Student Loans and Homeownership*

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Abstract

We estimate the effect of student loan debt on subsequent homeownership in a uniquely constructed administrative dataset 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 $1,000 increase in student loan debt lowers the homeownership rate by about 1.8 percentage points for public 4-year college-goers during their mid 20s, equivalent to an average delay of about 4 months in attaining homeownership. Validity tests suggest the results are not confounded by local economic conditions or changes in educational outcomes. (JEL D14, I22, R21)

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1 Introduction

While the overall U.S. homeownership rate has fallen markedly since the onset of the Great Recession, the decline has been particularly pronounced among young households. The homeownership rate for households headed by individuals aged 24 to 32 fell 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.[1] In trying to explain this rapid decline, rising student loan balances have been implicated as an important drag on homeownership for the young by an array of economists and policy makers, and by the popular press.[2] Theoretically, student loan debt could depress homeownership by reducing borrowers' ability to qualify for a mortgage or desire to take on more debt. In corroboration, recent surveys have found that many young individuals view student loan debt as a major impediment to home buying.[3] Despite the attention the issue has received and the intuitive appeal of the causal claim, the evidence establishing an effect of student loans on homeownership is far from definitive.

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 (2015)). These studies found only small negative effects of increased debt burdens on homeownership. 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 (2015) use the national average levels of student loan borrowing as an instrument. They find a more meaningful effect size,

[1] Source: Current Population Survey.

[2] Some of the prominent figures making this claim include Nobel laureates Larry Summers and Joseph Stiglitz ("Student Debt is Slowing the U.S. Housing Recovery," *The Wall Street Journal*, May 21, 2014) and Senator Elizabeth Warren ("Senator Elizabeth Warren Asks For—And Gets—Realtors' Help," www.inman.com, May 12, 2016. See also: "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.

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

but identification in their approach may be confounded by other aggregate trends.[4]

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 turned 18 between 1991 and 1999 and include data through 2014. The administrative nature of our data likely provides us with more accurate measures of financial variables than the self-reported data sets that are often used in the literature.

Second, we use an instrumental variables approach, along with a treatment/control group framework, to identify the causal effect of changes in student loan debt on the homeownership rate for individuals between the ages of 22 and 32. The instrument is generated by increases in average in-state tuition at public 4-year universities in subjects' home states. Specifically, we instrument for the total amount of federal student loans an individual had borrowed before age 23 with the average in-state tuition at public 4-year universities from the four school years following the individual's 18th birthday. This tuition rate directly affects the amount students at these schools may need to borrow to cover their educational expenses, but cannot be affected by any choice or unobservable characteristic of the individual. In our preferred specification, we further restrict the sample to the population who did not offset any tuition increases with need-based Pell Grant aid, and for whom the instrument is consequently most relevant.

To eliminate bias from any state level shocks that could affect both the homeownership rate and public school tuition, we split the sample into a treatment and a control group. The treatment group is the set of individuals who attended a public 4-year university at any point before age 23, while the control group is all others.[5] Treated individuals are directly exposed to the tuition changes and their debt balances reflect this. Control group individuals are not directly affected by the tuition at schools they did not attend, and so they absorb any variation in economic conditions at the state level that may be driving tuition rates. We show

[4]Other studies 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/`).

[5]In Section 4.5 we show the results are robust to restricting the control group to other college attendees.

that the instrument passes several placebo tests—for example, while instrumented student loan debt has a substantial negative effect on the homeownership rate of the treatment group, no such relationship between public school tuition and homeownership is apparent for the control group. The estimated effect of student loan debt on homeownership is also quite stable to the inclusion of various sets of controls, at both the individual and market level (including state-by-year fixed effects).

A concern with this framework is that selection into the treatment group, i.e. attendance at a public 4-year university before age 23, is a choice on the part of the individual. It would seem quite plausible that the attendance choices of prospective students depend on the tuition they face, and such endogenous selection would bias our estimates. We show, however, that an individual's probability of attending a public 4-year university is essentially uncorrelated with the average tuition charged, at least, for the relatively small increases in tuition used in this study to identify the effect of interest. In Section 4.5, we discuss the issue of endogenous selection in detail and place our findings in the context of the relevant literature.

Using the aforementioned treatment/control group framework, we find a substantial negative effect of student loan debt on homeownership early in the life cycle. In particular, a $1,000 increase in student loan debt accumulated before age 23 (representing an approximately 10 percent increase in early-life borrowing among the treatment group) causes a decrease of about 1.8 percentage points in the homeownership rate of treatment group students by their mid-twenties in our preferred specification.[6] Given the rapidly increasing age profile of homeownership early in the life cycle, our results imply that a young person's entry into homeownership would be delayed one year by an increase of a little over $3,000 in student loan debt.[7]

In Section 4.7, we present evidence that credit scores provide a significant channel by which student loan debt affects borrowers ability to obtain a mortgage. Higher debt balances increase borrowers' probability of becoming delinquent on their student loans, which has a negative impact on their credit scores and makes mortgage credit more difficult to obtain.

[6] In contrast, the estimated effect from the procedure based only on observable controls is negative but very small for individuals in their twenties, similar to the results from existing studies.

[7] Between 2005 and 2014, the average amount of student loans borrowed by young people before the age of 23 increased by about $3,300. In Section 5 we provide a back-of-the-envelope calculation of how this rise in debt may have affected homeownership among the young.

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 as the return to this additional education allows individuals to sufficiently increase their future incomes. Thus, our exercise is similar in spirit to a thought experiment in which a small amount of student loan debt is forgiven at age 22, without any effect on individuals' decisions on post-secondary education acquisition.

Another caveat to keep in mind is that our estimation sample mostly covers the period prior to the Great Recession. Our findings may therefore be more relevant for times of relatively easier mortgage credit, as opposed to the immediate post-crisis period in which it was much more difficult to get a home loan. We discuss in Section 2.2 how various underwriting criteria in the mortgage market may interact with student loan debt to restrict some borrowers' access to credit.

Several recent studies have looked at the effect of student loans in different contexts, finding that greater student loan debt can cause households to delay marriage (Gicheva (2016) and Shao (2015)) and fertility decisions (Shao (2015)), lower the probability of enrollment in a graduate or professional degree program (Malcolm and Down (2012), Zhang (2013)), reduce take-up of low-paid public interest jobs (Rothstein and Rouse (2011)), or increase the probability of parental cohabitation (Dettling and Hsu (2017), and Bleemer et al. (2014)). These studies suggest credit constraints after post-secondary education may also be relevant outside the mortgage market.

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 presents the estimator in detail, as well as the results of both the instrumental variable analysis and a naive "selection on observables" approach. The instrument is then subjected to a series of validity checks. We also extend the analysis to investigate whether student loans affect the size of the first observed mortgage balance, and whether credit scores provide a channel by which student loan debt can restrict access to homeownership. 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, and the use of such loans has been increasing in recent years. In 2005, 30 percent of 22-year olds had accumulated some student loan debt, with an average real balance among debt holders of approximately $13,000. By 2014, these numbers had increased to 45 percent and $16,000, respectively.[8]

The vast majority of students have access to federal student loans, which generally do not involve underwriting and can charge below market rates.[9] 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.[10] Student borrowers frequently exhaust their available federal loans before moving on to generally more expensive private loans, often with a parent as co-signer.[11] 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.[12]

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.[13] Student borrowers

[8]Statistics are based on authors' calculations using the nationally representative FRBNY Consumer Credit Panel/Equifax credit bureau data. Our analysis focuses on young people and the debt they have accumulated before age 23. Overall debt levels are notably higher, as individuals can continue to accumulate debt past the traditional college-going age. The average outstanding loan balance for the overall borrower population was $27,000 in 2014, up from $20,000 in 2005.

[9]Some restrictions in eligibility apply. For instance, the post-secondary institution the student attends has to be included under Title IV to be eligible for federal student aid. Also, students who are currently in default on a student loan may not take out another. In addition, students face maxima in the amount they can borrow both in a single year and over time. Graduate students taking PLUS loans—as well as parents taking Parent PLUS loans—must pass a credit check.

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

[11]The share of private 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 `https://www.consumerfinance.gov/data-research/research-reports/private-student-loans-report/`.

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

[13]Source: U.S. Department of Education, Federal Student Aid Data Center, Federal Student Loan Port-

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.[14]

2.2 Theoretical Mechanism

We conjecture that three underwriting factors provide a channel through which student loan debt can affect the borrower's ability to obtain a mortgage, and hence enter homeownership.[15] 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.[16] Second, a higher student loan payment increases the individual's debt-to-income (DTI) ratio, potentially making it more difficult for the borrower to qualify for a mortgage loan. Third, student loan payments can affect the borrower's credit score. 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 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. Available evidence points to the existence of debt aversion in different settings, suggesting this mechanism might play a role in reducing the probability of homeownership.[17]

folio.

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

[15]Even in a standard life-cycle model with perfect capital markets and no psychological cost of debt (i.e., no debt aversion), student debt can affect homeownership (or, more generally, post-college decisions) through a negative wealth effect. However, for a typical individual, this effect is likely quite small, since the total student loan debt will only be a small fraction of the present discounted value of total lifetime earnings.

[16]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. The FHA, for example, requires a down payment as low as 3.5 percent of the house's value.

[17]For example, Palameta and Voyer (2010) find that some (Canadian) students are willing to accept a financial aid package with a grant but do not accept one that combines the same amount of a grant and an optional loan. Field (2009) finds evidence of debt aversion in an experiment where loan-repayment terms were randomly varied at NYU-Law school. Loewenstein and Thaler (1989) and Thaler (1990) find that payoff rates of mortgages and student loans are irrationally rapid, suggesting the existence of debt aversion. Other studies find less of an effect. In particular, focusing on students attending a highly selective university, Rothstein and Rouse (2011) find that the increase in post-graduation income and the decrease

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 largest immediately upon entering repayment (see Figure 15 in Looney and Yannelis (2015)), there are at least four reasons to believe that the ceteris paribus effect of higher student loan debt on homeownership access might be 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, the Fair Credit Reporting Act prohibits the credit bureaus from reporting delinquencies more than seven years old, so any difficulties the borrower had meeting payments will eventually drop off her credit report. Lastly, 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.

A simple, two-period model illustrates the various mechanisms by which student loan debt can affect homeownership over time.[18] Let hand-to-mouth consumers enter period 1 of adult life with some amount of student loan debt, $L > 0$. They earn income Y_1 and can choose to default on their student loans ($D = 1$) or pay them off ($D = 0$). They can also purchase a home ($H_1 = 1$) at price P, which requires a down payment of fraction θ of the total house price, θP. In the second period, consumers earn $Y_2 > Y_1$. If they defaulted on their student loans in period 1, they must pay them off in period 2 as student loans cannot be discharged. If they purchased a home in period 1, they must pay the remainder of the balance, $(1 - \theta)P$, in period 2. If they did not previously purchase a home ($H_1 = 0$), they have the option of paying P to purchase a home in this period ($H_2 = 1$). Finally, houses

in the probability that students choose low-paid public interest jobs due to exogenous increases in student loans are more likely driven by capital market imperfections (i.e, credit constraints post-graduation) than by debt aversion.

[18]For simplicity of exposition, we abstract from allowing households to save in deposits and, in this way, carry over funds across periods. However, it is easy to see that in an alternative set-up where savings are allowed, higher student loan balances would reduce household ability to save in period 1 and use these savings to partially fund housing purchases in period 2.

purchased in period 1 cannot be sold, meaning that $H_1 \leq H_2$.

Consumers' utility each period is an increasing but concave function of consumption, c. Homeowners receive an additive individual idiosyncratic utility benefit γ in each period they own. However, in order to purchase a home in period 1, consumers must qualify for a mortgage. First, if consumers purchase a home in period 1, they must meet a DTI ratio constraint such that the loan-to-income ratio is less than a threshold α; i.e., $\frac{L}{Y_1} < \alpha$. Second, there is a credit history constraint. If the consumer chooses to default on their student loans in period 1 ($D = 1$), then they are disqualified from borrowing and cannot purchase a home in that period. Additionally, borrowers are allowed to be debt-averse. Specifically, consumers with greater student loan balances, L, experience greater disutility from taking on mortgage debt, all else equal. The utility lost to debt aversion is captured by the function $\epsilon(L)$ that falls with the student loan amount, L (i.e., $\frac{d\epsilon(L)}{dL} < 0$).[19] In our stylized model, the debt aversion only enters consumer's utility in period 1 when mortgage debt is required to buy a home. The consumer's problem then becomes:

$$\max_{c_1,c_2,H_1,H_2,D} U(c_1) + U(c_2) + \gamma(\epsilon(L)H_1 + H_2) \tag{1}$$

subject to the budget constraints

$$c_1 + \theta P H_1 + L(1 - D) \leq Y_1 \tag{2}$$

and

$$c_2 + ((1 - \theta) + (1 - H_1)\theta)PH_2 + LD \leq Y_2 \tag{3}$$

and the credit market constraint

$$H_1 \leq 1\left\{\frac{L}{Y_1} < \alpha \cap D = 0\right\}. \tag{4}$$

Our simple model illustrates how higher student loan debt levels, L, affect the decision to purchase a home in period 1. First, due to concave utility ($\frac{d^2U(c)}{dc^2} < 0$) and the strictly increasing income profile ($Y_1 < Y_2$), the relative utility (and thus the probability) of waiting to buy a house in period 2 rather than paying the cost of a down payment in period 1 is

[19] When $\frac{d\epsilon(L)}{dL} = 0$, the debt aversion channel is not operative.

increasing in student loan debt, L. Second, the DTI ratio constraint ($\frac{L}{Y_1} < \alpha$) also is more likely to bind for consumers with more student loan debt, all else equal. Third, at high debt levels, defaulting also becomes a more valuable option for student loan borrowers. As L increases, consumers are more likely to default in order to shift the burden of student loan payment into a period with higher income (i.e., period 2) at the cost of restricting their access to mortgage credit in period 1 ($\frac{dProb(D=1)}{dL} > 0$). Fourth, if borrowers are debt-averse ($\frac{d\epsilon(L)}{dL} < 0$), then the higher the student loan balance, the less utility from homeownership borrowers enjoy when financing a home with a mortgage in period 1 and, therefore, the less likely they will be to purchase a home in that period.

Higher debt levels can also affect the decision to purchase a home in period 2. For borrowers who defaulted on their student loans ($D = 1$), the unpaid student loan balance, L, is due in the second period. The larger this balance is, the greater the marginal utility of consumption in period 2 and the lower the probability of choosing to purchase a home. Additionally, defaulters are less likely to buy a home in period 2 than those who did not default (and therefore have no more student loan debt to pay off), so increased student loan debt also reduces the probability of home buying in period 2 by increasing the probability of default in period 1. Among those who did not default ($D = 0$), however, the original student loan debt is fully paid off by the time borrowers enter period 2, and so it does not have a direct effect on their decision to purchase a home in that period.[20]

While our discussion thus far suggests that the effect of student loan debt on homeownership attenuates over time due to student loan debt repayment and rising incomes, there may 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 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.

