The Housing Crisis and the Rise in Student Loans

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Abstract

The flow of new student loans increased by 50% between 2007 and 2010, and the amount borrowed per student rose by 30%. This shift occurred during the Financial Crisis, while credit markets were disrupted and home prices fell by about a third nationwide. We explore whether these two phenomena are linked, and in particular, whether the decline in access to home equity caused households to shift responsibility for education financing to students, in the form of loans. Student loans were one of the few forms of credit that remained accessible throughout the crisis. We estimate that for every dollar of home equity lost, households increased student loan debt by 20 to eighty cents. This substitution appears to be driven primarily by households with low levels of liquid assets. We extend our analysis using credit bureau data to trace longer-run effects of this leverage on students. Our results show that constrained households continued to enroll in college, but switched toward student loan financing. Our quantitative estimates suggest that the 30% average decline in house prices result in $500-1300 in additional borrowing per student on average, though the estimates are larger for liquidity-constrained and less-educated households. This channel explains 10-20% of the rise in student loan debt in our sample. We find evidence that this additional leverage affects the likelihood that a former student takes out a mortgage or an auto loan and suggests that at least part of the increase in student loans over the late 2000s represents a real transfer of the burden of paying for college from parents to students.

Keywords: Student loans, household finance, house prices, home equity credit, education.

JEL Codes: D12, D14, E21, E44, G20, I22, I24.

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1 Introduction

We study whether the dramatic changes in home prices over the 2000s affected how households financed college and how changes in how college is financed may have affected student outcomes. When house prices and credit supply were elevated, households were able to borrow against home equity and use relatively inexpensive mortgage debt to pay college tuition. As house prices collapsed and the financial crisis spread, households could no longer easily access home equity credit. In response parents may have shifted the burden of financing college enrollment to students through student loans. Consistent with this view, SallieMae [2013] reports a decline in the share of college costs covered by the students' families from 50% between 2003 and 2007 to 43% between 2007 and 2012. At the same time, student loans were the only type of consumer credit to increase throughout the financial crisis and recession. A shift in the financial burden of funding college from parents to students could have large ramifications for individuals' educational attainment, wealth accumulation, financial stability, entrepreneurship, and household formation (Ambrose, Cordell and Ma [2015], Bleemer, Brown, Lee and Van der Klaauw [2014], Brown and Caldwell [2013], Cooper and Wang [2014], Fos, Liberman and Yannelis [2017], and Rothstein and Rouse [2011]). Additionally, Eberly and Amromin [2016] argue that changes in who funds college enrollment, parents or students, can have important aggregate implications on savings and welfare. Given these potential effects, it is important to understand the extent to which the collapse in the access to home equity affected how much students had to borrow and whether or not the rise in student loans represents a real transfer in the burden of financing college.

Student loan borrowing increased significantly during the housing crisis and subsequent recession and much of this increase is unexplained. Figure 1 plots aggregate trends in house prices, enrollment, and student loans over the 2000s. Panel A shows that aggregate flows of federal student loans jumped by almost $30 billion per year from 2007 to 2010 as house prices fell. Some of this increase was likely due to the collapse in the private student loan market, but this market is too small to account for the total increase in federal loans. One potential explanation for this trend in aggregate student loan flows is the increase in enrollment over the 2000s, shown in the second panel. While enrollment rates increased by 4 to 6 percentage points from 2000 with much of this coming during the recession, panel B shows that average loan flows per student also increased sharply by about

$2,000. Another partial explanation is that federal loan limits increased to $31,000 for dependent students and $57,500 for independent students in 2008 (marked by the vertical red line in Panel B). However, average student loan borrowing was increasing significantly prior to this change in loan limits. Panel C reports median federal student loan balances upon repayment according to the type of school the student attended reported by Looney and Yannelis [2015]. These data show that typical borrowers are far from the federal loan limits when they enter repayment, and even those very far from the limits - such as students in two-year and non-selective institutions - increased their federal loan balances. Since the loan limit is unlikely to be binding for these borrowers, raising it cannot explain rising levels of student loan borrowing.

Another way to examine this increase in debt is to decompose the change in total student loan balances from 2006 to 2010 into a change in the number of students and a change in borrowing per student. If average debt balances were held to 2006 real values, the implied increase in borrowing due to enrollment would be $170 billion short of the actual aggregate debt balance of $790 billion in 2010. This ignores the potential impact of increases in tuition and net costs. However, the implied net cost of public school tuition only increased by about 2 percent annually after 2006 with costs at private non-profits essentially flat.[1] If we assume the average net cost increased at 2 percent, then the unexplained gap falls to about $120 billion in 2010 and $200 billion in 2011. Therefore, much of the increase in student loans is unexplained by either of these factors.

The increase in student loans took place at a point in time when student loans actually became more expensive relative to home equity. Figure 2 plots the interest rates on four types of household debt that might be used to finance college: subsidized and unsubsidized student loans, PLUS loans (parent student loans), and home mortgage debt. After 2005, the average interest rate on mortgage debt was lower than the rates on any kind of education loan except subsidized federal student loans, which are need-based and limited. While the choice between student or mortgage debt would also have to incorporate differences in default penalties and bequest motives, this suggests households with equity may have had access to a relatively inexpensive way to finance college during these years. However, there is no clear evidence that households substituted home equity for student loans or if

[1] See the College Board average net price trends `https://trends.collegeboard.org/college-pricing/figures-tables/average-net-price-over-time-full-time-students-sector`. Reliable for-profit figures are not available and those may have been higher than the two percent number used here, but as Panel C of Figure 1 shows growth in borrowing is almost uniform across types of school.

changes in access to home equity drove students to borrow more or alter their enrollment decision. Brown, Stein and Zafar [2015] use credit bureau data and find little evidence that declines in house prices and home equity borrowing caused households to take on more student debt, although they limit their analysis to the relationship between student loans and house prices in the same area.[2] Lovenheim [2011], Lovenheim and Reynolds [2013] and Stolper [2014] provide evidence that home equity affects both the intensive and extensive margins of college enrollment decisions, but it is not clear if this driven by wealth effects or liquidity and if declines in home equity access have symmetric effects. In general, a large literature has found conflicting evidence on the extent to which financial constraints affect student enrollment decisions (see Carneiro and Heckman [2002], Cameron and Taber [2004], Field [2009], and Stinebrickner and Stinebrickner [2008]). In contrast, Charles, Hurst and Notowidigdo [2015] showed that house price movements affected enrollment decisions through effects on labor markets and the opportunity cost of education.

Our paper focuses on how households respond to a shock to one particular type of college-financing, home equity credit, and the long-run implications of this shock. We do so by tracking the dynamics of debt between parents and students over time and how these dynamics vary with access to home equity. Our baseline analysis relies on data from the Panel Study of Income Dynamics (PSID) including the Transition to Adulthood Survey (TAS) supplement. These data have extensive information on household composition and balance sheets and, critically, they allow us to link changes in access to equity to that household's equity extraction, student loan debt, and college enrollment decisions. By observing these outcomes together we can determine if changes in access to home equity credit affect the extent to which student loans are used to finance college enrollment. To the best of our knowledge, this is the first time the links provided by the TAS data have been used to answer these questions. We supplement this work with individual-level credit bureau data from the New York Federal Reserve-Equifax Consumer Credit Panel (CCP) aggregated to form households and identify likely students. Because we only observe individuals if they have a credit report this sample is inherently selected, but it gives us a much larger sample and hence the ability to observe longer run outcomes for many more individuals than are available in the TAS data.

[2] Pence [2015] reports figures from the Michigan Survey of Consumers suggesting 3 percent of all households getting a cash-out refinance used the proceeds for education or medical expenses. Roughly 10 to 15 percent of homeowners have a child enrolled in college, so multiplying this number by 6 gives a rough estimate of 18% for the number of households financing college with home equity. We also provide an estimate using the PSID.

To identify the effects of access to home equity credit we exploit changes in household access to home equity coming from a household's prior mortgage obligations along with movements in local house prices to construct estimated loan-to-value ratios (our use of house price movements is similar to Lovenheim [2011]). We then compare households whose prior loan obligations combined with house price movements imply they are likely to be constrained in how much they can borrow to housholds unlikely to face this constraint. The primary advantage of this approach is that much of the variation in house prices over this period is likely to be outside of the control of households and so is appropriately considered a shock. Additionally, by studying these house price movements over the 2000s we exploit very large movements in home equity access in a period when home equity extraction was relatively common (Bhutta and Keys [2016], Greenspan and Kennedy [2008]). However, while it is plausible that changes in home values are outside the control of a household, it is still likely that they are correlated with other local factors that could affect enrollment or financing. Moreover, because we also use information about a household's past mortgage obligations it is possible that our results may also reflect household heterogeneity otherwise unrelated to home equity access. As a result, we check if our results are driven by an exhaustive set of potential confounders at the local and household level.

We first document that equity extraction is a relatively common way to fund college enrollment in our sample. After conditioning on a broad set of controls, including having a college-age household member, we find that households with a member enrolled in college are about three percentage points more likely to extract equity and take out over $3,000 of equity on average relative to households not enrolling a member in college. Conditional on extracting equity, we find that households extract an additional $15,000 of equity if they are enrolling a member in college. This is a sizable effect considering that in our sample households with a member enrolled in college have a 35 percentage points higher probability of having student loans and report a balance of about $5,000. These magnitudes suggest it is plausible that changes in home equity credit access might significantly affect the financing of college enrollment.

Our central result is that changes in the ability to extract home equity driven by house prices cause households to substitute between parents' mortgage debt and students' loans. Using only variation in home equity access, we estimate that for every dollar of home equity extracted to finance college enrollment a household borrows between twenty and fifty cents less in student loans.

This effect operates on both the extensive and intensive margins, if we allow the effect to vary non-linearly, and to an extensive set of controls including various levels of fixed effects. Specifically, we allow for the financing of enrollment to vary, cumulatively, with income quartiles by year, income growth quartiles by year, past-equity levels, local house-price growth, local unemployment changes, and non-parametrically by state and year. Strikingly, we find no evidence that changes in access to home equity affect the enrollment decision itself, but that households merely alter the way they finance enrollment.

