Student Loans, Access to Credit, and Consumer Credit Demand*

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February 2024

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.

*Corresponding author: kamila.sommer@frb.gov. A previous draft of this paper circulated under the title "Student Loans, Access to Credit and Consumer Financial Behavior." Haja Sannoh provided excellent research assistance. A special thanks is due to Karen Pence for help with attainment and construction of the data. We thank Neil Bhutta, Natalia Khorunzhina, David Low, Valery Polkovnichenko, Todd Stinebrickner, and Vincent Yao as well as the participants of the Copenhagen Business School's Department of Economics seminar, Kansas City FED seminar, and the 2019 European Economic Association in Manchester for helpful feedback. The analysis and conclusions contained in this paper are those of the authors and do not necessarily reflect the views of the Board of Governors of the Federal Reserve System, its members, or its staff. Mailing address of the Federal Reserve Board: 20th Street and Constitution Ave NW, Washington D.C., 20551.

1 Introduction

Student loan borrowing has risen rapidly over the past several decades. Outstanding student loan balances owed by U.S. households now total more than $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. This large, early life accumulation of debt may influence consumers' financial outcomes for many years to come. The ruling against broad-based student loan forgiveness by the Supreme Court in June 2023 re-ignites concerns about further increases in student loan indebtedness and underscores the need for a deeper understanding of the effects that student loan debt has on household finances.

A growing literature has investigated the effect of student loan debt, providing compelling evidence of a causal connection between various student loan-related interventions and subsequent equilibrium financial outcomes in distinct credit markets.[1] However, little research has been done to uncover the economic mechanisms behind these outcomes. While the literature generally frames the effects of student loan debt in terms of the constraints it imposes on individual borrowers' *access* to other forms of debt, standard models of consumer borrowing would predict an effect on borrowers' *demand* for other debts as well. Gaining an understanding of the deep structural forces—in particular, how the demand for and supply of consumer credit interact—is essential to predict the potential effects of future student loan debt policies.

[1] See for example Mezza et al. (2020), Bleemer et al. (2021), Herbst (2023), Mueller and Yannelis (2022), and Dinerstein et al. (2023), discussed in more detail below.

In this paper, we distinguish the effects student loan debts have on the supply of and demand for other forms of consumer credit. We provide the first evidence that increased student loan debt, caused by rising tuitions and accumulated early in life, actually increases borrowers' demand for additional debt in other consumer markets later on. Simultaneously, the additional student loan debt reduces borrowers' ability to access other consumer debt due to supply restrictions.

We start by showing in Section 2.2 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 non-permanent reduction in their disposable income, caused by the fixed schedule of student loan payments that they can neither easily refinance nor discharge. This positive demand effect pushes against a negative supply effect, as increased student loan debts tend to reduce their borrowers' access to other forms of credit.[2] Crucially, the model predicts that the sign of the net effect depends on the strictness of underwriting for consumer credit: In a tightly underwritten market the supply effect will dominate and the net effect of student loan debt on borrowing will be negative, while in a loosely underwritten market the demand effect will dominate and the net effect will be positive.

To test this prediction of the model empirically, we exploit the fact that different credit products vary by the stringency of their underwriting (documented in Section 2.3). Using quasi-experimental variation from changes in tuition charged at public schools, we show that variation in the stringency of underwriting across credit markets (i.e., auto loans, goods-secured loans, installment sales contracts, credit cards, and mortgages) determines whether

[2]See Mezza et al. (2020) for evidence that additional student loan debt has an adverse effect on borrowers' credit scores, for example.

the demand or supply channel dominates, and hence whether the net effect of student loan debt on the usage of other credit products is positive or negative.[3]

We estimate these effects of student loan debt on household borrowing using a 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 follow the identification strategy of Mezza et al. (2020), instrumenting for borrowers' student loan debts using changes in average in-state tuition at public 4-year universities from the subject's home state. To deal with state/year-level economic shocks, we compare students who attended those public schools (the treatment group, whose debt is a function of this charged tuition) 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 (i.e., over time) and between states, and within cohort and state between treatment and control groups. We discuss our estimator in detail in Section 4.2.

Consistency of this estimation approach requires that selection into the treatment group, i.e., attendance at a public 4-year university, is not endogenous to increases in tuition (our instrument). 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 in this same setting 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 these studies to identify the effects of interest. We reproduce those results in Section 4.2, and provide further evidence supporting the validity of the instrument in the Online Appendix.

[3]Auto loans, credit cards, goods-secured loans, and installment sales contracts, collectively represent approximately $2.8 trillion in debt.

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. In the *most* tightly underwritten market we consider, credit cards, we find that the same increase in student loan borrowing decreases the probability of having a credit card by as much as 2.4 percentage points, also consistent with the theoretical prediction of our model. 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.[4] 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.

Finally, we do not rule out the existence of other potential mechanisms by which student

[4]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 directly on the amount of the applicant's student loan debts.

loan debt could affect consumer borrowing. For example, 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)—as well as among student loan borrowers subject to the federal minimum wage (Gopalan et al. (forthcoming)). However, 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, where we find positive effects of student loan debt. Additionally, Rothstein and Rouse (2011) find that more indebted students from a highly selective university choose higher-earning careers post-college. However, a positive effect of student loan debt on income would not explain the negative effects we find on credit card holding and limits. Furthermore, there may be product-specific demand interactions that vary across credit markets in ways we do not model. While a variety of different mechanisms may be operating to some degree, the model we describe in this paper is the most parsimonious explanation consistent with our full set of results.

As student loan debt is a direct instrument of public policy, our results point to significant, and potentially unconsidered, welfare consequences of these policy choices. First, 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. Second, 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.[5] 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.[6] 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 (2022) 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.

Our paper contributes to a fast growing line of research in household finance investigating implications of student loan debt. The most closely related papers to this study are Mezza et al. (2020) and Bleemer et al. (2021), which show that increased student loan debt, driven by increases in tuition, causes a reduction in mortgage borrowing. In this paper, we offer a broader framework under which those and our new empirical findings may be understood, where student loan debt accumulation spills over into other credit markets, affecting both the supply of *and* demand for additional consumer credit. Using this framework, we find that supply-side effects, which prevail in the tightly-underwritten mortgage market, are dominated by demand-side effects in some other consumer credit markets where credit is more easily available (such as the auto loan market).

Two other strands of this literature have investigated the effects of student loan debt

[5]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.

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

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 related papers are Herbst (2023) and Mueller and Yannelis (2022). 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. Similarly, Dinerstein et al. (2023) estimate the effects of the recent federal student loan forbearance enacted in response to the COVID-19 pandemic on borrowers who originated their loans in the early 2000s.[7] Despite studying materially different interventions (a surpise reduction in payments during mid-life, as opposed to an anticipatable difference in debt levels established prior to joining the labor force), the effects of reducing payments estimated in those papers generally align with the effects of reducing loan balances estimated in the present paper and by Mezza et al. (2020). In particular, student loan delinquencies fall and the use of credit cards and mortgages increase when student loan burdens are eased, consistent with the credit supply mechanism we highlight.

In contrast to our findings, however, Mueller and Yannelis (2022) and Dinerstein et al. (2023) find that reduced payments lead to more auto borrowing, while Herbst (2023) does not find evidence of an effect. The difference between a mid-life shock to debt (as studied in these three papers) and an early life difference in initial debt (as we study) may be particularly relevant for the demand effects we posit are behind the auto loan findings. A payment reduction shock could make households already near the peak of life-cycle consumpion re-

[7]Another related paper is by Di Maggio et al. (forthcoming), 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 (2023), Mueller and Yannelis (2022), and Dinerstein et al. (2023) since the borrowers studied by Di Maggio et al. (forthcoming) were not making payments prior to the debt discharge, and so did not experience any direct increase in their disposable incomes.

optimize their consumption path, for which they require additional debt to immediately jump to that new, higher path. In contrast, our results suggest that young households facing the steep portion of the life-cycle income path prefer to take on more debt to smooth consumption as the disposable income-by-age gradient increases due to student loan payments. This difference in findings highlights how our study may be most relevant for anticipating the effects of future tuition inflation, for example, while this literature is directly relevant for considering unanticipated policy shocks to debt.

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. (2021), Black et al. (2020), Marx and Turner (2019), and Denning and Jones (2021)). 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.

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. (2023), Ehrlich and Perry (2020), Di Maggio et al. (2017), Abel and Fuster (2021) 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. Brevoort et al. (2020) and Bornstein and Indarte (2023) study the consumer credit market effects of reducing medical debt via Medicaid expansion. Other studies of shocks to wealth and debt include

Cookson et al. (2022) and Verner and Gyöngyösi (2020). As mentioned above, Aydin (2022) estimates the consumption and borrowing response to an exogenous increase in credit limits.

Across this literature, results suggest that the marginal effect of debt varies strongly with the household's economic circumstances. In this paper, we study effects on young households accumulating debt at the low point in their life-cycle earnings profile, and find much stronger effects than the literature that looks at households in mid life (e.g., Herbst, 2023, Mueller and Yannelis, 2022, or Dinerstein et al., 2023) and/or those who already own a home (Di Maggio et al., 2017 or Abel and Fuster, 2021). On the other hand, studies of households that qualify for Medicaid (i.e., those near or below the poverty level) find even stronger effects of debt on credit access than we do.[8]

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.

[8]Brevoort et al. (2020) find that access to Medicaid reduces medical debt by $900, leading to a 10 percent increase in the probability of being offered a credit card, while Bornstein and Indarte (2023) find that a 1 percent increase in the Medicaid coverage of the population leads to over a 1.2 percent increase in credit limits and a 0.7 percent increase in credit card borrowing. Together these findings suggest Medicaid expansion had a very large effect on access to consumer debt per affected individual, relative to the moderate decrease in medical debt it caused.

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.[9] 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.[10]

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

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

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

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 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.[11] 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

[11]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.

increasing their demand for debt.[12] 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).

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:

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

[12] 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.

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:[13]

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

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:

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

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

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

in bankruptcy and delinquent borrowers can have their wages garnished. Options to defer payments by refinancing are also limited.[14] 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.[15] 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]

[14] 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.

[15] Although we do not explicity model the effect of enrollment in income-driven repayment plans, these plans effectively function as a form of consumer debt by increasing disposable income today in exchange for reducing it in the future (as the payoff date of the student loan is delayed). As a substitute for consumer debt, we would expect enrollment in an income-driven repayment program to mitigate student loan debt's tendency to increase demand for additional consumer debt.

[16] Lenders may ration credit more tightly to individuals with high levels of student loan debts on their

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 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.[17] 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

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 access through a deterioration of credit scores.

[17] As on the demand side, we might expect enrollment in an income-driven repayment plan to mitigate the supply-side effects of additional student debt, depending on the mechanism by which additional student loan debt reduces $\bar{b}$. For example, lower scheduled monthly payments could help borrowers avoid delinquency, thereby preventing a blemish on the borrower's credit record and maintaining their access to other credit markets.

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.[18] 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.[19] 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.[20]

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

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

[19] 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.

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

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.[21] Mezza and Sommer (2016) show that these data match well to national statistics on attendance and degree attainment.

[21] All the merges of individual-level information have been performed by TransUnion, LLC, in conjunction with the National Student Clearinghouse and the Department of Education. The merges 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.

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 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.[22]

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 main estimation 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, in Section 4.4.2 we also estimate the effects of

[22]Mezza et al. (2020) find similar results of the effect of student loan debt on mortgage borrowing using both this federal student loan measure and a measure based on TransUnion credit records.

student loan debt on credit limits and utilization rates for credit cards, and for total balances for other debt products (i.e., non-revolving debt). 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 measure total balances for non-revolving debts, we need to adjust for the fact that many consumer credit products amortize very rapidly.[23] An installment sales contract, for example, could very easily have a loan term of less than two years and thus the consumer could borrow and pay back the loan entirely within the period between our biennial observations of their credit records. The current amount of consumer debt balances in a given time period thus offers a very poor measure of how much borrowing the consumer has engaged in to that point. However, the credit bureau data include a record of every loan reported to them by servicers, even if it had already closed by the date we get a snapshot of their records. These individual tradeline data include the opening date (which we use to construct our primary, extensive margin outcome variables) as well as the amount at origination. Using these data, we construct a measure of the cumulative total amount of new borrowing an individual has done in each credit product at each age from 22-32.[24]

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 in-state tuitions from

[23]Unlike auto loans, goods-secured loans, and installment sales contracts, credit cards are not installment loans with a fixed amortization schedule.

[24]For example, suppose an individual took out a $10,000 auto loan at age 24, paid it off over the next 5 years, and took out another $20,000 auto loan at age 30. We would measure their cumulative auto debt as $0 at age 23, $10,000 from ages 24 to 29, and $30,0000 from age 30 on.

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 TransUnion address information and the NSC and NSLDS school locations. Details on the procedure are provided in Appendix A.1. Data on the average in-state tuition at public 4-year universities by state and academic year are available from the National Center of Education Statistics. Average in-state tuition reflects the average undergraduate tuition and required fees.

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 and 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 0.05 percentage points.

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. For example, students with higher unobserved ability or greater expected future incomes may take on greater student loan debts (possibly by attending more expensive schools). Upon graduation, these students greater resources mean they have more access to credit than their poorly resourced peers, and so may be more likely to take on certain forms of debt—leading to

a positive bias in OLS estimates. Conversely, their higher incomes might mean less need for consumer credit to fund a particular consumption bundle—leading to a negative bias. Without being able to observe individuals' true ability, resources, and future incomes, the omitted variable bias could therefore be either positive or negative.

Furthermore, the magnitude and even the sign of the bias could vary by credit product type. Omitted variables include the socio-economic background of the borrower, and the direction of the correlation between these unobservables and the use of different forms of consumer debt could differ, product by product. Measures of socio-economic status tend to cluster, so that borrowers who can access credit products that are tightly underwritten (i.e., home mortgages and credit cards) likely have more unobservable advantages as well. Meanwhile, borrowers who make use of loosely-underwritten products, like installment sales contracts, may be more likely to have unobservable disadvantages.[25]

The sign and magnitude of the bias from these unobservables then depend on their further correlation with student loan debt. If larger amounts of student loan debt indicate the borrower has some unobservable advantages (i.e., they anticipate a higher income in the future), then we should expect a positive bias among tightly underwritten consumer credit products and a negative bias among loosely underwritten products. 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

[25]Consider the extreme case of pay-day loans (although we do not have data on this particular product). The fact that an individual has borrowed from a pay-day lender would be very informative of their socioeconomic status, relative to (for example) an individual who has taken out a home mortgage.

schooling.[26] 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.[27] 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, including the actual location of the school they ultimately attend.

The tuition public universities charge is likely tied to state-level economic performance. To avoid bias from local economic conditions, 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 from

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

[27] As described in Section 3 and Appendix A.1, the individual's home state is defined as their state of residence prior to any college attendance. This prevents any effect of tuition changes on the students' choice of which state to attend college in from introducing selection bias in our estimates.

home state s and birth year cohort t obtaining a tradeline by age a using equation 10:

$$Y_{ista} = \beta_0^a + \beta_1^a X_{ist} + \beta_2^a D_{ist} + \mathbf{W_{ist}}\boldsymbol{\beta_3^a} + \theta_{st}^a + \mu_{ista} \tag{10}$$

where Y_{ista} is a dummy variable indicating i (from state s and cohort t) has had the particular debt by age a. We also estimate specifications where we observe Y as a continuous variable: the credit limit, and utilization rates of all open credit card lines, and the balances on other consumer loans. X_{ist} is the amount of federal student loans borrowed by individual i prior to age 23, and D_{ist} is a dummy variable indicating i attended a public 4-year university before i turned 23. The vector $\mathbf{W_{ist}}$ can include a variety of controls at the individual level, and θ_{st}^a is a state-by-birth cohort fixed effect.

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

$$X_{ist} = \alpha_0 + \alpha_1 D_{ist} + \alpha_2 Z_{st} \times D_{ist} + \mathbf{W_{ist}}\boldsymbol{\alpha_3} + \phi_{st} + \epsilon_{ist} \tag{11}$$

where Z_{st} is the average tuition charged at public 4-year universities in i's home state s in the four school years following the school year in which members of i's cohort t turned 18. The interaction term, $Z_{st} \times D_{ist}$ (our instrument), is the only term excluded from equation 10 and ϕ_{st} is a state-by-birth cohort fixed effect.

The state-by-birth cohort fixed effect controls for correlations between tuition and consumer borrowering, while the term D controls for the average difference in outcomes between treatment and control groups. The effect of student loan debt on other borrowing behavior by age a, β_1^a, 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.[28]

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^a 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. We address these through a series of validity tests which closely follow those shown in 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.