The average marginal effect of student loan debt on homeownership for any given population will depend on the density of individuals near the relevant mortgage underwriting thresholds. The underwriting criteria, represented in our model by α, θ, and the require-

[20] A formal characterization of the solution to the model is available upon request.

ment that mortgage borrowers not default on their student loans, can change over time as mortgage credit availability eases and tightens. This paper investigates a population of individuals who were mostly making their home buying choices prior to the housing market collapse of the late 2000s. Mortgage credit tightened considerably in the following years and has subsequently been (slowly) relaxing. The average marginal effect of student loan debt may therefore be different in years with considerably different levels of credit availability, an important point to keep in mind when extrapolating our results to other time periods.

The mechanisms discussed in this section are not specific to student loan debt—auto loans and credit card debt could impose similar burdens on debtors in the housing market. Student loan debt is particularly interesting to study, however, because of its ease of availability. Young people without incomes or collateral are able to take on tens of thousands of dollars of debt to pay for their education without any underwriting of the loans. In contrast, a borrower without a credit history or source of income would face very tight limits in markets for privately provided credit. Student loans therefore present a unique channel for individuals to become heavily indebted at a young age. See Section 4.4 for an empirical treatment of the effects of total non-housing consumer debts.

3 Data

Our data are pooled from several sources.[21] Mezza and Sommer (2016) discuss the details of the data, check the representativeness of the merged data set against alternative data sources, and provide caveats relevant for the analysis.

By way of summary, the data set is built from a nationally representative random sample of credit bureau records 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 2014. Individuals are followed biennially between June 1997 and June 2003, then in December 2004, June 2007, December 2008, and then biennially again between June 2010 and June 2014. The data contain all major credit bureau variables, including credit scores, tradeline

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

debt levels, and delinquency and severe derogatory records.[22]

Since the credit bureau data do not contain information on individuals' education, historical records on post-secondary enrollment spells and the institutional-level characteristics associated with each spell were merged on the TransUnion sample from the DegreeVerify and Student Tracker programs by the National Student Clearinghouse (NSC). Additionally, individual-level information on the amount of federal student loans disbursed—our main measure of student loan debt—was sourced from the National Student Loan Data System (NSLDS). The NSLDS also provides information on Pell Grant receipts and enrollment spells funded by federal student loans, including the identity of each post-secondary institutions associated with the aid, which we use to augment the NSC data.

Information on individuals' state of permanent residence at the time they took the SAT standardized test—sourced from the College Board—was 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 were exiting high school.[23] Finally, we merged in institutional records, such as school sector (i.e. whether public or private, for-profit or not-for-profit, and 4- or 2-year), from the Integrated Postsecondary Education Data System (IPEDS).

In what follows, we describe the construction of key variables used in our analysis: homeownership status, student loan balances, and subjects' home state. A discussion of the remaining variables used in the analysis is available in Appendix A.1.

We are not able to directly observe the individual's homeownership status. Rather, the credit bureau data contain opening and closing dates for all mortgage tradelines that occurred prior to July 2014, which we use to infer homeownership by the presence of an open mortgage account. 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 cash-buyers. However, because our analysis is restricted to home-buying decisions made between the ages of 22 and 32, the population of cash-buyers is likely to be small, particularly among the sub-population that required student loans to fund their education.

[22]While we observe when all loan accounts have been opened and closed, as well as the complete delinquency events on these accounts, we only observe debt balances at the particular times when credit records were pulled, i.e., June 1997, June 1999, etc.

[23]The SAT is an elective competitive exam administered during students' junior and senior years of high school that is used in admissions determinations at selective colleges (and course placement at non-selective colleges). During the period we study, the SAT was fully elective and, as such, not all potential college entrants took it.

Furthermore, the credit-rationing mechanisms discussed in Section 2.2 would not bind on a buyer with enough liquid assets to purchase a house outright, so there is less scope for student loan debts to affect purchase decisions for any such individuals. 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 by a given month, the individual will be treated as a homeowner at all future dates.

The key explanatory variable, student loan balance, is measured as the total amount of federal student loans disbursed to an individual before they turned 23. We use disbursement of federal student loans from the NSLDS, rather than student loan balances from credit bureau data, for two reasons. First, balances in the credit bureau data are reported roughly biennially, so we do not observe student loan balances at the same ages for all individuals. Second, student loan balances from the credit bureau data are available to us for the first time in June 1997. By then, the oldest individuals in our sample were already 23 years old. A potential drawback of our approach is that the measure of total federal loans disbursed does not include accrued interest, repaid principal, or private student loans.

Our instrumental variables approach relies on the imputation of the subject's pre-college state of residence (henceforth, home state). To construct home states, we proceed in four steps. First, for individuals who took the SAT, we use these individuals' state of legal residence at the time when they took the test, as reported in the College Board data. Fifteen percent of our sample have their home state identified in this manner. Second, for individuals for whom SAT information is not available, we use the state of residence observed in the TransUnion credit records prior to their first enrollment in college, if these data are available. An additional 20 percent have their home state identified this way. Third, for the remaining 37 percent of the sample who attended college but did not fall in either of the above two categories, we impute the home state using data on the state in which the school associated with the first enrollment spell is located.[24]

This last step can certainly appear problematic given that it could reflect an endogenous location choice associated with state-level college costs or college quality. However, a case can

[24]In our data, 71 percent of individuals are identified as having attended college at some point. In the American Community Survey (ACS), only 64 percent of individuals in the cohort aged 23-31 in 2004 reported any college education by 2015. One possible source of discrepancy is the fact that not every person in the United States has a credit record. Those who did not attend college are possibly less likely to have interacted with formal credit markets, and so may be underrepresented in the TransUnion data.

be made that the state of the first college attended is a reliable indicator of the individual's home state among the sub-population that did not take the SAT or appear in credit bureau records prior to attending college. In particular, in the nationally representative 2003-04 Beginning Postsecondary Students (BPS) 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.[25] Under this definition, if the student moved into a state for the sole purpose of attending college, that state does not count as the student's legal residence. In our sample, 23 percent of students whose home state was identified by the SAT or their credit record attended an out of state post-secondary school.[26] These students represent 11 percent of our total sample of college attendees, 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.[27]

Finally, for the remaining 28 percent of individuals who neither attended college nor took the SAT, we impute their home states with the first state available in the credit records.[28] Public 4-year university tuition rates are assigned to individuals on the basis of their home state, as imputed by the procedure outlined above.[29]

Several filters are applied to the baseline cohort of 34,891 individuals. First, we drop 141 observations for which TransUnion was not able to recover personal identifying information on which to perform the merge. We then drop 40 individuals who were not residing in any of the 50 U.S. states or the District of Columbia before starting college and 6 individuals who we

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

[26]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. In the BPS, only 8 percent of students who did not take the SAT attended an out-of-state college.

[27]In Appendix A.3.2, we replicate our main results using a consistent definition of home state across observations, treating the state in which individuals first appear in the credit records as their home state. Results are broadly similar to our preferred specification, although slightly stronger. Our various sources to identify the home state coincide about 80-90 percent of the time when multiple sources are available for the same individual. Because a disproportionate fraction of out-of-state college attendees took the SAT (thereby giving us multiple measures of home state for more of these individuals), these numbers likely understate how accurate our preferred method of matching individuals to home states actually is.

[28]The average age at which we first observe a state for this group of individuals is 22.6

[29]The data on the average in-state tuition at public 4-year universities by state and academic year 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.

could not match to a home state. Moreover, we drop 698 individuals for whom we were not able to determine the school sectors they attended. Finally, we drop 571 individuals whose earliest enrollment record corresponds to the date a degree was obtained, rather than an actual enrollment record.[30] The resulting sample used in the analysis thus contains 33,435 individuals. Summary statistics for the variables we use in this analysis are presented in Table 1.

4 Estimation

In this section we present our findings. First, in Section 4.1, we describe some basic correlations between student loan debt and homeownership, including how these evolve over the life cycle and vary by education level. In Section 4.2 we show the results of several naive regressions, attempting to address the endogeneity of student loan debt by controlling for observable characteristics. Our main identification strategy, using an instrumental variables approach and the treatment/control group framing, is detailed in Section 4.3. We then present the results in Section 4.4. In Sections 4.5 and 4.6 we discuss potential failures of our identifying assumptions, and run a variety of tests to validate them. Finally, in Section 4.7, we estimate the effect of student loans on individuals' credit scores and delinquent status, and the size of their mortgage balances.

4.1 Patterns of Debt and Homeownership

Student loan debt is correlated with homeownership, but this relationship is not stable over the life cycle. Figure 1 plots the probability of ever having taken on a mortgage loan against the individual's age for different levels of student debt. In the top panel, we compare individuals who attended college before age 23 without taking on debt to those who did borrow, as well as individuals who did not attend college by that age. Debt-free college attendees have a higher homeownership rate than their indebted peers at age 22, but those with debt catch and surpass the debt free group by age 29. In the bottom left panel of Figure 1, we refine college attendees into three categories based on amount borrowed: no borrowing, less

[30]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).

than $15,000, and more than $15,000. Students who borrow moderate amounts start off less likely to own than non-borrowers, but eventually catch up. Those who borrowed the most start with the lowest homeownership rate at age 22, but are substantially more likely to be homeowners by age 32 (the median age of first home buying, according to the National Association of Realtors). From these plots one might be tempted to conclude that, at least in the medium run, higher student loan debt leads to a higher homeownership rate.

Determining how student loan debt affects homeownership is not so straightforward, 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 bottom right panel of Figure 1 restricts the sample to individuals who attained a bachelor's degree before age 23. Within this group, those without student loan debt always have a higher homeownership rate than borrowers. Comparing the bottom two panels, students who borrowed more than $15,000 had the highest homeownership rate among the general college going population after age 27, but have the lowest rate among the subset with a bachelor's degree at all ages. Bachelor's degree recipients with no student loan debt have the highest homeownership rate across the range of ages. As such, simple correlations clearly do not capture the whole picture.

4.2 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, choice of major, and local economic conditions, for example. One potential identification strategy is to attempt to absorb all these potential confounders with an extensive set of control variables. For the purpose of comparison with our instrumental variable estimates (presented in Section 4.4), we run age-specific regressions of an indicator for homeownership on student loan debts and various sets of controls using a probit model. In these and subsequent regressions, the individual level explanatory variables (including student loans disbursed) are all measured at the end of the individual's 22^{nd} year. All standard errors are clustered at the home state level.

Estimates of the effect of student loan debt on homeownership by age 26 are presented

in Table 2. Marginal probabilities, averaged over all individuals in the sample, are shown. Estimates are generally similar across the range of specifications in columns 1-4, which sequentially control for an increasingly rich set of covariates, including school sector, degree attained, college major, Pell Grant receipt, state and cohort fixed effects, and, finally, state by cohort fixed effects. A $1,000 increase in student loans disbursed before age 23 is associated with an approximately 0.1 percentage point reduced probability of homeownership by age 26. Figure 2 plots estimates of the marginal effect of student loan debt against borrower's age, derived from the regressions using the vector of controls in column 5 of Table 2. The estimated effect starts negative for borrowers in their early twenties and becomes positive when they reach their early thirties.

Our estimates from these selection-on-observables regressions are closely in line with previous findings in the literature. Using the National Longitudinal Survey of Youth, 1997, Houle and Berger (2015) estimate that a $1,000 increase in student loan debt decreases the probability of homeownership by 0.08 percentage points among a population composed largely of 20- and 25-year olds. Similarly, using the National Education Longitudinal Study of 1988, Cooper and Wang (2014) find that a 10 percent increase in student loan debt (approximately equivalent to a $1,000 increase for our sample) reduces homeownership by 0.1 percentage points among 25- and 26-year olds who had attended college.

4.3 Instrumental Variable Estimation

While the estimators used above control for some important covariates, there may still be unobservable variables biasing the results. It is not clear, *a priori*, in which direction the estimates are likely to be biased by such unobservable factors. For example, students with higher unobservable academic ability may borrow more, either because they choose to attend more expensive institutions or because they anticipate greater future incomes. These higher ability students would also be more likely to subsequently become homeowners, introducing a positive bias in the naive estimates. Conversely, students from wealthy backgrounds may receive financial assistance from their parents and therefore need to borrow less to pay for school than their less advantaged peers.[31] Parental contributions could help these same students to later purchase a home, which would tend to introduce a negative bias. The

[31]For example, Lovenheim (2011) finds shocks to housing wealth affect the probability families send their children to college.

covariates we have may not adequately control for these or other omitted factors. Reverse causality is also a potential source of bias, if purchasing a home before leaving school affects students' subsequent borrowing behavior. 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 during his or her prime college-going years provides quasi-experimental variation in eventual student loan balances. 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.[32] Additionally, this tuition cannot be affected by the choice of any particular individual. Rather, changes in the tuition rate depend on a number of factors that are arguably exogenous to the individual homeownership decision, ranging from the level of state and local appropriations to expenditure decisions by the state universities.

A short overview of the major drivers of prevailing tuition rates will help clarify the validity argument, and locate potential points of failure. One major source of tuition increases is changes to particular schools' cost structures. According to Weeden (2015), these costs include compensation increases for faculty members, the decision to hire more administrators, benefit increases, lower teaching loads, energy prices, debt service, and efforts to improve institutional rankings, all of which have been linked to tuition increases since the 1980s. Institutions also compete for students, especially those of higher academic ability, by purchasing upgrades to amenities such as recreational facilities or residence halls. These upgrades are often associated with increased tuition to pay for construction and operation of new facilities. Finally, tuition and fees are frequently used to subsidized intercollegiate athletic ventures. In recent years, athletic expenses have increased and now may require larger subsidies from tuition and fee revenue at many colleges.

Another major driver of tuition rates is the level of taxpayer support. As described in Goodman and Henriques (2015) and Weerts et al. (2012), public universities receive a large portion of their operating income from state and local appropriations. The amount of state and local revenue that public colleges receive is itself influenced by a diverse set of factors that weigh on legislators in allocating funds, including state economic health, state

[32]In our sample, nearly half of the students who had attended any college before age 23 had attended a public 4-year university in their home state.

spending priorities, and political support for affordable post-secondary education. Since public colleges can, in theory, offset the lost revenue from appropriations with increased tuition, appropriations for higher education can be crowded out by funding for other state programs.

