

# Credit Supply and the Rise in College Tuition: Evidence from the Expansion in Federal Student Aid Programs

**David O. Lucca**

Federal Reserve Bank of New York

**Taylor Nadauld**

Brigham Young University

**Karen Shen**

Harvard University

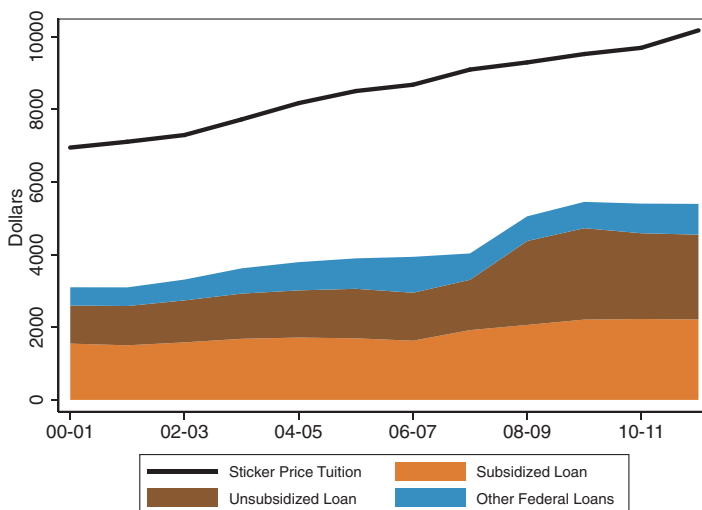
We study the link between the student credit expansion of the past 15 years and the contemporaneous rise in college tuition. To disentangle simultaneity issues, we analyze the effects of increases in federal student loan caps using detailed student-level financial data. We find a pass-through effect on tuition of changes in subsidized loan maximums of about 60 cents on the dollar and of about 20 cents on the dollar for unsubsidized federal loans. The effect is most pronounced for more expensive degrees and degrees offered by for-profit and 2-year institutions. (*JEL* G12, G31)

Received February 23, 2017; editorial decision March 8, 2018 by Editor Wei Jiang. Authors have furnished an Internet Appendix, which is available on the Oxford University Press Web site next to the link to the final published paper online.

The existence of a causal link between student loan availability and college tuition has been the subject of policy discussion and debate for at least three decades (e.g., Bennett (1987)) and has been no less relevant in recent years as tuition and student loan balances have continued to significantly outpace overall inflation. Average sticker-price tuition rose 46% in constant 2012 dollars between 2001 and 2012 (Figure 1), and despite a sharp deleveraging of other sources of debt by U.S. households after the Great Recession, student debt has continued to grow unabated, and now represents the largest form

---

We would like to thank Brian Melzer (discussant), Ian Fillmore, Paul Goldsmith-Pinkham, Erik Hurst, Lance Lochner, Chris Palmer (discussant), Johannes Stroebel (discussant), and Sarah Turner and seminar participants at the AFA 2016, New York Fed, BYU, NBER 2015 SI Corporate Finance Workshop, and the Julis-Rabinowitz Center for Public Policy and Finance's Annual Conference for helpful comments and discussions. Carter Davis provided excellent research assistance. The views expressed here are the authors' and are not representative of the views of the Federal Reserve Bank of New York or of the Federal Reserve System. Supplementary data can be found on *The Review of Financial Studies* Web site.



**Figure 1**  
**Sticker tuition and per-student federal student loans**

This figure plots average undergraduate sticker-price tuition and average federal student loan amounts per full-time-equivalent student. Amounts are shown in 2012 dollars.

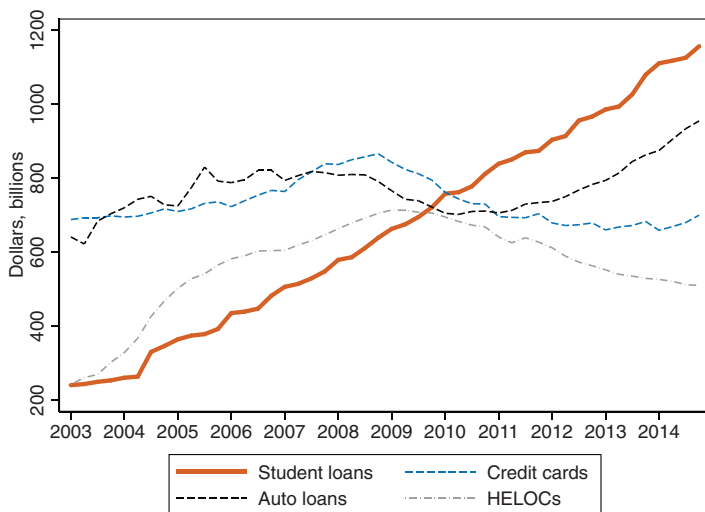
Source: IPEDS/Title IV.

of nonmortgage liability for households (Figure 2). While rising tuition costs almost certainly contribute to increased demand for student loans, an important policy question is whether the reverse effect is also true, that student loan supply allows tuition to rise, as postulated by the so-called “Bennett hypothesis.”<sup>1</sup>

In this paper, we propose an identification strategy to isolate the effect of an increase in student credit on tuition, an effect that has received limited attention in the literature as compared to student grants and especially in the context of large samples of postsecondary institutions. Our identification strategy uses variation in student credit supply that resulted from legislative changes in the maximum amounts that students are eligible to borrow from the federal subsidized and unsubsidized loan programs. These policy changes went into effect in the 2007–2008 and 2008–2009 school years and led to a large credit expansion, as these program maximums had remained unchanged since the early 1990s.<sup>2</sup> While exploiting the federal increase in credit supply for

<sup>1</sup> The then-Secretary of Education William Bennett (1987) argued that “[...] increases in financial aid in recent years have enabled colleges and universities blithely to raise their tuitions, confident that Federal loan subsidies would help cushion the increase,” a statement that came to be known as the “Bennett hypothesis.”

<sup>2</sup> The maximum subsidized federal loan amount for freshmen rose in the 2007–2008 academic year from \$2,625 to \$3,500 and for sophomores from \$3,500 to \$4,500; unsubsidized loan maximums rose by \$2,000 in the academic year 2008–2009. Pell Grant maximums are not our main focus, but we control for them. They rose gradually between the 2007–2008 and 2010–2011 school years and in prior years as a result of the yearly appropriation process of the Department of Education. Subsidized, unsubsidized loans and Pell Grants are the main “Title IV” programs. We discuss the institutional details of federal aid programs in Section 2.



**Figure 2**  
Non-mortgage-related household debt balances

This figure shows the time-series evolution of non-mortgage-related debt balances. Amounts shown are in nominal terms.

Source: NY Fed CCP/Equifax.

identification helps address simultaneity and reverse causality issues between credit and tuition, it presents two additional challenges. First, the increase in program maximums affected students at all institutions. Second, we only have reliable time-series data on the sticker-price of tuition rather than the net tuition paid by students after accounting for scholarships or discounts to lower-income students. However, as we discuss and show analytically, if the short-run supply of education “seats” is imperfectly elastic, a credit expansion will raise the tuition paid by all students, not just those who are constrained, through a pecuniary demand externality, where the size of the externality depends positively on the size of the credit-constrained population. These predictions suggest that to identify the impact of increases in loan supply on tuition one can utilize cross-sectional variation in the fraction of the credit-constrained student population across universities. We measure this fraction using detailed student-level data on borrowing behavior at each institution. We then interact this treatment intensity, or “exposure measure” with the timing of shifts in the supply of federal student aid to study tuition changes, similar to Card’s (1992) study of the effects of the federal minimum wage that exploits state variation in the fraction of workers earning below the new minimum.

We first validate our approach by documenting that institution-level loan amounts respond to the interaction of the legislated changes in maximum aid amounts with an institution’s exposure to the changes. Changes in per-student loan amounts measured at the institution level load with a coefficient of 0.6 on these interaction measures. We then use these measures to examine

variation in tuition increases in the same year as the credit expansions. We find that increases in institution-specific subsidized (unsubsidized) loan maximums lead to a sticker-price increase of about 60 (20) cents on the dollar. This effect represents the additional amount that institutions raised their tuition in the years of the policy changes relative to what would have been expected without the policy change, which we measure using institutional fixed effects to capture the average tuition increases at an institution. All of these effects are highly significant and consistent with the Bennett hypothesis and apply to a large sample of aid-eligible institutions. Direct quotes from earnings calls and large stock market reactions to the passing of these loan expansions lend additional support to these findings for the subset of publicly traded for-profit institutions. While not the main focus of this paper, we find positive but statistically insignificant effects of an increase in Pell Grants on tuition. The insignificance of this effect may not indicate economic differences between the two forms of aid but rather the result of the fact that changes in maximums for Pell Grants occurred much more gradually than for student loans, thus hampering the statistical measurement. The economic effects of increased aid on tuition should be qualitatively similar for loans and grants. However, quantitative implications may differ depending on the elasticity of demand for higher education services with respect to each type of aid and on how universities weigh the importance of high versus low income students, who are more likely to receive aid in the form of grants.

Of course, institutions may have mitigated the effect of these tuition increases through increases in institutional grants to some or all students. We have only noisy institutional grant data, but the evidence suggests that increases in subsidized loans, if anything, decreased institutional grants on average, suggesting that the tuition effect is on average not canceled out (and may even be amplified) by the inclusion of institutional grants. With respect to distributional effects, we provide some evidence suggesting that the tuition increases resulting from the credit expansion were not associated with additional tuition discounting for lower income students *vis-à-vis* higher income ones.

By adding controls to the baseline specification and through additional robustness checks we attempt to address a number of concerns about our estimates. First, we abstract from other forms of aid by controlling in our specifications for changes in Pell Grant maximums, which partially overlapped with those in the federal loan policies. A second concern is that the Great Recession may have boosted demand for education services at institutions where students were more dependent on student aid, or on the supply side, these same institutions may have experienced a drop in nontuition revenues from state appropriations or endowments, requiring an increase in tuition to bolster budgets. However, we note that tuition decisions for the year when the main policy of interest took effect would have likely been made in the spring of 2007, predating the recession. Indeed we find that our results are not affected by dropping the post-financial crisis sample. In addition, we consider specifications

in which we control for changes in nontuition funding sources. A third concern is that different types of schools were on different tuition trends because of other unobserved factors affecting certain sectors, and that school type is correlated with our exposure measures. We account for this effect by allowing trends to vary by school type, and along various other school characteristics. Finally, we run a parallel trends test that is agnostic about what variables might be driving the differential variation. In this test, we run regressions that compare tuition changes of highly and less exposed institutions in every year to see if tuition changes at treated and control institutions were similar in years in which policy changes did not take place. Results of this robustness analysis suggest that the subsidized loan effect is robust across specifications both in magnitude and significance, and passes the parallel trends test, while the unsubsidized loan effects is not as robust.

In addition, we investigate the characteristics of the institutions where the pass-through effects of credit to tuition are most pronounced. We find that the loan effect is most pronounced for more expensive degrees, and after controlling for tuition cost that the effect is strongest at for-profit institutions and 2-year or vocational programs. Our sample of for-profit institutions is small, so we add validation to this result using a large sample, but employing a more blunt test. We document abnormally large tuition increases by this sector in the academic years starting in 2007 and ending in 2010 relative to other years and other sectors, providing suggestive evidence that for-profit institutions, which rely heavily on federal aid, were highly responsive to these credit expansions. This evidence lends support to the significant policy concern surrounding for-profit institutions and student aid that has generated policies such as the 90/10 rule, which caps the share of for-profit institutions' revenues originating from federal aid sources (both grants and loans) at 90%.

This paper contributes to three main strands of literature. First, it builds on the expanding finance literature studying the role of credit supply in determining real allocations and prices. Much attention has been devoted to this question in the context of the housing market, for which credit is central, in an attempt to establish whether the U.S. housing boom of 2002–2006 and the ensuing bust can be explained by increased credit to subprime borrowers (see, e.g., Mian and Sufi 2009; Adelino et al. 2012; Favara and Imbs 2015). From a finance perspective, the market for postsecondary education has shared several features with the housing market despite the important difference that student loans fund a capital investment while mortgages fund an asset. Like housing finance, credit plays a key role in funding U.S. postsecondary education, and most of this credit is originated through government-sponsored programs. Our paper provides complementary evidence to the conjecture that credit expansions can result in aggregate pricing effects, rather than only affecting the prices of the assets purchased by credit recipients.

This paper also contributes to the economics of education literature studying the determinants of the price of postsecondary education, and in particular, the

strand of this literature that seeks to accept or reject the Bennett hypothesis. The majority of recent papers have focused on the Bennett hypothesis in the context of grants.<sup>3</sup> For example, Turner (2014) uses a regression discontinuity approach and finds that institutions alter institutional aid (scholarships) as a means of capturing the federal aid provided through the federal Pell Grant program.<sup>4</sup> The primary contribution of our study relative to this literature is our focus on the impact of loan programs, particularly in a comprehensive sample that includes all school types. We find large tuition effects when loan program maximums are relaxed but not when Pell Grant maximums are increased. We discuss in the main text that these differences may come about because of difficulties in the empirical identification of the grant effects but also may be due to differences in demand elasticities across different types of aid recipients or differences in how universities value enrolling higher- versus lower-income students.

Cellini and Goldin (2014) offer some suggestive evidence that the effect may extend beyond grants when they study the impact of overall federal aid eligibility by constructing a data set of comparable eligible and ineligible for-profit institutions and show that eligible institutions charge tuition that is about 75% higher than comparable institutions whose students cannot apply for such aid. Because almost all degree-granting institutions are federal-aid-eligible, their study is mostly limited to for-profit vocational programs. Our study looks instead at variation within eligible institutions (and thus includes 2- and 4-year degree programs, as well as not-for-profit institutions)- and is also able to specifically identify and quantify the role of loans using a difference-in-differences approach. To our knowledge, the only studies to have explored this thus far have used structural methods (e.g., Epple et al. 2013 and Gordon and Hedlund 2016). Both find that increases in borrowing limits generate tuition increases, with the latter finding that borrowing limit increases represent the single most important factor in explaining tuition increases between 1987 and 2010 at 4-year institutions, explaining 40% of the tuition increase, while supply-side factors such as rising costs or falling state appropriations have much less explanatory power. Their study assumes a representative college and is able to nicely explain the long-run rise in tuition, but matches higher frequency changes less well. We use a natural experiment that exploits cross-institution differences to explain differential increases in policy years. Our study should be seen as a complement to this work.

---

<sup>3</sup> For example, McPherson and Schapiro (1991) look at the period 1979–1986 and find no evidence of the Bennett hypothesis for private 4-year institutions, but find a pass-through of \$50 for every \$100 for public 4-year institutions. Singell and Stone (2007) find increases at private institutions but only in out-of-state tuition at public institutions using data from 1989 to 1996. Rizzo and Ehrenberg (2003) find evidence of the Bennett hypothesis in in-state tuition, but not out-of-state tuition, in a restricted sample of 91 public flagship state universities between 1979 and 1998.

<sup>4</sup> Similar studies have also found evidence of the Bennett hypothesis in tax credits (Long (2004b); Turner (2014)), and state grant aid programs (Long (2004a)). A review of some of these and other studies of the Bennett hypothesis can be found in Congressional Research Service (2014).