We estimate the partial correlation between the instrument and various education outcomes in Table 2. Column 1 of Table 2 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.

[28]Recent econometric work, such as Goodman-Bacon (2021), has pointed out that studies using cross-sectional and cross-time variation to identify treatment effects in a two-way fixed effects framework can provide inconsistent estimates of average treatment effects. The bias arises from time varying treatment effects, when post-treatment observations are used as controls for other observations moving from pre- to post-treatment. This critique does not apply to our setting, which uses state-by-cohort fixed effects, rather than two way fixed effects. Treatment effects are identified by comparing the differences in outcomes between 4-year public school attendees and others of the same age in states and from birth cohorts that vary in the price of tuition.

The effect is also small and insignificant. Column 4 shows that there is little correlation between tuition and the receipt of a bachelors's degree. Columns 6 and 7 also show that tuition changes have no effect on attending a public 4-year university or gaining a bachelor's degree 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. Interestingly, Column 2 shows that there is a more sizeable (although not statistically significant) relationship between changing tuitions at *2*-year colleges and attendance at those schools. This finding is consistent with a literature that has found some evidence of attendance effects from tuition changes for low income or generally disadvantaged students, who are more likely to attend 2-year colleges (see Deming and Dynarski (2010) and Denning (2017), for example). Mezza et al. (2020) also show that tuition at public 4-year universities does not affect major choice either. In apparent contradiction with these results, recent work by Hampole (2023) suggests that a generous tuition aid policy did cause some change in major choice. It is possible that the relatively large dollar amounts of aid studied in Hampole (2023) (leading to an approximately $5,000 reduction in debt per recipient) contributes to this difference in findings. As we describe in the next section, the variation in student loan debt caused by tuition changes over the 4 years post-high school we exploit has a standard deviation of $915.

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 (following Mezza et al. (2020)) 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 2 show the effect of tuition on the probability of receiving any Pell Grants before age 23 for the full sample and the college-going subsample, respectively. Both estimates are small and statistically insignificant. Additionally, in Appendix A.2, we show that our results are robust to the inclusion of Pell Grant recipients in the estimation sample.

A further 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. Unfortunately, we cannot directly test for such a potential violation of our exclusion restriction. In Appendix A.2, we show an alternative specification which limits the control group to the subsample consisting only of college attendees. Estimates are less precise due to a necessarily smaller sample size, and some of the estimated effects are no longer statistically distinguishable from zero. However, the signs of the point estimates are all the same as those from our main specification. The magnitudes of the point estimates also vary around those from our main specification. This provides at least weak evidence 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 or control groups.

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 3. 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. 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. 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.[29] 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.

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

[29] 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.

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. Finally, in Section 4.4.2, we show estimates of the effect of student loan debt on the intensive margin use of non-revolving consumer credit products.

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, an additional $1,000 of 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. The baseline rates at which individuals use these forms of credit are shown in Figure 3. As a percentage of these baselines, the $1,000 increase in student loan debt causes a 5 percent increase in the probability of having an auto loan, a 20 percent increase in the probability of having a goods-secured loan, and a 10 percent increase in the

probability of having an installment sales contract.

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. As a percentage of the baseline probability of having a credit card, this represents a 2 to 3 percent decrease. 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.[30] 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 non-prime 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

[30] Herbst (2023) and Bleemer et al. (2021) find similar effects in different settings.

scores, in part through the channel of increasing delinquencies on such debt.

The 2SLS results shown in Figure 5 are substantially different in magnitude than the OLS results shown in Figure 4 (for credit cards, the sign is different as well). In addition to omitted variable bias discussed in Section 4.2, it is possible that the local average treatment effect (LATE) our estimator identifies is different than the population average treatment effect (ATE). That is, student loan borrowers whose debt is sensitive to public tuition changes (i.e., "compliers") may have a different response to that debt than their less sensitive peers. In Appendix Section A.4, we test for heterogeneous first stages and treatment effects of student loan debt across different subpopulations. While the first stage shows a similar strength across many different cuts of the data, we do find that the first stage is stronger for (and hence our estimated LATE more heavily weights) students who completed their degrees than those who did not. This is unsurprising, as these students spent different amounts of time in school paying tuition. However, we find economically significant second stages among both groups, and the results are not significantly different between the two groups. This is suggestive that the population ATE is unlikely to be radically different from our estimated LATE.

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.[31] 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 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.[32]

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.[33] 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

[31] 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.

[32] 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.

[33] That is, we re-estimate equation 10, defining the outcome variable Y_{ista} 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.

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.

In addition to an effect through student loan debt, it is possible that increased tuition affects consumer borrowing through a wealth effect. Students (or their parents) draw down their assets to pay for college and higher tuitions may leave them with less left over to fund consumption, causing them to turn to credit markets. In the context of the model presented in Section 2.2, such a negative wealth effect operates just as an effect of additional debt would. However, we lack data on students' assets to match our data on their liabilities so we cannot directly test this channel.

Instead, we can look directly at the reduced-form effect of tuition on our main outcome variables for the treatment and control groups. Figure 8 presents results of regressing indicators of using our various consumer debt products directly on the instrument and control variables. An additional $1,000 of tuition increases the use of goods-secured loans, installment sales contracts, and auto loans by around 0.1 to 0.5 percentage points, depending on the student's age and credit product. The use of credit cards is decreased by 0.2 to 0.5 percentage points by the same tuition hike. The estimated effect of the marginal dollar of tuition is considerably less than that of the marginal dollar of student loan debt. This is not surprising; as we explain in Section 4.3 many college students do not pay four full years of tuition immediately following high school.

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 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.[34] 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 9, 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.[35] 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

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

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

limits that constrain consumers' borrowing below their desired levels (see Fulford (2015) and Aydin (2022)). 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 9 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 (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.[36] The model thus predicts that increased student loan debt causes utilization rates to increase.[37]

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

[36] 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 $\frac{dU}{d\epsilon} \geq 0$

[37] 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.

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 9. 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.[38]

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 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

[38]We cannot definitively rule out demand responses as an explanation for the results in Figure 9: 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.

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 10. 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.[39]

[39]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

Finally, we consider the intensive-margin response of auto loans, goods-secure loans, and installment sales contracts. Using the intensive margin measure of borrowing described in Section 3 as our outcome variable, we estimate the effects of student loan debt in our 2SLS framework and present the results in Figure 11 (Panels A through C). Reassuringly, the results mirror those on the extensive margin. A $1,000 increase in student loan debt is estimated to cause additional auto borrowing in the amount of $350-$950 between ages 22 to 32, $7-$80 of good-secured borrowing, and around $50 of installment sales contracts. These results all match the predictions of the model. Note that these estimates include any change along the extensive margin, as non-borrowers are coded as having taken out zero debt. The precision of these estimates is low, however, particularly at later ages, possibly due to the greater variance in total borrowing amounts.

To understand the net effect on overall consumer debt balances (which we define in the cumulative sense, as described above, rather than as outstanding current levels), it is important to consider the role of mortgages. The effect on debt balances among the consumer credit products we consider in this paper are all positive or (in the case of credit cards) essentially zero. However, an increase in student loan debt also causes a reduction in the probability of getting a mortgage. Mortgage balances are so large (mortgages currently account for approximately two thirds of total household debt) that the net effect of an increase in student loan debt on total borrowing is decidedly negative. In Panels D, E, and F of Figure 11, we show the estimated effects on mortgage and total non-revolving consumer debt, including and excluding mortgages, respectively. A $1,000 increase in student loan borrowing by age 23 reduces average mortgage debt by over $10,000 by the borrower's early

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.

30s, likely driven in a large part by the extensive margin. The precision of the estimates is again low, however.

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 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. In this paper 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 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. Second, 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.

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 $1,000)	33,435	19.780	6.027	7.506	43.562
School Sector Controls					
Ever Public 4-Year	33,435	0.262	0.440	0	1
Ever Public 2-Year	33,435	0.248	0.432	0	1
Ever Private 4-Year Not-for-profit	33,435	0.116	0.320	0	1
Ever Private 2-Year Not-for-profit	33,435	0.008	0.087	0	1
Ever Private For-profit	33,435	0.047	0.211	0	1
Degree and Pell Grant Controls					
No College	33,435	0.458	0.498	0	1
Associate's/Certificate	33,435	0.030	0.171	0	1
Bachelor's	33,435	0.113	0.317	0	1
Master's or More	33,435	0.001	0.039	0	1
Degree of Unknown Type	33,435	0.008	0.088	0	1
Ever Pell	33,435	0.206	0.404	0	1
Year of Birth	33,435	1977.02	2.544	1973	1981
Ever Delinquent					
On Student Loans	33,435	0.149	0.356	0	1
On Auto Loans	33,435	0.032	0.175	0	1
On Installment Sales Contracts	33,435	0.019	0.136	0	1
On Goods-Secured Loans	33,435	0.003	0.055	0	1
On Credit Card Debts	33,435	0.188	0.391	0	1
Loan Balance ($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 ($s)					
Auto Loans	99,009	17,421.7	10,076.2	1	99,609
Installment Sales Contracts	13,966	2,813.4	5,144.0	1	95,518
Goods-Secured Loans	4,776	2,887.0	2,799.5	2	58,199
Loan Maturity (months)					
Auto Loans	97,179	55.6	15.7	1	99
Installment Sales Contracts	13,407	24.9	16.6	1	96
Goods-Secured Loans	4,734	26.3	12.5	1	96
Required Monthly Payment ($s)					
Auto Loans	95,497	374.4	325.5	1	9,999
Installment Sales Contracts	12,354	111.3	193.9	1	6,146
Goods-Secured Loans	4,231	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 measured as 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 for non-student loan debt 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. 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: 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%.

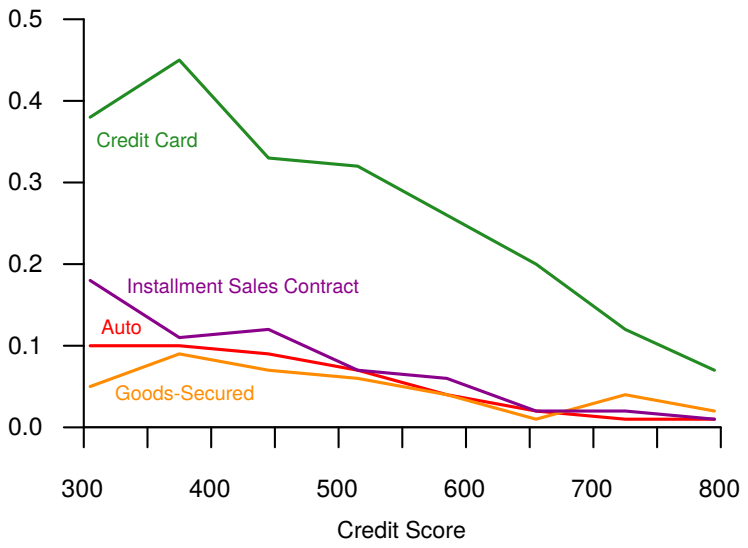
Table 3: Estimated Effects on Student Loan Amounts (First Stage)

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	0.082*	0.143***	0.172***	0.194***	0.138***
	(0.047)	(0.039)	(0.044)	(0.039)	(0.044)
Ever Public 4-Year	5.508***	1.761***	2.958***	−0.174	0.786
	(0.745)	(0.632)	(0.775)	(0.686)	(0.734)
No College		−1.991***		−2.804***	
		(0.337)		(0.366)	
Associate's/Certificate		−0.054		−0.882	−1.054
		(0.559)		(0.649)	(0.641)
Bachelor's		3.171***		1.586**	1.594***
		(0.642)		(0.601)	(0.589)
Master's or More		4.014**		2.293	2.329
		(1.926)		(1.968)	(1.975)
Degree of Unknown Type		−0.052		−0.820	−0.621
		(0.924)		(1.238)	(1.208)
Ever Public 2-Year		−2.394***		−2.003***	−1.748***
		(0.247)		(0.284)	(0.276)
Ever Private 4-Year Not-for-profit		8.081***		7.101***	6.964***
		(0.291)		(0.300)	(0.280)
Ever Private 2-Year Not-for-profit		1.803*		2.685**	2.483**
		(0.978)		(1.132)	(1.039)
Ever Private For-profit		1.897***		3.660***	3.682***
		(0.512)		(0.427)	(0.428)
Ever Pell		4.012***			
		(0.222)			
Constant	2.079***	1.998***	1.255***	2.792***	2.644***
	(0.077)	(0.284)	(0.060)	(0.302)	(0.253)
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

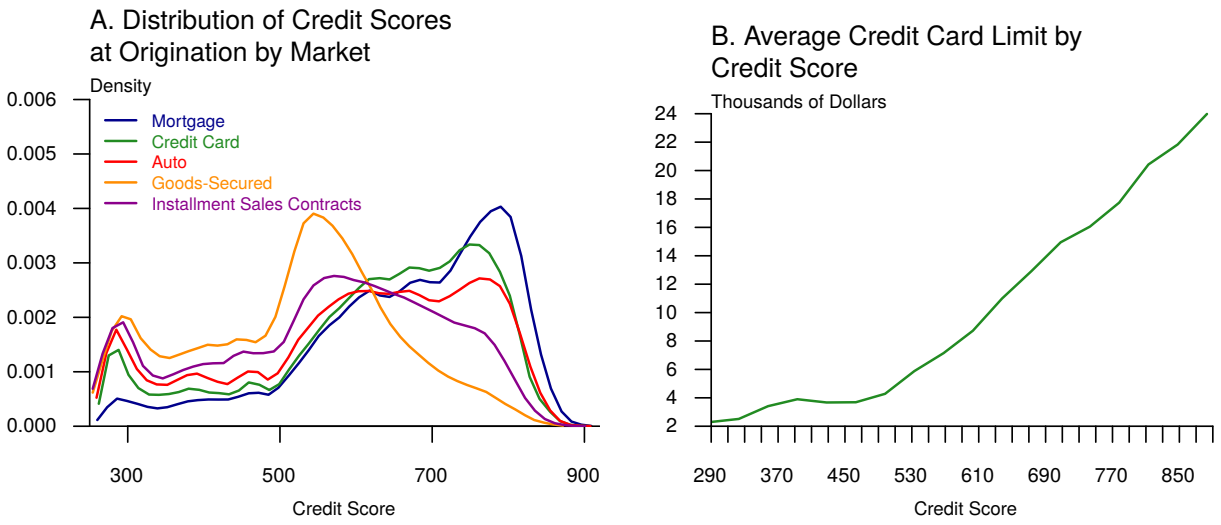
Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. Column (1) only controls for whether individuals ever attended a public 4-year college before age 23 and home state-by-cohort fixed effects. Sample is all individuals from 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



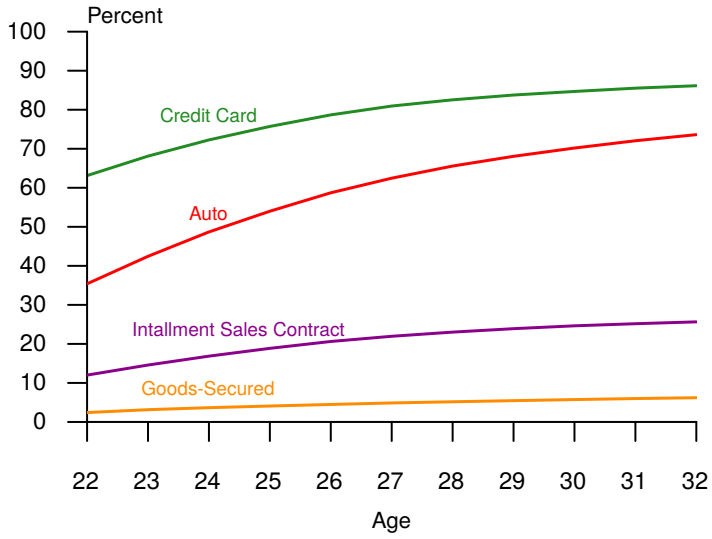
Note: Delinquency rates for each market correspond to the share of individuals who have ever been 90 days or more past due on each particular type of debt by age 32, plotted against individuals' credit score at age 22 (or at age 21 if credit score at age 22 is not available). The credit score ranges 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