Any correlation between the tuition charged at public universities and state level economic conditions (through the effect of economic conditions on appropriations) raises a concern about the validity of tuition as an instrument. To address this potential source of bias, we split our sample into treatment and control groups, with the treatment group defined as the individuals who attended a public 4-year university before they turned 23. We then compare the outcomes among the treatment group to those of the control group, which consists of all other individuals (except in specifications show in column 7 of Table 4, where the control group is all other individuals with at least some post-secondary education before age 23). Treatment group subjects pay the tuition charged at public 4-year universities, and so their total borrowing before turning 23 is directly affected by this tuition. In contrast, the control group is not directly affected by the tuition at public 4-year universities (which they did not attend). This framework therefore allows us to control for any correlations between state level shocks and tuition rates—either by including tuition rates directly as a control variable or by using state-by-year fixed effects—with the homeownership rate of the control group absorbing unobserved variation in economic conditions.[33]

A further concern might be that changes in tuition reflect other channels not absorbed by the control group, such as changes in school quality, and hence students' later economic outcomes. However, we can exploit a difference in the source of tuition funds to test for bias along these lines. Specifically, as pointed out by Belley et al. (2014), the net tuition paid by lower income students is divorced from the sticker price due to the availability of need-based grants. Our data allow us to further refine the treatment group into those who did not receive any federal need-based aid in the form of Pell Grants (and whose student loan borrowing therefore varied with the tuition rate) and those who did receive such aid before age 23. Estimates of the effect of tuition on these latter students' subsequent homeownership provides a placebo test for the instrument—students who receive Pell Grants experience the same changes in school and economic environment as their peers without Pell Grants, but are not exposed to the same variation in debt. We will demonstrate a strong effect of the

[33]We devote further consideration to the potential endogeneity of tuition in Section 4.5.

tuition charged at public 4-year universities on the student loan borrowing and subsequent homeownership only of students who did not receive any Pell Grant aid. Consistent with Belley et al. (2014), we will find little evidence that tuitions affect student loan borrowing or homeownership for students who did receive Pell Grants. The absence of any negative effect on their homeownership rates suggests that variation in school quality, or other state level factors specific to the treatment group, are not biasing our main results away from zero. We discuss these results in detail in Section 4.5.

We estimate the effect of student loans on homeownership via a two stage estimator that uses the interaction between tuition and an indicator for the treatment group as an instrument for student loan debt. The first stage of our instrumental variables regression is described in equation 5:

$$X_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 D_i + \alpha_3 Z_i \times D_i + \mathbf{W_i}\boldsymbol{\alpha_4} + \epsilon_i \tag{5}$$

where X_i is the amount of federal student loans borrowed by individual i prior to age 23, Z_i is the average tuition charged at public 4-year universities in i's home state in the four school years following i's 18^{th} birthday, and D_i is a dummy variable indicating i attended a public 4-year university before i turned 23. The vector $\mathbf{W_i}$ can include a variety of controls at the individual and state level, including fixed effects for individual's home state, birth cohort, or for the combination of the two, i.e., state-by-year fixed effects. The interaction term, $Z_i \times D_i$, is the only excluded term in the second stage. We estimate the second stage using equation 6:

$$Y_{it}^* = \beta_0 + \beta_1 X_i + \beta_2 Z_i + \beta_3 D_i + \mathbf{W_i}\boldsymbol{\beta_4} + \mu_{it} \tag{6}$$

where Y_{it}^* is a latent variable, and we observe Y_{it}, a dummy variable indicating i has become a homeowner by age t if and only if $Y_{it}^* > 0$.[34] The residual μ_{it} is assumed to follow a normal distribution. The parameter β_2 captures any partial correlation between tuition rates and homeownership among the control group, absorbing any state level shocks that affect both tuition and the homeownership rate. Note that in specifications with state-by-year fixed effects β_2 is not identified, as the average tuition rate is collinear with the fixed effects. The parameter β_3 captures the average difference in homeownership rates between

[34] We obtain essentially identical results using a linear probability model in a two-stage least squares estimator. See Appendix Section A.3.4.

the treatment and control groups. We are left identifying β_1, the effect of student loan debt on homeownership, by the widening or shrinking of the gap in homeownership rates between public 4-year school attendees and the general population as tuition rates change, analogous to a difference-in-differences estimator.

Estimates of β_1 may be inconsistent if membership in the treatment group is influenced by tuition rates. In particular, if the attendance decisions of students considering public 4-year universities are swayed by the prevailing tuition, then our estimates would suffer from sample selection bias. However, we will show that the variation in tuitions exploited in this study exert no meaningful effect on the probability of a student attending a public 4-year university. Given this result, we believe it is reasonable to consider treatment group membership to be exogenous. The issue of selection into the treatment group is discussed further in Section 4.6, in which we also consider the potential endogeneity of other educational outcomes.

The treatment group consists of traditional students—those that entered college immediately or very soon after high school, and attended a public 4-year university. Care should be taken when extrapolating our results to the general population which includes many individuals who enrolled in a private or public 2-year university, or who first attended college later in life. If such individuals respond to debt much differently than traditional students, we do not capture this heterogeneity of treatment effect in our estimates.

4.4 Instrumental Variable Estimation Results

First stage results from regressing student debt on the instrument and other controls are presented in Table 3. Across specifications, a \$1,000 increase in the sum of average tuition across the four years after the individual turned 18 is associated with an approximately \$150 increase in student loan debt for students in the treatment group. The estimates are strongly statistically significant. While we know of no formal test for weak instruments in the nonlinear maximum likelihood approach we are implementing, the F-statistics far exceed typical rule-of-thumb thresholds for linear models in all our specifications except column 1 (which does not include any control variables) and column 7 (which drops anyone who did not attend college from the control group). For reference, after controlling for state and cohort fixed effects, the residual of the four-year sum of in-state tuitions has a standard deviation of \$915 across our sample.

Turning now to the second stage, we find a considerably larger effect, in absolute terms, of student loan debt on homeownership than in the earlier specifications without the instrument. We present the results for the effect on homeownership at age 26 for a variety of specifications in Table 4. Across specifications, we find a $1,000 increase in student loan debt leading to an approximately 1 to 2 percentage point decrease in the probability of homeownership. Since the average treatment group student in our sample had accrued, in constant 2014 dollars, approximately $10,000 of federal student loan debt before age 23, the $1,000 increase in student loan balances represents approximately a 10 percent increase in borrowing for the average person in the treatment group. Further interpretation of the magnitude of these results is presented in Section 5.

Figure 3 plots estimates of the marginal effect of student loan debt against the borrower's age for several different specifications, along with 95 and 90 percent confidence intervals robust to clustering at the home state level. While the estimated magnitude of the effect of student loan debt is fairly consistent across specifications through student's mid-to-late 20s, statistical significance varies. In our most restrictive specification, using state-by-cohort fixed effects, we cannot reject the null hypothesis at conventional significance levels (top right panel). However, after discarding students who received Pell Grants (a subgroup whose debt should not be influenced by the instrument), we can reject the null at 5 or 10 percent confidence levels at every year but one from ages 24 to 31, even with the full set of fixed effects (bottom panel).

The estimates from the IV specifications imply a considerably stronger effect than those from the selection-on-observables estimates in Section 4.2. This difference suggests the presence of unobservable factors biasing the naive estimates. In particular, individuals with greater levels of student loan debt are positively selected into homeownership—that is, they have a greater underlying (unobservable) propensity to become homeowners than individuals with smaller amounts of debt do. It may be, for example, that students with greater labor market ability take on more student loan debt, either due to attending more expensive schools or because they anticipate higher lifetime incomes. These high ability (and highly indebted) individuals are then also more likely to become homeowners in their mid-20s.

The inclusion of educational controls in some specifications may pose a concern. On the one hand, changes in tuition could affect students' decisions about sectoral choice, completion, or which major to pursue. Failing to control for these variables could then lead to biased

estimation. On the other hand, these outcomes are potentially endogenous to unobserved determinants of homeownership, so their inclusion would introduce another source of bias. We show specifications with and without the controls (compare columns 1 and 2 of Table 4) and find qualitatively similar results. In Section 4.6 we show that there is little evidence that our measured educational outcomes are affected by movements in tuition.

It is worth keeping in mind that tuition changes could affect homeownership via channels not directly measured by student loan debt. If students (or their parents) have assets they draw down to pay for college, a higher tuition leaves them with less left over for an eventual down payment on a house. This behavior would tend to bias our estimates of the effect of debt away from zero.

Stripping away the assumed channel of student loan debt, we can look directly at the reduced form effect of tuitions on homeownership for the treatment and control groups. Table 5 presents results of regressing homeownership directly on the instrument and usual vectors of controls. Looking across the columns, every additional thousand dollars of tuition (charged over a four year period) leads to a 0.2 to 0.4 percentage point lower homeownership rate for the treatment group at age 26. In contrast, as illustrated in columns 1-4, tuitions do not appear to be negatively correlated with homeownership for the control group. It is not surprising that the reduced form effect of tuition is considerably smaller than the estimated effect of debt. Debts do not rise one-for-one with tuition hikes, as not all students attend school full time for four straight years post-high school, and not all students pay the sticker price of tuition (for example, if they receive need-based grants). Imposing an additional $1,000 cost on students would likely affect their homeownership rate more than the 0.2 to 0.4 percentage points estimated in the reduced form specification.

As we discuss in Section 2.2, student loan debt is unique in its availability to young people with poor credit or no credit history. That said, other debts could affect homeownership similarly, through many of the same channels that we discuss in that section. Ideally, we would like to capture the responses of students' entire debt portfolios—not only their federal student loans—to changes in tuition. However, the timing of data collection presents an obstacle to estimating the effect of total debt—credit bureau data are available only biennially, so we are able to observe debt before age 23 for only a subset of individuals in our data. Additionally, the oldest cohort was already 23 years old in 1997, the first year credit bureau data are available to us. Due to these features of the credit bureau data, we cannot

create consistent measures of total debt by age for everyone in our sample. In contrast, the student loan data from NSLDS that are merged on our core credit bureau sample provides a complete history of each subject's federal student loan borrowing, as it also spans the period prior to 1997.

Despite these data limitations, when we ran estimates using total non-mortgage debt (measured at age 23 or 24, with the estimation sample restricted to the population for whom these data were available) as the endogenous variable, we get similar results of the effect of the marginal dollar of debt on homeownership.[35] The full tables of results for our various specifications are presented in Appendix A.3.3. The second stage estimates are somewhat attenuated relative to those in Table 4, however. Using the specification from column 6, we estimate that a $1,000 increase in total debt reduces the probability of homeownership by 1.2 percentage points at age 26, while this figure was 1.75 percentage points for our main results using federal student loan debt records.

4.5 Endogeneity of Tuition

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 the estimates across columns 3, 4, and 5 in Table 4. Column 4 differs from column 3 by the inclusion of yearly home state level economic controls: namely, the unemployment rate, log of average weekly wages, and the CoreLogic house price index, all measured in the subject's home state at the age of 22. The estimated coefficient on student loan debt is stable across columns 3 and 4, suggesting that these local economic conditions are not driving the results. Furthermore, column 5 includes home state-by-cohort fixed effects which should absorb the effects of all broad economic conditions at the state level. Again, the coefficient of interest is quite stable to this stricter set of controls, suggesting our findings are not substantially biased by market level factors.

Further evidence that tuition affects homeownership only through the student loan chan-

[35]Total debt includes federal and private student loans, credit card balances and auto loans, as well as any accumulated interest on those debts. Individuals in our sample had about $11,500 in total debt on average by age 23 or 24, as opposed to about $5,000 of federal student loan debt disbursed before age 23. The two measures had a correlation of about 0.6.

nel is provided by the absence of any clear effect of tuition on the control group. The estimated coefficient on tuition, which measures the partial effect on the control group's homeownership rate, is small and changes sign across specifications. This can be seen by comparing columns 1 through 4 of Table 5. Since control group individuals do not pay tuition at public 4-year universities, their homeownership rates should not be correlated with that tuition except through omitted variable bias. We find no evidence that such omitted variables are affecting the correlations between tuition and homeownership. This is essentially a placebo test, validating the contention that we are picking up an effect of tuition rather than the influence of some unobservable factor correlated with it.

As we outlined in Section 4.3, another placebo test along these lines is suggested by Belley et al. (2014), which finds that the net tuition paid by lower income students is divorced from the sticker price due to the availability of need-based grants. While we do not observe family income in our data, we do observe Pell Grant receipt. We split the sample into those individuals who did and did not receive any Pell Grant aid before they turned 23. The former group received need-based aid, and so their student debt burden should be much less influenced by variation in the average in-state charged tuition. We re-estimate the first and second stages of our estimator on these two subgroups (including the full vector of controls and state-by-cohort fixed effects) and present the results in Table 6. The results for the subgroup that did not receive Pell Grants recapitulates columns 6 of Tables 3 and 4.

Among those who received some Pell Grant aid, we do not find a significant effect of tuition at public, 4-year universities on student loan debt in the first stage, as shown in column 1. The estimated (placebo) effect on homeownership, shown in column 3, is actually positive, although not significant. In contrast, we show in columns 2 and 4 that there is a strong first and second stage effect among the population that did not receive Pell Grant aid, and whose cost of college therefore varied directly with the charged tuition.[36] These findings further suggest that the correlation between the tuition measure and homeownership is causal. Taking the point estimates for the Pell recipient group seriously, however, this test might suggest that our main estimates are biased toward zero and we are somewhat underestimating the true effect of student loan debt on homeownership.

As constructed, our control group includes individuals who never attended college, as well

[36]Similar results hold for both subsamples over different specifications or when restricting the sample to only college goers. Results not shown, available upon request.

as students at private schools and public 2-year schools. A potential critique of the exclusion restriction is that tuition rates may reflect economic conditions relevant for college-goers, but not for their peers who did not receive any post-secondary education. If such were the case, our estimates may still be biased by the endogeneity of tuition to college attendee-specific economic shocks, despite the evidence discussed above. We deal with this issue by dropping all observations who had not enrolled in college before age 23 from the sample and re-estimating equations 5 and 6 on the sub-population with at least some college education. Results are presented in column 7 of Table 4. The estimated effect of student loan debt on homeownership is quite similar to that from previous specifications despite the redefined control group.

4.6 Endogeneity of Educational Outcomes

A further potential issue is bias from sample selection, due to the possibility that tuition rates may affect the relationship between debt and homeownership through the composition of the student population at public 4-year universities. Higher tuitions may deter some students from attending these schools. If such students have notably different propensities to become homeowners than inframarginal students, then our estimates of the effects of debt on homeownership would be biased. However, note that while the homeownership rate of the treatment group falls substantially when tuitions rise, this is not matched by an increase in the homeownership rate of the control group. The control group has a lower homeownership rate than the treatment group, so if individuals with a higher-than-average propensity to become homeowners switch out of the treatment group, then we would expect a significant increase in the control group's homeownership rate. As previously mentioned, columns 1 through 4 of Table 5 show that the estimated effect of tuitions on the homeownership of the control group is small, statistically insignificant, and changes sign across specifications.

To further address this potential source of bias, we can test whether our tuition measure affects students' decisions to attend a public 4-year university. If variation in the average in-state tuition is not correlated with enrollment decisions, then endogenous selection into the treatment group is not a concern.

