Student Loans, Access to Credit and Consumer Financial Behavior*

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Abstract

This paper provides novel evidence that increased student loan debts, caused by rising tuitions, increase borrowers' demand for additional consumer debt, while simultaneously restricting their ability to access it. The net effect of student loan debt on consumer borrowing varies by market, depending on whether the supply or demand channel dominates. In loosely underwritten credit markets, increased student loan debt causes borrowing to increase, while in tightly underwritten markets, increased student loan debt reduces the use of credit. These findings match predictions of a standard lifecycle model of household consumption and borrowing, augmented with a realistic student loan repayment contract.

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1 Introduction

Student loan borrowing has risen rapidly over the past several decades. Outstanding student loan balances owed by U.S. households now total up to approximately $1.7 trillion, the second largest category of household debt behind residential mortgages. Federally guaranteed loans make up the vast majority of student loan debt and are widely available to essentially any student enrolled at an accredited school. Even very young individuals without incomes or collateral are thus able to take on tens of thousands of dollars of debt to pay for their education without any underwriting of these loans. In contrast, most other forms of credit are rationed in some way through underwriting. Student loans thus present a unique channel for a large swath of individuals to become heavily indebted at a young age. As a large, early life accumulation of student loan debt may influence consumers' financial outcomes for many years to come, it is important to understand the effects of these debts.

In this paper, we provide novel evidence that increased student loan debts, caused by rising tuitions and accumulated early in life, increase borrowers' demand for additional debt in other consumer markets later in life, while simultaneously restricting the borrowers' ability to access it. These other credit markets, consisting of auto loans, credit cards, goods-secured loans, and installment sales contracts, collectively represent approximately $2.3 trillion in debt and serve the important role of helping households smooth consumption over time.[1]

An analysis of the joint effects of supply and demand on net borrowing requires both a theoretical framework and a compelling empirical approach. In Section 2, we first show that in a standard life-cycle model of consumption and borrowing with a fixed-payment student debt contract, student loan debt causes borrowers to increase their demand for other consumer debt early in life. This behavior results from households' desire to smooth consumption in the face of a fixed schedule of student loan payments that they can neither easily refinance nor default on. Introducing credit frictions to such a model complicates the picture. In many credit markets, the amount of debt lenders make available to borrowers

[1]Starting with the seminal works of Bewley (1983), Aiyagari (1994), and Deaton (1991), a large literature in macroeconomics documents the importance of credit constraints for consumption smoothing, consumption growth, and consumption inequality. Kaplan and Violante (2010) offer a recent quantitative assessment of the role of static borrowing constraints for consumption smoothing in standard macroeconomic frameworks, while Ludvigson (1999) highlights the role of time-varying credit constraints in explaining empirical relationships between consumption and income. Chatterjee et al. (2007) and Fulford (2015) extend the analysis to economies that include a fully specified market for unsecured consumer debt.

depends on their observable risk characteristics, and so will be a decreasing function of their outstanding student loan debts.[2] For credit constrained borrowers, higher levels of student loan debts will therefore mean lower levels of consumer borrowing. The net effect of increased student loan debts on consumer borrowing is therefore theoretically ambiguous, depending on whether the consumption smoothing (demand) or credit rationing (supply) channel dominates. Moreover, alternative mechanisms—such as debt aversion—may provide additional pathways for increased student loan debt to affect the use of other forms of consumer credit.[3] Empirical analysis is therefore necessary to understand the mechanisms, as well as to determine the sign and magnitude, of the potential effects.

Starting in Section 4, we test the theoretical predictions of the model in the data. To identify the effect of student loan debt on household borrowing, we use a unique, proprietary data set composed of merged consumer credit and educational histories, and employ an instrumental variable approach that exploits variation in debt driven by tuition increases. Individual student loan debt is endogenous to other determinants of borrowing, so we instrument for it using changes in average in-state tuition at public 4-year universities from the subject's home state. To deal with state-level economic shocks, we compare students who attended those public schools (the treatment group, whose debt is a function of this charged tution) to their same cohort, same state peers who did not attend those public universities (the control group, whose debt is not). We exploit the variation in student loan debt accumulated across cohorts and between states, and within cohort and state between treatment and control groups. We discuss our estimator in detail in Section 4.2.[4]

While the model sketched above does not predict whether the supply or demand channel dominates, it does produce a testable prediction that the effect of increased student loan debt on consumer borrowing should be relatively *more positive* in loosely undewritten markets than in tighter markets. This is because in looser markets credit constraints bind more rarely

[2]For example, Mezza et al. (2020) show that student loan debts make delinquencies more likely, all else equal, causing decreases in the borrowers' credit scores.

[3]Prior research has found evidence for the existence of debt aversion in other settings—see, for example, Loewenstein and Thaler (1989), Thaler (1990), Field (2009), and Palameta and Voyer (2010).

[4]A concern with this framework is that selection into the treatment group, i.e. attendance at a public 4-year university, is a choice on the part of the individual. It would seem quite plausible that the attendance choices of prospective students depend on the tuition they face, and such endogenous selection would bias our estimates. Mezza et al. (2020) show, however, that an individual's probability of attending a public 4-year university is essentially uncorrelated with the average tuition charged, at least, for the relatively small increases in tuition used in this study to identify the effect of interest. We replicate their results in Appendix A.2.

than in tighter markets, so the demand channel is relatively more salient. In tighter markets, the borrowing limit is binding for more borrowers, and so the supply channel is relatively more important. We can therefore test the model—as well as some alternative hypotheses—by estimating the effects of increased student loan debt separately for credit cards, auto loans, installment sales contracts, and goods-secured loans, as there are considerable differences between these markets in the availability of credit. We document the degree of tightness of underwriting by credit market in Section 2.3.

We find that in the least tightly underwritten consumer credit markets, increased student loan debt increases the use of other consumer credit. All else equal, an increase of $1,000 in student loan debts accrued by age 23 causes the probability of using an auto loan, goods-secured loan, and installment sales contract to increase by up to 1.2 to 2.5 percentage points over the next 10 years, depending on the market. This positive estimated effect is consistent with the demand channel posited in our model from Section 2. Further consistent with the theoretical predictions of our model, we find that the same increase in student loan borrowing decreases the probability of having a credit card, the most tightly underwritten form of consumer debt we consider, by as much as 2.4 percentage points. Finally, we find that credit card limits also fall considerably as a consequence of increased student loan debt, consistent with a tightening of credit supply.[5] In sum, our findings are consistent with the theory that student loan debt *increases* the demand for, but can also *reduce* the supply of, additional consumer debt.

A related, but distinct, hypothesis that could rationalize our empirical results described above is substitution between forms of credit. If increased student loan debt restricts the supply of tightly underwritten debt products, debt-burdened consumers may switch into less tightly underwritten products. For example, if credit card borrowing becomes less available to those with low credit scores, those applicants may choose more easily accessible forms of borrowing, such as goods-secured loans, to maintain the desired level of consumption. In Sections 4.4.1 and 4.4.2, we show evidence that this substitution story cannot explain the full scope of our results.

[5]While we argue in this paper for a general supply-contraction explanation for this finding, there are multiple potential mechanisms by which increased student loan debt could cause lower credit limits, and we do not attempt to identify the specific channel. For example, higher student loan debts could increase the likelihood that the borrower defaults on that debt, lowering the borrower's credit score and hence the availability of other forms of credit. It is not necessarily the case that credit card issuers set borrowing limits based on the amount of the applicant's student loan debts.

Finally, debt aversion is another potential mechanism by which student loan debt could affect other borrowing, and has some intuitive appeal. A debt aversion model would predict that more indebted students reduce their demand for other forms of borrowing, as their greater student loan debts make marginal debt more psychologically painful. This prediction is contradicted by our results for auto loans, goods-secured loans, and installment sales contracts, however, where we find positive effects of student loan debt.

Our findings that *ceteris paribus* increases in student loan debt induce significant changes in both the demand for and supply of other non-student loan credit may have significant welfare implications. First, a contraction in the credit supply in response to higher student loan debt (which we observe especially in the relatively tightly underwritten credit card market) may impair households' ability to weather income shocks and to smooth consumption, all else equal. The loss of access to credit cards can severely restrict current-period consumption, particularly for households with low levels of liquid assets.[6] This negative effect of increased student loan debt on consumption is exacerbated by the reduction in disposable income caused by higher scheduled payments since, in the absence of credit constraints, these payments would tend to increase the use of other consumer borrowing.[7] Furthermore, our finding that increased student loan debt causes a reduction in credit limits on the intensive margin suggests that reduced consumption may be a consequence even among households who maintain some access to credit markets. In a recent field-experiment study, Aydin (2019) finds that varying the limits bank customers face on their unsecured lines of credit causes substantial borrowing and consumption responses. This finding suggests that credit rationing, which we demonstrate is a consequence of increased student loan debt, meaningfully distorts consumption outcomes. Second, the increase in demand for additional non-student loan credit in less tightly underwritten markets (such as those for auto loans or installment sales contracts) suggests that even though the student loan market is largely federally guaranteed, increased student loan borrowing can spill over and increase borrowing where credit losses are not guaranteed by the government. All of these consequences are especially salient in the current political environment, as the Biden administration has openly considered policies of wide-ranging student loan debt forgiveness.

[6]Kaplan and Violante (2014) and Kuhn et al. (2020) show that approximately 30 percent of U.S. households have very low liquid assets, and hence rely on credit for any consumption smoothing.

[7]Studies such as Chatterjee et al. (2007) have found that exogenous changes in credit supply can have large welfare effects.

Our paper contributes to a fast growing line of research in household finance investigating implications of student loan debt. The paper complements Mezza et al. (2020), the most closely related paper to this study. That paper shows that increased student loan debt causes a reduction in access to mortgage debt, which is at least partly driven by negative effects of increased student loan delinquencies on credit scores. Using the same data and source of identifying variation, we extend these findings by studying the effects of increased student loan debt on other important consumer markets. In addition, we show how our results and those of Mezza et al. (2020) fit into a broader framework in which accumulating student loan debts spill over into other credit markets, affecting both the supply of and demand for additional consumer credit.

Two other strands of this litererature have investigated the effects of student loan debt in other contexts and using other identification strategies. The first strand investigates the impact of policies aimed at debt payment reductions, without upfront principal forgiveness, on household financial outcomes. Among these studies, two closely related papers are Herbst (2020) and Mueller and Yannelis (2020). Both papers explore the effect of enrollment in income-driven repayment plans on student loan delinquencies and uses of other forms of consumer credit for a middle-aged group of student loan borrowers in an information-experiment setting. Similar to Mezza et al. (2020), both sets of authors find that a reduction in student loan debt payments leads to a reduction in student loan deliquencies. Similar to the present paper, they both also find that reductions in student loan debt payments lead to increases in credit card borrowing.[8] The reduction in payments these studies exploit comes from, effectively, an unexpected refinancing during the repayment period that extends the loan term. Our study, in contrast, investigates the effects of varying loan balances at the outset. These interventions have very different implications for anticipated future student loan debt payments, and therefore may not be expected to have identical effects on other financial outcomes. In addition, we consider effects for a younger treatment population and over a much longer post-intervention period.[9]

[8]In agreement with Mezza et al. (2020), Herbst (2020) additionally finds that the use of mortgage loans increases when student loan payments are reduced.

[9]Another related paper is by Di Maggio et al. (2019), who study the effect of discharge of student loan debt among borrowers already in default. They find that the debt discharge improves household financial wealth. The mechanisms in that paper are different from those in our study, as well as those in Herbst (2020) and Mueller and Yannelis (2020), since the borrowers studied by Di Maggio et al. (2019) were not making payments prior to the debt discharge, and so did not experience any direct increase in their disposable incomes.

A second related strand of the literature has focused on the effects of increased access to student loan debt on household financial and economic outcomes (see, for example, Goodman et al. (Forthcoming), Black et al. (2020), and Denning and Jones (2019)). The conclusions from this literature are generally that increased access improves educational attainment or overall financial liquidity, and hence a variety of other economic outcomes. These findings are relevant for considering policies that would adjust student loan borrowing limits. Our paper does not speak directly to these issues, as we are estimating the *ceteris paribus* effect of increases in student loan debt levels for a given level of education. Our findings are thus applicable to policies that involve the forgiveness of some amount of student loan debt or marginal changes to the price of education.

More broadly, our paper also relates to a large literature studying how consumer financial behavior responds to variation in household balance sheets and scheduled payments. Examples include Agarwal et al. (2015), Ehrlich and Perry (2020), Di Maggio et al. (2017), and Ganong and Noel (2020), who investigate the consequences of reductions in mortgage payments for various outcomes, including payment delinquency and other consumer borrowing. Relatedly, Scharlemann and Shore (2016) and Agarwal et al. (2017) estimate the effects of mortgage principal reductions on repayment. Other studies of shocks to wealth and debt include Cookson et al. (2020) and Verner and Gyöngyösi (2020). As mentioned above, Aydin (2019) estimates the consumption and borrowing response to an exogenous increase in credit limits.

The rest of the paper is organized as follows. In Section 2, we overview the key features of the U.S. student loan market, describe the potential mechanims behind the changes in supply and demand, and introduce a standard optimization model augmented with student loan debt. We describe the data in Section 3. In Section 4, we present the estimator, detail the results, and consider (and reject) some alternative explanations for our findings. We conclude in Section 5.

2 Conceptual Framework

2.1 Key Features of the Student Loan Market

Federal student loans, which currently account for more than 90 percent of the oustanding U.S. student loan debt, have been widely available to students enrolled in post-secondary education for decades. As these loans are either guaranteed or directly issued by the government, the public sector holds the default risk, and it has been an explicit goal of this program to make the loans broadly available with nearly no form of underwriting.[10] Essentially any citizen or qualifying resident is eligible to receive these loans. Federal loans generally charge below-market rates, so borrowers will typically exhaust congresionally determined maxima of these loans before turning to other forms of credit to pay for schooling. Federal student loans thus represent an opportunity for very young individuals with no incomes or credit histories to amass large amounts of debt. Student loans are a very popular way to pay for school—in 2019, almost 40 percent of all 22-year-olds with a credit record accumulated some student loan debt.[11]

A distinguishing feature of federal student loans, in addition to their wide availability without underwriting, is the fact that they are not generally dischargeable in bankruptcy, and borrowers in default may be subject to wage garnishments and tax refund withholdings. Borrowers therefore need to be prepared to service the debt. Under the default federal student loan repayment plan, a student loan is fully amortized over a 10-year period, with fixed scheduled payments. This repayment plan can be adjusted somewhat with, for example, deferments, forbearances, or enrollment in an income-driven repayment plan (which have grown in popularity in the last few years). In our sample, which we describe in Section 3, the majority of borrowers in repayment faced a fixed payment schedule.

Despite the fact that they cannot be discharged in bankruptcy, delinquencies on student loans are quite common. Available to individuals early in their life cycle without underwriting, many households struggle to make payments, particularly if their education does

[10]Some restrictions in eligibility apply. For instance, the post-secondary institution the student attends has to be included under Title IV to be eligible for federal student aid. Also, students who are currently in default on a student loan may not take out another. Graduate students taking PLUS loans—as well as parents taking Parent PLUS loans—must pass a credit check.

[11]Statistic based on authors' calculation using the nationally representative FRBNY Consumer Credit Panel/Equifax credit bureau data for the month of December of 2019.

not lead immediately to well-paying employment. In 2019, about 18 percent of student loan borrowers whose loans were held by the Department of Education—which account for 88 percent of all federal loans—were in default, and additionally more than 4 percent were at least 90 days delinquent, although not yet in default.[12] At 90 days delinquent, servicers of federal student loans are required to start reporting the delinquencies to credit bureaus. The negative record on borrowers' credit histories could hamper their access to additional credit markets.