Our paper also contributes to a burgeoning literature on student loans more generally that evaluates the causes and real effects of increased levels of student debt. Looney and Yannelis (2015) and Mezza et al. (2016) identify school-level tuition changes as a first-order contributor to increasing student debt, whereas Cadena and Keys (2013) show that attitudes toward debt have also contributed. Our work highlights the potential for a feedback cycle of student debt and tuition, in which increases in student debt spurred by increased federal loan availability cause higher tuition, further fueling debt accumulation. This effect implies potential externalities from credit expansions. Recent literature has documented other potential negative effects of increased student debt: for example, Fos et al. (2017) document that increased levels of student debt reduce students' subsequent investment in human capital, Mezza et al. (2016) find that increased student debt causes a 1- to 2-percentage-point drop in student loan borrowers homeownership rate in the first 5 years after leaving school, and Yannelis (2016) finds that higher levels of student debt are also associated with higher levels of default. We discuss welfare implications more generally in the conclusion.

Finally, this paper is related to the public economics literature on tax incidence (Kotlikoff and Summers 1987), that studies how the burden of a particular tax is allocated among agents after accounting for partial and general equilibrium effects. In our setting, the student aid expansion is a disbursement of a public benefit. From an individual perspective, more aid is beneficial because of relaxed constraints, but in equilibrium the welfare effects of aid recipients could be negative because of the sizable and offsetting tuition effect.

## **1. Theoretical Framework**

In this section we discuss the theoretical framework that underlies the empirical approach in the paper. Appendix A provides a detailed presentation of the analytical model, but for the sake of brevity, we only summarize key insights here.

To address issues of reverse causality stemming from changes in tuition to student loan demand, this paper focuses on a natural experiment resulting from the expansion in federal student loan maximums. However, this quasi-experimental setting presents its own challenges. First, the increase in student caps in principle affected students at all universities. If loan maximums were the only factor influencing tuition, then one could back out the impact of credit on tuition from average tuition increases in years when loan maximums were raised. However, since tuition trends are influenced by many other factors (e.g., the business cycle, changes in the returns to higher education), such an approach is not feasible. In addition, because of data limitations, we mostly focus on sticker tuition as opposed to tuition net of discounts and grants. However, aid recipients often pay less than sticker price. So while net price could increase for all students, as would be predicted by our model, the tuition effects that

we can measure in the data are indirect ones as they likely involve nonaid recipients.

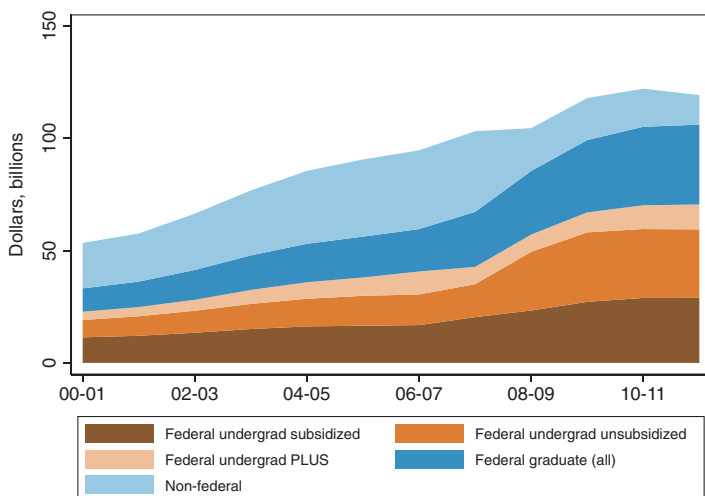
We consider a university that maximizes a combination of student quality and revenues as in Epple et al. (2006) subject to a short-run capacity constraint. Institutions can condition tuition offers on students' skills and income. These characteristics are imperfectly observed by each university so that in equilibrium an institution extracts some, but not the entire, rent from a student match. In equilibrium, tuition levels differ as a function of student characteristics as in a standard third-price discrimination problem, with credit-constrained students paying less than higher-income students. Despite the fact that the highest paying students' borrowing constraint is not binding, a relaxation of the credit constraint increases tuition for all students. This means that sticker tuition, the highest price at a university, can increase as a result of additional student aid even if no student paying sticker takes out a student loan. As shown in the model, this is because the increased ability to pay from the subset of constrained students increases the shadow cost of a seat for all students resulting in higher tuition not just for aid recipients. Furthermore, the shadow cost increase is larger at universities with more credit-constrained students. This is because the demand boost from the credit expansion is greater the more students take out loans. In the appendix, we solve the model for an "isoelastic" specification that uses exponential distributions for the unobservable components. This specification allows a closed-form solution and predicts equal changes in tuition for all students. However, in a more general setting, we would find positive cross-demand effects, but not necessarily of equal size.

The pecuniary demand externality from relaxing the borrowing constraint of aid-recipients validates our investigation of sticker tuition as a dependent variable. In addition, the differential cross-institution effects allow us to use a difference-in-differences approach based on cross-institution differences in the sensitivity of tuition to a relaxation of the borrowing constraint. Namely, we identify the effect of a credit expansion by comparing tuition changes around the credit expansions for universities with larger and smaller shares of credit-constrained students (similar to the continuous treatment effect approach of Card (1992)). Because of the demand externalities, a cross-institution comparison is better suited to capture the full effect of changes in credit availability as opposed to comparing students who did or didn't take out loans.

Next, we turn to a discussion of the federal student aid programs and of the policies that drive the relaxation of the borrowing constraint.

## **2. Federal Student Aid Programs**

Federal student aid programs are governed by Title IV of the 1965 Higher Education Act (HEA) and aim to support access to postsecondary education through the issuance of federal grants and loans. The majority of federal student



**Figure 3**  
**Aggregate student loan originations**

This figure shows the time-series evolution of aggregate student loan originations by program type. Amounts shown are in nominal terms.

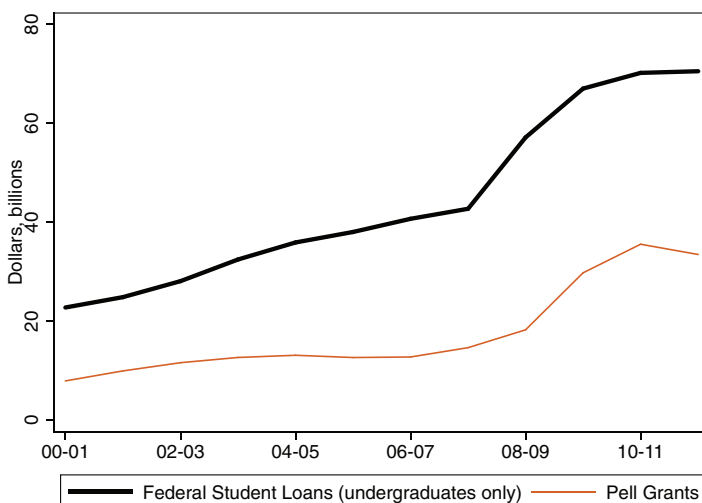
Source: College Board.

loans are administered under the William D. Ford Federal Direct Loan (DL) Program and come in two types: subsidized and unsubsidized.<sup>5</sup> The exact terms of federal loans have changed over time but typically involve low interest rates and flexible repayment plans.<sup>6</sup> The federal government pays the interest on a subsidized student loan during in-school status, grace periods, and authorized deferment periods. Qualification for subsidized loans is based on financial need, whereas unsubsidized loans, where the student is responsible for interest payments, are not. Together, these two programs make up about 85% of federal student loan originations, with the rest coming from PLUS and Perkins loans.<sup>7</sup> Federal loans are the principal form of student loans in the United States, representing an even larger share since the financial crisis (Figure 3).

<sup>5</sup> Historically, these were also administered under the FFEL program and known as “Stafford loans.” Under FFEL, private lenders would originate loans to students that were then funded by private investors and guaranteed by the federal government. Under the DL program, the Department of Education (ED) directly originates loans, which are funded by Treasury, to students. With the Health Care and Education Reconciliation Act of 2010, the FFEL program was eliminated, but the types of loans offered to students were not affected.

<sup>6</sup> Federal loan programs do not require repayment when still in school, and do not require a credit record or cosigner. Interest rates have varied and been both fixed and floating. Rates on all federal loans to undergraduates currently stand at 4.29%. Loan repayment starts after a 6-month grace period following school completion, and standard repayment plans are 10 years. Payments can be stopped for deferments (back to school) or forbearance (hardship). Under “income-based repayment” plans, borrowers can limit their loan payments to a fraction of their income.

<sup>7</sup> PLUS loans require that borrowers do not have adverse credit histories and are awarded to graduate students and parents of dependent undergraduate students. Finally, Perkins loans are made by specific participating institutions to students who have exceptional financial need.



**Figure 4**  
**Aggregate Pell Grant and federal loan amounts**

This figure plots Pell Grant disbursements by year as compared to total undergraduate federal student loan originations.

Source: Title IV.

In our analysis we control for changes in Pell Grant maximums as they partially overlapped with changes in federal student aid programs. Pell Grants are the main source of federal grants, and are awarded to low-income (undergraduate) students in financial need. Pell Grant disbursement averaged around \$30 billion in recent years, compared to an average of about \$70 billion for federal student loan originations to undergraduates (Figure 4).

## 2.1 Eligibility

Federal student aid amounts are determined by *individual maximums*, which depend on the particular education cost and family income of a student, and by overall *program maximums* that apply to all students, which we use for identification.

Eligible students can qualify for federal loans and grants by filling out the Free Application for Federal Student Aid (FAFSA). The primary output from the FAFSA is the student expected family contribution (EFC), which represents the total educational costs that students and/or their families are expected to contribute, and is computed as a function of family and student income and savings, family size, and living expenses.

A student’s aid package is determined through a hierarchical process starting with need-based aid, which includes Pell Grants and subsidized loans, as well as Federal Work Study and Federal Perkins Loans (which are small). Need-based aid is capped at a student’s “financial need,” or the portion of the cost of attendance (or COA, which is the sum of tuition, room and board, and other

costs or fees) that is not covered by the EFC. That is, need-based aid should satisfy

$$\text{Pell Grants} + \text{Subsidized Loans} \leq \text{Financial Need} \equiv \text{COA} - \text{EFC}, \quad (1)$$

where the left-hand side omits, for simplicity, other (less important) need-based aid. We refer to the right-hand side as the *individual maximum*. Pell Grants are subject to an additional EFC restriction, where only students with an EFC below a certain threshold are eligible, with the maximum amount offered decreasing with EFC. This is in contrast to subsidized loans, for which maximum amounts do not depend on EFC aside from (1). The hierarchical aid assignment is such that students who are eligible for a Pell Grant will be offered the grant to cover their financial need before any loan or other need-based aid.

Eligibility for non-need-based federal aid (which include unsubsidized loans and PLUS loans) is determined by computing the portion of the COA that is not covered by federal need-based aid or private aid (e.g., institutional grants). Non-need-based aid thus satisfies

$$\begin{aligned} &\text{Unsubsidized Loans} + \text{PLUS Loans} \\ &\leq \text{COA} - \text{Need-Based Aid} - \text{Private Aid}. \end{aligned} \quad (2)$$

where again, the right-hand side is the unsubsidized individual maximum. Irrespective of the individual maximums, aid amounts are always capped by each program maximum. However, if a student's individual need-based maximum is below the subsidized program maximum, students are allowed to borrow unsubsidized loans in an amount such that their joint subsidized and unsubsidized borrowing is equal to the subsidized program maximum.

## 2.2 Changes in program maximums

Table 1 shows the evolution of federal aid program maximums in our sample period. The subsidized maximum was raised in the 2007–2008 school year, unsubsidized loan maximums were raised in the 2008–2009 school year, and Pell Grant maximums were raised and frozen through a series of budget appropriations and acts. In this section, we discuss the policies that changed these maximums and their impact on aggregate student loan originations.

The Higher Education Reconciliation Act (HERA) of 2006 increased the yearly borrowing caps for subsidized loans, which had remained unchanged since 1992, for freshmen to \$3,500 from \$2,625 and to \$4,500 from \$3,500 for sophomores. Borrowing limits for upperclassmen remained unchanged at \$5,500. Signed into law in February of 2006, the act took effect July 1, 2007, so that the change was in place and well anticipated prior to the 2007–2008 academic year. Though HERA affected borrowing for subsidized loans and unsubsidized loans (because, as described above, the cap is technically a combined subsidized/unsubsidized borrowing cap), we expect this legislation to mainly increase originations of subsidized loans, since if eligible, students

**Table 1**  
**Changes in Title IV federal aid program maximums**

Year	Subsidized loans				Unsubsidized loans				Pell Grants
	Y1	Y2	Y3-4	Grad	Y1-4(D)	Y1/Y2(I)	Y3-4(I)	Grad	Y1-4
00-01	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	3,350
01-02	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	3,750
02-03	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	4,000
03-04	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	4,050
04-05	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	4,050
05-06	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	4,050
06-07	2,625	3,500	5,500	8,500	0	4,000	5,000	10,000	4,050
07-08	3,500	4,500	5,500	8,500	0	4,000	5,000	12,000	4,310
08-09	3,500	4,500	5,500	8,500	2,000	6,000	7,000	12,000	4,731
09-10	3,500	4,500	5,500	8,500	2,000	6,000	7,000	12,000	5,350
10-11	3,500	4,500	5,500	8,500	2,000	6,000	7,000	12,000	5,550
11-12	3,500	4,500	5,500	8,500	2,000	6,000	7,000	12,000	5,550

This table shows changes to the maximums (caps) (reported as dollar amounts) of the Federal Direct Loan and Pell Grant Program. Y1, Y2, Y3, Y4, and Grad are, respectively, the maximums for undergraduate freshmen, sophomores, juniors, seniors, and graduate students. (D) and (I) refers to dependent and independent students. See Section 2 for more details.

Source: Higher Education Act, subsequent amendments, and ED appropriations.

would always take out a subsidized over an unsubsidized loan. Thus, HERA would only affect *unsubsidized* borrowing for freshman and sophomores that met two criteria; first, they did not have enough financial need to qualify to take out the entire program maximum in subsidized loans, and second, they chose to borrow the difference between the program maximum and their personal maximum in the form of unsubsidized loans. These two joint conditions apply to less than 1% of students in our sample, suggesting that unsubsidized borrowing was not significantly increased in direct response to HERA. In comparison, Table 2 shows that roughly 26% of undergraduates in 2004 were borrowing subsidized loans at the subsidized loan cap, corresponding to 63% of subsidized loan borrowers, and 76% of students who, based on their financial need, were eligible to borrow at the cap. This table confirms that the subsidized loan maximum was highly relevant in determining subsidized loan borrowing before the policy.