In column 1 of Table 7, we show the results of regressing D_i—the indicator for having attended a public 4-year university before age 23—on our tuition measure and state and

cohort dummy variables. We find no evidence that changing tuition affects the probability an individual attends such a school. For completeness, in column 2 we show the estimated effect of tuition on the probability of college attendance regardless of sector, for which we find a similar null result. In column 6, we restrict the sample to only those who attended college before age 23, and again find no significant effect of tuition on the probability of attending a public 4-year university. This last test suggests that tuition at public 4-year universities does not induce switching between school sectors, at least for the relatively modest variation in the cost of schooling that our study exploits. Given the above evidence, we believe that defining our treatment group based on attendance at a public 4-year university does not meaningfully bias our estimates.

Previous studies have reached mixed conclusions as to the effect of tuition on college attendance. Similar to our estimates, Shao (2015) and Bleemer et al. (2017) use variation in tuition at public institutions to conclude the attendance and completion margins, respectively, are insensitive to costs. Other studies have found more significant effects. As discussed in a review paper by Deming and Dynarski (2009), this literature often focuses on low income or generally disadvantaged students, and the best identified papers find a $1,000 tuition increase (in 2003 dollars) reduces enrollment by 3 to 4 percentage points. These various findings may be reconcilable if the decision of traditional students to attend public 4-year colleges is price inelastic, while the attendance decision of marginal students considering community colleges or certificate programs is more price sensitive (Denning (2017)).[37]

We can test for this potential heterogeneity in price elasticity by regressing the probability of attending a public 2-year college against the average tuition charged by such schools in the individual's home state in the two years after they turned 18. Results of these regressions are shown in column 3 of Table 7. This test is analogous to our baseline experiment, shown in column 1 of Table 7. The point estimate of the effect of public 2-year tuition on enrollment at public 2-year colleges is over thirty times as large as the point estimate on the effect of public

[37]In apparent contradiction to our results, Castleman and Long (2016) and Bettinger et al. (2016) find that grant aid affects the enrollment of students at public 4-year universities. However, as argued in Denning (2017), grant aid may have stronger effects on the college attendance choice than changes in the sticker price of tuition do—the margin that we study. The grant aid programs studied in these papers target lower income students, which are likely more price sensitive, while changes in the sticker price affect a much larger base of students. Moreover, the size of the aid grants studied is meaningfully larger than the small year-to-year variation in tuitions we use, which could make for qualitatively different effects. In particular, the Cal Grant program studied by Bettinger et al. (2016) allows qualifying students to attend public universities tuition free.

4-year tuitions on attendance at public 4-year universities. Specifically, a $1,000 tuition increase (in 2014 dollars) decreases public 2-year college attendance by over 2 percentage points. This effect, although imprecisely estimated, is quite similar in magnitude to previous estimates covered in Deming and Dynarski (2009), especially when correcting for the 28 percentage points of inflation between 2003 and 2014.

Tuition may also affect other educational outcomes, such as degree completion, take up of financial aid, or the choice of major. These outcomes may in turn affect the probability of homeownership—for example, completing a college degree may boost the student's income and allow them to afford a home—which would violate the exclusion restriction. We therefore control for these outcomes in our preferred specifications. However, such outcomes may be endogenous to unobservable determinants of homeownership, in which case the estimator would still be inconsistent. Comparing columns 1 and 2 of Table 4, we can see that the estimated effect of student loan debt on homeownership is qualitatively similar regardless of whether additional educational controls are included. We can also test for whether tuition is correlated with any of these outcomes. In columns 4 and 7 of Table 7, we present estimates of the effect of tuition on the probability of completing a bachelor's degree before age 23, for the general population and the subsample that attended college, respectively. We do not find any significant correlation between tuition and the completion of a bachelor's degree. In columns 5 and 8, we estimate the effect of tuition on the probability of receiving any federal Pell Grants for the full sample and the college-going subsample. Again, there is no significant effect.

Finally, we estimate the effect of tuition on the choice of major for those attending a public 4-year school before age 23, modeled as a multinomial logit regression with majors categorized into one of 16 groups. Results are presented in Table 8. We find little evidence of an effect of tuition on major choice. The estimated effect on the risk ratio relative to no declared major is significant for only one major choice—public administration and social work (number 13). This major choice is quite uncommon as well—only 42 individuals in our treatment group sample majored in this field.

4.7 Additional Outcomes

As we discuss in Section 2.2, there are multiple channels by which student loans could theoretically affect homeownership. One such channel we hypothesize is the detrimental effect of student loan debt on the borrower's credit score.[38] Increased debt balances could worsen credit scores directly if the credit score algorithm places a negative weight on higher student debt levels.[39] Moreover, increased debt could lead to delinquencies which would have a further derogatory effect. Delinquencies are a salient concern for many student loan borrowers—according to a report by the Federal Reserve Bank of New York, the fraction of student loan balances that were 90 days delinquent or more increased from over 6 percent in 2004 to over 11 percent in 2017 (Federal Reserve Bank of New York (2018)). The sign of the overall 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.

We estimate the effect of student loan debt on credit scores, regressing the probability that a borrower's credit score ever fell below one of two underwriting thresholds by a given age against their student loan debt and the usual vector of controls. We use the same instrumental variables strategy as in Section 4.3 to deal with the endogeneity of student loan debt. The thresholds are chosen to roughly correspond to FICO scores of 620 and 680.[40] We refer to the lower credit range as "subprime," and to those in the intermediate range as "non-prime." Results from the full sample for age 26 are presented in the first and third columns of Table 9, with the specification corresponding to column 5 of Table 4. The second and fourth columns present the results of the regressions for the subsample who did not receive any Pell Grant aid before age 23, with the specification corresponding to column 6 of Table 4. Results are similar for both the full sample and restricted subsample, suggesting that a $1,000 increase in student loan debt causes a nearly 2 percentage point increase in the probability a borrower falls below each of the thresholds, although the estimates are slightly more precise for the group that did not receive any Pell Grant aid. It appears that student

[38] Unfortunately, we do not have direct measures of the other hypothesized constraints—DTI ratios, down payments, and debt aversion—to test whether these additional channels play a role in explaining our main result.

[39] Credit scores are generally based on proprietary algorithms, however Goodman et al. (2017) find a negative effect of federal student loan debt on TU Risk Scores.

[40] A FICO score of 620 is shown by Laufer and Paciorek (2016) to be a relevant underwriting threshold for mortgage lenders. We thank Ezra Becker and TransUnion for guidance in suggesting 680 as another significant threshold for underwriting.

loan debt plays a role in driving down borrower's credit scores.

In columns 5 and 6, we report the estimated effect of student loan debt on the probability of ever having been 90 days or more delinquent on a student loan payment for the full sample and restricted subsample. The results suggest that a $1,000 increase in debt increases the probability of ever having been 90 or more days delinquent by age 26 by 0.3 to 0.5 percentage points, respectively. The estimate for the non-Pell recipient population is larger and more precise. These results may suggest that borrowers are more likely to miss payments when their debt burdens are greater, and the resulting damage to their credit scores makes qualifying for a mortgage more difficult. In columns 7 and 8 we show the estimated effect of student loan debt on borrowers becoming delinquent on credit card debts or auto loans. Together, mortgages, student loans, auto loans and credit card balances account for over 96 percent of all household debt.[41] In neither sample do we find evidence that increased student loan debt leads to more delinquencies on these other forms of debt.

In Figure 4 we plot the estimated effect of student loan debt on having a non-prime credit score (corresponding to a FICO score of 680 or below), and on ever having been 90 days delinquent on a student loan payment, by age, from 22 to 32. Results for both the full sample (top panels) and the subsample without any Pell Grant aid (bottom panels) are shown. The estimated effects on credit scores, shown in the left hand panels, are not significant at first, but grow in magnitude and remain persistently significant after age 26 for both samples. In the right hand panels, we can see a similar pattern for the effect of student loan debt on delinquencies, although the estimates are only significant across multiple years for the subsample that did not receive Pell Grants. These results suggest access to homeownership could be impaired by student loan debt's negative effect on credit scores, in part through the channel of increasing delinquencies on those debts. However, because student loan debt begins to have a significant effect on both homeownership and credit scores at about the same age, we cannot rule out the possibility of reverse causality (i.e., that mortgage debt improves credit scores).

Another source of adjustment through which student loans could be affecting the housing market is by influencing the amount of mortgage debt borrowed. The direction of the effect is theoretically ambiguous. If DTI ratios or down payment constraints are binding, borrowers may substitute toward smaller mortgages in response to higher student debt levels.

[41] Authors' calculations based on credit bureau data from the FRBNY Consumer Credit Panel/Equifax.

Alternatively, borrowers could respond to increased debt by delaying the timing of their first home purchase. If the home purchase decision is delayed to a point in the life cycle at which the borrower has a greater demand for housing (due, for example, to the presence of children), mortgage balances could conceivably rise with student debt. In the same direction, student loan debt could affect the composition of the population of homeowners. That is, if marginal homeowners demand smaller mortgages than their inframarginal peers, then increased student loan debt would tend to increase the average observed mortgage as the marginal homeowners are selected out of the sample.

In the first column of Table 10, we present the results from regressing the 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 by age 32 are included in this regression. The estimated partial correlation is positive and statistically significant, implying a $1,000 increase in student loan debt is associated with approximately $330 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 substantially 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. While the point estimates suggest $1,000 in additional student loans reduces the expected first mortgage balance by about $3,200, we cannot rule out that the additional student loan debt actually increases the first mortgage balance by over $3,000.

5 Discussion of Findings

As we mentioned in Section 2.1, the average amount of student loan debt accumulated by 22 year olds increased by $3,300 between 2005 and 2014. How much of the decline in young people's homeownership rates over the same period can be attributed to this additional debt? In this section we provide a back-of-the-envelope extrapolation of our findings to the macro level.

To put the magnitude of the effects of increased student loan debt into a life cycle context,

Figure 5 plots the average age-profile of homeownership for young adults in 2005 (the black line).[42] The homeownership rate for these individuals rises sharply through young adulthood, from about 7 percent at age 22 to about 45 percent by age 32. For comparison, the red line simulates the homeownership rate under the counterfactual assumption that each individual is burdened with a $3,300 increase in student loan debts accumulated before age 23, using estimates from the specification of column 6 in Table 4.

Averaging across the ages 22 to 32, $3,300 additional student loan debt depresses the homeownership rate among young people by about 4.4 percentage points. The overall homeownership rate of this age group fell 9 percentage points, so this simple extrapolation would indicate that about half of the decline is due to increases in student loan debt. A number of caveats need to be kept in mind, however.

First, this exercise assumes an even distribution of the additional student loan debt across the population of young adults. In reality, the distribution of debt is quite skewed. Even in 2014, the majority of young adults had not taken any student loan debt at all before age 23, while the upper percentiles of student loan borrowing grew by far more than the mean of $3,300 between 2005 and 2014. The assumption that the increase in student loan debts was distributed evenly across the population exaggerates its estimated effects relative to the true, skewed distribution of the increase. This is because in a realistic (i.e. nonlinear) probability model, the marginal effect of debt on homeownership must decrease as the expected probability of homeownership approaches zero. In the appendix, we apply a more realistic distribution of student loan debts and find that only about 2 percentage points (20 percent) of the decline in homeownership among young people can be attributed to rising student loan debts. However, this exercise comes with further caveats of its own.

Second, we are assuming that the treatment effect estimated on public 4-year university students without any need-based aid can be extrapolated to the broader population. Young people who did not attend college, or who attended only 2-year schools, make up the majority of the control group and have lower homeownership rates than the treatment group. This may suggest that marginal home buyers are rarer in the general population than in our treatment group, and so the overall effect of an increase in debt may be exaggerated by

[42] As a reminder, the definition of homeownership we use in this paper is an absorbing state. Individuals who closed their mortgage account (either because they paid off the mortgage or were foreclosed on) are still counted as homeowners in our figures.

this extrapolation. The calculations in this section should therefore be considered an upper bound on the aggregate effect of student loan debt.

As an illustration of how students' relationship to debt may differ across sectors, note that trends in borrowing and enrollment behavior have differed markedly by institution type in recent years. For example, according to the BPS, the average student debt among 22 year olds whose first college was a public 4-year university increased by $4,700 in real terms between 2006 and 2014. Meanwhile, enrollment in this sector increased from 18 to 23 percent of the 22 year old population. In contrast, enrollment by age 22 increased only from 10 to 11 percent at private, non-profit 4-year schools, while average student loan debts increased $9,700 over the same period for this group. Attendance at public 2-year colleges also increased from 22 to 24 percent, but average debts remained essentially unchanged.[43]

Figure 5 also raises another possible interpretation of our results. Student loan debt may cause a delay in the timing of home buying, rather than a permanent reduction in the homeownership rate. In other words, increasing student loan debt may induce a rightward, rather than a downward, shift in the age-profile of homeownership. Interpolating linearly between the estimated points of the counterfactual homeownership curve, we calculate that with a $3,300 increase in student loan debt, the homeownership rate of a given cohort would be delayed by a little over one year at age 26. Due to the steepness of the homeownership-age profile during the early years of adult life, a fairly modest delay in the timing of home buying translates to a substantial decrease in the probability of homeownership at any particular age.

Even if student loans affect only the timing of home buying, with no effect on the ultimate attainment of homeownership, there are still significant aggregate implications. The overall homeownership rate would be lower than in a counterfactual world with less student loan debt, as each successive generation is delayed in becoming homeowners. Home equity is the major form of wealth holding for most households and housing services are a significant fraction of national income, so even a small change in homeownership can have wide ranging effects.[44]

[43] All figures are authors' calculations, based on data from the BPS and ACS.

[44] In the 2013 Survey of Income and Program Participation, the median homeowner household held over $80,000 in home equity. Housing services account for 15-18% of GDP according to the Bureau of Economic Analysis.

6 Conclusion

In summary, this paper estimates the effect of student loan debt on subsequent homeownership rates. We find that a $1,000 increase in student loan debt causes a 1 to 2 percentage point drop in the homeownership rate of student loan borrowers during their mid-twenties. These results represent a larger effect than estimates attempting to deal with the endogeneity of student loan debt using a selection-on-observables approach have found. We also show that student loan debt has a negative effect on borrowers' credit scores, potentially excluding some indebted students from the mortgage market.

What are the policy implications of our findings? If policymakers are interested in raising the homeownership rate among the young, our results suggest there may be additional value from promoting student loan forgiveness. Furthermore, policies directed at slowing the growth of tuition may aid student borrowers in becoming homeowners. As we show that damage to credit scores from delinquencies on student loans are a likely channel by which debts can affect homeownership, policies aimed at preventing delinquencies may also be beneficial. For example, income-driven repayment plans for student loans (such as the Income Based Repayment and Pay As You Earn programs offered by the Department of Education) which tie debtor's scheduled payments to their disposable income, may offer relief.