2.2 A Model of Consumer Borrowing

The effect of early-life student loan debt on borrowing in other forms of consumer credit later in life is theoretically ambiguous. Viewed through the lense of a standard life-cycle model, there are two primary and countervailing forces. On the demand side, larger student loan debt service payments mean that a lesser amount of borrowers' income is available for other uses, so households making student loan payments may choose to maintain a given level of spending by relying more on debt financing than cash financing for their purchases, thereby increasing their demand for debt.[13] On the supply side, if increased student loan borrowing leads to a deterioration of borrowers' credit profiles, then higher student loan obligations could reduce willingness to supply other forms of consumer credit to these borrowers. Taking on student loan debts early in life could thus end up restricting borrowers' access to credit later on, presumably to a greater extent in more tightly underwritten markets.

To illustrate these mechanisms, consider a standard life-cycle model augmented with student loan debt. In the model, households face an exogenous income process and can finance consumption by accessing credit markets. Assuming utility is concave in consumption, households will optimally attempt to smooth their consumption relative to their incomes. In particular, if the income profile is increasing in age, households will borrow in the early years of their life (when incomes are low) and pay the debt down when they are older (and incomes are higher).

[12]The additional federal loans—12 percent of the total—are Federal Family Education loans still in the books of financial institutions or placed in Asset-Backed Securities. Federal student loans are considered in default after 270 consecutive days of delinquency. Statistics based on Department of Education data for the fourth quarter of 2019.

[13]Alternatively, households could also respond to increased student loan payments, all else equal, by limiting their other spending and borrowing less to finance outlays. This would be the prediction of a model of debt aversion, for example.

Let households in the model live 3 periods, with utility given as the log of consumption over each period of their life, discounted by the time discount rate β:

$$U = \ln(c_1) + \beta \ln(c_2) + \beta^2 \ln(c_3). \tag{1}$$

Households can save and borrow at constant rate r. The Euler equation implies the marginal utility of consumption in one period must be equal to the discounted marginal utility of foregoing that consumption plus interest in the next period, so:

$$\frac{1}{c_1} = \beta(1+r)\frac{1}{c_2}, \tag{2}$$

and

$$\frac{1}{c_2} = \beta(1+r)\frac{1}{c_3}. \tag{3}$$

If $\beta = \frac{1}{1+r}$, then households perfectly smooth consumption over time:[14]

$$c_1 = c_2 = c_3. \tag{4}$$

Let households receive an exogenous income in each period, y_t, and allow them to access a credit market in period 1. Define the non-student loan consumer borrowing in period t, b_t, as the difference between consumption and income, so that:

$$b_1 = c_1 - y_1. \tag{5}$$

The budget constraint is standard:

$$c_1 - y_1 = \frac{1}{1+r}(y_2 - c_2) + (\frac{1}{1+r})^2(y_3 - c_3). \tag{6}$$

Substituting equations 4 and 5 into the budget constraint yields an expression for the optimal amount of non-student loan consumer debt in period 1:

$$b_1 = \frac{\beta(y_2 - y_1) + \beta^2(y_3 - y_1)}{1 + \beta + \beta^2}. \tag{7}$$

[14]The results of the model would strengthen if we introduce a wedge into credit markets such that $\beta > \frac{1}{1+r}$.

We now introduce student loan debt into the model. When real-world student loan borrowers enter into repayment, they face a schedule of fixed monthly payments until the balance is paid off. Student loans also lack a clean default option, as they are not dismissible in bankruptcy and delinquent borrowers can have their wages garnished. Options to defer payments by refinancing are also limited.[15] For a borrower in repayment, therefore, student loan debt essentially functions as a reduction in their income equivalent to the scheduled payments.

We model student loan debt as a persistent (but not permanent) decrease, ϵ, in consumer incomes in the early years of working life. That is, we consider the effect of decreases in disposable income available in periods 1 and 2 such that $y_1' = y_1 - \epsilon$ and $y_2' = y_2 - \epsilon$, with the consumer entering period 3 free of student loan debt. With otherwise perfect credit markets, these households will *increase* their early life non-student loan borrowing to make up for the reduced disposable incomes. Consumption will still be smoothed over time. To see this, substitute y_1' and y_2' for y_1 and y_2 in equation 7 and differentiate b with respect to ϵ:

$$\frac{db_1}{d\epsilon} = \frac{\beta^2}{1+\beta+\beta^2} > 0. \tag{8}$$

Departing from the frictionless model complicates the picture, however. If consumer credit markets are not perfect, consumption smoothing as in equation 4 may not be possible. Lacking unlimited access to additional unsecured credit at equivalent rates, borrowers will not be able to perfectly smooth consumption if they meet their scheduled student loan payments. Furthermore, the availability of additional credit may be a function of student debt balances, so the households desiring the largest amount of new borrowing may have the tightest debt limits, all else equal.[16]

With the addition of these credit market frictions, increased student loan debt early in life no longer unambiguously leads to higher levels of additional borrowing. To see this, consider

[15] In more recent years, the use of income-driven repayment plans has complicated this picture. For our sample period, these options were limited and not commonly used.

[16] Lenders may ration credit more tightly to individuals with high levels of student loan debts on their credit records if they view such borrowers as being more likely to default. Moreover, if higher student loan debt payments have caused the borrower to become delinquent on the debt, lenders will be even more restrictive in their willingness to extend additional credit to these individuals. Indeed, Mezza et al. (2020) show that increased student loan burdens increase the probability that a borrower becomes delinquent on their student loan debt. Such delinquencies lead to a reduction of credit aceess through a deterioration of credit scores.

the case of a borrower facing a binding credit limit. Let consumer borrowing be capped at the borrowing constraint $\bar{b}(\epsilon)$. Assume that the borrowing constraint is a decreasing function of student loan debt so that $\frac{d\bar{b}}{d\epsilon} < 0$ if $\bar{b} > 0$. Then, for consumers for whom the cap is binding (i.e. $b_1 = \bar{b}(\epsilon)$), it follows that:

$$\frac{db_1}{d\epsilon} = \frac{d\bar{b}}{d\epsilon} < 0 \text{ if } \bar{b} > 0. \tag{9}$$

In other words, because increased student loan debts cause a reduction in borrowing limits ($\bar{b}$), credit constrained borrowers will be forced to reduce their total consumer borrowing as well. This contrasts with unconstrained borrowers, who increase their total borrowing as shown in equation 8. Both the magnitude and direction of the net effect on increased student loan debt on overall consumer borrowing therefore depends on the sensitivity of the supply of credit to additional debt, as well as the fraction of the population facing binding borrowing constraints.

Thus far, we have been treating consumer credit markets as a single entity. In reality, a variety of consumer credit products exist that differ in important ways. In particular, underwriting standards vary widely across products, as we discuss below. We would therefore expect the effect of student loans on consumer borrowing to vary by submarket as well. In tightly underwritten credit markets, many consumers are credit constrained and additional student loan debt may make them look riskier, further restricting the availability of credit to them and driving net borrowing down. In loosely underwritten markets, where credit is generally available regardless of student loan debt and few consumers are credit constrained, the consumption smoothing effect dominates and net borrowing may increase.

2.3 Differences in Credit Supply across Markets

Lenders who run a credit check on prospective borrowers will learn about their credit history, summarized in a credit score. Credit scores may be adversely affected by higher levels of student loan debts. This effect could be very negative particularly if the borrower has been delinquent in making payments on their student loans. As shown in Mezza et al. (2020), higher student loan debt levels, ceteris paribus, cause borrowers to be more likely to get behind on their payments and correspondingly more likely to have a subprime credit score.

While lenders take the applicant's credit score into consideration when deciding whether

to approve an application for credit, the sensitivity of underwriting to the credit score ultimately depends on the combination of the probability of a loan default and the loss severity given default. Since these two factors vary by market, so do the consequences of a lower credit score on applicant's ability to qualify for a particular credit product. We will exploit this conditional variation in the availability of credit by market in our assessment of the validity of mechanisms proposed in Section 2.2.

Turning first to the default risk, for some credit products, the credit score is a very powerful predictor of delinquency. Other products show a weaker relationship. Looking across loan categories in the credit bureau data, which we describe more fully in Section 3, we see considerable variation in the importance credit scores play in predicting delinquencies. Figure 1 displays delinquency rates on a number of different credit products by the borrower's credit score.[17] As can be seen, delinquency rates on credit cards are both considerably higher than the other products (i.e., auto loans, installment sales contracts, and good-secured loans), and more strongly correlated with credit score.[18] Lenders should be more willing to extend credit to low-score borrowers if the credit product has a low default risk, so we would expect to see tighter underwriting in markets where the default risk is higher.

Lenders also consider loss severity given default in determining applicant's loan eligibility. Both loan amounts and ability to mitigate the loss exposure by, for example, repossesing collateral or recouping losses through bancrupcy proceedings will enter a lender's underwriting decision. With uncollateralized credit products, like a credit card or an installment sales contract, losses may be total so underwriting will be correspondingly tighter. Availability of collateral is not the only consideration, however. For example, while mortgage loans are collateralized by the home, mortgage losses tend to be high if the borrower defaults. Consumer protection laws delay foreclosure, and borrowers in default can end up living in the home for several years before the lender is able to repossess.[19]

The credit bureau data illustrate the considerable differences in the availability of credit

[17]We use the TransRisk Account Management Score 2.0, which ranges from 270 to 909 points.

[18]These differences in delinquencies by product have a number of explanations. Auto loans and good-secured loans are both collateralized, and the lender can straightforwardly reposess the collateral if the borrower defaults. This threat may ensure better repayment behavior. Installment sales contracts, while not collateralized, tend to be short-term loans for small dollar amounts. Credit cards, in contrast, are open-ended lines of credit that borrowers can keep revolving for years as balances accumulate, potentially leading to difficult repayment situations if, for example, borrowers experience a loss of income. Credit cards are also not collateralized, so lenders have fewer consequences to threaten borrowers with.

[19]During this time routine maintenance is likely to be neglected, affecting the resale value and exacerbating the lender's losses.

products conditional on the credit score. In particular, Figure 2, Panel A displays distributions of the credit score observed just before the origination of each of a variety of loan types in our data: home mortgages, credit cards, auto loans, goods-secured loans, and installment sales contracts. As can be seen, consistent with the large potential losses lenders face from the difficulty of repossesing a home, credit scores for new mortgage borrowers are shifted most strongly to the right, meaning that they go to the least risky borrowers, on average. Additionally, mortgage credit is further restricted by constraints on debt-to-income ratios and minimum down payments. The next most rightward-shifted product (i.e, least available to low credit score applicants) is credit cards. The tight underwriting on credit card availability likely reflects the very high delinquency rates among low credit score borrowers apparent in Figure 1. Credit card lenders also tightly ration credit to low credit score borrowers on the intensive margin by restricting their credit limits (thus mitigating the lender's potential losses, and possibly preventing the consumer from borrowing more than they are able to repay). Figure 2, Panel B displays the average total credit card limits of individuals with a credit card in our sample, by their credit score. Credit limits are much higher for low-risk borrowers.

Auto loans, installment sales contracts, and goods-secured loans are more common among lower credit score borrowers. Lenders' willingness to extend credit to borrowers further down the credit score spectrum for these borrowers likely reflects the better performance of such loans, as shown in Figure 1. In addition, repossessing an appliance or a car is much more straightforward than foreclosing on a house so loss given default may be mitigated.

As a consequence of these differences in credit availability, an increase in student loan debt should have different effects on access across markets. The availability of mortgage and credit card loans (on both the extensive and credit limit margins) may be reduced as the borrower's apparent risk increases with their student loan burden. Other forms of credit, however, may remain available as default risk is less relevant.

3 Data

This paper uses a dataset built around a representative sample of credit bureau records from approximately 35,000 individuals born in the years 1973 to 1981 who had a credit record as of December 2004. These data have previously been employed by Mezza and Sommer

(2016), Mezza et al. (2020), and Goodman et al. (2020).

The credit bureau records are provided by TransUnion LLC. Individual credit records are sampled biennially from 1997-2003, then in 2004, 2007 and then biennially from 2008-2014. The data fields provided include full tradeline information (including balances, limits, opening dates and delinquencies) categorized into credit product type. TransUnion also provides the individual's location and TU Trans Risk AM Score, a credit score predictive of borrower risk based on the individual's credit history.

Augmenting the credit bureau records, data on post-secondary schooling enrollment (including schools attended), major and degree receipt from the National Student Clearinghouse (NSC) were merged in at the individual level. Schools report their student administrative records to the NSC, which provides a service allowing students to provide verification of their credentials. Further records on attendance, degree receipt, federal student loan borrowing, and Pell grant aid comes from the National Student Loan Data System, maintained by the Department of Education. This information is available for any student that received federal student loans or Pell grant aid, and is similarly merged onto the TransUnion data at the individual level. Finally, for students that took the SAT, information from the College Board on their state of residence at the time of the test is merged in.[20] Mezza and Sommer (2016) show that these data match well to national statistics on attendance and degree attainment.

With the information about which specific institutions the students attended, we also merge in publicly available data about the schools from the Integrated Postsecondary Education Data System (IPEDS). From IPEDS, we derive the sector of every school attended (i.e. public or private, four- or two-year, for-profit or not-for-profit). We are attempting to estimate the effect that student loan debt accumulated prior to the age of 23 has on a variety of credit market outcomes over the subsequent 10 years. To construct our explanatory variable, we add up the amount of all federal student loans disbursed to the individual through age 22. In the credit bureau records, we observe the open and close dates and complete delinquency record of all loan accounts, including student loans. However, debt balances are observed only when records were pulled (approximately biennially), which for

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

the oldest individuals in our sample occurs after they turned 23. The NSLDS student loan records therefore have the advantage over the credit bureau records of allowing us to observe the cumulative amounts of federal student loan debt disbursed at the same age for every individual in our sample.

Our main outcome variables are indicators for whether the individual has ever made use of each of the categories of consumer debt by a given age. The consumer debt products we investigate are credit cards, auto loans, goods-secured loans, and installment sales contracts. For each person in the credit bureau records, each indicator is set to zero for age t if their records have never shown an open tradeline for a given product type by that age, and set to one once a tradeline is observed to open. In Figure 3, we show the fraction of our sample that has ever had each of these forms of debt by age, from 22 to 32 years old.

In addition to these binary outcomes, we also are estimating the effects of student loan debt on credit limits and utilization rates for credit cards. The credit card limit for an individual at age t is defined as the sum of credit limits across all open credit card tradelines if we observe the individual in the credit bureau records at age t. If not, the limit is set to be equal to the limit observed in the year prior to t. Utilization rates are defined as the sum of all balances on open credit card tradelines, divided by the credit limit. Just as for limits, the value from age $(t-1)$ is used for age t if we do not have credit bureau records for age t.

To deal with the potential endogeneity of student loan borrowing, we follow Mezza et al. (2020) and use an instrument based on varying public 4-year university tuitions from the individual's home state (see Section 4.2 for a complete description of the identification strategy and estimator). To identify the individual's home state, we use a combination of the information from the College Board, TransUnion address information, and the NSC and NSLDS school locations. Details on the procedure are provided in Appendix A.1.

For our final estimation sample, we drop 1,456 individuals due to missing data, consistent with Mezza et al. (2020). These mostly consist of individuals for whom we were not able to determine school sector or for whom the earliest enrollment record corresponds to the date a degree was attained, rather than the actual enrollment date.

Summary statistics for the variables we use in our analysis are presented in Table 1.