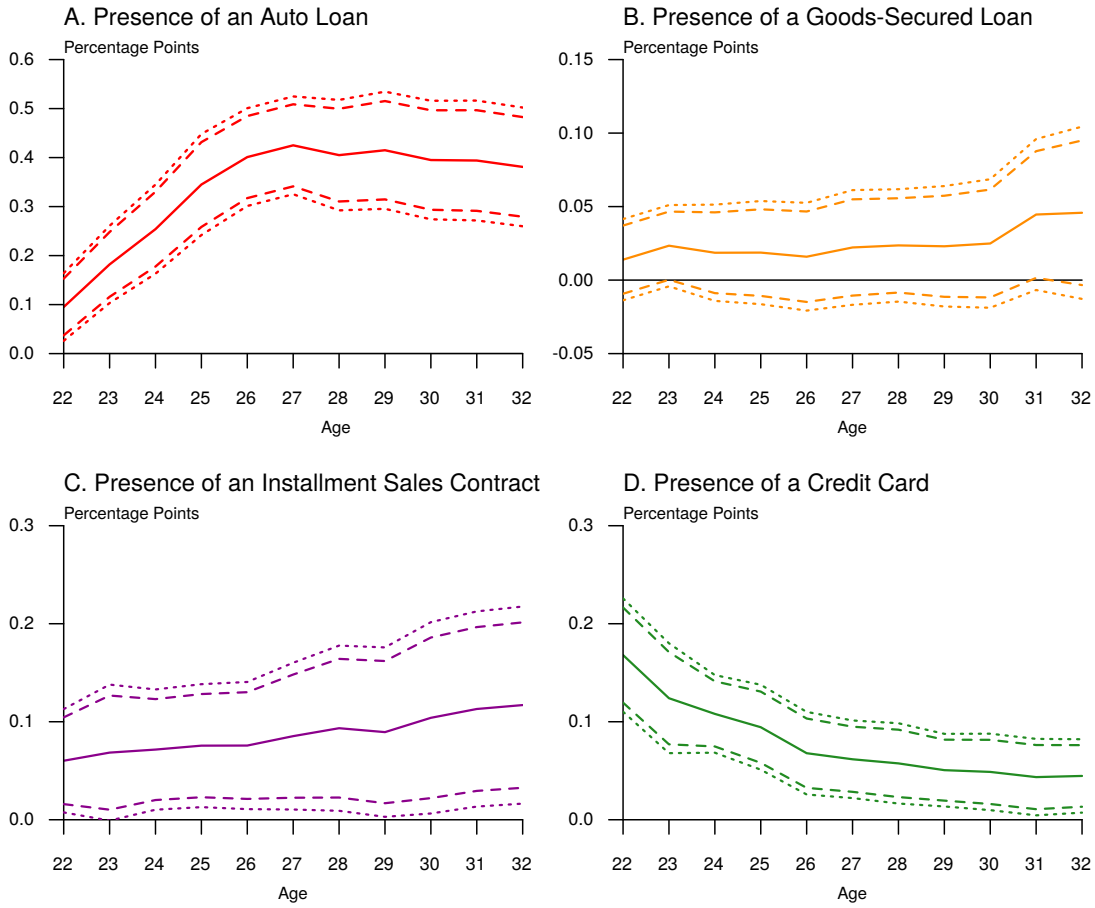
Note: Panel A displays kernel density distributions of credit scores by debt product. Credit scores are measured as of the most recent observation prior to or concurrent with origination, and the sample includes all loans originated after June 1997 for which a positive credit score observation is available. 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



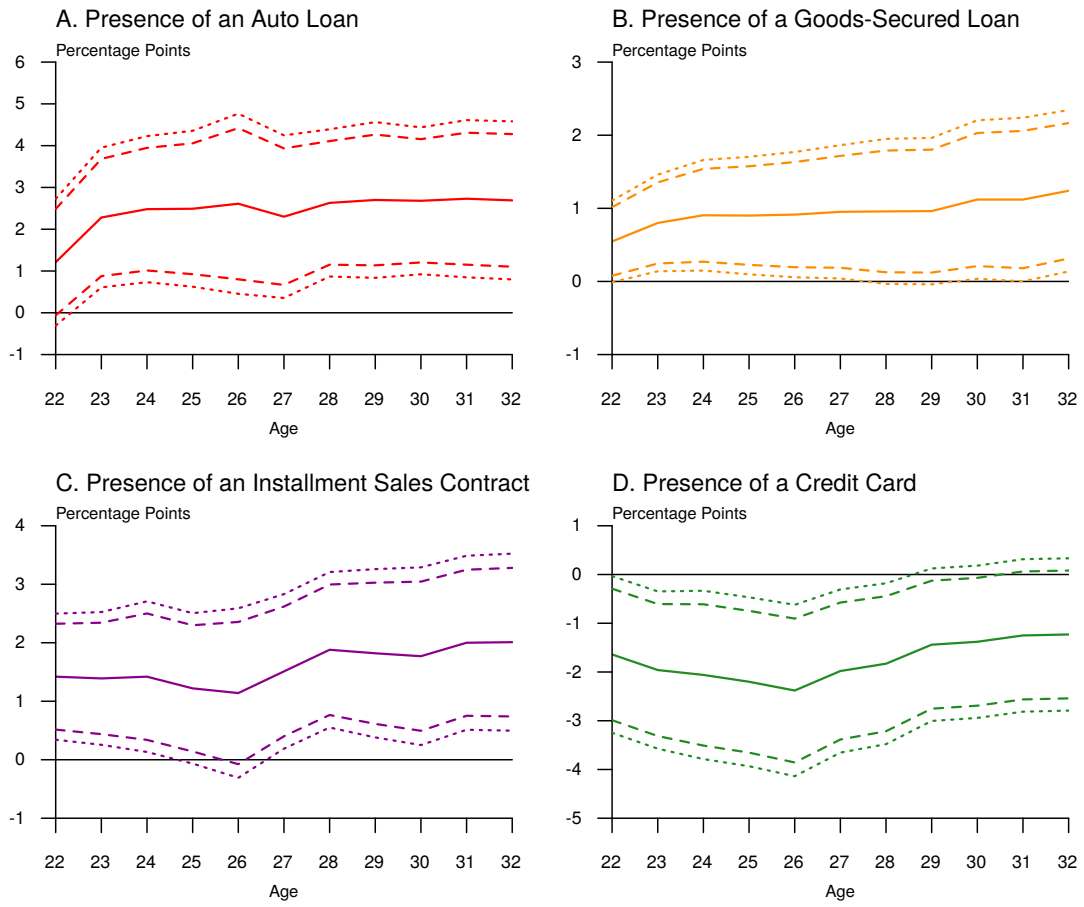
Note: The figure displays the percent of individuals in the sample that have ever had a credit card, an auto loan, a goods-secured loan, or an installment sales contract by age. The sample is all individuals from a nationally representative cohort of 23-31 year-old individuals with credit records in 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



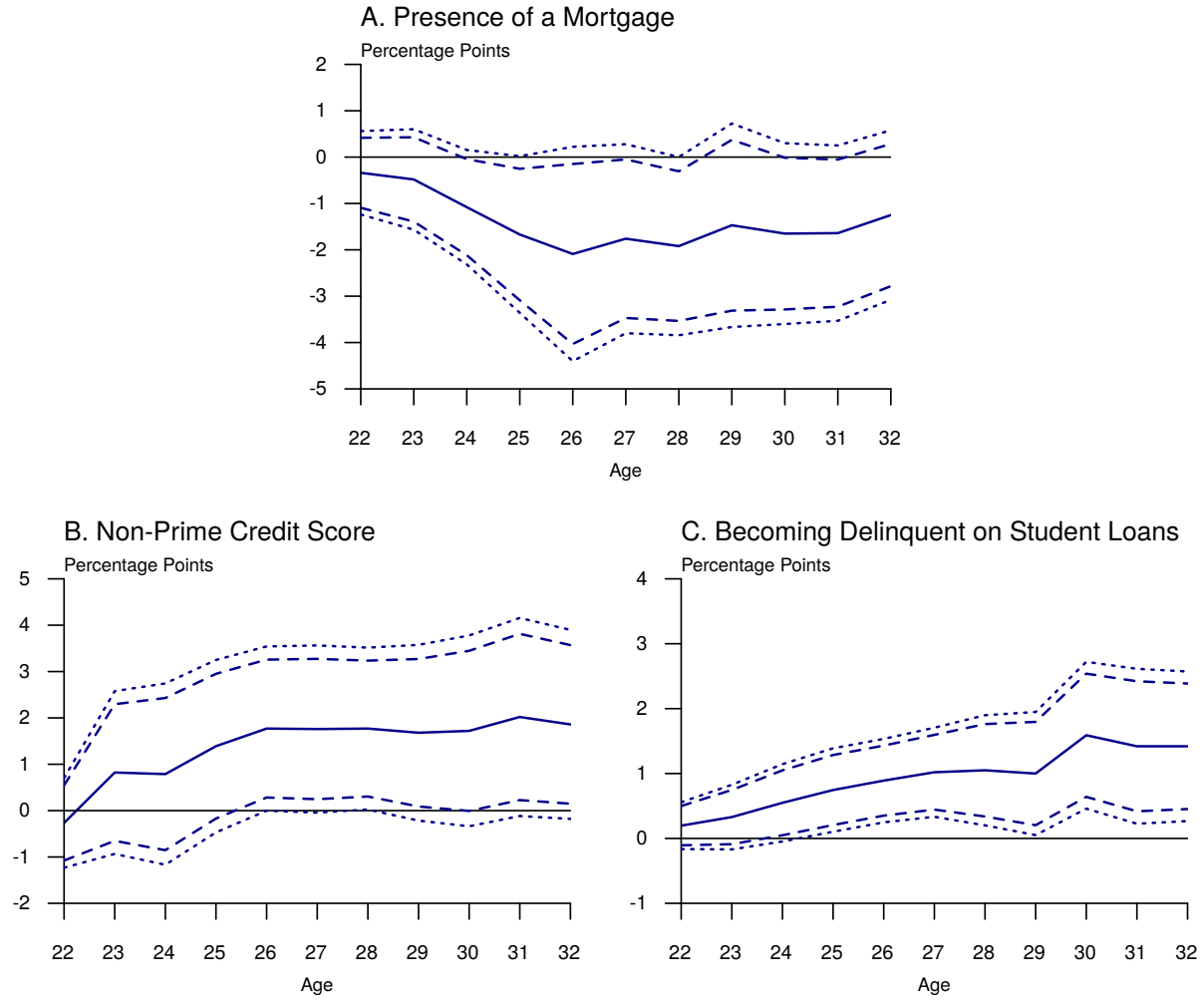
Note: Panels A, B, C, and D display OLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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 3. 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



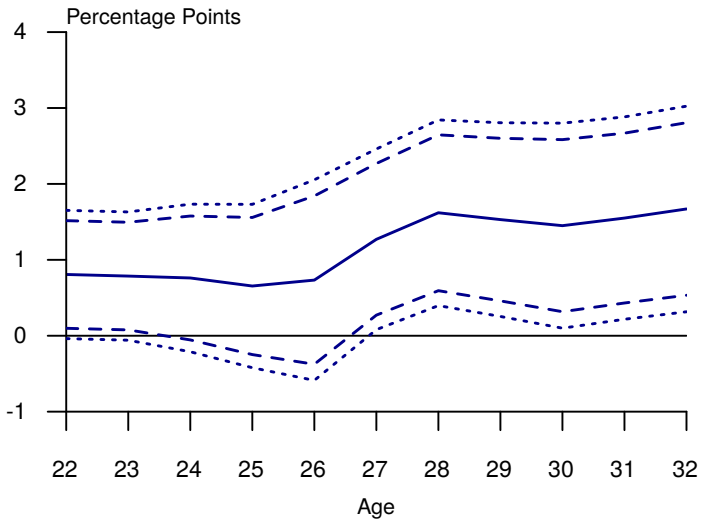
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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))



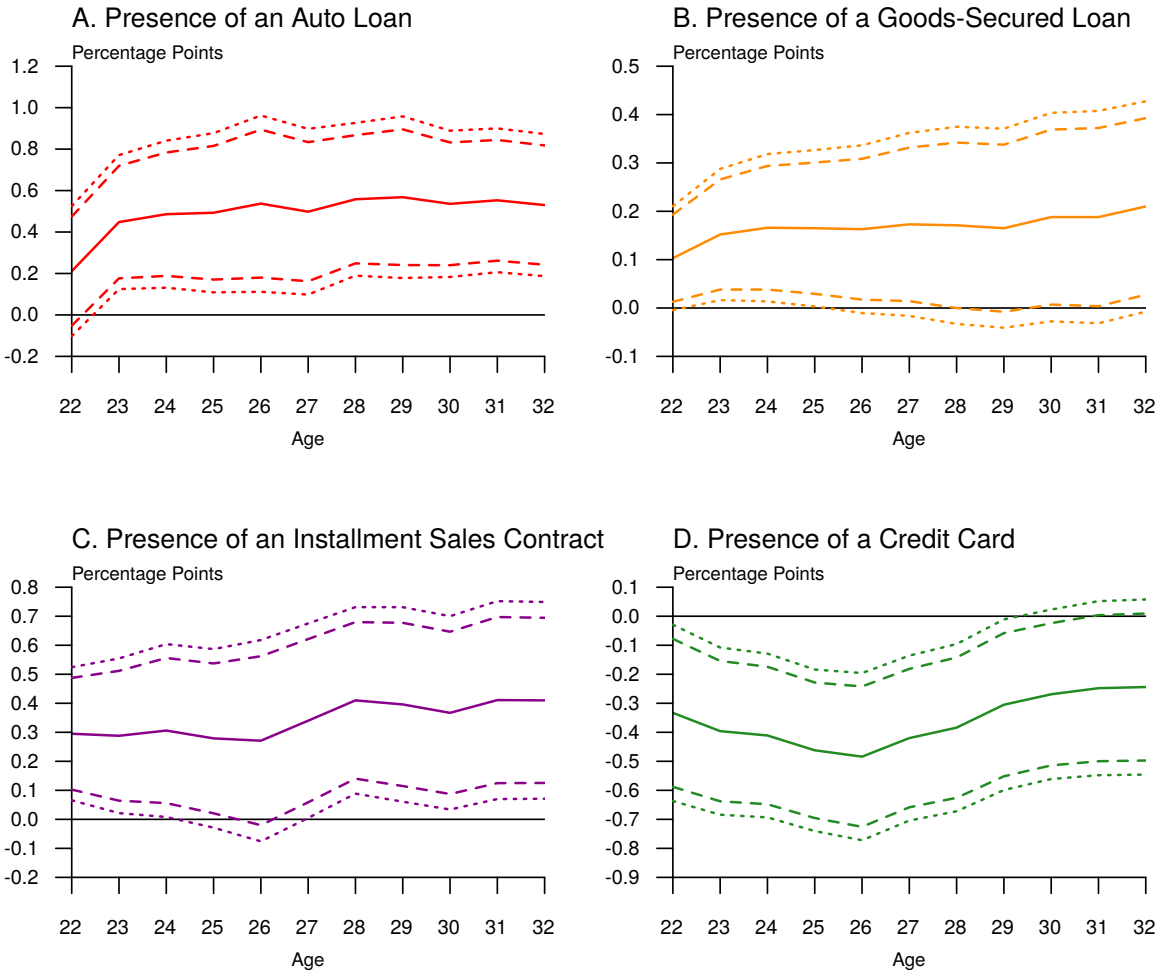
Note: Panels A, B, and C, display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having a mortgage, a non-prime credit score, and ever becoming delinquent on student loans, respectively, by age. Marginal probabilities are estimated from a linear probability 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



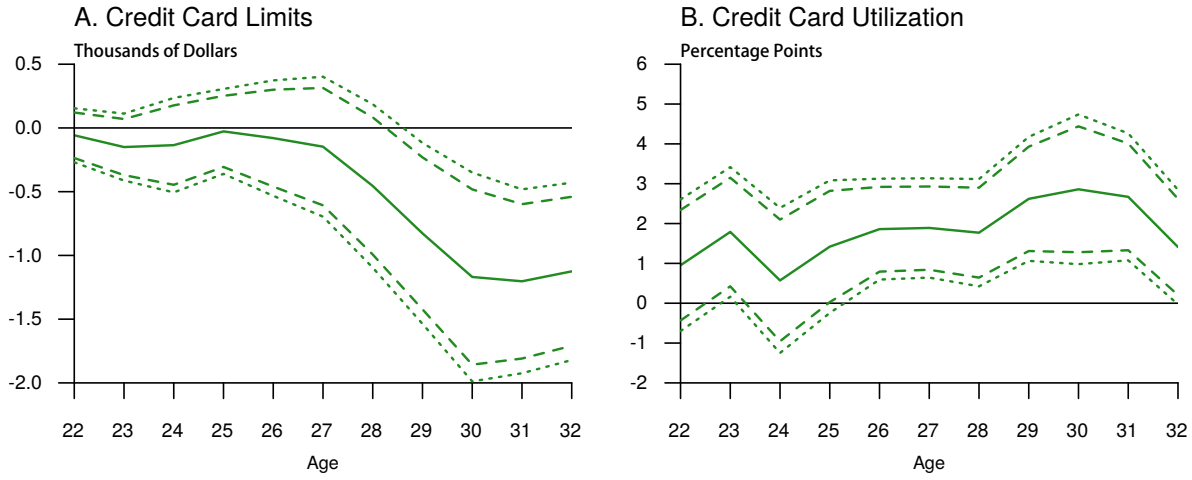
Note: The figure displays 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having both a credit card and either a goods-secured loan or installment sales contract, by age. Marginal probabilities are estimated from a linear probability model, instrumenting 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: Estimated Reduced-Form Effect of Instrument



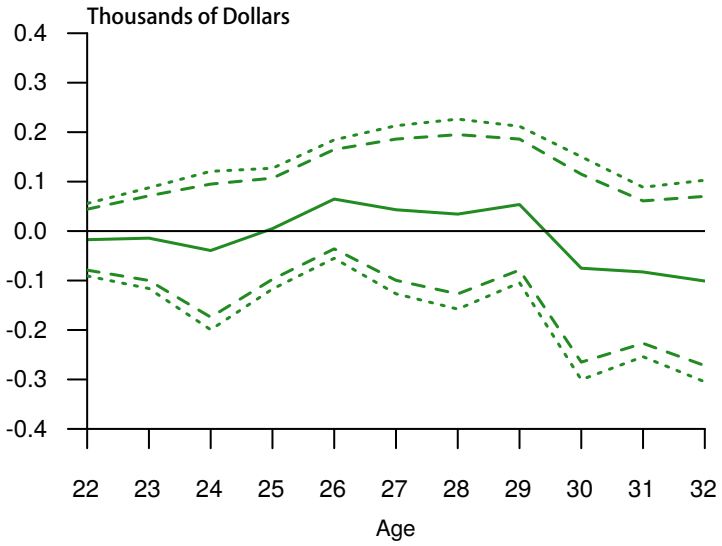
Note: Panels A, B, C, and D display OLS estimates of the effect of the interaction between tuition and an indicator variable for whether the individual ever attended a public 4-year college before age 23 on a presence of an auto loan, a goods-secured loan, an installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability model. 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. Tuition is measured 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 9: 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 10: 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.