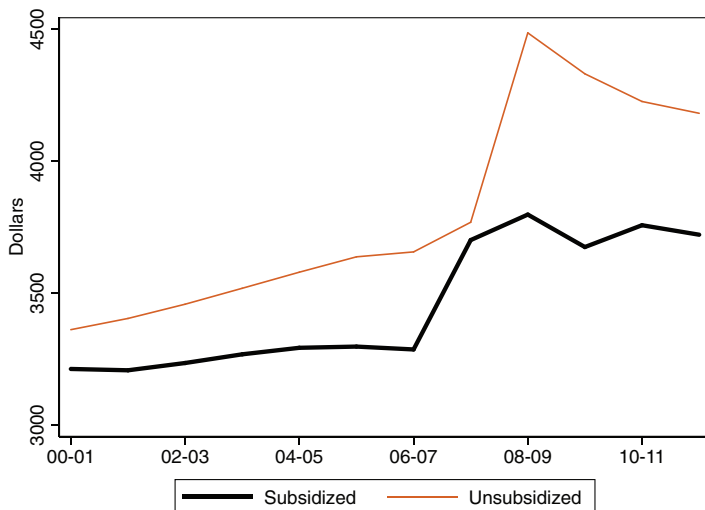
The data confirm that HERA substantially affected subsidized borrowing. In the 2007–2008 year, subsidized loan originations to undergraduates jumped from \$16.8 billion to \$20.4 billion (Figure 3), and consistent with the higher usage intensity, the average size of a subsidized loan rose from under \$3,300 to \$3,700, as shown in Figure 5, which reports average loan amounts per borrower. Unsubsidized loan originations show much smaller increases in 2007–2008, with the total amount borrowed by undergraduates increasing from \$13.6 to \$14.7 billion, and the average per-borrower amount increasing from \$3,660 to \$3,770. Because the majority of the impact of HERA was on subsidized borrowing, we subsequently refer to HERA as affecting the subsidized borrowing maximum to avoid confusion with legislation passed in subsequent years that primarily affected unsubsidized borrowing.

We provide additional evidence that these increases were due to the changes in the program maximums using loan-level data from the New York Fed/Equifax

**Table 2**  
**Fraction of students borrowing at the policy maximums**

	2004	2008	2012
		Subsidized loans	
All students	0.26 (1)	0.24 (1)	0.29 (1)
Borrowing	0.63 (0.41)	0.55 (0.43)	0.58 (0.50)
Cap-eligible	0.76 (0.34)	0.65 (0.37)	0.64 (0.46)
		Unsubsidized loans	
All students	0.05 (1)	0.07 (1)	0.24 (1)
Borrowing	0.26 (0.20)	0.31 (0.23)	0.52 (0.47)
Cap-eligible	0.62 (0.08)	0.63 (0.11)	0.76 (0.32)

This table shows the proportion of students that took out loans at the subsidized and unsubsidized loan maximums in each NPSAS survey year. The first row in each panel uses all undergraduate students, the second row uses only the students who borrowed in the subsidized (unsubsidized) loan program, and the third row uses only the students who, based on their cost of attendance and EFC, were eligible to borrow at the maximum. In parentheses, we report the fraction of all students belonging to these populations (e.g., in 2004, 41% of all students borrowed in the subsidized loan program, and 63% of those students borrowed at the subsidized loan cap). *Source:* NPSAS.

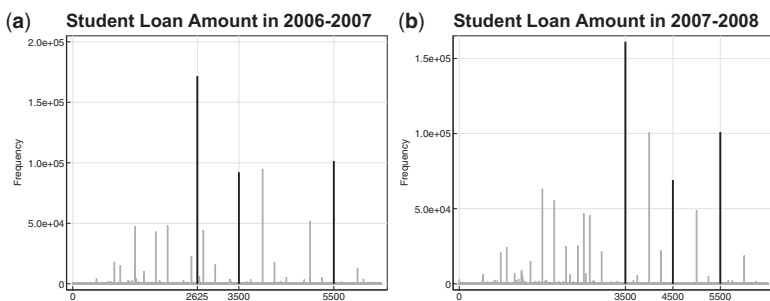


**Figure 5**  
**Per-borrower subsidized and unsubsidized federal student loan amounts**

This figure shows changes in the average borrowed amounts in the subsidized and unsubsidized loan programs. *Source:* IPEDS, Title IV.

Consumer Credit Panel.<sup>8</sup> These data cannot distinguish between federal and private student loans or subsidized and unsubsidized loans, but in Figure 6, we produce a histogram of all student loan amounts in the 2006–2007 school year and again for the 2007–2008 school year, after the policy change. The “before”

<sup>8</sup> A number of papers have used these data to study loan repayments (see, e.g., Lee et al. 2014). We use this alternative source because the NPSAS data are only available in the years 2004, 2008, and 2012 and are a repeated cross-section rather than a panel.



**Figure 6**

**Distribution of student loan amounts**

These figures plot the distribution of student loan amounts in the NY Fed CCP/Equifax panel in the year before (2006:Q3–2007:Q2) and after (2007:Q3–2008:Q2) the change in the subsidized loan maximum. The maximums are marked on the x-axis for each academic year.

Source: NY Fed CCP/Equifax. (a) Student Loan Amount in 2006-2007. (b) Student Loan Amount in 2007-2008.

plot shows a large mass of borrowers concentrated at the unconventional amount of \$2,625, the subsidized maximum for freshmen borrowers. In contrast, the “after” plot shows the largest mass of borrowers concentrated at \$3,500, the new maximum. The plots also show a large mass of borrowers at cap amounts established for upperclassmen before and after the policy change. This shift is evidence that there was a large and immediate effect of the policy change on loan amounts.

The second loan policy change we study is the Ensuring Continued Access to Student Loans Act of 2008. Prior to this act, in addition to the subsidized amounts discussed above, independent students were eligible for as much as \$5,000 (\$4,000 for freshman and sophomores) in additional unsubsidized loans. Dependent students were ineligible for these additional loans.<sup>9</sup> This act increased the maximums by \$2,000 for all students, meaning dependent students were eligible for \$2,000. Figure 3 shows that undergraduate unsubsidized loan originations jumped from under \$15 billion to \$26 billion in 1 year. Despite this large increase however, the second panel of Table 2 does show that conditional on being eligible for the maximum or conditional on borrowing anything at all, students were substantially less likely to borrow at the additional unsubsidized maximum than for subsidized loans.

It is also worth noting that the act was passed in anticipation of private student loans becoming more difficult to obtain due to the financial crisis, and so some or all of these new originations may have partly replaced private loans. Additionally, the act was passed in May 2008, after many financial aid packages had already been sent out for the academic year 2008–2009. Schools were told

<sup>9</sup> Students must meet certain requirements (e.g., being over 24 years of age, being a graduate or professional student, or being married) to be considered an independent student by the Federal Student Aid office; otherwise, they are considered dependent and assumed to have parental support, and thus may qualify for less aid.

they could revise their offers to accommodate the new policies for the upcoming school year, which seems to have been often the case based on the data series. That said, due to the timing of the change, the full impact of the higher caps may have had real effects in more than a single year.

While Pell Grants are not the main focus of this paper, Pell Grant maximums were adjusted several times during our sample period, rising from \$3,350 to \$5,550 between 2000–2001 and 2011–2012<sup>10</sup> (see Table 1). We control for these changes in our analysis. Pell Grant disbursements are plotted in Figure 4 against aggregate loan amounts; both show large increases over our sample period.

**2.2.1 Evidence from earnings calls.** Before turning to a systematic analysis of the effect of these policies on tuition, we provide some direct evidence of the relevance of these policy changes to tuition at for-profit universities by looking at earnings call discussions between senior management at for-profit universities and analysts around the time of the policy changes we study. Below, we quote from an earnings call of one of the most prominent for-profit education companies, the Apollo Education Group (which operates the University of Phoenix) in early 2007:

<Operator>: Your next question comes from the line of Jeff Silber with BMO Capital Markets.  
<Q - Jeffrey Silber>: Close, it is Jeff Silber. I had a question about the increase in pricing at Axia; I'm just curious why 10%, why not 5, and why not 15, what kind of market research went into that? And also if you can give us a little bit more color potentially on some of the pricing changes we may see over the next few months in some of the other programs?  
<A - Brian Mueller>: The rationale for the price increase at Axia had to do with Title IV loan limit increases. We raised it to a level we thought was acceptable in the short run knowing that we want to leave some room for modest 2 to 3% increases in the next number of years. And so, it definitely was done under the guise of what the student can afford to borrow. In terms of what we will do going forward with regards to national pricing we're keeping that pretty close to the vest. We will implement changes over time and we will kind of alert you to them as we do it.

Source: Apollo Education Group, 2007:Q2 Earnings Call, accessed from Bloomberg LP.

As evidenced by this quote, Title IV loan limit increases appear to directly affect how this institution chose to set its tuition in those years, and we provide additional excerpts in Appendix B. In Appendix C, we also show that the passage of the three pieces of student aid legislation were associated with nearly 10% abnormal returns for the portfolio of all publicly traded for-profit institutions. This is consistent with the fact that changes in Title IV maximums had large implications in terms of demand at these institutions. We turn to this issue in the rest of the paper using a statistical model.

<sup>10</sup> The 2001–2004 changes were made through the appropriation process which then froze the caps at \$4,050 for 4 years, until the Revised Continuing Appropriations Resolution of 2007 and the College Cost Reduction and Access Act scheduled more increases. These maximums are only available to students with an EFC below a certain threshold. However, students with slightly higher EFCs are eligible for smaller Pell Grants, according to a scale. For all of the policy changes we consider, these smaller Pell Grants increased proportionately with the maximum Pell Grant.

### 3. Data

We overview the data sources and sample used in the analysis and provide a more detailed description of each of the data sources in Appendix C. We use data from three main sources from the Department of Education: Integrated Postsecondary Education Data System (IPEDS), Title IV Administrative Data from the Federal Student Aid Office, which we refer to as “Title IV” data, and the restricted-use student-level National Postsecondary Student Aid Survey (NPSAS) data set.

Our measures of sticker price and enrollment come from IPEDS. IPEDS is a system of surveys conducted annually by the National Center for Education Statistics (NCES) with the purpose of describing and analyzing trends in postsecondary education in the United States. All Title IV institutions are required to complete the IPEDS surveys. Though IPEDS began in 1980, the survey covering sticker-price tuition was changed significantly in the 2000–2001 school year, and we thus start our sample in this year.

We measure federal aid amounts at the institution level using the Title IV Program Volume Reports, which report yearly institutional-level total dollar amounts and the number of recipients for each federal loan and grant program. These data are available beginning with the 1999–2000 academic year separately for subsidized loans, unsubsidized loans, and Pell Grants.<sup>11</sup> We end our sample in 2011–2012 to exclude the 2012–2013 school year and following years, when graduate students became ineligible to receive subsidized loans as a result of the Budget Control Act of 2011, which would complicate our measure of these loans.

Merging Title IV and IPEDS data, we obtain an annual panel of federal loan borrowing, Pell Grants, enrollment and sticker-price tuition for the universe of Title IV institutions. This sample contains 5,560 unique institutions. Institutional grant measures (graduate and undergraduate) are available from the IPEDS Finance survey for all the years in our sample, but we find them to be quite noisy as they come from survey data with changing definitions over time and across types of institutions and are reported as institutional totals rather than per-student values.

Finally, we merge the IPEDS/Title IV panel to NPSAS, a restricted-use student-level data set from NCES. NPSAS is a collection of surveys conducted approximately every 4 years starting in 1988 with a nationally representative sample of about 100,000 students at a cross-section of Title IV institutions. The primary purpose of the NPSAS data is to study student financing of higher education and they thus have detailed information on the amount and type

---

<sup>11</sup> Unfortunately, it does not separate loans given to undergraduates and loans given to graduate students until 2011 (Pell Grants are only given to undergraduates). However, because imputing the amount for undergraduates would require making several assumptions, we measure loan and grant usage at an institution using the total dollar amount scaled by the enrollment count (undergraduate and graduate, on a full-time-equivalent (FTE) basis) of the institution.

**Table 3**  
**Estimation sample composition**

	IPEDS-TitleIV	EstimationSamp
Selective 4Y NFP	0.13	0.22
Somewhat selective 4Y NFP	0.22	0.25
Not selective 4Y NFP	0.11	0.18
Two-year NFP	0.26	0.28
For-profit	0.27	0.07
Sticker tuition	13,343	14,549
EFC	29,754	29,982
Sub. loans	2,002	1,547
Unsub. loans	2,140	1,717
Pell grants	1,485	1,097
Inst. grants	2,239	2,734
FTE enrollment	3,441	7,144
N inst	4,400	930
N students	9,800,000	4,500,000
Observations	39,850	8,900

This table summarizes mean values of key variables in the comprehensive IPEDS-Title IV sample and compares them to our estimation sample, which includes only those institutions that were surveyed in the 2004 NPSAS wave.

of loans that each student takes out. We mainly rely on the 2004 NPSAS to document prepolicy cross-sectional variation that is only possible to observe with student-level data, since these data allow us to observe not just institutional-level loan and grant totals, but the number of students who are constrained by each of the policy maximums. The 2004 NPSAS contains this detailed financing data for students attending 1,334 unique institutions, with an average (median) of 104 (85) students surveyed per institution.<sup>12</sup> Our final estimation sample is dictated by the merge of the Title IV/IPEDS data with NPSAS and uses only institutions with NPSAS sample sizes of more than 10 students, giving a sample of 930 institutions.

We compare the representativeness of our main estimation sample with the full IPEDS/Title-IV sample along several dimensions in Table 3. We follow Looney and Yannelis (2015) and classify institutions into one of five categories: for-profit and not-for-profit split by 2-year, selective, somewhat selective, and nonselective 4-year institutions (selectivity as measured from Barron's Profile of Colleges 2007 edition). As previously noted, Table 3 confirms that a key difference in the estimation sample is that in relative terms NPSAS contains much fewer for-profit institutions than IPEDS. We also find that the institutions in our sample are larger on average, and have somewhat fewer average loans and grants per student. Other characteristics are broadly similar. Due to the limited number of for-profit institutions we attempt to supplement our main analysis with a comparison of for-profit and other institutions towards the end of the paper.

<sup>12</sup> We also employ the 2008 NPSAS survey for robustness. The survey contains 1,697 unique institutions with an average (median) of 111 (87) students surveyed per institution.

**Table 4**  
**Summary statistics**

	Mean	SD	Min	Max	Count
$\Delta$ StickerTuition <sub>it</sub>	796	726	-2,906	4,295	8,900
$\Delta$ PellGrants <sub>it</sub>	108	243	-1,429	1,883	8,900
$\Delta$ SubLoans <sub>it</sub>	86	248	-1,419	1,710	8,900
$\Delta$ UnsubLoans <sub>it</sub>	142	396	-2,918	4,127	8,900
SubLoanExp <sub>i</sub>	0.16	0.14	0	0.74	8,900
UnsubLoanExp <sub>i</sub>	0.28	0.2	0	0.8	8,900
PellGrantExp <sub>i</sub>	0.33	0.18	0	1	8,900
SubLoanExp08 <sub>i</sub>	0.086	0.081	0	0.6	5,810
UnsubLoanExp08 <sub>i</sub>	0.28	0.17	0	0.83	5,810
PellGrantExp08 <sub>i</sub>	0.37	0.14	0.022	0.92	5,810
$\Delta$ InstGrant <sub>it</sub>	194	587	-11,308	10,447	8,370
$\Delta$ StickerTuition <sub>it</sub> - $\Delta$ InstGrant <sub>it</sub>	620	805	-9,947	11,617	8,370
100 × $\Delta$ log(FTE <sub>it</sub> )	2.4	8.4	-49	54	8,330
$\Delta^2$ StateFunding <sub>it</sub>	1.6	2,823	-174876	112670	8,310
$\Delta^2$ FederalFunding <sub>it</sub>	138	824	-6,308	6,741	8,280
$\Delta^2$ OtherFunding <sub>it</sub>	458	3,327	-65,550	85,134	8,310
$\Delta^2$ PrivateFunding <sub>it</sub>	254	8,158	-71,856	73,105	8,270

This table reports summary statistics for the variables included in the regression tables. The unit of observation is a year (*t*) and institution (*i*). The sample starts in 2002 and ends in 2012. The  $\Delta$  operator indicates annual changes (between year *t* and *t* - 1). Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Section 3 and Appendix D provide additional details on the variables.

