownership rates. Relatedly, marginal homeowners may demand different sized mortgages than homeowners whose tenure choice is insensitive to student loan debt. A single instrument is not enough to separately identify the direct effect of student debt on multiple outcomes, all of which could be channels through which the others are influenced.

5 Discussion

The previously presented results indicate that increased student loan debt causes a substantial reduction in the probability of homeownership for any given time frame within a five year window after exiting school. This negative effect is dominated, however, by the rapid and steady increase in the probability of homeownership as the individual ages across this same period. Using the estimates from the IV-probit model, we can simulate the effect of an increase in debt on the rate of homeownership among our sample population across the five year post-college window. In Figure 5, we plot the observed homeownership rate profile over time alongside a counterfactual simulation in which each student loan borrower in our data is burdened with a 10 percent increase in student loan debt at the Final School Exit.

As previously shown in Figure 3, the marginal probability is nearly constant over the

estimation window, so the simulated counterfactual in Figure 5 looks like a parallel downward shift of the plotted data, decreasing the homeownership rate by approximately one percentage point at every time period. Because homeownership is increasing almost linearly in time since school over the five year window, this is equivalent to a parallel shift to the right. A 10 percent increase in student loan debt delays the time it takes the cohort of borrowers to reach a given homeownership rate by approximately three months.

The data we currently have access to cannot answer the important question of how the relationship between debt and homeownership changes past the five year window. As borrowers age, their incomes generally increase and student loan balances fall, which may lead us to expect that the relationship between an individual's initial debt levels and homeownership should weaken over time. While our estimates lose precision as the time since school exit increases, we do not see any indication of an attenuating effect. If former students exhibit habit formation in their housing tenure decisions, marginal homeowners may be induced to rent for many years by the effect of student loan debt on their housing decisions immediately after college.

6 Conclusions

In summary, this paper estimates the effect of student loan debt on subsequent homeownership rates. We instrument for the amount of the individual's debt using changes to the in-state tuition rate at public 4-year colleges in the student's home state. We find that a 10 percent increase in student loan debt causes a 1 to 2 percentage point drop in the homeownership rate of student loan borrowers for the first five years after exiting school. Validity tests suggest that the results are not confounded by local economic conditions or

non-random selection into the estimation sample.

Our findings have implications for several recent trends and policy proposals. Tuition rates continue to rise, so the amounts students will need to borrow may increase in the future. Increased debt levels could continue to depress homeownership rates for future cohorts of college students. Measures taken to reduce tuition—or to curb borrowing beyond what is necessary to fund attendance—could fight this trend. Similarly, our results provide a measure of how effective student loan forgiveness programs could be at increasing the homeownership rate of young adults. Limiting or expanding students' *access* to education loans in general, however, would have ramifications that are beyond the scope of this study. In particular, if student loans allow individuals to access college education—or, more broadly, acquire more of it—student loan debt could have a positive effect on homeownership, as long as the return to this additional education allows individuals to sufficiently increase their future incomes.

In extrapolating our results to the present day, we also have to consider some significant recent changes to mortgage market. Students in our sample left school between 1997 and 2005; their first few years post-college took place in a relatively easy environment for mortgage credit. Since the housing and financial crisis, underwriting standards have tightened substantially. It is possible that student loan debt acts as an even greater drag on homeownership now that lenders are more sensitive to DTI ratios and low down payments. The growing popularity of income-driven repayment plans further complicates the picture, as it is not immediately clear how these plans moderate the link between initial student loan debt and homeownership. On the one hand, enrollment in income-driven repayment plans reduces the ratio of student loan payments relative to income, thereby relaxing the DTI constraint. On the other hand, it can extend the repayment period significantly relative to a 10-year plan, thereby potentially increasing the total interest paid by the student loan borrower over

the life of the loan. We hope that further studies using even more recent data will be able to shine additional light on the issue.