Separating our effects by households likely to be liquidity constrained (Cooper [2013], Zeldes [1989]), we find that our results are primarily driven by households with low levels of liquid wealth. Liquidity-constrained households respond to increases in equity access by increasing equity extraction and reducing student loan borrowing, while households likely to be unconstrained do not respond to house price growth by extracting equity. While this suggests that liquidity constraints are the central mechanism driving the observed substitution, we also find that unconstrained households reduce their dependence on student loans as equity access increase. This behavior is consistent with households behaving in a buffer stock fashion (Carroll [1997]), where the increase in housing wealth allows households to divert liquid wealth to financing college instead of accumulating non-housing wealth. This suggests that changes in equity access affect student loan borrowing through both liquidity and wealth channels.

While we document the presence of substitution between home equity and student loans, the extent to which this is economically important depends on whether or not this substitution affects real outcomes for households. If the household behaves dynastically so that parents later assume the debt burden from the students, then the distribution of the debt between household members is largely irrelevant (notwithstanding differences in default behavior or interest rate differentials). To answer this question we rely on the CCP to examine longer-run effects of redistributing the financing burden between parents and students. We estimate how variation in student loans driven by declines in house prices and equity extraction affect the likelihood that a student takes out a mortgage and takes out a car loan, but we also use these data to examine if the pattern of substitution varies with the age of the student. Our preliminary results suggest that access to home equity does reduce student financial activity later in life, consistent with other literature. Critically, we also find that the substitution between home equity and student loans declines with the age of the (former) student. If a student is aged 23-27, then each additional dollar of parents' equity reduces student loans at 20% of the rate of students aged 18-22. This finding not only casts doubt on the fully dynastic model of household behavior, but it also suggests that at least some part of the increase in student loans over this period represents a real transfer of the obligation to finance college enrollment.

To summarize, we present the first evidence that households relied on home equity to fund college enrollment and so, when access to home equity fell, they turned to student loans. Our results suggest the degree of substitution was large enough that the deep decline in house prices is likely to have caused a significant shift in the financial burden of paying for college from parents to students. Our estimates imply that the effects of house prices and high leverage on equity access caused the average student to take out between $500-1,300 of additional student loans. This increase is on the order of about 10% of the median student loan balance in 2011 or to over 20% of the within-sample increase in the median student loan balance from 2007 to 2011. Since most households do not use home equity to finance enrollment, these results likely understate the size of the effect on households for whom this was a critical component of financing enrollment. Our estimate suggests that if parents were unable to extract any equity at all, then students took on about $28,000 more in student debt. This transfer between students and parents appears to be permanent and, because liquidity-constrained households were driving much of this substitution, the higher financing cost are likely to worsen financial constraints for the affected households.

2 Methodology and Data

Our aim is to understand how changes in access to home equity affects the way a household finances college and potentially enrollment itself. To identify shocks to home equity access we use a household's loan-to-value ratio assuming no change in mortgage debt two-years prior and estimating the household's current property value from local house price indexes. Specifically, for time t we construct $\widehat{LTV}_{i,t} = L_{i,t-2}/(V_{i,t-2} * g_{s,t,t-2})$.[3] Households with high levels of estimated LTV (for example, above one) will be generally unable to borrow against their homes because they will

[3]It is technically feasible to construct estimates of current household loan levels using amortization rates and information on interest rates, but the data on household loan characteristics are sufficiently noisy that this does no improve precision. As a result, we simply rely on lagged loan levels.

not have any equity while households with low LTVs will be more likely to access their equity. In this way we use variation in both house price dynamics across areas and household-level loadings on these house price dynamics as determined by pre-period leverage choices, similar to Bernstein [2015]. Later we discuss potential threats to identification, but in our baseline analysis we estimate the following types of difference-in-differences models

$$y_{it} = \alpha_{1i} + \beta_{1,1}1(\text{College Age})_{it} + \beta_{1,2}\widehat{LTV}_{i,t} + \beta_{1,3}1(\text{College Age})_{it} * \widehat{LTV}_{i,t} + \sigma_1 X_{it} + e_{1it}, \quad (1)$$

$$y_{it} = \alpha_{2i} + \beta_{2,1}1(\text{Enrolled})_{it} + \beta_{2,2}\widehat{LTV}_{i,t} + \beta_{2,3}1(\text{Enrolled})_{it} * \widehat{LTV}_{i,t} + \sigma_2 X_{it} + e_{2it}. \quad (2)$$

Let y_{it} be an outcome like the amount equity extracted for household i at time t. The variable $1(\text{College Age})_{it}$ indicates if the household has a member of college age (between, inclusively, 18 and 22) and $1(\text{Enrolled})_{it}$ is an indicator for whether or not the household has a non-parent member who has been enrolled in college within the last two years, and X is a vector of controls.

The coefficients $\beta_{n,3}$ are the parameters of interest as they measure how changes in equity access affect the relevant outcome for households with college-age or enrolled members. As is standard, we are assuming parallel trends between households enrolling members in college who have high and low estimated LTVs. Intuitively, in Equation 1 we compare two households with college age members, but one household experienced a decline in house prices that pushed them against the equity constraint while the other experienced an increase in home values that relaxed the constraint or had low levels of debt such that they remained unconstrained even when house prices declines. The coefficient $\beta_{1,2}$ nets out the effect of these differential equity constraints on households without college-age members while $\beta_{1,1}$ nets out the effects of having college-aged members. Similarly, Equation 2 compares two households enrolling a member in college who experienced different equity constraints. The extent to which movements in equity constraints are exogenous is critical to the validity of these estimates, but there are at least three important risks in this dimension. First, households may take actions to determine the value of their home, second, changes in the value of the home might be correlated with other shocks affecting the outcome of interest, and third, households with higher loan levels relative to property valuations may be more or less likely to rely on certain debt instruments. Large investments in houses or significant neglect do give households some dimension along which to alter the value of their home (Melzer [2010]). While we cannot control explicitly for these possibilities, we do observe if large (greater than $10,000) home improvements are correlated with the interaction and so can check if observed equity extraction is correlated with home improvements. A more problematic possibility is that changes in the value of a home are correlated with local or aggregate shocks to labor markets, particularly the health of the local construction sector. We check for this by including interactions of college age or enrollment with local conditions, particularly employment conditions and local house price growth. Finally, we address the possibility that household-heterogeneity correlated with estimated LTV is driving our results by including household fixed effects, which will absorb any constant variation, as well as interacting enrollment indicators with a variety of potentially important household-level confounding variables.

Even if house price movements are perfectly exogenous there is still a key question regarding the mechanism by which equity access affects behavior. In addition to effects on liquidity constraints, movements in house prices might also have a wealth effect that would not necessarily involve any actual extraction (Campbell and Cocco [2007], Carroll, Otsuka and Slacalek [2011]). We take an approach similar to Hurst and Stafford [2004] and exploit differences in responses between households likely to be liquidity constrained and households unlikely to be liquidity constrained to help distinguish between these mechanisms.

There are advantages and disadvantages to estimating the models set out above. While college enrollment is clearly endogenous, the model conditioning on enrollment (equation 2) is our preferred specification. Model 1 gives the average effect of LTV constraints net of any enrollment decision and so can help inform the extent to which house price movements affect enrollment. Since being of college age is almost certainly exogenous with respect to house price movements, the resulting estimates are not subject to endogeneity concerns along that dimension. However, not conditioning on the enrollment decision will bias our estimate of substitution towards zero because we include households who are not financing college enrollment. By estimating model 2, we only use information on households who actually have to finance college enrollment. However, if selection into enrollment does respond to house prices or leverage then the estimated effects from model 2 will reflect this selection bias. For example, if only very wealthy households continue to enroll in college as house prices fall and they finance college with non-housing wealth, we might estimate a complementarity between equity and student loans as all types of debt-financing fall. This is somewhat related to a broader concern that households enrolling a member in college differ along some other dimension that potentially interacts with house price movements or leverage (for example, wealth). To the extent those concerns can be addressed by controlling for household-level covariates, the estimate conditional on enrollment will answer the question of how households enrolling members in college relied on student loans as equity access changes.

The coefficients from these models provide useful evidence on the effects of house prices on households financing college enrollment, but we are also interested in the direct substitution between home equity credit and student loans. So we provide instrumental variable estimates from the following model where we use the enrollment indicator interacted with estimated LTV as an instrument for equity extraction

$$\begin{aligned} \text{EquityExtracted}_{it} &= \alpha_{1i} + \gamma_1 1(\text{Enrollment})_{it} * \%\Delta HP_{it} + \sigma_1 X_{1it} + e_{1it} \\ \text{StudentLoans}_{it} &= \alpha_{2i} + \beta \text{EquityExtracted}_{it} + \gamma_2 1(\text{Enrollment})_{it} + \eta \%\Delta HP_{it} + \sigma_2 X_{2it} + e_{2it}, \quad (3) \end{aligned}$$

The coefficient β gives how much a change in an additional dollar of equity affects the dollar amount of student loans.[4] The exclusion restriction for this model is that estimated LTV does not affect households enrolling members in college except through effects on equity extraction. It is important to note that this does not mechanically imply that β should be equal to negative one. The exclusion restriction does not preclude margins other than student loans from also adjusting to the change in home equity access. For example, households might choose to go to a less expensive school, work more, or draw down other savings. So long as these other changes are driven by access to home equity credit, β gives a consistent estimate of the dollar rate of substitution between home equity and student loans net of other margins of adjustment. This allows us to answer the question of whether or not the boom and bust in home equity borrowing drove some of the rise in student loans, even after households respond to the shock along other available margins.

[4]This coefficient is also given as the ratio of the coefficients from the difference-in-differences models on equity and student loan quantities.

2.1 Data

We rely on two distinct datasets for our analysis. We use data from the Panel Survey of Income Dynamics (PSID) for our baseline results. The PSID is particularly useful because its longitudinal structure lets us observe if a household contains a college-age member, college enrollment decisions (from 2005 onwards), and the household balance sheet including measures of equity extraction and student loans.

While the PSID data allow us to examine the basic mechanisms we are interested in, it is limited by a relatively small sample and low frequency (biennial in the years studied). This limits our ability to track student outcomes for longer horizons. Consequently, we extend our analysis with the New York Federal Reserve-Equifax Consumer Credit Panel (CCP). These data are a very large random sample of individuals with credit records and have been used extensively to study household debt behavior in recent years. While the CCP data are not constructed to measure household outcomes, we have the advantage of replicating the estimates in the PSID in order to quantify the extent of measurement error that we introduce.