Table 4 reports summary statistics for the variables included in the regressions.

#### 4. Empirical Method

We present the difference-in-differences specification used to isolate the impact of the federal loan credit expansion on tuition. Our empirical approach is similar to that of Card (1992), who studies the effect of a change in national minimum wage standards using a cross-state treatment effect based on the fraction of workers earning less than the minimum wage before the policy. In our setting, we construct an institution-specific treatment intensity measure based on the fraction of students in each institution that are eligible for and take out the program maximums.

This approach is also economically similar to empirical strategies that try to identify effects of aggregate economic shocks through cross-sectional variation in the importance of those shocks across different geographical regions (e.g., Bartik 1991; Blanchard and Katz 1992; see Goldsmith-Pinkham et al. 2017 for a review; see Deming and Walters 2017 for another recent example in postsecondary education). Like in these studies, and in contrast to standard difference-in-differences specifications, our treatment variable is measured in economic units (change in the loan maximums) rather than as an indicator for a policy change. This allows for the interpretation of our point estimates in terms of a passthrough rate from loan maximums to tuition. But more closely to difference-in-differences specifications, the economic shocks (policy changes) in this paper (with the important exception of Pell Grants) are clustered over 2

years, as opposed to taking place gradually over many years, thus sharpening identification.

In this section, we first discuss the construction of the treatment intensity, or “policy exposures,” and then describe the empirical specification.

#### **4.1 Policy exposures**

We use the student-level data from NPSAS to define a relatively precise measure of the prepolicy importance of different types of aid at each institution. Consider first the case of subsidized loans. If a student’s individual maximum is below the program maximum, she cannot qualify for the program maximum and is thus unaffected by any changes to it. Additionally, some students may choose to borrow less than the amount they are eligible for and will thus also be unaffected. We thus define an institution’s “exposure” to the subsidized loan policy change as the fraction of undergraduate students who borrowed subsidized loans at the policy maximum in 2004, since this corresponds to approximately the fraction of students we would expect to be able and willing to take advantage of the policy change to borrow more subsidized loans.

We also evaluate the effect of the 2008–2009 increase of \$2,000 in additional unsubsidized loans for all students. We separately calculate the exposures of dependent and independent students at each institution, and take the sum as the overall institution exposure. For independent students, we again take the fraction of students who were borrowing at the independent policy maximum in 2004. For dependent students, who were previously ineligible for unsubsidized loans and became eligible through the policy change, we construct a shadow participation rate since we cannot observe past participation. This measure captures the subset of eligible students, or the fraction of dependent students at each institution, that borrowed the maximum amount of subsidized loans that they were eligible for, including students who were not eligible for any subsidized loans.<sup>13</sup> The intuition for this rule is that a student that could, but did not, borrow in the subsidized program will not borrow in the unsubsidized program, as it is more expensive to do so, and should therefore not be counted as a student constrained by the unsubsidized program cap. However, this measure is likely not to be as reliable as the one for subsidized loans, since it assumes that any dependent student borrowing the maximum amount of subsidized loans would also borrow the maximum amount of unsubsidized loans once eligible.

Finally, for Pell Grants, changes in the maximum Pell Grant amounts shift the supply of grants for all grant recipients. Thus, the Pell Grant exposure variable is calculated as the percentage of students at a given institution awarded any positive Pell Grant amount as of 2004.

---

<sup>13</sup> As discussed in Section 2, because subsidized loans are need based, whereas unsubsidized loans are not, some students may be eligible for unsubsidized loans only.

Table 4 reports summary statistics for the exposure measures as of 2004. The average institution in our sample had about 16% of students borrowing at the subsidized loan cap in 2004 compared to 28% of students at the unsubsidized cap. In contrast, the average institution had about 33% of students receive a positive (not necessarily the maximum) amount of Pell Grant. The exposures also display significant variation, with a standard deviation of between 14% (subsidized loans) and 20% (unsubsidized loans). The table also reports summary statistics for the exposure variables computed from the 2008 NPSAS, for those institutions that reported both in the 2004 (baseline sample) and in the 2008 survey. Average levels of Pell Grant and unsubsidized loan exposures are very similar in the two surveys, but the subsidized exposure is significantly smaller, owing to the fact that the second NPSAS wave takes place after the increase in the subsidized loan maximum. Indeed, as the maximums are increased, the fraction of capped students should drop unless all students at the old maximum jump to the new maximums.

#### 4.2 Empirical specification

We regress the date  $t$  yearly change in institution  $i$  characteristic  $Y_{it}$

$$\Delta Y_{it} = \sum_a \beta_a \text{ExpFedAid}_{ai} \times \Delta \text{CapFedAid}_{at} + \gamma X_{it} + \delta_i + \phi_t + \epsilon_{it}, \quad (3)$$

on a set of controls, where  $i$  denotes an institution,  $t$  is a year and  $a$  indicates either subsidized loans, unsubsidized loans, or Pell Grants. In the main result, the dependent variable  $Y_{it}$  is changes in sticker tuition. We also use changes in aid amounts as the dependent variable to validate the treatment intensity, and in additional results, we explore effects using changes in institutional grants and enrollments as the dependent variable.

The main coefficient of interest is  $\beta_a$ , which measures the sensitivity of tuition changes to changes in the program maximums for each aid type  $a$ . The specification accomplishes this by interacting the program cap change ( $\Delta \text{CapFedAid}_{at}$ ) with the institutional-level treatment intensity measure described above ( $\text{ExpFedAid}_{ai}$ ). We estimate all three  $\beta_a$  coefficients simultaneously to control for correlations in exposures, timing of the policy changes, and substitution effects. Our regressions are specified in changes with institutional fixed effects  $\delta_i$  because there is wide dispersion across our sample in tuition charged (ranging from a few hundred dollars to about \$45,000), and tuition increases are often set as a percentage of past tuition. Institutional fixed effects allow us to control for the correlation of tuition increases with past tuition levels and look for abnormally large increases at the institution level. We validate that this allows us to meet the parallel trends assumption using placebo tests in Section 6. We include year effects  $\phi_t$  to control for economy-wide factors (e.g., increased demand for postsecondary education) that may have induced all institutions to increase their tuition more in some years than

**Table 5**  
**Baseline regression specification**

	(1) $\Delta \text{SubLns}_{it}$	(2) $\Delta \text{UnsubLns}_{it}$	(3) $\Delta \text{PellGrts}_{it}$	(4) $\Delta \text{STuition}_{it}$
$\text{SubLoanExp}_i \times \Delta \text{SLCap}_t$	0.571*** [0.12]	-0.080 [0.13]	-0.000 [0.07]	0.643*** [0.20]
$\text{UnsubLoanExp}_i \times \Delta \text{USLCap}_t$	0.041* [0.02]	0.598*** [0.04]	-0.043*** [0.01]	0.202*** [0.05]
$\text{PellGrantExp}_i \times \Delta \text{PGCap}_t$	-0.349*** [0.08]	-0.379*** [0.11]	1.343*** [0.08]	0.214 [0.17]
Inst and Year FEs?	Yes	Yes	Yes	Yes
Adj R <sup>2</sup>	0.09	0.24	0.50	0.38
N obs	8,900	8,900	8,900	8,900
N inst	930	930	930	930

The first four columns in this table report ordinary least squares (OLS) regression estimates of yearly changes in Pell Grants and subsidized/unsubsidized loan amounts per full-time equivalent student, and sticker tuition on interactions between cross-sectional institution exposures and yearly changes in program caps. The last column reports IV regression estimates of the effect of changes in federal loans and grants on sticker price tuition. The dependent variable is the annual change in sticker price tuition at the institution level. Observed changes in federal grants and loans per enrolled student are instrumented by the products of the corresponding aid exposures and changes in program caps, as described in the text. The unit of observation is a year ( $t$ ) and institution ( $i$ ). The sample starts in 2002 and ends in 2012. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

others. Finally, we control for a set of other controls  $X_{it}$  as described in the results section.

## 5. Main Empirical Results

### 5.1 Sticker tuition and aid sensitivity to changes in program caps

**5.1.1 Baseline specification.** Table 5 presents our main results on aid and sticker tuition sensitivities to the policy changes, measured as the product of the yearly change in each program cap (only varies over time) and the treatment intensity measure (only varies cross-sectionally) based on the fraction of students at each institution that qualify for (and are likely to accept) the increased student aid amounts. Each regression is estimated between the 2001-2002 and 2011-2012 academic years and includes year and institution fixed effects, with standard errors clustered at the institution level to account for serial correlation of the error terms.

Columns 1–3 validate our treatment measure by regressing yearly changes in student aid levels on the product of treatment intensity and policy change. In Columns 1 and 2, we find that yearly changes in subsidized loans load on the institutional-level change in the loan maximum with a coefficient of 0.57, and similarly unsubsidized loans load with a coefficient of about 0.6 on the unsubsidized maximum, suggesting that the demand elasticity for these loans is quite high. Both coefficients are different from zero and one at conventional levels. In Column 3, we find a coefficient for Pell

Grants of 1.3, suggesting that an increase in Pell Grant availability results in about one-for-one increase in the equilibrium grant amount disbursed, that is, that the demand elasticity for these grants is infinite, which is unsurprising.<sup>14</sup>

It is also interesting to look at substitution across aid types: In Column 3, we also observe that the coefficients of Pell Grant usage on changes in unsubsidized and subsidized loan maximums are essentially zero, implying that the greater availability of these other sources does not displace Pell Grants. On the other hand, in Columns 1 and 2, the changes in Pell Grant maximums enter each loan regression with a negative and statistically significant sign, suggesting that a greater availability of Pell Grants displaces other loan aid. This crowd-out effect may be the result of a lower demand or reduced eligibility for loans as implied by Equations (1) and (2) and is consistent with Marx and Turner 2015, who find using a kink regression discontinuity design that increases in Pell Grant aid lower student loan borrowing.

Having documented the large responses of federal aid amounts to our treatment variables, we focus next on treatment effects on sticker tuition. Point estimates (Column 4) suggest that a dollar increase in the subsidized cap and unsubsidized caps result in a 64-cent increase in sticker price ( $t$ -stat = 3.2), and 20-cent increase ( $t$ -stat = 4), respectively, while we find a positive but not significant effect on tuition from the increases in the Pell Grant maximum. These estimates lend support to the Bennett hypothesis in the context of loans, and are economically large. Since we are measuring sticker tuition, these estimates likely imply that the increased credit availability increases tuition for most students, rather than just those who are borrowers. The lack of a significant effect of Pell Grants could be seen as somewhat surprising because one might expect a greater demand response to a grant than a loan where the principal must be repaid. However, we note that the empirical identification of the Pell Grant effect is significantly more tenuous, as cap changes were smoother and took place over several years. Additionally, Pell Grants are available to a more restricted (more low-income) set of students, which may affect both their elasticity of demand and the way that universities respond to changes in their ability to pay. As discussed in Section 1, both the demand elasticity of the affected group and the relative weight that universities put on enrolling that group will affect the price impact of the increased aid availability. Thus, the limited impact of Pell Grants could also be explained by different elasticities of demand between students qualifying for Pell Grants and students qualifying for loans, or by the fact that universities, which may target a certain combination of high- versus low-income students, may decide to limit the price impact of a Pell Grant increase.

---

<sup>14</sup> The model in Appendix A abstracts from the differences in interest and principal payment across types of aid. But a straightforward extension would predict that the elasticity of Pell Grant demand should be infinite given that grants are not subject to repayment.

**Table 6**  
**Regression estimates for institutional grants and enrollments**

	(1) $\Delta ST_{it}$	(2) $\Delta IG_{it}$	(3) $\Delta ST_{it} - \Delta IG_{it}$	(4) $100 \times \Delta \log(FTE_{it})$
SubLoanExp <sub><i>i</i></sub> × $\Delta$ SLCap <sub><i>t</i></sub>	0.598*** [0.21]	-0.274 [0.27]	0.872** [0.34]	-0.002 [0.00]
UnsubLoanExp <sub><i>i</i></sub> × $\Delta$ USLCap <sub><i>t</i></sub>	0.172*** [0.05]	0.023 [0.05]	0.149** [0.07]	-0.002*** [0.00]
PellGrantExp <sub><i>i</i></sub> × $\Delta$ PGCap <sub><i>t</i></sub>	0.148 [0.18]	-0.208 [0.13]	0.356* [0.21]	0.014*** [0.00]
Inst and Year FEs?	Yes	Yes	Yes	Yes
Adj R <sup>2</sup>	0.39	0.16	0.11	0.10
N Obs	8,290	8,290	8,290	8,290
N Inst	850	850	850	850

This table reports OLS regression estimates of yearly changes in institution grant expenditure per FTE, difference between sticker price and institution grant expenditure and percentage growth rate of FTE on interactions between cross-sectional institution exposures and yearly changes in program caps. The unit of observation is a year (*t*) and institution (*i*). The sample starts in 2002 and ends in 2012. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

## 5.2 Net tuition, institutional grants, and enrollments

**5.2.1 Net tuition and institutional grants.** Because many universities award institutional grants, not all students pay the sticker tuition price for their education, and because many of these grants are need-based, it is likely that many students who borrow in the federal student loan program may not be paying sticker price. However, as discussed in Section 1, when capacity is imperfectly elastic in the short run, aid to one group of students will create a pecuniary demand externality that boosts prices paid by nonaid recipients. The results of our baseline regression show that nonrecipients (in particular, students paying sticker-price tuition) do indeed see price increases following an increase in loan supply. It is possible that universities increased sticker-price tuition while simultaneously increasing institutional grants, so that only sticker-price-paying students were ultimately affected by the increase in federal student loan caps.

We investigate this question using measures of institutional grants from the IPEDS Finance Survey. These measures are fairly noisy, and as shown in Column 2 of Table 6, we are unable to precisely estimate any of the potential effects. However, the magnitudes of the coefficients suggest that, if anything, increases in subsidized loan and Pell Grant supply were associated with decreases in institutional grants. This suggests, as we show in Column 3 of Table 6, that using average net tuition rather than sticker tuition would result in an even larger estimate of the effect of loan supply on tuition costs than our baseline estimate. For Pell Grants, Column 4 shows that including institutional grants increases the magnitude of the estimated effect and also its precision. This result, though still weakly estimated, would be consistent with Turner (2014), who, using a regression discontinuity approach, finds that institutions alter institutional aid to capture increases in Pell Grants.