4 Estimation

4.1 Selection on Observables

As a first pass at the data, we estimate naive OLS equations of the partial correlation between student loan debt borrowed before age 23 and the extensive margin outcomes of interest (whether the individual has ever had an auto loan, a goods-secured loan, an installment sales contract, and a credit card) for each age from 22 to 32. We control for a range of educational outcomes, including degree (if any) obtained, college major and college sector (i.e. public or private, for-profit or not-for-profit, 4-year or 2-year). We also include home state-by-birth cohort fixed effects. Standard errors are clustered at the state level. We present results, by age, in Figure 4.

The estimated "effects" of student loan debt on the probability of having each of these forms of consumer debt are positive, and generally statistically significant. The pattern of partial correlations across ages varies by market, however. Estimates for auto loans are plotted in Panel A. Individuals with more student loan debt are between 0.1 and 0.4 percentage points more likely to have an auto loan for every additional $1,000 of student loan debt. The partial correlation appears to strengthen rapidly with age early in adulthood. OLS estimates for installment sales contracts and goods-secured loans show different patterns—see Panels B and C. Borrowers with higher levels of student debt are slightly more likely to make use of an installment sales contract, but the relationship is not statistically significant for goods-secured loans. The estimated partial correlation with credit cards is positive as well—see Panel D. However, that relationship seems to attenuate with age. At 22, every $1,000 of additional student loan debt is associated with a 0.15 percentage point increase in the probability of having a credit card, while by age 32 this has fallen to less than 0.5 percent.

4.2 Instrumental Variable Estimation

We should be skeptical of interpreting the above estimates as causal, however, due to the possibility of omitted variable bias. Factors such as student ability, expectations about future incomes and parental resources are all difficult to observe and could affect both students'

decisions about borrowing for school as well as future financial behavior. We therefore need an exogenous source of variation in student loan debt. To this end, we make use of the identification strategy laid out by Mezza et al. (2020), who estimate the effect of student loan debt on homewnership.

Identifying variation in student loan debt is provided by changes in the cost of attending college that students face. When tuitions rise, students will borrow more to pay for their schooling.[21] The average tuition charged to in-state students at public 4-year universities in the individual's home state during their prime college-going years (i.e. ages 18 to 22) provides a good source of quasi-experimental variation. A large fraction of students attend public universities in their home state (in our sample, nearly half of the students who had attended any college before age 23 had attended a public 4-year university in their home state), so the tuition charged is the relevant cost they face. This measure of tuition also does not depend on the choices any individual student makes.

The tuition public universities charge is likely tied to state-level economic performance, so we use within-state variation to identify the effect of student loan debt on other consumer borrowing, splitting the sample into a treatment and control group. Treatment group individuals are students who attended a public 4-year university at some point before the age of 23, the control group is all individuals who did not. In this framework, the control group absorbs any state level shocks to borrowing behavior correlated with (prior) tuition hikes, as they are in the same markets as the treatment group but were not directly affected by the changes in public university tuition. With the inclusion of state and year fixed effects, we are comparing the difference in outcomes between the treatment and control groups, over time as tuitions rise, and across states with different patterns of tuition increases.

For each of the consumer credit products we consider—credit cards, auto loans, installment sales contracts, and goods-secured loans—we model the probability of individual i obtaining a tradeline by age t using equation 10:

$$Y_{it} = \beta_0 + \beta_1 X_i + \beta_2 Z_i + \beta_3 D_i + \mathbf{W_i}\boldsymbol{\beta_4} + \mu_{it} \tag{10}$$

where Y_{it} is a dummy variable indicating i has had the particular debt by age t. We also estimate specifications where we observe Y as a continuous variable, the credit limit, and

[21] For an overview of the major drivers of prevailing tuition rates, see Mezza et al. (2020).

utilization rates of all open credit card lines. X_i is the amount of federal student loans borrowed by individual i prior to age 23, Z_i is the average tuition charged at public 4-year universities in i's home state in the four school years following i's 18^{th} birthday, and D_i is a dummy variable indicating i attended a public 4-year university before i turned 23. The vector $\mathbf{W_i}$ can include a variety of controls at the individual and state level, including fixed effects for individual's home state, birth cohort, or for the combination of the two, i.e., state-by-year fixed effects.

We deal with the endogeneity of student loan debt by estimating a first stage in which X_i is modeled using equation 11:

$$X_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 D_i + \alpha_3 Z_i \times D_i + \mathbf{W_i}\boldsymbol{\alpha_4} + \epsilon_i \tag{11}$$

where the interaction term, $Z_i \times D_i$ (our instrument), is the only term excluded from equation 10.

The term Z controls for correlations between tuition and consumer borrowing among the control group, while D controls for the average difference in outcomes between treatment and control groups. The effect of student loan debt on other borrowing behavior, β_1, is identified by the widening or shrinking of the gap in Y between public 4-year school attendees and the general population as tuition changes.

Following Mezza et al. (2020), we drop individuals who received Pell grant aid from the estimation sample, as the out of pocket cost of college is less tightly correlated with tuition for these students (see Belley et al. (2014)), and the first stage for this subsample is weaker. In Appendix A.2, we show that our results are robust to the inclusion of these individuals in the estimation sample.

There are a number of potential threats to the validity of our estimation strategy. Among them, we may be concerned that estimates of β_1 are inconsistent if membership in the treatment group is influenced by tuition rates. In particular, if the attendance decisions of students considering public 4-year universities are swayed by the prevailing tuition, then our estimates would suffer from sample selection bias. Relatedly, changes in tuition could affect intermediate outcomes (e.g. degree completion or choice of major) which have their own direct effects on financial outcomes. Such effects could be misattributed by our estimator to the direct effect of student loan debt. A third concern is that the economic forces driving

tuition changes might not be adequately absorbed by the control group—in particular, college attendees may be affected differently than those who did not attend any post-secondary schooling.

We address the above concerns through a series of validity tests presented in Appendix A.2, several of which are replicated from Mezza et al. (2020). These tests show that the educational controls, treatment group membership, and Pell grant receipt may be taken as exogenous with respect to tuition. Additionally, we show that an alternative specification which limits the control group to the subsample consisting only of college attendees produces similar estimates as our main specification. This suggests that differences in tuition, rather than differences in exposure to economic shocks, are driving the outcomes. We also include a number of robustness checks, demonstrating the main results are not dependent on a particular set of controls.

4.3 First Stage Instrumental Variable Estimation Results

First stage results from regressing student debt on the instrument and other controls are presented in Table 2. Across various specifications (shown for robustness), a $1,000 increase in the sum of average tuition across the four years after the individual turned 18 is associated with an approximately $150 increase in student loan debt for students in the treatment group.[22] The estimates are strongly statistically significant across specifications.

While higher tuitions are clearly associated with higher levels of student loan debt, the estimated effect size indicates these debts do not rise one-for-one with tuition. There are several reasons for this. First, not all students attend college full time for the full four years from ages 18 to 22.[23] Another reason is that many students use other funding sources than debt to pay for college, such as work study programs or parental contributions. Finally, not all students pay the sticker price of tuition. For example, some students receive need-based or merit-based aid.

[22]For reference, after controlling for state and cohort fixed effects, the residual of the four-year sum of in-state tuition—our identifying variation—has a standard deviation of $915 across our sample.

[23] Individuals in our treatment group attended a public 4-year university for 570 days, on average, across the ages 18 to 22—about half of the possible school days. In addition, during the 1990s (the relevant time period for our sample), about 30% of undergraduates at such schools attended part time, as per the Digest of Education Statistics.

4.4 Second Stage Instrumental Variable Estimation Results

In this section, we use our instrumental variables strategy to estimate the effect of student loan debt on various key consumer credit outcomes. In Section 4.4.1, we show estimates of the effect on the extensive margin of use of different consumer credit products, as we did using a naive OLS estimator in Figure 4. The results are all consistent with the model specified in Section 2.2, which predicts additional student loan debt spurs demand for other consumer debt, but limits the availability of tightly underwritten forms of debt. We also consider and present evidence rejecting an alternative candidate explanation wherein supply restrictions cause borrowers to substitute between forms of debt. Specifically, in Section 4.4.2, analysis of the intensive margins of credit card limits and borrower usage allows us to further refine the set of possible models that explain the consumer borrowing response to an increase in student loan debt. We show evidence that additional student loan debt leads to a contraction in the supply of credit card debt, rather than the demand for it. Furthermore, we present additional evidence that the estimated effects of increased student loan debt on credit card balances are better explained by our model of consumption smoothing than by substitution between forms of credit.

4.4.1 Extensive Margin Borrowing

Using our instrumental variables estimator, we find very different patterns of the effect of student loan debt on other borrowing behavior than we did using the OLS estimator. Estimates of the effect of increased student loan debt on the probability of ever having an auto loan, goods-secured loan, installment sales contract or credit card are presented in Figure 5, by age.

We begin by discussing results for the three most loosely underwritten credit products in our study—auto loans, goods-secured loans, and installment sales contracts. Estimates for these products are shown in Panels A, B, and C of Figure 5, respectively. In all three cases, we find that an additional $1,000 in student loan debt causes an *increase* in the probability the borrower uses this form of consumer debt. Effects are apparent from the borrower's early 20s through their early 30s. In terms of magnitude, $1,000 additional student loan debt increases the probability of ever having an auto loan by 2 to 3 percentage points, of a goods-secured loan by one half to 1 percentage point, and of an installment sales contract

by 1 to 2 percentage points.

These findings are consistent with the predictions of the model presented in Section 2.2. In markets where credit is easily available—that is, where the borrowing constraint, $\bar{b}$, is well above most consumers' optimal borrowing levels, b_1—early-life consumer borrowing is an increasing function of student loan debt payments, ϵ. In this setting, increases in student loan debt induce some student loan borrowers to finance their desired level of consumption with additional consumer debt, generating an extensive margin response in the usage of consumer debt. In contrast, these findings can reject a model of debt aversion as the major mechanism by which increased student loan debt affects borrowing in other markets, which would predict that additional student loan debt causes a *reduction* in demand for other forms of consumer credit.

Next, we examine a market where underwriting is tighter—the credit card market. An inspection of the credit score at origination distributions shown in Figure 2 in Section 2.3 suggests that credit card debt is far more rationed than the products shown in Panels A through C of Figure 5. In Panel D, we plot the effect of increased student loan debt on the probability of having a credit card. In marked contrast to our earlier findings that higher student loan debt increases the odds of having an auto loan, goods-secured loan, and installment sales contract, we now see a large negative effect—a \$1,000 increase in student loan debt *decreases* the probability of having a credit card by 1.5 to 2 percentage points during borrowers' 20s. This finding also fits the predictions of our stylized model of consumer borrowing extended with endogenous credit constraints, presented in Section 2.2. If higher student loan debt burdens impair the risk profile of student loan borrowers and if credit is rationed to borrowers with elevated risk factors, consumer borrowing will fall as the higher level of student loan debt pushes the credit constraint $\bar{b}$ down to zero, generating an extensive margin response in credit card borrowing.

This interpretation is bolstered by the findings in Mezza et al. (2020) who, using the same estimator, show that student loans reduce the probability of obtaining a mortgage. As implied by the rightward shift of the credit score distribution in Figure 2, mortgage loans are the most tightly underwritten of all the major consumer loan products, followed by credit cards, so we would expect the same supply mechanisms to be at play in both markets. In Figure 6 we recapitulate several sets of estimates from that paper. As can be seen from the results displayed in Panel A, a \$1,000 increase in student loan debt decreases the probability

of having a mortgage by up to 2 percentage points.

In addition, the findings of Mezza et al. (2020) also provide evidence that increased student loan debt results in lower credit scores and increases the probability of delinquency on such debt. This deterioration in observable predictors of risk can explain the reduction in borrowing among tightly underwritten forms of credit shown in Panel D of Figure 5 and Panel A of Figure 6, as lenders deny credit to low-score applicants. In Panel B of Figure 6, we replicate this evidence, showing that credit scores provide a channel by which student loan debt affects borrowers' ability to access these markets. The panel displays the estimated effect of student loan debt on having a nonprime credit score (corresponding to a FICO score of 680 or below), by age. The estimated effects of credit scores are not significant at first but grow in magnitude and remain persistently significant after age 26. In Panel C, we can see a similar pattern for the effect of student loan debt on the probability of ever having been 90 days delinquent or more on student loan debt. These results suggest that access to some forms of credit could be impaired by student loan debt's negative effect on credit scores, in part through the channel of increasing delinquencies on such debt.

Up to this point, we interpreted our findings in the context of our theoretical model from Section 2.2 wherein an increase in student loan debt leads to a rise in demand for other forms of consumer debt. However, a related but distinct hypothesis that could rationalize our findings in Figure 5 is substitution between forms of credit. If increased student loan debt restricts the supply of tightly underwritten debt products, debt-burdened consumers may switch into less tightly underwritten products. Perhaps most plausibly, if credit card borrowing becomes less available to those with low credit scores, these borrowers may switch their credit demand to more easily accessible forms of borrowing, such as goods-secured loans and installment sales contracts, to maintain a desired level of consumption.[24] This form of substitution could generate results consistent with estimates plotted in Panels A, B, and C in Figure 5. However, the substitution hypothesis and our theoretical model make testably distinct predictions about the effect of student loan debt on the probability of *joint* holdings of multiple forms of consumer debt. In our model, increased student debt increases the demand for borrowing, so we may expect to see increased use of goods-secured loans and

[24]This is not the only possible type of debt substitution that could occur, although it may be the most likely to happen. In Appendix A.3, we analyze other forms of debt substitutions, particularly subsitution between mortgage debt and other forms of credit, finding evidence that supports the mechanism highlighted by the theoretical model presented in Section 2.2.

installment sales contracts among all segments of the population, including those who also hold credit cards. That is, the probability of having both a credit card and a goods-secured loan or installment sales contract should be increasing with student loan debt. In contrast, the substitution hypothesis can explain increased goods-secured or installment sales contract borrowing only among borrowers who lost access to credit card debt. The effect of student loan debt on the joint probability of holding both a credit card and either a goods-secured loan or installment sales contract should therefore be weakly negative.[25]

To test for the validity of this alternative hypothesis against that of our model, in Figure 7 we show the estimated effect of a $1,000 increase in student loan debt on the joint probability of having both a credit card and either a goods-secured loan or an installment sales contract.[26] The estimated effect on the joint probability of having a credit card and either a goods-secured loan or an installment sales contract is positive and, after age 26, statistically significant, providing evidence in favor of our theoretical model. While these results do not rule out that some substitution across forms of debt could be happening, they suggest that the alternative hypothesis of substitution across debts cannot entirely explain the results presented in Panels A, B, and C in Figure 5.

4.4.2 Credit Limits and Intensive Margin Borrowing

As shown in Section 4.4.1, increased student loan debt increases the use of loosely underwritten forms of consumer credit, while decreasing the use of more tightly underwritten forms. This pattern is consistent with the model presented in Section 2.2, in which the demand for consumer debt increases generally but the supply of tightly underwritten debt becomes more restricted. However, we may be concerned that other, unmodeled forces could instead be the explanation for these different empirical responses.

First, we consider the possibility that the reduction in the use of credit cards is a demand response, rather than a supply response as we have argued. Perhaps some idiosyncratic feature of the demand for credit card debt causes a reduction in use in response to higher student loan debts, in contrast to the increased demand we observed in other consumer debt

[25]In Section 4.4.2, we examine substitution away from credit card borrowing on the intensive margin (i.e. reducing borrowing while maintaining the presence of a credit card), and provide further evidence that the substitution hypothesis cannot explain our results.