2.1.1 Panel Survey of Income Dynamics

We restrict the PSID sample to homeowners with the same head of household for at least six surveys from 1999 to 2013. This helps reduce noise by ensuring households are seasoned enough to have children and provide enough variation to include a household-levle fixed effect. We also exclude any households that were renting or households that move from one owned home to another within the last two survey periods (four years). This structure provides us with a sufficiently large number of continuous observations so that we can absorb household fixed effects, which takes care of a large amount of potentially important but unobserved heterogeneity. As a result of these exclusions we are left with a sample of approximately 1,750 households.

Along with information from the baseline individual and family files, we import data from the Supplemental Wealth Files and the Transition into Adulthood Survey (TAS), which is only available from 2005 onwards. The TAS supplement interviews members of a PSID household who are at least 18 years old and who also participated in the Child Development Supplement (up to two children per family were initially covered). These data provide critical information on whether or not a child who left the household went to college, took on student debt, and other related outcomes. Prior to the TAS, college enrollment could only be inferred if the student lived at home or once they formed a new household (Lovenheim [2011]). The TAS fills this critical gap in coverage as over 50% of PSID children do not form a household covered by the PSID by the time they turn 24.[5] Students living away from home in college dormitories were recorded as "institutionalized" with no information about their borrowing behavior recorded. The TAS data allow us to correct for all of these gaps in coverage.

Tables 1 and 2 provide summary statistics for our sample in each year that we use for estimation. We do not weight our estimates as we are interested in estimating causal effects and applying the PSID longitudinal weights does not affect our estimates other than slightly reducing the precision (Solon, Haider and Wooldridge [2015]). We aggregate all quantities to the household level so that multiple members enrolled in college only count once, but if each member borrows some in student loans the entire amount across both students is counted. While this introduces some measurement error when interpreting the indicator variables it simplifies the estimation framework. Unless otherwise noted all quantities are in thousands of real dollars, adjusted by the PCE deflator with 2009 as the base year.

Because of the structure the age of the household head gradually increases and household size declines. About 20% of households have a college age member, which we define as having a member between 18 and 22. This number declines as the sample continues and households age. We record an individual as enrolled if they claim to be currently enrolled in college or have been enrolled in college in the last two years. Between 7 and 14% of households report having a member enrolled in college and roughly 59% of households with a college-age member have someone enrolled in college, similar to enrollment numbers reported in Lovenheim [2011]. We do not count college enrollment of non-traditional students such as parents or older adults. Between 2 and 8% of households report having a member with a student loan and, conditional on having a student loan, the median household reports a balance of $14,000 in 2009.

The median gross household income is $70,000, consistent with the sample being composed of homeowners. The median household is carrying about $45,000 in mortgage debt in 2001 and this declines to $37,000 by 2013, but the range is around $110,000. Households tend to carry

[5]See the user guide to the TAS `https://psidonline.isr.umich.edu/CDS/TA05-UserGuide.pdf`.

moderate sums of liquid (cash, savings and checking accounts, stocks and bonds) and illiquid (vehicle, retirement, and secondary real estate) wealth at $15,000 and $30,000 respectively. The median household starts the sample with about $90,000 of home equity, which peaks at about $110,000 in 2007 and then falls to $97,000 by 2013. Self-reported LTVs are relatively low ranging from a a median of 28 to 32. However, estimated LTVs are higher and range from 32 to 37 at the median household. While households might make mistakes with their self-reported homevalues, Lovenheim [2011] documents that they do not significantly differ from the FHFA repeat sale index for the years 1980 to 2005. Even if households do consistently make mistakes, for a household to extract equity they must at some point have an accurate idea of their home's current value if only from the loan officer. By relying on self-reported values for the initial homevalue we rely on information shared by the household, which should improve precision and the plausibility.

Given these high levels of home equity it is plausible that households might extract equity in response to large liquidity shocks such as financing college. While the PSID collects some information about equity loans and HELOCs it does not explicitly address if a household extracted equity through refinancing nor does it directly ask about the amount of equity extracted or its usage. To measure equity extraction, Cooper [2010] defines an equity extraction event as when a household either (1) increases its mortgage debt and does not move or (2) reduces its equity and does move. The resulting quantity of equity extracted is then the respective difference in debt or equity. We adopt this definition except we exclude equity extraction through moving and we require the household's current loans to contain at least one refinanced loan or some type of home equity loan. According to this definition between 10 and 22% of households in our sample extract equity with the median amount of equity extracted between $20-$30,000 across the years. These numbers are similar to those reported in Cooper [2010] for a different set of PSID households for the years 1999 to 2007, but they are smaller than the numbers reported by Bhutta and Keys [2016]. Since the frequency of our data is significantly lower it is plausible that our numbers are somewhat attenuated measurement error. Finally, the boom and bust in house prices are clearly visible in the self-reported home values. From 2007 to 2011 the median house price fell about 20% with a very large dispersion. Interestingly, the median household reports negative four-year growth even up to 2013, which diverges from aggregate house price indexes.

2.1.2 Consumer Credit Panel

The Federal Reserve Bank of New Yorks Consumer Credit Panel (CCP) is a longitudinal dataset of key fields from individual credit reports. The dataset is comprised of a 5% random sample of individuals with valid credit files collected by Equifax Inc. The individuals are drawn into the sample if their Social Security numbers end in one of five pre-determined digit pairs. Each quarter, Equifax Inc. provides data on these individuals liabilities and payment status, as reported to the credit bureau. These randomly selected individuals represent the primary sample of CCP households.[6] Lee and van der Klaauw (2010) provide an extensive summary of CCP sample design.

Importantly for our study design, the CCP is not limited to the primary sample. The dataset attempts to construct household identifiers by linking credit reports of each primary individual with all other reports associated with the same physical billing address in a given quarter. For instance, a 50-year old individual in the primary sample has the same billing address as another 52-year old and 18-year old individuals in a given quarter. These three individual borrowers are then assigned to the same household identifier. This linkage substantially increases the overall CCP dataset as it brings in individuals beyond the 5% primary sample. Crucially, it also allows one to form a fuller picture of household-level liabilities and potential reallocation of these liabilities within households over time.

However, constructing household-level debt dynamics is quite challenging. The household ID that is assigned to household members in a given quarter is not time invariant, although individual borrower IDs are permanent. Identifying a household over time thus requires proceeding from quarter to quarter, pooling together all records that share a household ID with the primary member and then assessing whether this association is real or spurious. For instance, a student-aged household member that moves out to attend college may change their billing address to that of their college dorm and appear with dozens of other household members sharing this address. In practice, we use the following algorithm to construct our sample.

Another key problem with CCP is that individuals that are associated with the primary sample member are tracked only for as long as they are estimated to reside in the same household. Thus,

[6]The panel of primary CCP households is refreshed each quarter. That is, each quarter starting in 1999:Q1 all households with the five pre-determined SSN digit pairs are drawn into the sample. This assures that deceased individual exit the sample and individuals with newly established credit files enter the sample.

if the primary sample member is an adult, we would observe their non-primary student only while they share the same billing address. Conversely, if the primary member is a student, we observe their parents only for part of the students credit bureau timeline. Since we are interested in tracking long-term outcomes of both parents and students, we restrict our sample to those households in which both the adult- and the student-aged members are a part of the primary sample.

We start by randomly selecting 20% of all primary credit records in the CCP for which the borrower is between the ages of 18 and 22 in 2005:Q1. We then pull all additional credit records that are ever associated with the primary student borrower by virtue of sharing the same household ID in a given quarter. These records are then jointly assigned a time-invariant household ID keyed off each primary borrower. This produces a sample of 106,326 households.

We then flag instances where there is a college-aged primary person (ages 18-22) and an adult-aged primary person (ages 40-65) in the household. For such "double-primary" households, we identify other non-primary adults that are observed for at least as many quarters. Doing so allows us to track non-primary adults (e.g. spouse of the primary adult) as well. These individuals are together designated as the "core household". For many analyses, only the debt associated with members in this core are included in aggregate household debt portfolios. By construction, each core household consists of a primary student borrower, primary adult borrower and possibly one or more non-primary adult borrowers. There are 6,736 double-primary households in the 2005 draw cohort.

To build a sample that covers the entire range of housing market experiences, we then draw another sample of primary student borrowers between the ages of 18 and 22 in 2006:Q1. After eliminating those primary students who were already sampled in 2005, we repeat the steps above. We follow the same process for all of the remaining years in 2005-2015 period. Altogether, we identify 54,915 double-primary core households.

Finally, we supplement the double-primary households with a control sample of CCP households. For each of the double-primary (treated) households, we find a primary-adult household of the same age that did not have student-age household members for at least a 5-year period centered on sample entry year. To illustrate, this algorithm would pair a double-primary sample household with a 55-year-old adult and a 20-year-old student who was drawn into the sample in 2005 with a household that contains a 55-year-old primary adult and that had no student-aged members between 2003 and 2007. This does not guarantee that these households do not have students, but it likely reduces this chance.

3 Empirical Results

In this section we first characterize how households in our PSID depend on home equity and student loans to finance college enrollment. We then quantify how variation in estimated equity constraints affect substitution between home equity and student loans and, then we check for robustness. Finally, we explore the role of liquid wealth and estimate the effects of additional student loan debt on long-run student outcomes.

3.1 Financing College Enrollment

Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, and year fixed effects. All specifications also include either state or household fixed effects as noted in the table. We restrict the sample to the years 2005 onwards, which is when the TAS data are available. All standard errors are clustered at the state level.

Table 3 reports estimates of how college enrollment is related to student loans and equity extraction. Column 1 reports that a household with a college-age member is about 41 percentage points more likely to have a member enrolled in college. This is consistent with estimates of the current fraction of 18- to 24-year olds enrolled in college over this time that range between 37 to 41 percent.[7] Column 2 shows that households with members enrolled in college are almost three percentage points more likely to extract equity relative to households without a member enrolled in college. Column 3 checks that this is actually driven by enrollment by controlling for the presence of college-age members. Households with college-age members who do not enroll in college are not more likely to extract equity and the estimated effect of enrollment on equity extraction is essentially unchanged. Columns 4 and 5 report estimates of the dollar amount of equity extracted as a function of enrolling in college and so reflect both the intensive and extensive margins. Both estimates suggest households extract about $2,800-3,400 more equity on average,

[7] Digest of Education Statistics 2013 `http://nces.ed.gov/programs/digest/d13/ch_3.asp`.

with none of this effect coming from households with college-age members not enrolling a member in college. Column 6 examines the intensive margin of equity extraction directly by including an indicator for equity extraction and interacting this with enrollment. This coefficient then recovers how much more equity households extract to finance enrollment conditional on already extracting equity. Households that are both extracting equity and enrolling a member in college withdraw almost $16,000 more equity, a significant effect relative to the average of $41,000.