Figure 11: Effect of a $1,000 Increase in Student Loan Debt on Intensive-Margin Outcomes, 2SLS

Note: Panels A, B, C, D, F, and E display 2SLS estimates of the effect of a $1,000 increase in student loans on the intensive-margin response of mortgage debt, auto debt, goods-secured loan debt, installment sales contract debt, total debt, and total debt excluding mortgages, respectively, by age. Intensive margin measures of borrowing are described in Section 3. Regressions instrument 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.

A Internet 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).[40] Hence, to estimate the model, we must impute home state for each subject in our sample, proceeding in two steps. First, for the 23 percent of individuals in the sample who have a record on a state of residence reported in the Transunion data prior to their first enrollment in college, we use this information to impute home state. Second, we split the individuals who did not have home state assigned in the previous step into two subgroups: (1) those who went to college (49 percent of the sample), and (2) those who did not go to college (28 percent of the sample).[41] 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. Mezza et al. (2020) show that results for mortgage borrowing are robust to an alternative home state definition, in which the first observed state in the TransUnion credit data is used to identify the home state for all individuals in the sample or in which students who took the SAT have their home state identified as the state in which they took the test.

[40] 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.

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

A.2 Validity and Robustness

In our main estimation sample, we drop Pell Grant recipients as these individuals' out-of-pocket education expenses are less affected by changes in the sticker price of tuition than a typical student. A potential concern, however, is that this exclusion could generate some sample selection bias in our estimates. 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.

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.[42]

A further 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. With a smaller sample, the estimates are less precise and many are no longer statistically distinguishable from zero. However, the signs are all the same as those from our main specification. The magnitudes of the point estimates, relative to our baseline findings, do vary

[42]Tables A1, A2, A3, and A4 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.

by product. In the college-only sample, the average estimated effect over the ages 22 to 32 for installment sales contracts is one third that of the full sample. The estimates for goods secured and auto loans are approximately one half and two thirds of their respective equivalents from the full sample, while the estimates for credit cards are 25 percent larger. Given the lack of precision, particularly in the more limited sample, this can only be a weak test of our contention that education group specific economic shocks are not accounting for our main results. Nevertheless, the directional similarity of the results from only college goers and our main sample fails to reject our identifying assumption that differential correlations of treatment and control group outcomes with tuition are due to the effect of student loan debt.

The combination of all these tests and those presented in Table 2 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 that limits the control group to the subsample consisting only of college attendees provides at least weak evidence that differences in tuition, rather than differences in exposure to economic shocks, are driving the outcomes.

Yet another potential threat comes from the possibility that tuition at public 4-year universities affects individuals in the control group (i.e., a SUTVA violation). Importantly, tuitions at public schools could affect private school tuitions in the same state. This could occur if the private and public sectors are competing for the same set of students and adjust prices in response to their competitors. To test for this, we estimate the correlation between tuition at public 4-year schools and private, non-profit 4-year schools by state and year. In Table A5, we show the results of regressing average tuition paid at private, non-profit 4-year schools from each individual's home state and cohort on the tuition charged at public 4-year

schools for the same group. Regressions include state and cohort fixed effects. The first column shows results for a regression where the unit of observation is every individual in our sample. A $1,000 increase in public school tuition is associated with $142 increase in private school tuition, and is far from statistically significant. In the second column we show the results of running this regression at the state/year level and find very similar results. It seems that private schools do not generally increase their tuitions to closely keep pace with local public schools. However, the standard errors for these estimates are large enough that we cannot rule out a fairly robust correlation between public and private tuitions.

Therefore, to ensure that our estimates are not being biased by the instrument affecting student loan borrowing in the treatment group, we re-run our main specification limiting the control group to individuals who never attended any college by age 23. Any change in private school tuitions correlated with the instrument could not affect this group. Results are shown in Figure A4. As can be seen, these estimates are very similar to our main results for which all non-4-year public school attendees form the control group. This suggests spillover from the instrument into the control group is not meaningfully biasing our results.

To further ensure that our results are not being driven by the treatment and control groups facing different labor makets, as a robustness check we drop from both the control and treatment groups any students who attended an out-of-state school by age 23. The main set of results for this subsample are presented in Figure A5, and they appear very similar to our baseline estimates.

Finally, the regression estimates for discrete outcomes presented in the main body of the text were estimated by two-stage least squares. For robustness, Figure A6 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.[43] 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

[43]Benmelech et al. (2023) 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 A7.

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. (2023) 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 A7.

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.

A.4 Heterogeneous Effects

A.4.1 The First-Stage Effects

We should not expect all students' student loan borrowing to respond to tuition increases in the same way. As a result, our estimates are implictly weighted toward the treatment effect of the population whose debt is responsive to the instrument (i.e a LATE). We therefore test for heterogeneity in the strength of the first stage. We found the strength of the first stage to be fairly consistent across a number of different divisions of the data that might

have been expected to have different treatment effects. We estimate first stage effects by bifurcating the treatment group and estimating separate regressions according to: whether the student finished a degree, majored in a high- or low-earning subject, their neighborhood income, and whether the individual had a prime versus a non-prime credit score (as defined in Section 4.4.1) by age 23 or 24, depending on the age at which we have an observation for that person.[44] As we discuss in the next subsection, some of these sample splits are potentially endogenous to tuition changes, but the results may still provide some insight. In all these cases the entire control group was used as a comparison. First stage results for these subsamples are shown in Tables A6, A7, A8, and A9, respectively. We did find a substantial difference in the first stage when we split the sample according to whether the borrowers finished their degrees. We find a $1,000 increase in tuition is associated with $269 extra debt for those with a degree, and only $144 extra debt for those who did not finish. This result is unsurprising, as students who did not receive their degree spend fewer semesters in school paying tuition, so their total student loan debt is less sensitive to tuition increases.

A.4.2 The Second-Stage Effects

We also should not expect all borrowers to react to increased student loan debt in the same way. From the model in Section 2.2, for example, we would expect credit-constrained borrowers to have a larger, negative borrowing response to tightened credit limits. The ideal

[44]We follow as close as possible the definition of majors presented in Table D2 in Hampole (2022) given our definition of majors found on Table 7 of Mezza et al. (2020). More specifically, we group the following as high earning majors: (3) biological, biomedical, and nature conservation studies; natural sciences; agriculture; (5) computer and information systems; (9) engineering; engineering technologies and trades; mechanic and repair technologies; precision production; (10) business, management, and marketing; and (11) health professions and related sciences. Low earning majors are the following: (1) architecture and urban planning; construction trades; (2) English, foreign languages and literatures; visual and performing arts; philosophy, religion, and theology; (4) communications and journalism; communications technologies and technicians; (6) criminal justice; (7) economics; geography, history, political science, sociology, and social sciences; psychology; area, ethnic, and gender studies; parks, recreation, and leisure studies; family and consumer sciences; (8) education; (12) legal professions and studies; (13) public administration and social work; (14) liberal arts and sciences; (15) personal and culinary services; transportation and materials moving; other.

experiment to test the model in this way would be to compare the effects of student loan debt on i) a population that is, for exogenous reasons, on the margin of denial for each of the credit products we consider against those from ii) a population that is (exogenously) very far from marginal. We lack such a clean division. However, we can test the relative effects by the borrower's credit score. This score may contain considerable information regarding who would be on the margin of credit approval even in the absence of tuition changes. However, it has significant deficits. One issue is that the score is measured after the initial student loan borrowing decision for most of our sample, and so is itself an endogenous outcome of student loan debt. Furthermore, credit scores may reflect liquidity constraints of the borrower, as well as indicating credit constraints. A borrower may have a low score because they had insufficient cash to make all their debt payments, for example. These liquidity constraints could interact with student loan debt to produce different demand effects in addition to any difference in supply effects across populations.

With these caveats in mind, we split the treatment population sample according to whether the individual had a prime versus a non-prime credit score (as defined in Section 4.4.1) by age 23 or 24, depending at which age we have an observation for that person. We then estimate treatment effects of increased student loan debt on both these subgroups (using the same control group). Results are presented below in Figures A8 and A9 for the prime and non-prime groups, respectively. The effect of student loan debt on the presence of a credit card appears more negative for the non-prime group, as would be expected if the supply effect dominated in this product. Interestingly, it appears that the positive effect on the presence of an auto loan, goods-secured loan or installment sales contract is stronger among the non-prime score population as well. In addition to any heterogeneity of the elasticity of demand for credit, it is possible that there is some substitution effect. That is,

low-score borrowers excluded from credit cards turning to more loosely underwritten forms of credit. However, as substitution between credit cards and auto loans is unlikely, this finding suggests some heterogeneity in the elasticity of demand as well. It is possible that liquidity constrained individuals are more likely to be on the margin between buying a very low-priced, low-quality car directly from another owner versus buying from a dealer and using an auto loan, and additional student loan debt payments could push them into demanding a loan. Less constrained borrowers, in contrast, may rarely consider that path, and the different estimated effects on auto loan borrowing reflect this difference. Of course, all of the above is merely suggestive as the different estimates across the prime and non-prime populations are not significantly different from each other.

We also estimate different treatment effects by neighborhood income. We split our treatment sample into two groups, based on the median income of the census tract we first observe associated with them in our data. Tracts are either above- or below-average income, based on a comparison to area median family income. In our data, the correlation between living in a below-average income neighborhood and having a non-prime credit score by age 24 is approximately 0.1, suggesting neighborhood income may be a weak proxy for the individual being on the margin of credit denial. Neighborhood income also shares an endogeneity concern with individual credit score, as student loan debt can affect where the borrower chooses to live (in no small part by affecting their access to mortgage credit). Furthermore, neighborhood income is also very likely to be correlated with liquidity constraints as well as credit constraints. Lacking the ideal experimental set up, however, imperfect tests must suffice. Results for this sample split are shown in Figures A10 and A11 for above- and below-average income areas, respectively. The effect of additional student loan debt on the presence of a credit card is initially similar between the two groups but starts to diverge as

the borrowers enter their late 20s, with borrowers in higher-income neighborhoods showing less of an effect. This finding is again consistent with the idea that less-advantaged borrowers are more likely to be credit constrained and hence the supply effect is more important for them. We again see a more positive effect on the presence of an auto loan among borrowers from lower-income neighborhoods, further suggesting a potential interplay of demand effects with liquidity constraints in addition to any differences in credit constraints. As with the sample split by credit score, differences between the high- and low-income neighborhood populations are not statistically significant.

Finally, we test for differences by whether the student finished their degree and college major. For the degree, results are shown in Figures A12 and A13 for those who finished versus did not finish their degrees, respectively. Again, we find some evidence of stronger effects (both positive and negative) of student loan debt on our primary debt outcomes for borrowers who did not finish their degree. This difference is never close to statistical significance, however. Lacking a degree, these borrowers likely have lower incomes and it would be unsurprising that both their demand for credit and the degree to which they are bound by credit constraints are more sensitive to the marginal dollar of student loan debt. If so, it would suggest that the LATE our estimator provides is somewhat biased toward higher-earning borrowers with more muted responses to student loan debt. For college major, results are shown in Figures A14 and A15 for high-earning and low-earning majors, respectively. As with the previous cuts of the data, differences in the estimated effects between these groups are not statistically significant.

Table A1: Effect of 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	0.034	0.024**	0.023*	0.026***	0.017
	(0.024)	(0.012)	(0.012)	(0.010)	(0.014)
Ever Public 4-Year	−0.154	−0.091**	−0.038	−0.068**	−0.044
	(0.163)	(0.046)	(0.073)	(0.030)	(0.044)
No College		−0.059*		−0.051	
		(0.032)		(0.036)	
Associate's/Certificate		0.215***		0.224***	0.209***
		(0.027)		(0.028)	(0.027)
Bachelor's		0.061		0.057*	0.068*
		(0.049)		(0.032)	(0.035)
Master's or More		0.092		0.088	0.091*
		(0.066)		(0.061)	(0.055)
Degree of Unknown Type		0.161***		0.178***	0.163***
		(0.043)		(0.047)	(0.043)
Ever Public 2-Year		0.091**		0.074**	0.057*
		(0.037)		(0.029)	(0.030)
Ever Private 4-Year Not-for-profit		−0.231**		−0.240***	−0.184*
		(0.097)		(0.069)	(0.095)
Ever Private 2-Year Not-for-profit		−0.016		−0.063	−0.024
		(0.040)		(0.058)	(0.060)
Ever Private For-profit		−0.028		−0.050	−0.011
		(0.024)		(0.035)	(0.051)
Ever Pell		−0.111**			
		(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 3 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 A2: Effect of 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	0.017	0.009*	0.011*	0.009*	0.005
	(0.012)	(0.005)	(0.006)	(0.005)	(0.006)
Ever Public 4-Year	−0.145*	−0.055***	−0.094***	−0.048***	−0.034*
	(0.082)	(0.020)	(0.035)	(0.016)	(0.019)
No College		0.034***		0.038**	
		(0.012)		(0.015)	
Associate's/Certificate		0.006		0.011	0.007
		(0.015)		(0.020)	(0.020)
Bachelor's		−0.035		−0.017	−0.011
		(0.021)		(0.016)	(0.016)
Master's or More		−0.030		−0.005	−0.001
		(0.028)		(0.035)	(0.032)
Degree of Unknown Type		0.001		0.015	0.015
		(0.022)		(0.027)	(0.025)
Ever Public 2-Year		0.030**		0.022*	0.011
		(0.015)		(0.012)	(0.012)
Ever Private 4-Year Not-for-profit		−0.087**		−0.076**	−0.045
		(0.039)		(0.034)	(0.041)
Ever Private 2-Year Not-for-profit		−0.023		−0.029	−0.002
		(0.015)		(0.025)	(0.021)
Ever Private For-profit		−0.012		−0.026	−0.011
		(0.010)		(0.019)	(0.025)
Ever Pell		−0.028			
		(0.021)			
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 3 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 of 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	0.021	0.013	0.020**	0.018**	0.003
	(0.021)	(0.010)	(0.010)	(0.008)	(0.014)
Ever Public 4-Year	−0.206	−0.099**	−0.175***	−0.091***	−0.042
	(0.140)	(0.040)	(0.055)	(0.025)	(0.042)
No College		0.040		0.060**	
		(0.029)		(0.028)	
Associate's/Certificate		0.034		0.053	0.040
		(0.030)		(0.038)	(0.038)
Bachelor's		−0.047		−0.027	0.003
		(0.043)		(0.028)	(0.035)
Master's or More		−0.093		−0.101*	−0.044
		(0.061)		(0.057)	(0.059)
Degree of Unknown Type		0.010		0.045	0.040
		(0.044)		(0.053)	(0.045)
Ever Public 2-Year		0.047*		0.039*	0.026
		(0.027)		(0.021)	(0.027)
Ever Private 4-Year Not-for-profit		−0.125		−0.144**	−0.040
		(0.081)		(0.058)	(0.094)
Ever Private 2-Year Not-for-profit		−0.036		−0.062	−0.012
		(0.033)		(0.052)	(0.054)
Ever Private For-profit		0.026		0.010	0.062
		(0.023)		(0.041)	(0.056)
Ever Pell		−0.025			
		(0.039)			
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 3 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 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	−0.073	−0.028**	−0.035**	−0.022**	−0.025**
	(0.057)	(0.014)	(0.016)	(0.009)	(0.012)
Ever Public 4-Year	0.698*	0.186***	0.421***	0.126***	0.130***
	(0.387)	(0.056)	(0.099)	(0.031)	(0.039)
No College		−0.255***		−0.267***	
		(0.033)		(0.031)	
Associate's/Certificate		0.034		0.014	0.008
		(0.022)		(0.025)	(0.028)
Bachelor's		0.110*		0.050	0.055
		(0.059)		(0.035)	(0.038)
Master's or More		0.137*		0.075	0.084
		(0.078)		(0.062)	(0.069)
Degree of Unknown Type		0.026		0.016	0.012
		(0.030)		(0.027)	(0.031)
Ever Public 2-Year		−0.078**		−0.059***	−0.057***
		(0.034)		(0.019)	(0.022)
Ever Private 4-Year Not-for-profit		0.267**		0.178***	0.198**
		(0.112)		(0.064)	(0.087)
Ever Private 2-Year Not-for-profit		0.037		0.031	0.055
		(0.046)		(0.045)	(0.049)
Ever Private For-profit		0.022		0.083**	0.102**
		(0.030)		(0.042)	(0.052)
Ever Pell		0.097*			
		(0.059)			
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 3 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 Tuition at Public, 4-year Schools on Average Tuition Paid at Private, Non-profit 4-year Schools