**Table 7**  
**Changes in the distribution of the ratio of net to sticker tuition in NPSAS 2004/2008**

Full sample (405 inst.)	2004	2008
25th pctile	0.823	0.775
50th pctile	0.936	0.906
75th pctile	0.973	0.972
Above-median SubExp (202 inst.)		
25th pctile	0.750	0.706
50th pctile	0.889	0.857
75th pctile	0.953	0.954
Below-median SubExp (203 inst.)		
25th pctile	0.896	0.845
50th pctile	0.982	0.952
75th pctile	0.991	0.988

This table uses the 2004 and 2008 NPSAS waves to calculate the 25th, 50th, and 75th percentiles of the within-institution distribution of the fraction of sticker tuition paid for all institutions that appear in both NPSAS waves. The top panel presents numbers for all institutions, and the bottom panels sort by their subsidized loan exposure.

An average increase in net tuition could still of course leave open the possibility of more nuanced distributional effects. In particular, institutions may use price discrimination to redistribute tuition increases captured from credit limit increases to lower-income students. To address this question one would need a yearly panel of net tuition at the student level and at yearly frequency. Short of that, in Table 7, we use the pre- and postpolicy NPSAS waves to broadly examine net tuition increases or declines by quartiles in the net tuition distribution. We restrict the sample to institutions appearing in both the 2004 and 2008 NPSAS waves (405 institutions, or less than half from the baseline). Because the 2008 NPSAS wave refers to the 2007–08 academic year, this analysis only includes the 2007–2008 change in subsidized loan caps. Within each institution, we sort students by sticker tuition net of institutional grants. This is the amount that students are required to pay to the institution through either (1) federal/state grants and loans or (2) out-of-pocket. We report the average 25th, 50th, and 75th percentile values of the net tuition/sticker tuition ratio in 2004 and 2008 for all institutions, above-median subsidized exposure institutions and below-median subsidized exposure institutions.

The results for the full sample of institutions (top panel of Table 7) indicate that the ratio of net tuition to tuition decreased for the 25th percentile student from 2004 to 2008, moving from an average of 82.3% to 77.5%. The net tuition/sticker tuition ratio also decreased for the median student, on average, from 93.6% to 90.6%. The average student at the 75th percentile did not see any change. This suggests that in 2008 relative to 2004, lower-income students were paying smaller fractions of sticker tuition and thus that increases in sticker tuition that occurred between 2004 and 2008 did not pass through to net tuition at equal rates across the distribution of students. However, comparing the two lower panels of Table 7, we find that the pattern is similar for the more-exposed and less-exposed institutions. Thus, the patterns may represent a general trend toward more price discrimination, rather than a specific response to the loan

policies we study. We return to these results as we discuss welfare implications of the student credit expansion in the paper's conclusion.

**5.2.2 Expenditures.** To evaluate the implications of the increase in federal student loan maximums, it is important to measure how universities used their increased tuition revenues. We report in the Online Appendix parameter estimates of the baseline regression specification using measures of various categories of per-pupil spending as the dependent variable. With the exception of a reduction in institution expenditure in student grants we find very few significant changes with respect to our exposure measures. Unfortunately, this result does not allow us to distinguish between cases in which money was not immediately spent, when the money went towards increased enrollment, or because our spending measures are noisy survey measures.

**5.2.3 Enrollments.** One of the main motivations for federal student aid is to relax participation constraints in postsecondary education, so understanding whether enrollment, in addition to price, responds to changes in loan supply is another crucial element to assess the welfare impact of these policies. To study enrollment effects we regress annual changes in enrollment on our measures of treatment intensity interacted with the timing of policy changes. As shown in Column 4 of Table 6, we find a positive and statistically significant coefficient on institution-specific changes in caps for Pell Grants, but an insignificant coefficient on subsidized loan caps and a significant but tiny negative coefficient on unsubsidized loan caps. The point estimate on Pell Grants is economically significant—for example, the 2010 increase in Pell amounts at the mean Pell exposure  $((5350 - 4731) \times .34 = 210)$  would have implied a boost in enrollment of about 2.9%—and is also consistent with the literature on grants and college participation (see, e.g., the review of Deming and Dynarski (2009)).<sup>15</sup> The relative ordering of these effects is consistent with economic priors, since, as previously noted, demand elasticities are largest for Pell Grants because the principal does not have to be repaid.

## 6. Additional Empirical Results

We first discuss the robustness of the main empirical findings from the previous section. We then attempt to identify the set of institutions for which the passthrough from student loans to tuition was strongest and finally, we focus on for-profit institutions, which are under-represented in the main sample.

---

<sup>15</sup> They conclude that most studies of federal aid find that additional grant aid is associated with significant increases in attendance (e.g., Sefor and Turner (2002) for Pell Grants; Angrist (1993), Stanley (2003), Bound and Turner (2002) for GI bills; Dynarski (2003) for Social Security student benefit program), though, for Pell Grants the evidence is mixed, as (Hansen (1983) and Kane (1995) find no significant increase in attendance following the introduction of Pell Grants). Fewer studies look at federal loan aid; one exception is Dynarski (2002), who finds a very small effect on attendance and a larger effect on college choice.

## 6.1 Robustness of baseline specification

We attempt to address two potential concerns about the estimated effects of the student loan expansion on tuition. The first is measurement issues related to the Great Recession and other shocks to institution funding. A second concern is that treated and control groups may differ along important dimensions which more generally affect their tuition levels even in the absence of the changes in student aid maximums. We address this latter concern by studying the parallel trends assumption and by interacting policy changes with other institutional characteristics.

**6.1.1 Excluding the Great Recession.** Policy changes for the student loan programs went into effect in the 2007–2008 (subsidized loan limit) and 2008–2009 (additional unsubsidized loan limits) academic years. One may be concerned about the impact of the Great Recession on tuition in these years. On the demand side, a high unemployment rate may have boosted demand for education services at institutions with a student population that is more dependent on student aid. On the supply side, these same institutions may have experienced a drop in state appropriations or endowments. Both of these effects could have led to disproportionate tuition increases. However, tuition decisions each academic year are generally made in the first half of each calendar year. This means that the increase in the subsidized loan maximum predates the recession, as tuition for the 2007–2008 academic year would have been set in the spring of 2007. The unsubsidized loan policy comes into effect before the failure of Lehman Brothers, but after the start of the recession. In Table 8, we present estimates of the baseline tuition specification for tuition (repeated in Column 1), but only including data up to the 2008–2009 and 2007–2008 academic years. We find that the subsidized loan effect is unaffected by the shorter samples, and that the unsubsidized loan effect is robust to excluding years beginning with the 2009–2010 school year (excluding 2008–2009 would exclude the main policy change).

**6.1.2 Additional controls.** Our second robustness check adds a set of controls  $X_{it}$  to the baseline specification (3). Anecdotal evidence, such as the quotes in Section 2, suggests that for-profit universities may be more likely to raise tuition in response to increased credit availability. It is also possible that selective institutions may behave differently than nonselective institutions or 4-year institutions differently from 2-year institutions, since they may have different objective functions. Persistent differences in tuition increases between these types of institutions would be captured by the institution-level fixed effects that are included in our baseline specification, but to allow for differential responses in the years of the policy changes, we include interactions of institution type (as defined in Section 3) and year dummies in Column 1 of Table 9. The inclusion of these controls does not significantly change the point estimates on the measures of institution-specific program caps.

**Table 8**  
**Subsamples for baseline tuition regression specification**

	(1)	(2)	(3)
$\Delta\text{StickerTuition}_{it}$	Full sample	Pre-2009	Pre-2008
$\text{SubLoanExp}_i \times \Delta\text{SLCap}_t$	0.643*** [0.20]	0.526** [0.21]	0.615*** [0.21]
$\text{UnsubLoanExp}_i \times \Delta\text{USLCap}_t$	0.202*** [0.05]	0.181*** [0.05]	-0.442 [0.33]
$\text{PellGrantExp}_i \times \Delta\text{PGCap}_t$	0.214 [0.17]	0.086 [0.24]	0.277 [0.27]
Inst and year FEs?	Yes	Yes	Yes
Adj R <sup>2</sup>	0.38	0.40	0.37
N obs	8,900	6,410	5,550
N inst	930	910	900

This table reports OLS regression estimates of yearly changes in sticker tuition on interactions between cross-sectional institution exposures and yearly changes in program caps. The unit of observation is a year ( $t$ ) and institution ( $i$ ). Column 1 reproduces Column 4 in Table 5 and is estimated between 2002 and 2012. The other two columns restrict the estimation sample as noted. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

**Table 9**  
**Regression estimates with additional controls**

$\Delta\text{StickerTuition}_{it}$	(1)	(2)	(3)	(4)
$\text{SubLoanExp}_i \times \Delta\text{SLCap}_t$	0.649*** [0.23]	0.584** [0.23]	0.633*** [0.21]	0.530** [0.26]
$\text{UnsubLoanExp}_i \times \Delta\text{USLCap}_t$	0.136** [0.06]	0.113** [0.06]	0.148*** [0.05]	0.038 [0.07]
$\text{PellGrantExp}_i \times \Delta\text{PGCap}_t$	0.139 [0.20]	0.036 [0.26]	0.117 [0.18]	-0.132 [0.28]
$\Delta^2\text{StateFunding}_{it}$			-0.005 [0.00]	-0.005 [0.00]
$\Delta^2\text{FederalFunding}_{it}$			-0.010 [0.01]	-0.001 [0.01]
$\Delta^2\text{OtherFunding}_{it}$			0.002 [0.00]	0.001 [0.00]
$\Delta^2\text{PrivateFunding}_{it}$			-0.002*** [0.00]	-0.001 [0.00]
Inst and year FEs?	Yes	Yes	Yes	Yes
$\text{Type}_i \times \text{Year}_t$	Yes	No	No	Yes
$\text{AdmitRate04}_i \times \text{Year}_t$	No	Yes	No	Yes
$\text{EFC04}_i \times \text{Year}_t$	No	Yes	No	Yes
$\text{Tuition04}_i \times \text{Year}_t$	No	Yes	No	Yes
Adj R <sup>2</sup>	0.38	0.39	0.38	0.39
N obs	8,900	8,840	8,170	8,140
N inst	930	910	850	840

This table reports OLS estimates of the baseline model (Table 5) with the inclusion of additional controls. The additional cross-sectional controls (for which coefficients are not reported) are each interacted with year dummies. Changes in other sources of funding are computed over a 2-year period ( $\Delta^2$ ). The unit of observation is a year ( $t$ ) and institution ( $i$ ). The sample starts in 2002 and ends in 2012. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

The second column of Table 9 applies the same logic above to several other dimensions of heterogeneity that may differentially affect tuition and aid: a school's selectivity, average student income (as measured by the expected family contribution or EFC), and tuition level (all measured in 2004). For this set of controls we include continuous measures as for our treatment intensities interacted with year fixed effects. By including these interactions we can control for time-varying trends associated with these institutional characteristics. As shown in Column 2 of Table 9, these controls reduce the size of the coefficients on subsidized and unsubsidized loans somewhat, but both coefficients remain statistically significant at conventional levels.<sup>16</sup>

The third column of Table 9 controls for changes in other sources of funding that could be affecting tuition. As discussed in detail in the Online Appendix, universities fund their operations both from tuition revenue and from sources such as government appropriations and other sources, including private donations. Much discussion has been devoted to this topic (see, e.g., Congressional Research Service, 2014) particularly in the context of changes in state funding and private contributions. We thus supplement the baseline specification with the 2-year change (per-student) in these sources of institution revenue (to account for possible delays between the time in which these other sources of funding are known to administrators) as controls. We find that declines in private funding are weakly associated with increases in tuition, though the magnitudes are small. The coefficients of interest are similar to those in the baseline regression, and remain significant. Finally, in Column 4, we include the controls from Columns 1, 2, and 3 simultaneously (a total of about 100 additional control variables relative to the baseline). In this specification, the coefficient on unsubsidized loans loses significance, but the subsidized loan effect remains robust and of a similar magnitude to the previous specifications.

**6.1.3 Parallel trends analysis.** As a more general test of our identifying assumption that is agnostic about the potential sources of omitted variable bias, we perform a parallel trends test for a continuous treatment setting. In the baseline model (Equation 3), we identified tuition and aid sensitivities from the regression coefficients  $\beta_a$ , on each interaction measure of treatment intensity and program cap changes. To see if more and less exposed institutions experienced similar tuition and aid trends in the years when caps were not raised, we follow, for example, Autor (2003), and analyze how the  $\beta_a$ s would

<sup>16</sup> We note that for Pell Grants, the above controls absorb much of the variation of our treatment intensity measure, and thus it is unsurprising that our estimated treatment effect decreases substantially. In fact, EFC is highly correlated with the Pell Grant exposure but displays low to moderate levels of correlation with unsubsidized and subsidized loans. This is because the exposure to Pell Grants is based on the fraction of students receiving any positive grant amount (which is highly correlated with institution's mean student income levels), while loan exposures are only based on students at caps (which depend on a specific percentile of the income distribution).

have been estimated had we (as a placebo) analyzed cross-sectional differences in tuition and aid in years where no actual policy occurred. For each aid of type  $a$  we estimate the following:

$$\Delta Y_{it} = \sum_s \xi_{as} \text{ExpFedAid}_{ai} \times \mathbb{I}(\text{year}=s) + \sum_{\alpha \neq a} \beta_{\alpha} \text{ExpFedAid}_{\alpha i} \times \Delta \text{CapFedAid}_{\alpha t} + \delta_i + \phi_t + \epsilon_{it}. \quad (4)$$

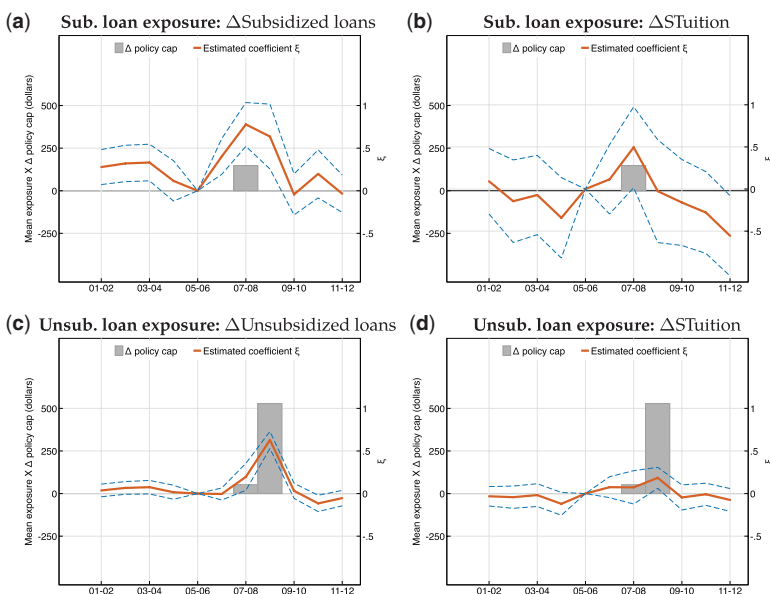
Here, we control for the other aid types ( $\alpha$ ) that are not subject to a placebo by interacting them with the corresponding actual changes in program caps as in the baseline specification (3). For aid  $a$ , instead, we estimate a series of yearly cross-sectional regressions of changes in tuition and aid on their exposures to aid. The coefficients  $\xi_{as}$  identify, in each year, abnormal changes in the dependent variables relative to the omitted or baseline year. We set the baseline year to be 2006, which is when the first of three major legislative acts affecting program caps was passed. Standard errors are clustered at the institution level.