Columns 7-10 report the relationships between the enrollment decision and student loans in the same fashion. Households enrolling a member in college are about 35 percentage points more likely to report a student loan and carry about $5,500 more in student loan debt. These estimates show that the households in the sample rely on both home equity and student loans to finance college enrollment. Consistent with Pence [2015], equity borrowing is less frequently used, but when utilized the quantities are large enough to be a primary form of financing enrollment for those households. Since both types of financing are prevalent in the data and in comparable quantities, some margin for substitution between the types of credit is possible.

We now turn to testing if access to equity affects the way households finance college enrollment conditional on actually enrolling a member in college. Later we examine the enrollment decision itself, and Table A1 reports specifications conditioning only on the household having a member of college age with qualitatively similar results. We expect that as access to equity increases households will rely more on equity extraction to finance college enrollment given its relatively low cost. In turn, this might result in households relying less on student loans.

Table 4 reports our difference-in-difference estimates where we interact estimated LTV with the indicator for the household enrolling a member in college. We include household-level fixed effects to check the robustness of the results to household unobservables. Column 1 examines the probability of equity extraction and show that a household enrolling a member in college is less likely to extract equity as estimated LTV increases: moving the estimated LTV from zero to one reduces the probability of equity extraction by about 10 percentage points. This effect is about 15% of the average effect of estimated LTV. Column 2 households with college-enrolled members do extract significantly more equity as access to equity increases. For these households the same increase in estimated LTV results in $16,000 more dollars of extracted equity on average, about 30% of the effect of estimated LTV on the average household. These regressions suggest that when households are faced with financing college enrollment, additional equity available leads to a greater reliance on equity extraction.

In contrast to the results on equity extraction, columns 3 and 4 show that households with a member enrolled in college are more likely to rely on student loans as access to equity declines. The magnitudes are slightly larger than those for the probability of equity extraction: increasing estimated LTV from zero to one increases the probability of a student loan by about 20 percentage points, but the balance of student loans increases by about $4,500. However, the relative effect is smaller given the higher average propensity to carry student loans. These results show that movements in house prices affect how households finance college enrollment. As the amount of equity available to households increases, households are more likely to depend on equity extraction and less likely to rely on student loans and the differences are economically large.

Comparing columns 2 and 4 suggest2 a rate of substitution on the order of -0.25, but to get a direct estimate we turn to the instrumental variable model (equation 3). In the first stage regression we instrument for the amount of equity extracted with the interaction of college enrollment and estimated LTV. We then regress the balance of student loans reported on the instrumented amount of equity extraction. Thus, the estimated coefficient on equity extraction will only pick up movements in student loans correlated with movements in equity extraction driven by access to equity. This reduces the chance that pure wealth effects not resulting in equity extraction are driving our results. Column 5 in Table 3 regresses the level of student loan debt on the amount of equity extracted without instrumenting and recovers a precise zero relationship. This is expected as most equity extraction is unrelated to financing education. Column 6 isolates the variation in equity extraction driven by estimated LTV and households enrolling a member in college. Consistent with the difference-in-difference estimates, we find an economically and statistically significant negative relationship between equity extraction and student loan debt of -0.27. Column 7 breaks out estimated LTV into quantiles to account for any non-linearity in the effects and uses the interaction of these quantiles with enrollment as instruments recovers an identical estimate. Column 8 uses only four-year changes in household-level homevalue growth interacted with enrollment as an instrument and recovers a negative, larger rate of substitution. Finally, column 9 uses the continuous measure of estimated LTV interacted with enrollment as an instrument for the probability of equity extraction. This estimate suggests that households that do not extract equity at all due to constraints will carry an additional $28,000 of student loans.

That this coefficient is not exactly negative one could result from several factors. First, complications in measuring equity extraction relative to student loan balances might reduce the apparent substitution due to mis-measurement. Focusing on substitution between the flow of equity extracted and the balance of student loans helps to alleviate this concern, but may not solve it entirely. Second, households are capable of adjusting along margins other than student loans as they respond to the change in equity. These margins include not enrolling in college, enrolling in a cheaper university, having the student work, relying on household savings, and alternative types of debt. We examine these additional outcomes below, but the central takeaway from Table 3 is that even with these alternative responses, each lost dollar of home equity translates into a substantial amount of student loan debt, particularly for households that would have relied on equity entirely.

These estimates provide direct evidence that the collapse in access to home equity increased the amount of student debt used to finance college enrollment. However, the distribution of this effect is far from uniform across households. Not all students come from parents that own their homes, not all homeowners finance college enrollment with equity, and potentially not all equity extracted when a household has a member enrolled is used to finance college. In our sample the average household extracting equity while enrolling a member in college in 2007 withdrew about $56,000 of equity. If all of the equity was intended to finance college our estimates imply this would result in the student borrowing $14–28,000 in student loans, enough to push a student well above the median debt load. To get a sense of the size of our effect within our sample, we calculate for each household the change in equity extraction driven by equity constraints and enrollment from our reduced form estimate and then multiply this number by the substitution estimate. Our estimates imply the average household in our sample enrolling a member in college in 2011 carried an additional $600-1,200 in student loans, which is about 13-25% of the average increase within the sample between 2009 and 2011.

We can also provide an aggregate back-of-the-envelope calculation using the aggregate changes in equity extractions and the fraction of students likely to be financing college with home equity. According to the Department of Education, an average of 20 million students were enrolled in college for each academic year 2009-2010 and 2010-2011. At the same time equity extraction fell by over 60% (Bhutta and Keys [2016]). According to the TAS data, about 70% of households that enrolled a member in college in our sample are homeowners and according to the Department of Education between 60 and 70% of students enrolled in college match the age range of our TAS sample.[8] If we apply the 60% decline in equity extraction along with our estimated rates of substitution ranging from -0.25 to -0.5 (our baseline estimate) to the 20 million students times 0.7 (homeowners) times 0.65 (appropriate age), times average equity extraction levels of $3,000 (across households that extract and do not extract), implies that across these two years households extracted $18 billion less in equity, which then resulted in an additional $5-9 billion dollars in student loan balances. Across these same years we calculate that the increase in student loans not explained by enrollment or cost increases totals about $127 billion dollars. Therefore, our estimates imply the collapse in house prices was responsible for about 4-7% of the aggregate increase in student loan balances from 2009 to 2011.

3.1.1 Robustness and Liquidity

Before turning to the role of liquid wealth, we address a broad set of robustness concerns. First, because LTV constraints are non-linear by construction, it is possible that only allowing LTV to affect outcomes linearly is critically mis-specified. While the instrumented effect in column Table 3 gives similar results, it is important to examine the reduced forms separately. Figure 3 reports the estimates from the difference-in-differences with estimated LTV broken out by quartile for both the probabilities of each type of financing (in the left panel) and the dollar amounts (in the right panel). As households move to the high-LTV range the likelihood of extracting equity drops sharply while the likelihood of taking out a student loan increases sharply. While the probabilities trade off at almost the same rates, the dollar amounts reflect the less than perfect rate of substitution between equity and student loans.

Tables 5, 6, 7, and 8 evaluate each reduced form outcome separately with a broad set of robustness checks. In each table the first column reports the baseline non-parametric result. Column 2 includes quartiles of household income interacted with enrollment to address the possibility of LTV being correlated with overall income levels. Column 3 interacts the enrollment-quartile effects with year fixed effects to allow for the effect of income to vary by year. Column 4 interacts enrollment with an indicator for a household having less than $10,000 in equity four years prior to

[8] `http://nces.ed.gov/programs/digest/d09/tables/dt09_191.asp`

account for constrained households having generally different borrowing behavior. Column 5 interacts enrollment with quartiles of income growth and year fixed effects to account for households that are more constrained potentially being more likely to be unemployed or experience negative income shocks. Column 6 includes an interaction of enrollment with two-year state-level house price growth to allow for the possibility that LTV constraints are reflecting broader house price shocks. Column 7 interacts enrollment with changes in state-level unemployment rates. Finally, column 8 interacts enrollment with state-year fixed effects to allow for financing behavior to vary non-parametrically with state-specific trends such as tuition shocks, labor market shocks not captured by unemployment, or other trends.

Across all of the outcomes our results are robust qualitatively and quantitatively. The only caveat is that the probability of equity extraction loses significance at standard levels, but the point estimates are economically large and consistent across controls, suggesting the loss of significance is simply due to a lack of precision. Comparing the quantity estimates in the reduced forms also gives consistent rates of substitution between equity and student loans on the order of -0.2. Overall, these results show that the effects of LTV constraints are extremely robust and not driven by other forms of borrower heterogeneity or local shocks.

Given that the results are robust to an extensive set of potential confounders, we turn to examing the mechanism. Economic theory and empirical evidence suggest that movements in house prices and home equity access are likely to be more important for households that are liquidity constrained, unless wealth effects are very large (Cooper [2013], Zeldes [1989]). Following this literature, we examine if households with at least $150,000 in liquid wealth in the previous survey (placing them in the top decile of households) react differentially to being constrained in LTV. We then re-estimate our basic difference-in-difference regressions but now including a third difference between households likely to be constrained and households likely to be unconstrained. If liquidity constraints are driving our results then we expect constrained households to drive the substitution while households unconstrained are less responsive.

Table 9 reports the estimated effects for equity extraction and student loan debt with the additional interaction. While the precision of the results varies, we find that the two types of households respond differently to changes in equity access. In column 1 unconstrained households essentially do not respond to being constrained. Turning to the dollar amount of equity extracted households likely to be "constrained" reduce equity extraction substantially while unconstrained households, despite generally having more valuable houses on average. In contrast, both households respond strongly to higher estimated LTVs by borrowing more in student loans, although unconstrained households are less likely to rely on student loans in general. Overall, this is consistent with evidence in Hurst and Stafford [2004] and Cooper [2013] that changes in credit access are more important for households that are otherwise liquidity constrained. But we also find that unconstrained households respond to being constrained by LTV by turning to student loans, but relaxing this constraint does not seem to lead to substantially more equity extraction.