Additionally, one might be tempted to interpret our findings as evidence supporting a reduction in access to federal student debt, by—for example—lowering federal student loan limits. However, our analysis does not support such a conclusion. In particular, we do not estimate the effect of access to student loans, which could directly affect students schooling choices. If access to student loans allows for increased educational attainment, the reduction in access could lead to a wide array of negative outcomes, ranging from reduced economic efficiency to increasing income inequality within and across generations (Avery and Turner (2012)). Furthermore, by lowering incomes of young individuals, reducing access to student loans could even cause lower homeownership rates.[45]

In extrapolating our results to the present day, we also have to consider some significant recent changes to the mortgage market. Individuals in our sample turned 23 years old between 1997 and 2004. Thus, the majority of our cohorts were entering their prime

[45] A large body of literature has found that returns to education remain high and indeed continues to grow—see Lochner and Monge-Naranjo (2014) and studies cited therein.

home-buying years 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, credit scores, and low down payments. Alternatively, if there were fewer young marginal homeowners during and in the immediate aftermath of the Great Recession (whether due to unemployment reducing demand or the general inaccessibility of mortgage loans to anyone without pristine credit) the effects of student loan debt may have been muted relative to the bulk of our sample period. However, as the recovery continues and underwriting conditions ease, mortgage market conditions similar to the late 1990s and early 2000s may re-emerge. 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

Variables	Obs	Mean	Std. Dev.	Min	Max
Homeownership Rate					
Own at 22	33,435	0.068	0.251	0	1
Own at 23	33,435	0.100	0.301	0	1
Own at 24	33,435	0.143	0.351	0	1
Own at 25	33,435	0.195	0.396	0	1
Own at 26	33,435	0.243	0.429	0	1
Own at 27	33,435	0.289	0.453	0	1
Own at 28	33,435	0.332	0.471	0	1
Own at 29	33,435	0.369	0.482	0	1
Own at 30	33,435	0.401	0.490	0	1
Own at 31	33,435	0.424	0.494	0	1
Own at 32	33,435	0.445	0.497	0	1
Student Loan Debt Measures					
Student Loans Disbursed (in $1,000)	33,435	4.990	11.109	0	184.294
Student Loans Disbursed (in $1,000), Conditional on Debt>0	9,720	17.166	14.681	0.002	184.294
Tuition (in $1,000)	33,435	19.835	6.020	7.506	43.562
School Sector Controls					
Ever Public 4-Year	33,435	0.262	0.440	0	1
Ever Public 2-Year	33,435	0.248	0.432	0	1
Ever Private 4-Year Not-for-profit	33,435	0.116	0.320	0	1
Ever Private 2-Year Not-for-profit	33,435	0.008	0.087	0	1
Ever Private For-profit	33,435	0.047	0.211	0	1
Degree and Pell Grant Controls					
No College	33,435	0.458	0.498	0	1
Associate's/Certificate	33,435	0.030	0.171	0	1
Bachelor's	33,435	0.113	0.317	0	1
Master's or More	33,435	0.001	0.039	0	1
Degree of Unknown Type	33,435	0.008	0.088	0	1
Ever Pell	33,435	0.206	0.404	0	1
Cohort					
1990-91	33,435	0.045	0.207	0	1
1991-92	33,435	0.115	0.319	0	1
1992-93	33,435	0.113	0.317	0	1
1993-94	33,435	0.109	0.312	0	1
1994-95	33,435	0.113	0.316	0	1
1995-96	33,435	0.113	0.317	0	1
1996-97	33,435	0.113	0.316	0	1
1997-98	33,435	0.118	0.323	0	1
1998-99	33,435	0.108	0.310	0	1
1999-00	33,435	0.054	0.225	0	1
Yearly State Controls					
Average Weekly Wages (in $1,000, Home State)	33,435	1.026	0.170	0.783	1.792
Unemployment Rate (Home State)	33,435	5.015	1.135	2.300	8.770
House Price Index (Home State)	33,435	100.316	19.475	63.580	206.730
Additional Outcomes					
Mortgage Amount (in $1,000)	10,475	152.261	112.419	0.148	2,600.000
Ever Non-prime	33,435	0.739	0.439	0	1
Ever Subprime	33,435	0.610	0.488	0	1
Ever Delinquent on Student Loans	33,435	0.149	0.356	0	1
Ever Delinquent on Credit Card Debts or Auto Loans	33,435	0.203	0.402	0	1

Note: Homeownership rate is measured as ever having a mortgage loan by a given age. Student loans disbursed are measured as the total amount of federal student loans disbursed to individuals before age 23. Tuition is the average in-state tuition at public 4-year colleges in the individual's home state over the four years following his or her 18th birthday. Student loans and Tuition are in constant 2014 dollars. School sector, degree, and Pell Grant controls represent the sectors, the attained degree and whether individuals received Pell Grants before age 23. Cohorts are defined as the school-year in which individuals turn 18 years old. Yearly state controls represent local economic conditions in individuals' home state at age 22. Mortgage amount represents the size of the first mortgage amount observed in the dataset between ages 22 and 32. Ever non-prime and subprime represent whether individuals had scores that roughly correspond to FICO scores of 620 and 680, respectively, between the ages of 22 and 32. Ever delinquent represents whether individuals were delinquent on student loan debt, or in credit card debts or auto loans, for at least 90 days between the ages of 22 and 32.

Table 2: Selection on Observables

Probability of Homeownership by Age 26

Variable	**(1)**	**(2)**	**(3)**	**(4)**
Student Loans Disbursed	−0.000	−0.001***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Tuition	−0.001	−0.002	0.000	
	(0.001)	(0.001)	(0.003)	
Ever Public 4-Year	0.072	0.022***	0.016**	0.014**
	(0.006)	(0.006)	(0.007)	(0.007)
No College		−0.061***	−0.057***	−0.058***
		(0.009)	(0.009)	(0.009)
Associate's/Certificate		0.166***	0.162***	0.167***
		(0.029)	(0.028)	(0.028)
Bachelor's		0.185***	0.195***	0.199***
		(0.026)	(0.027)	(0.027)
Master's or More		0.269***	0.293***	0.289***
		(0.066)	(0.069)	(0.067)
Degree of Unknown Type		0.250***	0.245***	0.244***
		(0.048)	(0.046)	(0.046)
Ever Public 2-Year		−0.009	0.001	−0.001
		(0.009)	(0.008)	(0.008)
Ever Private 4-Year Not-for-Profit		−0.006	−0.001	−0.002
		(0.007)	(0.008)	(0.007)
Ever Private 2-Year Not-for-Profit		0.059**	0.056	0.062
		(0.029)	(0.039)	(0.038)
Ever Private For-Profit		−0.029***	−0.027***	−0.029***
		(0.011)	(0.010)	(0.010)
Ever Pell		−0.045***	−0.040***	−0.039***
		(0.008)	(0.007)	(0.007)
Observations	33,435	33,435	33,435	33,310
College Major Controls	NO	YES	YES	YES
Home State/Cohort FEs	NO	NO	YES	NO
Home State by Cohort FEs	NO	NO	NO	YES

Note: This table reports probit estimates of the effect of student loans on the probability of becoming a homeowner by age 26. Marginal probabilities (defined as the average marginal effect across individuals) are reported. Variables defined as in Table 1. Column (1) only controls for tuition and whether individuals ever attended a Public 4-year college before age 23. Column (2) adds several educational controls summarized in Table 1 and 14 college major indicator variables described in the Appendix of Mezza and Sommer (2016). Omitted degree category is having attended college before age 23 without getting a degree by that age. Column (3) adds home state and cohort fixed effects (FEs). Column (4) includes home state by cohort fixed effects. Sample is all individuals from a nationally-representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 3: IV Estimation: 1st Stage

Total Federal Student Loans Disbursed before Age 23

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Instrument: Tuition x Ever Public 4-Year	0.089*	0.158***	0.157***	0.157***	0.156***	0.202***	0.099**
	(0.048)	(0.040)	(0.039)	(0.039)	(0.040)	(0.040)	(0.046)
Tuition	0.173***	0.034***	0.053	0.025			
	(0.034)	(0.012)	(0.061)	(0.056)			
Ever Public 4-Year	5.555***	1.497**	1.548**	1.545**	1.553**	−0.333	2.498***
	(0.781)	(0.667)	(0.662)	(0.661)	(0.674)	(0.729)	(0.735)
No College		−2.103***	−2.066***	−2.064***	−2.078***	−2.866***	
		(0.352)	(0.345)	(0.345)	(0.344)	(0.404)	
Associate's/Certificate		−0.014	−0.067	−0.063	0.014	−0.823	−0.037
		(0.552)	(0.543)	(0.543)	(0.548)	(0.621)	(0.555)
Bachelor's		3.214***	3.261***	3.265***	3.331***	1.726***	3.335***
		(0.611)	(0.603)	(0.602)	(0.613)	(0.589)	(0.616)
Master's or More		4.061**	4.288**	4.282**	4.356**	2.579	4.417**
		(1.869)	(1.857)	(1.852)	(1.840)	(1.920)	(1.841)
Degree of Unknown Type		−0.093	−0.166	−0.153	−0.015	−0.801	0.001
		(0.874)	(0.877)	(0.881)	(0.872)	(1.202)	(0.860)
Ever Public 2-Year		−2.580***	−2.477***	−2.473***	−2.499***	−2.086***	−2.427***
		(0.262)	(0.261)	(0.261)	(0.259)	(0.325)	(0.262)
Ever Private 4-Year Not-for-profit		8.303***	8.303***	8.305***	8.294***	7.326***	8.199***
		(0.323)	(0.307)	(0.307)	(0.310)	(0.302)	(0.300)
Ever Private 2-Year Not-for-profit		1.867**	1.861**	1.872**	1.854**	2.683***	1.791**
		(0.854)	(0.850)	(0.850)	(0.857)	(0.983)	(0.833)
Ever Private For-profit		1.871***	1.945***	1.944***	1.938***	3.814***	1.936***
		(0.522)	(0.530)	(0.530)	(0.529)	(0.456)	(0.547)
Ever Pell		4.155***	4.120***	4.122***	4.109***		4.109***
		(0.218)	(0.220)	(0.220)	(0.221)		(0.234)
Avg. Weekly Wages (in $1,000, Home State)				−0.096			
				(0.266)			
Unemployment Rate (Home State)				−0.111			
				(0.071)			
Corelogic House Price Index (Home State)				−0.009			
				(0.006)			
Constant	−0.587	1.587***	0.942	3.537**	2.092***	2.865***	0.627
	(0.413)	(0.365)	(1.233)	(1.473)	(0.294)	(0.340)	(2.584)
College Major Controls	NO	YES	YES	YES	YES	YES	YES
Home State/Cohort FEs	NO	NO	YES	YES	NO	NO	NO
Home State by Cohort FEs	NO	NO	NO	NO	YES	YES	YES
Observations	33,435	33,435	33,435	33,435	33,310	26,399	17,927
F-stat	18.278	79.280	76.843	77.076	71.706	117.088	11.542
R-squared	0.138	0.379	0.384	0.384	0.363	0.311	0.261

Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. Columns (1)–(3) use the same specifications as in Table 2. Column (4) includes local economic controls measured at the home state level when individuals were 22 years old. Column (5) builds on column (3) by adding home state by cohort fixed effects. Column (6) repeats the analysis in Column (5) but restricts the sample to individuals who did not receive Pell Grants before age 23. Column (7) repeats the analysis in Column (5) but restricts the sample to individuals who attended any post-secondary schooling before turning 23. Sample is all individuals from a nationally-representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. Student loans disbursed and tuitions are recorded in 1000s of year 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 4: IV Estimation: 2nd Stage

Probability of Homeownership by Age 26

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Student Loans Disbursed	−0.023*	−0.016*	−0.013	−0.013	−0.013	−0.018*	−0.020*
	(0.014)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.012)
Tuition	0.004	−0.000	0.001	0.001			
	(0.003)	(0.001)	(0.003)	(0.003)			
Ever Public 4-Year	0.221***	0.081**	0.062*	0.061*	0.063*	0.062**	0.086*
	(0.078)	(0.033)	(0.035)	(0.035)	(0.037)	(0.028)	(0.046)
No College		−0.088***	−0.077***	−0.077***	−0.080***	−0.105***	
		(0.016)	(0.016)	(0.016)	(0.017)	(0.025)	
Associate's/Certificate		0.157***	0.156***	0.156***	0.161***	0.175***	0.145***
		(0.032)	(0.031)	(0.031)	(0.030)	(0.037)	(0.045)
Bachelor's		0.224***	0.226***	0.226***	0.233***	0.217***	0.236***
		(0.035)	(0.037)	(0.037)	(0.038)	(0.032)	(0.029)
Master's or More		0.314***	0.332***	0.331***	0.331***	0.320***	0.339***
		(0.080)	(0.082)	(0.082)	(0.080)	(0.071)	(0.083)
Degree of Unknown Type		0.236***	0.235***	0.235***	0.235***	0.276***	0.207***
		(0.052)	(0.048)	(0.048)	(0.048)	(0.058)	(0.070)
Ever Public 2-Year		−0.046**	−0.027	−0.027	−0.031	−0.034*	−0.047
		(0.018)	(0.020)	(0.020)	(0.021)	(0.018)	(0.029)
Ever Private 4-Year Not-for-profit		0.117*	0.093	0.093	0.099	0.114*	0.148
		(0.069)	(0.072)	(0.072)	(0.077)	(0.066)	(0.101)
Ever Private 2-Year Not-for-profit		0.085***	0.076*	0.076*	0.083**	0.123***	0.088**
		(0.031)	(0.041)	(0.041)	(0.042)	(0.045)	(0.041)
Ever Private For-profit		0.001	−0.003	−0.003	−0.004	0.057	0.007
		(0.022)	(0.022)	(0.022)	(0.024)	(0.041)	(0.035)
Ever Pell		0.019	0.008	0.008	0.012		0.041
		(0.037)	(0.037)	(0.037)	(0.039)		(0.059)
Avg. Weekly Wages (in $1,000, Home State)				0.014			
				(0.017)			
Unemployment Rate (Home State)				−0.000			
				(0.005)			
Corelogic House Price Index (Home State)				−0.000			
				(0.000)			
College Major Controls	NO	YES	YES	YES	YES	YES	YES
Home State/Cohort FEs	NO	NO	YES	YES	NO	NO	NO
Home State by Cohort FEs	NO	NO	NO	NO	YES	YES	YES
Observations	33,435	33,435	33,435	33,435	33,310	26,399	17,927

Note: This table reports second stage IV-Probit estimates of the effect of student loans on the probability of becoming a homeowner by age 26. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 3 for sample selection and specification details. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 5: Reduced Form Effect of Instrument on Homeownership

Probability of Homeownership by Age 26							
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Instrument: Tuition x Ever Public 4-Year	−0.002*	−0.003**	−0.002	−0.002	−0.002	−0.004**	−0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Tuition	0.000	−0.001	0.001	0.000			
	(0.001)	(0.001)	(0.003)	(0.004)			
Ever Public 4-Year	0.108***	0.060***	0.044*	0.044*	0.044*	0.071**	0.045*
	(0.024)	(0.023)	(0.023)	(0.023)	(0.023)	(0.030)	(0.026)
Degree/Sector/Pell Grant/College Major Controls	NO	YES	YES	YES	YES	YES	YES
Home State Economic Controls	NO	NO	NO	YES	NO	NO	NO
Home State/Cohort FEs	NO	NO	YES	YES	NO	NO	NO
Home State by Cohort FEs	NO	NO	NO	NO	YES	YES	YES
Observations	33,435	33,435	33,435	33,435	33,310	26,399	17,927