Variable	**Individual 1**	**State-Year 2**
Variable	(1)	(2)
Tuition at public 4-year colleges	0.142	0.134
	(0.138)	(0.216)
Cohort FEs	YES	YES
Home State FEs	YES	YES
Observations	33,372	500
R-squared	0.825	0.646

Note: This table shows the results of regressing average tuition paid at private, non-profit-4-year schools from each individual's home state and cohort on the in-state tuition charged at public 4-year schools for the same group. Regressions include cohort and year fixed effects. The first column shows results for a regression where the unit of observation is every individual in our sample, while the second column is at the state/year level.. ***,**, and * denote significance at 1%, 5%, and 10%.

Table A6: First Stage for Those with and without a Degree

Total Federal Student Loans Disbursed before Age 23		
Variable	**With Degree**	**Without Degree**
	1	**2**
Instrument: Tuition x Ever Public 4-Year	0.269***	0.144***
	(0.0496)	(0.0445)
Ever Public 4-Year	-1.370	0.567
	(1.061)	(0.649)
No College	-2.727***	-2.919***
	(0.441)	(0.405)
Associate's/Certificate	-0.824	-0.117
	(0.655)	(0.863)
Bachelor's	1.514**	2.454***
	(0.621)	(0.844)
Master's or More	2.112	2.854
	(1.995)	(4.103)
Degree of Unknown Type	-0.903	0.410
	(1.230)	(1.597)
Ever Public 2-Year	-2.024***	-2.168***
	(0.383)	(0.310)
Ever Private 4-Year Not-for-profit	7.401***	6.948***
	(0.408)	(0.373)
Ever Private 2-Year Not-for-profit	2.421*	2.806**
	(1.261)	(1.177)
Ever Private For-profit	3.761***	3.474***
	(0.500)	(0.413)
Constant	2.719***	2.914***
	(0.409)	(0.349)
College Major Controls	YES	YES
Home State by Cohort FEs	YES	YES
Observations	23,181	24,251
R-squared	0.347	0.325

Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. 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. In addition, column (1) limits the sample to those who finished a degree and column (2) to those who did not in the treatment group. The control group in both columns include all individuals who have not attended a public 4-year college before age 23. The regressions include the same controls as those in Figure 4. 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 A7: First Stage for Those with a Low- and High-Earnings Majors

Total Federal Student Loans Disbursed before Age 23		
Variable	**Low-Earning Major 1**	**High-Earning Major 2**
Instrument: Tuition x Ever Public 4-Year	0.235***	0.302***
	(0.0457)	(0.0803)
Ever Public 4-Year	-0.287	-2.590
	(0.938)	(1.592)
No College	-2.682***	-3.054***
	(0.459)	(0.528)
Associate's/Certificate	-0.363	-0.158
	(0.898)	(0.661)
Bachelor's	2.333**	1.618**
	(0.933)	(0.628)
Master's or More	2.224	2.637
	(2.439)	(2.553)
Degree of Unknown Type	-0.368	0.187
	(1.281)	(1.549)
Ever Public 2-Year	-2.063***	-2.323***
	(0.414)	(0.469)
Ever Private 4-Year Not-for-profit	7.610***	7.104***
	(0.493)	(0.434)
Ever Private 2-Year Not-for-profit	2.715**	2.072
	(1.200)	(1.433)
Ever Private For-profit	3.809***	3.383***
	(0.541)	(0.467)
Constant	2.680***	3.049***
	(0.437)	(0.495)
College Major Controls	YES	YES
Home State by Cohort FEs	YES	YES
Observations	22,211	21,856
R-squared	0.355	0.369

Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. 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. In addition, column (1) limits the sample to those with a low-earning major and column (2) to those with a high-earning major in the treatment group. The control group in both columns include all individuals who have not attended a public 4-year college before age 23. Low- and high-earning majors defined as described in Appendix A.4.1, footnote 46. The regressions include the same controls as those in Figure 4. 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 A8: First Stage for Those in a Low- and High-Income Area

Total Federal Student Loans Disbursed before Age 23		
Variable	**Low-Income**	**High-Income**
	1	**2**
Instrument: Tuition x Ever Public 4-Year	0.201***	0.188***
	(0.0522)	(0.0383)
Ever Public 4-Year	0.177	-0.327
	(0.939)	(0.664)
No College	-2.538***	-3.033***
	(0.382)	(0.442)
Associate's/Certificate	-0.260	-0.696
	(0.902)	(0.614)
Bachelor's	2.328***	1.412**
	(0.869)	(0.623)
Master's or More	4.379	0.869
	(3.448)	(1.767)
Degree of Unknown Type	0.267	-0.915
	(1.464)	(1.317)
Ever Public 2-Year	-1.863***	-2.264***
	(0.360)	(0.344)
Ever Private 4-Year Not-for-profit	7.469***	6.992***
	(0.408)	(0.352)
Ever Private 2-Year Not-for-profit	2.878**	2.344*
	(1.193)	(1.268)
Ever Private For-profit	3.869***	3.426***
	(0.387)	(0.465)
Constant	2.526***	3.033***
	(0.356)	(0.383)
College Major Controls	YES	YES
Home State by Cohort FEs	YES	YES
Observations	22,654	24,778
R-squared	0.356	0.322

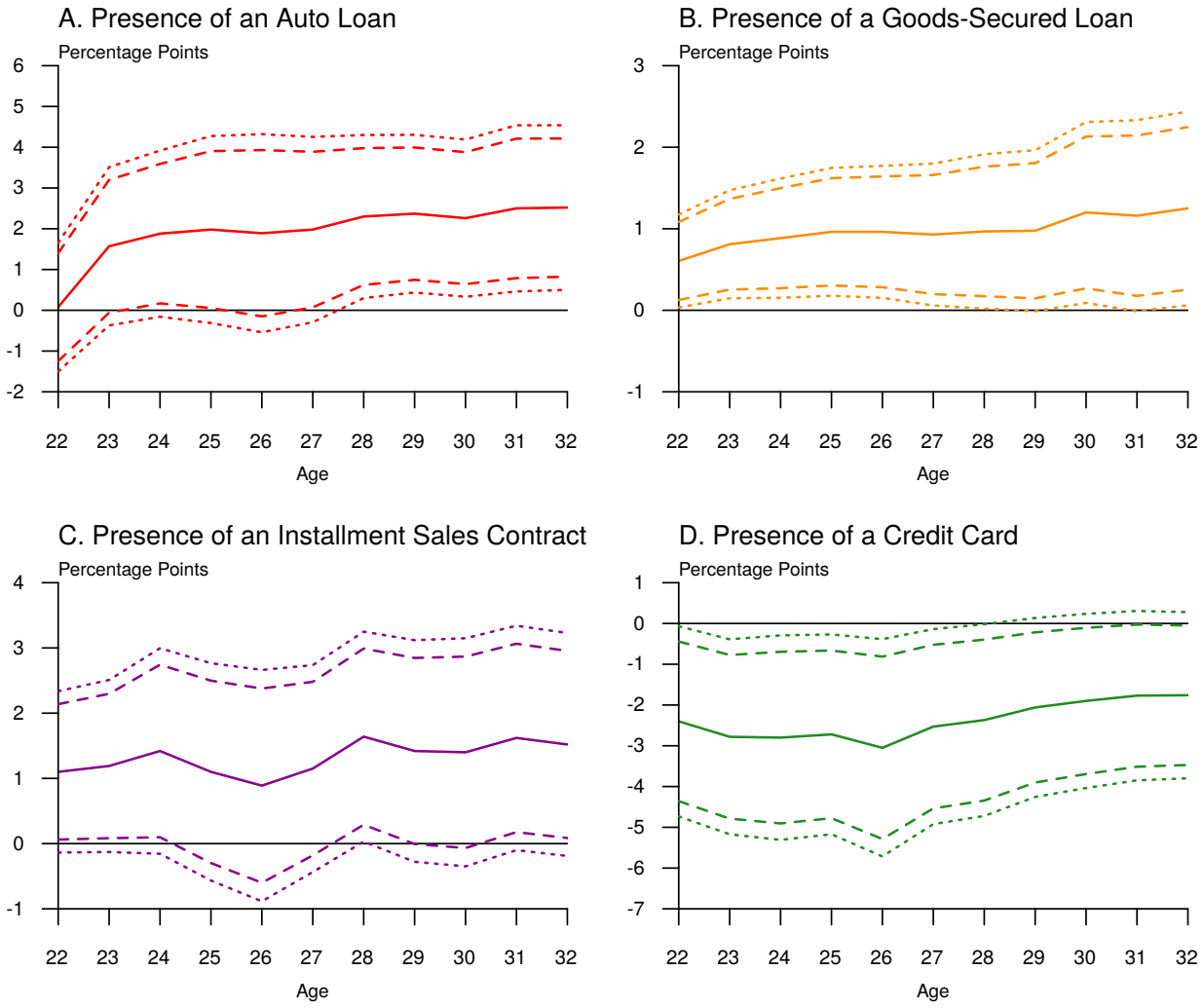
Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. Sample is all individuals from a nationally representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. In addition, column (1) limits the sample to those living in a below-average income area based on a comparison to area median family income and column (2) to those living in an above-average income area in the treatment group. The control group in both columns include all individuals who have not attended a public 4-year college before age 23. The regressions include the same controls as those in 4. 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 A9: First Stage for those with a Prime and Non-Prime Credit Score

Total Federal Student Loans Disbursed before Age 23		
Variable	**Prime**	**Non-prime**
	1	**2**
Instrument: Tuition x Ever Public 4-Year	0.205***	0.165***
	(0.0435)	(0.0537)
Ever Public 4-Year	-0.758	1.318
	(0.764)	(0.920)
No College	-2.776***	-2.968***
	(0.386)	(0.426)
Associate's/Certificate	-0.746	-0.310
	(0.571)	(0.909)
Bachelor's	1.718***	2.224**
	(0.515)	(0.906)
Master's or More	2.860	1.645
	(2.133)	(3.149)
Degree of Unknown Type	-0.697	0.165
	(1.132)	(1.728)
Ever Public 2-Year	-1.992***	-2.338***
	(0.327)	(0.335)
Ever Private 4-Year Not-for-profit	7.202***	6.937***
	(0.312)	(0.354)
Ever Private 2-Year Not-for-profit	2.238*	2.842**
	(1.263)	(1.180)
Ever Private For-profit	3.631***	3.469***
	(0.398)	(0.454)
Constant	2.770***	2.964***
	(0.329)	(0.386)
College Major Controls	YES	YES
Home State by Cohort FEs	YES	YES
Observations	24,548	22,702
R-squared	0.323	0.361

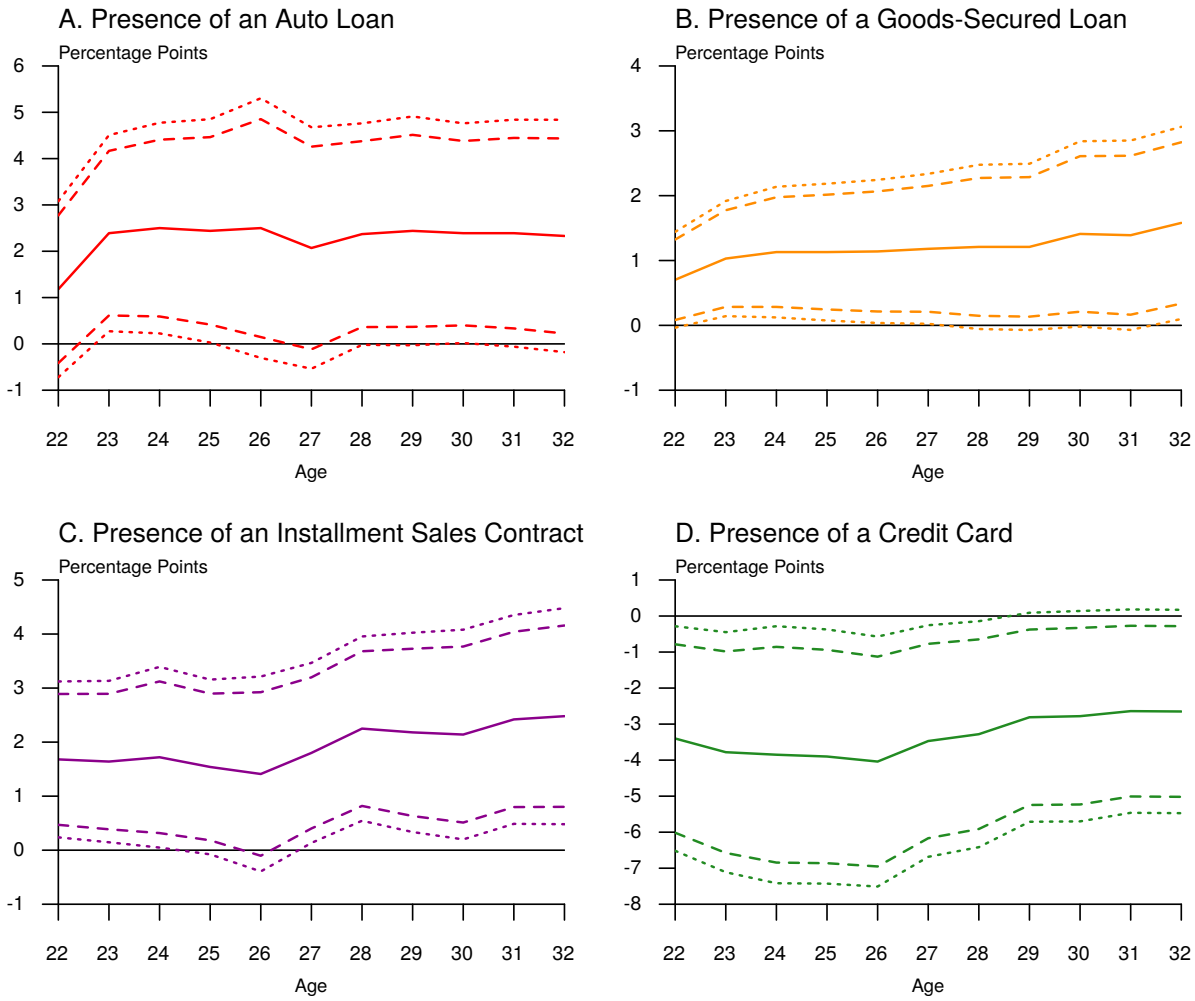
Note: This table reports first stage estimates of the effect of tuition on federal student loans disbursed at the individual level. Sample is all individuals from a nationally representative cohort of 23-to-31-year-old individuals with credit records in 2004 after applying the filters described in Section 3. In addition, column (1) limits the sample to those with a prime score in the treatment group and column (2) to those with a non-prime score. Credit scores are measured at age 23 or 24, depending on the age at which an observation is available for the person. The control group in both columns include all individuals who have not attended a public 4-year college before age 23. The regressions include the same controls as those in Figure 4. 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