One possibility is that equity levels have a wealth effect on unconstrained households that makes them more likely to finance college enrollment with alternative types of wealth. This type of behavior would be consistent with a buffer stock model of household savings motives (Carroll [1997]) where the additional housing wealth provides households with a pool of potentially liquid wealth accessible in the event of a negative shock. This might then allow households to reduce their rate of saving in order to finance enrollment.

Overall, the liquidity difference suggest that movements in equity access drive a trade-off between equity extraction and student loans for constrained households, but that house price growth also affects student loan borrowing for unconstrained households through a potential wealth effect. This points to a potential "hierarchy" of financing sources where constrained households largely rely on student loans, the relatively more expensive but generally accessible option, unless the household has sufficient equity. Wealthier households rely substantially less on student loans in general, and when house prices increase they cut their use even further. However, instead of substituting into home equity, these households may reduce savings or alternative consumption directly, consistent with the increase in equity providing a useful buffer stock in the event of surprise shocks.

3.1.2 Additional Outcomes

Given that our results appear to be driven by the effect of access to home equity, we now turn to additional outcomes that might be also be affected or additional margins of adjustment other than student loans. We first shed some light on these additional responses and other possible uses for the extracted equity in Table 10 using the PSID data. Column 1 reports the effects on the probability that a household with a college-age member actually enrolls a member in college. We see essentially no effect on the likelihood of enrollment for constrained households, suggesting we are not picking up the mechanism in Charles et al. [2015]. In column 2 we find the probability that an enrolled student takes out a credit card has a credit card or other loan. While this probability seems to decline slightly with constraints, the effect is relatively small and statistically insignificant. Column 3 examines the probability that the student reports working or looking for work with no significant effect.

We also examine how three potential uses of home equity covary with enrollment and estimated LTV as additional checks on the plausibility of the mechanism. Column 4 looks at the probability that the parents in the household cover some fraction of a student's tuition. Over half of the households with a member enrolled in college pay some portion of the student's tuition with the average amount of tuition covered annually at about $12,000 (unreported). Consistent with home equity financing college enrollment, households enrolling a member in college are less likely to cover tuition as estimated LTV increases, where moving estimated LTV from zero to one reduces the likelihood of support by over 20 percentage points. Column 5 checks the probability that the student has taken out a personal loan from their immediate family or relatives and finds no significant result, although this event is quite rare with households enrolling a member in college reporting only a four percentage points higher probability of using such a loan and the effect of LTV on the same order. Finally, we check if households are more likely to undertake large home improvement projects as LTV constraints vary.[9] If households took advantage of a member leaving the household to renovate their home then we might find the same relationship between estimated LTV, enrollment, and equity extraction but the causality would run in the opposite direction of the one we propose. Column 6 shows that this does not happen. In general there is a slightly lower probability of home improvements when a household enrolls a member in college (insignificant) and these households seem to respond even more to estimated LTV. If households had been either using increased equity to finance improvements or generating increased home value with home improvements we would expect the interaction term to be positive and significant. These results suggest the two actions are unrelated.

The long-term panel structure of CCP data allows us to examine the effect of student loan

[9] To measure home improvement we rely on the definition used by the PSID, which asks if the household has undertaken any additions or improvements to the home of at least $10,000.

borrowing driven by fluctuations in house prices on a variety of economic outcomes for both students and their parents.[10] Before examining these long-run outcomes, we first report the CCP-based estimate of the rate of substitution using the same regression approach taken in the PSID data where we instrument for equity extraction with house price growth interacted with the presence of a student. Table 11 reports these estimates in columns 1 and shows an even higher rate of substitutions, potentially due to the selection of households in the CCP where households must have a credit report. The second column re-estimates the rate of substitution, but now for households where the college-age member is between the ages of 23 and 27 and finds the rate of substitution to be substantially smaller. This suggests that households are not fully dynastic and that students who happen to enter college while their parents are constrained become responsible for bearing more of the burden of financing enrollment. While the baseline estimates are larger than the PSID results, these results confirm that households do substitute between these types of financing. Given the robustness of this substitution result in the CCP, we turn to using movements in student loans driven by house prices to understand how student leverage might affect long-run outcomes. This is done by first computing the cumulative change in HPI in the parents zip during the period their student was between the ages of 18 and 22 and then using this to estimate changes in student loans while controlling for a home county fixed effect. We then regress outcomes of interest on this quantity.

The first long-term outcome of our analysis is the propensity of students to become homeowners by the time they turn 30. To measure this, we flag all of the primary students in our sample who turn 30 by 2016 and whose credit bureau record indicates having a residential mortgage at any point in time. We drop any student who has not turned 30 by 2015, which reduces the size of the sample significantly. This measure of home ownership is then regressed on the instrumented value of student loans, controlling for indicators of other debt types (auto, credit cards) and student age. The regressions also control for local housing demand by including MSA dummies for student residence at age 30. The results, shown in the third column of Table 11 suggest that student loans indeed have a measurable effect on home ownership. A $10,000 HPI-driven increase in student loans decreases the likelihood of home ownership in early adulthood by 5.7 percentage points. This is a

[10] We are currently transporting our analysis to the CHRISM data merge which will allow us to exploit the same estimated LTV strategy.

sizable effect relative to the baseline ownership rate of 36% and consistent with results in Brown et al. [2015]. We also find large negative effects on the likelihood a student has an auto loan with the same increase in student loans reducing auto loan likelihood by 6.5 percentage points.

Overall, these results paint a consistent picture. Some households use home equity credit to help finance students' college enrollment, so when access to home equity falls some students take out more debt in order to continue financing enrollment. We find results consistent with this shift in financing reflecting a real transfer of the obligation from parents to students with older students receiving less support when parents extract equity. We find that this has some real effects on the student's propensity to have a mortgage and an auto loan later in life.

4 Conclusion

Using household-level panel data that allow us to observe outcomes for parents and children, we evaluate the effects of access to home equity credit on student loan debt and college enrollment. We find that as parents are unable to borrow against home equity, they push the burden of financing college enrollment onto students through student loans. The magnitude of substitution that we estimate is large: for each dollar of home equity credit that parents do not take out students borrow between 25 and 80 cents. These effects are strongest for households that are liquidity constrained, although we do find evidence that unconstrained households also reduce dependence on student loans, likely due to a wealth effect and buffer stock behavior. We find little evidence that access to equity affects the college enrollment itself, suggesting that the liquid student loan market helped buffer any real effects on human capital accumulation over the crisis.

Our results show that the collapse in house prices over the late 2000s contributed to a significant intergenerational shift in the financial burden of paying for college. This shift could have far-reaching consequences for housing market activity, and we find that leveraged students do appear to be less likely to have a mortgage or an auto loan. Given the very large increase in student loans over this period, the implications of the shift in the burden of paying for college may be large and long-running.

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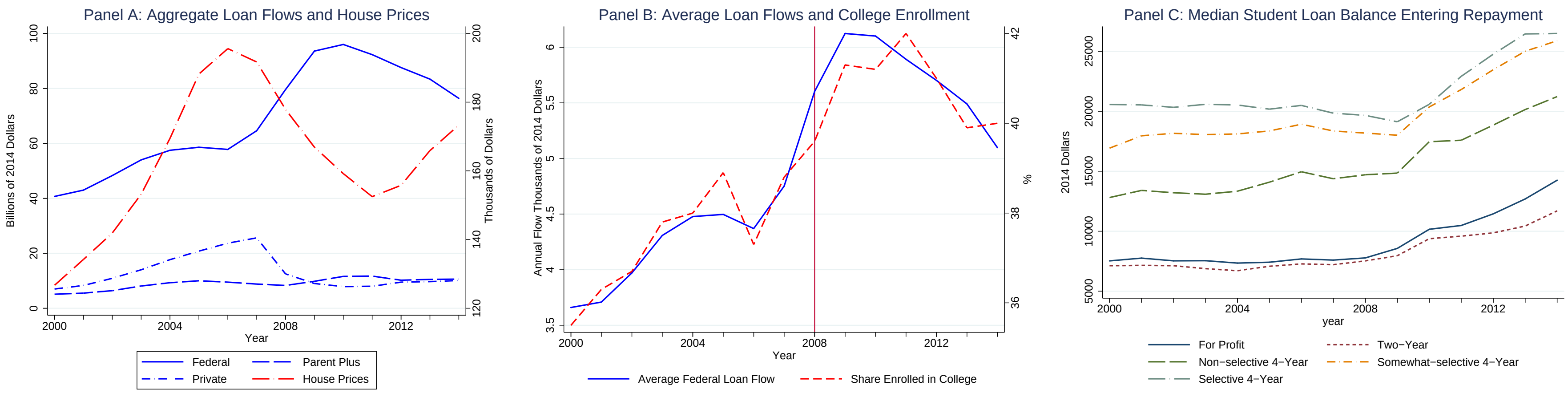
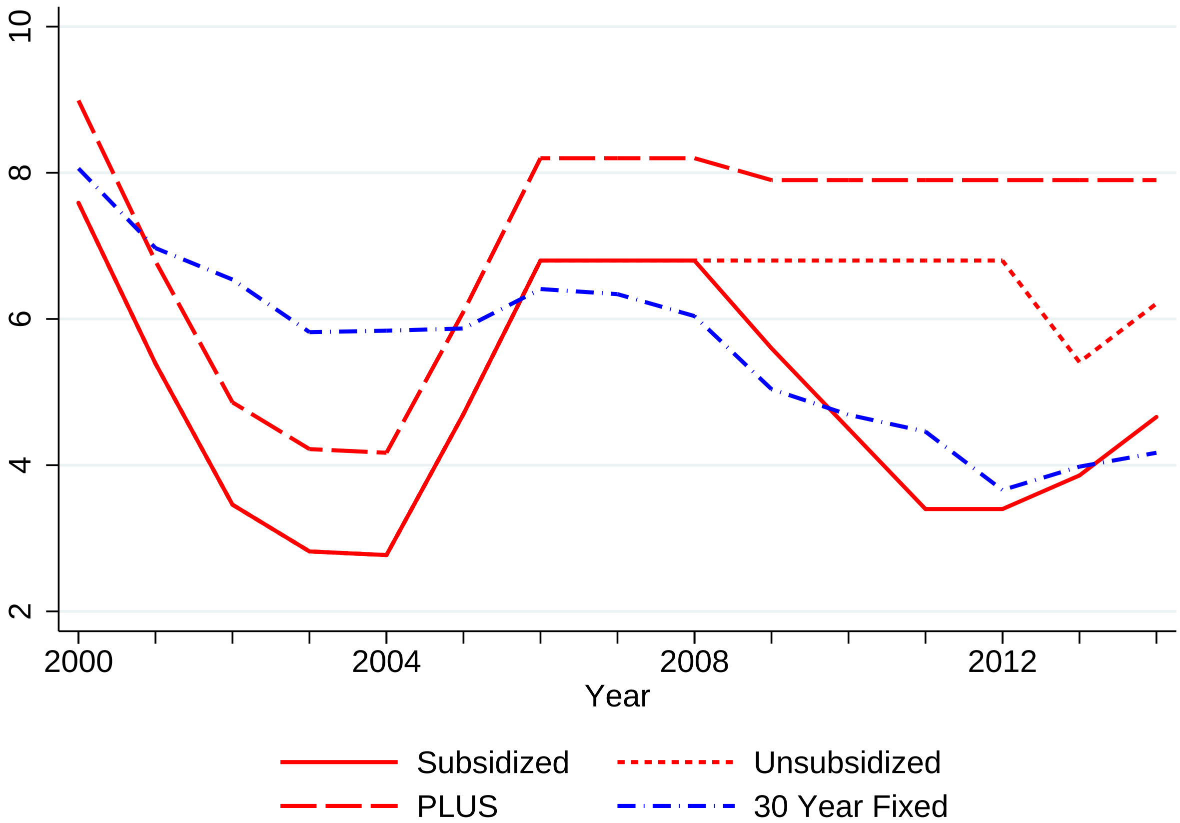
5 Figures