[26]That is, we re-estimate equation 10, defining the outcome variable Y_{it} as equal to 1 if i has ever had *both* a credit card and at least one of either a goods-secured loan or installment sales contract by age t, and zero otherwise.

markets? To address this concern, we take advantage of the fact that for credit cards we can observe individual credit limits. With some caveats, these limits represent the amount of credit a lender is willing to extend to a borrower, as distinct from the amount of credit the borrower actually uses. Credit constraints can bind not only by excluding borrowers from markets on the extensive margin, but also by limiting their credit usage on the intensive margin. Increased student loan debts could therefore affect credit limits.[27] To test for such an intensive margin effect, we reestimate equation 10 with the borrower's total available credit card limit (summed across all cards) as the dependent variable Y. Results are presented in Figure 8, Panel A. There does not seem to be much of an effect during the borrower's early 20s, but by their late 20s and into their early 30s an additional $1,000 in student loan debt (accumulated by age 22) causes total credit limits to fall by over $1,000.[28] This translates to approximately a 6 percent reduction relative to the average credit card limit of 30 year-olds in our data.

These results are consistent with a supply response, as lenders often impose borrowing limits that constrain consumers' borrowing below their desired levels (see Fulford (2015) and Aydin (2019)). Yet, we should not dismiss out of hand the possibility that the reduced credit limits may instead be reflective of changes in borrower demand. In particular, borrowers can request credit limit increases on an existing account or they can apply for an entirely new credit card. As such, we might see the same pattern shown in Panel A of Figure 8 if increased student loan debt causes borrowers to reduce their demand for limit increases. This could occur if student loan debt causes borrowers to reduce their intensive margin demand for credit card debt. With less credit card debt, these borrowers might then have less incentive to expand their available credit limits.

To better distinguish between these two mechanisms, we note that the demand story outlined above and the supply response in our model presented in Section 2.2 make different predictions about the effect of increased student loan debt on the credit card utilization rate. Our model of consumer borrowing has an analog to the utilization rate: the ratio of early-life consumer borrowing, b_1, to the borrowing limit, $\bar{b}$. As shown in Section 2.2, for unconstrained borrowers in our model the effect of student loan debt on borrowing is positive

[27] In the parlance of our model, this would mean a reduced, but non-zero, $\bar{b}$.

[28] Results are essentially unchanged when the sample is limited to individual's with non-zero limits (i.e. excluding anyone without a credit card).

(i.e., $\frac{db_1}{d\epsilon} > 0$ if $b_1 < \bar{b}$). Furthermore, in our model with endogenous borrowing constraints, the credit limit is decreasing in the amount of student loan debt (i.e., $\frac{d\bar{b}}{d\epsilon} < 0$). Therefore, it follows that the utilization rate, $b_1/\bar{b}$, is increasing in the amount of student loan debt.[29] The model thus predicts that increased student loan debt causes utilization rates to increase.[30]

In contrast, if the negative effect of student loan debt on credit card limits was due to decreased demand, we would also expect to see a negative effect on utilization rates. Even with some borrower ability to influence limits, demand for credit should affect balances (which, up to the credit limit, are entirely under borrowers' control) more than the limits themselves. Therefore, borrowers with lower credit demand should have lower utilization rates. As such, if additional student loan debt caused credit card borrowing demand to fall (rather than causing the supply to contract, as we have argued), we would also expect to see utilization rates fall (that is, credit card holders borrowing less given a certain credit limit).

We test which model best fits the data by estimating the effect of student loan debt on credit card utilization rates. We reestimate equation 10 using the utilization rate across all credit cards as the dependent variable. Results are presented in Panel B of Figure 8. As can be seen, utilization rates *increase* by approximately 1 to 3 percentage points throughout the borrower's 20s and early 30s in response to a $1,000 increase in student loan debt. In conjuction with the findings of reduced credit limits, this utilization rate finding suggests that it is the supply of, rather than the demand for, credit that is contracting in response to increased student loan debt.[31]

As we considered in Section 4.4.1, a second alternative explanation for our results (namely, that increased student loan debt increases the use of loosely underwritten credit products and decreases the use of tightly underwritten products) could be substitution between forms of consumer debt. As we showed, the effect of student loan debt on the use of goods-secured loans and installment sales contracts could not be explained purely as a result of

[29] If we define the utilization rate as $U = \frac{b_1}{\bar{b}}$, then $\frac{dU}{d\epsilon} = \left(\frac{db_1}{d\epsilon}\bar{b} - \frac{d\bar{b}}{d\epsilon}b_1\right)\bar{b}^{-2}$. Since b_1, $\bar{b}$ and $\frac{db_1}{d\epsilon}$ are all weakly positive and $\frac{d\bar{b}}{d\epsilon}$ is weakly negative, it follows that $\frac{dU}{d\epsilon} \geq 0$

[30] If the borrowing limit binds, then $b_1 = \bar{b}$ and the utilization rate, fixed at unity, does not respond to an increase in student loan debt. A positive effect of student loan debt on utilization rates in our model requires at least some fraction of the borrower population to have some slack debt capacity.

[31] We cannot definitively rule out demand responses as an explanation for the results in Figure 8: For example, if increased student loan debt caused a reduction in demand for credit limit increases without affecting the demand for actual credit card borrowing, we would expect to see those same results. However we think such a model of consumer behavior, which also requires an explanation for why it is confined to credit cards and not other forms of consumer borrowing, is less likely than the simple credit constraints story we presented in Section 2.2.

borrowers, denied access to a credit card on the extensive margin, switching to other forms of debt. However, as we have just seen, student loan debt can cause reductions in intensive as well as extensive access to credit card debt. It would seem possible that some households, constrained by a low credit limit from financing their desired purchases of durable goods entirely through credit card borrowing, might substitute some of that demand into other forms of borrowing as well.

To test for such a pattern of behavior, we note that this substitution story makes a sharp prediction about credit card balances. To produce the hypothesized shift into other forms of credit, the total amount of credit card borrowing must fall in response to increased student loan debts. In contrast, the model of consumer borrowing presented in Section 2.2 is ambiguous about the effect of student loans on credit card balances. On the one hand, lower credit limits and reduced extensive margin access should constrict borrowing for the subpopulation among whom the constraints bind. On the other hand, unconstrained borrowers might be expected to increase their demand for credit card debt (as $\frac{db_1}{d\epsilon} > 0$ if $b_1 < \bar{b}$). Which of these two effects dominates is not certain *a priori*.

We reestimate equation 10 using the outstanding balance across all credit cards as the dependent variable. Results are presented in Figure 9. The estimated effects are small and not significantly different from zero. In fact, the point estimates are actually positive for individuals in their mid-to-late 20's. While this test cannot conclusively reject the possibility that increased student loan debt causes credit card balances to fall, the evidence suggests that balances were little changed, on average. This finding fits our model of student loan debt causing credit demand among unconstrained borrowers to rise, as its prediction about the overall effect on balances is ambiguous. A model in which student loan debt causes increased credit demand only through the substitution channel fits this finding less well, as such a model would clearly predict a reduction in credit card balances.[32]

Taken together, our intensive margin results from this section and the extensive margin results from Section 4.4.1 fit the simple model described in Section 2.2. Other explanations for our results that we have considered make predictions that do not fit well or can be

[32]To be sure, it may be the case that some substitution from credit card debt into other forms of credit is occuring. If so, however, our point estimates would suggest this is offset by an increase in credit card borrowing by unconstrained individuals. Therefore, regardless of whether this substitution is occuring or not, a positive effect of student loan debt on credit demand is necessary to explain the full set of findings on credit cards, goods-secured loans, and installment sales contracts.

outright rejected by our data.

5 Conclusions

Rising outstanding student loan balances have spurred interest in understanding how this debt may affect the borrowers' later financial outcomes, particularly as they can be accumulated early in life by individuals without income, assets, or credit history. These effects are little understood, however, as most existing analysis of these issues has been limited both by lack of data and the difficulty of identifying the causal relationships. To bridge this gap, in this paper we use a proprietory dataset composed of merged consumer credit and educational histories and employ an instrumental variable approach that exploits variation in debt driven by tution increases to identify the effect of student loan debt on household financial outcomes.

We provide novel evidence that increased student loan debts, caused by rising public university tuition rates, increase borrowers' demand for additional consumer debt, while simultaneously restricting their ability to access it. The net effect of student loan debt on consumer borrowing varies by market, depending on whether the supply or demand channel dominates. In particular, in the least tightly underwritten credit markets (such as those for auto loans or installment sales contracts), increased student loan debt causes borrowing usage to increase. By contrast, in tightly underwritten credit markets (such as credit cards), increased student loan debt leads to a net reduction in the use of credit. These findings match predictions of a standard lifecycle model of household consumption and borrowing, augmented to include a realistic student loan repayment contract. In the model, we show that student loan debts (which include a fixed payment schedule, are not dischargeable in bankruptcy, and may not be easily refinanced) imitate the effect of reduced flow income during their borrowers' early life. Increased student loan debt therefore triggers a consumption smoothing response, with borrowers attempting to borrow more against their future incomes to offset the reduction in their disposable income. Lenders, on the other hand, view more indebted borrowers as risky and reduce the total supply of consumer debt available to them.

Our analysis has several important practical implications. First, the restriction in the supply of consumer credit to borrowers caused by increased student loan debts (on both

the intensive and extensive margins) could lead to reduced consumption. Particularly for borrowers with low levels of liquid assets, consumer credit is an important method for consumption smoothing. Second, the extensive-margin increases in auto loan, goods-secured loan, and installment sales contracts borrowing caused by increased student loan burdens indicate that even though the student loan market is largely federally guaranteed, increased student loan borrowing can spill over and increase borrowing where credit losses are not guaranteed by the government. This can be of particular concern as student loan debt continues to rise.

In a larger sense, our results also indicate that household debt can be a complement to itself, as consumers respond to higher debt service payments by increasing their demand for additional credit. On the supply side, lender's incentives push against this trend, preventing excessive accumulation of debts in most markets. Were these private safeguards to be relaxed, however, there is potential for accumulating household debt to multiply. As was seen during the credit boom years prior to the financial crisis, lenders may be induced to relax their underwriting standards under certain aggregate conditions. The compounding demand for household debt could then have consequences for financial stability if already heavily-indebted consumers face substantially relaxed credit rationing.

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Table 1: Summary Statistics

Variables	Obs	Mean	Std. Dev.	Min	Max
Student Loan Debt Measures					
Student Loans Disbursed (in $1,000)	33,435	4.990	11.109	0	184.294
Student Loans Disbursed (in $1,000), Conditional on Debt>0	9,720	17.166	14.681	0.002	184.294
Tuition (in $1,000)	33,435	19.835	6.020	7.506	43.562
School Sector Controls					
Ever Public 4-Year	33,435	0.262	0.440	0	1
Ever Public 2-Year	33,435	0.248	0.432	0	1
Ever Private 4-Year Not-for-profit	33,435	0.116	0.320	0	1
Ever Private 2-Year Not-for-profit	33,435	0.008	0.087	0	1
Ever Private For-profit	33,435	0.047	0.211	0	1
Degree and Pell Grant Controls					
No College	33,435	0.458	0.498	0	1
Associate's/Certificate	33,435	0.030	0.171	0	1
Bachelor's	33,435	0.113	0.317	0	1
Master's or More	33,435	0.001	0.039	0	1
Degree of Unknown Type	33,435	0.008	0.088	0	1
Ever Pell	33,435	0.206	0.404	0	1
Year of Birth	33,435	1977.02	2.544	1973	1981
Ever Delinquent					
On Student Loans	33,435	0.149	0.356	0	1
On Auto Loans	33,435	0.032	0.175	0	1
On Installment Sales Contracts	33,435	0.019	0.136	0	1
On Goods-Secured Loans	33,435	0.003	0.055	0	1
On Credit Card Debts	33,435	0.188	0.391	0	1
Loan Balance ($s)					
Auto Loans	100,492	15,345.1	11,932.7	1	99,515
Installment Sales Contracts	6,750	2,801.3	5,625.8	1	84,758
Goods-Secured Loans	1,858	3,087.8	2,976.0	23	30,673
Credit Card Debts	176,606	3,217.1	5,692.2	0	95,295
Loan Amount at Origination ($s)					
Auto Loans	99,009	17,421.7	10,076.2	1	99,609
Installment Sales Contracts	13,966	2,813.4	5,144.0	1	95,518
Goods-Secured Loans	4,776	2,887.0	2,799.5	2	58,199
Loan Maturity (months)					
Auto Loans	97,132	55.6	15.7	1	99
Installment Sales Contracts	13,405	24.9	16.6	1	96
Goods-Secured Loans	4,731	26.3	12.5	1	96
Required Monthly Payment ($s)					
Auto Loans	95,452	374.4	324.8	1	9,999
Installment Sales Contracts	12,345	111.2	193.9.0	1	6,146
Goods-Secured Loans	4,228	121.8	114.1	1	2,465
Additional Outcomes					
Ever Non-prime	33,435	0.739	0.439	0	1
Ever Subprime	33,435	0.610	0.488	0	1

Note: Student loans disbursed are measured as the total amount of federal student loans disbursed to individuals before age 23. Tuition is the average in-state tuition at public 4-year colleges in the individual's home state over the four years following his or her 18th birthday. Student loans and Tuition are in constant 2014 dollars. School sector, degree, and Pell Grant controls represent the sectors, the attained degree and whether individuals received Pell Grants before age 23. "Ever Delinquent" represents whether individuals were ever at least 90 days delinquent on student loan debt, credit card debts, auto loans, installment sales contracts or goods-secured loans between the ages of 22 and 32. Loan Balance represents the balance for the different consumer debts at the moment the credit records were pulled, starting in June 1997 and ending in December 2014. Balance amounts are in nominal terms. Statistics shown limited to loans with reported balances less than $100,000. Loan amount at origination represents the size of the loan when originated for installment-type loans. Originations amounts are in nominal terms. Statistics shown limited to loans with reported amounts at origination less than $100,000. Loan maturity is the duration of the loan in months for installment-type loans. Statistics shown limited to loans with maturity terms less than 100 months. Required monthly payment is the monthly payment borrowers are required to make to stay current on the loan. Dollar amounts are in nominal terms. Statistics shown limited to loans with monthly payments less than $10,000. "Ever Non-prime" and "Subprime" represent whether individuals ever had a TransUnion Risk Score below thresholds that roughly correspond to FICO scores of 620 and 680, respectively, between the ages of 22 and 32.