For each type of student loan, time-series estimates for  $\xi_{as}$  are shown as the black lines in Figure 7. We also plot 95% pointwise confidence intervals, and include gray bars indicating the actual changes in each program maximum weighted by the average cross-sectional exposures (measured on 2004 NPSAS) for each aid type. For comparability, scales are set equal across all charts. For subsidized loans, the loading on subsidized exposure  $\xi_{as}$  of subsidized loan amounts (panel A) and tuition (panel B) spike coincident to the changes in subsidized maximums (gray bar) and are both significant at the 5% levels. For sticker-price tuition we indeed observe the largest (and the only statistically significant) spike in 2007–2008, but also observe higher sensitivity in 2006–2007, which may be consistent with some anticipatory effects from announcement to implementation of these policies.

For unsubsidized loans we observe a very similar pattern with respect to loan amounts (panel C) with spikes on the loadings on exposure that are coincident to the policy changes (2007–2008 and especially 2008–2009). Tuition's loading on unsubsidized exposure (panel D) displays higher than average levels in 2006–2007, 2007–2008, and 2008–2009, but only the 2008–2009 change is significant.

We do not conduct a parallel trends test for the Pell Grant effect for two reasons: first, the Pell Grant effect was insignificant in the baseline regression, and second, because the Pell Grant maximum increases occurred in multiple years, any yearly placebo estimates would naturally be confounded by the changes in other years.

In the Online Appendix, we show additional robustness checks where we measure exposures from the 2008 NPSAS wave rather than the 2004 wave, and where we specify the dependent variables in logarithm changes rather than level changes. In addition, the baseline specification only includes institutions



**Figure 7**  
Parallel trends tests

This figure shows a time series (orange) of estimated  $\xi$  coefficients from Equation (4) measuring the sensitivity of  $\Delta$ Student loans and  $\Delta$ Tuition to an institution exposure to subsidized and unsubsidized loans. We omit the academic year 2005–2006 in the regression which fixes its coefficient to zero. Dotted blue lines represent 95% confidence intervals based on standard errors clustered at the institution level. For each aid type, the grey bars show the actual mean change in program maximums, measured as the mean of yearly cap changes times institution exposures.

with at least ten students in NPSAS, while in the appendix we consider higher thresholds. Finally, we run weighted OLS regressions where larger schools are weighted more. Overall, we find that point estimates are robust to these changes in specifications. In unreported results, we also estimate placebo regressions with per-student state appropriations and the sum of nontuition sources of revenues as dependent variables for the main specification and do not find significant effects for these alternative dependent variables. All told, we find a robust pass-through of federal aid to tuition in the form of subsidized loans and a significant but at times weaker effect of unsubsidized loans. This weakness may be due to limitations to our identification approach, since, as we have discussed in Section 4, the exposures are more difficult to measure, and the policy change coincided with the contraction in the private student loan market and the Great Recession. It is also quite possible that subsidized loans, which represent a more significant subsidy than unsubsidized loans and are awarded to less needy students than Pell Grants, are in fact more economically meaningful in tuition-setting decisions. We believe the results we present on subsidized and unsubsidized loans are new to the literature. We find a sensitivity of changes in tuition to changes in subsidized loan amounts on the order of about 55–65

cents on the dollar, with estimates that are highly significant in essentially all of the specifications considered.

## 6.2 Attributes of tuition-increasing institutions

Results presented thus far indicate that changes in the sticker price of tuition are, on average, sensitive to changes in the supply of subsidized and unsubsidized loans, with a larger and more robust effect for subsidized loans. In this section, we dig deeper into these results to characterize the attributes of institutions that displayed the largest passthrough effects of aid on tuition. For each type of loan, we interact in Table 10 the measure of institution-level exposure with institution type in Columns 1 and 3, and with institution type and tuition level in 2004 in Columns 2 and 4.<sup>17</sup> This analysis essentially studies treatment effect heterogeneity by school type and is complementary to the specifications in Table 9 where we found that our estimate of the average treatment effect is slightly, but not substantially affected, by controlling for various institution characteristics. Those results suggest that the potential bias in estimating the average treatment effect arising from treatment effect heterogeneity by school type is small, while results that we discuss below estimate heterogeneous treatment effects directly.

In terms of changes in subsidized loan caps, we find that (Table 10, Column 1) different institution types displayed similar tuition sensitivities to changes in loan caps, with selective not-for-profit 4-year institutions responding the most (76 cents on the dollar), and nonselective not-for-profit 4-year institutions responding the least (57 cents on the dollar). However, if we also allow the treatment effect to vary continuously with tuition level (Column 2) we find that 2-year institutions and for-profit institutions are the most sensitive. In sum, more expensive institutions are generally more responsive to loan supply shifts, and conditional on tuition, for-profit and 2-year institutions are most responsive to subsidized loan changes.

Results for changes in the unsubsidized loan caps (Columns 3 and 4) are similar. Selective 4-year institutions are the most responsive before controlling for tuition, but after controlling for tuition, only for-profit status has a significant effect on sensitivity levels.

This result is in line with much of the popular press and recent research around for-profit institutions. Since the 1972 HEA reauthorization allowed for-profit institutions to be eligible to receive federal student aid, the market share of for-profit institutions has grown substantially (Deming, Goldin, and Katz, 2012). For-profit institutions now receive over 76.7% of their revenue, on average, through Title IV programs. This heavy dependence on federal aid has led to increased regulation and concern. We presented some anecdotal evidence that for-profit institutions reacted to federal aid changes using earnings call

<sup>17</sup> As the model in Appendix A points out, in the context of  $\gamma$ , these interaction effects can be complex and nonlinear. Because here we are estimating linear models, estimates are only picking up average effects.

**Table 10**  
**Sensitivity of aid exposures to institution attributes**

$\Delta$ StickerTuition <sub>it</sub>	(1)	(2)	(3)	(4)
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub> × Select4YNFP	0.760*** [0.23]	0.305 [0.27]		
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub> × MidSelect4YNFP	0.705*** [0.25]	0.492* [0.25]		
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub> × Nonselect4YNFP	0.566** [0.24]	0.412* [0.24]		
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub> × 2YNFP	0.624** [0.30]	0.637** [0.30]		
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub> × For-Profit	0.618*** [0.22]	0.523** [0.22]		
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub> × Tuition04 <sub>i</sub>		0.229*** [0.06]		
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub> × Select4YNFP			0.224*** [0.06]	0.104 [0.07]
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub> × MidSelect4YNFP			0.089 [0.06]	0.034 [0.06]
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub> × Nonselect4YNFP			0.112* [0.06]	0.066 [0.06]
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub> × 2YNFP			0.025 [0.08]	0.024 [0.08]
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub> × For-Profit			0.206** [0.09]	0.166* [0.09]
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub> × Tuition04 <sub>i</sub>				0.065*** [0.02]
SubLnExp <sub>i</sub> × $\Delta$ SLCap <sub>t</sub>			0.620*** [0.20]	0.611*** [0.20]
UsbLnExp <sub>i</sub> × $\Delta$ ULCap <sub>t</sub>	0.208*** [0.05]	0.210*** [0.05]		
PellGrantExp <sub>i</sub> × $\Delta$ PGCap <sub>t</sub>	0.224 [0.17]	0.238 [0.17]	0.254 [0.18]	0.294 [0.18]
Inst and year FEs?	Yes	Yes	Yes	Yes
Adj R <sup>2</sup>	0.38	0.38	0.38	0.38
N obs	8,900	8,900	8,900	8,900
N inst	930	930	930	930

This table expands on the baseline results of Table 5 by allowing the coefficients to vary across these institution characteristics: a dummy for private institutions, a dummy for 4-year programs, the 2004 levels of tuition, and average the EFC (both in thousands). See notes to Table 5 for more details. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

discussions and stock market responses in Section 4, and the above results suggest that for-profit status is indeed related to larger sensitivity to these policies than other types of institutions (after controlling for heterogeneity by tuition level).

However, our data contains a relatively limited number of for-profit institutions, as discussed in Section 3. Thus, in Table 11, we provide additional evidence on the differential effect of these increases on for-profit institutions by comparing changes in aid amounts at for-profit (top panel) and other institutions (bottom panel) in our sample period. For each type of institution (and panel) we regress yearly changes on year dummy variables (reported at the top of each panel and with the year 2006, which is the year preceding the policy changes, serving as the omitted year) as well as on a policy year dummy variable which is equal to one for the 2007–2008, 2008–2009, and 2009–2010 academic years

**Table 11**  
Years of federal loan, Pell Grant, and tuition increases for for-profit and not-for-profit institutions

For-Profits								
	$\Delta PG_{it}$		$\Delta SL_{it}$		$\Delta UL_{it}$		$\Delta STuition_{it}$	
Year = 2002	178**	[14]	-74**	[19]	-246**	[29]	25	[49]
Year = 2003	110**	[13]	-64**	[17]	-194**	[27]	226**	[46]
Year = 2004	-28**	[12]	-84**	[17]	-210**	[26]	36	[25]
Year = 2005	-112**	[14]	-115**	[18]	-252**	[27]	86**	[25]
Year = 2007	-35**	[14]	-50**	[18]	-317**	[27]	83**	[25]
Year = 2008	89**	[14]	460**	[20]	-117**	[27]	205**	[27]
Year = 2009	252**	[14]	-53**	[18]	670**	[29]	269**	[29]
Year = 2010	728**	[17]	-264**	[18]	-485**	[27]	269**	[29]
Year = 2011	106**	[16]	-215**	[18]	-576**	[28]	88**	[28]
Year = 2012	-485**	[18]	-249**	[19]	-374**	[30]	-102**	[30]
Constant	85**	[8]	164**	[10]	371**	[15]	487**	[15]
	$\Delta PG_{it}$		$\Delta SL_{it}$		$\Delta UL_{it}$		$\Delta STuition_{it}$	
Year = 2008,09,10	386**	[8]	148**	[9]	272**	[13]	212**	[16]
Constant	50**	[2]	67**	[2]	126**	[4]	523**	[5]
Inst FEs?	Yes		Yes		Yes		Yes	
N obs	18,750		16,980		16,760		16,880	
N inst	2,050		1,910		1,900		2,090	
Not-for-Profits								
	$\Delta PG_{it}$		$\Delta SL_{it}$		$\Delta UL_{it}$		$\Delta STuition_{it}$	
Year = 2002	-106**	[7]	-260**	[9]	-513**	[12]	-164**	[12]
Year = 2003	-157**	[7]	-165**	[9]	-456**	[12]	-38**	[13]
Year = 2004	-229**	[7]	-174**	[9]	-477**	[12]	60**	[14]
Year = 2005	-252**	[7]	-201**	[9]	-483**	[12]	33**	[13]
Year = 2007	-262**	[7]	-257**	[9]	-588**	[12]	6	[12]
Year = 2008	-161**	[7]	-22**	[10]	-445**	[13]	46**	[12]
Year = 2009	-76**	[7]	-223**	[9]	10	[16]	79**	[12]
Year = 2010	294**	[9]	-186**	[9]	-452**	[14]	54**	[13]
Year = 2011	-32**	[8]	-237**	[10]	-688**	[14]	36**	[13]
Year = 2012	-315**	[8]	-241**	[9]	-560**	[13]	90**	[12]
Constant	260**	[5]	292**	[5]	630**	[7]	618**	[7]
	$\Delta PG_{it}$		$\Delta SL_{it}$		$\Delta UL_{it}$		$\Delta STuition_{it}$	
Year = 2008,09,10	159**	[4]	16**	[4]	94**	[7]	54**	[7]
Constant	118**	[1]	134**	[1]	241**	[2]	623**	[2]
Inst FEs?	Yes		Yes		Yes		Yes	
N obs	39,420		38,390		37,830		37,850	
N inst	3,550		3,440		3,420		3,630	

These tables regress annual changes in federal subsidized and unsubsidized loans, Pell Grants, and sticker price tuition against year dummies. A year in this table (e.g., 2008) denotes an academic year (e.g., 2007–2008) end date. The omitted dummy is for the year 2006. Year = 2008, 2009, 2010 is a dummy variable corresponding to those years, which is when the federal aid cap changes take effect. Standard errors clustered at the institution level reported in brackets. Significance: \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

when the federal aid changes went into effect (reported at the bottom of each panel). As shown in the bottom section of the panels, for-profit institutions experienced significantly larger increases in disbursed aid over the years of the aid cap changes. Correspondingly, these institutions also displayed sticker tuition increases of about \$212, on average, as compared to \$54 for not-for-profit institutions. These larger tuition increases are consistent with the results in

the paper and the heavy reliance of for-profit institutions on federal student aid. This raw comparison has obvious limitations; for example, it does not allow us to control for other events specific to the for-profit sector that may have affected tuition. However, given the recent policy initiatives directly targeting aid for students attending for-profit institutions, a better understanding of the role of student borrowing for these institutions remains an open and important issue.

## 7. Concluding Remarks

We studied the effects of a student credit expansion on tuition costs using a difference-in-differences approach around changes in federal loan program maximums to undergraduate students in the academic years 2007–2008 and 2008–2009. Institutions that were most exposed to these program maximums ahead of the policy changes experienced disproportionate tuition increases. We estimate tuition effects of changes in institution-specific program maximums of about 60 cents on the dollar for subsidized loans and 20 cents on the dollar for unsubsidized loans.

These results suggest that, consistent with the paper's theoretical framework, credit expansions can impact tuition to a broad set of students including those who were not recipients of federal loans. Such pricing demand externalities are often conjectured in the context of the subprime credit expansion on housing prices leading up to the financial crisis, and in this respect this study provides complementary evidence for the student loan market. Documenting a link between a credit expansion and tuition in a comprehensive sample of institutions also contributes to the literature studying the Bennett hypothesis that has mostly focused on substitution effects between federal and institution grants.

It is important to note that while tuition increased steadily over our full sample period starting in 2002 and ending in 2012, the policy changes we exploit for identification were concentrated over 2 years in the latter part of the sample. As a result, a back-of-the-envelope calculation using our estimated coefficients and the aggregate change in student loan caps cannot explain much of the total increase in tuition.<sup>18</sup> But this does not rule out a role of student credit in the observed tuition trends more broadly. While changes in policy caps help us identify tuition sensitivity to a credit expansion, the existence of the loan programs boost students' ability to pay increasingly higher tuition amounts over time. Consistent with this argument, Cellini and Goldin (2014) use a sample of comparable aid-eligible and noneligible for-profit vocational institutions to

---

<sup>18</sup> For example, a simple back-of-the-envelope calculation of the effect of the increase in the subsidized loan cap would multiply the average exposure (0.16) by the size of the credit increase (937.5) by the difference-in-differences estimate of the treatment effect (0.643) to get 96.5. Analogously for the unsubsidized loan policy we have  $0.28 \times 2000 \times 0.202 = 113.12$ . On the other hand we note from Figure 1 that average sticker tuition increased almost \$3,000 between 2001 and 2012.

show that greater aid availability is associated with higher tuition levels. Further research extending these results for other sectors remains an important line of academic and policy research.