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Table 1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Homeownership Rate					
0 Months After School Exit	5,610	0.1			
12 Months After School Exit	5,610	0.16			
24 Months After School Exit	5,610	0.23			
36 Months After School Exit	5,610	0.31			
48 Months After School Exit	5,610	0.37			
60 Months After School Exit	5,610	0.43			
Student Loan Debt and Enrollment Measures					
Student Loan Debt (in $1,000 dollars)	5,610	19.1	24.77	0.05	318.88
Instrument: Tuition Measure (in $1,000 dollars)	5,610	14.21	8.27	0.55	87.23
Cumulative Days In School	5,610	1420.84	666.34	56	4,407
Age At Final School Exit	5,610	24.69	2.72	18.29	32.88
Degree Controls					
Associate's/Certificate	5,610	0.06			
Bachelor's	5,610	0.28			
Master's or More	5,610	0.03			
At Least Associate's/Certificate	5,610	0.003			
At Least Bachelor's	5,610	0.15			
Degree of Unknown Type	5,610	0.05			
Pell Grant Controls					
Ever Pell	5,610	0.56			
Average Pell (in $1,000)	5,610	1.26	1.34	0	4.31
School Sector Controls					
Ever Public 4-Year	5,610	0.65			
Ever Public 2-Year	5,610	0.44			
Ever Private 4-Year Not-for-profit	5,610	0.35			
Ever Private 2-Year Not-for-profit	5,610	0.01			
Ever Private For-profit	5,610	0.13			
Credit Controls					
Lagged Credit Score	5,610	565.6	206.43	0	848
Credit Lag	5,610	390.65	204.97	1	731
Missing Credit Score	5,610	0.06	0.24	0	1
Lagged Auto Debt	5,610	2.15	5.82	0	58.46
Lagged Credit Card Debt	5,610	1.42	3.28	0	63.41
Year of Exit from School					
1997	5,610	0.06			
1998	5,610	0.08			
1999	5,610	0.1			
2000	5,610	0.11			
2001	5,610	0.13			
2002	5,610	0.16			
2003	5,610	0.16			
2004	5,610	0.14			
2005	5,610	0.05			
Yearly State Controls*					
Unemployment Rate (Home State)	5,610	5.1	1.11	2.3	8.42
Log Avg. Weekly Wages (Home State)	5,610	6.54	0.17	6.02	7.16
Log Median House Price (HomeState)	4,794	11.89	0.43	11.03	13.18

Note*: Yearly home-state controls measured at school exit.

Table 2: Selection on Observables: OLS

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Log Student Loan Debt	-0.0139**	-0.0282***	-0.0200***	-0.0209***	-0.0212***
	(0.00661)	(0.00723)	(0.00654)	(0.00724)	(0.00718)
Log Cumulative Days In School	0.0606***	0.0106	-0.000647	0.00665	0.00720
	(0.0102)	(0.0123)	(0.0117)	(0.0118)	(0.0116)
Age At Final School Exit	0.0439***	0.0406***	0.0395***	0.0394***	0.0395***
	(0.00327)	(0.00329)	(0.00331)	(0.00371)	(0.00376)
Associate's/Certificate		0.161***	0.131***	0.132**	0.132***
		(0.0452)	(0.0437)	(0.0491)	(0.0491)
Bachelor's		0.150***	0.112**	0.0997*	0.0996*
		(0.0496)	(0.0470)	(0.0520)	(0.0521)
Master's or More		0.204***	0.136**	0.130*	0.130*
		(0.0647)	(0.0651)	(0.0759)	(0.0762)
At Least Associate's/Certificate		0.224	0.191	0.173	0.172
		(0.139)	(0.138)	(0.138)	(0.136)
At Least Bachelor's		0.215***	0.157***	0.138**	0.138**
		(0.0532)	(0.0501)	(0.0550)	(0.0546)
Degree of Unknown Type		0.156**	0.110*	0.0884	0.0877
		(0.0610)	(0.0566)	(0.0613)	(0.0610)
Ever Pell		0.0383*	0.0317	0.0300	0.0299
		(0.0200)	(0.0190)	(0.0213)	(0.0212)
Average Pell		-0.0443***	-0.0270***	-0.0257***	-0.0258***
		(0.00689)	(0.00675)	(0.00738)	(0.00735)
Ever Public 4-Year		0.0427***	0.0336**	0.0258	0.0259
		(0.0157)	(0.0158)	(0.0174)	(0.0175)
Ever Public 2-Year		0.0227	0.0228	0.0211	0.0212
		(0.0138)	(0.0137)	(0.0132)	(0.0132)
Ever Private 4-Year Not-for-profit		0.0498***	0.0423**	0.0355*	0.0354*
		(0.0167)	(0.0159)	(0.0179)	(0.0179)
Ever Private 2-Year Not-for-profit		0.0125	-0.00837	-0.00398	-0.00446
		(0.0431)	(0.0416)	(0.0434)	(0.0432)
Ever Private For-profit		-0.00222	0.0164	0.0296*	0.0303*
		(0.0176)	(0.0164)	(0.0168)	(0.0170)
Lagged Credit Score			0.000499***	0.000524***	0.000523***
			(4.70e-05)	(5.08e-05)	(5.04e-05)
Credit Lag			5.22e-06	1.74e-05	2.13e-05
			(5.97e-05)	(6.84e-05)	(7.01e-05)
Missing Credit Score			0.323***	0.342***	0.341***
			(0.0335)	(0.0383)	(0.0380)
Lagged Auto Debt			0.00934***	0.00913***	0.00913***
			(0.00101)	(0.00108)	(0.00107)
Lagged Credit Card Debt			0.00404**	0.00448**	0.00448**
			(0.00194)	(0.00222)	(0.00222)
Unemployment Rate (Home State)					0.00212
					(0.0136)
Log Avg. Weekly Wages (Home State)					-0.132
					(0.200)
Log Median House Price (Home State)					-0.0161
					(0.0591)
Constant	-1.289***	-0.929***	-1.162***	-1.232***	-0.182
	(0.0930)	(0.105)	(0.121)	(0.123)	(1.397)
College Major Controls	NO	YES	YES	YES	YES
Home State/Year FE	YES	YES	YES	YES	YES
Observations	5,610	5,610	5,610	4,794	4,794
R-squared	0.110	0.146	0.187	0.181	0.182