Figure 1: Student Loans and the Housing Crisis



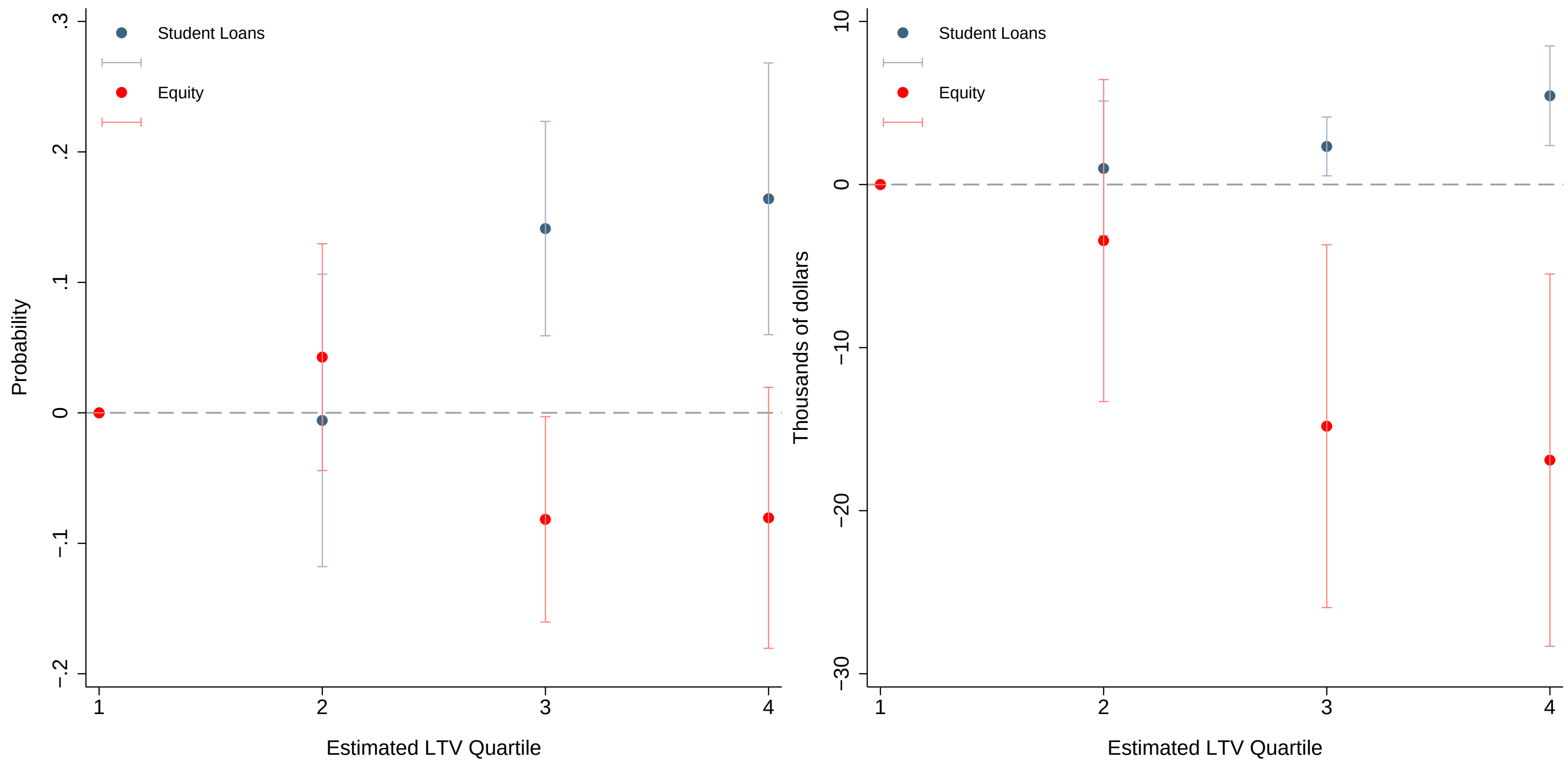
Note: This figure gives plots of trends in student loans, house prices, and enrollment to show that the increase in student debt has been large and broad based. The left figure plots the aggregate annual flow of total federal subsidized and unsubsidized loans, parent PLUS loans, non-federal loans, and the national hosue price index from Zillow. The figure shows a sharp increase in the flow of federal student loans from 2006 to 2010. At the same time, the private student loan market fell and parent PLUS loans increased slightly. The middle figure shows the average flow of federal student loans (subsidized and unsubsidized) per full-time equivalent student as well as the enrollment rate of 18-24 year olds in any degree-granting postsecondary institution. Enrollment does increase significantly, but the average flow of student loans also increased by almost $2,000 from 2006 to 2010. Finally, the figure on the reports median federal student loan balances upon entering repayment reported in Looney and Yannelis [2015] broken out by type of institution attended. The figure shows that cohorts who entered college during and after the collapse in the housing market entered repayment with larger student loan balances across all types of institutions. Student loan numbers are constructed by the College Board and are based on various sources. See `http://trends.collegeboard.org/sites/default/files/trends-student-aid-web-final-508-2.pdf` for more details. The aggregate loan flow numbers are taken from figure 5 and the average flow is from figure 6. The enrollment numbers are available from the National Center for Education Statistics here `http://nces.ed.gov/programs/digest/d15/tables/dt15_302.60.asp?current=yes`.

Figure 2: Interest Rates on Student Loans and Mortgages



Note: This figure plots the market interest rates on four types of household debt used to finance college enrollment: subsidized federal loans, unsubsidized federal loans, 30-year fixed rate mortgage debt, and PLUS loans. Starting in 2006, extracting equity is cheaper than student loans and PLUS loans. While subsidized student loans do track mortgage debt somewhat, these loans are need-based and have strict annual and total limits. Data are from the Department of Education and FRED.

Figure 3: Financial Constraints and College Financing Method



Note: This figure plots coefficients and 95 percent confidence intervals from the reduced form regressions of methods of college financing on quartiles of estimated loan-to-value (LTV) ratios interacted with an indicator for college enrollment where the first quartile is the omitted category. The left panel reports the effects on the probability a household reports extracting equity (in red) or carrying a student loan (in blue). The figure shows that constrained households enrolling a student in college are less likely to extract equity and more likely to report having a student loan. At the highest quartile (most constrained) a household is about 10 percentage points less likely to extract equity and about 15 percentage points more likely to have a student loan. The right panel reports the effects on the dollar amount of equity extracted (in red) or student loans borrowed (in blue). The figure shows that constrained households extract less equity and borrow more in student loans. At the highest quartile households extract about $18,000 less equity and take out about $5,000 more student loans.

6 Tables

Table 1: PSID Summary Statistics (1)

	2005 p50/iqr	2007 p50/iqr	2009 p50/iqr	2011 p50/iqr	2013 p50/iqr
Age of Head	52.0	54.0	55.0	57.0	59.0
	(17.0)	(16.0)	(17.0)	(17.0)	(17.0)
Size of Household	3.0	3.0	2.0	2.0	2.0
	(2.0)	(2.0)	(2.0)	(2.0)	(2.0)
Student Loans	4.9	9.2	14.9	14.3	13.9
	(8.0)	(11.2)	(24.0)	(22.9)	(17.6)
Gross Income	71.1	72.4	74.7	70.6	71.4
	(63.0)	(63.8)	(65.6)	(65.4)	(67.7)
Mortgage Debt	48.2	45.8	44.6	42.9	38.4
	(104.9)	(111.9)	(115.9)	(116.2)	(111.0)
Home Value	160.5	178.0	168.4	157.2	157.3
	(171.2)	(203.5)	(178.3)	(152.4)	(148.0)
Liquid Wealth	12.8	15.3	14.9	11.4	12.0
	(58.3)	(68.7)	(66.9)	(62.9)	(67.5)
Illiquid Wealth	32.1	30.9	29.6	28.6	27.8
	(95.0)	(106.1)	(80.0)	(113.4)	(114.7)
Home Equity	96.3	111.9	98.1	90.0	89.7
	(141.2)	(147.5)	(131.8)	(125.8)	(125.8)
LTV	31.5	28.6	30.8	32.0	29.3
	(58.9)	(55.7)	(63.7)	(65.6)	(65.9)
$\widehat{LTV}$	34.1	37.4	37.4	37.5	32.3
	(57.5)	(63.1)	(67.1)	(74.9)	(64.9)
Equity Extracted	22.0	24.8	27.1	18.1	24.9
	(39.5)	(42.1)	(40.1)	(46.9)	(37.0)
4-Year House Price Growth (%)	11.8	12.1	-3.8	-12.6	-9.0
	(40.0)	(39.6)	(26.3)	(25.0)	(20.8)

Note: This table reports medians and interquartile ranges for relevant observables from our baseline sample of households in the PSID. Each household has one observation per year. For student loans and equity extraction we only compute the relevant amount across non-zero observations. See the text for more details.