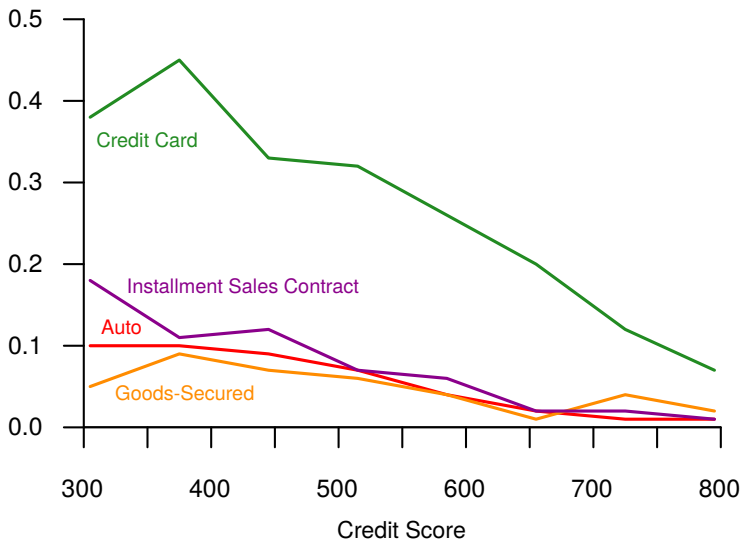
Table 2: Estimated Effects on Student Loan Amounts (First Stage)

Total Federal Student Loans Disbursed before Age 23

	Full Sample			No Pell	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Instrument: Tuition x Ever Public 4-Year	0.085*	0.156***	0.173***	0.205***	0.142***
	(0.046)	(0.039)	(0.045)	(0.039)	(0.045)
Ever Public 4-Year	5.440***	1.520**	2.886***	−0.380	0.688
	(0.737)	(0.644)	(0.762)	(0.697)	(0.737)
No College		−2.004***		−2.802***	
		(0.336)		(0.391)	
Associate's/Certificate		0.028		−0.794	−0.905
		(0.532)		(0.600)	(0.589)
Bachelor's		3.245***		1.670***	1.727***
		(0.592)		(0.571)	(0.563)
Master's or More		4.211**		2.470	2.451
		(1.782)		(1.870)	(1.941)
Degree of Unknown Type		−0.028		−0.841	−0.733
		(0.845)		(1.160)	(1.144)
Ever Public 2-Year		−2.424***		−2.037***	−1.808***
		(0.252)		(0.314)	(0.320)
Ever Private 4-Year Not-for-profit		8.086***		7.128***	7.013***
		(0.298)		(0.292)	(0.273)
Ever Private 2-Year Not-for-profit		1.795**		2.583***	2.381**
		(0.831)		(0.925)	(0.911)
Ever Private For-profit		1.883***		3.698***	3.791***
		(0.515)		(0.440)	(0.450)
Ever Pell		4.018***			
		(0.216)			
Constant	2.085***	2.019***	1.263***	2.799***	2.672***
	(0.074)	(0.288)	(0.057)	(0.329)	(0.280)
College Major Controls	NO	YES	NO	YES	YES
Home State by Cohort FEs	YES	YES	YES	YES	YES
Observations	33.435	33.435	26.546	26.546	11.233
F-statistic	16.100	74.700	67.500	120.600	14.700
R-squared	0.122	0.363	0.109	0.311	0.166

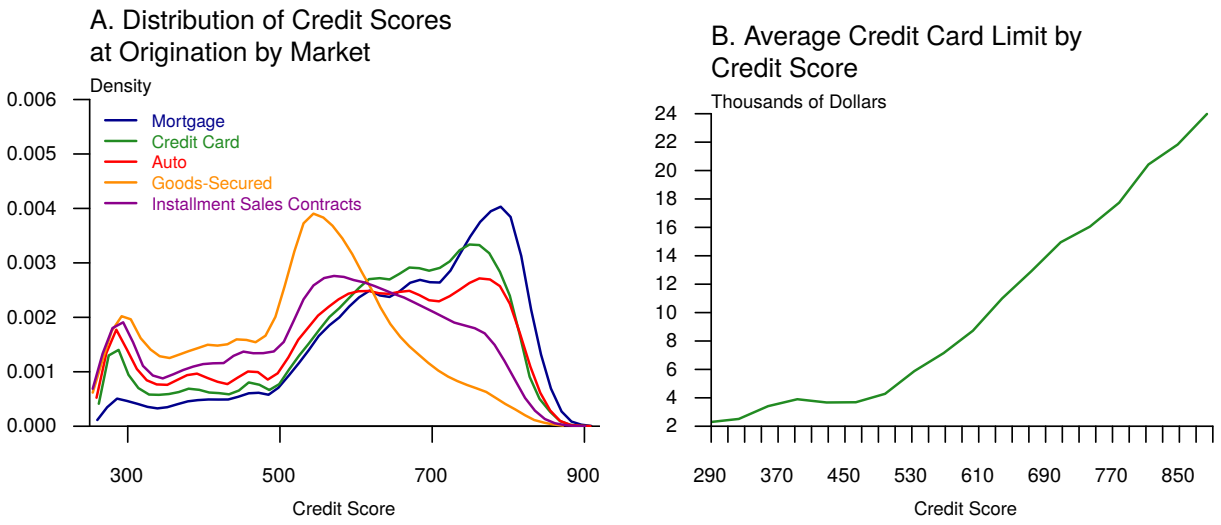
Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. Column (1) only controls for whether individuals ever attended a public 4-year college before age 23 and home state-by-cohort fixed effects. Sample is all individuals from a nationally-representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. Column (2) builds on column (1) by adding several educational controls summarized in Table 1 and 14 college major indicator variables described in Mezza et al. (2020). The omitted degree attainment category is having attended college before age 23 without getting a degree by that age. Columns (3) and (4) replicate the estimates in columns (1) and (2), respectively, while restricting the sample to individuals who did not receive Pell Grants before age 23. Column (5) replicates the estimates in column (4) further restricting the sample to individuals who attended any post-secondary schooling and did not receive Pell Grants before age 23. Student loans disbursed and tuition are recorded in 1000s of year 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Figure 1: Delinquency Rate, by Market and Credit Score



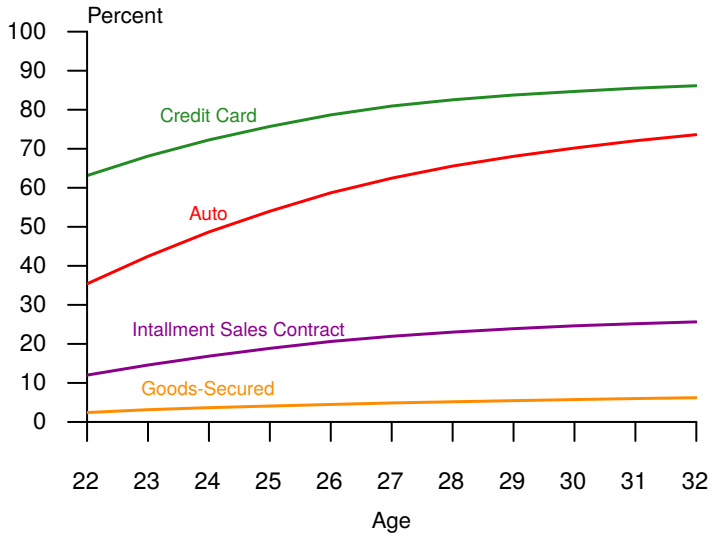
Note: Delinquency rates for each market correspond to the share of individuals who have ever been 90 days or more past due on each particular type of debt by age 32, plotted against individuals' credit score at age 22 (or at age 21 if credit score at age 22 is not available). The credit score ranges from 270 to 909 and delinquencies are calculated for 9 equal-size, 71-point range, credit score groups.

Figure 2: Credit Availability, by Market and Credit Score



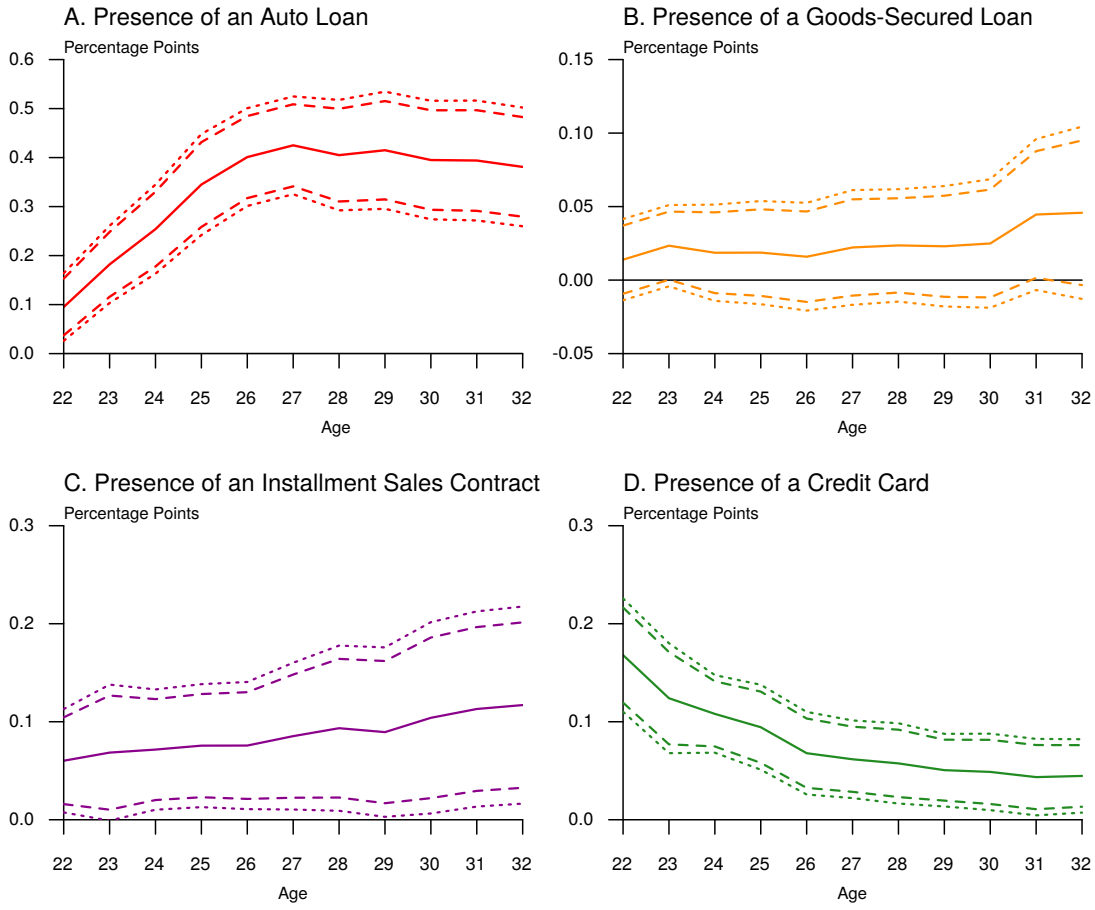
Note: Panel A displays kernel density distributions of credit scores by debt product. Credit scores are measured as of the most recent observation prior to or concurrent with origination, and the sample includes all loans originated after June 1997 for which a positive credit score observation is available. Panel B displays the average credit limit in thousands of nominal dollars for credit card accounts with positive limits and positive credit scores associated with them. The credit score ranges from 270 to 909 and the average credit limits are calculated for 18 equal-size, 35-point range, credit score groups.

Figure 3: Fraction of the Sample that Has Ever Had a Trade Line, by Age



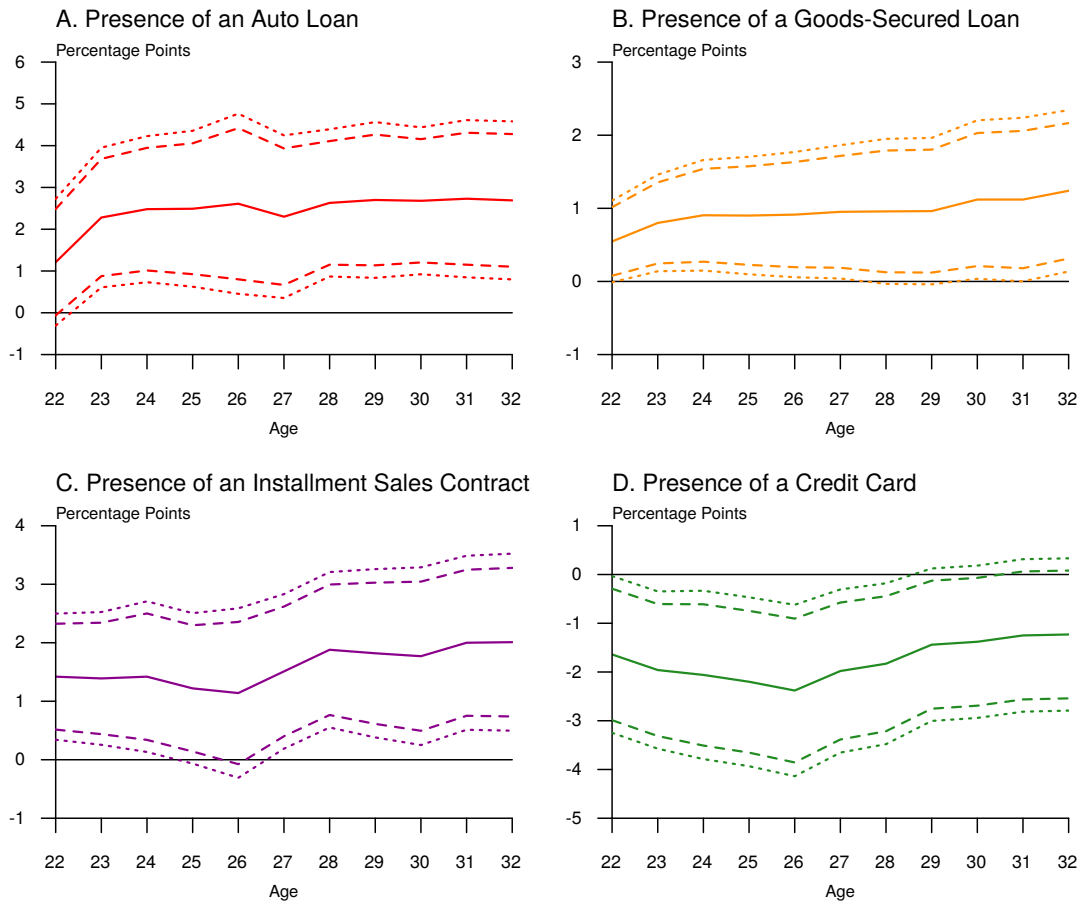
Note: The figure displays the percent of individuals in the sample that have ever had a credit card, an auto loan, a goods-secured loan, or an installment sales contract by age. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23.

Figure 4: Effect of a $1,000 Increase in Student Loan Debt, OLS



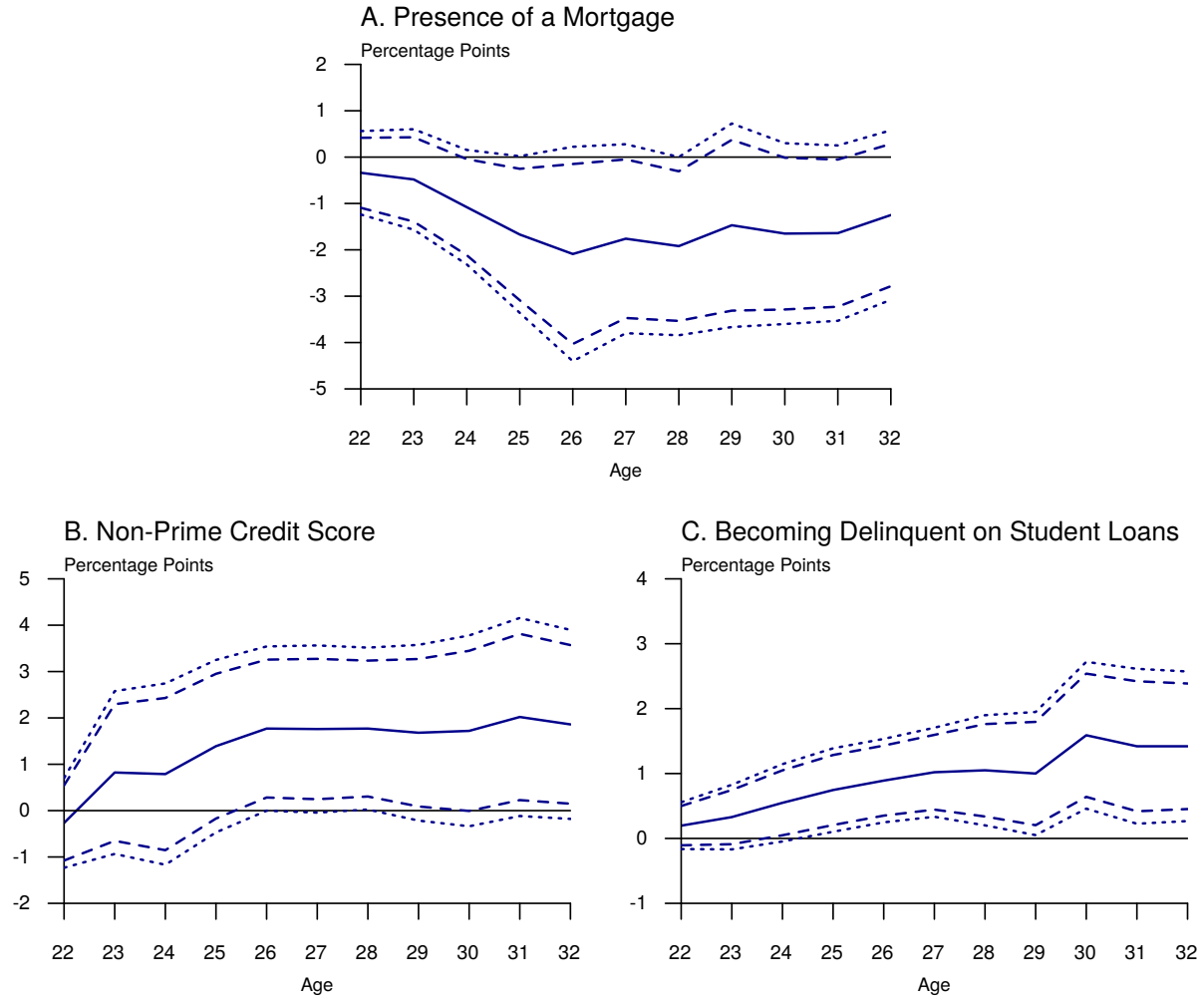
Note: Panels A, B, C, and D display OLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability model by OLS. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include controls for tuition and whether the individuals ever attended a public 4-year university before age 23, several educational controls summarized in Table 1 and 14 college major indicator variables described in Table 7 of Mezza et al. (2020). The regressions also include home state-by-cohort fixed effects. For more details on the controls included, see column 4 of Table 2. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure 5: Effect of a $1,000 Increase in Student Loan Debt, 2SLS