Note: Standard errors in parentheses (clustered at home-state level).

Table 3: Selection on Observables: Probit

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Log Student Loan Debt	-0.0132**	-0.0259***	-0.0174***	-0.0182**	-0.0186**
	(0.00630)	(0.00687)	(0.00671)	(0.00767)	(0.00763)
Log Cumulative Days In School	0.0758***	0.0225*	0.00661	0.0136	0.0141
	(0.0101)	(0.0122)	(0.0117)	(0.0125)	(0.0124)
Age At Final School Exit	0.0431***	0.0403***	0.0392***	0.0395***	0.0396***
	(0.00300)	(0.00288)	(0.00287)	(0.00323)	(0.00325)
Degree Controls	NO	YES	YES	YES	YES
College Major Controls	NO	YES	YES	YES	YES
School Sector Controls	NO	YES	YES	YES	YES
Pell Grant Controls	NO	YES	YES	YES	YES
Credit Controls	NO	NO	YES	YES	YES
Home State Yearly Controls	NO	NO	NO	NO	YES
Home State/Year FE	YES	YES	YES	YES	YES
Observations	5,610	5,610	5,610	4,794	4,794
Pseudo R-squared	0.106	0.140	0.179	0.170	0.170

Note: Standard errors in parentheses (clustered at home-state level).

Table 4: IV Estimation: 1st Stage

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Instrument: Tuition Measure	1.227***	1.273***	1.279***	1.308***	1.337***
	(0.201)	(0.167)	(0.167)	(0.195)	(0.198)
Log Cumulative Days In School	-0.143	-0.401**	-0.418***	-0.435**	-0.459**
	(0.193)	(0.153)	(0.152)	(0.177)	(0.183)
Age At Final School Exit	0.0701***	0.0387***	0.0311***	0.0319***	0.0330***
	(0.00570)	(0.00428)	(0.00458)	(0.00521)	(0.00536)
Associate's/Certificate		-0.187*	-0.168	-0.166	-0.169
		(0.104)	(0.103)	(0.104)	(0.105)
Bachelor's		0.169	0.188*	0.174	0.169
		(0.110)	(0.105)	(0.111)	(0.111)
Master's or More		0.0211	0.0785	0.0642	0.0521
		(0.141)	(0.141)	(0.152)	(0.154)
At Least Associate's/Certificate		-0.381**	-0.331*	-0.312	-0.326
		(0.189)	(0.193)	(0.204)	(0.205)
At Least Bachelor's		0.758***	0.798***	0.800***	0.787***
		(0.110)	(0.104)	(0.105)	(0.107)
Degree of Unknown Type		-0.325***	-0.296**	-0.274**	-0.283**
		(0.112)	(0.111)	(0.113)	(0.115)
Ever Pell		0.0811**	0.0731**	0.0894***	0.0897***
		(0.0307)	(0.0311)	(0.0325)	(0.0324)
Average Pell		-0.00147	-0.0122	-0.0117	-0.0130
		(0.0104)	(0.0112)	(0.0129)	(0.0128)
Ever Public 4-Year		-0.0205	-0.0148	-0.0247	-0.0233
		(0.0288)	(0.0287)	(0.0324)	(0.0319)
Ever Public 2-Year		-0.206***	-0.207***	-0.211***	-0.211***
		(0.0250)	(0.0244)	(0.0282)	(0.0286)
Ever Private 4-Year Not-for-profit		0.257***	0.263***	0.265***	0.265***
		(0.0325)	(0.0315)	(0.0358)	(0.0365)
Ever Private 2-Year Not-for-profit		-0.0304	-0.0186	0.0180	0.0238
		(0.0789)	(0.0803)	(0.0756)	(0.0766)
Ever Private For-profit		0.195***	0.168***	0.163***	0.170***
		(0.0458)	(0.0431)	(0.0455)	(0.0448)
Lagged Credit Score			-0.000565***	-0.000584***	-0.000587***
			(9.58e-05)	(0.000109)	(0.000109)
Credit Lag			-4.10e-05	-6.42e-05	-9.94e-05
			(8.29e-05)	(9.49e-05)	(9.76e-05)
Missing Credit Score			-0.509***	-0.503***	-0.506***
			(0.0641)	(0.0733)	(0.0743)
Lagged Auto Debt			-0.00370**	-0.00409*	-0.00407*
			(0.00182)	(0.00205)	(0.00202)
Lagged Credit Card Debt			0.0129***	0.0138***	0.0138***
			(0.00275)	(0.00308)	(0.00315)
Unemployment Rate (Home State)					0.0493*
					(0.0254)
Log Avg. Weekly Wages (Home State)					-1.089**
					(0.535)
Log Median House Price (Home State)					0.314**
					(0.145)
Constant	-1.191	0.882	1.518**	1.584*	4.852
	(0.860)	(0.723)	(0.735)	(0.865)	(4.336)
College Major Controls	NO	YES	YES	YES	YES
Home State/Year FE	YES	YES	YES	YES	YES
Observations	5,610	5,610	5,610	4,794	4,794
R-squared	0.370	0.477	0.484	0.482	0.484