Table 2: PSID Summary Statistics (2)

	2005	2007	2009	2011	2013
College Age (%)	21.8	21.3	18.6	20.4	19.1
Enrolled in College (%)	7.4	11.9	13.5	14.5	14.4
Has Student Loans (%)	2.2	4.9	8.1	7.8	7.3
Extract Equity (%)	22.5	18.3	15.8	13.1	11.0

Note: This table reports means for relevant observables from our baseline sample of households in the PSID. Each household has one observation per year. See text for more details.

Table 3: College Enrollment and Source of Funding

	P(Enrolled in College)	P(Extract Equity)	Equity Extracted				P(Has Student Loans)	Student Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)
College Age	0.415***		-0.004		-0.959	-0.830*		
	(0.016)		(0.010)		(0.628)	(0.463)		
Enrolled in College		0.026**	0.028**	2.835***	3.436***	-0.496*	0.350***	5.513***
		(0.011)	(0.012)	(0.833)	(0.981)	(0.285)	(0.030)	(0.868)
Equity Extracted						41.073***		
						(2.547)		
Enrolled*Extracted						15.804***		
						(4.842)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	12771	12771	12771	12771	12771	12771	12771	12771
Clusters	51	51	51	51	51	51	51	51
R2	0.355	0.070	0.070	0.051	0.051	0.460	0.661	0.526

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: This table reports OLS estimates of college enrollment and types of borrowing as functions of college age and enrollment indicators. These estimates show in column 1 that households with college age members enroll members in college. In columns 2 and 3 we show that households enrolling members in college are more likely to extract equity (by three to four percentage points) and in columns 4 and 5 we show that these this results in an additional $3,100-3,400 in extracted equity. Column 6 conditions on an indicator for equity extraction explicitly and interacts this indicator with the indicator for enrollment in college. This shows that equity extraction averages to about $40,000 on average, but households enrolling a member in college withdraw about $14,000 more equity from their home. In columns 7 and 8 we also show these households are much more likely to report student loans and in columns 9 and 10 that they carry $9,000-9,500 more in student loans. Each specification includes a state-year fixed effect, a quadratic in the age of the head of household, the household size, lags of mortgage loan-to-value ratio, income, liquid wealth, and non-housing wealth. Standard errors are clustered at the state level. This sample is restricted to homeowners whose home was worth at least $100,000 in the previous year's survey and who did not have negative equity. See the text for more details.

Table 4: Access to Home Equity and Financing College

	P(Extract Equity)	Equity Extracted	P(Has Student Loans)	Student Loans					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					OLS	IV	IV-Non-Parametric	IV-House Prices	IV
	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)
Equity Extracted (1,000s)					0.004	-0.279*	-0.249**	-0.559**	
					(0.008)	(0.142)	(0.123)	(0.226)	
Equity Extracted									-41.683**
									(19.771)
Enrolled in College	0.077***	8.540**	0.239***	3.609***	5.611***	5.954***	5.951***	6.779***	6.780***
	(0.028)	(3.929)	(0.033)	(1.119)	(0.815)	(0.968)	(0.959)	(1.438)	(1.255)
Enrolled*$\widehat{LTV}$	-0.108**	-16.162***	0.214***	4.489***					
	(0.051)	(5.813)	(0.048)	(1.638)					
$\widehat{LTV}$	-0.657***	-55.669***	-0.010	-0.655	0.378	-16.146**			-28.051**
	(0.055)	(5.228)	(0.016)	(0.474)	(0.497)	(7.486)			(12.503)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	7098	7098	7098	7098	7041	7041	7041	7372	7041
Clusters	49	49	49	49	49	49	49	50	49
R2	0.125	0.177	0.212	0.066	0.051	-0.785	-0.648	-3.542	-4.526
Robust F-stat						7.707	3.792	12.823	4.563
Weak ID P-value						0.019	0.080	0.046	0.042

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: This table reports estimates of the effects of access to home equity on equity extraction and student loans. The first four columns estimate the difference-in-differences models interacting enrollment with estimated LTV. The outcomes are probability of equity extraction, equity extraction amounts, probability of student loans, and student loan amounts. As households are more likely to be constrained they are less likely to finance enrollment with equity and more likely to finance enrollment with student loans. Column 5 regresses student loan amounts on the amount of equity extracted and finds no relationship. Columns 6-8 instrument for the amount of equity extracted with the interaction of enrollment with measures of access to home equity. Column 6 instruments with the continuous measure of estimated LTV, column 7 breaks estimated LTV into quartiles, and column 8 uses four-year house price growth only. All three estimates show that households substitute between equity and student loans as access to equity falls with the degree of substitution ranging between 25 and 55 cents. Column 9 instruments for the probability a household extracts equity with estimated LTV interacted with enrollment. This suggests that a household that does not extract equity at all due to constraints take out almost $30,000 less in student loans. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 5: Robustness: College Enrollment, Access to Equity, and Probability of Equity Extraction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)
Enrolled*$\widehat{LTV}(2)$	0.043	0.042	0.047	0.050	0.056	0.056	0.055	0.052
	(0.044)	(0.049)	(0.052)	(0.053)	(0.052)	(0.052)	(0.052)	(0.063)
Enrolled*$\widehat{LTV}(3)$	-0.081**	-0.078*	-0.079*	-0.078*	-0.073	-0.073	-0.072	-0.043
	(0.040)	(0.041)	(0.045)	(0.045)	(0.045)	(0.045)	(0.046)	(0.061)
Enrolled*$\widehat{LTV}(4)$	-0.080	-0.080	-0.088	-0.092	-0.087	-0.087	-0.090	-0.080
	(0.051)	(0.050)	(0.053)	(0.057)	(0.056)	(0.056)	(0.055)	(0.059)
Enrolled*Low Equity (T-4)				-0.000	-0.025	-0.024	-0.018	-0.012
				(0.093)	(0.107)	(0.106)	(0.105)	(0.125)
Enrolled*%Δ HPI						-0.118	-0.116	
						(0.202)	(0.196)	
Enrolled*ΔUR							0.011	
							(0.014)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE X Year	–	–	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X %Δ Income FE X Year	–	–	–	–	Yes	Yes	Yes	Yes
Enroll X State X Year	–	–	–	–	–	–	–	Yes
N	7098	7098	7098	7098	7098	7098	7098	7098
Clusters	49	49	49	49	49	49	49	49
R2	0.110	0.110	0.116	0.117	0.125	0.125	0.125	0.188

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: This table reports regressions of the probability of equity extraction as a function of enrollment interacted with LTV quartiles with the first quartile being omitted. Although precision declines, the regressions show that the effects are robust and not driven by omitted variables. The first column reports the baseline estimate. Column 2 includes interactions of income quartiles with enrollment. Column 3 interacts the income-enrollment interactions with year fixed effects. Column 4 interacts the enrollment indicator with an indicator for whether or not the household had equity below $10,000 four years ago. Column 5 includes an interaction of quartiles of income growth with enrollment and year fixed effects. Column 6 includes an interaction of enrollment with 2-year state-level house price growth. Column 7 interacts enrollment with the state-level change in the unemployment rate. Column 8 includes the interaction of enrollment with state-year fixed effects. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 6: Robustness: College Enrollment, Access to Equity, and Value of Equity Extraction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)	β /(se)
Enrolled*$\widehat{LTV}(2)$	-4.0	-5.0	-4.8	-4.6	-4.1	-4.1	-4.1	-4.9
	(4.9)	(5.2)	(5.4)	(5.4)	(5.4)	(5.4)	(5.4)	(6.7)
Enrolled*$\widehat{LTV}(3)$	-14.2**	-14.8**	-15.1**	-15.0**	-14.5**	-14.5**	-14.5**	-15.5*
	(5.6)	(5.9)	(6.0)	(6.1)	(6.1)	(6.1)	(6.1)	(7.8)
Enrolled*$\widehat{LTV}(4)$	-16.5***	-17.0***	-17.3***	-17.5***	-17.4***	-17.5***	-17.4***	-20.4***
	(5.9)	(6.0)	(6.0)	(6.0)	(6.2)	(6.2)	(6.1)	(7.2)
Enrolled*Low Equity (T-4)				-2.3	-4.9	-4.9	-5.0	-6.6
				(5.7)	(6.6)	(6.6)	(7.1)	(6.8)
Enrolled*%Δ HPI						10.0	10.0	
						(16.6)	(16.6)	
Enrolled*ΔUR							-0.3	
							(1.2)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE X Year	–	–	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X %Δ Income FE X Year	–	–	–	–	Yes	Yes	Yes	Yes
Enroll X State X Year	–	–	–	–	–	–	–	Yes
N	7098	7098	7098	7098	7098	7098	7098	7098
Clusters	49	49	49	49	49	49	49	49
R2	0.147	0.148	0.153	0.154	0.161	0.161	0.161	0.227

* p < 0.1, ** p < 0.05, *** p < 0.01.