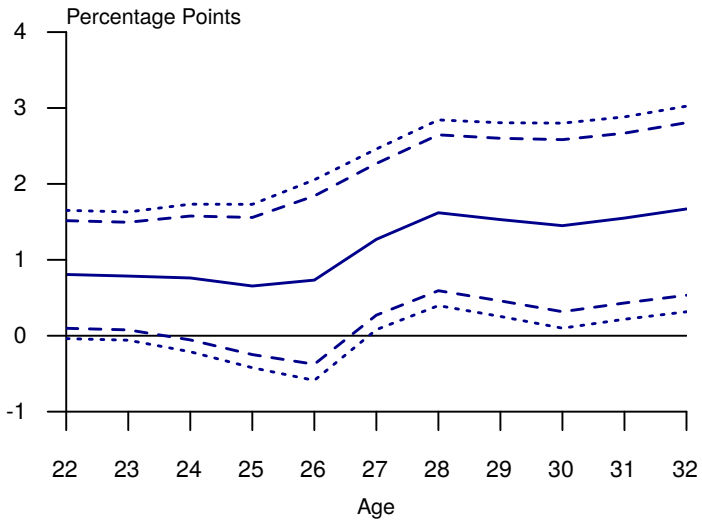
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include the same controls as those in Figure 4. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure 6: Effect of a $1,000 Increase in Student Loan Debt on Other Credit Outcomes, 2SLS (from Mezza et al. (2020))



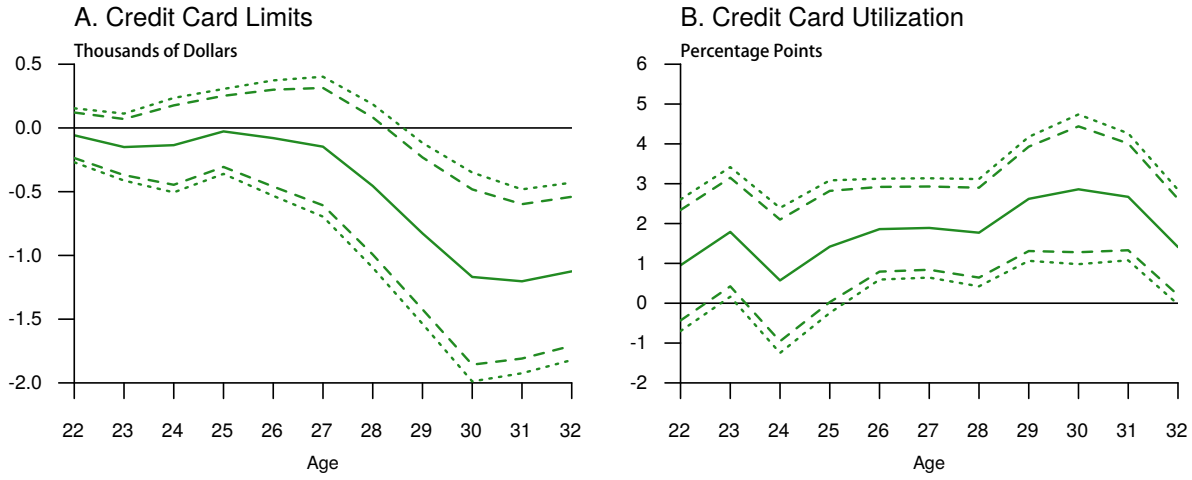
Note: Panels A, B, and C, display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having a mortgage, a non-prime credit score, and ever becoming delinquent on student loans, respectively, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include the same controls as those in Figure 4. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure 7: Joint Effect of a $1,000 increase in Student Loan Debt, 2SLS



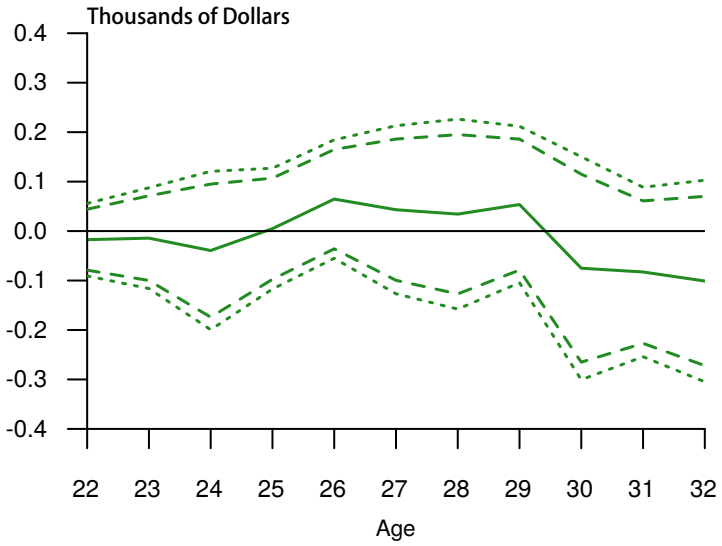
Note: The figure displays 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having both a credit card and either a goods-secured loan or installment sales contract, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include the same controls as those in Figure 4. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure 8: Effect of a $1,000 Increase in Student Loan Debt on Credit Card Limits and Utilization Rates, 2SLS



Note: Panels A and B display 2SLS estimates of the effect of a $1,000 increase in student loans on credit card limits and utilization rates, respectively, by age. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include the same controls as those in Figure 4. Student loan debt is recorded in thousands of 2014 dollars, and instrumented with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure 9: Effect of a $1,000 Increase in Student Loan Debt on Credit Card Balances, 2SLS



Note: The figure displays 2SLS estimates of the effect of a $1,000 increase in student loans on credit card balances, by age. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include the same controls as those in Figure 4. Student loan debt is recorded in thousands of 2014 dollars, and instrumented with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

A Appendix

A.1 Construction of Home State

The identification of our model is derived from changes in the average public 4-year university tuition rates in individual's home state (i.e., pre-college state of residence).[33] Hence, to estimate the model, we must impute home state for each subject in our sample. To construct the home state, we follow a waterfall methodology from Mezza et al. (2020) and impute the home state in three main steps. First, for those individuals in our sample who took the SAT (about 15 percent of our sample), the College Board data provide us with information on their state of residence at the time the SAT test was taken. Second, for individuals who did not take the test but have a record on a state of residence reported in the Transunion data prior to their first enrollment in college (the next 20 percent of our sample), we use this information to impute home state. Third, we split the individuals who did not have home state assigned in the previous two steps into two subgroups: (1) those who went to college (37 percent of the sample), and (2) those who did not go to college (28 percent of the sample).[34] For the college-going subgroup, we impute the home state using data on the state in which the college associated with the first post-secondary enrollment record is located. For the non-college going sub-group, we impute their home states as the first state available in the TransUnion credit records.

In principle, imputing the home state as the state of first college attendance for some students could be problematic. Students can move out of state to attend a college, and this choice could potentially be correlated with changes to the in-state tuition of either the origin or destination state. To validate this imputation approach, we note that in the nationally representative 2003-04 Beginning Postsecondary Students (BPS) Longitudinal Study only about 11 percent of first-time, non-foreign college entrants attended a post-secondary institution outside of their state of legal residence.[35] By comparison, 23 percent of college goers in our sample whose home state was identified from the SAT or credit

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

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

[35]The state of legal residence defined as the student's true, fixed, and permanent home. A student who has moved out-of-state solely to attend school would be recorded as still living in their home state.

records (equivalent to 11 percent of our total sample of all college goers) attended an out of state post-secondary school. Therefore, the group of individual who either took the SAT or whose home state was identified from their pre-college credit record in step (2) above account for the entire expected population of out-of-state students as identified in the BPS data. Misidentification of the subject's home state from the endogenous choice to attend an out-of-state school is likely a negligible issue in our data.

A.2 Validity and Robustness

A potential threat to the validity of our estimation strategy is if the attendance decisions of students considering public 4-year universities is affected by tuition at those schools. If so, the endogenous selection into the treatment group would lead to inconsistent estimates of the effects of interest. Column 1 of Table A1 explores this possibility. In particular, column 1 shows the result of regressing an indicator of having attended a public 4-year university before age 23 on the average tuition measure and state and cohort dummy variables. The estimated effect is small and statistically insignificant. Thus, an individual's probability of attending a public 4-year university by age 23 is essentially uncorrelated with the average tuition charged, at least for the relatively small changes in tuition we exploit in this study to identify the effect of interest. Column 2 shows the effect of tuition on college attendance, independently of the sector chosen, for completeness. The effect is also small and insignificant. Column 6 also shows that tuition changes have no effect on attending a public 4-year university even when we limit the sample to students who had attended some college before age 23, suggesting that small changes in tuition at public 4-year universities do not induce switching between school sectors.[36]

Based on the findings of Belley et al. (2014) that net tuition paid by lower-income students is less strongly linked to the sticker price due to the availability of need-based grants, our main analysis is limited to the sample of individuals who did not receive Pell Grants before age 23, for which the instrument is consequently more relevant. That said, this sample selection could lead to bias if Pell Grant take up is influenced by tuition charged at public 4-year universities. To address this concern, Columns 5 and 8 of Table A1 show the effect of tuition on the probability of receiving any Pell Grants before age 23 for the full sample

[36]Mezza et al. (2020) describe in detailed all the estimates presented in Table A1 and provide evidence that these results are consistent with those in the existent literature on the effect of tuition on college attendance.

and the college-going subsample, respectively. Both estimates are small and statistically insignificant. Additionally, Figure A1 shows that estimates for the whole sample are very similar to those in Figure 5 for the sample of non-Pell Grants recipients.

Another potential threat to validity of the instrument is that changes in tuition could affect intermediate outcomes (e.g., degree completion), which have their own direct effects on financial outcomes and could be misattributed by our estimator to the direct effect of student loan debt. Columns 4 and 7 of Table A1 show estimates of the effect of tuition on the probability of completing a bachelor's degree before age 23 for the whole sample and the college-going subsample, respectively. Results suggest there is no significant correlation between tuition at public 4-year universities and the completion of a bachelor's degree.[37].

Relatedly, in our preferred specification we include controls for educational outcomes (school sector, degree attained, and major choice) because these covariates could affect earnings and the supply of and demand for different types of debt conditional on tuition. Failing to control for these outcomes could therefore bias our estimates of interest. However, these controls are potentially endogenous, which could then introduce a different bias. To address this concern, Figure A2 replicates the main results in Figure 5 excluding all education controls. Results are broadly similar, so neither source of bias seems to be of much concern.[38]

A final potential threat is that economic forces driving tuition changes might not be adequately absorbed by the control group. In particular, college attendees may be affected differently than those who did not attend any post-secondary schooling. To address this concern, Figure A3 replicates the analysis presented in Figure 5 for the sample of college-goers. While results are less precisely estimated than for the full sample, the general patterns are similar.

The combination of all these tests show that the educational controls, treatment group membership, and Pell grant receipt may be taken as exogenous with respect to tuition. Additionally, the similarity in results between our main specification and from the alternative one

[37]Mezza et al. (2020) show in Table 7 that tuition at public 4-year universities does not affect major choice either

[38]Tables A2, A3, A4, and A5 show estimates for the probability of ever having an auto loan, a goods-secured loan, an installment sales contract, and a credit card by age 27, respectively, for alternative specifications. Columns 1 and 2 in each table reflect estimates for the whole sample when educational controls are and are not included, respectively. Columns 3 and 4 reflect the same estimates restricted to the sample of individuals who had not received any Pell Grants (measured at age 23). Finally, column 5 repeats the analysis in column 4 for the sample of individuals with some college education before age 23. All these estimates also reflect that including education controls has a relatively minor effect on the estimates of interest.

that limits the control group to the subsample consisting only of college attendees suggests that differences in tuition, rather than differences in exposure to economic shocks, is driving the outcomes.

The regression estimates for discreate outcomes presented in the main body of the text were estimated by two-stage least squares. For robustness, Figure A4 replicates this analysis using the iv-probit routine in Stata. Results do not differ by whether we use a linear or nonlinear probability model.

A.3 Substitution

In Sections 4.4.1 and 4.4.2 we considered the possibility that increased student loan debt causes increases in the demand for other forms of consumer credit via a substitution effect. Specifically, if high levels of student loan debt cause borrowers to be excluded from credit card markets (or to have their credit limits set constrainingly low), the borrowers may respond by increasing their use of goods-secured loans and installment sales contracts, which are more readily available. We provided evidence that our finding of increased use of goods-secured loans and installment sales contracts cannot be explained by the substitution hypothesis, but better fits our model outlined in Section 2.2. Furthermore, loss of access to credit card borrowing is not a plausible explanation for the increased use of auto loans we found in Section 4.4.1. Auto loans clearly dominate credit cards as a form of borrowing for auto purchases, with lower rates and easier availability due to their collateralized nature.

However, increased student loan debt restricts borrower access to mortgage loans as well (Mezza et al. (2020)). While borrowers denied mortgage credit cannot simply shift the form of their borrowing while maintaining the same consumption bundle (auto loans, goods-secured loans, and installment sales contracts cannot be used to buy a house), being denied a home loan could cause a shift in the consumption bundle itself.[39] This might appear in credit records as increased demand for other forms of consumer debt. In this section we test whether our finding of increased demand for consumer credit in response to increased student loan debt could be coming from frustrated mortgage applicants.

First, we consider auto lending. Perhaps student loan borrowers, denied access to mortgage credit, buy a new car with the disposable income that would have gone toward mortgage

[39] Benmelech et al. (2017) find that home purchases are associated with a substantial increase in households' purchases of durable goods.

payments. This story makes a specific prediction about the effect of student loan debt on the joint probability of having neither a mortgage nor an auto loan. Increased student loan debt reduces the probability of having a mortgage, so if the increased auto borrowing was coming entirely from rejected mortgage applicants, the joint probability of having neither should be increasing, or at least zero (if every marginal rejected mortgage applicant responded by getting an auto loan). The substitution story cannot explain a decrease in the joint probability of having neither a mortgage nor an auto loan (that is, an increase in auto lending not offset by a decrease in mortgage borrowing for some set of individuals).