Note: This table reports probit estimates of the effect of the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23 on homeownership, measured at age 26. Marginal probabilities (defined as the average marginal effect across individuals) are reported. See Table 1 for variable definitions. Sample selection and specification details in Columns 1-7 are the same as in Table 3. Tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 6: First and Second Stage by Pell Grant Receipt

	Student Loans Disbursed		**Homeownership by Age 26**	
Variable	**With Pell (1)**	**Without Pell (2)**	**With Pell (3)**	**Without Pell (4)**
Instrument: Tuition x Ever Public 4-Year	0.097	0.202***		
	(0.090)	(0.040)		
Student Loans Disbursed			0.019	−0.018*
			(0.013)	(0.009)
Ever Public 4-Year	4.389***	−0.333	−0.106	0.062**
	(1.207)	(0.729)	(0.080)	(0.028)
Degree/Sector/Pell Grant/College Major Controls	YES	YES	YES	YES
Home State Economic Controls	NO	NO	NO	NO
Home State/Cohort FEs	NO	NO	NO	NO
Home State by Cohort FEs	YES	YES	YES	YES
Observations	6,594	26,399	6,594	26,399

Note: This table reports first and second stage IV-Probit estimates of the effect of student loans on the probability of becoming a homeowner by age 26. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. Marginal probabilities (defined as the average marginal effect across individuals) are reported. See Table 1 for variable definitions and Table 3, column 5 for sample selection and specification details. Columns 1 and 3 are restricted to individuals who received Pell Grant aid. Columns 2 and 4 are restricted to individuals who did not receive Pell Grant aid. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 7: Endogeneity of Education Outcomes

	Full Sample					College Attendees		
	Ever Public 4-Year (1)	**Any College (2)**	**Ever Public 2-Year (3)**	**Bachelor's (4)**	**Any Pell (5)**	**Ever Public 4-Year (6)**	**Bachelor's (7)**	**Any Pell (8)**
Tuition (Public 4-Year)	-0.001	-0.006		0.001	-0.000	0.001	0.003	0.002
	(0.003)	(0.007)		(0.002)	(0.004)	(0.008)	(0.003)	(0.003)
Tuition (Public 2-Year)			-0.021					
			(0.016)					
Home State/Cohort FEs	YES	YES	YES	YES	YES	YES	YES	YES
Observations	33,310	33,310	33,224	33,310	33,310	18,121	18,121	18,121

Note: This table reports probit estimates of the effect of tuition on several educational outcomes (denoted by the column headers). Marginal probabilities (defined as the average marginal effect across individuals) are shown. Tuition (Public 4-Year) is the average in-state tuition at public 4-year colleges in the individual's home state through the four years following his or her 18th birthday, while Tuition (Public 2-Year) is the average tuition at public 2-year colleges in the individual's home state through the two years following his or her 18th birthday. Student loans and tuition are in constant dollars of 2014. See Table 1 for other variable definitions. Sample in columns (1) through (5) is all individuals from a nationally-representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. The sample in columns (6) through (8) is restricted to individuals who have attended any post-secondary schooling before age 23. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 8: Effect of Tuition on Major Choice

Major Category	**Coef.**	**Std. Err.**
1	0.133	(0.153)
2	0.028	(0.051)
3	-0.010	(0.049)
4	0.088	(0.062)
5	0.147	(0.096)
6	0.092	(0.118)
7	-0.038	(0.023)
8	0.005	(0.060)
9	0.060	(0.066)
10	-0.043	(0.044)
11	0.122	(0.085)
12	0.246	(0.262)
13	0.258**	(0.122)
14	-0.107	(0.069)
15	0.041	(0.047)
Observations	8,774	

Note: This table reports multinomial logit estimates for the effect of tuition on major choice. Major categories are defined as described in the Appendix of Mezza and Sommer (2016) and the omitted category is having no degree (thus, no major) before age 23. Tuition is the average in-state tuition at public 4-year colleges from the four school years following the individuals' 18th birthday and is expressed in constant dollars of 2014. Sample is all individuals from a nationally-representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3 who have attended at least a public 4-year college before age 23. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 9: Probability of a Low Credit Score and Delinquency

	Subprime		**Non-prime**		**Ever 90 Days or More**			
					Student Loans		**Non-student Loan Debt**	
Variable	**All**	**No Pell Grant**	**All**	**No Pell Grant**	**All**	**No Pell Grant**	**All**	**No Pell Grant**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Student Loans Disbursed	0.018	0.019**	0.017*	0.017**	0.003	0.005**	-0.005	-0.002
	(0.011)	(0.008)	(0.009)	(0.008)	(0.003)	(0.002)	(0.007)	(0.006)
Ever Public 4-Year	-0.170***	-0.182***	-0.148***	-0.149***	0.012	-0.001	0.029	0.003
	(0.039)	(0.027)	(0.038)	(0.031)	(0.015)	(0.007)	(0.031)	(0.020)
Degree/Sector/Pell Grant/College Major Controls	YES	YES	YES	YES	YES	YES	YES	YES
Home State/Cohort FEs	NO	NO	NO	NO	NO	NO	NO	NO
Home State by Cohort FEs	YES	YES	YES	YES	YES	YES	YES	YES
Observations	33,397	26,431	33,410	26,509	32,330	23,550	32,823	25,816

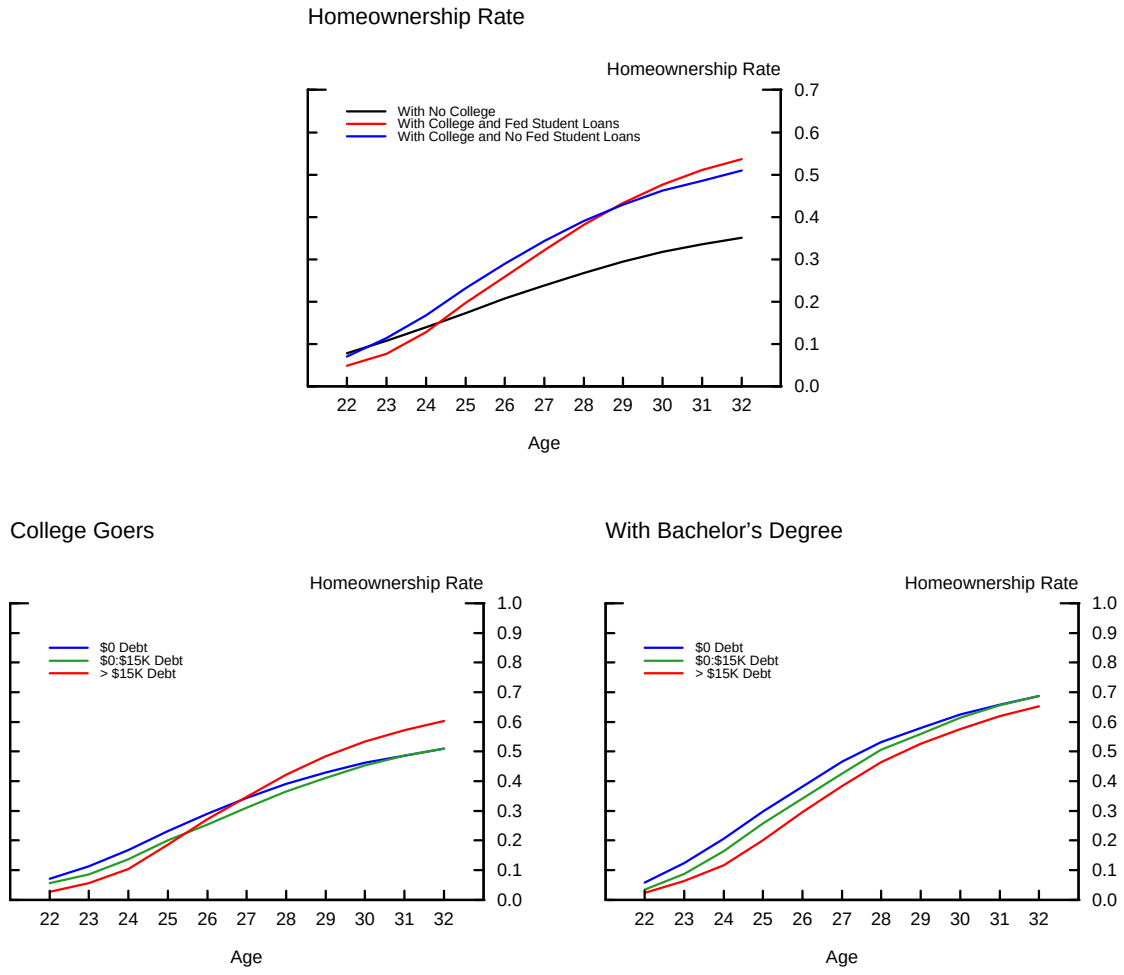
Note: This table reports IV-Probit estimates of the effect of student loans on the probability an individual is ever observed with a credit score that roughly corresponds to a FICO score of 620 (or a subprime score) between the ages of 22 and 26 in columns (1) and (2). In columns (3) and (4), the estimated effect of student loans on the probability of observing a credit score that roughly corresponds to a FICO score of 680 (or non-prime score) is reported. In columns (5) and (6), the estimated effect of student loans on the probability of being delinquent on a student loan debt for at least 90 days is reported. In columns (7) and (8), the estimated effect of student loans on the probability of being delinquent on credit card debts or auto loans for at least 90 days is reported. Odd columns report results for the whole sample. Even columns report results for the sample that had not received Pell Grant aid before age 23. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 and 3 for variable definitions and sample selection details. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 10: Dollar Value of Initial Mortgage

Variable	**(OLS)**	**(2SLS)**
Student Loans Disbursed	0.333***	-3.230
	(0.099)	(3.270)
Tuition	-4.754*	-4.289*
	(2.388)	(2.500)
Ever Public 4-Year	9.355**	24.440*
	(3.684)	(14.440)
Degree/Sector/Pell Grant/College Major Controls	YES	YES
Home State/Cohort FEs	YES	YES
Observations	10,475	10,475

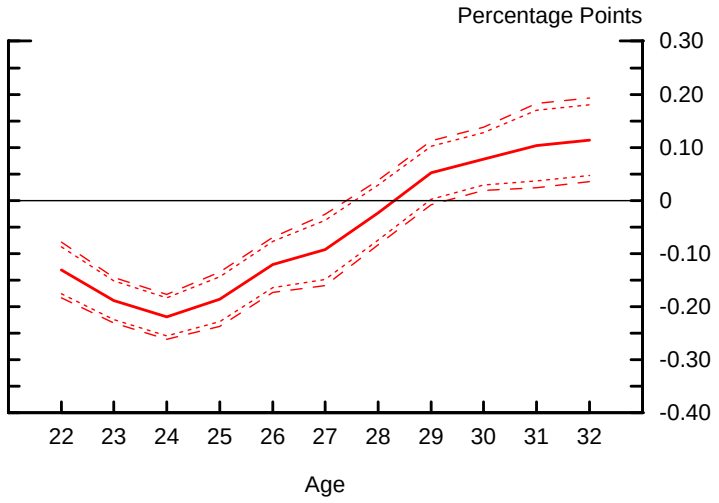
Note: This table reports OLS and 2SLS estimates of the effect of student loans on the first observed mortgage balance for individuals who opened their first mortgage tradeline between the ages of 22 and 32. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 and 3 for variable definitions. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Figure 1: Homeownership Rate by Age, Debt Level and Education



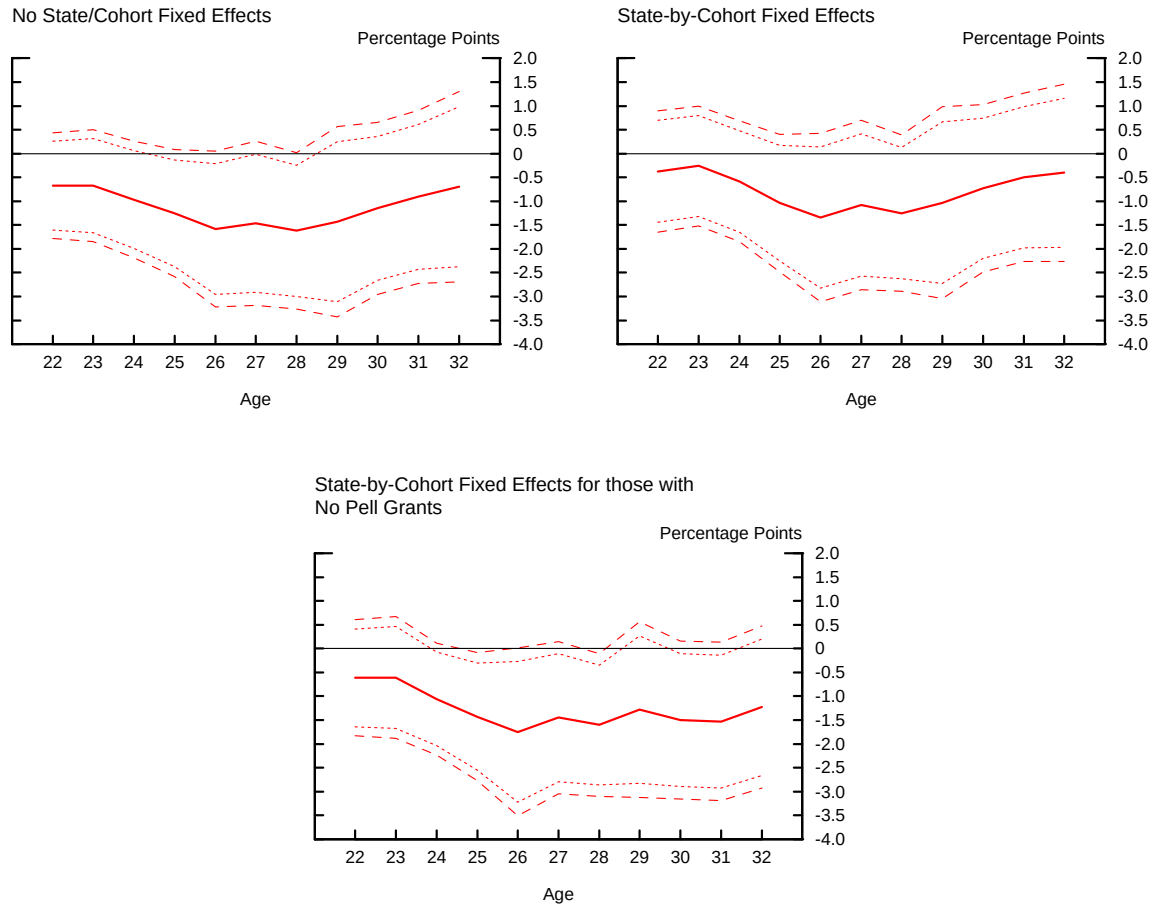
Note: College attendance and degree attained are defined based on whether individuals have attended college and obtained a degree before age 23, respectively. Student loan debt amounts reflect the amount of federal student loans disbursed before age 23. Homeownership rate at a given age is defined as ever having taken a mortgage by that age.