Should one conclude that federal student loan programs represent bad public policy? The answer to this question depends on a number of factors. First, in some ways, the spillovers we document may correspond to transfers from higher-income students to lower-income students at these institutions. We have only noisy institutional grant data, but the evidence suggests that increased student loan caps, if anything, decreased institutional grants on average. From a distributional perspective we also provided some evidence, although only over a 4-year period and thus with very limited identification, showing that while tuition discounting has become more common over our sample period, such discounting wasn't disproportionately evident for institutions affected by the policy changes that we study.

Second, the welfare impact of the credit expansion depends on how universities use the additional tuition revenue. Deming and Walters (2017) argue that increased per-pupil spending, as opposed to decreased costs, has a larger effect on educational attainment. With respect to our policy changes we were unable to find significant changes in next-year expenditures following the increased student loan maximums. However, these data, similar to our data on institutional grants (both come from the same IPEDS survey), are noisy, and the increased revenue might take time to affect institutional budgets.

Finally, from a welfare perspective, one key motivation for relaxing borrowing constraints is to increase access and participation in postsecondary education. Expansion in enrollment means increased access to postsecondary education, which is particularly important given the positive gap between the cost of education and its social or private benefit.<sup>19</sup> We found limited short-term enrollment effects around changes in program maximums, but it is also the case that institutions cannot quickly adjust capacity, which is what supports the pricing effects that we document in the first place. Many other studies have found longer-term responses of enrollment to aid, though relatively few of these have focused on loans. Dynarski (2002) uses a longer time frame for a natural experiment approach similar to ours and finds weak evidence of an increase in enrollment as a result of increased loan eligibility, but stronger evidence that the increases in eligibility altered college choice toward 4-year institutions. These potential benefits of alleviating credit constraints for prospective college students ultimately should be weighted against the tuition effects we have documented, resulting in difficult-to-sign welfare effects.

---

<sup>19</sup> While the literature disagrees on the exact magnitude of the returns to higher education (Card, 1992, Avery and Turner, 2012), the "college wage-premium" has been shown to be rising over the past two decades due to demand for skilled workers outpacing supply, and contributing to growing wage inequality in the United States (Goldin and Katz, 2009). Given this premium, to the extent that greater access to credit increases access to postsecondary education, student aid programs may help lower wage inequality by boosting the supply of skilled workers.

## Appendix A. Model

We present a detailed model that underlies the theoretical framework in the main text. The model aims to explain how increased student loan supply may affect sticker tuition, as well as the empirical identification assumption. A distinguishing feature of college pricing is the extent to which price discrimination takes place, with universities often using scholarships, grants, or other mechanisms to offer different prices to students of different incomes, skills, or backgrounds. Eligibility for most federal student aid, on the other hand, is based primarily on income considerations. We consider a school that conditions tuition offers on students' observable characteristics. In the model, an increase in the federal student loan maximum boosts demand from lower-income students by relaxing their borrowing constraints. In equilibrium, the increased ability to pay raises tuition for all students, and not just for the aid recipients. This pecuniary demand externality is an important feature of the model, to explain how sticker price responds to changes in federal loans, although aid recipients are likely charged discounted prices rather than sticker. The tuition effect is also largest for universities in which a large number of students are exposed to the policy change, a result that we use in the empirical section to identify the effects of an increase in loan maximums on sticker tuition.

To simplify the exposition, we assume that short-run school capacity is fixed at  $N$  seats, so schools only decide whom to admit and what tuition to charge them. In reality short-run seat supply is imperfectly elastic rather than fixed, but only this more general assumption is needed for our main model predictions. Schools observe coarse measures of student characteristics along two dimensions: quality and income. A student  $i$  can be of high-quality,  $q_H$ , or low-quality,  $q_L$ , and either income-constrained,  $n_C$ , or unconstrained,  $n_U$ . A fraction of students  $s$  is constrained, and a fraction  $r$  is low-quality, and for simplicity the two characteristics are uncorrelated. We assume a population 1 of potential students and that student type is sufficiently large so schools can pick any type distribution, or  $N < \min(s, 1 - s, r, 1 - r)$ . Schools make tuition offers conditional on observables, meaning students at a school pay one of four tuition levels  $t(q_i, n_i)$ .

Students accept a school's tuition offer if their valuation of the school exceeds the tuition cost and if they are able to afford the tuition cost given their income and aid. Thus, in addition to affecting the tuition they are charged, students' quality and income also determine their decision to attend. A student  $i$ 's valuation of a school's offer depends negatively on her observed quality, because a high-quality student is likely to have better offers from other schools or employers. Additional unobserved components to both quality and income are present to capture residual uncertainty for a school as to whether a student accepts an offer and its ability to extract rent as in standard third-degree price discrimination models (Tirole, 1988). The idiosyncratic unobserved component to a student's valuation of a school's offer is distributed as  $v_i \sim \text{Exp}(\delta)$ , and she is willing to accept the school's offer when

$$v_i - q_i \geq t(q_i, n_i). \tag{A1}$$

Similarly, we assume that a student's unobservable income shock is distributed as  $W_i \sim \text{Exp}(\omega)$ . Constrained students are offered a federal student loan of balance  $B$  and thus can afford to attend if their income and aid are such that<sup>20</sup>

$$W_i + n_i \geq t(q_i, n_C) - B. \tag{A2}$$

An unconstrained student does not face a financial constraint and does not qualify for federal aid; that is,  $W_U$  is sufficiently large that the financial constraint corresponding to (A2) never binds. Because of the unobservable components, a school does not know with certainty whether a student

<sup>20</sup> We are assuming that the interest charged is zero, as is the case, for example, for subsidized loan recipients when the student is in school. We are also assuming a fixed loan balance. In practice the loan balance is capped by the smaller of the loan maximum and the gap between cost of attendance and family contribution. We are therefore considering the case in which tuition levels are sufficiently high. This assumption can be relaxed.

accepts an offer. The demand from a high-income student with quality  $q_i$  is then equal to the probability that the student's unobserved valuation is sufficiently high:

$$d(q_i, n_U) = P(v_i \geq t + q_i) = e^{-\delta(t+q_i)}, \quad (A3)$$

while the demand from a low-income student with quality  $q_i$  is equal to the joint probability of a sufficiently high school valuation *and* income shock:

$$d(q_i, n_C) = P(v_i \geq t + q_i)P(W \geq t - B - n_C) = e^{-\delta(t+q_i) - \omega(t - B - n_C)}, \quad (A4)$$

where  $t = t(q_i, n_i)$ . The corresponding total demand functions from the four combinations of income and skills are given by the product of individual demands and the mass of students of each type combination.<sup>21</sup> Demand elasticities are  $\delta$  for unconstrained students, and  $\delta + \omega$  for constrained ones. Also, let  $D^H$ ,  $D^L$ ,  $D^U$ , and  $D^C$  be the sums of the corresponding demand elements or the aggregate demand from high-quality, low-quality, unconstrained, or constrained students and  $D$  be the sum of all these terms.

We assume that colleges maximize a combination of student quality and revenues as in Epple, Romano, and Sieg (2006):<sup>22</sup>

$$\max_{t(q,n)} \gamma N^{-1} (q_H D^H + q_L D^L) + (1 - \gamma) \left( \sum_{(q,n)} t(q,n) D^{q,n} - cD \right)$$

subject to:

$$D \leq N, \quad (A5)$$

where  $\gamma$  is the weight placed by the school on the average quality of its student population, and  $1 - \gamma$  is the weight on profits. The school incurs a unit cost  $c$  to provide a seat up to its maximum capacity  $N$ . The equilibrium levels of  $t$  are obtained from the first order conditions of this objective function:

**Proposition A1.** Let  $\lambda$  be the Lagrange multiplier on (A5). Then the optimal tuition levels satisfy

$$\begin{aligned} t^{q,U} &= c + \frac{1}{\delta} - \frac{q\gamma}{(1-\gamma)} + \frac{\lambda}{1-\gamma}, \\ t^{q,C} &= c + \frac{1}{\delta + \omega} - \frac{q\gamma}{(1-\gamma)} + \frac{\lambda}{1-\gamma}. \end{aligned} \quad (A6)$$

The Online Appendix provides all the proofs. This proposition states that the tuition charged to each group of students is a markup over marginal cost  $c$  that is inversely related to their demand elasticity and to their quality. Thus, lower quality students pay higher markups, as do less constrained students who have lower demand elasticities.

To study how an increase in  $B$  may affect tuition, we note that from (A4) an increase in the borrowing cap leads to an upward parallel shift of the demand curve for given  $t$ . It follows, that increasing the borrowing amount  $B$  affects equilibrium tuition through the shadow cost of a seat and that the effect is the same for all types of students:

<sup>21</sup> These are  $D^{H,U} = (1-s)(1-r)d(q_H, n_U)$ ;  $D^{L,U} = (1-s)r d(q_L, n_U)$ ;  $D^{H,C} = s(1-r)d(q_H, n_C)$ ; and  $D^{L,C} = sr d(q_L, n_C)$ .

<sup>22</sup> In Epple et al. (2006) schools maximize investment expenditure on students rather than revenues and also balance annual budgets so that the two conditions are equivalent. See also Gordon and Hedlund (2016) for similar modeling assumptions.

**Proposition A2.** . An increase in the federal loan amount  $B$  leads to equal increases in  $t^{H,U}$ ,  $t^{L,U}$ ,  $t^{H,C}$ , and  $t^{L,C}$ :

$$\frac{\partial t(q,n)}{\partial B} = \frac{1}{1-\gamma} \frac{\partial \lambda}{\partial B} = \frac{D^C \omega}{\delta N + D^C \omega} > 0 \tag{A7}$$

for  $q \in \{H, L\}$ ,  $n \in \{U, C\}$ .

That the tuition effects are exactly equal relies on our specific assumption that all  $C$  students borrow the exact same amount, but the general prediction that there is a price effect across types from relaxing the constraint for the constrained type holds even when we relax this assumption.

In the empirical section, we study the response of tuition to an increase in federal student loan caps, which we model here as an increase in  $B$ . If loan maximums were the only factor influencing tuition, estimates of (A7) could be backed out from average tuition increases in years when loan maximums were raised. However, since tuition trends are influenced by many other factors (e.g., the business cycle, changes in the returns to higher education), we abstract from these omitted variables using a difference-in-differences approach that exploits cross-sectional differences in the sensitivity of tuition changes to an increase in  $B$ . From (A7), the effect of  $B$  on tuition is greater the more  $C$  students attend ( $D^C/N$ ) and the higher the elasticity of  $C$  students versus  $U$  students ( $(\delta + \omega)/\delta$ ). While elasticity differences are difficult to measure, we use data on the share of aid recipients to measure  $D^C/N$ . However, because  $D^C/N$  is an equilibrium quantity, we show in the proposition below that the tuition effect is differentially larger for schools facing a higher  $s$ , that is, the fraction of low-income students in the population served by the school.

**Proposition A3.** The larger the share of  $C$  students, the higher the sensitivity of tuition to  $B$ .

$$\frac{\partial}{\partial s} \frac{\partial t}{\partial B} = \frac{\delta N \omega}{(\delta N + D^C \omega)^2} \frac{\partial D^C}{\partial s} > 0. \tag{A8}$$

The above proposition justifies our empirical approach of relating institutional exposures, calculated as the share of students who are constrained by a particular policy maximum, to predicted tuition increases in policy years. Given that our sample comprises for-profit and not-for-profit institutions, a natural question is to what extent the tuition effect depends on  $\gamma$ . The effect is ambiguous and depends on the difference between the quality of  $H$  and  $L$  students. This is because,  $\gamma$  and the distribution of student quality interact in determining the share of low-income students served by each institution.<sup>23</sup> In the empirical analysis, we study differential responses of tuition increases to shifts in loan caps as a function of  $D^C/N$ , and control for population quality and  $\gamma$  by including institution fixed effects in the empirical model.

## Appendix B. Additional Earnings Calls Transcripts

In this section we provide additional passages taken from earnings calls of the Apollo Group discussing the changes in federal student aid maximums.

<sup>23</sup> More precisely, we show in the appendix that

$$\frac{\partial}{\partial \gamma} \frac{\partial t}{\partial B} < 0 \Leftrightarrow \frac{D^{H,C}}{D^C} < \frac{\delta D^{H,U} + (\delta + \omega) D^{H,C}}{\delta D^U + (\delta + \omega) D^C}. \tag{A9}$$

<**Q - Mark Marostica**>: My question first relates to Brian's comment on the national pricing strategy, and I was wondering if you can give us some more specifics around that and whether or not you are actually planning to lower prices as part of that.

<**A - Brian Mueller**>: It is something that we are considering. I have talked about it the last couple of conferences we've attended. We have a very unique opportunity in July. Loan limits go up for first and second level students, which is fairly long overdue. By the time we get to July I am estimating that upwards of 70% of all students who are studying at the University of Phoenix at the level one and level two at those levels will be at Axia College at Axia College tuition rates. So there will be some room for us to raise tuition there from maybe 265 to 295 and from 285 to maybe 310, without putting a burden on students from a standpoint of out-of-pocket expense. At the graduate level there is a lot of room. We are actually quite a bit under the competition in our graduate programs, and there is a lot of room from a Title IV standpoint so that, again, we wouldn't put a burden on students from an out-of-pocket expense.

**Source:** Apollo Education Group, 2006:Q4 Earnings Call, accessed from Bloomberg LP Transcripts.

<**Q - Mark Hughes**>: And then any early view on whether Axia, with the price increase there affecting start levels in May?

<**A - Brian Mueller**>: Whether it's affecting start levels in May?

<**Q - Mark Hughes**>: Right. 10% increase in tuition. Is anybody balking at that, or trends steady?

<**A - Brian Mueller**>: No, thank you for asking that. No, because loan limits are raised on July 1, for level 1 and 2 students. And so students know as they go in if they're going to have enough title IV dollars to cover the cost of their tuition, so, no, it's not affecting new student starts.

**Source:** Apollo Education Group, 2007:Q2 Earnings Call, accessed from Bloomberg LP Transcripts.

<**Q - Brandon Dobell**>: One final one. Maybe as you think about discounting, at least the philosophy around affordability, pricing, discounting across the different brands or different programs, maybe, Brian, if you could speak to, has there been any change in terms of how you guys think about that? Do you think that discounting generates the wrong type of student or the right type of student, or how flexible do you think it will be going forward in terms of how you think about affordability issues?