Note*: Standard errors in parentheses (clustered at home-state level).

Table 5: IV Estimation: 2nd Stage 2SLS

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Log Student Loan Debt	-0.269***	-0.208***	-0.183***	-0.169***	-0.181***
	(0.0590)	(0.0523)	(0.0506)	(0.0531)	(0.0585)
Log Cumulative Days In School	0.320***	0.155***	0.128***	0.125***	0.135***
	(0.0621)	(0.0441)	(0.0438)	(0.0463)	(0.0508)
Age At Final School Exit	0.0590***	0.0456***	0.0429***	0.0425***	0.0430***
	(0.00473)	(0.00324)	(0.00308)	(0.00346)	(0.00360)
Degree Controls	NO	YES	YES	YES	YES
College Major Controls	NO	YES	YES	YES	YES
School Sector Controls	NO	YES	YES	YES	YES
Pell Grant Controls	NO	YES	YES	YES	YES
Credit Controls	NO	NO	YES	YES	YES
Home State Yearly Controls	NO	NO	NO	NO	YES
Home State/Year FE	YES	YES	YES	YES	YES
Observations	5,610	5,610	5,610	4,794	4,794
R-squared		0.029	0.092	0.104	0.092

Note*: Standard errors in parentheses (clustered at home-state level).

Table 6: IV Estimation: 2nd Stage IV-Probit

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Log Student Loan Debt	-0.187***	-0.154***	-0.130***	-0.113**	-0.115**
	(0.0327)	(0.0391)	(0.0402)	(0.0444)	(0.0473)
Log Cumulative Days In School	0.237***	0.124***	0.0957***	0.0884**	0.0906**
	(0.0319)	(0.0326)	(0.0343)	(0.0382)	(0.0408)
Age At Final School Exit	0.0440***	0.0379***	0.0358***	0.0361***	0.0363***
	(0.00230)	(0.00247)	(0.00246)	(0.00269)	(0.00270)
Degree Controls	NO	YES	YES	YES	YES
College Major Controls	NO	YES	YES	YES	YES
School Sector Controls	NO	YES	YES	YES	YES
Pell Grant Controls	NO	YES	YES	YES	YES
Credit Controls	NO	NO	YES	YES	YES
Home State Yearly Controls	NO	NO	NO	NO	YES
Home State/Year FE	YES	YES	YES	YES	YES
Observations	5,610	5,610	5,610	4,794	4,794

Note*: Standard errors in parentheses (clustered at home-state level).

Table 7: Probability of Homeownership for Non-College Goers

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Age	18	19	20	21	22	23	24	25	26	27	28	29	30
Instrument: Tuition Measure	-0.00597	0.00523	0.0184	0.0529	0.0237	0.0483	0.0387	0.0243	0.0171	0.0420	0.0497	0.0399	0.115
	(0.0171)	(0.0242)	(0.0306)	(0.0361)	(0.0449)	(0.0505)	(0.0503)	(0.0607)	(0.0650)	(0.0680)	(0.0598)	(0.0703)	(0.108)
Home State/Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,927	8,927	8,927	8,927	8,927	8,927	8,927	8,927	8,927	8,927	8,835	8,723	7,940
R-squared	0.007	0.006	0.008	0.010	0.013	0.016	0.020	0.020	0.020	0.020	0.019	0.020	0.023

Note*: Standard errors in parentheses (clustered at home-state level).