In contrast, our model does not preclude a reduction in the joint probability of having neither a mortgage nor an auto loan in response to increased student loan debt. We might expect to see such a reduction if, for example, the marginal auto borrowers were well below the margin of being homeowners. To test our model against the substitution hypothesis, we construct an outcome variable equal to one if the individual has never had a mortgage or an auto loan by age t, and zero otherwise. We re-estimate equation 10 using this joint probability as the outcome variable, and present results in the left-hand panel of Figure A5.

As can be seen, the estimated effects of a $1,000 increase in student loan debt are uniformly negative, and statistically significant in the borrowers late 20s and early 30s. The point estimates are quite large as well—almost the same magnitude as the total effect on auto loan borrowing shown in Panel A of Figure 5. This test can reject the mortgage-auto loan substitution hypothesis as the sole explanation, and the point estimates indicate it is most likely responsible for little or none, of the increased auto lending.

Next, we consider goods-secured loans and installment sales contracts. While the findings of Benmelech et al. (2017) would predict that being denied mortgage credit might decrease the use of these forms of credit (which are often used for durable goods purchases), some substitution may be possible. Again, such a subsitution story would predict the increased student loan debt causes a weakly increasing joint probability of having neither a mortgage nor a goods-secured loan or installment sales contract, while our model allows for negative effects (positive effects are possible under our model as well, if the marginal users of goods-secured loans or installment sales contracts are infra-marginal homeowners). We create an outcome variable equal to one if the individual has never had a mortgage, goods-secured loan, or installment sales contract by age t, and zero otherwise, and re-estimate equation 10. Results are presented in the right-hand panel of Figure A5.

The point estimates are again generally negative, although from ages 25 to 27 they are very close to zero and actually positive at age 26. Estimates are only marginally statistically significant at ages 22 and 32. This test does not reject the substitution hypothesis as cleanly as for the mortgage-auto loan channel, but the balance of the evidence points toward the conclusion that increased student loan debt decreases the probability of having neither a mortgage nor a goods-secure loan or installment sales contract. This finding fits our model, but not the substitution hypothesis.

Table A1: Effect of Tuition on Educational Outcomes

	Full Sample					College Attendees		
	Ever Public 4-Year (1)	**Any College (2)**	**Ever Public 2-Year (3)**	**Bachelor's (4)**	**Any Pell (5)**	**Ever Public 4-Year (6)**	**Bachelor's (7)**	**Any Pell (8)**
Tuition (Public 4-Year)	0.0002	-0.005		0.001	-0.0005	0.001	0.003	0.002
	(0.004)	(0.008)		(0.003)	(0.004)	(0.008)	(0.004)	(0.003)
Tuition (Public 2-Year)			-0.022					
			(0.020)					
Home State/Cohort FEs	YES	YES	YES	YES	YES	YES	YES	YES
Observations	33,435	33,435	33,324	33,435	33,310	18,121	18,121	18,121

Note: This table reports linear probability model estimates of the effect of a $1,000 increase in tuition on several educational outcomes (denoted by the column headers). Tuition (Public 4-Year) is the average in-state tuition at public 4-year colleges in the individual's home state through the four years following his or her 18th birthday, while Tuition (Public 2-Year) is the average tuition at public 2-year colleges in the individual's home state through the two years following his or her 18th birthday. Student loans and tuition are in 1000s of 2014 dollars. See Table 1 for other variable definitions. Sample in columns (1) through (5) is all individuals from a nationally-representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. The sample in columns (6) through (8) is restricted to individuals who have attended any post-secondary schooling before age 23. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table A2: Effect a $1,000 Increase in Student Loan Debt on the Probability of Having an Auto Loan, 2SLS

Probability of Ever Having an Auto Loan by Age 27					
	Full Sample			No Pell	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Student Loans Disbursed	0.028	0.020*	0.021	0.023**	0.013
	(0.024)	(0.012)	(0.013)	(0.010)	(0.015)
Ever Public 4-Year	−0.117	−0.075*	−0.027	−0.061**	−0.034
	(0.160)	(0.045)	(0.078)	(0.031)	(0.049)
No College		−0.065**		−0.057	
		(0.030)		(0.036)	
Associate's/Certificate		0.213***		0.219***	0.203***
		(0.027)		(0.026)	(0.027)
Bachelor's		0.073		0.063*	0.073**
		(0.047)		(0.034)	(0.036)
Master's or More		0.118		0.112	0.129*
		(0.073)		(0.071)	(0.069)
Degree of Unknown Type		0.159***		0.175***	0.158***
		(0.042)		(0.050)	(0.046)
Ever Public 2-Year		0.083**		0.070**	0.052
		(0.034)		(0.029)	(0.031)
Ever Private 4-Year Not-for-profit		−0.202**		−0.226***	−0.160
		(0.095)		(0.072)	(0.110)
Ever Private 2-Year Not-for-profit		−0.007		−0.054	−0.018
		(0.036)		(0.058)	(0.064)
Ever Private For-profit		−0.019		−0.041	0.003
		(0.023)		(0.037)	(0.062)
Ever Pell		−0.094**			
		(0.048)			
College Major Controls	NO	YES	NO	YES	YES
Home State by Cohort FEs	YES	YES	YES	YES	YES
Observations	33.435	33.435	26.546	26.546	11.233

Note: This table reports 2SLS estimates of the effect of student loans on the probability of having an auto loan by age 27. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 2 for sample selection and specification details. Student loans disbursed and tuition are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table A3: Effect a $1,000 Increase in Student Loan Debt on the Probability of Having a Goods-Secured Loan, 2SLS

Probability of Ever Having a Goods-Secured Loan by Age 27

	Full Sample			No Pell	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Student Loans Disbursed	0.018	0.009**	0.012**	0.010**	0.006
	(0.012)	(0.004)	(0.006)	(0.005)	(0.006)
Ever Public 4-Year	−0.151*	−0.055***	−0.100***	−0.049***	−0.039**
	(0.080)	(0.018)	(0.036)	(0.015)	(0.019)
No College		0.034***		0.039***	
		(0.011)		(0.015)	
Associate's/Certificate		0.004		0.008	0.007
		(0.016)		(0.021)	(0.020)
Bachelor's		−0.036*		−0.020	−0.016
		(0.020)		(0.018)	(0.017)
Master's or More		−0.035		−0.012	−0.010
		(0.028)		(0.038)	(0.036)
Degree of Unknown Type		−0.001		0.014	0.016
		(0.022)		(0.027)	(0.026)
Ever Public 2-Year		0.030**		0.023*	0.013
		(0.014)		(0.012)	(0.013)
Ever Private 4-Year Not-for-profit		−0.087**		−0.079**	−0.056
		(0.035)		(0.033)	(0.041)
Ever Private 2-Year Not-for-profit		−0.022		−0.028	−0.006
		(0.016)		(0.023)	(0.021)
Ever Private For-profit		−0.012		−0.029	−0.017
		(0.009)		(0.018)	(0.025)
Ever Pell		−0.029			
		(0.019)			
College Major Controls	NO	YES	NO	YES	YES
Home State by Cohort FEs	YES	YES	YES	YES	YES
Observations	33.435	33.435	26.546	26.546	11.233

Note: This table reports 2SLS estimates of the effect of student loans on the probability of having a goods-secured loan by age 27. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 2 for sample selection and specification details. Student loans disbursed and tuition are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table A4: Effect a $1,000 Increase in Student Loan Debt on the Probability of Having an Installment Sales Contract, 2SLS

Probability of Ever Having an Installment Sales Contract by Age 27

	Full Sample			No Pell	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Student Loans Disbursed	0.020	0.012	0.018**	0.015**	0.001
	(0.017)	(0.008)	(0.009)	(0.007)	(0.014)
Ever Public 4-Year	−0.192	−0.092***	−0.164***	−0.084***	−0.038
	(0.118)	(0.032)	(0.050)	(0.022)	(0.039)
No College		0.037		0.053**	
		(0.026)		(0.027)	
Associate's/Certificate		0.033		0.051	0.040
		(0.032)		(0.038)	(0.038)
Bachelor's		−0.040		−0.022	0.008
		(0.039)		(0.031)	(0.039)
Master's or More		−0.103*		−0.107*	−0.052
		(0.062)		(0.062)	(0.061)
Degree of Unknown Type		0.014		0.050	0.047
		(0.041)		(0.050)	(0.045)
Ever Public 2-Year		0.043*		0.034*	0.023
		(0.023)		(0.019)	(0.028)
Ever Private 4-Year Not-for-profit		−0.110*		−0.126***	−0.029
		(0.063)		(0.049)	(0.093)
Ever Private 2-Year Not-for-profit		−0.029		−0.052	−0.009
		(0.025)		(0.038)	(0.045)
Ever Private For-profit		0.029		0.019	0.067
		(0.019)		(0.035)	(0.055)
Ever Pell		−0.019			
		(0.032)			
College Major Controls	NO	YES	NO	YES	YES
Home State by Cohort FEs	YES	YES	YES	YES	YES
Observations	33.435	33.435	26.546	26.546	11.233

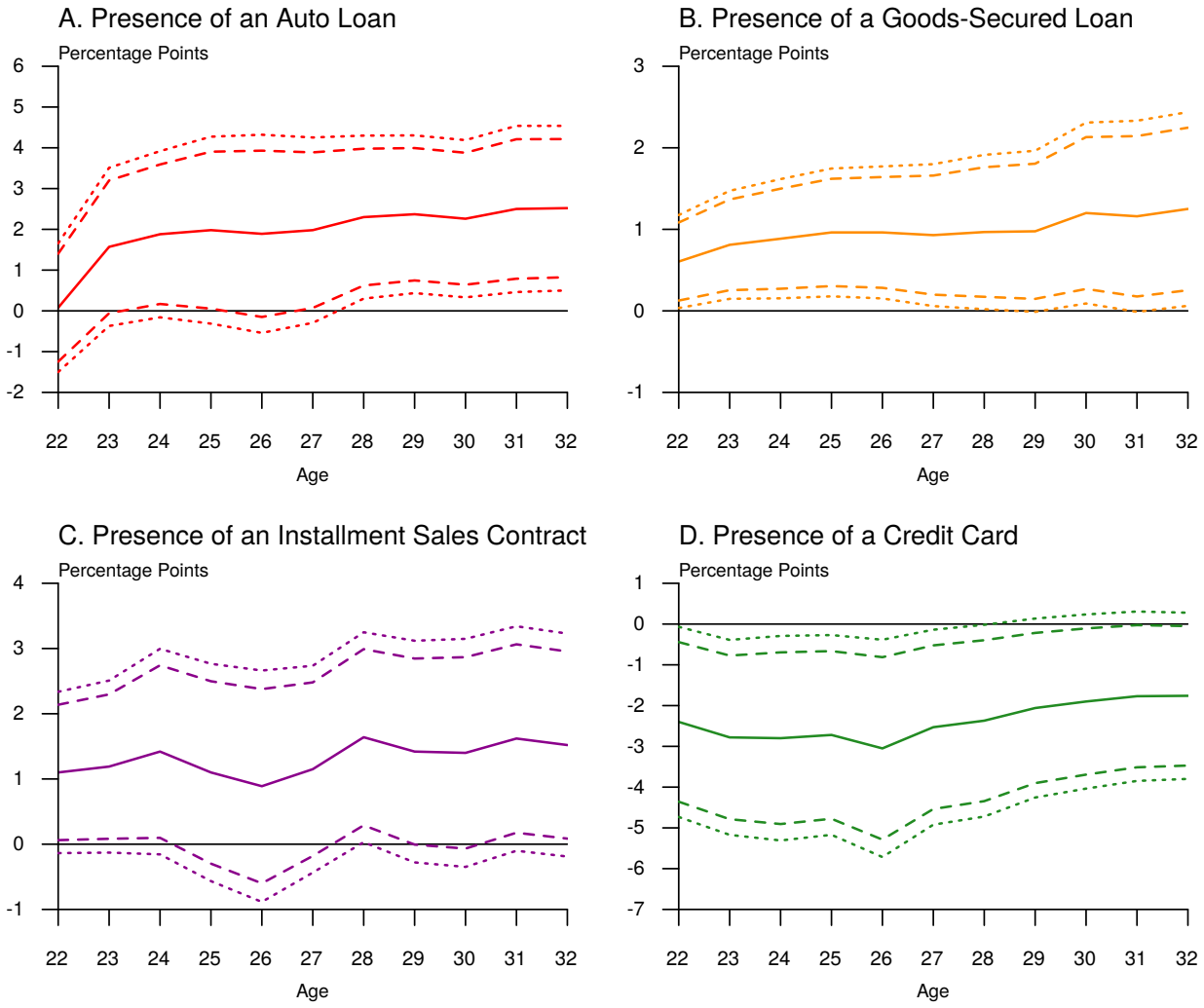
Note: This table reports 2SLS estimates of the effect of student loans on the probability of having an installment sales contract by age 27. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 2 for sample selection and specification details. Student loans disbursed and tuition are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Table A5: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Having a Credit Card, 2SLS

Probability of Ever Having a Credit Card by Age 27					
	Full Sample			No Pell	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Student Loans Disbursed	−0.071	−0.025**	−0.035**	−0.020**	−0.022**
	(0.054)	(0.012)	(0.016)	(0.009)	(0.011)
Ever Public 4-Year	0.682*	0.172***	0.418***	0.119***	0.122***
	(0.364)	(0.049)	(0.097)	(0.029)	(0.035)
No College		−0.248***		−0.262***	
		(0.029)		(0.028)	
Associate's/Certificate		0.036*		0.014	0.014
		(0.019)		(0.022)	(0.023)
Bachelor's		0.101*		0.047	0.053
		(0.052)		(0.032)	(0.034)
Master's or More		0.129*		0.071	0.081
		(0.071)		(0.058)	(0.065)
Degree of Unknown Type		0.026		0.014	0.015
		(0.026)		(0.024)	(0.027)
Ever Public 2-Year		−0.072**		−0.056***	−0.053***
		(0.029)		(0.017)	(0.020)
Ever Private 4-Year Not-for-profit		0.242**		0.164***	0.180**
		(0.099)		(0.060)	(0.077)
Ever Private 2-Year Not-for-profit		0.031		0.024	0.047
		(0.038)		(0.040)	(0.039)
Ever Private For-profit		0.015		0.077**	0.095**
		(0.027)		(0.039)	(0.046)
Ever Pell		0.085*			
		(0.051)			
College Major Controls	NO	YES	NO	YES	YES
Home State by Cohort FEs	YES	YES	YES	YES	YES
Observations	33.435	33.435	26.546	26.546	11.233