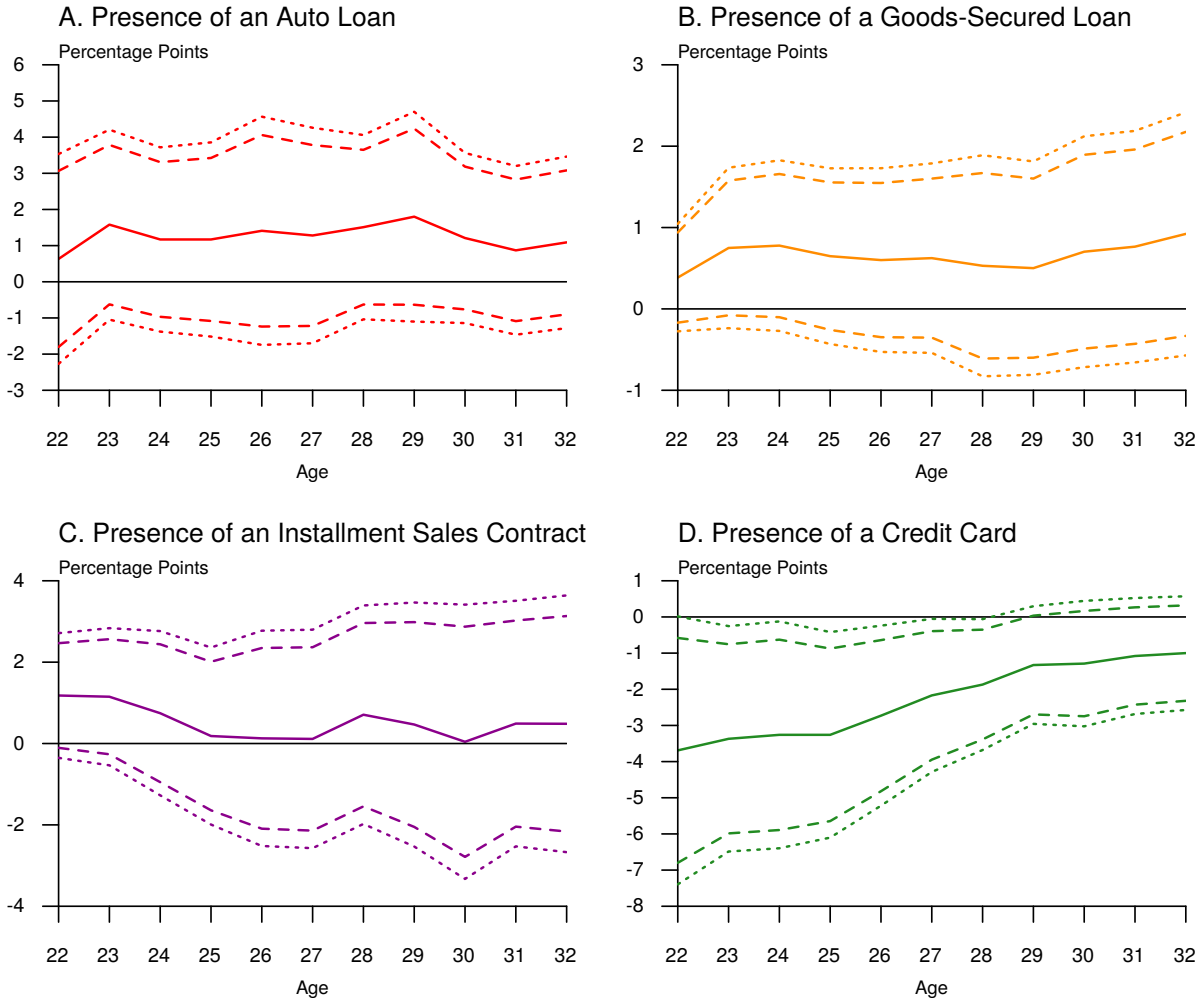
Note:Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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 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 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



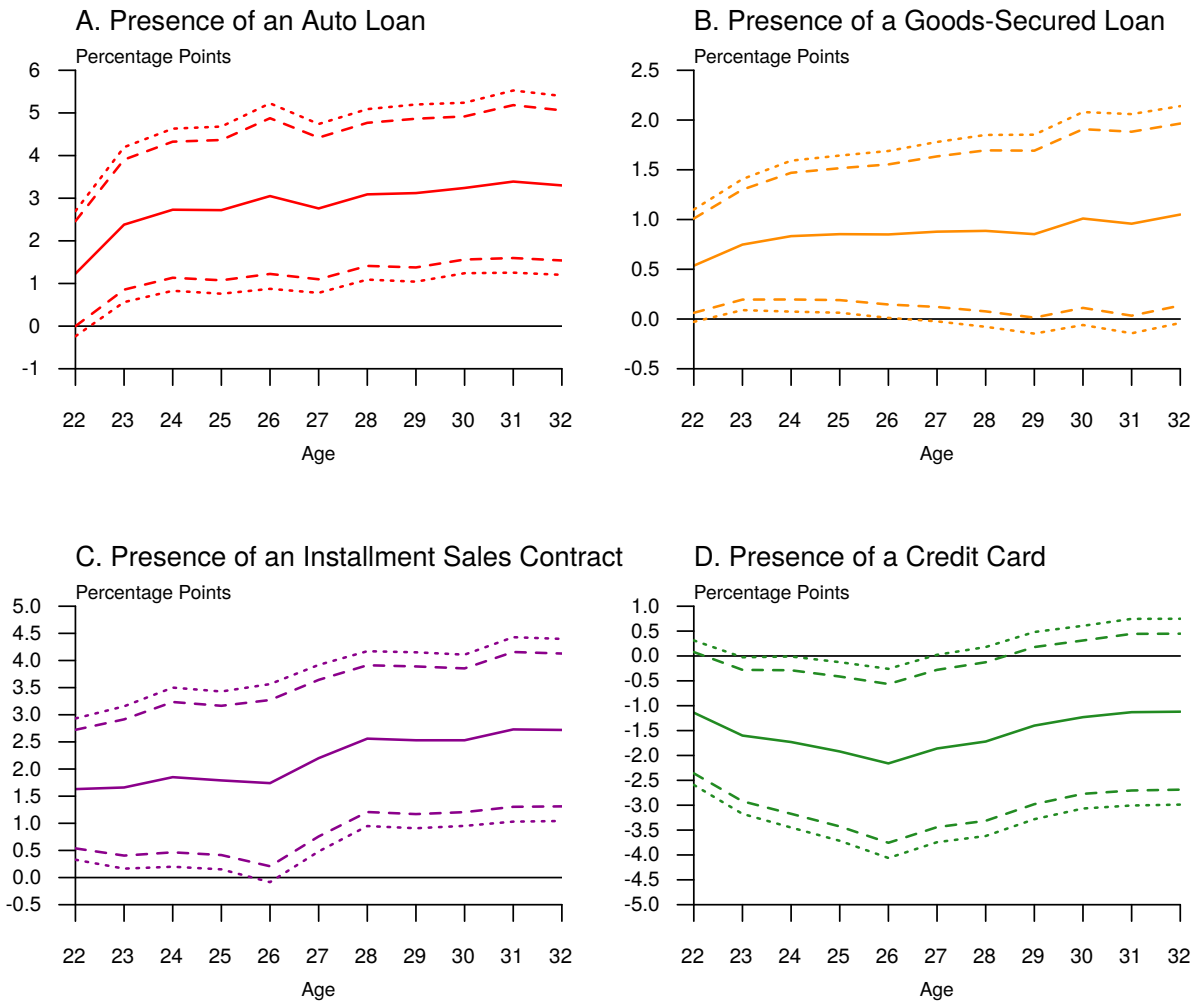
Note: Panels A, B, C, and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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 3. 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



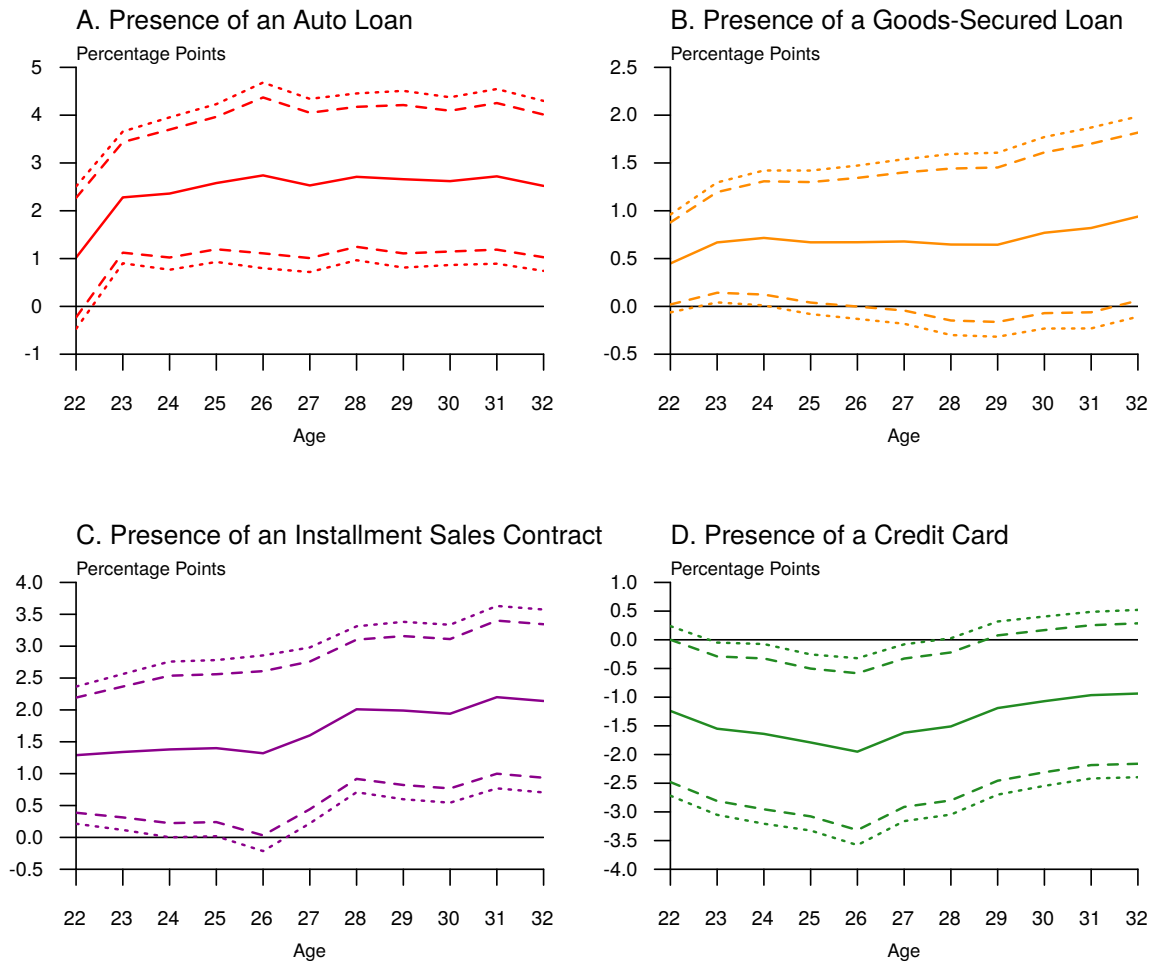
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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 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 A4: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, Non-college Goers Only in the Control Group, 2SLS



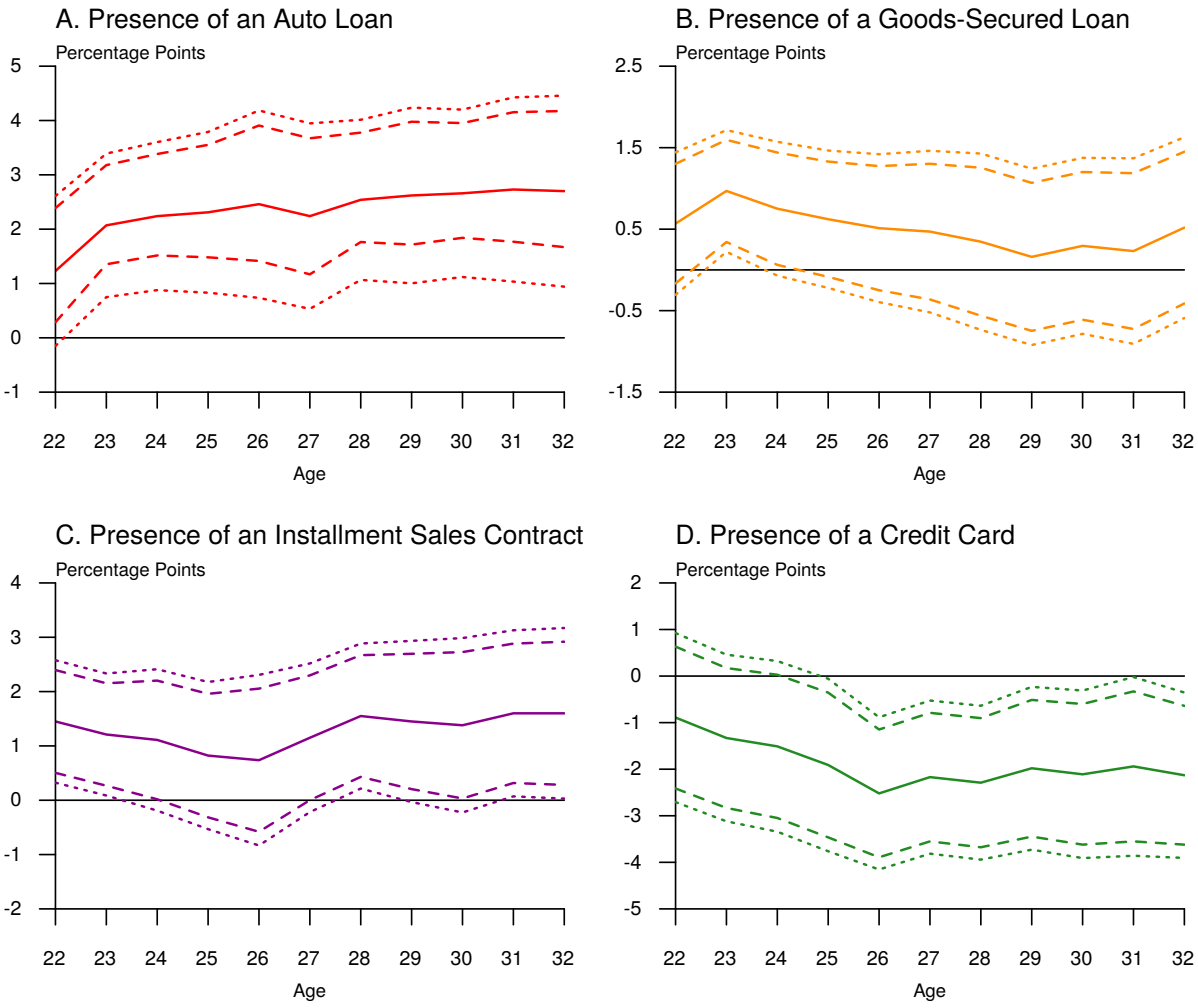
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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 and limitting the control group to individuals who never attended any college by 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 A5: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, No Out-of-State Students, 2SLS



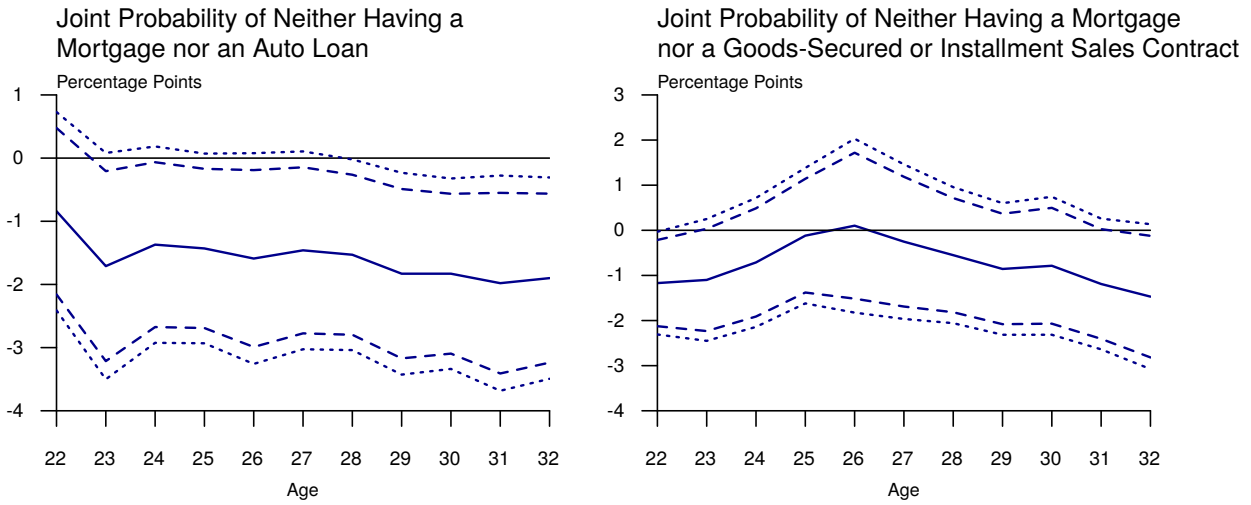
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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 or attended an out-of-state college before age23. 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 A6: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Trade Line, IV-Probit