Table 8: Reduced Form—All College Goers

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Instrument: Tuition Measure	-0.361***	-0.315***	-0.274***	-0.282***	-0.288***
	(0.0463)	(0.0420)	(0.0406)	(0.0439)	(0.0450)
Log Cumulative Days In School	0.376***	0.314***	0.266***	0.274***	0.280***
	(0.0456)	(0.0419)	(0.0412)	(0.0444)	(0.0453)
Age At Final School Exit	0.0361***	0.0343***	0.0326***	0.0325***	0.0325***
	(0.00206)	(0.00229)	(0.00248)	(0.00261)	(0.00261)
Degree Controls	NO	YES	YES	YES	YES
College Major Controls	NO	YES	YES	YES	YES
School Sector Controls	NO	YES	YES	YES	YES
Pell Grant Controls	NO	YES	YES	YES	YES
Credit Controls	NO	NO	YES	YES	YES
Home State Yearly Controls	NO	NO	NO	NO	YES
Home State/Year FE	YES	YES	YES	YES	YES
Observations	11,628	11,628	11,628	10,262	10,262

Note*: Standard errors in parentheses (clustered at home-state level).

Table 9: Probability of College Attendance

Variable	**Coefficient**
Instrument: Tuition Measure	-0.0839**
	(0.0407)
Home State/Year FE	YES
Observations	25,790
R-squared	0.035

Note*: Standard errors in parentheses (clustered at home-state level).

Table 10: Log of First Observed Mortgage Balance

Variable	**(OLS)**	**(IV)**
Log Student Loan Debt	0.029**	-0.037
	(0.013)	(0.106)
Log Cumulative Days In School	0.110***	0.163
	(0.040)	(0.0419)
Age At Final School Exit	-0.020***	-0.020***
	(0.006)	(0.007)
Degree Controls	YES	YES
College Major Controls	YES	YES
School Sector Controls	YES	YES
Pell Grant Controls	YES	YES
Credit Controls	YES	YES
Home State/Year FE	YES	YES
Observations	2,410	2,410

Note*: Standard errors in parentheses (clustered at home-state level).

Table 11: Risk Categories

	Subprime		Deep Subprime	
Variable	**(Probit)**	**(IV Probit)**	**(Probit)**	**(IV Probit)**
Log Student Loan Debt	0.012***	0.066*	0.007*	0.085**
	(0.004)	(0.040)	(0.004)	(0.035)
Log Cumulative Days In School	-0.047***	-0.089***	-0.038***	-0.098***
	(0.010)	(0.030)	(0.009)	(0.027)
Age At Final School Exit	-0.007***	-0.008***	-0.005***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
Degree Controls		YES		
College Major Controls		YES		
School Sector Controls		YES		
Pell Grant Controls		YES		
Credit Controls		YES		
Home State/Year FE		YES		
Observations		15,135		

Note*: Standard errors in parentheses (clustered at home-state level).

Table 12: Student Loan Delinquencies

Variable	**(Probit)**	**(IV Probit)**
Log Student Loan Debt	0.011***	0.074***
	(0.003)	(0.024)
Log Cumulative Days In School	-0.007	-0.055***
	(0.006)	(0.030)
Age At Final School Exit	-0.003**	-0.005***
	(0.001)	(0.001)
Degree Controls	YES	
College Major Controls	YES	
School Sector Controls	YES	
Pell Grant Controls	YES	
Credit Controls	YES	
Home State/Year FE	YES	
Observations	15,095	

Note*: Standard errors in parentheses (clustered at home-state level).