Figure 2: Marginal Effect of Student Loans on Access to Homeownership, by Age



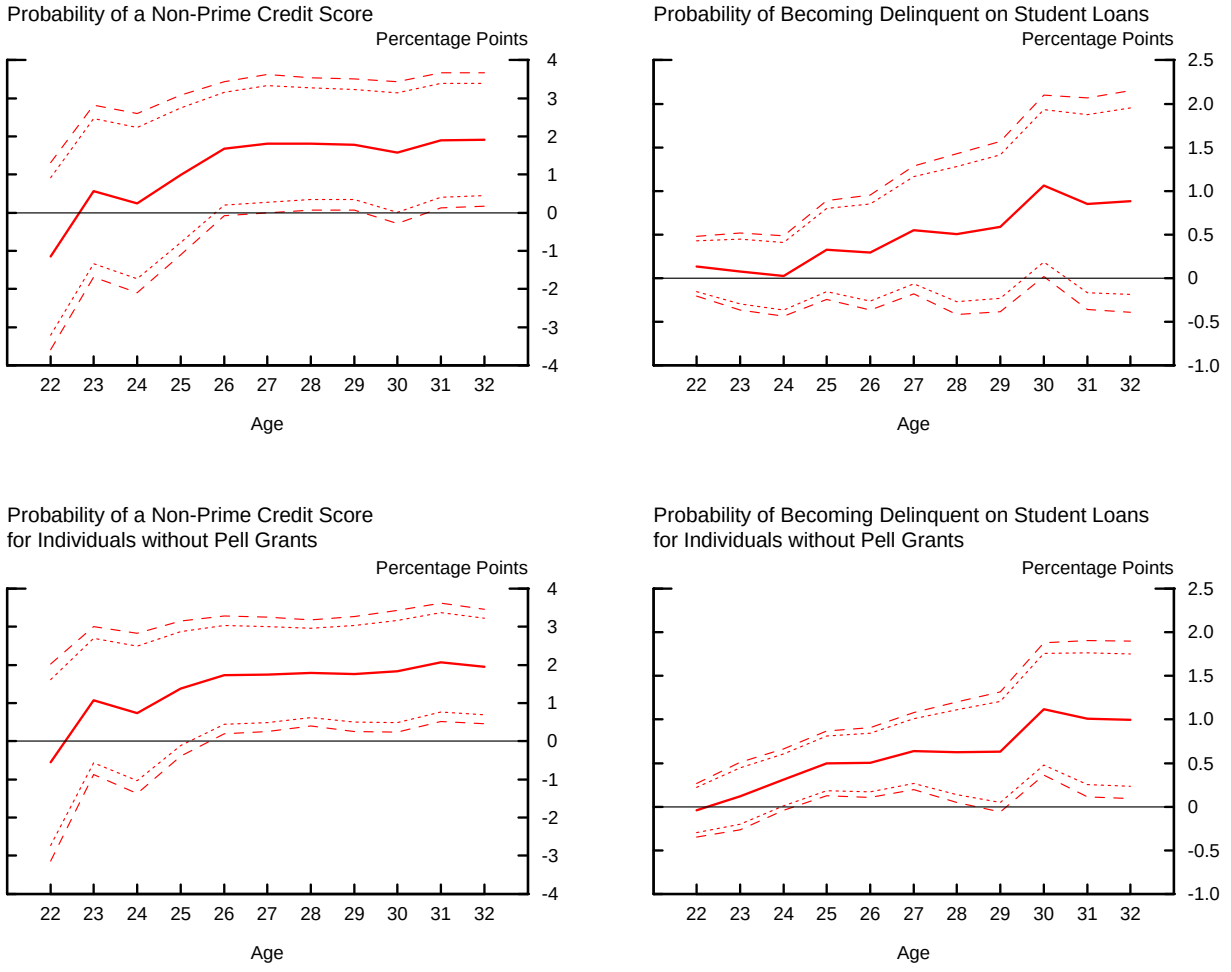
Note: This figure plots probit estimates of the marginal effect of student loan debt on the probability of becoming a homeowner against the borrower's age. These estimates are derived from the regressions using the vector of controls in columns 5 of Table 2. Dashed and dotted lines represent 95 and 90 percent confidence intervals, respectively. Standard errors adjusted for clustering at the home state level.

Figure 3: Estimates by Age



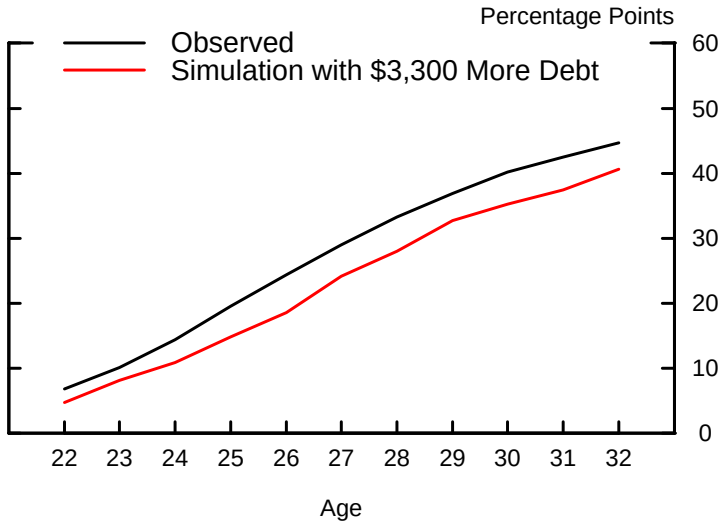
Note: This figure plots estimates of the marginal effect of $1,000 of student loan debt on the probability of becoming a homeowner against the borrower's age for three different specifications. These estimates are derived from the instrumental variable regressions using the vector of controls in column 2 (top left panel), column 5 (top right panel) and column 6 (bottom panel) of Table 4. Dashed and dotted lines represent 95 and 90 percent confidence intervals, respectively. Standard errors adjusted for clustering at the home state level.

Figure 4: Effect of Student Loans on Credit Outcomes



Note: This figure plots estimates of the marginal effect of student loan debt on the probability an individual is observed with a non-prime credit score (left panels) and had become delinquent on those loans (right panels) by a certain age. Non-prime refers to a TU credit score approximately corresponding to a FICO score of 680 or below. The bottom panels present estimates restricted to individuals who had not received any Pell Grant aid before age 23. The specifications correspond to columns 3 through 6 of Table 9. Dashed and dotted lines represent 95 and 90 percent confidence intervals, respectively. Standard errors adjusted for clustering at the home state level.

Figure 5: Observed and Simulated Homeownership Profiles



Note: This figure plots the average age profile of homeownership for our sample of young adults (the black line) and the simulated homeownership rates of this group if their debt levels were uniformly increased by $3,300 (the red line), according to the specification presented in column 6 of Table 4.

A Appendix

A.1 Variable Definitions

In this section we describe the construction and data sources for the variables not covered in Section 3.

School Sector: We construct a set of five non-mutually exclusive binary indicators capturing all school sectors with which an individual was before age 23: (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 (2016) 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.

Highest Degree Attained: We construct a set of six mutually exclusive binary indicators for the highest degree ever attained before age 23. We group degrees into the following categories: (1) no college, (2) dropouts (i.e, those with at least some college but no attained degree), (3) associate's or certificate degree holders, (4) bachelor's degree holders, and (5) holders of a master's degree or more. Moreover, for some individuals, we observe a degree has been attained, but have no information on the type of degree. In such instances, we assign individuals to the category (6) degree of unknown type.[46]

Major: College majors are available only for those with completed degrees. We aggregate them into 15 different categories, described in detail in Mezza and Sommer (2016). Only majors associated with degrees earned before age 23 are used.

Ever Pell Grants: This binary variable indicates whether the individual received Pell Grants to finance their post-secondary education before age 23.

Credit scores, balances on mortgages and other consumer loans, and 90+ dpd student

[46]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.

loan, auto loan and credit card delinquencies: These variables are sourced from TransUnion and are defined in Section 4.7.[47]

Unemployment rate, average weekly wages, and house price index at the state level: The unemployment rate is sourced from the yearly Local Area Unemployment Statistics series by the Bureau of Labor Statistics (BLS). The average weekly wages are sourced from the Quarterly Census of Employment and Wages by the BLS. Finally, the house price index is sourced from CoreLogic. All three variables are measured at the individual's home state in the year when the individual turned 22.

A.2 Effect of the Post-Crisis Rise in Student Loan Debts

In this appendix, we conduct an alternative extrapolation exercise to estimate how much of the 9 percentage point decline in homeownership among young people between 2005 and 2014 can be attributed to student loan debt. Unlike the extrapolation in Section 5, we attempt to match the actual distribution of student loan debts.

First, we note that the fraction of individuals who borrowed any student loans at all increased (a shift along the extensive margin) from 30 to 40 percent. Additionally, the amounts borrowed at the upper end of the distribution increased more rapidly than in the middle. While the median individual had no borrowing in both cohorts, the debt level of the 95^{th} percentile increased by about \$6,500 between 2005 and 2014. We pose our hypothetical exercise thusly: if our estimation sample cohort (aged 24-32 in 2005) had the age-22 debt profile of their peers from 2014, holding everything else equal, how much lower would their homeownership rate be? We take this difference as representing a rough approximation to the portion of the overall decline in homeownership among the young that can be imputed to the rise in student loan debt.

For any given individual, their observed characteristics $\mathbf{W}$, student loan debt X and the estimated coefficients from column 6 of Table 4 generate a predicted probability of homeownership by age t. Our counterfactual exercise involves replacing the observed values of X with those drawn from an equivalent cohort, 10 years later. To accomplish this, we use another representative draw of credit records provided by the CCP FRBNY/Equifax. From these data we construct the distribution of outstanding student loan balances persons aged

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

24-32 in 2014 held when they were 22 years old.

Next, we match percentiles of student loan debt across the two cohorts. For example, the 75^{th} percentile of debt was about $3,500 in 2005 and $7,500 in 2015. We then replace individuals observed debt levels with their matched values from 2015. Using the IV-Probit estimates from column 6 of Table 4, we simulate counterfactual homeownership probabilities for each individual using these new debt burdens. The age-specific model was chosen to match the observed age of the individual in 2005. The simulated homeownership rate is simply the average counterfactual probability of homeownership across the sample.

With 2014 debt levels, we calculate that the homeownership rate among our estimation sample would be approximately 2.1 percentage points lower than that observed in 2005 in the TransUnion data. In another representative draw of credit records provided by the CCP FRBNY/Equifax, the homeownership rate (measured as ever having a recorded mortgage loan) for people aged 24-32 fell from 29 percent in 2005 to 18 percent in 2014. Our rough extrapolation therefore implies that 20 percent of the decline in homeownership among the young can be attributed to their increased student debt burdens.

This calculation has the advantage over the exercise presented in Section 5 that it better matches the true distribution of student loan debts. As a result, we find a much smaller aggregate effect of the increase in debt. A large increase in the debts of a small number of individuals has less effect on the aggregate homeownership rate than a small increase in the debts of a large number of individuals for a given total increase in the aggregate debt balance. However, a further caveat applies to this extrapolation as well. Our framework estimates the effect that the amount of federal debt disbursed before age 23 has on the homeownership rate between 24 and 32. Unfortunately, we do not have a single data source that allows us to measure the amount of federal debt disbursed before age 23 for the cohorts who were 24 to 32 in 2005 and 2014. For the 2014 cohort, we therefore use data provided by the CCP FRBNY/Equifax. These data provide us loan balances at age 22, rather than total federal loans disbursed before age 23, so an inconsistent definition of debt may be introducing some error into our extrapolation.

Table 11: Effect of Student Loan Debt on Homeownership at Age 26, Robustness Check

	Explanatory Variables Recorded at Age 23		Explanatory Variables Recorded at Age 24	
	First 4 Years of Tuition as IV	First 6 Years of Tuition as IV	First 4 Years of Tuition as IV	First 6 Years of Tuition as IV
Variable	**(1)**	**(2)**	**(3)**	**(4)**
Student Loans Disbursed	-0.013	-0.011	-0.012	-0.010
	(0.009)	(0.009)	(0.009)	(0.009)
Ever Public 4-Year	0.059	0.052	0.060	0.049
	(0.044)	(0.044)	(0.053)	(0.052)
Degree/Sector/Pell Grant/College Major Controls	YES	YES	YES	YES
Home State/Cohort FEs	NO	NO	NO	NO
Home State by Cohort FEs	YES	YES	YES	YES
Observations	33,310	33,310	33,310	33,310

Note: This table reports first and second stage IV-Probit estimates of the effect of student loans on the probability of becoming a homeowner by age 26. In columns 1 and 2, student loans and all other explanatory variables are measured when the individual was 23. In columns 3 and 4 these variables are measured when the individual was 24. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college. In columns 1 and 3, tuition is measured as the average in-state tuition charged at public 4-year universities in the individual's home state, summed over the 4 years after they turned 18. In columns 2 and 4, the tuition measure is summed over the 6 years after turning 18. Marginal probabilities reported. See Tables 1 for variable definitions and 3 for sample selection and specification details. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

A.3 Robustness Checks

A.3.1 Alternative Age Cutoff for Variable Measurement

In this appendix, we check the robustness of our findings to a specification in which the explanatory variables are observed at ages 23 and 24 (instead of 22, as in our baseline results), and in which the tuition measure is computed over the first six years after the individual left high school (rather than the first four years). Results using the same set of controls as in column 5 of Table 4 are presented in Table 11.

A.3.2 Alternative Identification of Home State

In this appendix, we check the robustness of our findings to a specification in which the home state is identified from the TransUnion records only. The home state is identified as the first state in the credit bureau records with which the individual was ever associated. The sample is restricted to individuals who have a home state identifiable in this manner before age 23. Results for the first and second stage of the IV-Probit estimation are summarized in Tables 12 and 13.