Note: This table reports regressions of the amount of equity extracted as a function of enrollment interacted with LTV quartiles with the first quartile being omitted. The regressions show that the effects of estimated LTV are robust and not driven by omitted variables. The first column reports the baseline estimate. Column 2 includes interactions of income quartiles with enrollment. Column 3 interacts the income-enrollment interactions with year fixed effects. Column 4 interacts the enrollment indicator with an indicator for whether or not the household had equity below $10,000 four years ago. Column 5 includes an interaction of quartiles of income growth with enrollment and year fixed effects. Column 6 includes an interaction of enrollment with 2-year state-level house price growth. Column 7 interacts enrollment with the state-level change in the unemployment rate. Column 8 includes the interaction of enrollment with state-year fixed effects. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 7: Robustness: College Enrollment, Access to Equity, and Probability of Student Loans

	(1) β /(se)	(2) β /(se)	(3) β /(se)	(4) β /(se)	(5) β /(se)	(6) β /(se)	(7) β /(se)	(8) β /(se)
Enrolled*$\widehat{LTV}(2)$	-0.006	0.001	0.011	0.013	0.016	0.017	0.015	-0.007
	(0.057)	(0.060)	(0.060)	(0.060)	(0.059)	(0.060)	(0.061)	(0.071)
Enrolled*$\widehat{LTV}(3)$	0.141***	0.150***	0.144***	0.143***	0.152***	0.150***	0.151***	0.134**
	(0.042)	(0.041)	(0.040)	(0.039)	(0.041)	(0.041)	(0.042)	(0.056)
Enrolled*$\widehat{LTV}(4)$	0.164***	0.169***	0.129**	0.123**	0.126**	0.125**	0.120**	0.089
	(0.053)	(0.058)	(0.058)	(0.058)	(0.056)	(0.056)	(0.058)	(0.069)
Enrolled*Low Equity (T-4)				0.088	0.067	0.061	0.072	0.027
				(0.088)	(0.085)	(0.084)	(0.084)	(0.077)
Enrolled*%Δ HPI						0.454	0.458	
						(0.285)	(0.284)	
Enrolled*ΔUR							0.022	
							(0.014)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE X Year	–	–	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X %Δ Income FE X Year	–	–	–	–	Yes	Yes	Yes	Yes
Enroll X State X Year	–	–	–	–	–	–	–	Yes
N	7098	7098	7098	7098	7098	7098	7098	7098
Clusters	49	49	49	49	49	49	49	49
R2	0.214	0.218	0.264	0.265	0.283	0.285	0.287	0.438

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: This table reports regressions of the probability a household reports a student loan as a function of enrollment interacted with LTV quartiles with the first quartile being omitted. The regressions show that the effects of estimated LTV are robust and not driven by omitted variables. The first column reports the baseline estimate. Column 2 includes interactions of income quartiles with enrollment. Column 3 interacts the income-enrollment interactions with year fixed effects. Column 4 interacts the enrollment indicator with an indicator for whether or not the household had equity below $10,000 four years ago. Column 5 includes an interaction of quartiles of income growth with enrollment and year fixed effects. Column 6 includes an interaction of enrollment with 2-year state-level house price growth. Column 7 interacts enrollment with the state-level change in the unemployment rate. Column 8 includes the interaction of enrollment with state-year fixed effects. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 8: Robustness: College Enrollment, Access to Equity, and Value of Student Loans

	(1) β /(se)	(2) β /(se)	(3) β /(se)	(4) β /(se)	(5) β /(se)	(6) β /(se)	(7) β /(se)	(8) β /(se)
Enrolled*$\widehat{LTV}(2)$	1.0	0.9	1.5	1.6	1.5	1.5	1.5	0.6
	(2.1)	(2.3)	(2.3)	(2.3)	(2.2)	(2.2)	(2.2)	(2.2)
Enrolled*$\widehat{LTV}(3)$	2.4**	2.3**	2.0**	2.0**	1.8**	1.8**	1.9**	1.0
	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)	(1.6)
Enrolled*$\widehat{LTV}(4)$	5.4***	5.4***	3.8***	3.7***	3.5***	3.5***	3.4***	3.2**
	(1.6)	(1.6)	(1.4)	(1.4)	(1.2)	(1.2)	(1.2)	(1.2)
Enrolled*Low Equity (T-4)				1.6	0.8	0.8	1.1	-2.1
				(2.8)	(3.3)	(3.3)	(3.4)	(4.0)
Enrolled*%Δ HPI						-2.6	-2.5	
						(9.8)	(9.9)	
Enrolled*ΔUR							0.7	
							(0.6)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X Income FE X Year	–	–	Yes	Yes	Yes	Yes	Yes	Yes
Enroll X %Δ Income FE X Year	–	–	–	–	Yes	Yes	Yes	Yes
Enroll X State X Year	–	–	–	–	–	–	–	Yes
N	7098	7098	7098	7098	7098	7098	7098	7098
Clusters	49	49	49	49	49	49	49	49
R2	0.069	0.069	0.131	0.131	0.154	0.154	0.155	0.332

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: This table reports regressions of the balance of student loans as a function of enrollment interacted with LTV quartiles with the first quartile being omitted. The regressions show that the effects of estimated LTV are robust and not driven by omitted variables. The first column reports the baseline estimate. Column 2 includes interactions of income quartiles with enrollment. Column 3 interacts the income-enrollment interactions with year fixed effects. Column 4 interacts the enrollment indicator with an indicator for whether or not the household had equity below $10,000 four years ago. Column 5 includes an interaction of quartiles of income growth with enrollment and year fixed effects. Column 6 includes an interaction of enrollment with 2-year state-level house price growth. Column 7 interacts enrollment with the state-level change in the unemployment rate. Column 8 includes the interaction of enrollment with state-year fixed effects. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 9: Access to Equity, Financing College Enrollment, and Liquidity

	P(Extract Equity)	Equity Extracted	P(Has Student Loans)	Student Loans
	(1)	(2)	(3)	(4)
	β /(se)	β /(se)	β /(se)	β /(se)
Enrolled	0.053	6.754	0.286***	4.141***
	(0.041)	(4.682)	(0.043)	(1.339)
Enrolled*$\widehat{LTV}$	-0.094	-14.350**	0.141**	3.548*
	(0.067)	(6.147)	(0.058)	(2.108)
Enrolled*Unconstrained	0.057	5.509	-0.137**	-1.630
	(0.054)	(4.691)	(0.055)	(2.462)
Enrolled*$\widehat{LTV}$*Unconstrained	-0.011	-8.754	0.271**	3.718
	(0.118)	(11.094)	(0.110)	(4.600)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
N	7169	7169	7169	7169
Clusters	49	49	49	49
R2	0.126	0.176	0.216	0.067

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: This table reports triple-difference estimates where the interaction of enrollment and estimated LTV is also interacted with a dummy for the household being unconstrained, defined as having at least $150,000 in liquid wealth in the previous reporting period. This table shows that both constrained and unconstrained households increase their use of student loans as they face binding equity constraints, but most of the decline in equity extraction is driven by “constrained” households. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 10: Access to Equity and Other Outcomes

	(1) P(Enroll) β /(se)	(2) P(Credit Cards) β /(se)	(3) P(Student Working) β /(se)	(4) P(Tuition Covered) β /(se)	(5) P(Personal Loan) β /(se)	(6) P(Home Improvement) β /(se)
College Age	0.386***					
	(0.027)					
$\widehat{LTV}$	-0.038*	0.010	-0.028	0.024*	-0.005	-0.011
	(0.020)	(0.019)	(0.023)	(0.015)	(0.011)	(0.029)
College Age*$\widehat{LTV}$	-0.035					
	(0.039)					
Enrolled		0.299***	0.444***	0.545***	0.044**	-0.005
		(0.041)	(0.030)	(0.049)	(0.021)	(0.026)
Enrolled*$\widehat{LTV}$		-0.027	0.063	-0.220***	0.041	-0.068*
		(0.064)	(0.054)	(0.057)	(0.032)	(0.040)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
N	7098	7098	7098	7098	7098	7098
Clusters	49	49	49	49	49	49
R2	0.310	0.143	0.236	0.284	0.026	0.013

Note: This table reports OLS estimates for the younger household members enrolling in college, having credit cards, joining the labor force while a student, for the family providing tuition support, the student taking out a personal loan from relatives, and the household undertaking home improvements. The estimates show that facing equity constraints has almost not effect on the likelihood of enrolling in college. Similarly, households likely to face equity constraints are much less likely to provide tuition support to a student enrolled in college (column 4). The combination of college enrollment and low equity also seems to slightly reduce the likelihood of undergoing home improvements. There are no significant effects on the probability of having a credit card, working, or a personal loan. Each specification includes a quadratic in the age of the head of household, the household size, lagged loan-to-value ratio of the household, log income, and year fixed effects. Standard errors are clustered at the state level. See text for more details.

Table 11: Substitution and Long-run Outcomes (CCP)

	(1) Student Loans β /(t)	(2) Student Loans β /(t)	(3) P(Has Mortgage) β /(t)	(4) P(Has Auto) β /(t)
Equity Extracted (18-22)	-0.819***			
	(-6.76)			
Equity Extracted (23-27)		-0.170***		
		(-6.83)		
Student Loan Amount			-5.71*	-6.56**
			(-2.16)	(-2.74)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	–	–
Home County FE	–	–	Yes	Yes
N	1,986,186	1,986,186	31,545	31,545

Note: This table reports instrumental variable estimates of substitution and student loan effects using the CCP data.The estimates show both that our substitution results are robust to changing datasets and that student loan debt significantly reduces the probability a former student has taken out a mortgage. However, student loans do not seem to affect any of the other long-run outcomes. Each specification includes a controls for credit score, household debt, home county fixed effects, and year fixed effects. Standard errors are clustered at the county level. See text for more details.

A Appendix Tables

Table A1: The Effect of House Prices on Financing College Enrollment: College Age

	P(Extract Equity)	Equity Extracted	P(Has Student Loans)	Student Loans
	(1)	(2)	(3)	(4)
	β /(se)	β /(se)	β /(se)	β /(se)
College Age	0.026*	2.188	0.104***	1.322**
	(0.014)	(2.026)	(0.014)	(0.555)
college_age_ltv_est	-0.024	-5.105	0.084**	1.311
	(0.035)	(3.548)	(0.033)	(0.941)
$\widehat{LTV}$	-0.671***	-57.254***	-0.009	-0.465
	(0.056)	(5.296)	(0.018)	(0.521)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
N	7098	7098	7098	7098
Clusters	49	49	49	49
R2	0.124	0.174	0.104	0.034
Robust F-stat				
Weak ID P-value				

* p < 0.1, ** p < 0.05, *** p < 0.01.

Note: This table replicates our baseline difference-in-difference estimates but uses an indicator for the household having a college-age member instead of an indicator for the household enrolling a member in college. All of our results hold qualitatively in this specification, but the precision and quantities are substantially smaller, consistent with only a fraction of households with college-age members enrolling someone in college. Each specification includes a quadratic in the age of the head of household, the household size, income quartiles, lagged log income, lagged equity quartiles, lagged non-housing wealth, household fixed effects, and year fixed effects. Standard errors are clustered at the state level. See text for more details.