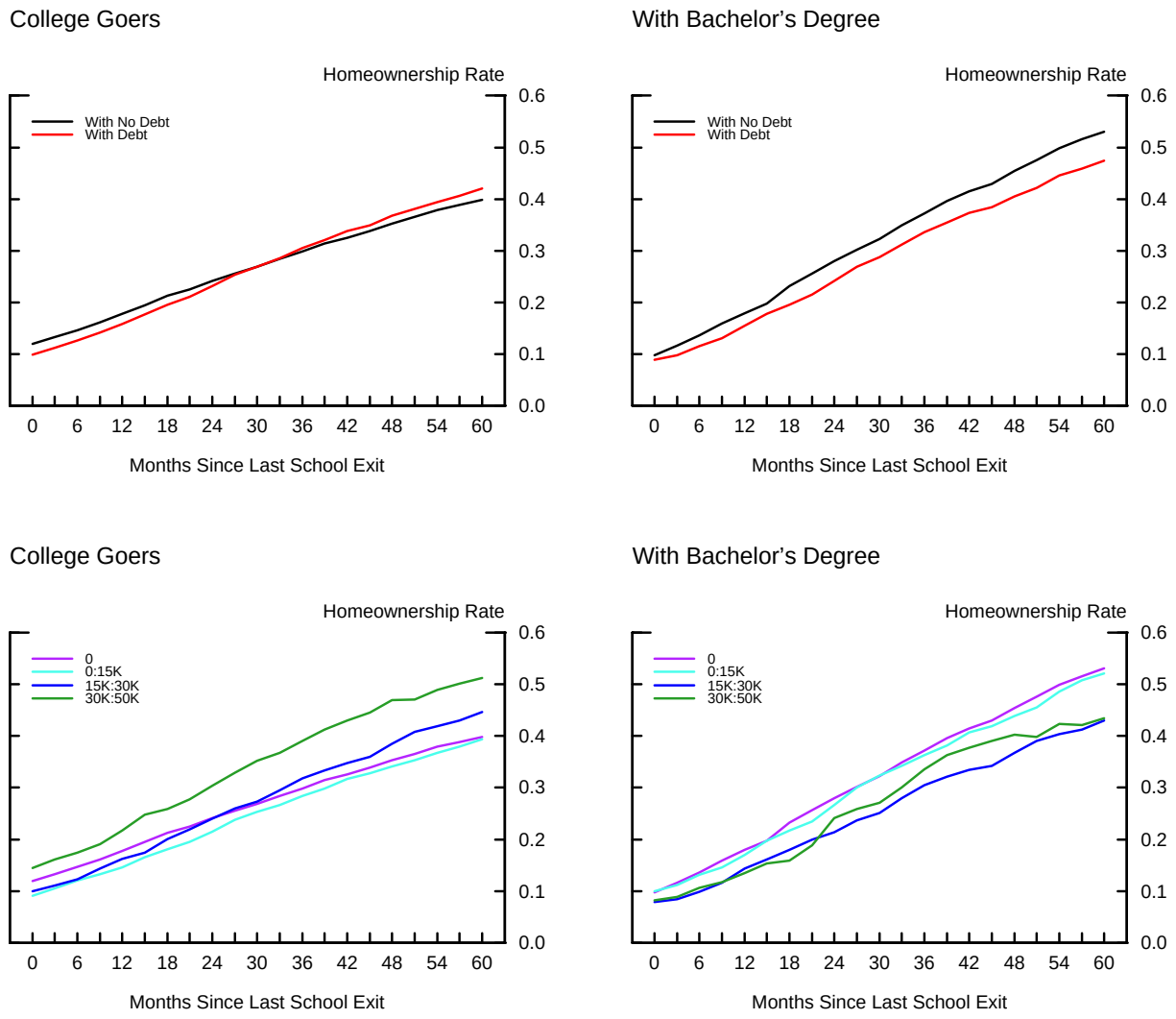

Figure 1: Homeownership Rates by Months Since Leaving School, Debt Level and Education