Table 12: IV Estimation: First Stage (Alternative Identification of Home State)

Total Federal Student Loans Disbursed before Age 23

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Instrument: Tuition x Ever Public 4-Year	0.058	0.136***	0.133***	0.133***	0.129***	0.159***	0.073
	(0.050)	(0.040)	(0.040)	(0.040)	(0.040)	(0.039)	(0.045)
Tuition	0.193***	0.041***	0.063	0.035			
	(0.038)	(0.013)	(0.083)	(0.081)			
Ever Public 4-Year	5.961***	1.909***	1.977***	1.975***	2.076***	0.380	3.046***
	(0.785)	(0.677)	(0.681)	(0.681)	(0.683)	(0.693)	(0.758)
No College		-2.336***	-2.308***	-2.307***	-2.320***	-3.252***	
		(0.352)	(0.345)	(0.345)	(0.346)	(0.362)	
Associate's/Certificate		0.035	-0.044	-0.040	0.025	-0.653	-0.006
		(0.507)	(0.503)	(0.502)	(0.509)	(0.649)	(0.526)
Bachelor's		3.240***	3.254***	3.258***	3.342***	1.478*	3.297***
		(0.644)	(0.645)	(0.644)	(0.654)	(0.741)	(0.673)
Master's or More		4.232*	4.348*	4.345*	4.298**	2.009	4.237*
		(2.225)	(2.201)	(2.194)	(2.137)	(1.670)	(2.167)
Degree of Unknown Type		0.524	0.379	0.387	0.602	0.284	0.655
		(0.898)	(0.922)	(0.927)	(0.952)	(1.258)	(0.994)
Ever Public 2-Year		-2.671***	-2.579***	-2.577***	-2.588***	-2.376***	-2.514***
		(0.250)	(0.242)	(0.241)	(0.239)	(0.271)	(0.241)
Ever Private 4-Year Not-for-profit		8.198***	8.189***	8.191***	8.202***	7.305***	8.137***
		(0.310)	(0.301)	(0.301)	(0.298)	(0.317)	(0.282)
Ever Private 2-Year Not-for-profit		1.908***	1.950***	1.956***	1.860***	2.512***	1.749***
		(0.679)	(0.670)	(0.670)	(0.629)	(0.738)	(0.610)
Ever Private For-profit		1.601***	1.678***	1.677***	1.659***	3.411***	1.679***
		(0.512)	(0.517)	(0.518)	(0.515)	(0.461)	(0.540)
Ever Pell		4.128***	4.079***	4.080***	4.060***		4.040***
		(0.203)	(0.205)	(0.206)	(0.206)		(0.210)
Avg. Weekly Wages (in $1,000, Home State)				-0.000			
				(0.000)			
Unemployment Rate (Home State)				-0.125			
				(0.082)			
Corelogic House Price Index (Home State)				-0.008			
				(0.009)			
Constant	-0.543	1.706***	0.716	3.399	2.329***	3.242***	2.298***
	(0.450)	(0.353)	(1.780)	(2.573)	(0.306)	(0.299)	(0.288)
Observations	24,077	24,077	24,077	24,077	23,931	18,212	14,522
F-stat	5.290	39.689	37.750	37.864	34.393	45.951	4.770
R-squared	0.129	0.369	0.374	0.374	0.353	0.303	0.232

Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level for an alternative definition of home state derived from TransUnion records only. See Tables 1 for variable definitions and 3 for sample selection and specification details. Sample limited to individuals with an address recorded in TransUnion records before age 23. Student loans disbursed and tuitions are recorded in 1000s of year 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 13: IV Estimation: Second Stage (Alternative Identification of Home State)

Probability of Homeownership by Age 26

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Student Loans Disbursed	-0.031**	-0.022***	-0.018**	-0.018**	-0.020**	-0.027***	-0.026***
	(0.013)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Tuition	0.006**	0.000	0.002	0.001			
	(0.003)	(0.001)	(0.004)	(0.004)			
Ever Public 4-Year	0.248***	0.101***	0.082**	0.082**	0.088**	0.087***	0.112***
	(0.061)	(0.032)	(0.037)	(0.037)	(0.038)	(0.027)	(0.031)
No College		-0.090***	-0.081***	-0.081***	-0.085***	-0.124***	
		(0.020)	(0.022)	(0.022)	(0.022)	(0.031)	
Associate's/Certificate		0.151***	0.150***	0.150***	0.157***	0.165***	0.118***
		(0.033)	(0.031)	(0.031)	(0.031)	(0.041)	(0.045)
Bachelor's		0.241***	0.247***	0.247***	0.256***	0.222***	0.231***
		(0.030)	(0.033)	(0.033)	(0.033)	(0.031)	(0.031)
Master's or More		0.323***	0.348***	0.348***	0.355***	0.305***	0.319***
		(0.086)	(0.086)	(0.086)	(0.085)	(0.078)	(0.103)
Degree of Unknown Type		0.243***	0.245***	0.245***	0.250***	0.281***	0.196***
		(0.052)	(0.053)	(0.053)	(0.053)	(0.061)	(0.070)
Ever Public 2-Year		-0.060***	-0.037	-0.037	-0.043*	-0.051**	-0.060**
		(0.020)	(0.025)	(0.025)	(0.026)	(0.022)	(0.024)
Ever Private 4-Year Not-for-profit		0.161**	0.137*	0.137*	0.150*	0.183***	0.201***
		(0.068)	(0.078)	(0.078)	(0.080)	(0.064)	(0.069)
Ever Private 2-Year Not-for-profit		0.117***	0.115***	0.115***	0.114***	0.151***	0.105***
		(0.031)	(0.035)	(0.035)	(0.036)	(0.045)	(0.039)
Ever Private For-profit		0.003	0.002	0.002	0.003	0.089**	0.018
		(0.024)	(0.025)	(0.025)	(0.025)	(0.035)	(0.028)
Ever Pell		0.040	0.026	0.026	0.033		0.070
		(0.039)	(0.042)	(0.042)	(0.044)		(0.047)
Avg. Weekly Wages (in $1,000, Home State)				-0.000			
				(0.000)			
Unemployment Rate (Home State)				0.001			
				(0.005)			
Corelogic House Price Index (Home State)				-0.000			
				(0.000)			
College Major Controls	NO	YES	YES	YES	YES	YES	YES
Home State/Cohort FEs	NO	NO	YES	YES	NO	NO	NO
Home State by Cohort FEs	NO	NO	NO	NO	YES	YES	YES
Observations	24,077	24,077	24,077	24,077	23,931	18,212	14,522

Note: This table reports second stage IV-Probit estimates of the effect of student loans on the probability of becoming a homeowner by age 26 for an alternative definition of home state derived from TransUnion records only. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 3 for sample selection and specification details. Sample limited to individuals with an address recorded in TransUnion records before age 23. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table 14: IV Estimation: First Stage (Total Non-Housing Debt)

Total Non-Housing Debt Accumulated before Age 23

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Instrument: Tuition x Ever Public 4-Year	0.202***	0.259***	0.242***	0.243***	0.242***	0.353***	0.241***
	(0.067)	(0.064)	(0.066)	(0.066)	(0.067)	(0.079)	(0.068)
Tuition	0.109***	-0.015	-0.176	-0.220			
	(0.030)	(0.021)	(0.178)	(0.232)			
Ever Public 4-Year	3.253***	0.213	0.373	0.370	0.387	-2.124*	0.496
	(1.029)	(0.997)	(1.033)	(1.036)	(1.045)	(1.094)	(1.059)
Debt Available Only at Age 24	11.100***	9.926***	13.030***	13.030***	13.050***	13.860***	8.710***
	(0.278)	(0.310)	(0.281)	(0.281)	(0.282)	(0.392)	(0.309)
Has Debt but Value of Debt is Missing	-6.792***	-7.062***	-6.247***	-6.241***	-6.221***	-4.974***	-10.140***
	(0.226)	(0.224)	(0.207)	(0.204)	(0.207)	(0.176)	(0.367)
Degree/Sector/Pell Grant/College Major Controls	NO	YES	YES	YES	YES	YES	YES
Home State Economic Controls	NO	NO	NO	YES	NO	NO	NO
Home State/Cohort FEs	NO	NO	YES	YES	NO	NO	NO
Home State by Cohort FEs	NO	NO	NO	NO	YES	YES	YES
Observations	30,974	30,974	30,974	30,974	30,974	24,287	17,565
F-stat	22.415	38.522	33.735	33.745	32.843	45.345	5.696
R-squared	0.129	0.173	0.190	0.190	0.182	0.152	0.119

Note: This table reports first stage estimates of the effect of tuition on total non-housing debt accumulated at the individual level. See Tables 1 for variable definitions and 3 for sample selection and specification details. The variable "Debt Available Only at Age 24" is an indicator variable for credit records only being available for the individual at age 24. "Has debt but value of debt is missing" is an indicator variable for the amount of total debt not being reported in the individual's credit record. Sample limited to individuals with an observed credit record at age 23 or 24. Total non-housing debt accumulated before age 23 and tuitions are recorded in 1000s of year 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

A.3.3 Total Non-housing Debt

In this appendix, we present findings of the effect of all non-housing debt (rather than of student loans only) on homeownership. Total debt includes federal and private student loans, credit card balances and auto loans, as well as any accumulated interest on those debts.. Results for the first and second stage of the IV-Probit estimation are summarized in Tables 14 and 15.

A.3.4 Linear Probability Model

In this section, we check the robustness of our main findings to the choice of model. We recreate Figure 3 using a linear probability model of homeownership. The outcome variable, control variables, endogenous variable and instrument are all the same as in our main spec-

Table 15: IV Estimation: Second Stage (Total Non-housing Debt)

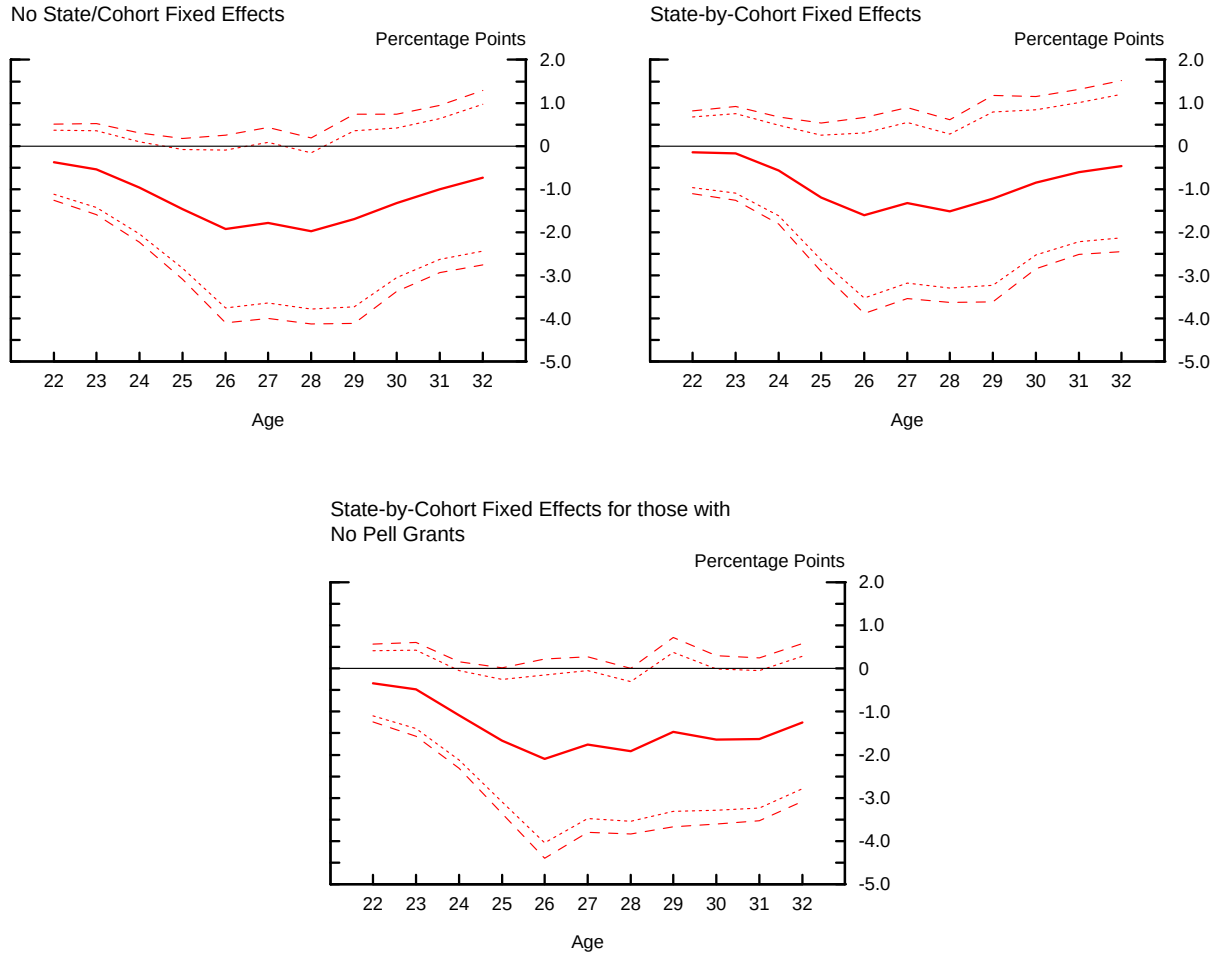
Probability of Homeownership by Age 26

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Student Loans Disbursed	-0.011	-0.011	-0.009	-0.009	-0.010	-0.012*	-0.017
	(0.010)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.016)
Tuition	0.001	-0.001	-0.002	-0.002			
	(0.001)	(0.001)	(0.003)	(0.003)			
Ever Public 4-Year	0.126**	0.065**	0.047	0.047	0.049	0.053*	0.078
	(0.064)	(0.032)	(0.033)	(0.033)	(0.035)	(0.028)	(0.066)
Debt Available Only at Age 24	0.206*	0.193***	0.221**	0.222**	0.234**	0.290***	0.276
	(0.109)	(0.071)	(0.095)	(0.095)	(0.101)	(0.094)	(0.204)
Has Debt but Value of Debt is Missing	-0.087	-0.080	-0.057	-0.056	-0.064	-0.038	-0.192
	(0.068)	(0.051)	(0.047)	(0.047)	(0.050)	(0.038)	(0.131)
Degree/Sector/Pell Grant/College Major Controls	NO	YES	YES	YES	YES	YES	YES
Home State Economic Controls	NO	NO	NO	YES	NO	NO	NO
Home State/Cohort FEs	NO	NO	YES	YES	NO	NO	NO
Home State by Cohort FEs	NO	NO	NO	NO	YES	YES	YES
Observations	30,974	30,974	30,974	30,974	30,974	24,287	17,565

Note: This table reports second stage IV-Probit estimates of the effect of total non-housing debt accumulated before age 23 on the probability of becoming a homeowner by age 26. Total non-housing debt is instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 3 for sample selection and specification details. "Debt Available Only at Age 24" is an indicator variable for credit records only being available for the individual at age 24. "Has debt but value of debt is missing" is an indicator variable for the amount of total debt not being reported in the individual's credit record. Sample limited to individuals with an observed credit record at age 23 or 24. Student loans disbursed and tuitions are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

ification. We estimate the model using two stage least squares. Results are presented in Figure 6. They are extremely similar to our findings using a nonlinear probability model.

Figure 6: Estimates by Age, Linear Probability Model



Note: This figure plots estimates of the marginal effect of $1,000 of student loan debt on the probability of becoming a homeowner against the borrower's age for three different specifications, using a linear probability model. These estimates are derived using the same instrumental variable and vectors of controls as in column 2 (top left panel), column 5 (top right panel) and column 6 (bottom panel) of Table 4. Dashed and dotted lines represent 95 and 90 percent confidence intervals, respectively.