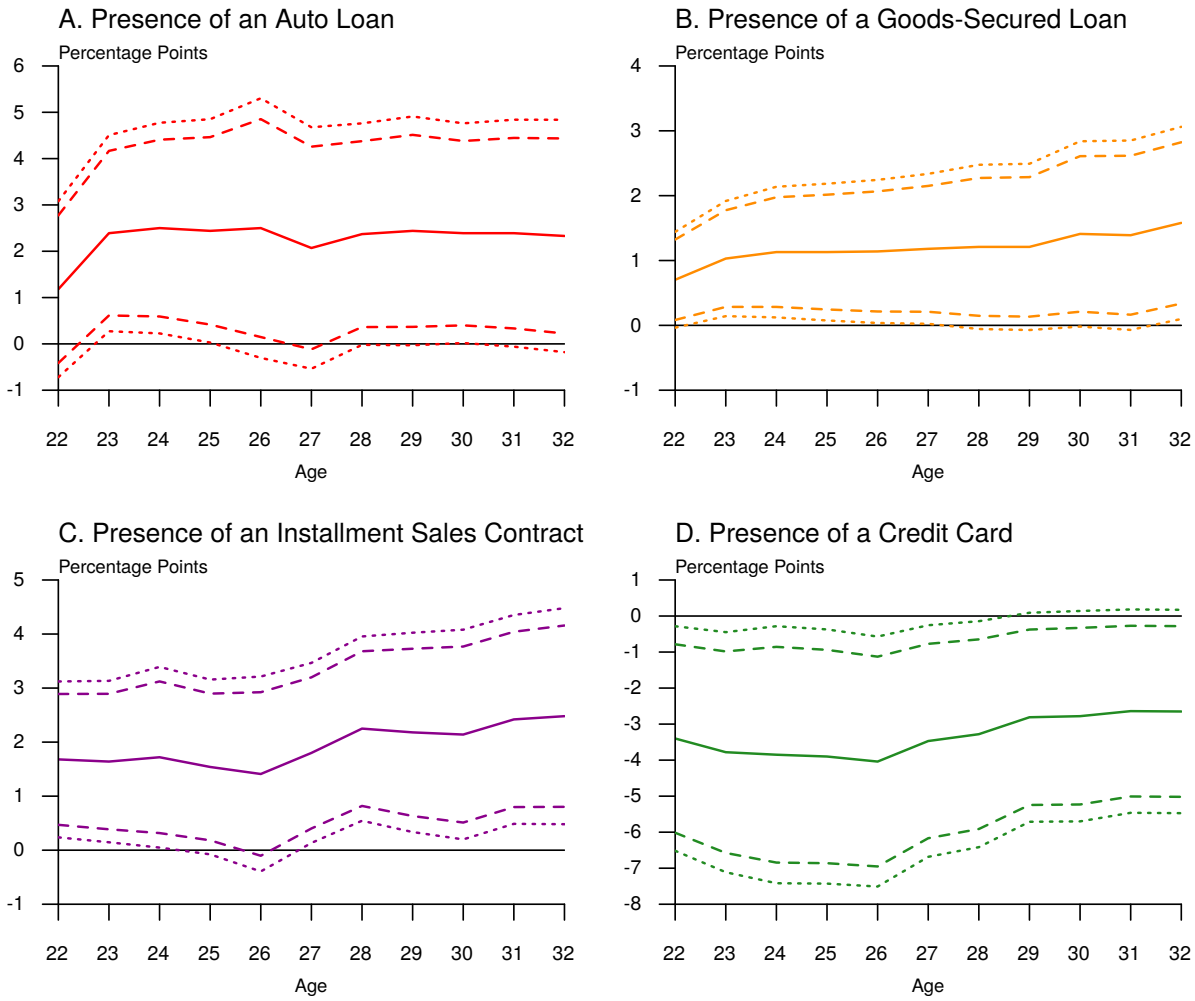
Note: This table reports 2SLS estimates of the effect of student loans on the probability of having a credit card by age 27. Student loans are instrumented for using the interaction between tuition and an indicator variable for whether the individual ever attended a Public 4-year college before age 23. See Tables 1 for variable definitions and 2 for sample selection and specification details. Student loans disbursed and tuition are recorded in 1000s of 2014 dollars. Standard errors in parentheses (clustered at the home state level). ***,**, and * denote significance at 1%, 5%, and 10%.

Figure A1: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, Whole Sample, 2SLS



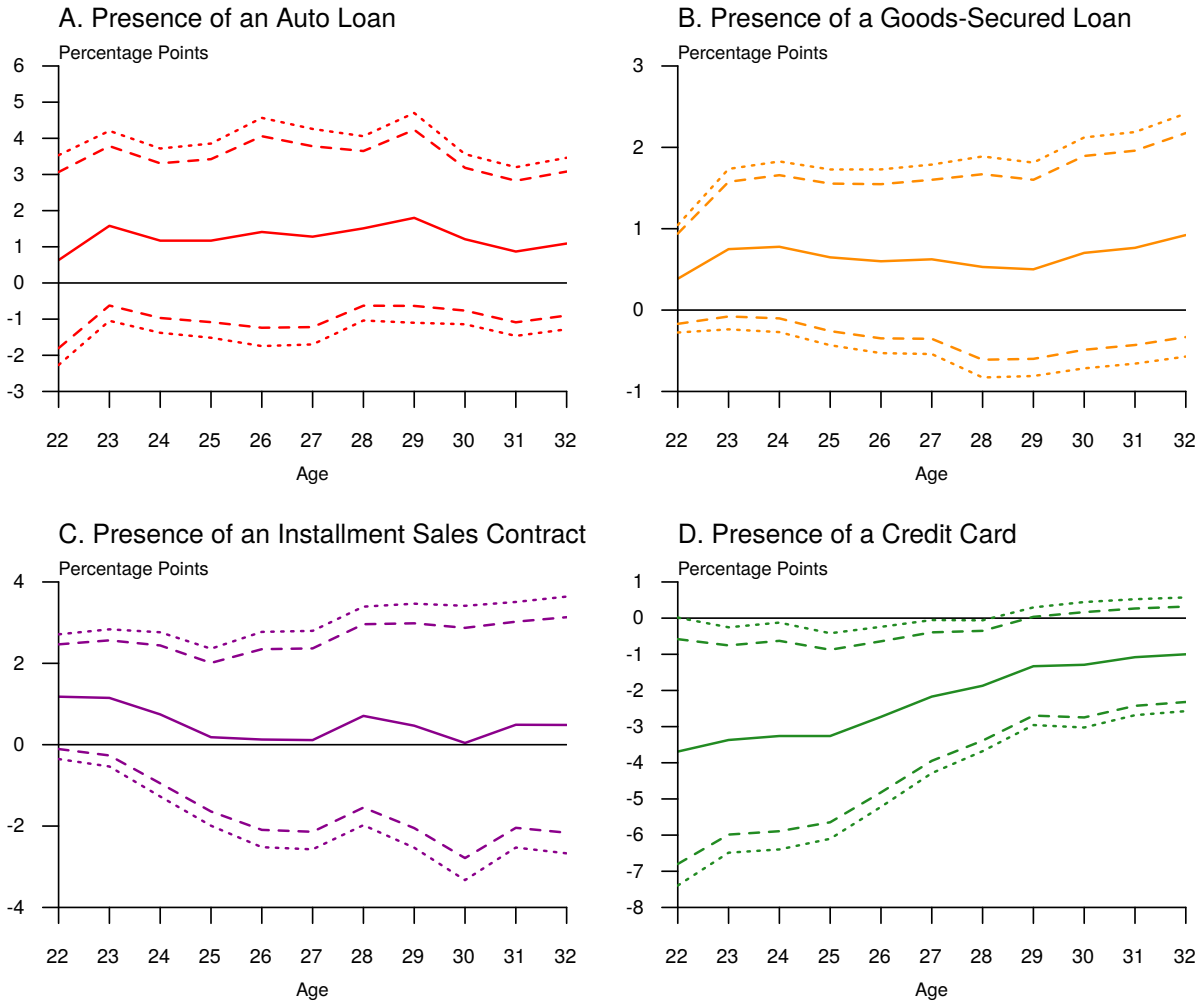
Note:Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31-year-old individuals with credit records in 2004 after applying the filters described Section 3, including those who received Pell Grants before age 23. The regressions include controls for tuition and whether the individuals ever attended a public 4-year university before age 23, several educational controls summarized in Table 1 and 14 college major indicator variables described in Table 7 of Mezza et al. (2020). The regressions also include home state-by-cohort fixed effects. For more details on the controls included, see column 4 of Table 2. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure A2: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, Excluding All Educational Controls, 2SLS



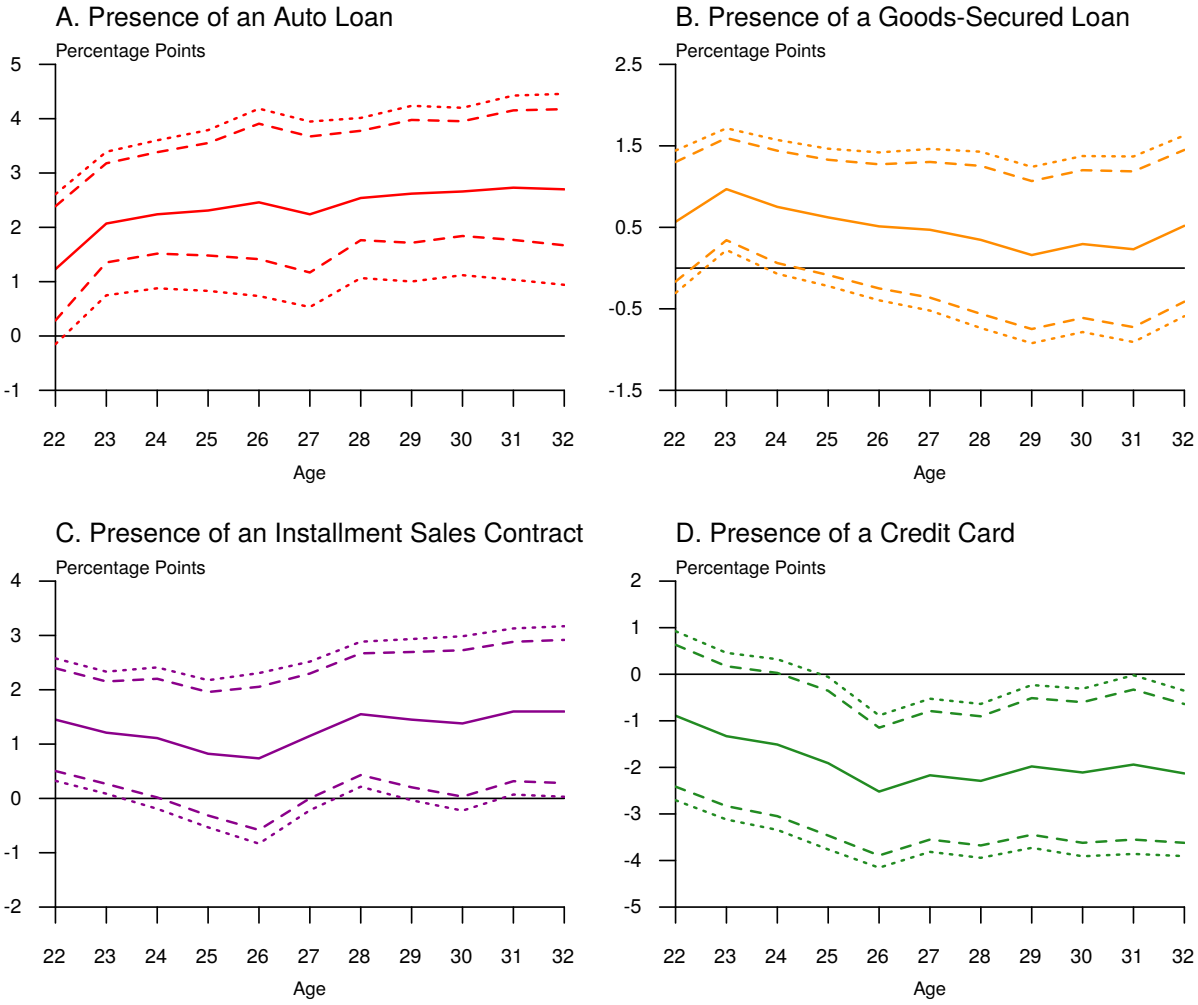
Note: Panels A, B, C, and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3, including those who did not receive Pell Grants before age 23. The regressions include controls for tuition and whether the individuals ever attended a public 4-year university before age 23 (but not any other educational controls), as well as home state-by-cohort fixed effects. For more details on the controls included, see column 4 of Table 2. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure A3: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, College-Goers only, 2SLS



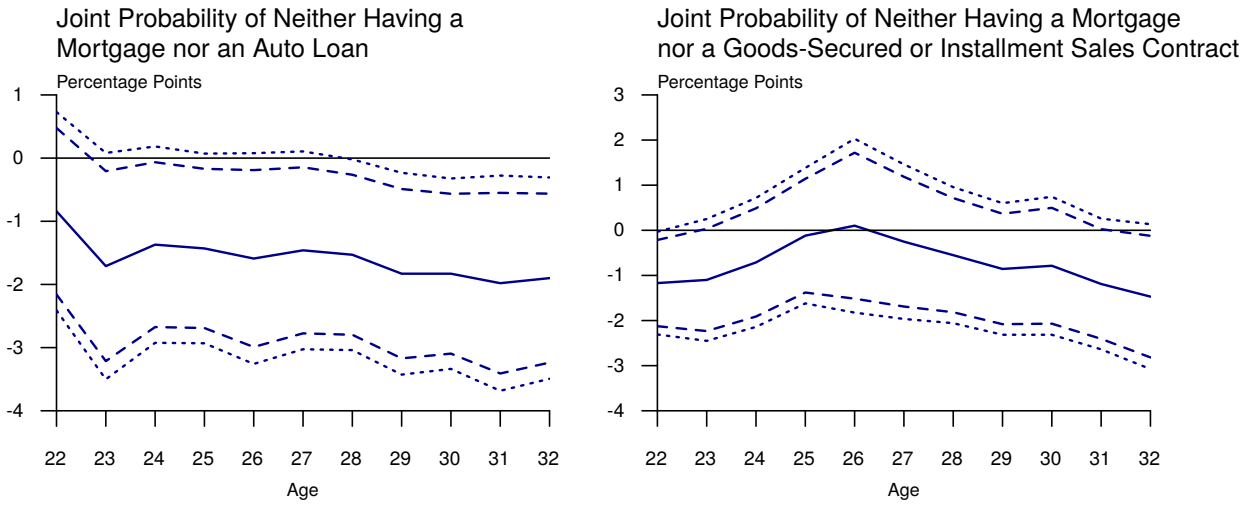
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3, excluding those who received Pell Grants or did not attend college before age 23. The regressions include controls for tuition and whether the individuals ever attended a public 4-year university before age 23, several educational controls summarized in Table 1 and 14 college major indicator variables described in Table 7 of Mezza et al. (2020). The regressions also include home state-by-cohort fixed effects. For more details on the controls included, see column 4 of Table 2. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure A4: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, IV-Probit



Note: Panels A, B, C and D display instrumental variable probit estimates of the marginal effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. The sample is all individuals from a nationally representative cohort of 23-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3, excluding those who received Pell Grants before age 23. The regressions include controls for tuition and whether the individuals ever attended a public 4-year university before age 23, several educational controls summarized in Table 1 and 14 college major indicator variables described in Table 7 of Mezza et al. (2020). The regressions also include home state-by-cohort fixed effects. For more details on the controls included, see column 4 of Table 2. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.

Figure A5: Joint Effects of a $1,000 Increase in Student Loan Debt on the Probability of Other Credit Outcomes, 2SLS



Note: Panels A and B display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of neither having a mortgage nor an auto loan and of neither having a mortgage nor a goods-secured or installment sales contract, respectively, by age. Marginal probabilities are estimated from a linear probability model, instrumenting for student loan debt with the interaction term between home-state tuition at public, 4-year universities and an indicator for having attended such a school. The sample is all individuals from a nationally representative cohort of 23-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3 and restricting the sample to those who did not receive Pell Grants before age 23. The regressions include the same controls as those in Figure 4. Student loan debt is recorded in thousands of 2014 dollars. Dotted and dashed lines represent 95 percent and 90 percent confidence intervals, respectively. Standard errors are adjusted for clustering at the home state level.