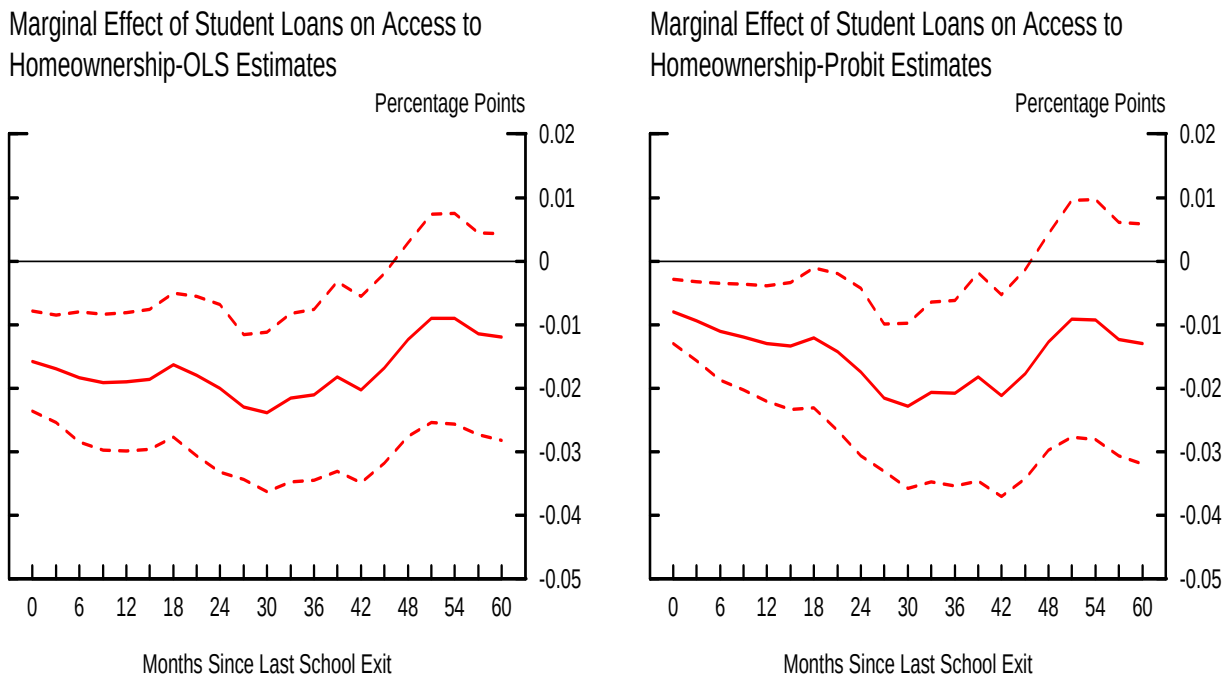

Figure 2: Estimation Coefficients: OLS vs Probit

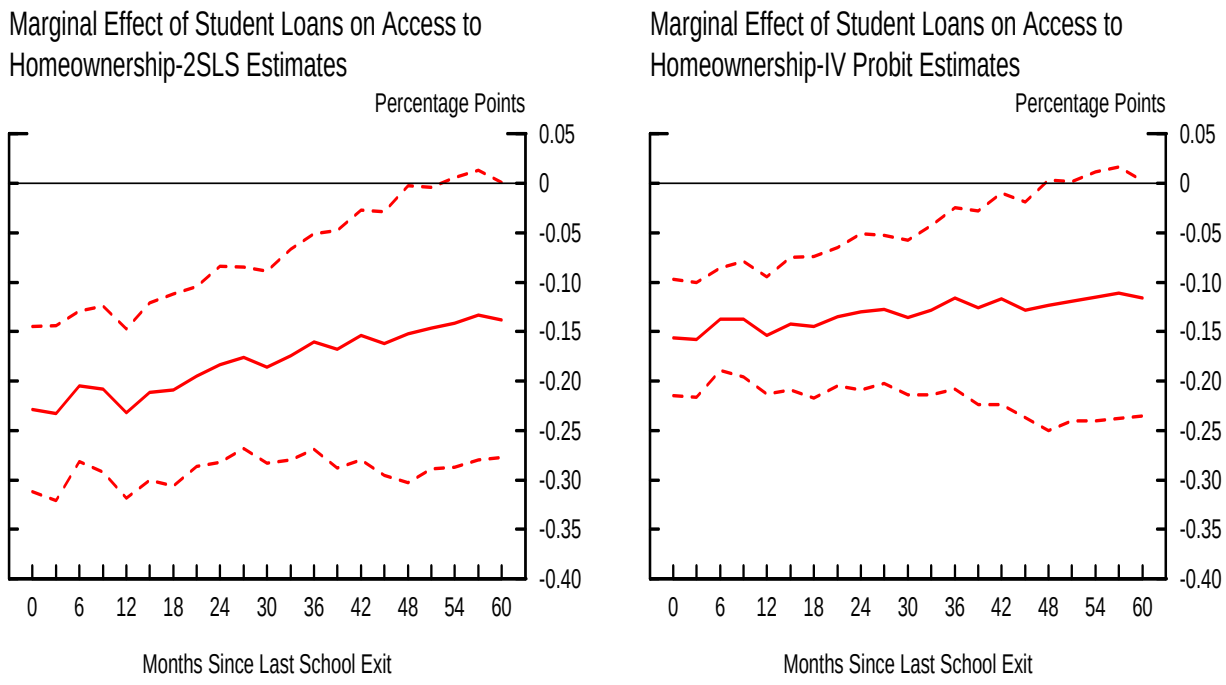

Figure 3: Estimation Coefficients: 2SLS vs IV Probit