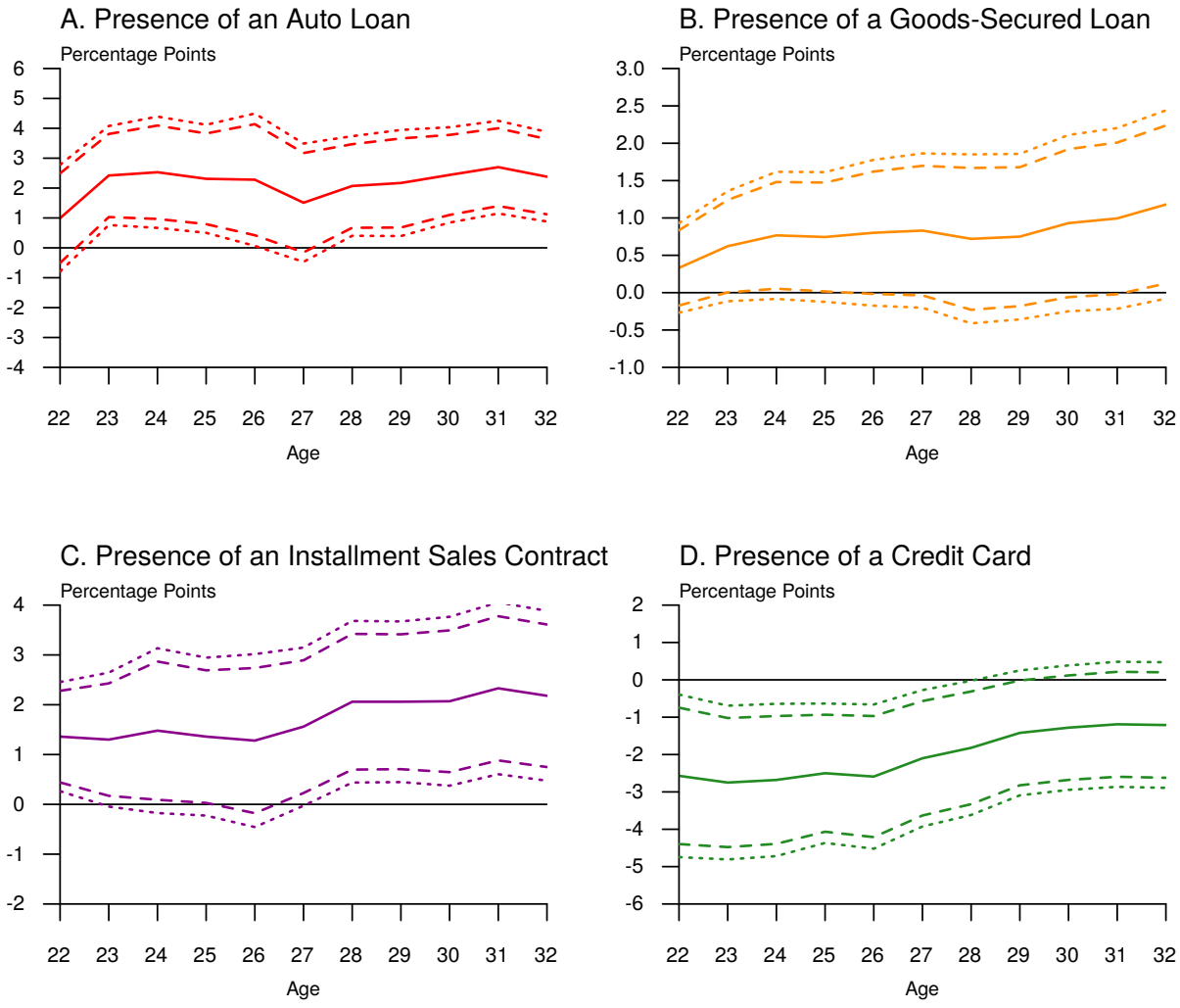
Note: Panels A, B, C and D display instrumental variable probit estimates of the marginal effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. The sample is all individuals 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 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 A7: Joint Effects of a $1,000 Increase in Student Loan Debt on the Probability of Other Credit Outcomes, 2SLS



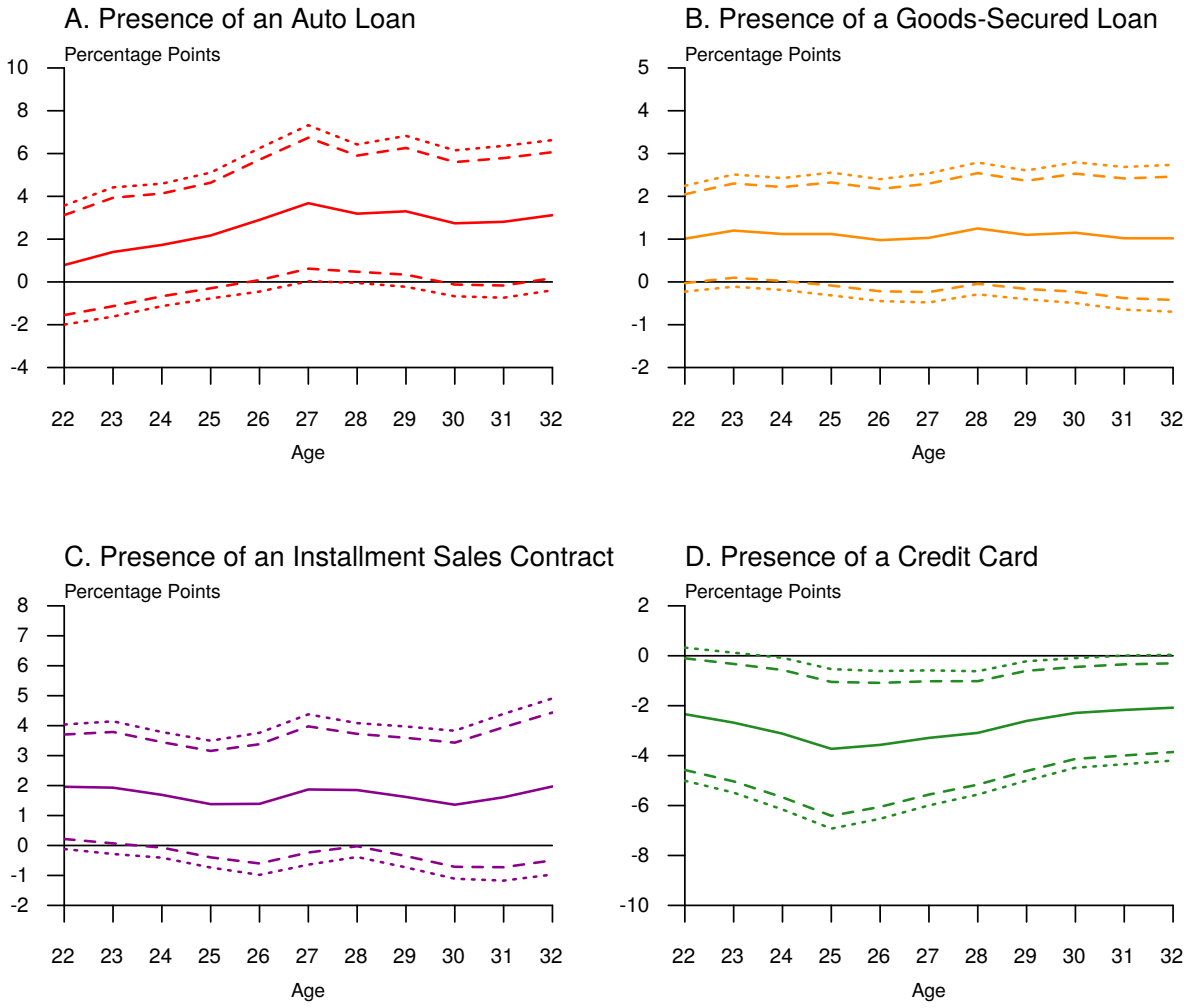
Note: Panels A and B display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of neither having a mortgage nor an auto loan and of neither having a mortgage nor a goods-secured or installment sales contract, respectively, by age. Marginal probabilities are estimated 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.

Figure A8: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, Prime Borrowers, 2SLS



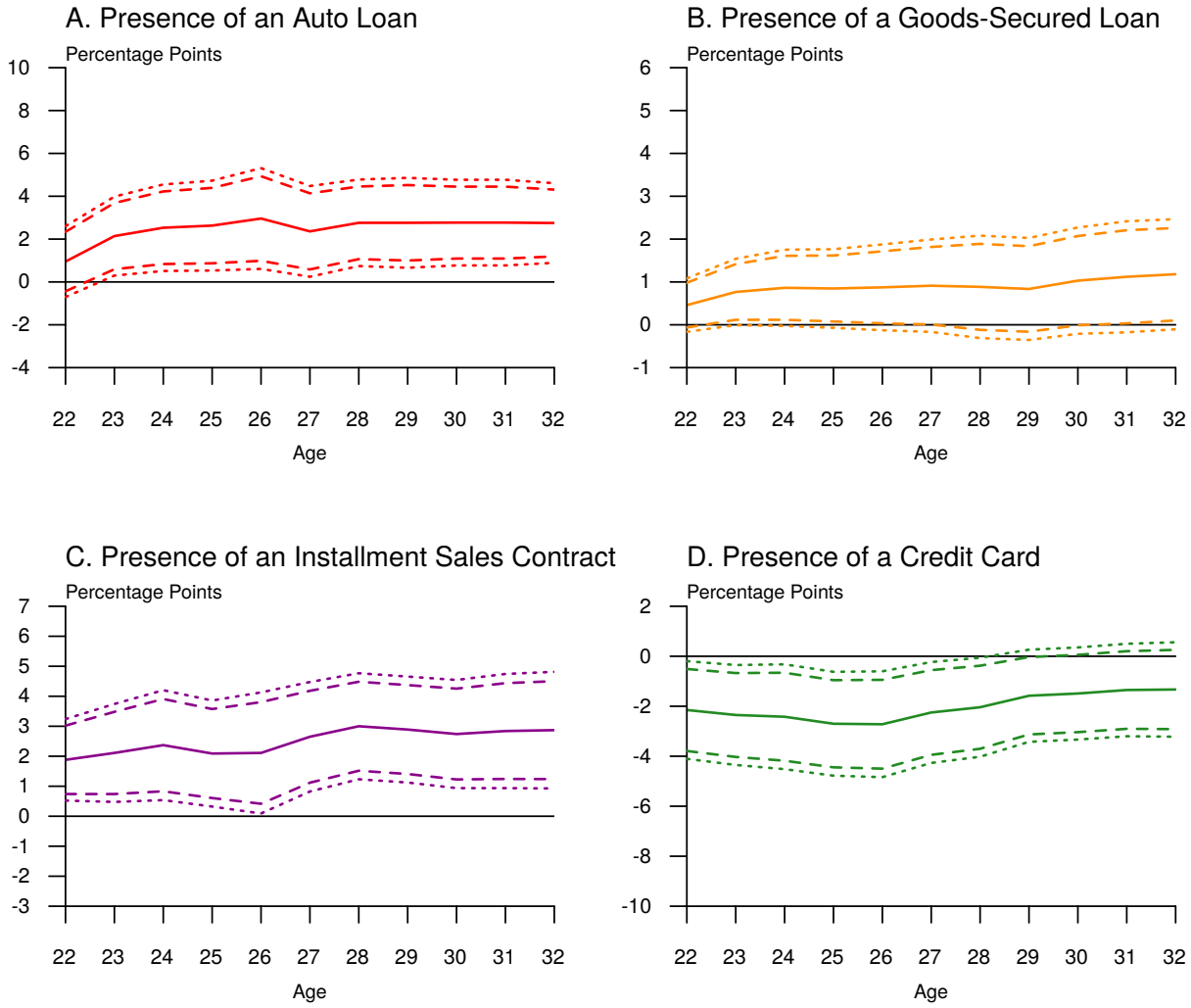
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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. In addition, the treatment group is limited to individuals who had a prime credit score by age 23 or 24, depending on the age at which an observation is available for the person, while the control group includes all individuals who have not attended a public 4-year college 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 A9: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, Non-Prime Borrowers, 2SLS



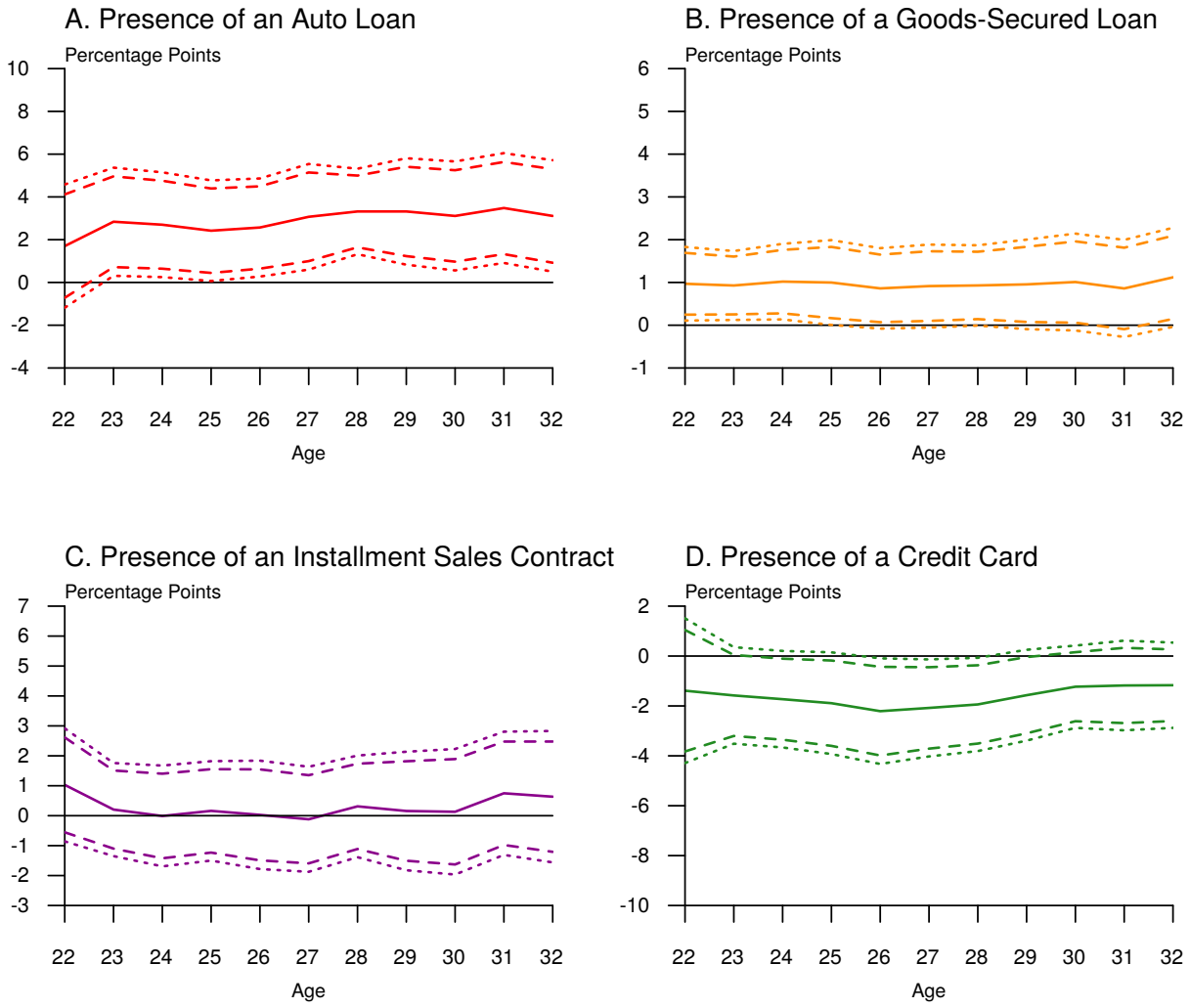
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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. In addition, the treatment group is limited to individuals who had a non-prime credit score by age 23 or 24, depending on the age at which an observation is available for the person, while the control group includes all individuals who have not attended a public 4-year college 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 A10: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, Above-Average Income, 2SLS