<**A - Brian Mueller**>: We're not changing our thinking about that. It's really clear what's going on in the country economically, with the middle class getting squeezed. People don't have disposable income to spend for private school education but they understand its impact on their long-term career so they're willing to borrow the money at really good rates from a Title IV standpoint. And so if you can build your operations to the point that you can be profitable and keep those tuition rates inside Title IV loan limits you're going to do positive things with regards to retention, which will offset maybe the 4% to 6% increases that we would have gotten in the past.

**Source:** Apollo Education Group, 2007:Q2 Earnings Call, accessed from Bloomberg LP Transcripts.

## Appendix C. Stock Market Event Study Analysis

Here, we discuss stock market responses of publicly traded for-profit institutions to the three legislative changes discussed in Section 2. Table C1 reports event studies for abnormal returns over 3-day windows surrounding the passage of the three legislative changes to the HEA. Fourteen for-profit education companies were publicly traded around at least one of these legislative changes (and eight across all changes), including the Apollo Education Group among others. The cumulative abnormal returns are computed as each stock's excess return to the CRSP index returns, summed over the 3-day event window. We then calculate the (market cap) weighted and unweighted average of the cumulative abnormal returns of the eight publicly traded for-profit institutions to the index.

In the top panel of Table C1, we see that average 3-day cumulative abnormal returns around the 2006 reauthorization of HEA, which increased the subsidized loan limits for freshman and

**Table C1**  
**The stock market's reactions to changes in the federal aid policy**

Event	Date	Mkt wghts	Policy	Window	Cum. ab. ret.	z-score
Congress reauthorized the Higher Education Act	February 1, 2006	v	Loans	(-1,+1)	3.64%	(3.216)
		e			2.90%	(2.545)
College Cost Reduction and Access Act passes Congress	September 7, 2007	v	PG	(-1,+1)	2.17%	(2.204)
		e			2.22%	(2.242)
Ensuring Equal Access to Student Loans Act of 2008 is passed by the Senate	April 30, 2008	v	Unsub	(-1,+1)	4.86%	(2.570)
		e			4.80%	(2.480)
Ensuring Equal Access to Student Loans Act of 2008 is passed by Congress	May 1, 2008	v	Unsub	(-1,+1)	3.30%	(1.752)
		e			3.62%	(1.933)

This table reports 3-day cumulative abnormal returns for a portfolio of 14 publicly traded for-profit universities surrounding dates of legislative passage to changes in Federal Aid Policy. Returns are computed in excess of the CRSP index on a value-weighted and equal-weighted basis.

sophomores, were 3.64% and 2.9% under the value- and equally weighted market benchmarks, respectively. The abnormal returns are statistically significant and economically large. As shown in the middle panel, 3-day cumulative abnormal returns surrounding the 2007 legislative passage that increased Pell Grant amounts were 2.17% and 2.22%, respectively. Finally, we consider two separate event windows for the passing of the Ensuring Equal Access to Student Loans Act of 2008 which increased unsubsidized borrowing amounts.<sup>24</sup> Depending on the exact window used, abnormal returns on the for-profit institution portfolio ranged between 4.8% and 3.3%.

In sum, we find evidence that the passage of three pieces of legislation were associated with sizable abnormal stock market responses for the portfolio of publicly traded for-profit institutions. The nearly 10% abnormal return is consistent with the fact that students at for-profit institutions rely heavily on federal student aid to fund their education. In addition, anecdotal evidence also supports the view that changes in Title-IV programs boosted tuition at these institutions.

## Appendix D. Data Detail

This appendix complements Section 3 in providing a more detailed data description. The data used in the empirical analysis throughout this paper comes from three sources: IPEDS, Title IV, and NPSAS. We provide institutional details on each. We then describe in detail the variables we constructed using the data from each of these sources.

**Survey Data:** The IPEDS survey covers seven areas: institutional characteristics, institutional prices, enrollment, student financial aid, degrees and certificates conferred, student retention and graduation rates, and institutional human resources and finances. While IPEDS is the most comprehensive dataset on postsecondary education available, because it is based on surveys of administrators, it is not always sufficiently detailed or reliable for our purposes. For measures of federal aid at the institutional level, we found that the figures contained in the IPEDS “Student Financial Aid” survey did not meet our needs for a couple reasons. First, the survey restricts the

<sup>24</sup> On April 30, 2008, the Senate passed the Act, after already having received approval by the House. However, the Senate’s approving vote included some changes that had to be subsequently ratified by the House. Thus, the bill essentially passed on April 30, 2008, but the changes made by the Senate were not voted on, and subsequently passed by the House, until May 1, 2008. For completeness, we estimate 3-day abnormal returns around both event dates, though the two-event window obviously overlaps on one day.

universe to aid amounts for “full-time first-time degree-seeking undergraduates,” which is not our student population of interest; second, in part because of this restriction, the survey has been labeled as the most burdensome of surveys (Government Accountability Office (2010)); and third, until recently, the survey did not distinguish between federal loans and other loans, and still does not distinguish between subsidized and unsubsidized loans, which makes our identification more difficult. For these reasons, Title IV data serve as our primary data source for measuring federal loans and Pell Grants at the institution level.

**Sample:** Our sample begins in the 2000–2001 school year, the first year that the tuition sticker price survey from IPEDS more or less takes the current form. We end our sample in 2011–2012, since in 2012–2013, changes to graduate financial aid occur that may interfere with our identification. IPEDS and NPSAS data are reported at institution level (UNITID), while Title IV is reported at the OPEID level. This is because there may be multiple UNITIDs associated to one OPEID, as branches (UNITID) of the same institution are sometimes surveyed separately. Our regressions are done at the OPEID level, where when we are using averages of variables in IPEDS, we take enrollment-weighted averages of the UNITIDs.

**Sticker-Price Tuition:** Our main dependent variable is yearly changes in the sticker-price tuition at the institutional level. These data come from the IPEDS Student Charges survey. For full academic-year programs, we use the sum of the out-of-state average tuition for full-time undergraduates and the out-of-state required fees for full-time undergraduates. For other programs, we use the published tuition and fees for the entire program. For public universities, we use out-of-state tuition rather than average tuition to abstract from variation driven by changing fractions of in-state versus out-of-state students. We generally find that the in-state and out-of-state differences are highly correlated.

**Enrollment:** Enrollment can be measured both as headcount and full-time equivalent students. In general, we use an IPEDS formula to calculate a full-time-equivalent (FTE) enrollment measure. In certain cases though, we use total headcounts from the IPEDS enrollment survey, which are available by student level and attendance status.

**Federal Loan and Grant Usage:** For federal loan and grant totals, we rely on Title IV administrative data rather than the student financial aid survey from IPEDS, which appears to be somewhat unreliable as it is survey based. Title IV data contains the number of recipients, and total dollar amount of loans originated or grants disbursed for each institution and each of subsidized loans, unsubsidized loans, and Pell Grants. We only consider undergraduate policy changes and tuition in this paper, so we would want these amounts to be for undergraduates only. However, the Title IV data do not break out undergraduate and graduate loans separately until 2011. Pell Grants are only available to undergraduates, so are not affected. Since imputation of an undergraduate measure requires making several assumptions, our preferred measure of loan and grant usage at an institution is just the total dollar amount scaled by the FTE count of the university. We also report results for robustness when we scale the total dollar amount by the total enrollment count. Finally, also for robustness, we make an attempt to impute an undergraduate measure as follows: since the maximum subsidized loan amount changes only for undergraduates in our sample, we assume a constant average graduate loan amount over time,  $\bar{g}_i$  conditional on borrowing. In addition, we assume that the fraction of all subsidized loan borrowers who are graduate students also does not change,  $\gamma_i$ . To calculate  $\bar{g}_i$  and  $\gamma_i$ , we take the averages of the 2011 and 2012 values.<sup>25</sup> For prior years, given the total subsidized loan amount  $S_{it}$ , we calculate the undergraduate dollar amount borrowed as  $S_{it} - \gamma_i \bar{g}_i$ . We then scale this measure by total undergraduate enrollment.

**Exposures:** We calculate exposures using confidential NPSAS data as described in Section 4.3.

<sup>25</sup> We drop institutions from our sample where the 2011 and 2012 values differ significantly.

**Net Tuition and Institutional Grants:** Our institutional grant data comes from the IPEDS Finance Survey, which records as an expenditure item total grant dollars spent on scholarships and fellowships. We scale this measure by the FTE enrollment. We compute net tuition by subtracting institutional grants per FTE from sticker price.

**Financing Controls:** We follow the Delta Cost Project data in separating revenue data into a few main parts. The first is net tuition revenue, as described above. The next is federal funding, excluding Pell Grants. The third is state (and local) funding through appropriations and contracts. The fourth is private funding (from donations or endowment investment income), and the fifth is revenue from auxiliary operations (e.g., hospitals, dormitories). We use changes in these amounts, scaled by FTE enrollment, as controls in our regressions.

**Other Controls:** Average EFC comes from NPSAS data, and the admission rate comes from IPEDS.

**Trimming:** Because of some of the survey data issues discussed above, there are some large outliers in the yearly change variables that we use as our main dependent variables and as some independent variables. For this reason, we trim any observation that is more than 3 standard deviations away from the average value of a given variable.

## References

- Adelino, M., A. Schoar, and F. Severino (2012). Credit supply and house prices: evidence from mortgage market segmentation. Technical report, National Bureau of Economic Research.
- Angrist, J. D. (1993). The effect of veterans benefits on education and earnings. *Industrial and Labor Relations Review* 46(4), 637–652.
- Autor, D. H. (2003). Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics* 21(1), 1–42.
- Avery, C. and S. Turner (2012). Student loans: Do college students borrow too much? or not enough? *The Journal of Economic Perspectives*, 165–192.
- Bartik, T. J. (1991). Who benefits from state and local economic development policies? *Books from Upjohn Press*.
- Bennett, W. J. (1987). Our greedy colleges. *New York Times* 18, A27.
- Blanchard, O. J. and L. F. Katz (1992). Regional evolutions. *Brookings Papers on Economic Activity*, 1–75.
- Bound, J. and S. Turner (2002). Going to war and going to college: Did world war ii and the gi bill increase educational attainment for returning veterans? *Journal of Labor Economics* 20(4), 784–815.
- Cadena, B. C. and B. J. Keys (2013). Can self-control explain avoiding free money? evidence from interest-free student loans. *Review of Economics and Statistics* 95(4), 1117–1129.
- Card, D. (1992). Using regional variation in wages to measure the effects of the federal minimum wage. *Industrial & Labor Relations Review* 46(1), 22–37.
- Cellini, S. R. and C. Goldin (2014). Does federal student aid raise tuition? new evidence on for-profit colleges. *American Economic Journal: Economic Policy* 6(4), 174–206.
- Congressional Research Service (2014). Overview of the relationship between federal student aid and increases in college prices. Technical Report R43692.
- Deming, D. and S. Dynarski (2009). Into college, out of poverty? policies to increase the postsecondary attainment of the poor. Technical report, National Bureau of Economic Research.
- Deming, D. and C. Walters (2017). The impacts of price and spending subsidies on us postsecondary attainment. Technical report.

Deming, D. J., C. Goldin, and L. F. Katz (2012). The for-profit postsecondary school sector: Nimble critters or agile predators? *Journal of Economic Perspectives* 26(1), 139–64.

Dynarski, S. (2002). Loans, liquidity, and schooling decisions. *Kennedy School of Government Working Paper*.

Dynarski, S. M. (2003). Does aid matter? measuring the effect of student aid on college attendance and completion. *The American Economic Review* 93(1), 279–288.

Epple, D., R. Romano, S. Sarçça, and H. Sieg (2013). The us market for higher education: A general equilibrium analysis of state and private colleges and public funding policies. Technical report, National Bureau of Economic Research.

Epple, D., R. Romano, and H. Sieg (2006). Admission, tuition, and financial aid policies in the market for higher education. *Econometrica* 74(4), 885–928.

Favara, G. and J. Imbs (2015). Credit supply and the price of housing. *The American Economic Review* 105(3), 958–92.

Fos, V., A. Liberman, and C. Yannelis (2017). Debt and human capital: Evidence from student loans.

Goldin, C. D. and L. F. Katz (2009). *The race between education and technology*. Harvard University Press.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2017). Bartik instruments: What, when, why and how. *Unpublished Working Paper*.

Gordon, G. and A. Hedlund (2016, February). Accounting for the rise in college tuition. Technical report, National Bureau of Economic Research.

Government Accountability Office (2010). Institutions' reported data collection burden is higher than estimated but can be reduced through increased coordination. Technical report.

Hansen, W. L. (1983). Impact of student financial aid on access. *Proceedings of the Academy of Political Science*, 84–96.

Kane, T. J. (1995). Rising public college tuition and college entry: How well do public subsidies promote access to college? Technical report, National Bureau of Economic Research.

Kotlikoff, L. J. and L. H. Summers (1987). Chapter 16 tax incidence. In A. J. Auerbach and M. Feldstein (Eds.), *Handbook of Public Economics*, Volume 2 of *Handbook of Public Economics*, pp. 1043 – 1092. Elsevier.

Lee, D., W. Van der Klaauw, A. Haughwout, M. Brown, and J. Scally (2014). Measuring student debt and its performance. *FRB of New York Staff Report* (668).

Long, B. T. (2004a). How do financial aid policies affect colleges? the institutional impact of the georgia hope scholarship. *Journal of Human Resources* 39(4), 1045–1066.

Long, B. T. (2004b). The impact of federal tax credits for higher education expenses. In *College choices: The economics of where to go, when to go, and how to pay for it*, pp. 101–168. University of Chicago Press.

Looney, A. and C. Yannelis (2015). A crisis in student loans?: How changes in the characteristics of borrowers and in the institutions they attended contributed to rising loan defaults. *Brookings Papers on Economic Activity* 2015(2), 1–89.

Marx, B. M. and L. J. Turner (2015). Borrowing trouble? student loans, the cost of borrowing, and implications for the effectiveness of need-based grant aid. Technical report, National Bureau of Economic Research.

McPherson, M. S. and M. O. Schapiro (1991). *Keeping college affordable: Government and educational opportunity*. Brookings Institution Press.

Mezza, A., D. R. Ringo, S. Sherland, and K. Sommer (2016). On the effect of student loans on access to homeownership.

Mian, A. and A. Sufi (2009). The consequences of mortgage credit expansion: Evidence from the u.s. mortgage default crisis. *The Quarterly Journal of Economics* 124(4), 1449–1496.

Rizzo, M. J. and R. G. Ehrenberg (2003, February). Resident and nonresident tuition and enrollment at flagship state universities. Technical report, National Bureau of Economic Research.

Seftor, N. S. and S. E. Turner (2002). Back to school: Federal student aid policy and adult college enrollment. *Journal of Human Resources*, 336–352.

Singell, L. D. and J. A. Stone (2007). For whom the pell tolls: The response of university tuition to federal grants-in-aid. *Economics of Education Review* 26(3), 285–295.

Stanley, M. (2003). College education and the midcentury gi bills. *The Quarterly Journal of Economics*, 671–708.

Tirole, J. (1988). *The theory of industrial organization*. MIT press.

Turner, L. J. (2014). The road to pell is paved with good intentions: The economic incidence of need-based student aid. Technical report.

Yannelis, C. (2016). Asymmetric information in student loans. Technical report, New York University.