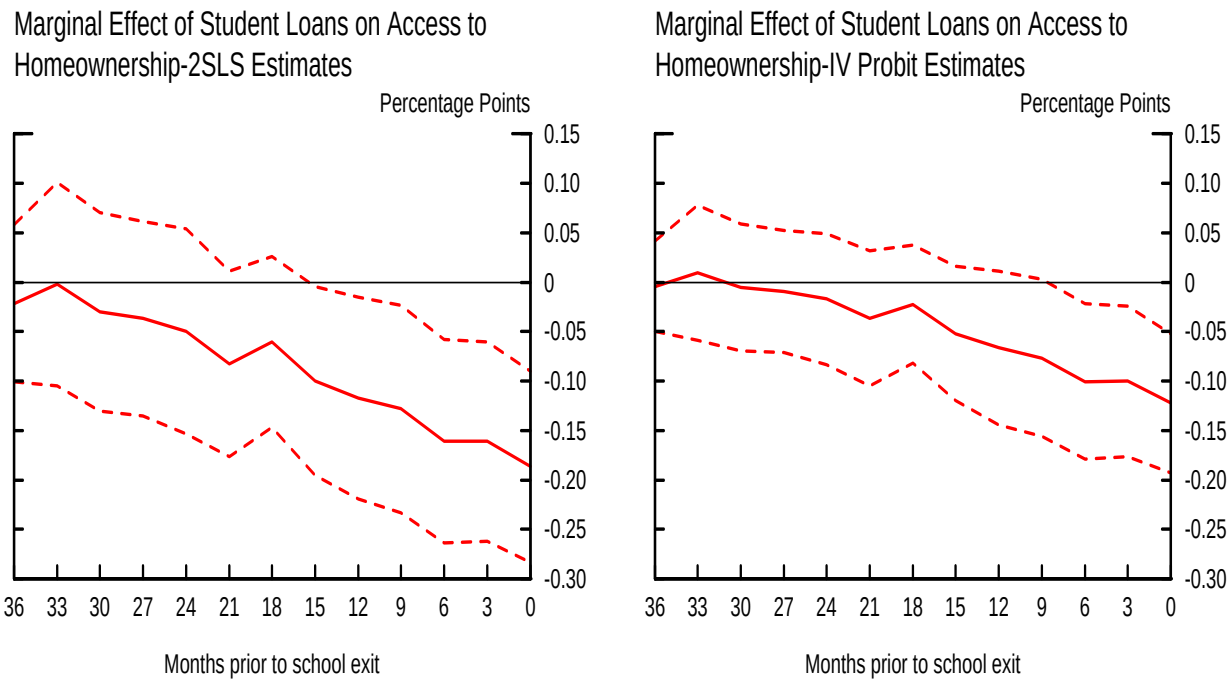

Figure 4: Estimation Coefficients Prior to Exiting College: 2SLS vs IV Probit

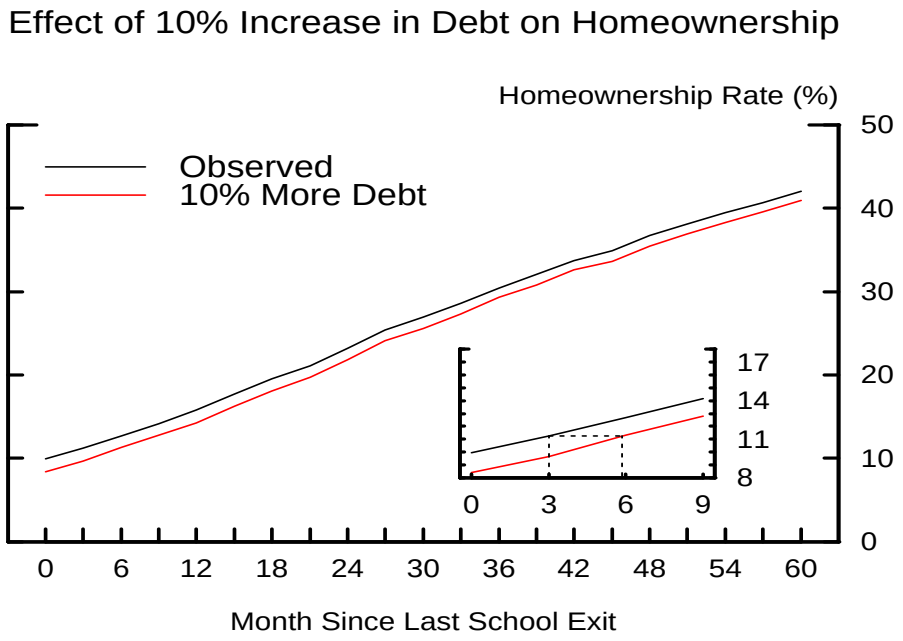

Figure 5: Simulation of Counterfactual Homeownership Rate