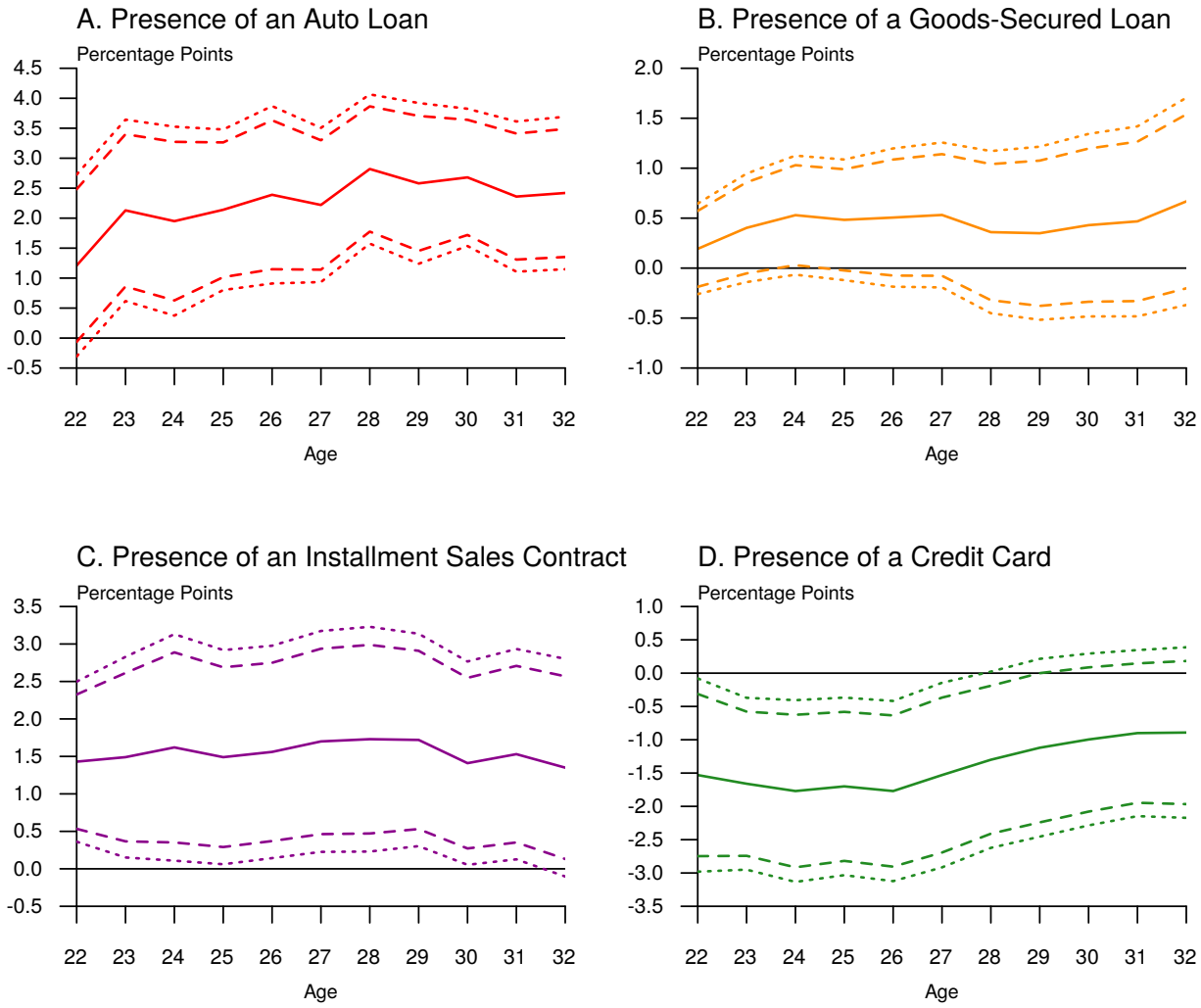
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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. In addition, the treatment group is limited to individuals living in an above-average income are based on a comparison to area median family income, while the control group includes all individuals who have not attended a public 4-year college 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 A11: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, Below-Average Income, 2SLS



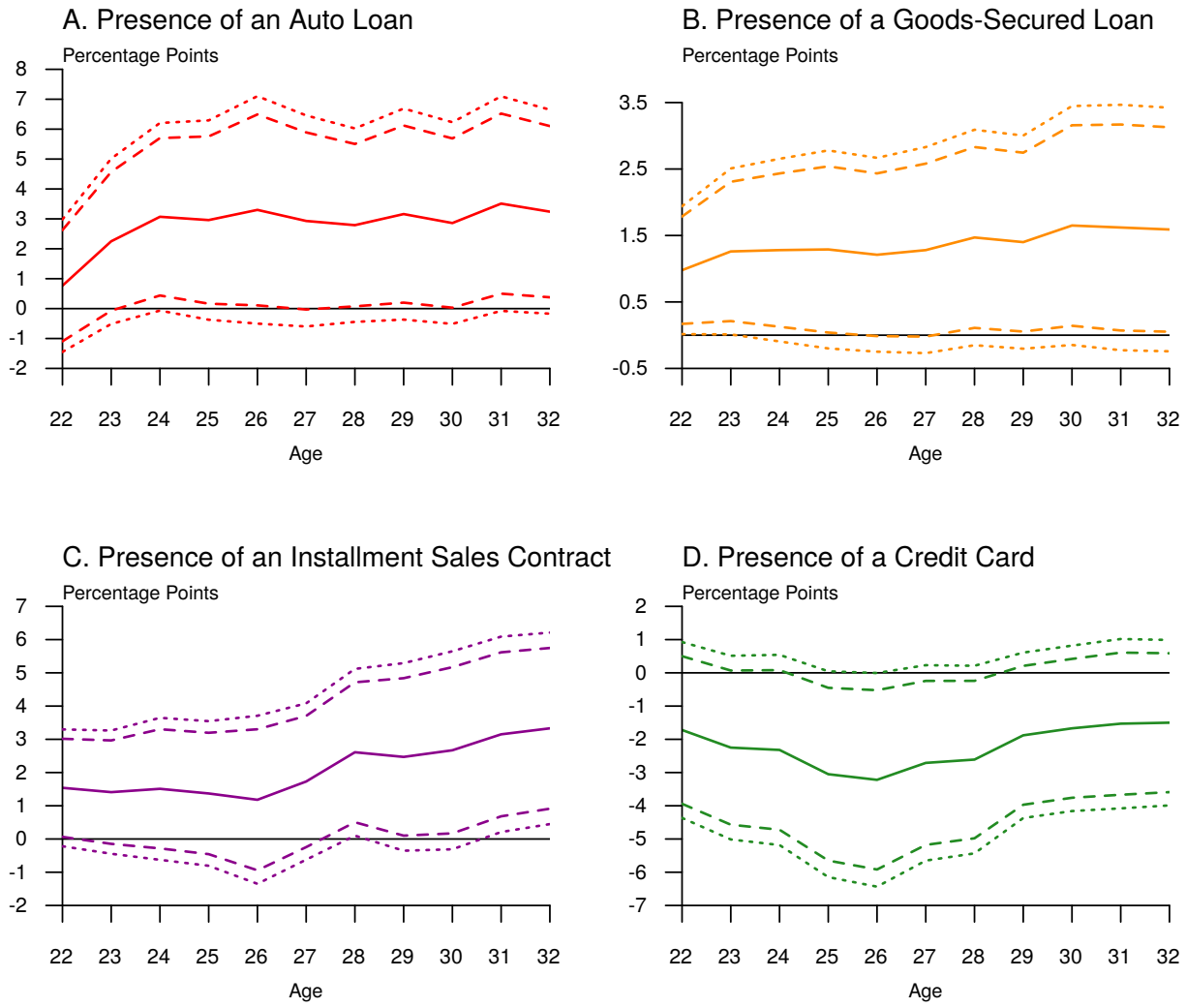
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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. In addition, the treatment group is limited to individuals living in a below-average income are based on a comparison to area median family income, while the control group includes all individuals who have not attended a public 4-year college 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 A12: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, With Degree, 2SLS



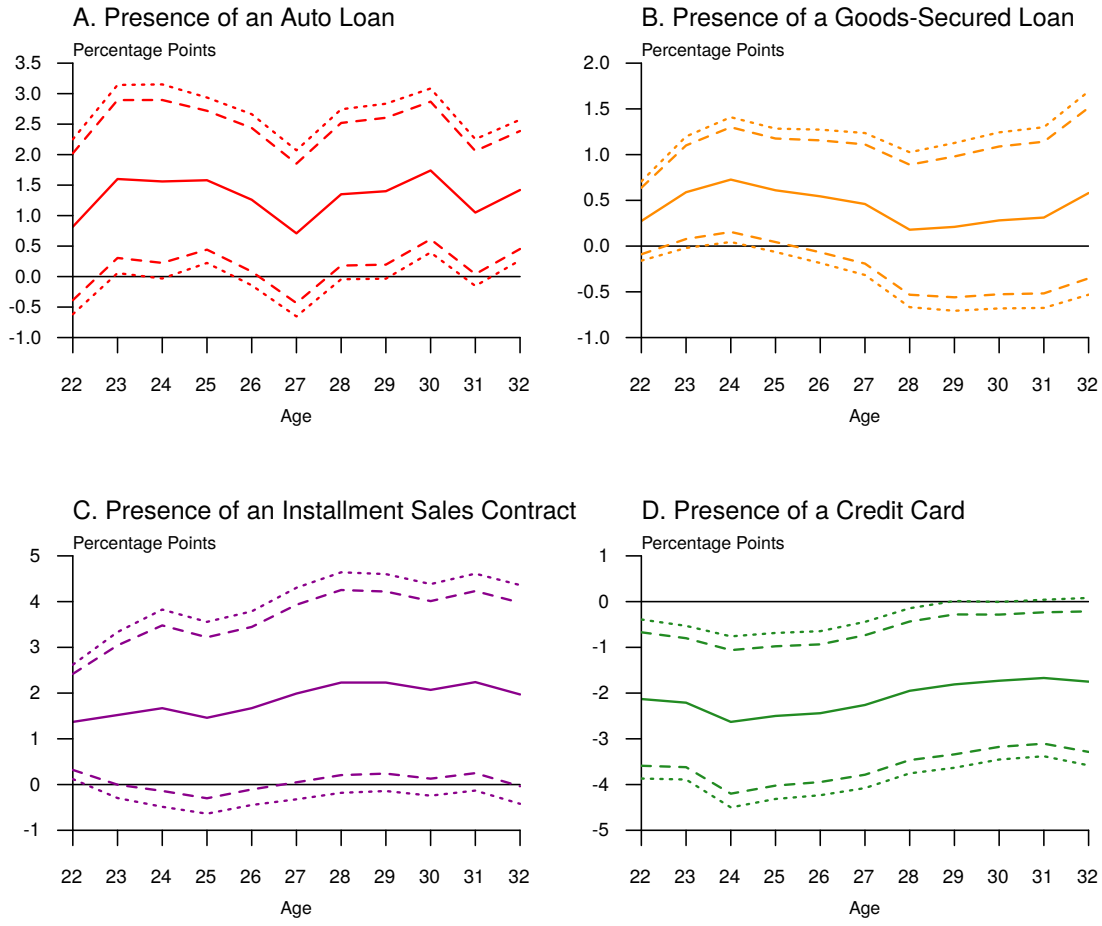
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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. In addition, the treatment group is limited to individuals who finished a degree, while the control group includes all individuals who have not attended a public 4-year college 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 A13: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, Without Degree, 2SLS



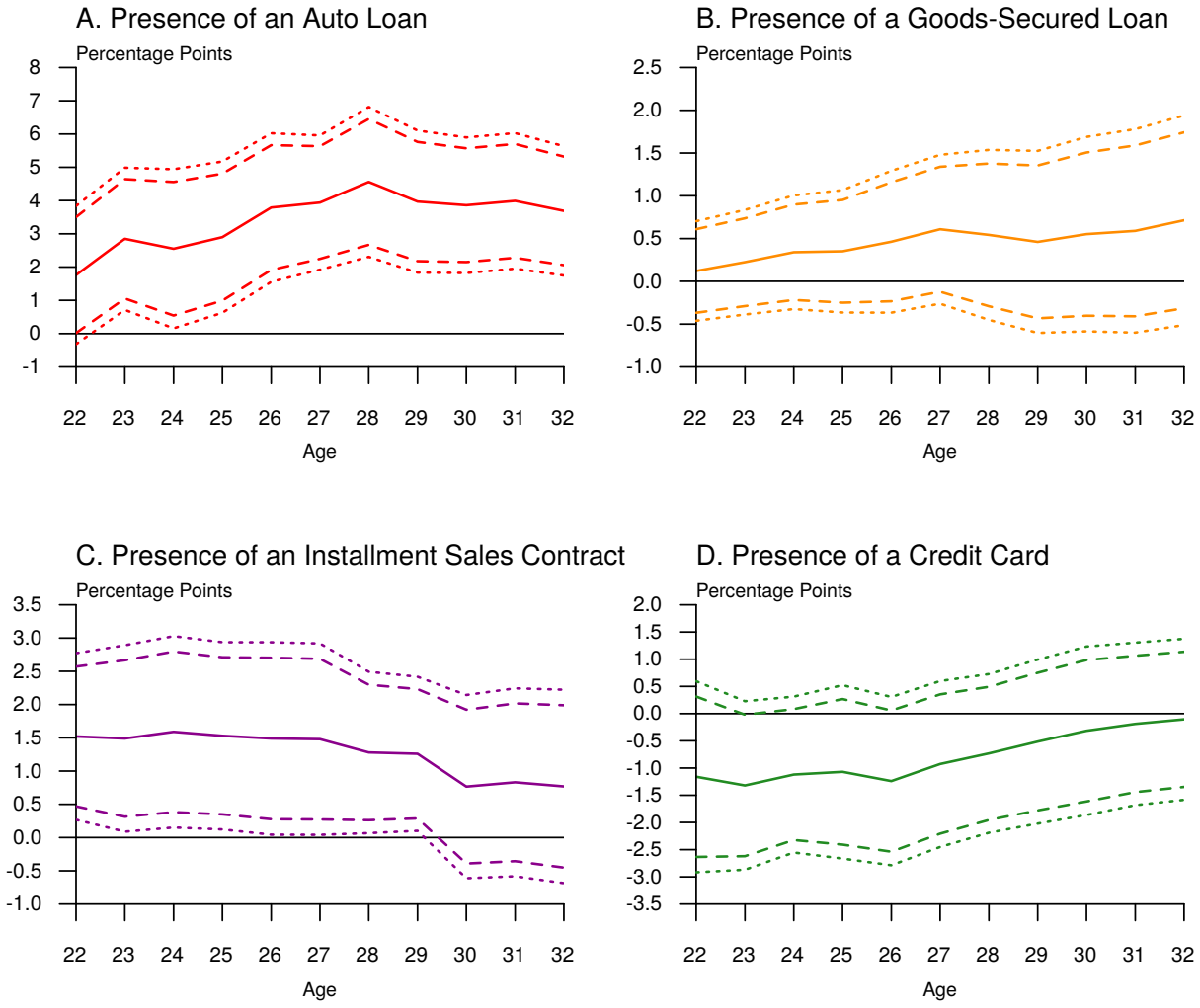
Note: Panels A, B, C and D display 2SLS estimates of the effect of a $1,000 increase in student loans on the probability of ever having an auto loan, goods-secured loan, installment sales contract, and a credit card, respectively, by age. Marginal probabilities are estimated from a linear probability 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. In addition, the treatment group is limited to individuals who did not finish a degree, while the control group includes all individuals who have not attended a public 4-year college 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 A14: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, With High-Earning Major, 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. In addition, the treatment group is limited to individuals with a high-earning major as defined in Appendix A.4.1, footnote 46, while the control group includes all individuals who have not attended a public 4-year college 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 A15: Effect of a $1,000 Increase in Student Loan Debt on the Probability of Ever Having a Tradeline, With Low-Earning Major, 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. In addition, the treatment group is limited to individuals with a low-earning major as defined in Appendix A.4.1, footnote 46, while the control group includes all individuals who have not attended a public 4-year college 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.